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THE

WORKS

OF THE HONOURABLE

ROBERT BOYLE.

In SIX VOLUMES.

To which is prefixed

The LIFE of the AUTHOR.

VOLUME THE FIRST.

A NEW EDITION.

LONDON:

Printed for J. and F. Rivington, L. Davis, W. Johnston, S. Crowder, T. Payne, G. Kearsley, J. Robson, B. White, T. Becket and P. A. De Hondt, T. Davies, T. Cadell, Robinson and Roberts, Richardson and Richardson, J. Knox, W. Woodfall, J. Johnson, and T. Evans. MDCCLXXII.

THE

PREFACE.

(To the Folio Edition of 1744.)

THE general reasons for collecting into one body the works of the honourable *Robert Boyle* are as obvious, as the excellence of the several parts of them is universally acknowledged. It may suffice therefore to acquaint the reader, that as a complete set of those before published is not perhaps to be found any where, except in the vast library of his learned friend Sir *Hans Sloane*, Bart. and as the whole contains a very large collection of philosophical essays on a great variety of subjects, full of curious experiments and observations, with several valuable discourses upon the most important subjects of religion; such a collection had been long desired, both for the public benefit, and as a justice due to the memory of that great restorer of the mechanical philosophy; and is now executed in the following manner:

I. THE several tracts formerly printed are taken from the most improved editions with the utmost correctness, and disposed in the order of time in which they were first published. This disposition of them was determined upon, as well from the reason of the thing, and with a view of shewing the rise, progress, and dependencies of the Author's discoveries; as in conformity to his own judgment: who, upon occasion of a Latin edition of his works at *Geneva* in 1677, complained publickly by his friend Mr. *Oldenburg*, secretary of the Royal Society, in the *Philosophical Transactions*, No. CXXX. p. 766, 767. "That the year in the frontis-" piece of that edition is one and the same, as if the several books con-" tained in this Latin volume had been published in one year; and the " enumeration of the several treatises, made in the catalogue of this Latin " edition, is not according to the time wherein they were first printed. " For the *first* of the books mentioned in the said catalogue was published " in English A. 1660, the *fifth* and *sixth* A. 1661, the *second* A. 1662, the " *seventh* A. 1664, the *fourth* A. 1666, the *third* A. 1670, the *eighth* A. " 1671, the *tenth* A. 1672, the *ninth* A. 1673. So preposterously are those " books ranged in this catalogue and volume: which the reader was to be

 " informed

" informed of, that by comparing the ſeveral true dates of the firſt edition " of this Author's works with the books of others ſince printed, the " priority of the experiments and conſiderations, reſpectively contained in " them, may be truly ſtated."

II. Some very conſiderable additions are made in this edition, which were never before publiſhed; namely, fragments of an *Appendix* to the *firſt Part* of the *Chriſtian Virtuoſo*, and of the *ſecond Part* of that work, preſerved purſuant to the Author's own deſire; with a large collection of letters of Mr. *Boyle* and his friends upon various ſubjects, ſelected from about fifteen hundred written by moſt of the great men of the laſt age both at home and abroad, with whom he correſponded. Theſe additions the public owes to the reverend and learned Mr. *Henry Miles* of *Tooting* in *Surrey*, and F. R. S. who is poſſeſſed of thoſe manuſcripts of Mr. *Boyle*, which were put into his hands, with leave to make uſe of them for the public good, by the late worthy Mr. *Thomas Smith*, apothecary in the *Strand*, who lived ſeventeen years with Mr. *Boyle*, and was with him at his death; and which have been lately increaſed by a part of the original collection, that had been communicated to Dr. *William Wotton*, author of *the Reflections upon ancient and modern Learning*, and were reſtored by his ſon-in-law the reverend Mr. *William Clarke*, canon reſidentiary of *Chicheſter*. Theſe manuſcripts are very numerous, but many of them written while the Author was very young, and few completed. However I ſhall ſubjoin to the *Contents* the liſt of them as drawn up by his own order, all of them being ſtill extant, except thoſe marked with an aſteriſk before them.

III. The copper-plates illuſtrating the ſeveral pieces are engraved with much greater exactneſs and elegance than thoſe in any former edition.

IV. There being only two original pictures of Mr. *Boyle* now known to be extant, it was thought proper to have them both engraved. One, which repreſents him in the 38th year of his age, is placed in the title-page of each volume, copied from a drawing of Mr. *Faithorne*, communicated by Sir *Hans Sloane*, from which likewiſe Mr. *Faithorne* himſelf engraved his print, with the inſtruments accompanying the head, according to the deſign of Dr. *Robert Hooke*, who thought the *face very carefully and well done, and very like*; as appears by his letters to Mr. *Boyle* of Auguſt 25, September 8, and December 15, 1664, printed in Vol. VI. p. 487, 488, and 501, of this edition. The other, which fronts the title-page of the firſt volume,

is

is taken from an original painting done in the latter part of his life, and now in the collection of Dr. *Mead*.

V. The *Life* contains Mr. *Boyle's* own *Account of himself during his minority*, extending to the year 1642; and is continued to his death from the beſt materials that could be procured at this diſtance of time, and without moſt of thoſe advantages which Dr. *Burnet* biſhop of *Saliſbury*, and Dr. *William Wotton*, who had the ſame deſign near fifty years ago, might have obtained for the execution of it. The only qualities I can engage for are induſtry and fidelity; but the genius and abilities of a *Gaſſendus* would be requiſite to do juſtice to a character ſuperior even to that of the celebrated *Peireſkius*.

I must not conclude this preface without returning my ſincereſt acknowledgments to Mr. *Miles* abovementioned, to whoſe great labour, judgment, and ſagacity, the conduct and improvements of this edition are chiefly to be aſcribed; and to Mr. *John Ward*, profeſſor of rhetoric at *Greſham* college, whoſe advice and aſſiſtance I have found extremely ready and uſeful to me upon this and all other occaſions.

THOMAS BIRCH.

London, Nov. 16, 1743.

THE

LIFE

OF THE HONOURABLE

ROBERT BOYLE.

By THOMAS BIRCH, M. A. and F. R. S.

THE honourable *Robert Boyle* was descended from a family, whose name before the conquest was written *Biuvile*. The first footing, that we find of them in the county of *Hereford*, was at *Pixely* court, near *Ledbury*, of which *Humphrey de Biuvile* was lord, as is recorded in Doom's-day book, tit. 28. *Terra Humfridi de Biuvile in Radelau Hund. Humfridus de Biuvile tenet de Rege Picbelei Austil tenuit.* These words *Austil tenuit*, in the general construction of antiquaries, import, that it was in the time of *Edward* the Confessor *(a)*.

Lodowick Boyle (b), who lived in the reign of king *Henry* III. was father of *John Boyle*, and he of *James*, who had issue *Lodowick*, whose son was succeeded by *James* his son and heir, father *(c)* of *Lodowick Boyle* of *Bidney*, and of the Friers in the city of *Hereford*, in the reign of king *Henry* VI. This *Lodowick* married *Elizabeth*, daughter of *William Russel*, Esq; and had issue *(d)* a daughter, *Eleanor*, married to *Watkin Russel*, and two sons, *John Boyle*, Esq; who had the estate in *Herefordshire*, and *Roger Boyle*, second son. This *Roger* married *Jane*, daughter of *Thomas Pattishal* of the county of *Hereford*, and had issue *John Boyle* of *Hereford*, *Roger*, second son, and *Michael Boyle* of *London*, third son, who left a numerous issue, whereof *Richard*, his second son, was bishop of *Corke* and *Ross*, being allowed also to keep the See of *Clogne* in commendam, was afterwards archbishop of *Tuam*, and died March 19. 1644. He left issue *Michael Boyle*, archbishop of *Armagh*, and lord chancellor of *Ireland*, who died, aged 93, in the year 1702, and was father to *Murrough Boyle*, created lord viscount *Blessington*. *Michael*, another son of the said *Roger*, was bishop of *Waterford* and *Lismore*; and Sir *George Boyle*, his fourth son, died without issue. *Roger Boyle*, the second son, married *Joan*, daughter of *John Naylor* of *Kenvile* in the county of *Kent*, and had issue *John Boyle*, bishop of *Corke* and *Ross* in *Ireland*; *Richard Boyle*, second son, earl of *Corke*, and father of that great man, of whom I am to treat; *Hugh Boyle*, third son, who died without issue; and a daughter married to Sir *Pierce Power* of *Ireland*, Knt.

Richard, the second son, was one of the greatest men of his age; and as he has left some memoirs of himself, written in 1632, in the sixty-seventh year of his age, which were never yet published entire, I shall insert so valuable a piece here in justice to his character and memory.

(a) Letter of Doctor *John Beale* to Mr. *Samuel Hartlib*.

(b) Visitat. Co. Hereford. in Biblioth. Harleyana 90. A. 17. p. 72, 73. apud *Collins's* Peerage of *England*, Vol. II. p. 358. ed. edit. *London*, 1741.

(c) *Seager's* Baron. M. S. in Biblioth. Cotton. apud *Collins*, p. 359.

(d) Visitat. Co. Hereford, *ut antea*.

" *Sir*

" *Sir* RICHARD BOYLE, *Knt. earl of* CORKE, *his true Remembrances.*

I Sir *Richard Boyle*, Knt. lord *Boyle*, baron of *Youghall*, viſcount *Dungarvan*, earl of *Corke*, lord high treaſurer of *Ireland*, one of his majeſty's honourable privy council, and one of the two lords juſtices for the government of this kingdom, do commend theſe *true remembrances* to poſterity this 23d day of June, *anno Domini* 1632, having lived in this kingdom of *Ireland* full forty-four years, and ſo long after as it ſhall pleaſe almighty God.

MY father, Mr. *Roger Boyle*, was born in *Hereforddſhire*. My mother *Joan Naylor*, daughter to *Robert Naylor* of *Canterbury* in the county of *Kent*, Eſq; was born the 15th of *October* in the 21ſt year of king *Henry* VIII. and my ſaid father and mother were married in *Canterbury* the 16th of *October* in the 8th year of queen *Elizabeth*. My father died at *Preſton* near *Feverſham* in *Kent* the 24th of March 1576. My mother never married again, but lived ten years a widow, and then departed this life at *Feverſham* aforeſaid, the 20th of March 1586. And they both are buried in one grave in the upper end of the chancel of the pariſh church of *Preſton*; in memory of which my deceaſed and worthy parents, I, their ſecond ſon, have *in anno Domini* 1629 erected a fair alabaſter tomb over the place where they were buried, with an iron gate before it for the better preſervation thereof.

SIR *Richard Boyle*, now earl of *Corke*, the ſecond ſon of *Roger Boyle*, Eſq; was born in the city of *Canterbury*, as I find it written by my father's own hand, the 3d of October 1566.

AFTER the deceaſe of my father and mother, I being the ſecond ſon of a younger brother, having been a ſcholar in *Bennet* college *Cambridge*, and a ſtudent in the *Middle Temple*, *London*, finding my means unable to ſuppor me to ſtudy the laws in the inns of court, put myſelf into the ſervice of Sir *Richard Manwood*, Knt. lord chief baron of her majeſty's court of Exchequer, whom I ſerved as one of his clerks; and perceiving, that the employment would not raiſe a fortune, I reſolved to travel into foreign kingdoms, and to gain learning, and knowledge, and experience abroad in the world. And it pleaſed the Almighty by his divine providence to take me, I may ſay juſtly, as it were, by the hand, and lead me into *Ireland*, where I happily arrived at *Dublin* on the Midſummer-Eve, the 23d day of June, 1588. I was married at *Limerick* to Mrs. *Joan Apſley*, one of the two daughters and coheirs of *William Apſley*, Eſq; the 6th of November 1595, who brought me in 500 *l.* lands *per* year, which I ſtill enjoy, it being the beginning and foundation of my fortune; and ſhe died at *Moyallo* the 14th of December 1599, in travel of her firſt child, who was born a dead ſon, and both of them buried together in *Buttavant* church.

WHEN firſt I arrived at *Dublin* in *Ireland*, the 23d of June 1588, all my wealth then was twenty-ſeven pounds three ſhillings in money, and two tokens, which my mother had given me, viz. a diamond ring, which I have ever ſince and ſtill do wear, and a bracelet of gold worth about ten pounds; a taffety doublet cut with and upon taffety, a pair of black velvet breeches laced, a new *Milan* fuſtian ſuit laced and cut upon taffety, two cloaks, competent linen and neceſſaries, with my rapier and dagger. And ſince, the bleſſing of God, whoſe heavenly providence guided me hither, hath enriched my weak eſtate in beginning with ſuch a fortune, as I need not envy any of my neighbours, and added no care or burthen of my conſcience thereunto. And the 23d of June 1632, I have ſerved my God, queen *Elizabeth*, king *James*, and king *Charles*, full forty-four years, and ſo long after, as it ſhall pleaſe God to enable me.

WHEN

WHEN God had bleſſed me with a reaſonable fortune and eſtate, Sir *Henry Wallop* of *Wares*, Sir *Robert Gardiner*, chief juſtice of the King's Bench, Sir *Robert Dillam* chief juſtice of the Common Pleas, and Sir *Richard Bingham*, chief commiſſioner of *Connaught*, being diſpleaſed for ſome purchaſes I had made in the province, they all joined together, by their lies complaining againſt me to queen *Elizabeth*, expreſſing, that I came over a young man, without any eſtate or fortune; and that I had made ſo many purchaſes, as it was not poſſible to do it without ſome foreign prince's purſe to ſupply me with money: that I had acquired divers caſtles and abbeys upon the ſea-ſide fit to receive and entertain Spaniards; that I kept in my abbeys fraternities and convents of friers in their habits, who ſaid maſs continually: and that I was ſuſpected in my religion; with divers other malicious ſuggeſtions, whereof having ſome ſecret notice, I reſolved to go into *Munſter*, and ſo into *England*, to juſtify myſelf. But before I could take ſhipping, the general rebellion in *Munſter* broke out: all my lands were waſted, as I could ſay, that I had not one penny of certain revenue left me, to the unſpeakable danger and hazard of my life. Yet God preſerved me, as I recovered *Dingle*, and got ſhipping there, which tranſported me to *Briſtol*, from whence I travelled to *London*, and betook myſelf to my former chamber in the Middle Temple, intending to renew my ſtudies in the law, till the rebellion were paſt over. Then *Robert* earl of *Eſſex* was deſigned for the government of this kingdom, into whoſe ſervice I was recommended by Mr. *Anthony Bacon*. Whereupon his lordſhip very nobly received me, and uſed me with favour and grace, in employing me in the iſſuing out his patent and commiſſions for the government of *Ireland*; whereof Sir *Henry Wallop*, treaſurer, having notice, and being conſcious in his own heart, that I had ſundry papers and collections of *Michael Kettlewell*'s his late under-treaſurer, which might diſcover a great deal of wrong and abuſe done to the queen in his late accounts; and ſuſpecting, if I were countenanced by the earl of *Eſſex*, that I would bring thoſe things to light, which might much prejudice or ruin his reputation or eſtate, although I vow to God, until I was provoked, I had no thought of it; yet he, utterly to ſuppreſs me, renewed his former complaints againſt me to the queen's majeſty. Whereupon, by her majeſty's ſpecial directions, I was ſuddenly attached and conveyed cloſe priſoner to the Gate-houſe; all my papers ſeized and ſearched; and although nothing could appear to my prejudice, yet my cloſe reſtraint was continued till the earl of *Eſſex* was gone to *Ireland*, and two months afterwards; at which time, with much ſuit, I obtained the favour of her ſacred majeſty to be preſent at my anſwers, where I ſo fully anſwered and cleared all their objections, and delivered ſuch full and evident juſtifications for my own acquittal, as it pleaſed the queen to uſe theſe words, viz. "By God's death, theſe are but inventions againſt this young man; and all his ſufferings are for being able to do us ſervice, and thoſe complaints urged to foreſtal him therein. But we find him to be a man fit to be employed by ourſelves; and we will employ him in our ſervice; and *Wallop* and his adherents ſhall know, that it ſhall not be in the power of any of them to wrong him; neither ſhall *Wallop* be our treaſurer any longer." Thereupon ſhe directed her ſpeech to her lords in her council there preſent, and commanded them preſently to give her the names of ſix men, out of which ſhe might chuſe one to be treaſurer of *Ireland*, her election falling upon Sir *George Carew*, of *Cookington*. And then the queen aroſe from council, and gave orders, not only for my preſent enlargement, but alſo diſcharging all my charges and fees during my reſtraint, and gave me her royal hand to kiſs, which I did heartily, humbly thanking God for that deliverance.

BEING

BEING commanded by her majesty to attend at court, it was not many days before her highness was pleased to bestow upon me the office of clerk of the council of *Munster*, and to recommend me over to Sir *George Carew*, after earl of *Totness*, then lord president of *Munster*. Whereupon I bought of Sir *Walter Ralegh* his ship called the *Pilgrim*, into which I took a freight of ammunition and victuals, and came in her myself by long seas, and arrived at *Carrig-Foyl-Kerry*, where the lord president and the army were at the siege of that castle; which when he had taken, I was there sworn clerk of the council of *Munster*, and presently after made a justice and quorum throughout all that province. And this was the second rise that God gave to my fortune.

THEN, as clerk of the council, I attended the lord president in all his employments, and waited upon him all the whole siege of *Kingsale*, and was employed by his lordship to her majesty, with the news of that happy victory; in which employment I made a speedy expedition to the court; for I left my lord president at *Shannon* castle, near *Corke*, on the Monday morning about two of the clock, and the next day, being Tuesday, I delivered my packet, and supped with Sir *Robert Cecil*, being then principal secretary of state, at his house in the *Strand*; who after supper held me in discourse till two of the clock in the morning; and by seven that morning called upon me to attend him to the court, where he presented me to her majesty in her bedchamber, who remembered me, calling me by my name, and giving me her hand to kiss, telling me, that she was glad, that I was the happy man to bring the first news of that glorious victory. And after her majesty had interrogated with me upon sundry questions very punctually, and that therein I had given her full satisfaction in every particular, she again gave me her hand to kiss, and recommended my dispatch for *Ireland*, and so dismissed me with grace and favour.

AT my return into *Ireland*, I found my lord president ready to march with the army to the siege of *Beer-haven* castle, then fortified and possessed by the *Spaniards* and some Irish rebels; which after battering we had made assaultable, we entered, and put all to the sword. His lordship fell then to reducing those western rebels of the province to subjection and obedience of her majesty's laws; and having placed garrisons and wards in all places of importance, made his return to *Corke*, and in his way homewards acquainted me with his resolution; it being presently to employ me into *England*, to obtain licence from her majesty, for his repair to her royal presence: at which time he propounded unto me the purchase of all Sir *Walter Ralegh*'s lands in *Munster*, offering me his best assistance for the compassing thereof; which he really performed. For, upon my departure for *England*, he wrote by me two effectual letters, one to Sir *Robert Cecil*, wherein he was pleased to magnify my service and abilities; and concluding with a request, that he would make intercession with Sir *Walter Ralegh* to sell me all his lands in *Ireland*, that were then altogether waste and desolate. To Sir *Walter Ralegh* he also wrote, advising him to sell me all his lands in *Ireland*, then untenanted, and of no value to him; mentioning withal, that, in his lordship's knowledge, his estate in *Ireland* never yielded him any benefit, but contrariwise stood him in two hundred pounds yearly for the maintenance and support of his titles. Whereupon there was a meeting between Sir *Robert Cecil*, Sir *Walter Ralegh*, and myself; where Sir *Robert Cecil* mediated and concluded the purchase between us. Accordingly my assurances were perfected; and this was a third addition and rise to my estate.

THEN I returned to *Ireland* with my lord president's licence to repair to court, where in his way to *Dublin* (where he proposed to embark) he dealt very nobly and fatherlike by me, in persuading me, it was high time for me to take a wife, in hopes of

posterity to inherit my lands; advising me to make choice of Sir *Geoffry Fenton*'s daughter; and that if I could affect her, he would treat with her parents to have the match between us; wherein he prevailed so far, as the 9th of March 1602, I was, in his lordship's presence, contracted to her in her father's house at *Dublin* *.

THE 25th of July, 1603, I was married to my second wife, Mrs. *Catharine Fenton*, the only daughter of Sir *Geoffry Fenton*, principal secretary of state, and privy counsellor in *Ireland*, with whom I never demanded any marriage portion, neither promise of any, it not being in my consideration; yet her father, after my marriage, gave me one thousand pounds in gold with her. But that gift of his daughter unto me I must ever thankfully acknowledge as the crown of all my blessings; for she was a most religious, virtuous, loving, and obedient wife unto me all the days of her life, and the happy mother of all my hopeful children, whom with their posterity I beseech God to bless.

THE 10th of July, 1620, my eldest brother, Dr. *John Boyle*, lord bishop of *Corke* and *Ross*, departed this life at *Bishop's-Court* near *Corke*; and on the 12th of that month was buried in my new tomb, erected in the chapel, which I re-edified at *Youghall*. After whose death I obtained those bishoprics from his majesty for my uncle *Michael Boyle*'s son, *Richard Boyle*, for whom I formerly obtained the deanery of *Waterford*, who now succeeds my brother in those bishoprics.

I *Richard*, earl of *Corke*, was knighted by Sir *George Carew*, lord deputy of *Ireland*, at St. *Mary*'s abbey, near *Dublin*, the 25th of July 1603, being St. *James*'s day, and the very day that I was married to my second wife, Mrs. *Catharine Fenton*, I was sworn a privy counsellor to King *James* for the province of *Munster* at the council table at *Dublin* by the lord *Chichester*, then lord deputy of *Ireland*, the 12th of March, 1606, with commandment from the lord deputy and council to *Henry Dunkard*, then lord president of *Munster*, to admit me into that council; who, upon former direction from this state, had refused either to swear or admit me a counsellor of that province.

* Dr. *Anthony Walker*, in p. 45, 46, of his funeral sermon on *Mary*, countess dowager of *Warwick*, daughter to the earl of *Corke*, relates a very remarkable story, concerning this marriage, which he had from that lady's own mouth, and is as follows: "Mr. *Boyle*, after earl of *Corke*, who was then a widower, came one morning to wait upon Sir *Geoffry Fenton*, at that time a great officer of state in the kingdom of *Ireland*; who being engaged in business, and not knowing who it was who desired to speak with him, a while delayed him access, which time he spent pleasantly with his young daughter in her nurse's arms. But when Sir *Geoffry* came and saw whom he had made stay somewhat long, he civilly excused it. But Mr. *Boyle* replied, he had been very well entertained, and spent his time much to his satisfaction in courting his daughter, if he might obtain the honour to be accepted for his son-in-law. At which Sir *Geoffry* smiling, to hear one, who had been formerly married, move for a wife carried in arms, and under two years old, asked him, if he would stay for her? To which he frankly answered him, he would; and Sir *Geoffry* as generously promised him, he should then have his full consent. And they both kept their words honourably; and by this virtuous lady he had thirteen children." Mr. *Evelyn*, in a letter to Dr. *William Wotton*, dated at *Wotton* in *Surrey*, Sept. 10th, 1703, mentions likewise this story in the following terms; "that Mr. *Boyle* coming to advise with Sir *Geoffry Fenton*, now knighted, finding him engaged with another client, and seeing a pretty child in the nurse's arms, entertained himself with them, till Sir *Geoffry* came to him, making his excuse for causing him to wait so long. Mr. *Boyle* pleasantly told him, he had been courting a young lady for his wife. And so it fortuned, that sixteen years after, Mr. *Boyle* made his addresses in good earnest, and married the young lady." Both these accounts are attended with great difficulties, especially that of Dr. *Walker*, which is absolutely irreconcilable with the earl of *Corke*'s own narrative, from which it appears, that his first lady died on the 11th of December 1599, and that he married his second, Sir *Geoffry Fenton*'s daughter, on the 25th of July, 1603; so that it is impossible, that while he was a *widower*, he should enter into any engagements for the latter, while she was *in her nurse's arms*, and *under two years old*, since there was only an interval of three years and eight months between the death of his first wife and his marriage with the second.

I WAS sworn a privy counsellor of state of the kingdom of *Ireland* by the lord *Chichester*, then lord deputy, at *Chichester* house, the 15th of February, being the day that I arrived out of *England* at *Dublin*, 1612.

I WAS created lord *Boyle*, baron of *Youghall*, on Michelmas day, the 29th of Sept. 1616.

I WAS created lord viscount of *Dungarvan* and earl of *Corke*, the 26th of October, 1620.

Adam lord *Loftus* of *Ely*, and lord chancellor of *Ireland*, and I, *Richard*, earl of *Corke*, were sworn lords justices for the joint government of this kingdom of *Ireland*, the 26th of October, 1629, with the entertainment of one hundred pounds sterling per month to each of us.

I WAS made lord high treasurer of *Ireland*, and sworn the 9th of November, 1631.

The several days of the births of all the children, that God hath blessed me withall, with the places where they were born, and the names of their godfathers and godmothers.

1. MY first son *Roger*, born at *Youghall* the 1st of August 1606. Sir *Allen Apsley*, Sir *Thomas Brown* godfathers, and lady *Alice Fenton* godmother. He died at *Deptford* in *Kent*, the 10th of October 1615, and was buried there*.

(e) Married to the earl of *Barrimore*.

2. MY first daughter, *Alice Boyle* (e), born at *Youghall*, the 20th of March, 1607; Sir *Robert Tynt* godfather, lady *Fenton* and Mrs. *Bernard* godmothers.

3. MY second daughter, *Sarah Boyle*, born at *Dublin*, the 29th of March, 1609; Sir *William Usher* godfather, lady *Winch* and lady *Ely* godmothers. She was married to Sir *Thomas Moore* first, and then to lord *Robert Digby*, the 15th of December, 1626.

(f) Married to the lord *Goring*.

4. MY third daughter, *Lettice* (f), born the 25th of April, 1610; earl of *Thomond* godfather, lady *Chichester* and lady *Moore* godmothers.

5. MY fourth daughter, *Joan*, born the 14th of June, 1611; Sir *William Fenton* godfather, and —— *Brown* and lady *Fenton* godmothers. She was married to the earl of *Kildare*, Aug. 15, 1628, and had two children, *Richard* and *Elizabeth*.

6. MY second son, *Richard*, was born at the college of *Youghall*, the 20th of October, 1612; earl of *Thomond*, Sir *Richard Aldworth*, and Mr. *Thomas Ball* of *London*, godfathers, and lady *Ann Parsons* godmother. God grant he may serve and fear him religiously, and be a faithful subject and servant to the king's majesty and his heirs, and live many years full of good works and of virtuous children, and be a worthy pillar and patriot in this kingdom. He being viscount of *Dungarvan*, was knighted in my house in *Youghall* the 13th of Aug. 1624, by the lord *Falkland*, lord deputy general of *Ireland*; and my said son departed *Dublin*, to begin his travels into foreign kingdoms, the 4th of June, 1632, I allowing him one thousand pounds a year in his travels.

(g) Married to the lord viscount *Ranelagh*.

(h) Doctor *George Stonhope*'s letter to *John Evelyn*, Esq; dated Sept. 6, 1703, and Mr. *Evelyn*'s letter to Dr. *W. Wotton*, dated at *Wotton* in *Surrey*, Sept. 10, 1703.

7. MY daughter *Catharine* (g), born the 22d of March, 1614; Sir *Robert Bolton* godfather, lady *Fenton* and lady *Harris* godmothers.

* He died at *Say's-Court* in that town, and was interred in the parish church, were a monument was erected to him with the following inscription (h):

H. S. E. *ROGERUS BOYLE, Richardi Comitis Corcaciensis Filius primogenitus; qui in Hiberniâ natus, in Cantio solo patris natali denatus, dum hic ingenii cultum capessit, puer eximiæ indolis præcacitatem ingenii funere luit immaturo. Sic luculenti, sed terreni patrimonii factus exhæres, cœlestem crevit hæreditatem. Decessit Anno* CIODCXV. VI. *Eid.* VIII*bris.*

8. MY

8. My son *Jeoffry* born at *Youghall* the 10th of April, 1616. He died.

9. My daughter *Dorothy*, born the 31st of December, 1617. She was married to lord *Loftus*.

10. My son *Lewis*, born the 23d of May, 1619, and in the year 1628 he was created baron of *Bandonbridge*, and lord viscount *Boyle* of *Kenelmeaky*.

11. My son *Roger*, born the 25th of April, 1621. He was created lord *Boyle*, baron of *Broghill*, 1628.

12. My son *Francis* (*i*), born the 25th of June, 1623.

13. My daughter *Mary* (*k*), born the 11th of November, 1624.

14. My seventh son *Robert*, born the 25th of January, 1626.

15. My eighth and last daughter *Margaret* (*l*), born in *Channel Row* in *Westminster* the 30th of April, 1629.

(*i*) Afterwards lord *Shannon*.
(*k*) Married to the earl of *Warwick*.
(*l*) She died unmarried.

THE great God of heaven I do humbly and heartily beseech, to bless all these my children, whom he hath in his mercy so graciously bestowed upon me, with long and religious lives, and that they may be fruitful in virtuous children and good works, and continue till their lives end loyal and dutiful subjects to the king's majesty and his heirs, and approve themselves good patriots and members to his commonwealth; which is the prayer and charge of me their father in the 67th year of my age, 1632.

My dear wife, the crown of all my happiness, and mother of all my children, *Catharine* countess of *Corke*, was translated at *Dublin* from this life into a better the 16th of February 1629-30, and was the 17th privately buried in the night in the upper end of the choir of St. *Patrick*'s church in *Dublin*, in the grave or vault, wherein Dr. *Weston*, her grandfather, and good lord chancellor of *Ireland*, and Sir *Geoffry Fenton*, his majesty's principal secretary of state for this realm, were entomb'd. Her funerals were honourably solemnized in publick the 11th of March *Anno Domini* 1629. In the perpetual memory of which my virtuous and religious deceased wife, and of her predecessors and posterity, I have caused a very fair tomb to be erected, with a cave or cellar of hewed stone underneath it.

I HAVE purchased from the dean and chapter of St. *Patrick*'s church the inheritance of the upper part of the chancel, wherein the cave or cellar under ground is made, and wherein the tomb is built, to be a burying place for me and my posterity, and their children."

THIS noble earl continued in great prosperity till the breaking out of the rebellion in *Ireland* in 1641; and the county of *Corke* was the last that suffered under the violences of the Papists, being the best inhabited with English of any part of that kingdom, by the plantations made by his lordship, and was in a great measure preserved by his generosity and diligence. He was then just returned out of *England*, and on this irruption immediately raised two troops of horse, which he put under the command of his sons, the lord viscount *Kinelmeaky* and the lord *Broghill*, maintaining them and 400 foot for some months at his own charge (*m*); and at the battle of *Liscarrol*, on the 3d of September 1642, wherein the English obtained a compleat victory, had no less than four of his sons engaged, viz. his eldest son the lord viscount *Dungarvan*, the lord viscount *Kinelmeaky*, the lord *Broghill*, and Mr. *Francis Boyle*, afterwards created lord viscount *Shannon*. But in this engagement he had the misfortune of losing one of his sons, the lord *Kinelmeaky*, governor of *Bandon*, who in several encounters had defeated the rebels, and with his father and brothers preserved the county of *Corke* (*n*). At length this great man, in the midst of these confusions, departed this life at *Youghall*, and was interred there near the date (if not on the day) of

(*m*) Cox's History of *Ireland*, Vol. II. p. 95.
(*n*) Ibid. p. 112.

(o) Borlase's Reduction of *Ireland*. p. 209.
(p) Id. ibid.
(q) Ibid.

of the cessation of arms concluded at *Sigginstown*, Sept. 15, 1643 (o) *unwilling to survive what he suspected might not be auspicious to the English, or conducible to the end for which it was designed; wherein he prophesied not ill* (p). Mr. *Borlase* tells us (q) that his lordship "was a person, for his abilities and knowledge in the affairs of the world, "eminently observable, inasmuch, as though he was no peer of *England*, he was ad-"mitted to sit in the lords house upon the wool-sacks, *ut consiliarius*. And for all the "estate he arrived at, (which was the greatest in the memory of the last age) none "ever taxed him with exorbitances, but such as thought princes had too little, and "religious men not enough." Another writer (r) likewise gives him this character, that he was "one of the most extraordinary persons, either that or any other age "hath produced, with respect to the great and just acquisitions of estate that he "made, and the publick works that he began and finished, for the advancement "of the English interest and the protestant religion in *Ireland*; as churches, alms-"houses, free-schools, bridges, castles, and towns, *viz. Lismore*, *Tallow*, *Cloghma-"kilty*, *Iniskeen*, *Castletown*, and *Bandon*, (which last place cost him 14,000*l.*) inso-"much that when *Cromwell* saw these prodigious improvements, which he little ex-"pected to find in *Ireland*, he declared, that *if there had been an earl of* Corke *in "every province, it would have been impossible for the Irish to have raised a rebellion.* "And whilst he was carrying on these solid works, he lived in his family at a rate "of plenty, that exceeded those who consumed great estates in the lavish ways of "ill-ordered excess. His motto, *God's providence is my inheritance*, shews, from "whence he derived all his blessings; the greatest of which was the numerous and "noble posterity he had to leave his estate unto."

(r) Cox, Introduction to the 2d volume of his History of *Ireland*.

ROBERT BOYLE, Esq; his seventh and youngest son, and one of the greatest ornaments of this noble family, as well as the age and country in which he lived, has left us some memoirs of the younger part of his life, drawn up soon after his return from his travels, which the reader will undoubtedly choose to read in his own words.

An Account of PHILARETUS, [i. e. Mr. R. BOYLE,] during his Minority.

NOT needlessly to confound the herald with the historian, and begin a relation by a pedigree, I shall content myself to inform you, that the immediate parents of our *Philaretus* were, of the female sex, [*Catherine*, daughter of Sir *Geoffry Fenton*] a woman, that wanted not beauty, and was rich in virtue, and on the father's side, that *Richard Boyle*, earl of *Corke*, who, by God's blessing on his prosperous industry, from very inconsiderable beginnings, built so plentiful and so eminent a fortune, that his prosperity has found many admirers, but few parallels.

HE was born the 14th child of his father (of which five women, and four men, do yet survive) in the year 1626-7, upon St. *Paul*'s conversion day, at a country-house of his father's, called *Lismore*, then one of the noblest seats and greatest ornaments of the province of *Munster*, in which it stood; but now so ruined by the sad fate of war, that it serves only for an instance and a lecture of the instability of that

that happineſs, that is built upon the uncertain poſſeſſion of ſuch fleeting goods, as itſelf was.

To be ſuch parents ſon, and not their eldeſt, was a happineſs, that our *Philaretus* would mention with great expreſſions of gratitude; his birth ſo ſuiting his inclinations and deſigns, that, had he been permitted an election, his choice would ſcarce have altered God's aſſignment. For as on the one ſide, a lower birth would have too much expoſed him to the inconveniencies of a mean deſcent, which are too notorious to need ſpecifying; ſo on the other ſide, to a perſon, whoſe humour indiſpoſes him to the diſtracting hurry of the world, the being born heir to a great family is but a glittering kind of ſlavery, whilſt obliging him to a public entangled courſe of life, to ſupport the credit of his family, and tying him from ſatisfying his deareſt inclinations, it often forces him to build the advantages of his houſe upon the ruins of his own contentment.

A MAN of mean extraction is ſeldom admitted to the privacy and ſecrets of great ones promiſcuouſly, and ſcarce dares pretend to it, for fear of being cenſured ſaucy, or an intruder; and titular greatneſs is ever an impediment to the knowledge of many retired truths, that cannot be attained without familiarity with meaner perſons, and ſuch other condeſcenſions, as fond opinion, in great men, diſapproves and makes diſgraceful.

BUT now our *Philaretus* was born in a condition, that neither was high enough to prove a temptation to lazineſs, nor low enough to diſcourage him from aſpiring. And certainly to a perſon, that affected ſo much an univerſal knowledge, and arbitrary viciſſitudes of quiet and employments, it could not be unwelcome to be of a quality, that was a handſome ſtirrup to preferment, without an obligation to court it, and which might at once both protect his higher pretenſions from the guilt of ambition, and ſecure his retiredneſs from contempt.

WHEN once *Philaretus* was able, without danger, to ſupport the incommodities of a remove, his father, who had a perfect averſion for their fondneſs, who uſed to breed their children ſo nice and tenderly, that a hot ſun, or a good ſhower of rain, as much endangers them, as if they were made of butter, or of ſugar, ſends him away from home, and commits him to the care of a country nurſe, who by early inuring him, by ſlow degrees, to a coarſe but cleanly diet, and to the uſual paſſions of the air, gave him ſo vigorous a complexion, that both hardſhips were made eaſy to him by cuſtom, and the delights of conveniences and eaſe were endeared to him by their rarity.

SOME few years after this, two great diſaſters befel *Philaretus*; the one was the deceaſe of his mother, whoſe death would queſtionleſs have exceſſively afflicted him, had but his age permitted him to know the value of his loſs; for he would ever reckon it amongſt the chief miſfortunes of his life, that he did never know her that gave it him, her free and noble ſpirit (which had a handſome manſion to reſide in) added to her kindneſs and ſweet carriage to her own, making her ſo hugely regretted by her children, and ſo lamented by her huſband, that not only he annually dedicated the day of her death to ſolemn mourning for it, but burying in her grave all thoughts of after marriage, he rejected all motions of any other match, continuing a conſtant widower till his death.

THE ſecond misfortune, that befel *Philaretus*, was his acquaintance with ſome children of his own age, whoſe ſtuttering habitude he ſo long counterfeited, that at laſt he contracted it; poſſibly a juſt judgment upon his deriſion, and turning the effects of God's anger into the ſubject matter of his ſport. Divers experiments, believed

lieved the probableſt means of cure, were tried with as much ſucceſsleſſneſs as diligence; ſo contagious and catching are men's faults, and ſo dangerous is the familiar commerce of thoſe condemnable cuſtoms, that being imitated but in jeſt, come to be learned and acquired in earneſt.

BUT to ſhow, that theſe afflictions made him not leſs the object of heaven's care, he much about this time eſcaped a danger, from which he owed his deliverance wholly to providence, being ſo far from contributing to it himſelf, that he did his endeavour to oppoſe it: for waiting on his father up to *Dublin*, there to expect the return of his eldeſt brother (then landed out of *England*, with his new wife, the earl of *Cumberland*'s heir) as they were to paſs over a brook, at that time ſuddenly by immoderate ſhowers ſwelled to a torrent, he was left alone in a coach, only with a foot-boy, where a gentleman of his father's very well horſed accidentally eſpying him, in ſpite of ſome others and his own unwillingneſs and reſiſtance, (they not believing his ſtay dangerous) carried him in arms over the rapid water, which proved ſo much beyond expectation both ſwift and deep, that horſes with their riders were violently hurried down the ſtream, which eaſily overturned the unloaded coach, the horſes (after by long ſtruggling they had broke their harneſs) with much ado ſaving themſelves by ſwimming.

As ſoon as his age made him capable of admitting inſtruction, his father by a Frenchman, and by one of his chaplains, had him taught both to write a fair hand, and to ſpeak French and Latin, in which, eſpecially the firſt, he proved no ill proficient, adding to a reaſonable forwardneſs in ſtudy a more than uſual inclination to it.

THIS ſtudiouſneſs obſerved in *Philaretus* endeared him very much unto his father, who uſed highly to commend him both for it and his veracity, of which latter he would often give him this teſtimony, that he never found him in a lie in all his lifetime. And indeed lying was a vice both ſo contrary to his nature, and ſo inconſiſtent with his principles, that as there was ſcarce any thing he more greedily deſired than to know the truth, ſo was there ſcarce any thing he more perfectly deteſted, than not to ſpeak it: which brings into my mind a fooliſh ſtory I have heard him jeered with by his ſiſter my lady *Ranelagh*, how ſhe having given ſtrict order to have a fruit-tree preſerved for his ſiſter-in-law, the lady *Dungarvan*, then big with child, he accidentally coming into the garden, and ignoring the prohibition, did eat half a ſcore of them, for which being chidden by his ſiſter *Ranelagh*, (for he was yet a child) and being told by way of aggravation, that he had eaten half a dozen plums, Nay truly, ſiſter, (anſwers he ſimply to her) I have eaten half a ſcore. So perfect an enemy was he to a lie, that he had rather accuſe himſelf of another fault, than be ſuſpected to be guilty of that. This trivial paſſage I have mentioned now, not that I think, that in itſelf it deſerves a relation, but becauſe as the ſun is ſeen beſt at his riſing and his ſetting, ſo men's native diſpoſitions are clearlieſt perceived, whilſt they are children, and when they are dying. And certainly theſe little ſudden accidents are the greateſt diſcoverers of mens true humours; for whilſt the inconſiderateneſs of the thing affords no temptation to diſſemble, and the ſuddenneſs of the time allows no leiſure to put diſguiſes on, men's diſpoſitions do appear in their true genuine ſhape, whereas moſt of thoſe actions, that are done before others, are ſo much done for others; I mean moſt ſolemn actions are ſo perſonated, that we may much more probably gueſs from thence, what men deſire to ſeem, than what they are; ſuch publick formal acts much rather being adjuſted to men's deſigns, than flowing from their inclinations.

Philaretus had now attained, and ſomewhat paſt the eighth year of his age, when his father (who ſupplied what he wanted in ſcholarſhip himſelf, by being both a paſſionate

passionate affector, and eminent patron of it) ambitious to improve his early studiousness, and considering, that great men's children breeding up at home tempts them to nicety, to pride, and idleness, and contributes much more to give them a good opinion of themselves, than to make them deserve it, resolves to send over *Philaretus* in the company of Mr. *F. B.* his elder brother, to be bred up at *Eton* college near *Windsor*, whose provost at that time was Sir *Henry Wotton*, a person, that was not only a fine gentleman himself, but very well skilled in the art of making others so, betwixt whom and the earl of *Corke* an ancient friendship had been constantly cultivated by reciprocal civilities. To him therefore the good old earl recommends *Philaretus*, who having laid a week at *Youghall* for a wind, when he first put to sea, was by a storm beaten back again, not only a taste, but an omen of his future fortune. But after eight days further stay, upon the second summons of a promising gale, they went aboard once more, and (though the Irish coasts were then sufficiently infested with Turkish gallies) having by the way touched at *Ilford-combe*, and *Minehead*, at last they happily arrived at *Bristol*.

Philaretus, in the company of his brother, after a short stay to repose and refresh themselves at *Bristol*, shaped his journey directly for *Eton* college, where a gentleman of his father's, sent to convey them thither, departing, recommended him to the especial care of Sir *Henry Wotton*, and left with him, partly to instruct, and partly to attend him, one *R C.* one that wanted neither vices, nor cunning to dissemble them; for though his primitive fault was only a dotage upon play, yet the excessive love of that goes seldom unattended with a train of criminal retainers; for fondness of gaming is the seducingest lure to ill company, and that the subtlest pander to the worst excesses. Wherefore our *Philaretus* deservedly reckoned it, both amongst the greatest and the unlikeliest deliverance he owed providence, that he was protected from the contagion of such precedents; for though the man wanted not a competency of parts, yet perverted abilities make men but like those wandering fires, philosophers call *ignes fatui*, whose light serves not to direct, but to seduce the credulous traveller, and allure him to follow them in their deviations. And it is very true, that during the minority of judgment, imitation is the regent in the soul, and those that are least capable of reason, are most swayed by example. A blind man will suffer himself to be led, though by a dog, or a child.

NOT long our *Philaretus* staid at school, ere his master, Mr. *Harrison*, taking notice of some aptness, and much willingness in him to learn, resolved to improve them both by all the gentlest ways of encouragement; for he would often dispense from his attendance at school at the accustomed hours, to instruct him privately and familiarly in his chamber. He would often as it were cloy him with fruit and sweetmeats, and those little dainties, that age is greedy of, that by preventing the want, he might lessen both his value and desire of them. He would sometimes give him unasked play-days, and oft bestow upon him such balls, and tops, and other implements of idleness, as he had taken away from others, that had unduly used them. He would sometimes commend others before him, to rouse his emulation, and oftentimes give him commendations before others, to engage his endeavours to deserve them. Not to be tedious, he was careful to instruct him in such an affable, kind, and gentle way, that he easily prevailed with him to consider studying, not so much as a duty of obedience to his superiors, but as the way to purchase for himself a most delightsome and invaluable good. In effect, he soon created in *Philaretus* so strong a passion to acquire knowledge, that what time he could spare from a scholar's task, which his retentive memory made him not find uneasy, he would usually employ so greedily in reading,

reading, that his master would sometimes be necessitated to force him out to play, on which, and upon study, he looked, as if their natures were inverted. But that, which he related to be the first occasion that made him so passionate a friend to reading, was the accidental perusal of *Quintus Curtius*, which first made him in love with other than pedantick books, and conjured up in him that unsatisfied appetite of knowledge, that is yet as greedy, as when it first was raised. In gratitude to this book, I have heard him hyperbolically say, that not only he owed more to *Quintus Curtius*, than *Alexander* did; but derived more advantage from the history of that great monarch's conquests, than ever he did from the conquests themselves.

WHILST our youth was thus busied about his studies, there happened to him an accident, that silence must not cover: for being one night gone to bed, somewhat early, whilst his brother was conversing with some company by the fire-side, without giving them the least warning or summons to take heed, a great part of the wall of their chamber, with the bed, chairs, books, and furniture of the next chamber over it, fell down, at unawares, upon their heads. His brother had his band torn about his neck, and his coat upon his back, and his chair crushed and broken under him; but by a lusty youth, then accidentally in the room, was snatched from out the ruins, by which *Philaretus* had, in all probability, been immediately oppressed, had not his bed been curtained by a watchful providence, which kept all heavy things from falling on it; but the dust the crumbled rubbish raised was so thick, that he might there have been stifled, had not he remembered to wrap his head in the sheet, which served him as a strainer, through which none but the purer air could find a passage. So sudden a danger, and hasty an escape, *Philaretus* would sometimes mention, with expressions both of gratitude and wonder. To which he would add the relation of divers other almost contemporary deliverances: of these, one was, that, being fallen from his horse, the beast ran over him, and trod so near his throat, as within less than two inches of it to make a hole in his band, which he long after reserved for a remembrancer. Another was, that riding through a town, upon a nag of his own, whose starting quality he never observed before, his horse, upon a sudden fright, rose bolt upright upon his hinder feet, and falling rudely backward with all his weight against a wall, had infallibly crushed his rider into pieces, if, by a strange instinct, he had not cast himself off at first, and was quit of it for a slight bruise. The last was, *Philaretus* being newly recovered of a flux, the doctor had prescribed him a refreshing drink: the fellow, that should administer it, instead of it, brings him a very strong vomit, prepared and intended for another. *Philaretus* was that morning visited by some of his school-fellows, who (as he was not ill beloved amongst them) presented him with some sweet-meats, which having eaten, when afterwards he would have eaten his breakfast, his stomach, whether out of squeamishness, or divination, forced him to render it again. To which lucky accident the physician ascribed his escape from the apothecary's error; for in the absence of those that tended him, his physick cast him into hideous torments, the true cause of which he never dreamed of, and remained long unconjectured, until the effects betrayed it; for after a long struggling, at last the drink wrought with such violence, that they feared that his life would be disgorged together with his potion. This accident made him long after apprehend more from the physicians, than the disease, and was possibly the occasion, that made him afterwards so inquisitively apply himself to the study of physick, that he might have the less need of them, that profess it. But *Philaretus* would not ascribe any of these rescues unto chance, but would be still industrious to perceive the hand of heaven in all these accidents; and indeed he would profess, that in the passages of his

his life, he had obſerved ſo gracious and ſo peculiar a conduct of providence, that he ſhould be equally blind and ungrateful, ſhould he not both diſcern and acknowledge it.

Philaretus having now for ſome two years been a conſtant reſident at *Eton* (if you except a few viſits, which, during the long vacations, he made his ſiſter my lady *Goring* at *Lewis* in *Suſſex)* when about *Eaſter* he was ſent for up to *London* to ſee his eldeſt brother the lord *Dungarvan*, where being viſited with a tertian ague, after the queen's and others doctors remedies had been ſucceſsleſsly aſſayed, at laſt he returned again to *Eton* to derive that health from a good air and diet, which phyſic could not give him. Here to divert his melancholy, they made him read the ſtate adventures of *Amadis de Gaule*, and other fabulous and wandering ſtories, which much more prejudiced him, by unſettling his thoughts, than they could have advantaged him, had they affected his recovery; for meeting in him with a reſtleſs fancy, then made more ſuſceptible of any impreſſions by an unemployed penſiveneſs, they accuſtomed his thoughts to ſuch a habitude of roving, that he has ſcarce ever been their quiet maſter ſince, but they would take all occaſions to ſteal away, and go a-gadding to objects then unſeaſonable and impertinent; ſo great an unhappineſs it is for perſons, that are born with ſuch buſy thoughts, not to have congruent objects propoſed to them at firſt. It is true, that long time after *Philaretus* did in a conſiderable meaſure fix his volatile fancy, and reclaim his thoughts, by the uſe of all thoſe expedients he thought likelieſt to fetter, or at leaſt to curb the roving wildneſs of his wandering thoughts. Amongſt all which the moſt effectual way he found to be the extraction of the ſquare and cube roots, and eſpecially thoſe more laborious operations of algebra, which both accuſtom and neceſſitate the mind to attention, by ſo entirely exacting the whole man, that the ſmalleſt diſtraction, or heedleſſneſs, conſtrains us to renew our trouble, and rebegin the operation. Six weeks had *Philaretus* been troubled with his ague, when he was freed from it by an accident, which is no ſlender inſtance of the force of imagination; for the phyſician having ſent him a purge, to give (as he ſaid) the fatal blow to the diſeaſe, our *Philaretus* had ſo perfect an averſion to all phyſic, and had newly aſſayed it ſo unſucceſsfully, that his complaints induced the maid ſervants of the houſe he lodged in (partly out of complaiſance to him, and partly out of a belief that phyſic did but exaſperate his diſeaſe) unknown to him, to pour out the potion, and fill the vial with ſyrup of ſtewed prunes, a liquor ſo reſembling it, that *Philaretus* (ſee the force of fancy!) ſwallowed it with the ſame reluctancy, and found the taſte as loathſome as if it had been the purge; but being after acquainted with the couſenage, whether it was that his ſickneſs (as having already reached its period) would have expired of itſelf, or that his mirth diſpatched it, I pretend not to determine; but certain it is, that from that hour to this, agues and he have ſtill been perfect ſtrangers, and he had much ado to refrain from laughter, when going to thank and reward the doctor for his recovery, he found it wholly aſcribed to the efficacy of the potion he had never ſwallowed, but in imagination.

HE had now ſerved well near half an apprenticeſhip at ſchool, when there arrived intelligence of his father's being landed in *England*, and gone to *Stalbridge*, a place in *Dorſetſhire* then newly purchaſed by him. Thither *Philaretus* accompanies his ſiſter the counteſs of *Kildare* to wait upon him. The good old earl welcomed him very kindly, for whether it were to the cuſtom of old people (as *Jacob* doted moſt on *Benjamin* and *Joſeph)* to give their eldeſt children the largeſt proportions of their fortunes, but the youngeſt the greateſt ſhare of their affections; to a likeneſs obſerved

in *Philaretus*, both to his father's body and his mind; or, as it seems most likely, to his never having lived with his father to an age that might much tempt him to run in debt, and take such other courses to provoke his dislike, as in his elder children he severely disrelished; to which if these causes the effect is to be ascribed, it is not my task to resolve, but certain it is, that from *Philaretus*'s birth, until his father's death, he ever continued very much his favourite. But after some weeks enjoyment of the summer diversions at *Stalbridge*, when his father removed to *London*, he left him by the way at *Eton* college, from whence at his return in the west, some few months after, he took him absolutely away, after *Philaretus* had spent in that school (then very much thronged with young nobility) not much beneath four years, in the last of which he forgot much of that *Latin* he had got, for he was so addicted to more solid parts of knowledge, that he hated the study of bare words naturally, as something that relished too much of pedantry to consort with his disposition and designs; so that by the change of his old courteous schoolmaster for a new rigid fellow, losing those encouragements that had formerly subdued his aversion to verbal studies, he quickly quitted his Terence and his grammar, to read in history their gallant acts, that were the glory of their own, and the wonder of our times. And indeed it is a much nobler ambition to learn to do things, that may deserve a room in history, than only to learn, how congruously to write such actions in the gown-men's language.

As soon as *Philaretus* was arrived at *Stalbridge*, his father assigned the care of teaching him to one Mr. *W. Douch*, then parson of that place, and one of his chaplains; and, to avoid the temptations to idleness, that home might afford, made him both lodge and diet where he was taught, though it were not distant from his father's house above twice a musket-shot. This old divine instructing our youth both with care and civility, soon brought him to renew his first acquaintance with the Roman tongue, and to improve it so far, that in that language he could readily enough express himself in prose, and began to be no dull proficient in the poetic strain; which latter he was naturally addicted to, resenting a great deal of delight in the conversation of the Muses, which nevertheless he ever since that time forbore to cultivate; not out of any dislike or undervaluing of poetry, but because in his travels having by discontinuance forgot much of the Latin tongue, he afterwards never could find time to redeem his losses by a serious study of the ancient poets. And then for English verses, he said, they could not be certain of a lasting applause, the changes of our language being so great and sudden, that the rarest poems within few years will pass for obsolete; and therefore he used to liken the writers in English verse to ladies, that have their pictures drawn with the clothes now worn, which, though at present never so rich, and never so much in fashion, within a few years hence will make them look like anticks. Yet did he at idle hours write some few verses, both in French and Latin, and many copies of amorous, merry, and devout ones in English; most of which, uncommunicated, the day he came of age, he sacrificed to Vulcan, with a design to make the rest perish by the same fate, when they came within his power, though amongst them were many serious copies, and one long one, amongst the rest, against wit profanely or wantonly employed; those two vices being ever perfectly detested by him in others, and religiously declined in all his writings.

ABOUT this time also *Philaretus* began to be taught some skill in the music, both of the voice, and hand; but the discouragement of a bad voice quickly persuaded him to desist. It was now the spring of the year, when news was brought to *Stalbridge* of the approach of his sister, the lady *Goring*, and in her company two of his brothers, the lords of *Kinelmeaky* and of *Brogbill*, then newly returned from their three years

years travels. In their company arrived one Mr. *Marcombes*, a French gentleman, who had been their governor, and behaved himself so handsomely in that relation, that the old earl removed *Philaretus* (his brother lying sick at a doctor's house) to his own house again, and intrusted his whole education with this gentleman. He was a man, whose garb, his mien, and outside, had very much of his nation, having been divers years a traveller and a soldier; he was well fashioned, and very well knew what belonged to a gentleman. His natural were much better than his acquired parts, though divers of the latter he possessed, though not in an eminent, yet in a very competent degree. Scholarship he wanted not, having in his greener years been a professed student in divinity; but he was much less read in books than men, and hated pedantry as much as any of the seven deadly sins. Thrifty he was extremely, and very skilful in the slights of thrift; but less out of avarice, than a just ambition, and not so much out of love to money, as a desire to live handsomely at last. His practical sentiments in divinity were most of them very sound; and if he were given to any vice himself, he was careful by sharply condemning it, to render it uninfectious, being industrious, whatsoever he were himself, to make his charges virtuous. Before company he was always very civil to his pupils, apt to eclipse their failings, and set off their good qualities to the best advantage; but in his private conversation he was cynically disposed, and a very nice critic both of words and men; which humour he used to exercise so freely with *Philaretus*, that at last he forced him to a very cautious and considerate way of expressing himself, which after turned to his no small advantage. The worst quality he had was his choler, to excesses of which he was excessively prone; and that being the only passion, to which *Philaretus* was much observed to be inclined, his desire to shun clashing with his governor, and his accustomedness to bear the sudden sallies of his impetuous humour, taught our youth so to subdue that passion in himself, that he was soon able to govern it habitually and with ease; the continuance of which conquest he much acknowledged to that passage of St. *James*, *For the wrath of man worketh not the righteousness of God.* James i. 20. And he was ever a strict observer of that precept of the apostle, *Let not the sun go down upon your wrath*; for continued anger turns easily to malice; which made him, upon occasion of this sentence of St. *Paul*, to say, that anger was like the Jewish manna, which might be wholesome for a day or two, but if it were kept long, it would breed worms, and corrupt. With this new governor our *Philaretus* spent the greatest part of the summer, partly in reading and interpreting the *Universal History*, written in Latin, and partly in familiar kind of conversation in French, which *Philaretus* found equally diverting and instructive, which was as well consonant to the humour of his tutor as his own.

ABOUT this time his eldest brother, the lord of *Dungarvan*, having at his own charges raised a gallant troop of horse for the king's service in the Scotch expedition, his father sent two other of his sons, *Kinelmeaky* and *Broghill*, to accompany him in that service, and designed *Philaretus* for the same employment; but the sickness of his next elder brother, Mr. *F.* whom he was to go along with in that voyage, defeated all our young man's greedy hopes. During his stay at *Stalbridge* all that summer, his father, to oblige him to be temperate, by freely giving him the opportunity to be otherwise, trusted him with the keys of all his gardens and orchards. And indeed *Philaretus* was little given to greediness, either in fruits or sweetmeats; in the latter he was almost abstemious, and in the former he was very moderate; so valuing such niceties and dainties, that though he enjoyed them with delight, he could want them without the least regret. During this pleasing season, when the intermission

sion of his studies allowed *Philaretus* leisure for recreations, he would very often steal away from all company, and spend four or five hours alone in the fields, and think at random, making his delighted imagination the busy scene, where some romance or other was daily acted; which, though imputed to his melancholy, was in effect but an usual excursion of his yet untamed habitude of roving, a custom (as his own experience often and sadly taught him) much more easily contracted, than destroyed.

TOWARDS the end of this summer, the kingdom having now obtained a seeming settlement by the king's pacification with the Scots, there arrived at *Stalbridge* Sir *Thomas Stafford*, gentleman usher to the queen, with his lady, to visit their old friend, the earl of *Cork*, with whom, ere they departed, they concluded a match betwixt his fourth son, Mr. *F. B.* and *E. K.* [*Killigrew*] daughter to my lady *S.* by Sir — *K.* and then a maid of honour, both young and handsome. To make his addresses to this lady, Mr. *F.* was sent (and *Philaretus* in his company) before up to *London*, whither within few weeks they were followed by the earl and his family, of which a great part lived at (the lady *Stafford*'s house) the *Savoy*; the rest (for his family was much encreased by the accession of his daughters, the countess of *Barrimore* and the lady *Ranelagh*, with their lords and children) were lodged in the adjacent houses, but took their meals in the *Savoy*, where the old earl kept so plentiful a house, that in months his accompts for bare house-keeping exceeded pounds.

NOT long after his arrival, *Philaretus*'s brother having been successful in his addresses to his mistress, was, in the presence of the king and queen, publickly married at court, with all that solemnity that usually attends matches with maids of honour. But to render this joy as short as it was great, *Philaretus* and his brother were within four days after commanded away for *France*, and after having kissed their majesty's hands, they took a differing farewel of all their friends; the bridegroom extremely afflicted to be so soon deprived of a joy, which he had tasted but just enough of to encrease his regrets, by the knowledge of what he was forced from; but *Philaretus* as much satisfied to see himself in a condition to content a curiosity, to which his inclinations did passionately addict him. With these differing resentments of their father's commands, accompanied by their governor, two French servants, and a lacquey of the same country, upon the end of *October*, 1638, they took post for *Rye* in *Sussex*, where the next day hiring a ship, though the sea were not very smooth, a prosperous puff of wind did safely by the next morning blow them into *France*.

AFTER a short refreshment at *Dieppe*, they travelled through *Normandy* to the chief city of it, *Rouën*; but by the way received advertisement of a robbery freshly committed in a wood, they must traverse by night; but judging the fear of being apprehended would deter the robbers from a sudden return to the same place, after so recent a crime, the company quietly continued on their journey to *Rouën*, and arrived safely at it; where, amongst other singularities, *Philaretus* took much notice of a great floating bridge, which rising and falling as the tide-water does, he used to resemble to the vain amorists of outward greatness, whose spirits resent all the floods and ebbs of that fortune it is built on. From *Rouën* they passed to *Paris*, and having spent some time in visiting that vast chaos of a city, they shaped their course for *Lyons*, where, after nine days unintermitted travel, they arrived, having by the way (besides divers considerable places) passed by the town of *Moulins* (here famed enough for the fine tweezes it supplies us with) a part of the French *Arcadia*, the pleasant *Pays de Forest*, where the marquis *d'Urfé* was pleased to lay the scene of the adventures and amours of that *Astrea*, with whom so many gallants are still in love, so long after both his and her decease: being also by the way usefully diverted by the company of two

two Polonian princes, who had as well a right unto that title by their virtue and their education, as their birth.

AFTER some stay at *Lyons* (a town of great resort, and no less trading, but fitter for the residence of merchants than of gentlemen) they traversed those lofty mountains, that formerly belonging to the duke of *Savoy*, were now by stipulation (in exchange of the marquisate of *Saluzzo*) devolved to the French king; and having by the way beheld that famous place, where the swiftest and one of the noblest rivers of *Europe*, the *Rhosne*, is so streightened betwixt two neighbour rocks, that it is no such large stride to stand on both his banks, the third day after their departure from *Lyons*, they arrived safely at *Geneva*, a little commonwealth, which their early embracing and constant profession of the reformed religion, together with that peculiar care of providence, in so long and so unlikely a preservation, has rendered very much the theme, not only of discourse, but some degree of wonder.

Philaretus's instructions commanding him a long stay in this place, his governor (having both a wife and children in the town) provided him lodgings and entertainment in his own house, and at set hours taught him both rhetoric and logic, whose elements (not the expositions) *Philaretus* wrote out with his own hand; though afterwards he esteemed both those arts, as they are vulgarly handled, not only unseasonably taught, but obnoxious to those inconveniences, and guilty of those defects, he does fully particularize in his *Essays*. After these slighter studies, he fell to learn the mathematics, and in a few months grew very well acquainted with the most useful part of arithmetic, geometry, with its subordinates, the doctrine of the sphere, that of the globe, and fortification; in all which being instructed by a person that had a greater regard for his scholar's proficiency, than the gains he might derive from the common tedious and dilatory way of teaching, he quickly grew so enamoured of those delightful studies, that they very often proved both his business and his diversion in his travels, and he afterwards improved his opportunities to the attainment of a more than ordinary skill in divers of them. He also frequently conversed with a voluminous, but excellent French book, called, *Le Monde*, which so judiciously informs its readers, both of the past and present condition of all those states that now possess our globe, and by a delectable and instructive variety not only satisfies men's curiosity, but so copiously supplies them with matter, both of curious and serious discourse, that he used to say of that book, that it was worth its title, which means, *The World*.

BUT to employ his body, as well as his mind, because *Philaretus*'s age was yet unripe for so rude and violent an exercise as the great horse, he spent some months in fencing, and ten or twelve in learning to dance, the former of which exercises he ever as much affected, as he contemned the latter. His recreations, during his stay at *Geneva*, were sometimes mall, tennis (a sport he ever passionately loved) and, above all, the reading of romances, whose perusal did not only extremely divert him, but (assisted by a total discontinuance of the English tongue) in a short time taught him a skill in French somewhat unusual to strangers. In effect, before he quitted France, he attained a readiness in the language of that country, which enabled him, when he made concealment his design, to pass for a native of it, both amongst them that were so, and amongst foreigners also; and in all his writings, whilst he was abroad, he still made use of the French tongue, not out of any intention to improve his knowledge in it, but because it was that he could express himself best in.

BUT during *Philaretus*'s residence at *Geneva*, there happened to him an accident, which he always used to mention as the considerablest of his whole life. To frame a

right

right apprehension of this, you must understand, that though his inclinations were ever virtuous, and his life free from scandal and inoffensive, yet had the piety he was master of already so diverted him from aspiring unto more, that Christ, who long had lain asleep in his conscience (as he once did in the ship) must now, as then, be waked by a storm. For at a time, which (being the very heat of summer) promised nothing less, about the dead of night, that adds most terror to such accidents, *Philaretus* was suddenly waked in a fright with such loud claps of thunder (which are oftentimes very terrible in those hot climes and seasons) that he thought the earth would owe an ague to the air, and every clap was both preceded and attended with flashes of lightning so frequent and so dazzling, that *Philaretus* began to imagine them the sallies of that fire that must consume the world. The long continuance of that dismal tempest, where the winds were so loud, as almost drowned the noise of the very thunder, and the showers so hideous, as almost quenched the lightning, ere it could reach his eyes, confirmed *Philaretus* in his apprehensions of the day of judgment's being at hand. Whereupon the consideration of his unpreparedness to welcome it, and the hideousness of being surprised by it in an unfit condition, made him resolve and vow, that if his fears were that night disappointed, all his further additions to his life should be more religiously and watchfully employed. The morning came, and a serener cloudless sky returned, when he ratified his determination so solemnly, that from that day he dated his conversion, renewing, now he was past danger, the vow he had made, whilst he believed himself to be in it; that though his fear was (and he blushed it was so) the occasion of his resolution of amendment, yet at least he might not owe his more deliberate consecration of himself to piety to any less noble motive, than that of its own excellence.

THUS had this happy storm an operation upon *Philaretus*, resembling that it had upon the ground; for the thunder did but terrify, and blasted not; but with it fell such kind and genial showers, as watered his parched and almost withered graces, and reviving their greenness, soon rendered them both flourishing and fruitful. And though his boiling youth did often very earnestly solicit to be employed in those culpable delights, that are useful in, and seem so proper for that season, and have repentance adjourned till old age; yet did its importunities meet ever with denials, *Philaretus* ever esteeming, that piety was to be embraced, not so much to gain heaven, as to serve God with. And I remember, that being once in company with a crew of mad young fellows, when one of them was saying to him, what a fine thing it were, if men could sin securely all their life-time, by being sure of leisure to repent upon their death-beds; *Philaretus* presently replied, that truly for his part he should not like sinning, though on those terms, and would not all that while deprive himself of the satisfaction of serving God, to enjoy so many years fruition of the world. In effect it is strange, that men should take it for an inducement to an action, that they are confident that they shall repent of it. But *Philaretus* himself having sufficiently discoursed that point of early piety, in the sixth treatise of his *Christian Gentleman*, I shall at present only add to the arguments you may find there alledged, that he used to say, that it was a kind of meanness in devotion, to consider the very joys of the other life more as a condition, than a recompense. But (as when in summer we take up our grass-horses into the stable, and give them store of oats, it is a sign that we mean to travel them) our *P.* soon after he had received this new strength, found a new weight to support; for spending some of the spring in a visit to *Chambery*, the chief town of *Savoy*; *Aix* famed for its baths; *Grenoble*, the head town of *Dauphin*', and residence of a parliament, his curiosity at last led him to those wild mountains,

where the first and chiefest of the Carthusian abbies does stand seated; where the devil taking advantage of that deep raving melancholy, so sad a place, his humour, and the strange stories and pictures he found there of *Bruno*, the father of that order, suggested such strange and hideous thoughts, and such distracting doubts of some of the fundamentals of Christianity, that, though his looks did little betray his thoughts, nothing but the forbiddenness of self-dispatch hindered his acting it. But after a tedious languishment of many months in this tedious perplexity, at last it pleased God, one day he had received the sacrament, to restore unto him the withdrawn sense of his favour. But though since then *Philaretus* ever looked upon these impious suggestions, rather as temptations to be suppressed, than doubts to be resolved; yet never after did these fleeting clouds cease now and then to darken the clearest serenity of his quiet, which made him often say, that injections of this nature were such a disease to his faith, as the tooth-ach is to the body; for though it be not mortal, it is very troublesome. And however, as all things work together to them that love God, *Philaretus* derived from this anxiety the advantage of groundedness in his religion: for the perplexity his doubts created obliged him, to remove them, to be seriously inquisitive of the truth of the very fundamentals of Christianity, and to hear what both Turks, and Jews, and the chief sects of Christians could alledge for their several opinions; that so, though he believed more than he could comprehend, he might not believe more than he could prove, and not owe the stedfastness of his faith to so poor a cause, as the ignorance of what might be objected against it. He said (speaking of those persons, that want not means to enquire, and abilities to judge) that it was not a greater happiness to inherit a good religion, than it was a fault to have it only by inheritance, and think it the best, because it is generally embraced, rather than embrace it, because we know it to be the best. That though we cannot always give a reason for what we believe, we should be ever able to give a reason why we believe it. That it is the greatest of follies to neglect any diligence, that may prevent the being mistaken, where it is the greatest of miseries to be deceived. That how dear soever things taken up on the score are sold, there is nothing worse taken up upon trust than religion, in which he deserves not to meet with the true one, that cares not to examine whether or no it be so.

AND now *Philaretus* having spent one and twenty months in *Geneva*, about the middle of September 1641 departed towards *Italy*, and having traversed *Swizerland*, and by the way seen *Lausanne* (an academy seated upon the great Genevan lake) *Zurich*, and *Soleurre*, the heads of Cantons wearing the same name, and wandered seven or eight days amongst those hideous mountains, where the *Rhosne* takes it source, and he saw the *Rhine* but a brook; he at length arrived at the *Valtollina*, a spacious valley walled round with the steep *Alpes*, but so delicious, and (especially in that season) so crowned with all that *Ceres* and *Bacchus* are able to present, that it deserved to be the motive, but not the stage of those late wars it has occasioned betwixt the rival crowns of *France* and *Spain*. There *Philaretus* had the curiosity to visit the place on which stood *Piur*, a pleasant little town, once esteemed for its deliciousness, but now much more and more meritedly famous for its ruin, which happened some two dozen of years since, by the sudden and unexpected fall of a neighbouring hill upon it, which struck the whole town so deep into the ground, that no after-search by digging has ever prevailed to reach it. Having visited the singularities of this earthly paradise, *Philaretus* and his company began to climb that mountain of the *Alpes*, which denominated from a town, that is seated upon its foot, is usually called, *La montagna di Morbegno*. The hill was eight miles in ascent, and double that

that number downwards. It was then free from ſnow; but all the neighbouring hills, where ſtore of cryſtal is digged, like perpetual penitents, do all the year wear white. Upon the top of this hill, which is entirely uninhabited, *Philaretus* had the pleaſure to ſee the clouds, which they paſſed through in their deſcent, beneath them darkening the middle of the mountain, while on the top they had a clear ſerenity. But notwithſtanding the fairneſs of the day, they ſpent it all in traverſing this hill, at the height of which they left the *Griſons* territory, and at the bottom entered a village belonging to that of the *Venetians*; but having paſſed through ſuch a purgatory as the *Alpes* to their Italian paradiſe, I cannot but ſuppoſe them ſomewhat weary, and ſo my pen obliged to let them and itſelf take ſome ſhort reſt.

Philaretus being thus entered into the vaſt and delicious plains of *Lombardy*, traverſed the greateſt part of that rich province, and having ſtayed the ſtomach of his curioſity with the obſervables of *Bergamo*, *Breſcia*, *Verona*, *Vincenza*, and *Padua* (a famous univerſity, but more peculiarly devoted to *Æſculapius* than *Minerva*'s arts) he gave it a full meal at *Venice*, where the great concourſe of foreign nations numerouſly reſorting thither for trade or nobler buſineſs, preſents the ſenſes with a no leſs pleaſing than conſtant variety. From *Venice* returning through *Padua*, and paſſing through *Bologna* and *Ferrara* (towns, whoſe names allow me to ſpare their characters) he at laſt arrived at *Florence*, with a determination (having diſpoſed of the horſes he rode on from *Geneva* thither) to paſs the winter there. *Florence* is a city, to which nature has not grudged a pleaſing ſituation, and in which architecture has been no niggard either of coſt or ſkill, but has ſo induſtriouſly and ſumptuouſly improved the advantages liberally conferred by nature, that both the ſeat and buildings of the town abundantly juſtify the title the *Italians* have given it of *Fair*. Here *Philaretus* ſpent much of his time in learning of his governor (who ſpake it perfectly) the Italian tongue, in which he quickly attained a native accent, and knowledge enough to underſtand both books and men; but to ſpeak and expreſs himſelf readily in that language was a ſkill he ever too little aſpired to acquire. The reſt of his ſpare hours he ſpent in reading the modern hiſtory in Italian, and the new paradoxes of the great ſtar-gazer *Galileo*, whoſe ingenious books, perhaps becauſe they could not be ſo otherwiſe, were confuted by a decree from *Rome*; his highneſs the Pope, it ſeems, preſuming, and that juſtly, that the infallibility of his chair extended equally to determine points in philoſophy as in religion, and loth to have the ſtability of that earth queſtioned, in which he had eſtabliſhed his kingdom. Whilſt *Philaretus* lived at *Florence*, this famous *Galileo* died * within a league of it, his memory being honoured with a celebrating epitaph, and a fair tomb erected for him at the public charges; but before his death being long grown blind, to certain friers (a tribe, whom for their vices and impoſtures he long had hated) that reproached him with his blindneſs, as a juſt puniſhment of heaven, incenſed for being ſo narrowly pried into by him, he anſwered, that he had the ſatisfaction of not being blind, till he had ſeen in heaven what never mortal eyes beheld before. But to return to *Philaretus*, the company of certain Jewiſh rabbins, who lodged under the ſame roof with him, gave him the opportunity of acquainting himſelf with divers of their arguments and tenets, and a riſe of further diſquiſitions in that point. When carnaval was come (the ſeaſon, when madneſs is ſo general in *Italy*, that lunacy does for that time loſe its name) he had the pleaſure to ſee the tilts maintained by the Great Duke's brothers, and to be

* He died January the 8th, 1641-2, N. S. See *General Dictionary*, Vol. V. p. 373. in the article of *Galileo*.

preſent

present at the gentlemen's balls. Nor did he sometimes scruple, in his governor's company, to visit the famousest *Bordellos*, whither resorting out of bare curiosity, he retained there an unblemished chastity, and still returned thence as honest as he went thither, professing, that he never found any such sermons against them, as they were against themselves; the impudent nakedness of vice clothing it with a deformity description cannot reach, and the worst of epithets cannot but flatter. But though *Philaretus* were no fuel for forbidden flames, he proved the object of unnatural ones; for being at that time in the flower of youth, and the cares of the world having not yet faded a complexion naturally fresh enough, as he was once unaccompanied diverting himself abroad, he was somewhat rudely pressed by the preposterous courtship of two friers, whose lust makes no distinction of sexes, but that which its preference of their own creates, and not without difficulty and danger forced a scape from those gowned Sodomites, whose goatish heats served not a little to arm *Philaretus* against such people's specious hypocrisy, and heightened and fortified in him an averseness for opinions, which now the religieux discredit as well as the religion.

Philaretus having thus spent the winter in *Florence*, towards the end of March began his journey to *Rome*; and having passed thorough and seen the singularities of *Sienna*, *Montefiascone*, and some other remarkable places in his passage, at the end of five days he safely arrived at that imperious theme of fame, which destinated to some kind or other of universal monarchy, is now no less considerable by its present superstition, than formerly by its victorious arms; the modern Popes bringing it as high a veneration as the ancient Cæsars, and the Barberine bees flying as far as did the Roman eagles. The more conveniently to see the numerous rarities of this universal city, *Philaretus*, to decline the distracting intrusions and importunities of English Jesuits, passed for a Frenchman, which neither his habit nor language much contradicted. Under this notion he delightfully payed his visits to what in *Rome* and the adjacent villages most deserved them, and amongst other curiosities and antiquities had the fortune to see the Pope at chapel, with the Cardinals, who severally appearing mighty princes, in that assembly looked like a company of common friers. Here *Philaretus* could not chuse but smile to see a young churchman, after the service ended, upon his knees carefully with his feet sweep into his handkerchief the dust his holiness's gouty feet had by treading on it consecrated, as if it had been some miraculous relic. Nor was *Philaretus* negligent to procure the Latin and Tuscan poems of this Pope, whose name *Urbanus* his actions did not belie, he having more of the gentleman in him, than his pontifical habit would seem to let him wear. A poet he was, and a * * * * *

* * * HE never found the Pope less valued than in *Rome*, nor his religion fiercelier disputed against than in *Italy*; and sometimes added, that he ceased to wonder, that the Pope should forbid the sight of *Rome* to protestants, since nothing could more confirm them in their religion.

Philaretus having in a short time surveyed the principal rarities of this proud mistress of the world, was unwillingly driven thence by his brother's disability to support the encreasing heats, which there prove often insupportable to strangers, the neighbouring country being very scorched, and barren, and uninhabited. Wherefore he took his way back towards *Florence*, by that delicious valley that ennobles *Perugia*, and passing by *Pistoia* came to *Florence*, where, after a short repose, they descended the river *Arno* into *Pisa*, and from thence to *Livorno*, where in a Felucca, with a good wind, they ventured, for expedition sake, some fifteen or sixteen miles into the sea, and coasting along the country, still near the shore for fear of sudden

storms, they each night lay in some town, drawing their boat ashore (which was not uneasy in regard of the inconsiderable tides of the Mediterranean there) and soon, though not without danger, reached proud *Genoa*. * * * * *

* * * * * THE next day *Philaretus* prosecuted his journey, and passing by *Monaco* (a very strong place, then newly betrayed by the prince to the French) and by *Mentone*, a little principality belonging to the same prince, and stopping a while at *Nizza*, a place extremely and meritoriously famous for that strength, which nature and art have emulously given it, by night they landed at *Antibe*, one of the towns of *France*, that most approaches *Italy*.

THE morning, that succeeded *Philaretus*'s arrival at *Antibe*, he left it to cross the country to *Marseilles*, but he was welcomed into *France*, by an accident, which was very hazardous, and might have proved tragical. During the whole time of his being a traveller, or resident in *Italy*, *Philaretus* had religiously adhered to his * * * *

* * * * ENGLISH gentlemen thinking it as much better as safer to take off their hats, than to venture their heads, complimented with the crucifix; but *Philaretus*, without the least act of superstition, though not without ill words, and worse menaces, ventured and past boldly thorough them all, as ever resolving, that the soul should not more transcend the body in its own value, than in his esteem. This danger thus happily escaped, *Philaretus* continues his way to *Marseilles*, where the third day he arrived, with intent there to expect bills of exchange promised to be then sent thither, to enable him to prosecute his future travels. His detention here was shortened by his visits of so excellent a harbour for gallies and small vessels. * * * Town every night like it assured with lock and key. Here *Philaretus* had the pleasure to see the French king's fleet of gallies put to sea, and about two thousand poor slaves tug at the oar to row them.

HERE unfortunately Mr. *Boyle*'s manuscript memoirs break off abruptly, which I shall endeavour to continue from such lights as his other papers afford me.

DURING his travels, he pursued his studies with great vigour; and his brother *Francis*, afterwards lord *Shannon*, used to say, that even then he would never lose any vacant time; for if they were upon the road, and walking down a hill, or in a rough way, he would read all the way; and when they came at night to their inn, he would still be studying till supper, and frequently propose such difficulties, as he met with in his reading, to his governor *(s)*.

IN *Italy* he read over the lives of the ancient philosophers with the utmost attention. The sect, which then struck him most, was that of the Stoics; and he tried his proficiency in their philosophy, by enduring a long fit of the tooth-ach with great unconcernedness *(t)*. At *Marseilles*, he and his brother *Shannon* were obliged to wait for bills of exchange, in order to prosecute their journey; but, instead of that supply, they received from their father, in May 1642, a sad relation of the general rebellion in *Ireland*, with this account, that it was with great difficulty he had procured for them two hundred and fifty pounds, to supply their expences in their return home. But this money being intrusted with one *Perkins*, a citizen of *London*, who was esteemed a man of considerable substance, to be sent them in bills of exchange, he proved unfaithful, so that they never received the least part of it. Being thus left destitute in a strange country, seven or eight hundred miles from home, they were by means of Mr. *Marcombes* their governor brought to *Geneva*, till supplies could be received to enable them to return; where, by reason of the dreadful confusion of affairs in *Ireland* and *England*, they fruitlessly waited for about two years; during all which

(s) Dr. *Dent*'s letter to Dr. *W. Wotton*, dated from *Westminster Abbey*, May 20, 1699.

(t) Mr. *Boyle*'s memorandums of his own life, dictated by himself to bishop *Burnet*.

which time having no money, either to discharge Mr. *Marcombes*, or to return homewards, they were obliged to live at a great expence, and to run in debt to him on that account. " At length, adds Mr. *Boyle (u)*, though I could not receive any supply at " all from my friends, yet being unable to subsist any longer, I was forced to remove " thence; and having upon his [Mr. *Marcombes*'s] credit made shift to take up some " slight jewels at a reasonable rate, we made sale of them from place to place, and " by their help, at last, by God's assistance, we got safe into *England* towards the " middle of the year 1644, where we found things in such confusion, that although " the manor of *Stalbridge* were, by my father's decease, descended unto me, yet it " was near four months before I could get thither."

(u) Letter to the steward of his manor of *Stalbridge* in *Dorsetshire*.

UPON his return to *England*, which was unexpected to all his relations, he went immediately to his beloved sister *Catharine*, viscountess *Ranelagh*, a lady remarkable for her uncommon genius and knowledge, in whose house he lodged four months and an half. It was by accident, that he found her out, but such an accident, as he frequently afterwards, with great thankfulness to the providence of God, used to ascribe a good part of his future happiness to. For had he not been so detained, he had gone into the army, where though there were, besides " the excellent king himself, " divers eminent divines, and many worthy persons of several ranks; yet the gene- " rality of those he would have been obliged to converse with, were very debauched, " and apt, as well as inclinable, to make others so *(x)*;" whereas by staying in *London* in a family that was strictly religious, his early years were seasoned with such principles of piety and virtue, as never forsook him during the rest of his life. He reaped also a collateral advantage by it, which was, that a sister-in-law of lady *Ranelagh*, who was with them in the house, and was wife of one of the principal members of the then house of commons, brought him into the acquaintance and friendship of some great men of that party, which was then growing, and soon after victorious; by whose means he got early protection for his English and Irish estates *(y)*. However it is probable from a letter of his to his brother lord *Broghill*, dated from *London*, August 25th 1645, which begins thus, " The necessities of my affairs " calling me away (according to the leave the parliament has given me) into *France*," that he went abroad again, possibly to settle his accounts with Mr. *Marcombes*, who, as is already observed, had advanced money, and engaged his credit for him. But he did not continue long out of *England*; for he was at *Cambridge* for a short time in December following. In March 1645-6, he retired to his manor at *Stalbridge*; where he resided for the most part till May 1650; and upon his arrival there wrote the following letter to his sister, the lady *Ranelagh*, which will shew his talent in the epistolary style, even at that early age.

(x) Papers of Mr. *Boyle*'s life, delivered by him to bishop *Burnet*.

(y) Ibid.

" SISTER,

" IF the busy idleness of receiving senseless visits (whose continuance, if otherwise " unavoidable, were capable, in my opinion, to justify the retiredness of an " hermit) had not so totally taken up my leisure, you should not so long have had a " reprieve from the importunity of my letters. But now at last, to make you amends " for my fault (if at least the amends itself be not a fault) I will present you with a " piece of a real romance in the story of my peregrination hither. The morning I " had the unhappiness to take my leave of you and my lady *Molkin*, I bid farewel " to the city, and began my journey upon a courser. Him I rid to dinner to *Egham*; " and at the end of the town, there it was my good fortune (as we are pleased to " miscall it) to overtake an express sent from the parliament to the general, making

" ceremonies with his horſe, whether of them two ſhould lead the way. I quickly " put an end to their difference, by making *Roger* take the honour upon him; and " we had not rid far, before we met with ſome carriers, that had lately left *Andover*, " who aſſured us, that the way was very ſecure; though preſently after we came to " diſcover, that thoſe youngſters would not have been very ſorry, that we had been " ſnapped by ſome of their party, and been made to take *Oxford* in our way to *Stal-* " *bridge*; for we were no ſooner come to *Bagſhot*, but we met a carrier coming the " way we were going, who aſſured us, that the cavaliers had eaſed them at *Baſing-* " *ſtoke* of all their ſuperfluous moveables, and were advanced as far as *Hartley-row*, " the very village that I had deſigned for that night's lodging. So believing, that " our quarters would be already taken up, we called a council of war to advise and " reſolve what to do. It was *Roger*'s opinion, to ſtay where we were, till the paſſage " were made ſecure (ſtrange, that ſo well-armed an head ſhould be fearful!) but the " meſſenger eaſily perſuaded us to go to *Farnham* (though both that and all the " country in the way were ſmartly alarmed) aſſuring *Roger*, that in caſe of danger he " could ſtrike into a way, out of the way, to bring us to the place we went to. As " we went along, we met divers little parties, with whom we exchanged fears, and " found, that the malignant humours, that were then abroad, had frighted the " country into a ſhaking ague, till we came to *Farnham*, which we found empty and " unguarded, all the townſmen being gone out to oppoſe the king's party, and chu- " ſing rather to have their houſes empty, than repleniſhed with ſuch gueſts, as other- " wiſe they were neceſſarily to expect. There invited by the coolneſs of the evening, " and the freſhneſs of the garden I was walking in, I almoſt loſt myſelf in meditat- " ing, how fooliſhly raſh were our controlments of, and repinings at the wiſe con- " trivances of that all-ſwaying providence, whoſe proceedings ſhould be as far above " our cenſures, as they are above our reach. How apt are we, upon the leaſt thwart- " ing of our deſigns, to murmur againſt providence, and in a pettiſh humour throw " the helve after the hatchet, *le manche apres la coignée*, and give ourſelves for gone, " when, had we had our will in the means, we ſhould have been fruſtrate in our ex- " pectation of the end! Whereas that ſuperlative wiſdom, that we grumble at for " thwarting us in the means, by croſſing us in the way, brings us the ſooner to our " journey's end. With divers contemplations upon this ſubject, I went to ſupper, and " thence to bed, not without ſome little fear of having our quarters beaten up by " the cavaliers that night; when lo! to ſecond my apprehenſions, about the dead of " my ſleep, and that night, I heard a thundering at the door, as if they meant to " fright it out of the hinges, and us out of our wits. I preſently leaped out of my " bed, in my ſtockings and clothes (my uſual night-poſture, when I travel); and " while *Roger* was lighting a candle, got my bilboa and other inſtruments from under " my pillow: whereupon *Roger* opening the door, ſaw it beſet with muſketeers, who " no ſooner ſaw us, but ſaid aloud, that we were not the men they looked for; and " being intreated to come into the chamber, refuſed it, and he, that brought them " thither, excuſed their troubling us, with as tranſcendent compliments as the brown " bill could afford. I wondered at their courteſy, till I knew, that it was the town- " conſtable, that, making a ſearch for ſome ſuſpicious perſons, and coming by my " chamber, that wanted a lock, either had a mind to make us take notice of ſo con- " ſiderable an officer, or no mind that we ſhould ſleep, whilſt our betters watched; " and for his not coming in, ſome accents of fear, that fell from him, made me " ſuſpect I was obliged for that to myſelf: and I remember, that juſt at the opening " of the door, he peeping in, eſpied me drawing a piſtol out of one of my holſters,

" which,

" which, I believe, made him ſo niggardly of his company. Well, away went my " gentleman in proſecution of his ſearch, and I even took my bows and arrows, and " went to ſleep. The next day we dined at *Wincheſter*, and ever and anon, by the " trembling paſſengers we met, were as nicely catechized concerning our ways, as if " we were to be elected in the number of the new lay elders. From thence we " reached *Saliſbury* that night, though before we came thither, we were fain to paſs " in the dark through a wood, where we had warning given us, that about an hun- " dred woodmen (we have got wild Engliſh too now) lay leiger, where theſe night- " birds uſed to exerciſe their charity in eaſing weary travellers of ſuch burthenſome " things as money and portmanteaus. There was nothing in all my journey vext me " ſo much as the gravity of my ſteed; for though he were none of the freeſt of his " legs, yet he kept my body in a reſtleſs agitation, which was none of the pleaſanteſt " in the world neither; and if now and then I did ſolicit his ſides, to perſuade him to " gallop (a dialect, that his feet were utterly unacquainted with) it ſhould be ſhort " itſelf, and both uſhered in and epilogized with ſo long and ſo jolting a trot, that " the trouble of it was far greater than the eaſe. At *Saliſbury* I overtook my trunks " I had ſent thither before; and the next morning took them along with me over the " plain; where when we had gone about half the way, we were ſuddenly environed " with a party of horſe (beyond whom we might diſcover a body of foot) who came " powdering ſo furiouſly upon us, that they ſcarce gave us leiſure to draw; but com- " ing nearer, and knowing the ſtate's meſſenger (as he called himſelf) they durſt not " meddle, neither with us, nor with my trunks, which they eyed though very loving- " ly; and had not we been there, would, I believe, have opened to ſearch for " malignant letters, ſuch as uſe to be about the king's picture in a yellow boy. The " foot we ſaw were poor preſſed countrymen, whom this party of horſe were ſent, " not to convey, but to guard. Amongſt them I ſaw one poor rogue, lacqueyed by " his wife, and carrying a child upon his ſhoulders. A pretty device, methinks, to " make thoſe, who have no goods, to fight for their wives and children! Good God! " that reaſonable creatures, that call themſelves Chriſtians too, ſhould delight in " ſuch an unnatural thing as war, where cruelty at leaſt becomes neceſſity, and un- " procured poverty becomes a crime; and a man with his whole family muſt be " ſubject to be unavoidably undone, becauſe the violence perhaps of thoſe very " ſoldiers that preſs him, had made him poor. At laſt on Saturday night I arrived, " God be praiſed, at *Stalbridge*, and found by experience the truth of that ſenſeleſs " proverb, the longeſt way about is the neareſt way home. And here the fair weather, " that had been my conſtant companion from *London* hither, as ſoon as it ſaw me " houſed, took leave of me; in whoſe abſence winter weather has always ſo fully and " uninterruptedly domineered, that we all ſuſpect the almanac-maker of a miſtake, " in ſetting down March inſtead of January. It confines me to my chamber, and is " ſo drooping, that it dulls me to all kinds of uſeful ſtudy, and (which is worſt of " all) it renders me obnoxious to theſe country viſits (or viſitations rather) which, " you know, uſe to ſupply with their length what they want in their goodneſs. As " ſoon as the weather will give me leave, I intend to take *Marſton Bigot* in my way " to *Briſtol*, to put ſome end or other to the buſineſs —— I am loaded with civil " language and fair promiſes, but I have always obſerved, that in the troopers " dictionary the pages are ſo cloſe and thick written with promiſes, that there is no " room left for ſuch a word as performance.

" MY

" MY *Ethics* * go very slowly on; neither have I been possibly able to do any " other business, save to make my brother's sixty trees bear him some golden fruit, " of which (though I did my uttermost endeavour to ripen it) I must gather but one " third at May-day next, the other at, &c.

" MY stay here, God willing, shall not be long, this country being generally in- " fected with three epidemical diseases (besides that old leiger sickness, the troop-flux) " namely the plague, which now begins to revive again at *Bristol* and *Yeovil* six miles " off, fits of the committee, and consumption of the purse; to which so violent ex- " pulsives, if so potent an attractive, as a letter from you, were but added, it would " both extremely sweeten the stay, and accelerate the departure of,

" My dearest sister,

Stalbridge, March 30, 1646.

" your most affectionate brother,

" and humble servant,

" ROBERT BOYLE."

DURING his retirement at *Stalbridge*, he applied himself with great vigour to his studies of various kinds, particularly those of natural philosophy and chymistry. Nor did he neglect to keep up a correspondence with his friends by letters, some of which are extant of so early a date as the year 1646, wherein he not only gives an account of his studies, but likewise discovers a great penetration, and a very solid judgment, concerning men and things, far above his age. And though he meddled as little as possible with politics, yet his interest, his good-nature, and his piety, would not suffer him to be an unconcerned spectator of those miseries, under which his country groaned; and in some of his remarks on men's presages concerning the consequences of the war, and their expectation of a settled peace, he passed a true judgement, founded on wise and solid reasons, and verified by the event. For however many of his friends flattered themselves with an happy issue of the war, he had all along different apprehensions of it. I shall insert therefore some extracts of the letters, according to these dates of them. The first was written to Mr. *Marcombes*, October 22, 1646, from *London*, where he then was.

" MONSIEUR,

" SINCE discountenance of the practice of your language has robbed me of that " little readiness your converse had taught me in it, I shall take the liberty to " make use of mine, which I know you understand equally with your own.

" IN my last I promised you a more full account of sundry particulars, I had then " the leisure but to touch at; and for my disengagement I shall tell you, that we are " in a very doubtful condition for the present, though in all probability a few days " will determine either our hopes or our fears. In *England* the great and uninter- " rupted successes have transcended as well their own hopes, as their opposers fears. " In *England* there is not one malignant garrison untaken, and in *Wales* but two or

* A treatise, which he was then composing on that subject, and still extant in his own hand-writing. This was one of his first books, and was begun before he was twenty years of age.

" three rocky places hold out for the King, and these too so inconsiderable, that they " more advantage their enemies forces, by keeping them from idleness, than they " are able to prejudice them by their opposition. The Scots being now to quit the " kingdom, the parliament had compounded with them for all their arrears, upon " whose payment they are to deliver up their garrisons, and retire into their own " country. The sum total, being in all 300,000l. is already agreed upon; but the " first payment is yet in debate. His majesty is still at *Newcastle*, both discontenting " and discontented; and the Scots will now, upon their departure out of *England*, " be forced some way or other to dispose of his person, which the houses have here " voted to remain at the disposition of both houses of parliament. The greater part " of men in these parts are pleased to flatter themselves with the hopes of a speedy " settlement of things; but for my part, that have always looked upon sin as the " chief incendiary of the war, and yet have by careful experience observed the war " to multiply and heighten those sins, to which it owes its being, as water and ice, " which by a reciprocal generation beget one another, I cannot without presumption " expect a recovery in that body, where the physic that should cure, but augments " the disease. And this opinion of mine is over and above grounded upon such po- " litic considerations, (though known to very few besides myself) that in this I must " even wish to have less reason on my side. Those that appear for the King in *Scot-* " *land*, are bodied in an army of above 8000 men; a power so formidable in that " country, that certainly, if by the Scots ill usage to his Majesty it be provoked, it " will be able, if not to conquer, at least to ruin that beggared kingdom.

" As for *Ireland*, the news of my lord of *Ormonde*'s peace with the Irish, toge- " ther with the articles upon which it was concluded, have, I am confident, reached " *Geneva* long ere this. But our latest intelligence out of those parts informs us, " that peace is very likely to produce its contrary amongst them that made it, not " only by dividing betwixt the Irish and themselves, for not only the Irish attempted, " though vainly, to surprise my lord of *Ormonde*, and cut off his party, whilst they " were in their quarters; but the best general, and most numerous army, together " with divers of their greatest towns, have positively declared against the peace; to " which the old English or catholic lords of the English pale, (so we call the coun- " ties about *Dublin*) with many others, have submitted; so that these two flints are " striking such sparks, as are like to kindle a fine bonefire for the English, if they " have the wit but to lay hold on the opportunity, and blow these private discontents " into a civil war. Now this relation, however it seem not to carry with it any great " probability, I am the more apt to believe, because it is credited by the knowledge " of the Irish interests; for the lords of the pale, though by manners and inclina- " tion Irish, yet being English by descent, do with reason suspect, that if they give " way to a total expulsion or extirpation of the English, their turn will come next " to drink out of the same cup; and therefore are very willing, by assenting to this " peace, to secure themselves from that fear, and from the manifest danger, that " threatened them from their nearness to *Dublin*, in the protestants hands, and the " most considerable place of the kingdom. On the other side, the mere natives " promised themselves for the general, by this rebellion, to exchange the throne of " *England* for St. *Peter*'s chair; or at least to shake off the *English* yoke for that of " some foreign prince of their own religion; and in particular the nobility had al- " ready devoured in their greedy hopes all the protestants estates; upon whose pre- " sumed accession they built strange and imaginary castles in the air. And the " clergy, the main firebrand of this rebellion, expected no less than to be reinstated

" into

" into their ancient poſſeſſions; ſo that both theſe latter parties being thus fruſtrated " of their hopes, endeavour to foment among the people (very fit tinder to catch " at ſuch a fire) a diſlike of the preſent peace, which the former oppoſe by their " authority, and the latter by thundering out excommunications againſt all thoſe " that do act; a courſe that has a ſtrange influence upon that kind of people, whoſe " ſuperſtition makes them believe a reality of force in thoſe ſcare-crow thunderbolts, " that derive all their power only from the people's weakneſs, and are terrible for " nothing but becauſe they are pleaſed to fear them.

" THIS day with kingly ſtate was buried the great earl of *Eſſex*, having 400 offi- " cers, not one ſo low as a captain, the houſe of peers, the houſe of commons, the " city, and the aſſembly of divines, for his mourners, and all the other parting com- " pliments of honour, that ever ſubject could aſpire unto. His ſickneſs was an " apoplexy, which did not long make him linger; and thus he, that had eſcaped ſo " many mutinies, at laſt periſhed by a mutiny of the humours. But I have uſually " obſerved, that in theſe great funeral ſolemnities, the pageantry of ſorrow has " eaten up the reality; and the care of the blaze diverted men from mourning. " Beſides theſe coſtly flatteries of the dead (with neglect of the poor, whom that " charge might keep alive) ſeem to endeavour to make them guilty of prodigality " in their very graves, whilſt it waſtes that upon a ſenſeleſs carcaſs, that is to it as " uſeleſs as it is needleſs; whereas it were much better for them to procure the " prayers of the living, than their admiration.

" MY lord of *Inchiquin*'s abſence from *Munſter*, the greater part of this ſummer, " by leaving my brother *Broghill* the ſole command of his army, gave him an oppor- " tunity to manifeſt to the world his gallantry, which he did with an unwonted ſuc- " ceſs; and that no leſs conſtant than it was great. But his own wants at home at " laſt reduced him to that ſtarving condition, that when he prayed for his daily bread, " his requeſt reached at leaſt as far as his expectation. He is now there ſoliciting for " ſupplies for diſtreſſed *Munſter*, which, though very liberally voted, are ſo ſlow in their " diſpatch, that many think they have juſt cauſe to apprehend, that the phyſic will " not get thither before the patient be dead. His actions have hereby gained him " a general eſteem; and he has (if he were not my brother, I durſt ſay deſervedly) " acquired the repute of none of the leaſt wits of the time. He and I within theſe " ten days intend, God willing, for the Weſt, in order to his journey to *Munſter*, " whither he is now to carry over, under his own command, a gallant brigade of " 4000 effective foot and 1500 horſe, for the parliament, in that province, till the " enſuing ſpring enable them to tranſport thither a more conſiderable power.

" THE preſbyterian government is at laſt ſettled (though I ſcarce think it will " prove long lived) after the great oppoſition of many, and to their no leſs diſlike; " though it ſeemed very high time unto others, that ſome eſtabliſhed and ſtrict diſci- " pline ſhould put a reſtraint upon the ſpreading impoſtures of the ſectaries, which have " made this diſtracted city their general rendezvous, which entertains at preſent no leſs " than 200 ſeveral opinions in point of religion, ſome digged out of thoſe graves, " where the condemning decrees of primitive councils had long ſince buried them; " others newly faſhioned in the forge of their own brains; but the moſt being new " editions of old errors, vented with ſome honourable title and modern diſguiſements; " ſo that certainly if the truth be any where to be found, it is here ſought ſo many ſeveral " ways, that one or other muſt needs light upon it. But others, that juſtly pretend " to a greater moderation, ſuſpect, that our dotage upon our own opinions makes us " miſtake many for impoſtures, that are but glimpſes and manifeſtations of obſcure

" or

" or formerly concealed truths, or at leaſt our own pride or ſelf-love makes us ag-
" gravate very venial errors into dangerous and damnable hereſies. The parliament
" is now upon an ordinance for the puniſhment of many of theſe ſuppoſed errors;
" but ſince their belief of their contrary truths is confeſſedly a work of divine reve-
" lation, why a man ſhould be hanged, becauſe it has not yet pleaſed God to give
" him his ſpirit, I confeſs, I am yet to underſtand. Certainly to think by a halter
" to let new light into the underſtanding, or by the tortures of the body to heal the
" errors of the mind, ſeems to me like the applying a plaſter to the heel, to cure a
" wound in the head; which doth not work upon the ſeat of the diſeaſe.

" MY brother *Broghill* continues very much your friend, and, I am confident, will
" be very ready upon occaſion to realize his profeſſions; and the like I dare boldly
" affirm of my ſiſter *Ranelagh*. An employment fit for you we cannot yet procure,
" becauſe all our nobility ſtands at a gaze, to ſee whether the iſſue of the treaties
" now in debate will be either peace or war; in either of which caſes it is probable,
" that a good many of them will make viſits to foreign climates.

" THE ſadneſs of your condition I very much reſent, and would offer you my aſ-
" ſiſtance to ſweeten it, if I did not think the proffer ſuperfluous; but truly I believe
" it would leſs afflict you, if you were a ſpectator of our miſeries here, where every
" day preſents us with much more unuſual diſpenſations of providence, where I my-
" ſelf have been fain to borrow money of ſervants, to lend it to men of above
" 10,000l. a year.

" I was yeſterday in company with our *Iriſh* St. *Auſtin*, the archbiſhop of *Armagh*;
" and having told him, that you unfolded his myſtery of *the Incarnation* in *French*,
" he ſeemed very willing, that you ſhould publiſh it, upon the aſſurance I gave him
" of the fidelity of its tranſlantion. * * * * * *

" AND now it is high time I ſhould give you ſome account both of myſelf and
" of my condition; which truly hath been chequered with a great deal of variety of
" fortune, and a great many viciſſitudes of plenty and want, danger and ſafety, ſick-
" neſs and health, trouble and eaſe; wherein I were guilty of an ingratitude great
" as the favour I have received, if I did not acknowledge a great deal of mercy in
" God's diſpenſation towards me; which truly hath been ſo kind, as oftentimes to
" work my good out of thoſe things I moſt feared the conſequences of, and changed
" thoſe very dangers, which were the object of my apprehenſion, into the motives
" of my joy. I was once a priſoner here upon ſome groundleſs ſuſpicions, but
" quickly got off with advantage. The roguery of *Tom. Murray* gave me a great
" deal of trouble to diſcover and prevent; but I thence reaped the benefit of making
" further diſcoveries into œconomical knowledge, than ever otherwiſe I ſhould have
" done. I turned him away laſt year, to let him know, that I could do my buſineſs
" very well without him; but now, having attained to a knowledge of my own ſmall
" fortune beyond the poſſibility of being cheated, I am likely to make uſe of him
" again, to ſhew my father's ſervants, that I wiſh no hurt to the man, but to the
" knave.

" I HAVE been forced to obſerve a very great caution, and exact evenneſs in my
" carriage, ſince I ſaw you laſt, it being abſolutely neceſſary for the preſervation of
" a perſon, whom the unfortunate ſituation of his fortune made obnoxious to the
" injuries of both parties, and the protection of neither. Beſides I have been forced
" to live at a very high rate, (conſidering the inconſiderableneſs of my income) and,
" to furniſh out theſe expences, part with a good ſhare of my land, partly to live

" here like a gentleman, and partly to perform all that I thought expedient in order " to my Irish estate, out of which I never yet received the worth of a farthing. * * *

" As for my studies, I have had the opportunity to prosecute them but by fits and " snatches, as my leisure and my occasions would give me leave. Divers little essays, " both in verse and prose, I have taken the pains to scribble upon several subjects; " some of the least bad of which I shall venture to send you over, as soon as my next " vacation spares me time to lick them into some less imperfect shape.

" THE *Ethics* hath been a study, wherein I have of late been very conversant, and " desirous to call them from the brain down into the breast, and from the school to " the house. I have endeavoured to make it not only a lanthorn, but a guide, in a " just, though a brief treatise, that I am writing of it; having already with much " trouble in some sixteen chapters travelled through the most difficult part of it, and " that wherein I saw others deficient, I believe I shall leave the rest to be compleated " by those, who enjoy more leisure.

" THE other humane studies I apply myself to, are natural philosophy, the me- " chanics, and husbandry, according to the principles of our new philosophical col- " lege, that values no knowledge, but as it hath a tendency to use. And therefore " I shall make it one of my suits to you, that you would take the pains to enquire " a little more thoroughly into the ways of husbandry, &c. practised in your parts; " and when you intend for *England*, to bring along with you what good receipts or " choice books of any of these subjects you can procure; which will make you ex- " tremely welcome to our *invisible college*, which I had now designed to give you a " description of, but a gentleman, whom I have been forced to keep talk with all " the while I was writing this, together with the fear of having too much already " trespassed upon your patience, call upon me to end your trouble and this letter " together."

HE was likewise at *London* in February 1646-7, whence he wrote the following letter, dated the 20th of that month, to Mr. *Francis Tallents*, then fellow of *Magdalen* college in *Cambridge*.

SIR,

" I SHOULD venture to apologize for my silence, if I thought it not less guilty " than meritorious; since to reprieve you from the importunity of my letter, I have " hitherto denied myself that happiness, that your civility makes me confident I might " have enjoyed by the receiving of yours. I have been every day these two months " upon visiting my own ruined cottage in the country; but it is such a labyrinth this " *London*, that all my diligence could never yet find the way out on't, and hath but " just now put me in a probability of leaving it within these two or three days. The " best on't is, that the corner-stones of the *invisible*, or (as they term themselves) " the *philosophical college*, do now and then honour me with their company, which " makes me as sorry for those pressing occasions that urge my departure, as I am at " other times angry with that solicitous idleness that I am necessitated to during my " stay; men of so capacious and searching spirits, that school-philosophy is but the " lowest region of their knowledge; and yet, though ambitious to lead the way to " any generous design, of so humble and teachable a genius, as they disdain not to " be directed to the meanest, so he can but plead reason for his opinion; persons " that endeavour to put narrow-mindedness out of countenance, by the practice of so

" extensive

" extenſive a charity, that it reaches unto every thing called man, and nothing leſs " than an univerſal good-will can content it. And indeed they are ſo apprehenſive " of the want of good employment, that they take the whole body of mankind for " their care.

" BUT leſt my ſeeming hyperbolical expreſſions ſhould more prejudice my repu- " tation than it is able any ways to advantage theirs, and I be thought a liar for " telling ſo much truth, I will conclude their praiſes with the recital of their chiefeſt " fault, which is very incident to almoſt all good things; and that is, that there is " not enough of them.

" FOR news, I believe you do not ignore, what a ſtream of ſucceſs the parliament, " ſince I had the honour to ſee you, has had. I will only now take the freedom to " tell you, that I am greatly afraid, moſt men flatter themſelves in their prognoſtica- " tions of peace, which are calculated rather to the meridian of their deſires, than to " that of their reaſon. And though I muſt confeſs, the traveller ſeems to be very " near the inn, yet I know not why the horſe may not ſtumble at the threſhold; for " I am ſomewhat unapt to perſuade myſelf, that the judgment will ceaſe, while the " the cauſe continues; but am rather very ready to apprehend, that while adverſity " makes ſome obſtinate, and others wanton, though the war perhaps may die, the " judgment will be kept alive. The pulpits were never more adorned with excellent " divines, than they now are here, but with ſo unſuitable a ſucceſs in many of the " people, that I can ſometimes think it no breach of charity to believe, that the " ſmall-pox has ſtricken inward, and many of them have but baniſhed their vices " from the body into the heart. For my part, the excellency of the miniſtry, ſince " waited on by ſuch an improficiency, increaſes my preſaging fears of the approach- " ing miſery of the people; for I ſhall eaſily be drawn to ſuſpect that horſe to be " very ſick, that thrives not in ſo plentiful a paſture. And truly, methinks, it is but " a very ſad ſymptom, when the phyſic augments the diſeaſe. For matter of ſects, " it ſeems, that moſt of thoſe at *Amſterdam* have been returned us over by bill of ex- " change, which our Engliſh ſearchers have been ſo induſtrious to improve, that there " are few days paſs here, that may not juſtly be accuſed of the brewing or broach- " ing of ſome new opinion. Nay, ſome are ſo ſtudiouſly changeling in that particular, " they eſteem an opinion as a diurnal, after a day or two ſcarce worth the keeping. " If any man have loſt his religion, let him repair to *London*, and I'll warrant him, " he ſhall find it: I had almoſt ſaid too, and if any man has a religion, let him but " come hither now, and he ſhall go near to loſe it. Pray God, it fare not with re- " ligion amongſt thoſe novelties, as it does ſometimes with a great commander, when " he is taken priſoner by a company of common ſoldiers, who every one tugging to " have him for himſelf, at laſt pull him to pieces, and ſo each get a limb, but none " enjoys him whole. For my part, I ſhall always pray to God to give us *the unity* " *of the ſpirit in the bond of peace*, and deſire you to believe, that, amongſt all the " apoſtaſies of the time, I ſhall be the leaſt capable of being ſeduced by that that " may oppoſe my being and continuing ſo,

" Your moſt affectionate friend and faithful ſervant,

" ROBERT BOYLE."

MR. *Tallents*, to whom this letter was written, had been, while he was an undergraduate, ſub-tutor to ſeveral ſons of the earl of *Suffolk* (one of whoſe daughters was

married to lord *Broghill)* being removed from *Peter* house to *Magdalen* college for that purpose; and about the year 1642 travelled abroad with them as their tutor. After his return, he became senior fellow and president or vice-master of his college; and having spent near twenty years in the university, was settled of St. *Mary*'s in *Shrewsbury*, whence he was ejected for nonconformity in 1662. In the year 1670 he went a second time as tutor to two young gentlemen, Mr. *Boscawen* and Mr. *Hamden*. In the last part of his life he was pastor to a congregation of protestant dissenters at *Shrewsbury*, where he died April 11th, 1708, in the 89th year of his age. He was author of the Chronological Tables[a].

MR. *Boyle* returned to *Stalbridge* soon after his writing the abovementioned letter to Mr. *Tallents*, as appears from one to his sister, the lady *Ranelagh*, dated there Feb. 27, 1646-7, in which he writes as follows:

" * * * As to my *Dublin* business, I must expect from your speedy information " (I now beg of you) of their names, to whom the parliament has given power to " oblige me there, directions both how to frame, and to whom to address my request. " I must acknowledge to you, amongst God's mercies of the first magnitude, both " my lord of *Ormonde*'s unexpected coming in, and the parliament's less expected dis- " patch; though both very consonant to the wonted method of that gracious provi- " dence, that we find then oftentimes the nearest for our rescue, when that is furthest " from our expectations. For my part, I am bold to believe, had we no other ar- " guments to prove a providence, yet the strange revolutions, of which in so short " a time our ruined country has been the unhappy scene, were more than sufficient to " demonstrate that truth, to which alone I shall allow a greater infallibility, than to " that other most certain one of my being,

" Sister, your most truly affectionate

" brother and humblest servant,

" ROBERT BOYLE."

HIS next letter, dated from the same place, March 6, 1646-7, shews, that he was then entering upon his chemical studies.

" * * * THAT great earthen furnace, says he, whose conveying hither has taken " up so much of my care, and concerning which I made bold very lately to trouble " you, since I last did so, has been brought to my hands crumbled into as many " pieces, as we into sects; and all the fine experiments, and castles in the air, I had " built upon its safe arrival, have felt the fate of their foundation. Well, I see I " am not designed to the finding out the philosophers stone, I have been so unlucky " in my first attempts in chemistry. My limbecks, recipients, and other glasses have " escaped indeed the misfortune of their incendiary, but are now, through the mis- " carriage of that grand implement of *Vulcan*, as useless to me, as good parts to salva- " tion without the fire of zeal. Seriously, Madam, after all the pains I have taken, and " the precautions I have used, to prevent this furnace the disaster of its predeces- " sors, to have it transported a thousand miles by land, that I may after all this re-

[a] See his character by Mr. *Matthew Henry* subjoined to his *Funeral Sermon*.

" ceive

" ceive it broken, is a defeat, that nothing could recompense, but that rare lesson it " teaches me, how brittle that happiness is, that we build upon earth."

His acquaintance with Mr. *Samuel Hartlib* began very early. The father of that gentleman was a very considerable merchant in *Poland*, but obliged, on account of his attachment to the protestant religion to retire to *Elbing* in *Prussia*. His two first wives were Polish ladies, of noble extraction, and his third, the mother of Mr. *Samuel Hartlib*, had two sisters, both of whom were very honourably married, one to Mr. *Clarke*, the son of a lord mayor of *London*, afterwards to Sir *Richard Smith*, a privy counsellor, and at last to Sir *Edward Savage*; and the other to Mr. *Peake*, a younger brother of a good family [a]. Mr. *Hartlib*, Mr. *Boyle*'s friend, came to *England* about the year 1630, and soon distinguished himself by his great zeal for the improvement of natural knowledge, and making it useful to human life. For which purpose, besides what notices he could procure at home, he entertained a constant correspondence with the virtuosi in most other parts of *Europe*; and *Milton*, in his *Tractate of Education*, which he addressed to him, speaks of him, as " a person sent hither by some " good providence from a far country, to be the occasion and the incitement of great " good to this island." He observes likewise himself, in his letter to Dr. *Worthington* abovecited, that " as long as he had lived in *England*, he had spent yearly out of his " own betwixt three and four hundred pounds sterling. And when, says he, I was " brought to publick allowance, I have had from the parliaments and councils of " state a pension of three hundred pounds sterling a year, which as freely I have " spent for their service and the good of many." But notwithstanding the eminent advantages, which the public had reaped from his writings, and the assistances he had given to those of other men, he was reduced, after the Restoration, to very necessitous circumstances, as appears from the letters of Dr. *Worthington* to him, published at the end of that divine's *Miscellanies*, and from a petition [b] of Mr. *Hartlib* to the house of commons, that " he might find from their goodness and bounty some relief " in his distressed condition, by being freed from his debts, and put in a capacity to " continue his service to the public, to advance in his generation the best objects for " the use of mankind in all kinds."

THE first letter I find of Mr. *Boyle* to him was dated from *Stalbridge*, March 19, 1646-7.

" Dear Mr. *Hartlib*,

" I NEED a great deal of rhetoric to express to you, how great a satisfaction I " received in the favour of your letter, both for the sake of the theme, and more " for that of the author. But my contentment was greatly qualified by the miscar- " riage of the general writing (which should be stiled the Universal Truchman, or " General Interpreter, if I were to be godfather) you were pleased to send me; for " the commendations you give it are too great not to make me very sensible of its " loss. If the design of *the Real Character* [c] take effect, it will in good part make " amends to mankind for what their pride lost them at the tower of *Babel*. And

[a] Mr. *Samuel Hartlib*'s letter to Dr. *John Worthington*, Aug. 3, 1660, communicated by Mr. *John Ward*, professor of rhetoric in *Gresham* college.

[b] Communicated by Mr. *Ward*.

[c] This seems to relate to an excellent design, which was afterwards executed by Dr. *John Wilkins*, and published at *London*, 1668, in folio, under the title of *An Essay towards a Real Character, and a Philosophical Language*.

" truly,

" truly, since our arithmetical characters are understood by all the nations of *Europe* " the same way, though every several people express that comprehension with its own " particular language, I conceive no impossibility, that opposes the doing that in " words, that we see already done in numbers. As for the pneumatical engine, that " I use to call a wind-gun, which you mention in your letter as presented to the " king, and forbidden by him to have any companions, sure the artist, that received " the command, was more ingenious than obedient; for I remember very well to " have seen one of them not exceeding in bigness, nor differing much in shape from " an ordinary carabine, which being charged by the sole impression of the air, would, " by violence of the contracted Boreas, send forth a leaden bullet, just the caliber, " with force to kill a man at twenty-five or thirty paces distance from him. This " wind-gun I saw both charged and discharged; and now it comes into my mind, I " read, not long since, in a late mechanical treatise of the excellent *Mersennus*, both " the construction and the use of this engine; and amongst the uses one, whose strata- " gem obliged me to take of it particular notice; and it was, how by the help of this " instrument to discover the weight of the air; which, for all the prattling of our " book-philosophers, we must believe to be both heavy and ponderable, if we will " not refuse belief to our senses. Your *Imago Societatis*, and your *Dextera Amoris*, " I have great longings to peruse; and though with a deep sense of my insufficiency, " I shall very freely express my obedience in delivering the opinion of

" Your humble servant,

" ROBERT BOYLE."

THE next letter extant to that gentleman is of the 8th of April, 1647.

" Dear Mr. *Hartlib*,

" I HOPE I shall need none other apologies for my last week's silence, than my " absence, when your letters arrived here. Your *Imago Societatis* with a great " deal of delight I have perused, but must beg some leisure to acquaint you with my " opinion of it, which now were almost impossible for me to do, I having already " presented it to a person of quality, with whom, if it take suitably to my wishes, it " may thence have no obscure influence upon the public good, concerning which I " have lately traced a little dialogue in my thoughts, which my unceasing domestic " distractions will by no means as yet permit me to blot paper with. The epistle " prefixed to the *Imago* is both pithy and to the purpose. And truly I am extremely " glad to see a person of Mr. *Hall*'s years employ, in attempts of this nature, that " youth, which the most of those, that are as little indebted to time as he, think too " good for their Maker, though they think it not too good to be squandered away. " *Campanella*'s *Civitas Solis* [a], and that same *Respublica Christianopolitana*, which he " mentions, will both of them deserve to be taught in our language. Of the *Utopia* " he is modelling, though I cannot judge, before it sees the light, yet my expecta- " tions will be none of the smallest, if I proportion them to the ingenuity of the " author. And for the Divine Emblems, that he makes us hope for, I must reserve " my sense of them for their perusal, since the opinions I embrace, both about the

[a] This piece contains the idea of a commonwealth, after the manner of Sir *Thomas More*'s *Utopia*, and is printed with *Campanella*'s *Realis Philosophiæ Epilegisticæ partes quatuor*, at *Francfort*, 1623, in 4to.

" nature

" nature and the teaching of virtue, will doubtless appear as paradoxical to others, " as they seem probable to me. For those designs of Mr. *Hall*'s Tutor, which you " have so appropriatingly christened, I believe the requests of the excommunicated " petition now on foot in the army will scarce receive a slower satisfaction; for the " particulars, in which he requires it, do not only ask a profound knowledge and " solid judgment (qualities of themselves not very epidemical) but likewise a leisured " and a great multiplicity of reading, and so an intimacy as well with authors as " with things; qualities, that in this stirring and necessitous age (where men are " forced to court don *Plutus* and my lady *Fortune* with more assiduity than the Muses) " make very unfrequent matches in the self-same person: besides that the dissenting " opinions of the *Ptolemeans*, the *Tychonians*, the *Copernicans*, (to which I was once " very much inclined) and the other novelists, are both so irreconcilable among them- " selves, and leave a man so little latitude of neutrality, that it will be perhaps but " one remove from impossible, precisely to declare, what has been hitherto both per- " fectly demonstrated and confessedly on all sides; the one taking that (as I have " known by experience, when I studied that problem of the earth's peregrination) " for an undeniable demonstration, which the other will either absolutely reject as a " paralogism, or at least call in question as no more than a bare probability.

" YOUR *Common Writing* (for which you have my humble thanks) is at last come " safe to my hands; but my occasions have not yet allowed the leisure to fix my " thoughts upon it: only if the dictionary (whose edition, had my wishes the power " to swiften it, should be very sudden) do not over-swell and disease it of a tympany, " methinks the bulk of the grammar is very reasonable in reference to what the title " promises, which I was well pleased to see so, apprehending (nor are my fears yet " entirely suppressed) lest that this way of saving the labour of learning a language " should prove like a new device, I have lately seen, to perform all the operations " of arithmetic by the help of an instrument, where I found it much more difficult " to learn the uses of the instrument, than the rules of the art.

" THAT in my last I took no notice of Mr. *Dury*'s excellent discourse of teaching " logic, was not at all my neglect, but the treacherousness of my memory; for which " meaning to apologise to the author himself, I shall for this time add no more to " your trouble, but an humble request to assure him, that I am his, and yourself, " that I am

" Your most really affectionate friend and humble servant,

" ROBERT BOYLE."

THE letter, which he wrote on the 3d of *May*, 1647, to Mr. *John Dury*, famous for his attempts to reconcile the Lutherans and Calvinists, will shew Mr. *Boyle*'s moderation, at that time, with respect to differences in religious opinions.

" IT has been long, says he, as well my wonder, as my grief, to see such compa- " ratively petty differences in judgment make such wide breaches and vast divi- " sions in affection. It is strange, that men should rather be quarrelling for a few " trifling opinions, wherein they dissent, than to embrace one another for those many " fundamental truths, wherein they agree. For my own part, in some two or three " and forty months, that I spent in the very town of *Geneva*, as I never found that " people discontented with their own church-government (the gallingness of whose " yoke is the grand scare-crow that frights us here) so could I never observe in it

" any ſuch traſcendent excellency, as could oblige me either to bolt heaven againſt, " or open *Newgate* for all thoſe, that believe they may be ſaved under another. " Wherefore I muſt confeſs, it would be extremely my ſatisfaction, if I could ſee, " by God's bleſſing, your pious endeavours of twiſting our froward parties into a " moderate and ſatisfactory reconcilement, as ſucceſsful, as I am confident they will " be prudent and unwearied. As for our upſtart ſectaries (muſhrooms of the laſt " night's ſpringing up) the worſt part of them, if not exaſperated by, inſtead of " lighting them into the right way with the candle, flinging the candleſtick at their " heads, like *Jonah*'s gourd, ſmitten at the root with the worm of their irrationality, " will be as ſudden in their decay, as they were haſty in their growth; and indeed " perhaps the ſafeſt way to deſtroy them is rather to let them die, than attempt to " kill them."

ON the 8th of the ſame month he wrote the following letter to Mr. *Hartlib*.

" Dear Mr. *Hartlib*,

" IT was very needleſs in your laſt to make apologies for the glad parliamentary " news you began your letter with; for beſides that its goodneſs authoriſes its na- " ture, and were able to prefer ſo pleaſing a diſobedience to the moſt exact compli- " ance with my deſires; beſides this, I ſay, you intereſt yourſelf ſo much in the " *Inviſible College*, and that whole ſociety is ſo highly concerned in all the accidents of " your life, that you can ſend me no intelligence of your own affairs, that does not " (at leaſt rationally) aſſume the nature of *Utopian*. And truly, Sir, for my parti- " cular, had you been to coin and ſhape news, not ſo much to inform, as to delight " me, you could ſcarce have made choice of any, that were more welcome, either to " my wiſhes for their own particular ſatisfaction, or for thoſe that I dedicate to the " good of the public, which can acknowledge your merits with no advantage, that has " not a direct tendency to its own, and which, by the higheſt expreſſions of gratitude " for your ſervice to it, does but enable your zeal to multiply and continue them. " The phraſes of the ordinances (which theſe alone of yours have brought me into " charity with again) were indeed extremely civil in reſpect of thoſe that framed " them; and yet but barely juſt in regard of him for whom they were deſigned. " Certainly the taking notice of, and countenancing men of rare induſtry and publick " ſpirit, is a piece of policy as vaſtly advantageous to all ſtates, as it is ruinouſly ne- " glected by the moſt. And therefore we may evidently obſerve thoſe common- " wealths (as the *Hollanders* and the *Venetian*) to be the moſt happy and the moſt " flouriſhing, where ingenuity is courted with the greateſt encouragements. Mr. " *Hall*'s unmerited elogium of me I muſt in juſtice aſcribe rather to his civility, than " to his opinion; to the former of which I am alſo redevable for a very handſome " complimental letter, he was lately pleaſed to honour me with, to which I ſhall re- " queſt a ſpeedy conveyance of the incloſed (though unſuitable) anſwer, and a belief, " that I liſt it not amongſt the leaſt of your favours, to procure me the acquaintance " of a perſon, that treading antipodes to the ſtrain of his contemporaries, has Sep- " tember in his judgment, whilſt we can ſcarce find April upon his chin.

" MY ſenſe of his propoſitions concerning the College I muſt neceſſarily ſuſpend, " till a more exquiſite information of the particulars of his whole deſign. Only by " the by I ſhall take the freedom to tell you, that though I eſteem Mr. *Hall* very " moderate in the point of pecuniary duties, you can ſcarce be too tender in taſking " young collegiates as to the duties of the brain, ſince they being all of them to be " perſons

" persons of quality and voluntiers, will hardly support with alacrity any thing that " savours of constraint; besides that the gallantry and nobleness of their own prin- " ciples will carry them on unimposedly to do much more, than your strictest con- " stitutions can reasonably enjoin them. The expedients you propose to Mr. *Hall*'s " tutor are not at all unlikely to take; and the applications you counsel him to make " to those three famous mathematicians, can promise a great deal of probability for " their success: especially *Gassendus*, a great favourite of mine, I take to be a very " profound mathematician, as well as an excellent astronomer, and one that has " collected a very ample treasury of numerous and accurate observations of all that " belongs to the abstruse science of those sublimer bodies. I find you very happy, " or rather very judicious, in the nominating of the persons, Mr. *Hall*'s tutor is by " you addressed to; and am confident, as well as you, that those elevated spirits will " not prove half so costive and so pedantical, as the great scholarians of our colleges, " whom yet I am apt enough to pardon, in consideration of the usefulness (for the " most part) of the knowledge they conceal, which perhaps being admired but as " long as kept in a mystery, an imparting discovery would depreciate.

" THE rise you have now to resume your former correspondencies with the great " *Mersennus*, I hope you will greedily embrace, he being a man truly incomparable " in his own way, and the mechanics he transcends in as greatly beneficial as little " understood. The Englishing of, and additions to *Oughtred*'s *Clavis Mathe-* " *matica* does much content me, I having formerly spent much study on the original " of that algebra, which I have long since esteemed a much more instructive way of " logic, than that of *Aristotle*. No body has yet been charitable enough to send me " either the long desired *Office of Address*, or Mr. *Dury*'s wished for discourse con- " cerning Accommodation, though my longings for their sight have been very suit- " able to the contentments I expect from their perusal. I have written a long letter " to Mr. *Dury*, by the same post, that is to deliver you this; and it shall not be the " neglect of my improving my rhetoric to the uttermost, that shall impede my pre- " vailing with him, by exemplifying his rules, to clothe with flesh and skin his ex- " cellent skeleton of the Art of Reasoning.

" FOR your bedfellow's receipt for the stone (which certainly wants a parallel, if " it be not more easy than effectual) I beseech you to return her (together with the " present of my humble service) most humble thanks, which I mean very shortly, " God willing, to pay you in an epistle I have drawn up to persuade men to commu- " nicate all those successful receipts, that relate either to the preservation or recovery " of our health; to which (if you will pardon me a clinch) I shall add, as to the " disease last named (so cruel in its tortures, and so fatal in its catastrophe) that they " must have their hearts more hard than a very stone, that can refuse a sanative remedy " for the stone.

" As for me, during my confinement to this melancholy solitude, I often divert " myself at leisure moments in trying such experiments, as the unfurnishedness of the " place, and the present distractedness of my mind, will permit me; which when " once my vacant intervals of time will give me leave to blot paper with, and make " some short discourses and reflections upon, you may (with all the services you shall " be pleased to command their author) confidently expect from,

" Sir, your most affectionate friend and humble servant,

" ROBERT BOYLE."

THE *Invisible College*, which Mr. *Boyle* speaks of in this letter, as well as in those to Mr. *Marcomes* and Mr. *Tallents*, probably refers to that assembly of learned and curious gentlemen, who, after the breaking out of the civil wars, in order to divert themselves from those melancholy scenes, applied themselves to experimental inquiries, and the study of nature, which was then called the new philosophy, and at length gave birth to the Royal Society. This assembly Dr. *Wallis*, who was one of its members, gives the following account of*. "About the year 1645 (says he) "while I lived in *London*, I had the opportunity to be acquainted with divers worthy "persons, inquisitive into natural philosophy and other parts of human learning, and "particularly of what hath been called the new or experimental philosophy. We "did by agreement, divers of us, meet weekly on a certain day, to treat and dis-"course of such affairs. Of which number were Dr. *John Wilkins*, afterwards bishop "of *Chester*, Dr. *Jonathan Goddard*, Dr. *George Ent*, Dr. *Glisson*, Dr. *Merret*, doctors "in physic; Mr. *Samuel Foster*, then professor of astronomy at *Gresham* college, Mr. "*Theodore Haak*, a German of the Palatinate, and then resident in *London*, who, I "think, gave the first occasion, and first suggested those meetings, and many others. "These meetings we held sometimes at Dr. *Goddard*'s lodging in *Wood-street* (or some "convenient place near) on occasion of his keeping an operator in his house, for "grinding glasses for telescopes and microscopes; and sometimes at a convenient "place in *Cheapside*; sometimes at *Gresham* college, or some place near adjoining. "Our business was (precluding matters of theology and state-affairs) to discourse and "consider of philosophical inquiries, and such as related thereunto, as physic, ana-"tomy, geometry, astronomy, navigation, statics, magnetics, chemics, mechanics, "and natural experiments, with the state of these studies, as then cultivated at home "and abroad. About the year 1648, 1649, some of us being removed to *Oxford*, "first Dr. *Wilkins*, then I, and soon after Dr. *Goddard*, our company divided. Those "in *London* continued to meet there, as before, and we with them, when we had "occasion to be there. And those of us at *Oxford*, with Dr. *Ward*, since bishop of "*Salisbury*, Dr. *Ralph Bathurst*, now president of *Trinity* college in *Oxford*, Dr. "*Petty*, since Sir *William Petty*, Dr. *Willis*, then an eminent physician in *Oxford*, "and divers others, continued such meetings in *Oxford*, and brought those studies "into fashion there, meeting first at Dr. *Petty*'s lodgings in an apothecary's house, "because of the convenience of inspecting drugs, and the like, as there was occa-"sion; and after his remove to *Ireland* (though not so constantly) at the lodgings of "Dr. *Wilkins*, then warden of *Wadham* college; and after his removal to *Trinity* "college in *Cambridge*, at the lodgings of the honourable Mr. *Robert Boyle*, then "resident for divers years in *Oxford*. These meetings in *London* continued, and after "the king's return, in 1660, were increased with the accession of divers worthy and "and honourable persons, and were afterwards incorporated by the name of the "*Royal Society*, and so continue to this day."

But to return to Mr. *Boyle*, his early zeal for religion and virtue, and his friendly faithfulness in reproving and counselling such as transgressed the laws of both, are evident from two letters sent to a noble lord, one or both written when he was but little turned of twenty years of age. I shall subjoin one of them.

* See his letter to Dr. *Thomas Smith*, dated January 29, 1696-7, published in Mr. *Thomas Hearne*'s Appendix to his preface to *Peter Langtoft*'s chronicle, Vol. I. p. 161. edit. *London*, 1725.

"Dear

" Dear Count,

" WHEN I ſhould have anſwered yours by the laſt poſt, ſome phyſic I had " taken the night before ranging very briſkly through all my veins had ſo " ſtirred up and exaſperated my ill humours, that I durſt not handle a pen. But now " I am calmed enough to let you know, that I leſs applaud your conſtancy than your " gallantry, that makes you ſo unſeaſonably turn Preſbyterian, by devoting yourſelf " to the *Kirk*. I hope you will never juſtify the proverb of, *the nearer the Church,* " *the farther from God*; but this ſame epithet of *incomparable*, with the appertaining " ſtory, if it be not rather your compliment, than your opinion, reliſhes of more " paſſion, than is allowed you for any woman but one. If I durſt ſo much injure " your judgment, as to diſtruſt your conſtancy, I ſhould tell you, that marriage is " not a bare preſent, but a legal exchange of hearts; and the ſame contract, that " gives you right to another's, ties you to look upon your own as another's goods, " and too ſurely made over, to remain any longer in your gift. I ſhould tell you " farther, that no change could be leſs excuſable than yours, not only becauſe you " are bleſſed with a lady, whoſe beauty and merit make injuries to your faith and " happineſs inſeparable, but becauſe too you cannot father upon your obedience any " temptation to a ranging flame; for you aſſumed the entire liberty of chuſing to " yourſelf whom you muſt now ſolely blame, if you matched not to your ſatisfaction, " when you matched ſo purely for it, that had you as much motive to fickleneſs, as " you have to the contrary virtue, you ought to expect as little pity, as, God be " praiſed, you need; and could betray no diſlike of your election, that would not " either criminate your honour, or diſparage your judgment. * * *

" BUT I hope, dear count, by this time you will think me as mad for writing all " this to you ſeriouſly, as I ſhould think you, if my ſuſpicions were juſt; for I hope " your innocence will turn all this into a compliment to Mrs. *Kirke*, by diſtruſting " her eyes of the power of kindling a flame, which ſuch obſtacles oppoſe, and ſo " great inconveniences would attend.

" THE diſaſter of your addreſſes to the council of ſtate I am extremely troubled " at, both for your ſake and my own; ſince not only that diſappointment defeats me " of good company, but, I much fear, miſleads you into ill. And truly, my lord, " I am enough your ſervant to tell you freely, that a young man, that ventures him- " ſelf in ill company, tempts as many temptations, and expoſes himſelf to as many " inconveniences, as to reckon up would require a homily, and to inſiſt on a vo- " lume, rather than a letter. The ſeveral goods of body, mind, fame, and fortune, " are blaſted by ill company. For firſt (to begin with the laſt) it engages us to that " riot and expenſiveneſs, which, beſides that it is ever ruinous, is now ſignally un- " ſeaſonable, being not only extremely unſuitable to the ſad condition of our times, " but peculiarly obſtructive to thoſe who make applications to the ſtate for relief or " compenſation of loſſes; ſince the ſame expences that beget want, paſs for argu- " ments of plenty, in the opinion of thoſe whoſe belief of our want is as great a " requiſite to the removal of it, as the juſtneſs of that belief were a misfortune. " How much the body ſuffers by thoſe debauches, that are epidemical to looſe com- " pany, though the inſtances be numerous and obvious, few youths will believe, " till their experience hath ſadly convinced them; not to mention, that thoſe diſtem- " pers that are both the effects and puniſhments of exceſs, are as uneaſy upon the " ſcore of ſhame, as that of pain; and health impaired by diſſoluteneſs has com- " monly the fate of needing pity, and wanting it. As for the mind, ill company is

" an infected air for it; which, though it be precisely no disease, disposes to the most " mortal ones. Bad examples are the general pimps of every vice; and I dare " boldly affirm, that fewer people go to hell for pride, or oaths, or incontinency, " than for company; which I therefore desire to draw in blacker colours to you, be- " cause I have ever observed it worst for the best natures, since they being pliable and " easy do many things that offend their consciences, merely to please the company " which a man must be like to be liked, and which not to imitate is interpreted to " reprove. And when once compliance has betrayed one to a vice, custom detains " us there, it happening to men in sins, as in taking of tobacco, which, though at " first sucked in with reluctance, barely to please the company, men afterwards find " a gusto in, and are unable to leave off. And therefore I prefer few laws before " that of a nation we are pleased to call barbarous, amongst whom it was allowed to " sue a man for keeping evil company, as well as for riot, adultery, or any other " crime it leads to; which will appear less strange and more equitable to them that " consider, that amongst us a bawd is as legally punishable as a whore or wenching. " And lastly, a good name can by nothing be strained deeplier than by ill company. " Few people have either the charity or opportunity to know men intimately; and " there is no way either more probable, or more frequented, to judge of those we " know not, than by their company, since men are not unreasonably supposed to " delight in those qualities they delight in others for. For besides that the choice of " ill company does not only make men thought to have been vicious, but to be made " so, as longing for unhealthy and extravagant trash does not only argue, that a maid " has the green-sickness, but serves to increase the disease; young men consider not " enough, how deep a root the first impressions they give of themselves take, and " consider not, how difficult it is to bring men to think well, when to do so they " must acknowledge to themselves, that they have thought amiss; especially since " most human acknowledgments being of so changeable-coloured a complexion, that " like pigeons necks they give various representations, as they are variously looked " on. A disfavouring prepossession brings even our good actions under the disad- " vantage of suspicion, and procures our indifferent ones the harshest construction; " where the intention is doubtful, men generally collecting it from their opinion of " the person, and being disposed to believe rather that he is faulty, than they " mistaken."

IT was about the same time, that he wrote his *Free Discourse against customary Swearing*; the manuscript of which in his own hand-writing is still extant; and by the neatness and exactness of it appears not to have been the first draught, though it is dated *Anno MDCXLVII.* The first leaf has this title, *The Swearer silenced by* R. B. but in the first page it is called, *A Free Discourse against customary Swearing.* It is dedicated to his sister, the countess of *Kildare.* There is annexed to it, *A Dissuasive from Cursing, from Mr.* W. D. *to Sir* G. L. These two pieces were published at *London*, 1695, in 8vo, by Dr. *John Williams*, afterwards bishop of *Chichester*, who observes in his preface, that the *Discourse* was the third treatise, which Mr. *Boyle*, though then but twenty years of age, had prepared for the public; his *Seraphic Love*, afterwards printed, and his *Essay on mistaken Modesty*, being referred to in this piece.

BUT the course of his studies was interrupted for some time in the summer of the year 1647, by a severe fit of the stone, to which distemper he was extremely subject. However, in September following he went to *Bristol* and *Salisbury*; and in February, " 1647-8,

1647-8, made a voyage to *Holland*, partly to visit the country, and partly to accompany his brother *Francis* in conducting his wife from the *Hague* [a]. But he did not stay long there, for on the 15th of April, 1648, he was at *London*; and on the 13th of May at *Stalbridge*, whence he wrote to his sister *Ranelagh*, giving her an account, that he should possibly soon send her his thoughts upon the subject of *Toleration*, in an essay of his, intitled, *Of Divinity* [b]. He returned to *London* the same month, where he was likewise in July; and in a letter to the countess of *Monmouth* at *Moor-Park*, dated the 7th of that month, mentions a piece of his writing, then in the hands of that lady [c]. He was in August following at *Leez* in *Essex*, the seat of his sister the countess of *Warwick*, where on the 6th of that month he finished his treatise, intitled, *Some Motives and Incentives to the Love of God, pathetically discoursed of in a Letter to a Friend:* of which I shall give some further account below, when I have occasion to mention the publication of it. But he was returned to *Stalbridge* on the 9th of September following; and on the 26th of March 1649, was at *Marston* in *Somersetshire*, the seat of his brother, the lord *Broghill*; and on the 2d of August at *Bath*; in a letter from whence to his sister *Ranelagh* of that date [d], he takes notice of his having been troubled for three or four weeks with a quotidian ague, "though "in the intervals of the fit (says he) I both began and made some progress in the "promised discourse of *Public Spiritedness*. But now truly weakness, and the doctor's "prescriptions, have cast my pen into the fire, though in spite of their menaces I "sometimes presume to snatch it out a while, and blot some paper with it. My "present employment is reviewing some *Consolatory Thoughts on the Loss of Friends*, "which my poor lady *Susan*'s death obliged me to entertain myself with, and which "I am now recruiting." But on the 31st of that month he was returned to *Stalbridge*, as appears from a letter of his of that date to the same lady [e], wherein he writes thus: "I will not now presume to entertain you with those moral speculations, "with which my chemical practices have entertained me; but if this last sickness had "not diverted me, I had before this presented you with a discourse (which my vanity "made me hope would not have displeased you) *of the theological use of natural philosophy*, endeavouring to make the contemplation of the creatures contributory to "the instruction of the prince, and to the glory of the author of them. But my "blood has so thickened my ink, that I cannot yet make it run; and my thoughts "of improving the creatures have been very much displaced by those of leaving "them. Nor has my disease been more guilty of my oblivion, than my employment, since it has begun to release me; for *Vulcan* has so transported and bewitched "me, that as the delights I taste in it, make me fancy my laboratory a kind of "Elysium, so, as if the threshold of it possessed the quality the poets ascribed to "that *Lethe* their fictions made men taste of, before their entrance into those seats of "bliss, I there forget my standish and my books, and almost all things, but the unchangeable resolution I have made, of continuing till death your, &c."

He was at *London* on the 15th of November, 1649; on the 20th of the same month at *Marston*; on the 21st of December, and in the month of January following, again at *London*; and on the 1st of May, 1650, at *Stalbridge*, whence he wrote the following letter to Mr. *Hartlib*:

[a] Mr. *Boyle*'s letter to Mr. *Marcombes*, dated from *London*, February 22, 1647-8, in which he mentions his intentions of setting out for *Holland* the next day.
[b] See his Works, Vol. VI. p. 45.
[c] Ibid. p. 47.
[d] Ibid. p. 48.
[e] Ibid p. 49.

"SIR,

" SIR,

" I HAVE here so little time to dispatch a great deal of indispensible business in, " that my knowledge of your goodness, and yours of my hurry, promises me " your pardon for the necessitated fault of returning to so long and so excellent a " letter so short and so hasty an answer, which ought to be wholly employed in ac- " knowledgments and thanks for the exact intelligence you are pleased to oblige me " with from *Utopia* and *Breda*; my inclination as much concerning me *in Republicâ* " *Literariâ*, as my fortune can do *in Republicâ Anglicanâ*. Nor am I here altogether " idle, though my thoughts only are not at present useless to the advancement of " learning; for I can sometimes make a shift to snatch from the importunity of my " affairs leisure to trace such plans, and frame such models, as, if my Irish fortune " will afford me quarries and woods to draw competent materials from, to construct " after them, will fit me to build a pretty house in *Athens*, where I may live to phi- " losophy, and Mr. *Hartlib*,

Stalbridge, May-day, 1650.

" A cordial friend, and not wholly useless servant,

" R. B.

" IN great haste, which I beseech you pardon."

To this I shall add here another letter to the same gentleman, the date of which is wanting.

" Dear Mr. *Hartlib*,

" I AM sure, that you have too much charity to want justice; and therefore on the " score of your serious promise I am bold, not only to desire, but to expect at " your hands a *Mercurius Philosophicus*, in an account of the projects and successes of " that *college*, whereof God has made you hitherto the midwife and nurse. I shall " not beg any information from you of that *diurnal* news, of which a whole sheet " may be had for a penny, and yet be over-bought. No, I will stint my requests to " your *Utopian* intelligence, as the only way to keep me in charity with men, by " letting me see, that the degenerate world yet harbours some, that do not undeserve " the name. As for me, knowing, that my letters will be guilty of faults enough " in rhetoric, without loading themselves with any needless crimes in morality, I " here openly disclaim all compliments, and solemnly engage myself (as to me) to " give them a perfect banishment from our converse, wherein I am confident I shall " have but a very easy office to surmount; since for my part I ever esteemed them at " best but the froth (not to say the scum) of civility; and for yours, I know the " gallantry of your principles leads you to the noblest way of putting them out of " countenance, by carrying you to doing men really more good than they dare pro- " mise. And since you do not disdain the meanest workman, that is but willing to " lay some few stones towards the building of your *college*, I shall in my following " epistles (if this procure them a pass) take the liberty to acquaint you with what " thoughts and observations of mine I shall judge useful in reference to so glorious " a design; to which I shall think it very much my happiness, if any endeavours of " mine can have the honour in the least measure to contribute, not only as they owe

" a duty

" a duty to the public (though, I must confess, that of itself a very prevalent motive) " but because I know you so vastly affectionate to that public, that my invention " will furnish me with no fitter way, than that of my services to it, to give you real " and accepted testimonies of my being, &c."

He had now gained so eminent distinction in the republic of learning, that in 1651 Dr. *Nathaniel Highmore*, a physician, who had been a member of *Trinity* college in *Oxford*, addressed to him his Treatise, printed at *London* that year, under the title of *The History of Generation; examining the several opinions of divers authors, especially that of Sir* Kenelm Digby *in his* Discourse of Bodies. *With a general relation of the manner of Generation as well in plants as animals; with some figures delineating the first originals of some creatures, evidently demonstrating the rest. To which is joined a Discourse of the Cure of Wounds by Sympathy, or without any real application of medicines to the part affected, but especially by that power, known chiefly by the name of Sir* Gilbert Talbot's *powder*. The dedication is dated from *Sherborne* May 15, 1651, with this inscription, *To the honourable Mr.* Robert Boyle, *son to the right honourable the earl of* Corke, *my much honoured friend*; and begins thus:

" Noble Sir,

" WHERE virtue shall be found in conjunction with nobility in such black, " the last and worst times, it no less invites and amazes the eyes and hearers " of beholders, than some new star or blazing comet; but with this difference, the " one is cause of their fear, the other gives life to their hopes and joy. You have, " Sir, so enriched your tender years with such choice principles of the best sort, and " even to admiration managed them to the greatest advantage, that you stand both a " pattern and wonder to our nobility and gentry, who in these past times, many of " them, have so spent their precious minutes, that they are scarce able to account for " one, or spend an hour but in vice; that cannot brook virtue, because it is not " born with them; that hate all things, that must be obtained by industry; who " most degenerately intrusting their wits as well as fortunes with their inferiors, have " made them masters of both; a sad forerunner (I will not say author) of these sadder " times. But you have made a better and far nobler choice; you have not thought " your blood and descent debased, because married to the arts. You stick not to " trace nature in her most intricate paths; to torture her to a confession, though with " your own sweat and treasure obtained."

It was about the year 1652, that Mr. *Boyle* began an *Essay on the Scripture*, still extant in manuscript, from which his *Considerations on the Style of the Scriptures* were chiefly taken. This is evident from a letter of his, dated June 19, 1652, to his brother the lord *Broghill*, at whose request he composed that *Essay*; which letter, with a few alterations, was afterwards made the Epistle Dedicatory to the *Considerations on the Style*, &c. Under what disadvantages the *Essay* was written, the reader is informed in the preface to the *Considerations*: " The Essay, *says he*, having not been " all written in country, but partly in *England*, partly in another kingdom, and " partly too on ship-board, it were strange, if in what I writ, there did not appear " much of unevenness, and if it did not betray the unleisuredness, and relish of the " unsettledness of the wandering author, who by thus rambling was reduced, for " want of a library, to comply with the request of his friend, who was more desirous " to receive from the author apples and pears growing in his own orchard, than " oranges and lemons fetched from foreign parts." A discourse written in this

manner, without any foreign assistance, and under so many disadvantages in the exercise of his own talents, shews his abilities in a much more conspicuous light, than if he had compiled some finished piece with the usual helps, and at a time of leisure and freedom from distractions. The public will undoubtedly be glad to see his thoughts concerning the last English translation of the Bible, and of the advantage of understanding the original languages of the scriptures, with some account of his own application to the study of them, extracted from some loose sheets, intended as a part of his *Essay on the Scripture*.

" THOUGH I think, says he, the authors of our last translation have made it much " more correct than our former was, and preferable to most I have met with in other " languages; and have therefore been sincerely and industriously instrumental to the " British churches edification, according to their light; yet that it were very possible, " as well as to be wished, that an English translation, yet in many places more cor- " rect, might be framed, will scarce seem improbable to an intelligent person, that " duly considers, how much the knowledge of tongues, and (what would be highly " useful, not to say necessary, in such a work) of Jewish and other Eastern learning, " is, since our last version was made, encreased and refined, and consequently, how " many texts by that greater light, that now shines among critics and antiquaries, " may be rendered more fully, or more warily, or more clearly, or more cohe- " rently to the context, or more congruously to the analogy of faith, or that of " reason.

" I CANNOT but reckon the want of (not superficial rudiments, but) solid know- " ledge and practical readiness in the originals of the Bible amongst the chief impe- " diments of our discovery of the נפלאות *niphlaot*, or unobvious rarities of that " divine book, from which he much derogates that stints them to those discernible " in a translation. But I pretend not, by what I have now said, or reserve to say " elsewhere, either to impose a necessity of learning Greek and Hebrew on those, " whose incapacities or employments deny them the requisites or conveniences of such " a study; or to discourage those that cannot understand the Scriptures in its ori- " ginals, for reading it in translations; for (at least among his own children) he, that " requires less, where less hath been intrusted, will not exact a measure of knowledge " disproportionate to the means he hath vouchsafed of it; and translations of the " Bible may be very effectual to God's purposes in it, though not so much so as their " originals to all, especially as to the practical maxims of religion. Experience makes " it probable, that translations hold them forth clearly and unanimously enough " to make their faithful practisers acceptable unto God through Jesus Christ, as the " Egyptians were wondrously enriched by the river *Nilus*'s streams flowing from an " unknown fountain. To which purpose I remember, that that excellent Christian " scholar and divine Dr. *Usher* archbishop and primate of *Armagh* (whose encourage- " ments I gratefully acknowledge to have much engaged me to the study of the holy " tongues) talking to me one day in his study of his intended edition of the Septua- " gint, earnestly implored of him by the transmarine critics, observed to me, not " without some just wonder, that for divers ages the church (as to the greater part " of it) and even the apostles, and even those that most flourished in piety, scarce " used any other scripture than the Septuagint's translation, which is one of the " faultiest versions of the Bible, and wants not gross mistakes. And indeed, so " there be a devout, industrious, and modest search after the truths of scripture " proportionable to the abilities and opportunities that God vouchsafes, and a con- " scionable conforming of our lives to our discoveries, it is much to be hoped, that

" we

" we may miſs a great many theological truths without miſſing ſalvation. But as I " ſhall not exact the ſtudy of the original from thoſe whoſe want of parts or leiſure " diſpenſeth them from it; ſo cannot I but diſcommend thoſe, who wanting neither " abilities, time, nor conveniences to range thorough I know not how many other " ſtudies, can yet decline this; and who ſparing no toil nor watches to put it out of " the power of the moſt celebrated philoſophers to deceive them in another doctrine, " leave themſelves obnoxious to the ignorance, fraud, or partiality of an interpreter " in that of ſalvation; and thereby ſeem more ſhy of taking any opinions upon truſt, " than thoſe, in whoſe truth or falſeneſs no leſs than God's glory, and peradventure " their own eternal condition, is concerned. Methinks thoſe, that learn other lan- " guages, ſhould not grudge thoſe that God hath honoured with ſpeaking to us, and " employed to bleſs us with that heavenly doctrine, that comes from him, and leads " to him. When I have come into the Jewiſh ſchools, and ſeen thoſe children that " were never bred up for more than tradeſmen, bred up to ſpeak (what hath been " peculiarly called) God's tongue, as ſoon as their mother's, I have bluſhed to think, " how many gown-men, that boaſt themſelves to be the true *Iſraelites*, are perfect " ſtrangers to the language of *Canaan*; which I would learn, were it but to be able " to pay God the reſpect uſual from civil inferiors to princes, with whom they are " wont to converſe in their own languages. For my part, I * * * that have a me- " mory ſo unhappy and ſo unfit to [ſupply] my intellectual deficiencies, and the reſt " of my diſabilities, that it often ſtrongly tempts me to give over my ſtudies, and " abandon an employment, wherein my ſlow acquiſts are (by the treacherouſneſs of " my memory) ſo eaſily loſt; beſides this diſadvantage, I ſay, thoſe excellent ſciences, " the mathematics, having been the firſt I addicted myſelf to, and was fond of, and " experimental philoſophy with its key, chemiſtry, ſucceeding them in my eſteem " and applications; my propenſity and value for real learning gave me ſo much " averſion and contempt for the empty ſtudy of words, that not only I have viſited " divers countries, whoſe languages I could never vouchſafe to ſtudy, but I could " never yet be induced to learn the native tongue of the kingdom I was born and " for ſome years bred in. But in ſpite of the greatneſs of theſe indiſpoſitions to the " ſtudy of tongues, my veneration for the ſcripture made one of the greateſt de- " ſpiſers of verbal learning leave *Ariſtotle* and *Paracelſus* to turn grammarian, and " where he could not have the help of any living teacher, engaged him to learn as " much Greek and Hebrew, as ſufficed to read the old and new teſtament, merely " that he may do ſo in the Hebrew and Greek, and thereby free himſelf from the " neceſſity of relying on a tranſlation. And after I had almoſt learned by rote an " Hebrew grammar, to improve myſelf in ſcripture-criticiſms, in the Jewiſh way of " reading the oracles committed to them, I, not over-cheaply, purchaſed divers " private conferences with one of their ſkilfulleſt doctors (as St. *Hierom* had thoſe " nocturnal meetings, which ſo much helped to make him the ſolideſt expoſitor of " all the fathers, with *Barraban* or * * * the Jew) I received of him few leſſons, that " coſt me not twenty miles riding, at a time, when I was in phyſic, and my health " very unſettled. A Chaldee grammar I likewiſe took the pains of learning, to be " able to underſtand that part of *Daniel*, and thoſe few other portions of ſcripture, " that were written in that tongue; and I have added a Syriac grammar purely to " be able one day to read the divine diſcourſes of our Saviour in his own language: " in which I can truly profeſs, with the famous publiſher of the Syriac teſtament, " *Guido Fabricius* (in his dedication of that book, and his verſion of it, to the then " *French* King) that I had no inſtructor to teach me ſo much as to know the letters,

" but have been, to use the words he borrows of the learned *Budæus*, αὐτοδίδακτοι καὶ " θεοδίδακτοι, have had no other living teacher but God and myself in the little " grammatical learning I have acquired in those four tongues, in which the better " understanding and relishing of the scripture limit my pretensions. Nor do I at all " repent my labour, though, to secure my progress and acquists in these languages, " my bad memory still reduces me to a constant and frequent recollection of some " choice institutions of them all. For certainly the satisfaction of understanding " God, and those excellent persons celebrated even in his book, express themselves " in their own very terms and proper languages, doth richly recompense the pains " of learning them; for according to the known saying,

" *Quamvis allatâ gratus sit sæpe in undâ,*
" *Dulcius ex ipso fonte bibuntur aquæ.*

" Though we stream-waters not unpleasant think,
" Yet with more gusto of the spring we drink.

" It is true, that a solid knowledge of that mysterious language, God and his " prophets spake (whatever is given out to the contrary by superficialists, amongst " whom I remember a Jewish professor of my acquaintance used to reckon many " that are thought and think themselves Hebricians, because they could without " hesitation and the help of a translation or a dictionary read and render in their own " tongue an Hebrew chapter) is, I say, somewhat difficult, but not so difficult, but " that so slow a proficient as I could in less than a year, of which not the least part " was usurped by frequent sicknesses and journies, by furnaces, and by (which is " none of the modestest thieves of time) the conversation of young ladies, make a " not inconsiderable progress towards the understanding of both testaments in both " their originals. * * *

" For my part, that reflect often on *David*'s generosity, who would not offer as " a sacrifice to the Lord his God that which cost him nothing, I esteem no labour " lavished, that illustrates or endears to me that divine book; my addictedness to " which I gratulate to myself, as thinking it no treacherous sign, that God loves a " man, that he inclines his heart to love the scriptures, where the truths are so pre- " cious and important, that the purchase must at least deserve the price. And I con- " fess myself to be none of those lazy persons, that seem to expect to obtain from " God the knowledge of the wonders of his book upon as easy terms as *Adam* did " a wife, by sleeping profoundly, and having her presented to him at his awaking."

In the same year 1652 he went to *Ireland*, whence he wrote the following letter in January 1652-3 to *his highly honoured friend John Mallet, Esq; at Poynington, near Sherburne in Dorsetshire:*

" SIR,

" It is so long since that I have been made happy in an unanswered letter of " yours, that it would now almost as justly produce my blushes that it hath lain " so long unacknowledged, as it did then my satisfaction to find it so obliging; if it " had not so abruptly come to my hands, and with so many other papers, that till I " was this afternoon informed by Mr. *Lillyes*, that he sent it me, I neither knew by " whom you had written it me, nor by whom I might securely return you for it my " highly merited thanks. But though this ignorance lessened, it destroyed not the " happiness

" happinesſ I received in a paper, whoſe writer kept it from needing endearing cir-
" cumſtances. And certainly, without a huge deal of inſenſibility, I could not but
" be highly ſenſible of a civility, that came to ſeek me out in a country, which thoſe,
" that have the moſt relation to it, ſeldom think on any longer than they are in it.
" Nor was I only mightily affected to find the welcome continuation of your friend-
" ſhip for me, but very much delighted to find too, that you began to have a friend-
" ſhip for the Eaſtern tongues. For though to a perſon ſo uſed to the ſtudy, and
" repleniſhed with the knowledge of things, as Mr. *Mallet*, the learning of words
" cannot but at firſt be very tedious; yet ſince to be a good grammarian is neceſſary
" to be a good divine; and he that hath no ſkill in the original ſcripture himſelf,
" may be deluded by thoſe that tranſlate it for him; you will find a rich compenſa-
" tion for the trouble of learning the holy tongue in the advantages of having learned
" it; and by the help of that primitive language, wherein they were written, you
" may gain a free and ſafe acceſs to thoſe theological myſteries, which he that is no
" linguiſt, muſt either totally ignore, or take upon truſt. And certainly a know-
" ledge ſo directly conducive to that knowledge that is called *life eternal*, cannot but
" deſerve the being laboured for. And to ſatisfy you, that the difficulties of attain-
" ing ſo precious a knowledge are not inſuperable, I ſhall tell you, that I have a
" young kinſwoman newly marriageable, who to the French and Latin hath added
" ſkill enough in the Greek and Hebrew tongues to read the two teſtaments in them.
" I am glad to hear by Mr. *Lillyes*, that you have got ſo good an inſtructor as Mr.
" *Bythner*, whoſe ſhort grammar of the laſt edition is that for which I left off many
" others of the moſt approved, after having ſurveyed them all. But his inſtitutions
" being almoſt confined to the etymological part of grammar, are chiefly proper for
" a beginner; and therefore, when you are proficient enough to decline moſt nouns,
" and conjugate (though not all verbs) all ſorts of verbs; it would be requiſite for
" the ſyntactical part of grammar to have recourſe to ſome that have handled it
" expreſly; wherein next *Glaſſius* (whoſe works are voluminous enough, and of
" various ſubjects, but, in my green judgment, theological and excellent) the elder
" *Buxterfius* hath been the moſt full and accurate of thoſe I have yet met with. There
" is likewiſe an old book of *Flacius Illyricus*, which he calls *Clavis Scripturæ*, which,
" though little taken notice of, is more than a little uſeful, and very well worth
" peruſing; ſince giving the reader a particular account of the Hebraiſms, as they
" lie in ſcripture, he oftentimes teaches him at once both grammar and divinity. But
" I muſt beg your pardon for troubling you with theſe perhaps needleſs advertiſe-
" ments: I have cauſe to believe Mr. *Bythner*'s will be much riper; only the pre-
" judice I ſuſtained, by not having ſeaſonably an addreſs to the authors I have
" named, makes me willing, rather to hazard the exerciſe of your patience by ſuper-
" fluous directions, than venture to let you miſpend any of your ſtudious hours, for
" want of ſuch addreſſes, as are not bad, though they prove not the very beſt. I
" forget to tell you, that there is one *Gerhard* (ſon of the famous Lutheran divine)
" who hath publiſhed an Harmonical Synopſis of the Hebrew, Chaldee, Syriac,
" Arabic, and Ethiopic tongues; which would be no bad iſagoge to the Eaſtern
" languages, if it were not ſo wretchedly falſe printed, that it expoſes the learned in
" almoſt every page to perplexities or miſtakes. I wiſh I could give you ſome
" account of my ſtudies here; but I muſt ſadly confeſs, that the perpetual hurry
" I live in, my frequent journeys, and the neceſſary trouble of endeavouring to
" ſettle a very long neglected and diſjointed fortune, have left me very little time to
" converſe with any book, ſave the Bible, and ſcarce allowed me leiſure to ſew toge-

" ther some loose sheets, that contained my thoughts about the scripture, of which " I wish I had a copy to submit to your censure. But I hope by God's blessing to be " able to bring over one myself this spring into your parts; to which it is not my " least of invitations, that I shall there enjoy the happiness of a conversation that " hath made me upon so many scores,

" SIR,

" your most affectionate friend,

" and obliged humble servant,

" ROBERT BOYLE."

" MY most humble service, Sir, I beseech you, to Sir *Thomas Mallet* and my lady, " and the fair young lady you are happy in. And I shall beg you to do me the fa- " vour to convey my high respects to the *Sherbone* family, when you go thither. The " news here is not considerable enough to deserve your notice. My friends here are, " God be praised, well, and those that have the happiness to know you, much your " servants."

HE came back from *Ireland* to *England*, probably about August 1653, and was at *London*, where he met with Dr. *John Wilkins*, then warden of *Wadham* college in *Oxford*, and afterwards bishop of *Chester*, to whom he intimated his resolutions of settling at *Oxford*[f]. In September he was a few days at *Stalbridge*; but being obliged by his affairs to return to *Ireland*, in his way thither he wrote the following letter to his friend Mr. *Mallet*:

" SIR,

" IF the suddenness of that unwelcome necessity, which hurried me from *Stal-* " *bridge*, would have allowed me so much as two days stay in those parts, I should " not be now reduced to make you an apology for my leaving *Stalbridge*, before I " kissed your hands at *Poynington*. And though the urgency of my occasions allowed " me but one day to pack up bag and baggage, and settle my affairs there, I should " have spent part of that day in paying you the duty, and giving myself the satis- " faction of a visit, if the greatest obstacle my haste put to my desires, had not " been the tiredness of my horses by my preceding journies. The apprehension of " losing my passage, unless my arrival here prevented the feared change of the wind, " posted me so abruptly hither, that it allowed me to comply neither with my " civility, nor with my inclinations, the latter of which are never more gratified " than in your charming conversation, which the wind's crossness since my arrival here " makes me very much regret I did not venture to give myself the happiness of. " The sad condition of *Ireland* making me somewhat irresolute of my going thither " this winter, I do not wholly deny myself the pleasing thoughts of being happy " in your company at *Poynington*. But in case a fair wind blow away those thoughts, " I must implore, in the want of your immediate conversation, that which is practised

[f] See Dr. *Wilkins*'s Letter of Sept. 3, 1653, Vol. VI. p. 633, 634.

" by

" by letters, as the next contentment to the former by nearliest approaching it. In " the necessity of repairing into *Ireland* to settle my affairs there, now things seem " tending to a settlement in that unhappy country, I shall leave behind me at *Stalbridge* " the bearer, *Roger Ball*, and with him a lease of that manor for some yet unexpired " years; in which lease you were pleased to allow me to have your name put in as a " trustee for me. The reason of my mentioning this to you is, that by the mistaking " confidence of honest old Mr. *Mawdesley*, then steward of my courts at *Stalbridge*, " I made several grants there, as presuming I had power to do so; which, upon " more knowledge of the law, and a more heedful perusal of my own more than " ordinarily strict entail, I found, though hardly overthrowable in equity, yet to be " questionable in strictness of law. My just tenderness in cases of this nature made " me extremely troubled, that my ignorance had made me do what knowingly no- " thing should; and therefore by Sir *Thomas Mallet*'s excellent advice, finding a " lease of *Stalbridge* for about thirty years unexpired to remain in the hands of an " old servant of my father's, I made it one of my motives to go into *Ireland*, and " one of my concerningest businesses there, to get this lease assigned over in trust to " yourself and *Roger Ball*, with whom I have left order, that in case God should call " me to himself before my return to *Stalbridge*, he shall beg you to join with him in " making every tenant, whose grant is questionable, a lease of as many years, as " your authority extends to grant, that so these poor men, in case their titles (con- " trary to my expectation and to probability) should be overthrown as to their first " grants, may have a good title to all the unexpired years of your lease; and that " number, according to common estimation, will be as advantageous, if not better, " than their first grants. I find, Sir, no small trouble in the necessity of giving you " this great one; but I am confident you are such a friend to justice, that you will " pardon a fault that proceeds only from some tenderness of that virtue in a servant " of yours, who very well knowing gratitude to be a virtue as well as justice, if not " part of it, is not made more desirous by the laws of the former virtue to secure " his tenants, than the duties of the latter will make him of the opportunity of ex- " pressing and evincing, how highly he is Mr. *Mallet*'s

" affectionate, faithful, and obliged

Bristol, Sept. 23, 1653.

" humble servant,

" ROBERT BOYLE.

" I SHALL beg the favour of having my most humble service presented to Sir " *Thomas Mallet* and my lady, and to your own fair lady, with the rest of your " family; to all which you have engaged me to be a servant. If the bearer shall, " in my absence, apply himself to you for directions, your vouchsafing them to him " will very much oblige me. If you please to make me at any time happy in your " letters, it will as well instruct as satisfy me, to be informed, how you proceed in " relation to your Eastern studies."

HE pursued his intended journey to *Ireland*; for we find him there in January 1653-4 [a], where he proposed to continue till Midsummer 1654 [b], though his residence in:

[a] See Mr. *Hartlib*'s letter to him of Feb. 28, 1653-4, Vol. VI. p. 78.

[b] Ibid. p. 55, and 83.

in that kingdom was by no means agreeable to him, since in a letter[i], which he wrote in the beginning of that year to Mr. *Clodius*, an eminent chemist, who married the daughter of Mr. *Hartlib*, he stiles it "a barbarous country, where chemical spirits "were so misunderstood, and chemical instruments so unprocurable, that it was hard "to have any Hermetic thoughts in it." For this reason he exercised himself in making anatomical dissections, being assisted therein by his friend Dr. (afterwards Sir) *William Petty*, then physician to the army and to the deputy of *Ireland*, and one of the most extraordinary men of his time, who in 1648, at the age of twenty-five, inscribed to Mr. *Hartlib*, *Advice for the Advancement of some particular parts of Learning*, printed at *London* in 4to; in which he gives plans of a mechanical and medical college, wherein youths might be educated, and the history of all mechanical arts and trades transmitted to posterity; and this piece seems to have been one of the first draughts of a philosophical college that was ever made in *England*. Mr. *Boyle* was brought acquainted with this gentleman by means of Mr. *Hartlib*, as early as the year 1647[k], and received great advantage, while he continued in *Ireland*, from his conversation and their joint studies in anatomy; in the course of which "I satisfied "myself (says he[l]) of the circulation of the blood, and the freshly discovered and "hardly discoverable *receptaculum chyli* made by the confluence of the *venæ lacteæ*, "and have seen (especially in the dissections of fishes) more of the variety and con-"trivances of nature, and the majesty and wisdom of her author, than all the books "I ever read in my life could give me convincing notions of."

He made also strict enquiries after the minerals, which *Ireland* afforded; and though he could meet with few, who had either skill or curiosity in that way, yet silver ore was brought to him, which was found upon one of his brother's estates, that upon trial was estimated to be worth between thirty and forty pounds a tun. A silver mine was then farmed of the state; and he was assured by experienced men, that *no country in Europe was so rich in mines as Ireland, had but the inhabitants the industry to seek them, and the skill to know them*[m].

After his return to *England*, which was probably, according to his intentions, in the latter end of June, 1654, he went to reside at *Oxford*, in order to prosecute his studies with the greater advantage, and continued there for the most part till April 1668, when he settled at *London* in the house of his sister *Ranelagh* in *Pall-Mall*. At *Oxford* he chose to live in a private house, rather than in a college, both for his health, and because he had more room and conveniency to make experiments, than he could have had in a society. The person, with whom he lodged was Mr. *Crosse*, an apothecary, whose great acquaintance with Dr. *John Fell*, afterwards bishop of *Oxford*, (who made him one of the executors of his pious and charitable will) and his own noble foundation of an hospital near *Ampthill* in *Bedfordshire*, for the maintenance of thirteen decayed citizens of *Oxford*, give him a right to be mentioned with honour in this history[n]. *Oxford* indeed was the only place in *England*, where at that time Mr. *Boyle* could have lived with much satisfaction to himself. Dr. *Wilkins*, who had married a sister of *Oliver Cromwell* the Protector, was a man of excellent temper, and admirable abilities, and by the influence of his example and authority, supported a spirit of rational piety, and a right taste of learning in that university. Dr. *John Wallis* and Dr. *Seth Ward*, the two Savilian professors of geometry and astronomy, Dr. *Thomas Willis*, the physician, then student of *Christ-Church*, Mr. *Christopher Wren*, then

[i] Vol. VI. p. 54. [k] Ibid. p. 76. [l] Ibid. p. 55. [m] Ibid.
[n] Dr. *Wotton's* papers, c. iii. § 1.

fellow of *All-Souls* college, Dr. *Goddard*, warden of *Merton* college, Dr. *Ralph Bathurst*, fellow of *Trinity*, and afterwards president of the same, and dean of *Wells*, were eminent likewise for their genius and application to the most useful parts of literature, which these great men united their endeavours to cultivate and promote. For this purpose they held frequent meetings, in which they conferred chiefly on philosophical subjects; and being satisfied, that there was no certain way of arriving at any competent knowledge, unless they made a variety of experiments upon natural bodies, in order to discover what phænomena they would produce, they pursued that method by themselves with great industry, and then communicated their discoveries to each other.

THIS was an employment, and this a society, which exactly suited Mr. *Boyle*'s inclinations. He had before laid in a great stock of mathematical and chemical knowledge: and as for the *Aristotelian* way of explaining the phænomena of nature, he rejected it, as a mere system of words, that would never make any man more intelligent than he was before. The *Cartesian* philosophy began to make a noise in the world; but he resolved to acquiesce in no single man's hypothesis, and to draw no conclusions from premises in natural things, which he could not actually verify himself; and so for many years he would not read over *Des Cartes*'s *Principles*, lest he might be biassed by the ingenuity or authority of that philosopher. With these dispositions he set himself to philosophise, and to persuade the nobility and gentry of the nation, who had the means and leisure to pursue such sorts of studies, to follow his example. He was convinced, that it would be of inestimable use to mankind to engage them in these enquiries; it would divert them from those impertinent and criminal amusements, with which most of them busied themselves, and would make them not only better Christians, but likewise more useful members of society.

IT was during Mr. *Boyle*'s residence at *Oxford*, that he invented that admirable engine, the *Air-Pump*, which was perfected for him in 1658 or 1659, by the very ingenious Mr. *Robert Hooke*, after he had seen a contrivance for that purpose by Mr. *Garterix*, which was too gross to perform any great matters [a]. Mr. *Hooke*, who was afterwards professor of geometry in *Gresham* college, and doctor of physic, then lived with Mr. *Boyle*, whom he assisted in chemistry, having been recommended to him by Dr. *Willis* the physician, whom he had before served in the same capacity [b]. By this engine Mr. *Boyle* made such experiments, as have gone very far to enable him, and those who have succeeded him, to form a just theory of the air. By this he demonstrated its elasticity; and that property alone was a means to find out abundance more. He begun also to compose histories of its particular qualities, all founded upon experiments or observations, of which he kept very exact registers, hoping by this means to leave such materials, as future ages might build complete theories upon. There were at that time no philosophical societies erected in other parts of *Europe*: what men had hitherto performed, they had mostly done upon their single stock, and that rather by the way of abstracted reasonings, than by making experiments. In *Italy* indeed the great *Galileo*, and his scholar *Evangelista Torricelli*, had made some experiments, which had excited the curiosity of mankind; as did especially that famous one of the former, in which by a tube filled with water, inverted and plunged into a vessel of the same, he kept a cylinder of it suspended at the height of thirty-four feet; by which the weight of the air was first demonstrated. The philosophy of *Des Cartes* had for

[a] Life of Dr. *R. Hooke*, by *R. Waller*, Esq; p. 3.

[b] Mr. *Ward*'s lives of the professors of *Gresham* college, p. 170.

some

ſome years been publickly taught in *Holland*; and though built upon principles very oppoſite to thoſe either of *Ariſtotle*, or the chemiſts, yet had not the neceſſary vouchers of repeated experiments, purpoſely tried, to make it good. However that philoſopher had done eminent ſervice, in leſſening the devotion which had been implicitly paid to *Ariſtotle*'s philoſophy, and infuſing an inquiſitive ſpirit into mankind. A diſpoſition to think freely concerning the cauſes of natural things, had now ſpread itſelf in *England*, *France*, and *Italy*; and that diſpoſition occaſioned all thoſe alterations in men's ways of ſtudy, and produced all thoſe extraordinary diſcoveries of nature, which diſtinguiſhed the laſt age.

BUT philoſophy and enquiries into nature were not the only things which engaged Mr. *Boyle*'s attention. His eyes were then ſtrong enough to follow thoſe critical ſtudies which he had begun before, and which he thought neceſſary to underſtand the ſcriptures thoroughly: and for this he had peculiar advantages at *Oxford*. Dr. *Edward Pococke*, Mr. *Thomas Hyde* of *Queen*'s college, and Mr. *Samuel Clarke*, who were men of great eminence for their ſkill in the Eaſtern languages, reſided there; and Mr. *Hyde* particularly was frequently conſulted by him during the reſt of his life upon any difficulties which he met with in the courſe of his reading upon thoſe ſubjects. But one of his moſt intimate friends, with whom he converſed upon theological points, was Dr. *Thomas Barlow*, then chief library-keeper of the *Bodleian* library, and afterwards biſhop of *Lincoln*. The doctor was a man of prodigious reading, and a proportionable memory; he knew what the fathers, ſchoolmen, or canoniſts had ſaid upon any queſtion in divinity, or caſe of conſcience; and being with all theſe accompliſhments very communicative of his knowledge, he gained the higheſt degree of Mr. *Boyle*'s eſteem and friendſhip, who uſed, as long as he lived, to conſult him upon caſes of conſcience, wherein he was ſure at the ſame time of learning all that had been hitherto urged upon the queſtion propoſed to Dr. *Barlow*[q], many of whoſe anſwers are ſtill extant. But however important theſe advantages were, which he enjoyed at *Oxford*, for the proſecution of his ſtudies, he extended them by a correſpondence with perſons eminent for their knowledge in other parts of *England*, and in foreign countries. Among theſe correſpondents, one of the principal was *Henry Oldenburg*, Eſq; a native of *Bremen* in *Lower Saxony*, who was for ſeveral years agent for that city in *England*, and afterwards tutor to the lord *Dungarvan*, nephew to Mr. *Boyle*, with whom he travelled abroad in the year 1657. He was at laſt ſecretary to the Royal Society, and died ſuddenly in September 1677, and his wife, the daughter of Mr. *John Dury*, dying about the ſame time, left two orphans, whom Mr. *Boyle* took care of for the preſent, no will of Mr. *Oldenburg* being found, nor had he or his wife any relation in *England*. The firſt letter extant of this gentleman to Mr. *Boyle* is dated April 15, 1657; and in another written from *Saumur* in *France* in the latter end of that year, he mentions his[r] ſatisfaction, that the council had granted Mr. *Boyle*'s deſires for the promotion of knowledge; "which (ſays he) I ſuppoſe to be thoſe, that "were couched in a certain petition you were pleaſed to impart unto me at *Oxford*; "wherein, if I remember well, a matter of twelve thouſand pounds ſterling was of-"fered to purchaſe confiſcated lands and houſes with in *Ireland*, and to commit the "profit thereof into the hands of certain truſtees, for to employ it in the entertain-"ment of an agent, ſecretary, tranſlators, for keeping intelligence, diſtributing re-"wards, *&c.* in order to the end aforeſaid."

[q] Dr. *Wotton*'s papers. [r] Vol. VI. p. 142.

DR.

DR. *John Beale*'s letters to him are very numerous, and upon subjects of great importance. This divine was born of a good family in *Herefordshire*, and educated at *Eton* school, whence he was transplanted to *King*'s college in the university of *Cambridge*, where he soon distinguished himself by his uncommon genius and learning. He was one of the first members of the Royal Society, and in 1665 was promoted to be chaplain to king *Charles* II. He was likewise doctor of divinity, and resided chiefly at his living of *Yeovil* in *Somersetshire*, where he died about the year 1683. There are several of his papers in the *Philosophical Transactions*.

ANOTHER of Mr. *Boyle*'s correspondents, and most intimate friends, was *John Evelyn*, Esq; of *Wotton* in *Surrey*, born there on the 31st of October, 1620, and educated at *Baliol* college in *Oxford*; who having travelled for seven years into the politer countries of *Europe*, upon his return to *England*, was one of the first of those gentlemen, who met for the establishment of the Royal Society. He passed through several considerable posts in the reigns of king *Charles* II. *James* II. and *William* III. and died at *London*, February the 27th, 1705-6, in the 86th year of his age[s]. His works are of various kinds and very valuable; and Dr. *Burnet*[t] stiles him *a most ingenious and virtuous gentleman*; and informs us, that he was *not satisfied to have advanced the knowledge of this age by his most useful and successful labours about planting and divers other ways, but was ready to contribute every thing in his power to perfect other mens endeavours*. His acquaintance with Mr. *Boyle* began, as he observes himself, in his letter to Dr. *Wotton*[u], "from courteous visits he made me at my house at "*Say's-Court*[w], which I as constantly repayed; so it grew reciprocal and familiar, "divers letters passing between us at first in civility, and the stile peculiar to him "upon the least sense of obligation. But those compliments lasted no longer than "till we became perfectly intimate, and had discovered our inclinations of cultivating "the same studies and designs, especially in search of natural and useful things, my-"self then intent on collections of notes in order to an *History of Trade*, and other "mechanical furniture, which he earnestly incited me to proceed in. So that our "intercourses of letters were now mostly upon that subject, and were rather so many "receipts and processes, than matter of letters; what I gathered of this nature, "(especially for the improvement of *Gardening* and *Sylva*, *Kalendarium*, *Acetaria*, &c. "being but part of that work, a plan whereof is published) would astonish you to "see... These designs and apparatuses growing beyond my forces, were left "imperfect, upon the restoration of the banished King, when every body expected a "new world, and had other prospects than what the melancholy and almost despair "suggested to pass away anxious thoughts by those innocent employments I have "mentioned; the establishment of the Royal Society taking in all these subjects; "which made our personal meeting (unless at *Gresham* college, where we assiduously "met and conversed) less necessary." Among Mr. *Evelyn*'s letters is a very long and curious one[x], dated at *Say's-Court*, Sept. 3, 1659, and containing a scheme of a *Philosophical College*, in which he wished to have Mr. *Boyle* for a fellow-member, *who is alone*, says he, *a society of all that were desirable to a consummate felicity*. And on the 29th of the same month he wrote to him another letter[y], upon reading the manuscript of Mr. *Boyle*'s *Seraphic Love*, which he had done with so high a satisfaction, that he observes at the conclusion of his letter, that "he extremely loved Mr. *Boyle* be-"fore, but that his heart was infinitely knit to him now. For what (says he) are

[s] General Dictionary, Vol. V. p. 126—129. [t] History of the Reformation, Part III. B. 3. p. 417. [u] Dated at *Wotton* in *Surrey*, Sept. 10, 1703. [w] In *Deptford*. [x] Vol. VI. p. 288, 289, 290, 291. [y] Ibid p. 291.

" we now to expect from so timely a consecration of your excellent abilities. The " *primitiæ* sanctified the whole harvest; and you have at once by this incomparable " piece taken off the reproach, which lay upon piety, and the enquiries into nature, " that the one was too early for young persons, and the other the ready way to athe- " ism; than which as nothing has been more impiously spoken, so nor has any thing " been more confuted."

Dr. *John Pell* was another of Mr. *Boyle*'s friends. He was descended of an ancient family in *Lincolnshire*, and born at *Southwyke* in *Sussex*, of which his father was minister, on the 1st of March, $16\frac{10}{11}$. At thirteen years of age he was sent to *Trinity* college in *Cambridge*, being at that time as good a scholar as most masters of art in the university, which degree he took in 1630, and soon became eminent for his skill in the mathematics, and in the Latin, Greek, Hebrew, Arabic, Italian, French, Spanish, and High and Low Dutch languages. He was several years professor of mathematics at *Amsterdam*, and afterwards at *Breda*, and in 1654 was appointed envoy from the protector *Oliver* to the protestant cantons in *Swisserland*, where he continued till 1658. After the restoration he entered into holy orders, and became doctor of divinity, and chaplain to Dr. *Gilbert Sheldon*, archbishop of *Canterbury*, though he never enjoyed preferment equal to his merit, and died in mean circumstances in the parish of St. *Giles's in the Fields*, on the 12th of December, 1685, and was interred in that church[a]. Among other pieces published by him, he translated from the Low Dutch *A Tract touching the skill of a better way of Anatomy of Man's Body, by* Yonker de Bills, *Lord of Koppensdam*, printed at *London*, in 1659, in 12mo; to which Mr. *Boyle* wrote a preface, prefixed to that edition, which was procured by himself. Mr. *Hartlib*, in a letter to Mr. *Boyle* from *London*, dated November the 1st that year[a], mentions his intention to send a copy of this translation and preface to monsieur *Bills* himself, *to let him know, what honour hath been done unto him by a young nobleman in these parts, and many more to friends in the Low Countries. Really*, continues he, *you have done an excellent work for spreading this anatomical magnale upon the honest learned world.* He sent likewise the following extract of a letter, which he had just received from abroad, with relation to the author himself. " Monsieur *Bills* lives at present at *Rotterdam* " in the *English Court* (so is the name of the house, where he lives, in *Rotterdam.*) " He is a Frenchman, speaks French, Low Dutch, and Latin. He hath four bodies, " two men and two women, so anatomised, that he can shew the inward state of a " man's body; for all the parts of the body are in the body, except the guts and " brains, which lie by. He asketh above 100,000 Dutch guilders, and then he will " teach the anatomy out of them to scholars, surgeons, &c. He saith this cost him " not only infinite pains and head-breakings, but that of his own patrimony he " spent to this work 60,000 guilders. He affirms, that *Paris* hath presented him " about 20,000 francs, and *Amsterdam* about 3000 francs for two *corpora*; but he " will not sell them apart, but in that manner as I mentioned. It seems it is very " rare, that he anatomised in such a manner those bodies, and prepared also, that " they can be clearly seen, every thing in his proper place. The like is never yet " done in this world."

Dr. *John Wallis* was likewise a very intimate friend and correspondent with our great philosopher. This excellent divine and mathematician was son of a clergyman, and born at *Ashford* in *Kent* on the 23d of November 1616, and about Christmas 1632 sent to *Emanuel* college in *Cambridge*, where he took the degree of master

[a] General Dictionary, Vol. VIII. p. 250—253.

[a] See Mr. *Boyle*'s works, Vol. VI. p 130.

of

of arts in 1640, and about the ſame time was choſen fellow of *Queen*'s college in that univerſity, and received holy orders from Dr. *Walter Curle* biſhop of *Winchester*. In 1644 he was appointed one of the ſcribes or ſecretaries to the aſſembly of divines at *Weſtminſter*, being at that time miniſter of St. *Gabriel Fen-church* in *London*, whence he removed to the church of St. *Martin Ironmonger Lane*; and in 1649 was made Savilian profeſſor of geometry in the univerſity of *Oxford*; and on the 31ſt of May 1654 took the degree of doctor of divinity. February the 17th, 1657-8, he was choſen *Cuſtos Archivorum* of that univerſity; in which place, as well as in his profeſſorſhip of geometry, he was confirmed at the reſtoration, when he became chaplain to his Majeſty[b]. He died October the 8th, 1703, being eighty-ſeven years, three months, and five days old, and was interred in the choir of St. *Mary*'s church in *Oxford*, where a monument is erected to his memory, though the moſt valuable one is the collection of his own writings, publiſhed together in Latin at *Oxford* in 1699, in three volumes folio. His early regard for Mr. *Boyle* is evident from his dedication to him of his book *de Cycloide & Corporibus inde genitis*, with this inſcription; *Honorabili doctiſſimoque Viro D. Roberto Boyle armigero, tum illuſtri familiâ, tum magnis virtutibus nobili*; and in his dedication he takes notice of Mr. *Boyle*'s eminent ſkill in divinity, in the ſacred and ancient, as well as modern languages, and in political and publick affairs both at home and abroad; and his inceſſant and ſucceſsful cultivation of true philoſophy by experiments of all kinds, *&c.* *Qui præter tuam in theologicis tum cognitionem tum praxin, atque in linguis tum ſacris antiquiſque, tum modernis etiam peritiam, in politicis item & negotiis publicis, & rerum & perſonarum, domi peregréque, intimam notitiam,*

Qui mores hominum multorum noris & urbes;

in phyſicis etiam & veræ philoſophiæ venatione ita perpetuis & ſubtiliſſimis experimentis, medicis, chemicis, anatomicis aliiſque omne genus, naturam quaſi ferro & igne proſequaris, (nedum per omnes ſubtiliorum in quacunque arte opificum officinis) in ſecretos abditiſſimoſque ſequeris receſſus, & quaſi in viſcera penetras, ut mirum ni tibi tandem ſe in prædam dedat: quique naturam, tanquam equuleo impoſitam, ſeverâ ſaltem, ne crudeli dicam, quæſtione, torquendo vexas & retorquendo, quo verum tandem faſſa ſecreta pandat omnia: idem, in matheſi, æquationes analyticas (equuleum mathematicum) verſas, quo vix ſubtilius aliud inſtrumentum abdita perquirendi aut extricandi involuta.

In the ſame year 1659, Mr. *Boyle* being acquainted with the circumſtances of the learned Dr. *Robert Sanderſon*, afterwards biſhop of *Lincoln*, who had loſt all his preferments on account of his attachment to the royal party, he conferred upon him an honorary ſtipend of fifty pounds a year. This ſtipend was given as an encouragement to that excellent maſter of reaſoning, to apply himſelf to the writing of *Caſes of Conſcience*, 1. "Explaining the nature of conſcience, the obligation of it, how far "that extends, and the reaſons of it, with ſome univerſal principles and general "rules concerning it, *in theſi*; and then, 2. *in hypotheſi*, to fall to the reſolving par-"ticular caſes[c]." Dr. *Sanderſon* therefore publiſhed his treatiſe, intitled, *De Obligatione Conſcientiæ Prælectiones decem, Oxonii in Scholâ Theologicâ habitæ Anno Domini* 1647, and addreſſed it to Mr. *Robert Boyle* in an elaborate dedication, dated at *Boothby Pannel* in *Lincolnſhire*, November 22, 1659; wherein he ſpeaks of his patron

[b] General Dictionary, Vol. X. p. 89—101. [c] Dr. *Barlow*'s letter to Mr. *Boyle*, Sept. 13, 1650. Vol VI. p 301. See likewiſe *Iſaac Walton*'s life of biſhop *Sanderſon*, and biſhop *Barlow*'s letter to *Walton*, dated at *London* May 10, 1678, and ſubjoined to *Walton*'s *life of biſhop* Sanderſon.

as much more diſtinguiſhed by his excellent diſpoſitions, love of learning, humanity, piety, and all kinds of virtue, than by his birth and quality: *Cum natalium ſplendore illuſtrem, tum generoſæ mentis indole, amore literarum, humanitate, pietate, & omni virtutum genere multò etiam illuſtriorem.*

UPON the reſtoration in 1660, Mr. *Boyle* was treated with great civility and reſpect by the King as well as by the earl of *Southampton*, lord high treaſurer, and the earl of *Clarendon*, lord chancellor of *England*. And he was ſolicited by the latter to enter into holy orders, not only out of regard to him and his family, but chiefly with a view to the ſervice of the church itſelf, which could not but receive a great ſupport, as well as a powerful example, from one, who would ſoon be raiſed to one of the higheſt ſtations in it. This, as he owned to biſhop *Burnet*, made ſome impreſſion upon him. His mind was even then at three and thirty ſo entirely diſengaged from all the projects and concerns of this world, that as the proſpect of dignity in the church could not move him much, ſo the probability of his doing good in it was much the ſtronger motive. But two things determined him againſt it. One was, that his having no other intereſts with relation to religion, beſides thoſe of ſaving his own ſoul, gave him, as he thought, a more unſuſpected authority in writing or acting on that ſide. He knew, that the irreligious fortified themſelves againſt all that was ſaid by the clergy, with this, that *it was their trade*, and that *they were paid for it*. He hoped therefore, that he might have the more influence, the leſs he ſhared in the patrimony of the church. But his main reaſon was, that he had ſo high a ſenſe of the obligations, importance, and difficulty of the paſtoral care, that he durſt not undertake it; "eſpecially, ſays biſhop *Burnet*[4], not having felt within himſelf *an inward* "*motion to it by the holy Ghoſt*; and the firſt queſtion, that is put to thoſe who come "to be initiated into the ſervice of the church, relating to that *motion*, he, who had "not felt it, thought he durſt not make the ſtep, leſt otherwiſe he ſhould have lied "to the holy Ghoſt. So ſolemnly and ſeriouſly did he judge of ſacred matters." He choſe therefore to purſue his philoſophical ſtudies in ſuch a manner, as might be moſt effectual for the ſupport of religion; and began to communicate to the world the fruits of thoſe ſtudies. The firſt of theſe was printed in 1660 at *Oxford*, in 8vo, under the title of *New Experiments phyſico-mechanical touching the Spring of the Air and its Effects, made for the moſt part in a new pneumatical Engine: written by way of letter to the right honourable* Charles *lord viſcount* Dungarvan, *eldeſt ſon to the earl of* Corke: dated at *Beconsfield* December the 20th, 1659. In this diſcourſe he diſcovered and demonſtrated the elaſtic power or ſpring of the air, and by this means exploded the notion of a *Fuga Vacui*, and ſhewed, that the ſtrange effects, which were before aſcriped to that imaginary cauſe, ariſe merely from the native ſelf-expanſion of the air. The extent of which elaſtic expanſion he found divers ways to meaſure by his engine, which likewiſe diſcovers the influence which the air hath upon flame, ſmoke, and fire; that it hath none in magnetical operations; that it is probably greatly interſperſed in the pores of water, and compreſſed by the incumbent atmoſphere even in theſe cloſe receſſes: what operation the exſuction of the air hath on other liquors, as oil, wine, ſpirit of vinegar, milk, eggs, ſpirit of urine, ſolution of tartar, and ſpirit of wine: the gravity and expanſion of the air under water: the effect of the air in the vibrations of pendulums, and in the propagation of ſounds: that fumes and vapours aſcend by reaſon of the gravity of the ambient air, and not from their own

[4] Funeral Sermon on Mr. *Boyle*, p. 29. edit. in 4to. and memorandums dictated by Mr. *Boyle* to biſhop *Burnet* concerning his own life.

positive levity: the nature of suction, the cause of filtration, and the rising of the water in siphons: the nature of respiration, illustrated by trials made on several kinds of animals, and the effects of the air in the operations of corrosive liquors. However this work, which was likewise translated into Latin, was attacked by *Franciscus Linus* and Mr. *Thomas Hobbes* of *Malmesbury*; which occasioned Mr. *Boyle* to subjoin to a second edition of it, printed at *London* 1662, in 4to. *A Defence of the Doctrine touching the Spring of the Air against the Objections of* Linus, *wherewith the Objector's* Funicular Hypothesis *is examined*; *and an Examen of Mr.* Tho. Hobbes's Dialogus Physicus de Naturâ Aeris; *with an Appendix (Chap.* VII.) *touching Mr.* Hobbes's *Doctrine of Fluidity and Firmness.* The third edition of the *New Experiments Physico-mechanical* was printed at *London* in 1682, in 4to.

HE produced likewise to the world in 1660, a piece on another subject, which I have observed above to have been finished as early as the year 1648, and was now printed at *London* in 8vo, and entitled, *Some Motives and Incentives to the Love of God pathetically discoursed of in a Letter to a Friend*, or, as it is called in the running title, *Seraphic Love.* In the *Advertisements* prefixed to it we are informed, that this letter was not written single, being but the last of divers, wherein love in general was confessed, justified, and celebrated; the received way of making love was explicated, defended, and opposed; constancy and inconstancy in love were argued for and against; Platonic love was explicated and celebrated; the cure of love was professed and prosecuted; and the controverted points concerning love were discoursed of in a way suited to the several themes, and to the humours and principles of the supposed writers. But all the former papers, drawn up in a compliment to a lady, though very free from the guilt of either licentiousness or profaneness, were judged by Mr. *Boyle* too little serious, either to appear alone, or to accompany the discourse published by him, wherein he had expressed his own opinion, as in the former he but deduced those of imaginary persons. The 9th edition of this book was published at *London* in 1708, in 8vo. It was also translated into Latin.

AMONG the letters to him written in the year 1660, there is one dated Oct. 2. that year from Mr. *Robert Codrington*, a gentleman known by his English versions of *Quintus Curtius*, and *Justin*, the Roman historian, and his other translations from the Latin and other languages. In this letter he mentions his having dedicated a book to Mr. *Boyle* about ten years before, for which he had received a gratuity; and now addresses a Latin poem, which he intended to publish among the rest of his poetical works [e].

ON the 10th of that month likewise Mr. *Robert Southwell*, afterwards a knight, and envoy from King *Charles* II. to the King of *Portugal*, and president of the Royal Society, wrote to him from *Florence*, to inform him, that the Grand Duke of *Tuscany* was extremely desirous of a correspondence with him, that Prince being not only a patron of learning, but also a great master of it himself [f].

THE year following Mr. *Boyle* published at *London* in 4to, *Certain physiological Essays, and other Tracts*, viz. I. *Some Considerations touching experimental Essays in general.* II. *Two Essays concerning the Unsuccessfulness of Experiments, containing divers Admonitions and Observations (chiefly chemical) touching that Subject.* III. *Some Specimens of an attempt to make chemical Experiments useful to illustrate the Notions of the corpuscular Philosophy.* IV. *A physico-chemical Essay containing an Experiment, with some Considerations touching the differing Parts and Redintegration of Salt-Petre.* V. *The*

[e] Vol. VI p. 636, 637. [f] Ibid. p. 296, 297.

History

History of Fluidity and Firmness. In the *Advertisement to the Reader* it is observed, that these tracts were not written near about the same time, (some of them being divers years older than others) nor were they now published in the same order in which they were written. For the first of them, though drawn up about four years before, *viz.* in the year 1657, was not only written after the second, third, and fourth, but after divers other Essays, which Mr. *Boyle* had then lying by him among his papers, it being intended for a kind of introduction to all those treatises, which under several names, but chiefly that of *physiological Essays*, he had then composed. But having, during the confusions before the restoration, so disposed of his papers to secure them, that he could not himself seasonably recover them; and being engaged by promise to some friends to let about half a dozen of his small tracts come abroad into the world by such a time, he was obliged to send them to the press, as they came, some at one time, some at another, to his hands. The greatest part of them are addressed to his nephew Mr. *Richard Jones*, only son to the lord viscount *Ranelagh* by Mr. *Boyle*'s sister. In these *Essays* and *Tracts* he encourages and persuades the making *Experiments*, and collecting *Observations*, and gives the necessary cautions which are to be used in such designs. He imparts a very considerable luciferous experiment concerning the different parts and redintegration of *Salt-petre*; from which he shews, that *motion*, *figure*, and *disposition* of parts are sufficient to produce all the *secundary Affections* of bodies, and consequently that there is no need of the substantial forms and qualities of the schools. In the *History of Fluidity* he shews, that a body seems to be fluid by consisting of corpuscles touching one another only in some parts of their surfaces; whence, by reason of the numerous spaces between them, they easily glide along each other, till they meet with some resisting body, to whose internal surface they exquisitely accommodate themselves; and that the requisites to fluidity are chiefly these; smallness of the component particles, a determinate figure of them, vacant spaces between them, and agitation of them variously and apart by their own innate motion, or by some thinner substance, which shall toss them about in its passage through them. He shews likewise, how a consistent body may be rendered fluid; and having illustrated the doctrine of fluidity by experiments, observes, that the reason why some fluids will not mix with others, is their particular textures, and the peculiar motion of their minute parts. In the *History of Firmness* he informs us, that firmness consists principally in this, that the particles which compose it, being somewhat gross, are also at rest, and have a mutual cohesion, whereby they are rendered unapt to diffuse themselves every way; so that its three principal causes appear to be grossness, quiet contact, and implication of the component parts. He observes, that a juxta-position of parts is not the only cause of cohesion, but that the spring and weight of the air is one great cause; though a juxta-position of the parts of glass seems requisite and sufficient to make such a compact substance, the parts of matter which compose it, being first minutely divided by the fire before their union. He further tells us, that both the figures and textures of the parts of a body may not only contribute to their solidity, but that the interposition of the minute parts of another body may render some liquors solid, and that the addition of a powder only may make a liquor solid: that fluids consist not of parts divisible into fluids, as quantity into quantity: that there is a plastic power inherent in several bodies; and that mixture itself is sufficient to produce putrefaction. These Essays were translated into Latin. The second edition of them was printed at *London* 1669, in 4to, and in it some of the Tracts were enlarged by experiments, and an addition made to it of *A Discourse about the absolute Rest of bodies.*

THESE

THESE *Essays*, together with his *New Experiments*, being sent by Mr. *Hartlib* to Mr. *John Beale*, the latter wrote the following letter concerning them [s].

" THIS day I received the books, so that I cannot so suddenly give you my judgement on them: only this I can say, that if an oracle had offered me my choice of what questions I most desired to be informed, I am well assured, that those that are handled by Mr. *Boyle*, had been the very first matter of my election. He wants a spirit, that takes not pneumaticals to be the spring of life, and weightiest of naturals, and now most considerable, since *Jordanus Brunus* and others have inclined our modern philosophers to approximate air to the chief of the ambient heavens. And for my own part, I have long conceived this ambient air to consist of as many and different ingredients, as our earth or seas, but I never expected in this life to see the diversities so handled and traced out, and the properties so fully discovered; yet this I am inforced to confess before I have seen half of the book. In the other treatises I am informed of the cause of such frequent misconduct in the best kind of medicines, and the strange Protean force of salt-petre, and that nitre, which is so highly stiled by some, as if it were the spirit of the world. For the Corpuscularian philosophy, I had long ago complained to you, that Sir *Kenelm Digby* and some others had said enough of it to make me giddy in their *pro & contra*; and my refuge was lord *Bacon*'s *Circa ultimates rerum frustranea est inquisitio*. But now I see a stay for the light of reason and experience. Neither had I any thing more in chace than the cause of *firmness*, which in lord *Bacon*'s language, (who first awakened my attention to it) is frequently called *consistency*. Being now in my devotions before the oracle, you can expect but few lines, and in these my advice, that you procure them for yourself before they are all gone out of the shops; for I supplicate, that these may rather return to you in cash than in kind. It is my great joy, that Mr. *B.* is so far engaged to give us the rest of his Notes and following Experiments. In these he hath obliged all the intelligent inhabitants of this world, and hath given us hope, that we shall shortly complete human sciences. Some families amongst us have answered to all lord *Bacon*'s votes for *Advancement of Learning*; and this honourable family deserves to be reputed the first college in this university or œcumenical academy."

And in another letter he wrote as follows:

" MY last was very abrupt and concise, because at that time, as I then told you, I was attentive in my devotions before the noble oracle, which I may now, upon the reviewed solemnities of holy oath, stile the most satisfying oracle in my apprehensions, that ever appeared in the converse of mortals on this habitable globe, for the discovery of all the works of nature. To those that have been so tired and wearied as I have been, in the several ways of *Telesius*, *Flood*, *Gassendus*, the Cartesian, and Atomical or Corpuscularian philosophers, and all others that I could hear of ever since I was concerned in the victory of school-sophisters: to those, I say, that have condescended to take deep notice of the insufficiency of language, and conjectures, and ungrounded ratiocinations, and have submitted their patience to the severity of lord *Bacon*'s inquisitions, and, amongst these, to myself, here are

[s] Mr. *Hartlib* sent a copy of that letter to Dr. *Worthington* inclosed in one from himself, dated Aug. 26, 1661, communicated to me by Mr. professor *Ward*.

[t] offered

" offered ſuch pleaſing refreſhments, as gain us the reliſh of that *Virgilian* ſimplicity, " which was ſo highly admired by *Scaliger* in theſe verſes;

" *Tale tuum carmen nobis, divine poeta,*
" *Quale ſopor feſſis in gramine, quale per æſtum*
" *Dulcis aquæ ſaliente ſitim reſtinguere rivo.*

" To indulge my own freedom, and a little to practiſe my pen and judgment in the " impartial collections of other men's deepeſt or moſt inticing reaſonings, I had in " philoſophy and theology reduced *Interlocutores* in the manner of *Tully's Oratory* and " his *Academical Queſtions*, to offer their ſeveral ſtrengths, (as you have ſeen a ſpeci- " men pretending to the vindication of a rural life) for which academical ſcene I had " long ago provided thoſe arguments in theology, which have ſwayed no leſs men " than *Grotius* and the earl of *Briſtol*, and do make the loftieſt cedars ſtagger and re- " volt ſeveral ways, at as much uncertainty, as the variation of the compaſs. And " from all theſe I could procure a concurrence as to the unchangeable oracle for a " holy life. For philoſophical ſatisfactions I did chiefly addreſs to philoſophical ex- " periments, in which I ſeemed to have the beſt overtures of aid from lord *Bacon*; " but of this I complained, that in the progreſs of late years we had not brought " his experiments, or added our own, to any degree of ripeneſs. And this was indeed " my diſcouragement. Now I confeſs I am ſurpriſed with wonder at the preſent " advancement; and I dare promiſe our poſterity, that knowledge ſhall in this fol- " lowing age abound in very great perfection, and to the beſt of noble operations. I " can now no longer forbear to enquire for the return of Mr. *Oldenburg*, which was " promiſed ſome weeks ago. From him I hope to be informed, when Mr. *Boyle* will " be pleaſed to oblige the world with the publication of his other works:

" 1. THAT of *promiſcuous Experiments* mentioned in the *philoſophical Eſſays*, p. 14, " 15, 16.

" 2. THE *Dialogues concerning Heat, Flame, and Fire*, mentioned in theſe *new Ex-* " *periments*, p. 104, 119, and 222.

" 3. THE *magnetical Steams of the Earth*, mentioned *ibid.* p. 120.

" 4. THE *Continuation of Experiments made by the pneumatical Engine.*

" AND this we may well diſcern, that nothing can drop from his pen, that doth " not oblige the future ages univerſally; neither can preſent envy appear againſt ſuch " bright luſtre. I pray you be mindful to lay this load upon Mr. *Oldenburg*, for it " enables him to oblige us in that ſweet entertainment of hope and expectation, which " eaſeth all kinds of griefs. The note of animals in vinegar, *Phil. Eſſays*, p. 40. may " ſhew what conſiderable uſe may yet be made of our beſt magnifying glaſſes, if the " overtures were well proſecuted."

THE next work, which Mr. *Boyle* publiſhed in 1661, was his *Sceptical Chemiſt: or Chemico-phyſical Doubts and Paradoxes touching the Experiments, whereby vulgar Spagiriſts are wont to endeavour to evince their Salt, Sulphur, and Mercury, to be the true Principles of Things:* Oxford, in 8vo. To the ſecond edition, printed at *Oxford* 1679 [h], in 8vo, are ſubjoined *divers Experiments and Notes about the Producibleneſs of chemical Principles.*

IN this treatiſe he obſerves, that the number of the *Peripatetic Elements* and *chemical Principles* is doubtful; and that the words *Element* and *Principle* are here uſed as

[h] The title-page erroneouſly ſays 1680. See the *Advertiſement.*

equivalent

equivalent terms, and signify those primitive and simple bodies, of which the mixed ones are said to be composed, and into which they are ultimately resolved: that the matter of all bodies was originally divided into small particles of different shapes and sizes; and that these particles might unite into small parcels not easily separable again: that a great variety of compounds may arise from a few ingredients: that various substances are obtainable from bodies by fire: that fire is not the true and genuine analyser of bodies, since it does not separate the principles of a body, but variously compounds and alters the texture of a body, which it acts upon: that some things obtained from a body by fire were not its proper ingredients: that all compound bodies differ only in some mechanical properties: that it does not appear, that three is precisely and universally the number of the distinct substances or elements, into which all mixed bodies are resolvable by fire: that some bodies afford more than three principles: that earth and water are to be reckoned among the chemical principles, and at least one certain alkaline, if not also an acid spirit. He censures the ambiguity of the chemical writers, and shews, that the chemical principles are dissimilar in their nature. He enquires, whether the five chemical principles should be rejected for not being homogeneous; whence the notion of these principles arose; whether fire be necessary in the composition of bodies; and whether there be any elements at all. He shews, that bodies are not composed from all the mere elements, by the growth of bodies, vegetables, animals, minerals and metals, and the analysis of bodies: that the *tria prima* seem to be the productions of fire: that neither phlegm nor earth are universal and pre-existent ingredients of mixed bodies: that gold itself is destructible, and new qualities producible in bodies by a mere change of texture: that chemical principles are transmutable, as appears from the phænomena afforded by an essential oil in repeated distillations: that those principles are not ingenerable and incorruptible: that salts may be produced and destroyed; and that alkalies are transmutable into other substances, and common salt, different kinds of spirits, sulphurs, mercuries, phlegm, and earth are producible. He informs us, that the chemical theory of qualities is narrow, defective, and uncertain, supposing things not proved, often superfluous, and frequently contradicting the phænomena of nature. That the principles found in bodies are not the cause of their qualities, but that contrary qualities are ascribed to the same body: that, upon the whole, the chemist's salt, sulphur, and mercury, themselves are not the first and most simple principles of bodies, but rather primary concretions of corpuscles or particles more simple than they, as being endued only with the first most radical and most universal properties of simple bodies, bulk, shape, motion, or rest, by the different conventions or coalitions, of which minutest portions of matter are made those differing concretions, which chymists call their principles. He then makes some reflections upon the hypothesis of alkali and acid; and shews, that the supposition of them is precarious, their offices arbitrary, and the notion of them unsettled; that the taste is no judge of them, and the hypothesis of them often needless and insufficient, affording but unsatisfactory solutions of phænomena. In a marginal note on the *Preface introductory* in the second edition, he mentions some other *Dialogues* concerning *heat*, *fire*, *flame*, &c. which had been seen by two secretaries of the Royal Society, but were missing, with some other pieces of his, after the hasty removal of his goods by night in the great fire of *London*. This work was translated into Latin.

IN 1662 a grant of the forfeited impropriations in the kingdom of *Ireland* was obtained from the king in Mr. *Boyle*'s name, though without his knowledge; for the application of which to the support of religion and promotion of learning he was

 greatly

greatly solicitous, as appears from a letter of his to Dr. *Michael Boyle*, bishop of *Corke*, dated May the 27th that year [i]; in which he mentions, that he expected from an honest gentleman in public employments, and very well versed in the affairs of that country, an account, " how those impropriations, which are, *says he*, within " the grant made me by his Majesty, are, or are like to be, provided of ministers " and maintenance. And though the procuring me this account would, I know, " require some time; yet the requisiteness of it to assist me, how to order myself on " this unhappy occasion, made me unwilling to write to your lordship, till I had re- " ceived it; and the adjournment of the parliament made me hope I might receive " it seasonably enough. But being surprised since the last post with the news of the " parliament's being met again, and of the quick progress that is making in the " act for the settlement of *Ireland*, the promise I made your lordship at our " separation forbids me to delay to say something to you about the business we " then discoursed of; but it is now too late at night, that I must not till a fitter op- " portunity enlarge upon particulars, but must make haste to inform your lordship " that having, as I told you I intended, applied myself to that excellent prelate the " bishop of *Lincoln* [k], for his advice on so nice an occasion, I came upon the whole " matter to this result, that the king's grant made to me having preceded both the " grant made in the act to the church, and even the church's application to his " Majesty for that grant, I might justly challenge the impropriations mentioned in " my proviso. . . . . And therefore I would not have what I intend to do, reflect " upon those persons of honour, that (though unknown to me) made use of my name " to obtain the grant from the king; since in such disputable cases, persons that act " very differently from one another, may yet all satisfy their own judgments and " consciences in their proceedings. But in regard my intention in general was to " apply an addition of my revenue, if my friends procured any for me, to good " uses, though, I confess, I designed it rather for the advancement of real learning, " than to any other purpose; yet since it so falls out, that unknown to me it is cast " upon impropriations, it is very likely, that by the account I expect of the state of " them, I may see cause to make the more immediate service of religion (by re- " lieving the poor in those places, and contributing, if need be, towards the main- " tenance of ministers there or elsewhere, or promoting other good works, as from " time to time occasion may require) the principal of these good uses, to which I " have thoughts to apply about two-thirds of what will, *de claro*, come to me out of " my share of this grant, hoping also, that the other persons of honour; that are " concerned in it, will have a care, that the impropriations, whose revenues are " given them, be provided with ministers. And I should possibly employ the other " third part also the same way, but that his majesty has been pleased, without my " seeking, or so much as knowledge, to appoint me governor to a corporation for " the propagating of the gospel among the heathen natives in *New England* and " other parts of *America*. And this corporation being at great charges for necessary " works, and especially for the translating and printing of the Bible in the Indian " tongue, above one-half of their revenue is injuriously detained from them by a " person, who had sold it to a corporation erected for the same purposes with ours " under the late usurping powers, and now has repossessed himself of his land, be- " cause those that he sold it to, were not legally qualified to have a sale made to " them. By which means so pious a design, as is pursued by this corporation, is

[i] Vol. VI. p. 56.

[k] Dr. *Robert Sanderson*.

" now

" now in danger to miscarry for want of maintenance; so that the work being so " charitable, and I having a peculiar call to promote it, I think, after having ad- " vised with the bishop of *Lincoln* in the case, that it becomes me, on such a junc- " ture of circumstances, to apply the other third part, or thereabouts, of what the " king's grant will yield me for six or seven years at least, to the carrying on of so " unquestionably good a work; so that the main benefit I intend to derive from the " king's bounty is the opportunity of doing some good with what, if my friends " had not obtained it, might have been begged by others, who would have " otherwise employed it. And by this, I hope, your lordship will be confirmed in " the belief of what I formerly assured you of, namely, that had I known, that " any thing was asked for me, whose grant would have been prejudicial or unwel- " come to the church, I should not have consented to have my name made use of " for a much greater matter, than the proviso is like to yield me. And the same " day your lordship did me the honour to inform me of the state of the case, I offered " some of the parties concerned to relinquish the grant, though it had already passed " the king and council, so I nor my name might have nothing to do with it; but one " of the interested parties being too far off to be consulted, the proffer was declined. " From all which, I hope, your lordship will do me the right to conclude, that I " was far from designing the prejudice of the church; and that though it cannot " in reason be expected, that I should do any thing to hinder the other persons con- " cerned in my proviso to pursue the having it passed, and to enjoy the benefit of it; " yet I do not at all desire, that either your lordship, or any other person, that thinks " the proviso may be justly excepted against upon the church's behalf, should, for " my sake, at all abstain from acting and speaking, as his judgment and conscience " shall direct him. For as my partners are (and I think not causelessly) confident " of being able to carry on in *Ireland* that, which has already passed the scrutiny " here; so, for my part, all I pretend to by this grant is, to have the power in my " own hands, to see, that the proceed of it be carefully employed to the good uses " whereto I design it." The bishop of *Corke* in his answer to this letter of Mr. *Boyle*, dated at *Dublin* Aug. 13, 1662, observes[1], that with regard to the business abovementioned, he was perfectly satisfied, that Mr. *Boyle*'s design, " (if, *says he*, " you had any, for I cannot yet imagine you to be any otherwise than passive in the " whole matter) in acquiring those impropriations was charitable and religious; for " you cannot do any thing that is otherwise. I must likewise very much applaud " your pious intendments for the advancement of real learning, and especially for " the extensiveness of your charity towards the poor heathen natives of *New Eng-* " *land*. But yet I must humbly take leave to acknowledge myself unsatisfied, why " that additional revenue, which his majesty designed and promised for the better " support of the clergy here, that they may with greater comfort attend the cures " of their several churches, should be diverted to any other use, though in itself it " be generous and handsome, especially when I consider, how many congregations " depend upon the service of those parishes; how destitute they will be of a church " to resort unto; how the poor people will be compelled to wander through the " country, to find out an opportune place for the performance of their public duties " and devotions unto God. For although at present your piety provides all that " possibly you may, to prevent these sad consequences; yet you know not how they " shall be minded, who succeed you; nor are you certain, that those other persons

[1] Vol. VI. p. 638.

" (whoever they be) that have obtained this grant under your patronage, will be as " inclinable upon that account as you are. If they be not (as it is great odds on " your side against most men living) it will not then be unworthy your consideration, " whether something of their fault will not be chargeable upon you, by whose only " means they were enabled to do the prejudice. As to that advice and resolve, " which was given you by that most excellent prelate, whose piety and learning " makes him deservedly honoured by all that have either known or read him, I " must necessarily believe it was founded on a very great mistake. For in case the " difference be otherwise than it is stated in your letter, if that blessed Martyr *Charles* " I. did in his life-time solemnly devote all impropriations unto the church, that " should be any way invested in him; if his most gracious majesty, that now is, " should, as an early evidence of his gratitude to God for his miraculous restoration, " immediately after his access into his kingdom, engage himself in the like bounty " to this church of *Ireland*; if his majesty had not only promised, but actually " granted them under his privy signet, before any concession of what your grant " contains was made unto you (whereof each particular may, as I suppose, be made " somewhat conspicuous) then certainly you will think it worthy some further consi- " deration, whether you will insist upon that grant or no."

MR. *Boyle* in his letter of May 27, 1662, above cited, mentions his having been appointed by the king governor of the *Corporation for propagating the Gospel in* New England, *and the parts adjacent in* America, and that the revenue of this corporation was injuriously detained from them. The case was this: in July 1649, there was an ordinance passed in the parliament, *for the promoting and propagating of the Gospel of Jesus Christ in* New England, by the erecting a corporation in perpetual succession, to be called by the name of *the president and society for the propagation of the Gospel in* New England, " to receive and dispose of monies in such manner, as shall best and " principally conduce to the preaching and propagating the gospel amongst the " natives, and for the maintenance of schools and nurseries of learning for the edu- " cation of the children of the natives; for which purpose a general collection was " appointed to be made in and through all the counties, cities, towns, and parishes " of *England* and *Wales*, for a charitable contribution, to be as the foundation of so " pious and great an undertaking [m]." With the monies thus collected lands were purchased to the value of between five and six hundred pounds a year [n], and settled in a corporation of citizens of *London* in trust, *Henry Ashhurst*, Esq; being their treasurer. Upon the restoration of king *Charles* II. the corporation being dead in law, colonel *Bedingfield*, a papist, who had sold an estate of 322 *l. per ann.* [o], which had been settled for the uses of it, repossessed himself of it, and at the same time refused to pay back the money which he had received for it. Mr. *Boyle* therefore used his interest with the lord chancellor *Clarendon*, to prevent that act of injustice; and the corporation being revived in 1661, by an express charter [p], he was made go-

[m] Account of the Society for propagating the gospel in foreign parts, established by the royal charter of King *William* III. p. 4. edit. *London*, 1706, in 4to.

[n] Mr. *Daniel Neale*'s history of *New England*, Vol. I. Chap. VI. p. 263. *who observes, that Mr.* Baxter *in his* Life, fol. 290. *is mistaken in computing that purchase at seven or eight hundred pounds* per ann.

[o] *Neale, ubi supra.*

[p] The copy of the charter is extant among Mr. *Boyle*'s papers, and not being, as far as I can learn, hitherto printed, is inserted in the *Appendix* to this Life.

vernor

vernor of it, and the estate, which had been detained by *Bedingfield*, was restored to the corporation by the chancellor's decree [q].

IN 1663 the Royal Society being incorporated by King *Charles* II. by letters patents, dated the 22d of April, Mr. *Boyle* was appointed by the charter one of the council of the learned body [r]; and as he had been one of the principal persons, to whom that society owed its first rise and progress, he continued during the rest of his life one of its most useful members.

IN June the same year he published at *Oxford*, in 4to, *Some Considerations touching the Usefulness of experimental natural Philosophy, proposed in a familiar Discourse to a Friend by way of Invitation to the study of it.* The *first Part* contains five Essays: I. Of the Usefulness of experimental Philosophy, principally as it relates to the mind of man. II. Of the same. III. A continuation of the same. IV. A requisite Digression concerning those that would exclude the Deity from intermeddling with Matter. V. Wherein the Discourse interrupted by the Digression is resumed and concluded. The *second Part* consists of two sections; *viz.* Section I. of the Usefulness to Physic, containing five Essays, *viz.* 1. Some particulars tending to shew its Usefulness to the physiological Part: 2. To the pathological part of Physic: 3. To the semeiotical part of Physic: 4. To the hygieinal part of Physic: 5. To the therapeutical part of Physic. This book was reprinted at *Oxford*, in 4to, the year following. In the advertisement to the reader Mr. *Boyle* observes, that divers parts of this work were sent to the press in 1660 and 1661, as well as in 1663, and that the very last Essay of it was written several years before; since which time those papers were left, sometimes in the hands of friends, and sometimes in distant places, where he could not come at them; a circumstance which he mentions, that the reader might neither wonder nor blame him, if he should meet with some things in them, that had already been published by others, or were more vulgarly known, than his mentioning them implied; and that particularly a great portion of the *first Part* was written about ten or twelve years before, when he was scarce above twenty-one or twenty-two years of age.

HIS next book was *Experiments and Considerations touching Colours: first occasionally written, among some other Essays, to a Friend, and now suffered to come abroad as the beginning of an experimental History of Colours.* To this were added, *A short Account of some Observations made by Mr.* Boyle *about a Diamond, that shines in the dark; first inclosed in a Letter written to a Friend, and now, together with it, annexed to the foregoing Treatise, upon the score of the affinity between* Light *and* Colours: *and Observations made October* 27, 1663, *about Mr.* Clayton's *Diamond, and read before the Royal Society the day following.* This book was printed at *London* 1663, in 8vo, and reprinted there in 1670. It was likewise translated into Latin. In this discourse concerning *Colours*, he observes, that they may be considered either as a quality residing in the body, that is said to be coloured, or to modify the light after such or such a manner; or else as the light itself, which so modified strikes upon the organ of sight, and so causes that sensation which we call *Colour*; but that this latter is the more proper acceptation of the word *Colour*: that it is not without reason, that he ascribes colour chiefly to the superficial parts of bodies: and that a blind man at *Maestricht* in the *Low Countries* could at certain times distinguish colours by the touch of his fingers.

[q] Bishop *Burnet*'s memorandums concerning Mr. *Boyle*'s Life, and Mr. *R. Baxter*'s Funeral Sermon for *Henry Ashhurst*, Esq; p. 46.

[r] *Sprat*'s history of the Royal Society.

As to particular colours, he tells us, that *whiteness* depends upon the roughness of the surface of the body called white, which gives it innumerable small superficies, which acting like so many *specula* in various positions, reflect the rays of light, that fall on them, not towards one another, but externally towards the spectator: that blackness results from a peculiar kind of texture of the superficial particles of a body, by which it damps the light, which falls on it, so that very little is reflected to the eye. He gives us likewise several experiments, which shew, that both black and white may be mechanically produced and destroyed.

He gave the public also the same year *Some Considerations touching the style of the Holy Scriptures, extracted from several Parts of a Discourse concerning divers particulars belonging to the Bible, written divers years since to a Friend:* London, in 8vo, and reprinted there in 1675 in the same form. A Latin translation of it was printed at *Oxford* in 1665. The *Discourse*, from which these *Considerations* were extracted, was the *Essay on Scripture*, of which I have given some account above; and it was published by Mr. (afterwards Sir) *Peter Pett*, attorney-general for *Ireland*, to whom the letter of Mr. *Boyle* prefixed to it is addressed, and who was meant by the letters *P. P. A. G. F. J.*[a]. It was upon occasion of these *Considerations*, that Mr. *Boyle* received from an anonymous hand the following letter:

October 26, 1665.

" Heavenly *Aretaphilus*,

" A LITTLE book, under the title of *Considerations on the Style of the Holy* " *Scriptures*, coming to my hands, the value your writings merit from all " men, engaged me in its perusal; the sympathy of which contents with my genius " makes me challenge so near a relation to the author, as to style myself his admirer. " I have observed not only the insignificant tautology, the unpleasant circumstances, " the inconsiderableness of the design, and the emptiness of the end of most books, " which crowd daily to the press, wresting and censuring the scriptures; but also the " danger of several tenets, which the authors would make seem agreeable to the " scriptures, merely to proselyte men to their opinion, who are apt to be in love with " their rhetoric, and are thereby seduced to their own damnation, that I almost fear " to read any book with the title of holy. I presume, if men did consider the " weight of that text, *Whoso adds, or diminisheth*, &c. there would not appear so " many comments and large volumes in public, to confound and mislead the multi- " tude, who are apt enough to schismatize, where they have a patron, *viz. Paul*, or " *Apollos*, or *Cephas*, &c. and to turn to the crooked way of destruction, when that " little book, which affords matter for those great volumes, and points to the streight " path of salvation, is neglected and slighted. But *nitimur in vetitum*, we are too " prone to forbidden customs; yet I hope this *Enchiridion* may prove a remedy or " antidote to some against the public infection. In the mean time I rejoice, that " there is one who has that esteem for so worthy and profitable a book as the Bible, " without consorting with men's pretended helps and additions, concluding with St. " *Paul*, that it is *alone able to make the man of God perfect*. And now pardon this " trouble, and accept it, till God grant me an opportunity of a nearer conference,

[a] See Sir *Peter Pett*'s dedication to lord *Altham* before the Memoirs of the right honourable *Arthur* earl of *Anglesey*, edit. *London* 1693, in 8vo.

" that

" that I may improve my talent of knowledge, and press towards the mark of all
" true Christians,

" Yours.

" *To the honourable Mr.* Robert Boyle, *at lady*
" Ranelagh's *in* Pall-Mall, London.

IN 1664, Mr. *Boyle* was elected into the company of the Royal Mines [t]; and in September that year received the following letter from *John Winthrop*, Esq; governor of *Connecticut* and *New Haven* in *New England:*

" Honourable Sir,

" A BARK of this place meeting with one of the frigates at sea near the port of
" their arrival, I had thereby the favour of your honour's letter, and after-
" wards met that gentleman Dr. *Sackvill*, recommended therein, at *Gravesant*, a small
" town upon the west end of *Long Island*, and had there some acquaintance with him.
" But that being a time and place of much action, and my stay no longer than that
" the fort and town at *Manhatos* was reduced to his majesty's obedience, I could not
" then have much conference with him. He abideth yet at that place (now called
" *New York)* which is above a hundred miles from my present habitation; but I
" hope there may be better opportunities, and I shall be ready to do him any ser-
" vice. After the reducing of that place, and that I saw it in the peaceable posses-
" sion of his majesty's honourable commissioners, I came back towards *Hartford* to
" have met the commissioners of the colonies there, it being their usual appointed
" time, and that the place for this year; but being taken with a fever at *New Haven*
" in the way homeward, I was disappointed of being with them at that time; but
" understanding after from those that supplied in my absence there, that there was a
" proposition in your honour's letter to them for their advice, how a great stock
" might be employed for the furthering that good work among the Indians, although
" that question I suppose hath been answered by the commissioners then met; yet I
" am bold to add this motion, that your honour would please to cause that paper,
" which I left with the honourable corporation in *England*, to be reviewed. I sup-
" pose it doth give an hint of a foundation of an useful employ of a large stock,
" even to those good ends the corporation principally aim at. I am very deeply ob-
" liged to your honour for multiplied favours, and in particular this of late received,
" of the good advice, and intimations, and informations, in reference to those
" matters in your honour's letter. I do endeavour greatly to attend your commands,
" and to dispose all people to that duty and observance towards those honourable com-
" missioners sent by his majesty, as may testify their true loyalty and affection to his
" majesty, from whom they come, and I hope for the good of these poor plantations.
" I shall not give your honour further trouble at present, than that you will please to
" read, that I am in sincerity,

" Honoured Sir,

Hartford in *New England*,
Sept. 25, 1664.

" your most faithful humble servant,

" J. WINTHROP."

[t] Mr. *Oldenburgh*'s letter to Mr. *Boyle*, dated at *London* December 10, 1664. Vol. VI p. 185.

THE

THE year following Mr. *Boyle* gave the public his *Occasional Reflections upon several Subjects: whereto is premised a Discourse about such kind of Thoughts:* London, 1665, in 8vo. reprinted in 1669, in 8vo. and translated into Latin, but never published in that language. This piece was drawn up when he was very young, as appears from the following passage in a letter of his to his sister the lady *Ranelagh*; *Nor shall I disobey your threatened order for looking you out a mislaid scribble I drew up* in my infancy *concerning occasional Reflections.* And this consideration, added to that of the custom of the age, in which the imagination was more indulged upon important subjects, than the severity of a true taste of writing will admit, may serve to apologise for this treatise, against the insult upon it in Dr. *Swift's pious Meditation on a Broom Staff*, who has certainly not shewn in that piece a just regard to the interests of religion, any more than to the character of Mr. *Boyle*, by allowing himself to treat such subjects, and so excellent a person, with the most licentious buffoonery. The same year he published some small pieces in the *Philosophical Transactions*, viz. *An Account of a very monstrous Calf*, N° I. p. 10. together with *an Observation* imparted to him by Mr. *David Thomas* touching some particulars further considerable in that monster, N° II. p. 11. *Observations upon a monstrous Head of a Colt*, N° V. p. 85. *Some anatomical Observations of Milk found in Veins instead of Blood, and of Grass found in the Wind-pipes of some Animals*, N° VI. p. 100. *Of a place in* England, *where, without petrifying Water, Wood is turned into Stone*, Ibid. p. 101. *A further Account of an Observation abovementioned about white Blood*, Ibid. p. 117. But a more important work was that printed at *London* 1665, in 8vo. under the title of *New Experiments and Observations touching Cold; or an experimental History of Cold begun. To which are added, an Examen of Antiperistasis, and an Examen of Mr.* Hobbes's *Doctrine about Cold. Whereunto is annexed an Account of Freezing* * *brought into the Royal Society by the learned Dr.* C. Merret, *a fellow of it. Together with an Appendix containing some promiscuous Experiments and Observations relating to the precedent History of Cold.* The second edition was printed at *London* 1683, in 4to. In this work he gives an account of the defectiveness of common weather glasses, the advantages of the new hermetical thermometers, and an enquiry concerning the cause of the condensation of the air, and ascent of water by cold in the ordinary weather-wisers. But these are only preliminaries; for the main discourse presents us with an account, what bodies are capable of freezing others, and what of being frozen: the ways to estimate the degrees of coldness: how to measure the intenseness of cold produced by art beyond that employed in ordinary freezing: in what proportion water will be made to shrink by snow and salt: how to measure the change produced in water between the greatest heat of summer, the first degree of winter cold, and the highest of art: how to discover the different degrees of coldness in different regions: a way of freezing without danger to the vessel: what may be the effects of cold, as to the preserving or destroying the texture of bodies. whether specific virtues of plants are lost through congelation, and then thawing: whether electrical and magnetic virtues are altered by cold: the expansion and contraction of bodies by freezing: how they are caused, and how their quantity is to be measured: the strength of water freezing, and an enquiry into the cause of that prodigious force: the sphere of activity of cold: how far the frost descends in earth and water: an experiment shewing, whether cold can act through an hot medium: a way of accounting the solidity of ice, and the strength of the adhesion of its parts: what liquors are its quickest dissolvents; and an experiment

* Made in December and January 1662.

of

of heating a cold liquor with ice. He afterwards confutes the vulgar notion of *Anteperiſtaſis*, and Mr. *Hobbes*'s Doctrine of Cold; and obſerves, that though he will not undertake to prove the nature of cold to be privative, yet he thinks it eaſy to ſhew, that the arguments produced for its being poſitive are not concluſive.

His excellent character in all reſpects had gained him ſo high an intereſt with the King, that about Auguſt the ſame year 1665, he was nominated to the provoſtſhip of *Eton* college, then vacant by the death of Dr. *John Meredith*, a poſt of great honour as well as profit. However he declined accepting it, as well becauſe he was probably apprehenſive, that the courſe of his ſtudies, to which he had ſo many years devoted himſelf, might receive ſome interruption from the duties of that poſt, as on account of his inſuperable diſinclination to entering into holy orders, which is thought neceſſary to that office [v]. But whatever other reaſons might concur with this laſt, to determine his refuſal of the provoſtſhip, the fact of his nomination to it is evident from a letter of Mr. *Oldenburg* to him, dated at *London* Auguſt the 29th, 1665 [w], in which he ſays: "If he [Dr. *John Beale*] be not miſinformed by one of his majeſty's "chaplains, who now waits at *Saliſbury* [x], and laſt week gave him a viſit, you, Sir, "are nominated provoſt of *Eton*. Which if ſo, our ſaid friend hopes, and ſo do I, "you will not refuſe it, conſidering, that that place hath many opportunities of in-"fluence on the univerſities, and is able to oblige many; which ſtrengthens our "hope, that you will accept of it, eſpecially ſince it is, as I underſtand, no burthen "and much accommodation, it being alſo but half a day's journey from thence to "*London*, and not much more to *Oxford*." And this is confirmed by a letter of Dr. *Beale* from *Yeovill*, Sept. 7, 1665 [y], which begins thus: *I wiſh this may find you in* Eton *college, and well ſettled there.* On the 8th of that month we find him created doctor of phyſic in that univerſity [z].

In February following Mr. *Henry Stubbe*, a phyſician at *Stratford* upon *Avon* in *Warwickſhire*, addreſſed a letter to Mr. *Boyle* upon the ſubject of the famous Mr. *Valentine Greatraks*, the Iriſh Stroker. This extraordinary perſon was ſon of *William Greatraks* of *Affane* in the county of *Waterford*, Eſq; by a daughter of Sir *Edward Harris*, Knt. one of the juſtices of the king's bench in *Ireland*, in the reign of king *Charles* [a]. He was born at *Affane* on the 14th of February 1628, and educated at the free-ſchool at *Liſmore*, where he continued till he was thirteen years of age, and was deſigned the year following for the college of *Dublin*; but the rebellion breaking out in that nation, was obliged, with his mother and ſeveral other ſmall children, to fly for refuge into *England*, where they were relieved by the favour of his uncle Mr. *Edward Harris*, after whoſe death, his mother, for his further progreſs in literature, committed him to the care of Mr. *John Daniel Getſeus*, a German, who was miniſter of *Stock Gabriel* in the county of *Devon*, with whom he ſpent ſome years in ſtudying humanity and divinity. After five or ſix years abſence from his native country he returned thither, but found it in a moſt miſerable ſtate; which made him retire to the caſtle of *Caperquin*, *where I ſpent*, ſays he [b], *a year's time in contemplation, and ſaw ſo*

[v] See Dr. *John Fell*'s Life of Dr. *Richard Alleſtry*, prefixed to the Sermons of the latter, printed at *Oxford* 1681, in folio.

[w] Vol. VI p. 192.

[x] Where the king then was, on account of the plague's raging at *London*.

[y] Vol. VI. p. 390.

[z] *Wood*, *Faſti Oxon*, Vol. II.

[a] A brief account of Mr. *Valentine Greatraks* and divers of the ſtrange cures by him lately performed, p. 1 c. edit. *London* 1666, in 4to.

[b] Ibid. p. 17.

much of the madness and wickedness of the world, that my life became a burthen to me, and my soul was as weary of this habitation of clay, as ever the gally-slave was of the oar, which brought my life even to the threshold of death, so that my legs had hardly strength to carry my enfeebled body above. In the year 1649 he became lieutenant in the regiment of *Roger* lord *Brogbill*, afterwards earl of *Orrery*, then acting in *Munster* against the Irish and Papists; and in 1656 a great part of the army there being disbanded, and he among the rest, he retired to his estate at *Affane*, and was soon after appointed clerk of the peace for the county of *Corke*, and register for transplantation, and justice of the peace [c]. About the year 1662, *I had an impulse*, says he [d], *or a strange persuasion in my own mind (of which I am not able to give any rational account to another) which did very frequently suggest to me, that there was bestowed on me the gift of curing the king's evil; which, for the extraordinariness of it, I thought fit to conceal for some time; but at length I communicated this to my wife, and told her, that I did verily believe, that God had given me the blessing of curing the king's evil; for whether I were in private or public, sleeping or waking, still I had the same impulse. But her reply to me was, that she conceived this was a strange imagination; but to prove the contrary, a few days after there was one* William Maher *of* Salterbridge *in the parish of* Lismore, *that brought his son* William Maher *to my house, desiring my wife to cure him, who was a person ready to afford her charity to her neighbours, according to her small skill in chirurgery. On which my wife told me, there was one, that had the king's evil very grievously in the eyes, cheek, and throat. Whereupon I told her, that she should now see, whether this were a bare fancy or imagination, as she thought it, or the dictates of God's spirit on my heart; and thereupon I laid my hands on the places affected, and prayed to God for Jesus sake to heal him; and then I bid the parent two or three days afterwards to bring the child to me again, which accordingly he did, and then I saw the eye was almost quite whole, and the node, which was almost as big as a pullet's egg, was suppurated, and the throat strangely amended, and, to be brief (to God's glory I speak it) within a month discharged itself quite, and was perfectly healed, and so continues, God be praised.* His next cure was of *Margaret Mac-shane* of *Ballinecly* of the parish of *Lismore*, who had been afflicted with the evil above seven years in a much more violent degree; and soon after his fame encreasing, he cured the same disease in many other persons for three years, not meddling with any other distempers, till about the end of those three years, the ague growing epidemical, he found as formerly, that there was bestowed on him the gift of curing that disease [e]. "Within some small time after "this, *continues he* [f], God was pleased by the same or the like impulse to discover "unto me, that he had given me the gift of healing; which the morning following "I told my brother and wife, but neither of them could be prevailed with to believe "it, though for my own part I had a full assurance thereof within me. This impulse "I had the sunday after Easter-day, the 2d of April 1665 early in the morning; "and the Wednesday ensuing I went to cornet *Dean*'s (about some occasions I had "with him) to *Lismore*, where there came into his house to me a poor man, that with "a violent pain in his loins and flank went almost double, and had also a most grie-"vous ulcerous leg very black, wherein were five ulcers; who desired me for God's "sake, that I would lay my hands on him, and do him what good I could. [But by "the way take notice, that as God gave me the several gifts from time to time, he "always sent patients, that applied themselves to me, for I never sought after any "from the first moment to this very instant.] Whereupon I put my hand on his

[c] Ibid. p. 18, 19. [d] Ibid. p. 22. [e] Ibid. p. 22—25. [f] Ibid. p. 25.

"loins

" loins and flank, and immediately run the pains out of him, so that he was re-
" leased, and could stand upright without the least trouble. Then I put my hand
" on his ulcerous leg (which the chirurgeons, after they had shewed all their skill on
" him, told him it was perished at the bone, and so must be cut off, but that he
" wanted 3 *l.* to give one of them for his pains, as he informed me) which forth-
" with changed colour, and became red, and three of the five ulcers closed up, and
" the rest within a few hours afterwards; so that he went out well, that could hardly
" by the help of his staff crawl in, and within two days afterwards he fell to his
" labour, being a mason by trade, and so continued several months afterwards to
" my knowledge, and to this instant, for ought I know." The Thursday following he cured colonel *Phaire* of *Cahirmony* in the county of *Corke* of an ague, and afterwards many other persons of different distempers by stroking [g]; so that there was a resort to him from all parts. But being cited into the bishop's court at *Lismore*, and not producing a licence for practising, he was prohibited from laying his hands on any person for the future, but still continued to do so till January 1665-6, when he came to *England* at the request of the earl of *Orrery*, in order to cure the lady of the lord viscount *Conway* of *Ragley* in *Warwickshire*, who had for many years laboured under a most violent head-ach. He staid at *Ragley* three weeks or a month; and though he failed in his endeavours to relieve that lady, he cured vast numbers of people in those parts, and at *Worcester*. He was then by his Majesty's order sent for to *Whitehall*, and continued some time in *London*, performing many remarkable cures there in the presence of persons eminent for their skill and integrity [h].

MR. *Stubbe*, who was witness to several of his cures in *Warwickshire*, published therefore at *Oxford* in 4to, a piece, entitled, *The miraculous Conformist: or, An Account of several marvellous cures performed by the stroking of the hands of Mr.* Valentine Greatarick; *with a physical Discourse thereupon, in a Letter to the honourable* Robert Boyle, *Esq; with a Letter relating to some other of his miraculous Cures, attested by* E. Foxcroft, *M. A. and Fellow of* King's *college in* Cambridge. Mr. *Stubbe*'s letter to Mr. *Boyle* is dated at *Stratford* upon *Avon*, February 18, 1665-6, and begins thus: " Since the best and most agreeable retribution I can make you for the honour
" you do me in your remembrances, and all your other signal favours, is but to
" gratify your curiosity with any remarkable intelligence, that may advance either
" physic or philosophy, I shall endeavour to be as generous in my acknowledgments
" to you, as you have always been in obliging me. Since my last unto you, my lord
" *Conway* did me the honour particularly to invite me to his house and acquaintance,
" giving me withal a fair opportunity of conversing with Mr. *Greatarick*, and be-
" holding several of those performances, the report whereof, as it gives just causes
" of astonishment to you that are more remote, so the effects fill with admiration the
" most learned or suspicious beholders. In truth they are such, that he is not at all
" obliged to the ignorant for the esteem he hath acquired, nor is it possible for the
" most tender or superstitious and censorious zealots to destroy his repute. He is a
" man of a graceful personage and presence, and, if my phantasy betrayed not my
" judgment, I observed in his eyes and mien a vivacity and sprightliness, that is no-
" thing common. As far as I could inform myself by a long and private discourse,
" he is a man of a very good life, of tender and charitable principles, as extensive
" as the effects of his goodness are. He professeth conforming unto the doctrine
" and discipline of the church of *England*, yet without that censoriousness, whereby

[g] Ibid. p. 25—28. [h] Ibid. p. 28—40.

" ſome ſignalize themſelves. His thoughts concerning himſelf are modeſt and hum-
" ble; and he preſumes ſo well of others, that even in ſome colourable circum-
" ſtances he regulates his apprehenſions by the revealed mercies of God, and not the
" ſeverity of men. In fine, without any prejudice to this age be it ſaid, he ſeemed
" to me, by his faith and by his charitableneſs, to include in his ſoul ſome grains of
" the golden age, and to be a relick of thoſe times, when piety and miracles were
" ſincere." The poſition which Mr. *Stubbe* maintains in this letter, is[1], that *God had beſtowed upon Mr.* Greatraks *a peculiar temperament, or compoſed his body of ſome particular ferments, the effluvia whereof being introduced ſometimes by a light, ſometimes by a violent friction, ſhould reſtore the temperament of the debilitated parts, reinvigorate the blood, and diſſipate all the heterogeneous ferments out of the bodies of the diſeaſed, by the eyes, noſe, mouth, hands, and feet.* He then endeavours to explain the nature and manner of Mr. *Greatraks*'s working upon his patients for their cure.

BUT ſome paſſages in this letter of Mr. *Stubbe* giving offence to Mr. *Boyle*, the latter wrote, immediately upon the receipt of it, animadverſions upon it, in a letter to him, which having never been yet publiſhed, deſerves a place here from the original in his own hand-writing.

" SIR, March 9, 1665-6.

" IT was ſo late yeſternight before I received your account of Mr. *Greatraks*'s ſtu-
" pendous performances, that I had much ado to run it over before I went to
" bed; and this morning being to take care of ſome little affairs in order to a re-
" move, that I am to make in the afternoon for ſome days, I am obliged to anſwer
" your letter in as much haſte as you tell me you writ it in; which intimation will,
" I hope, excuſe me to you for my not taking a ſolemn notice of thoſe ſuperfluous
" acknowledgments you are pleaſed to begin with, for ſervices that are not conſider-
" able enough to deſerve or expect a public retribution, having been but ſuch, as a
" leſs intereſt in the Muſes than yours would have entitled you to from one, that is
" ſo much their ſervant as I.

" To begin then, I muſt confeſs to you, that I was ſomewhat ſurprized to find
" this epiſtle of yours brought me from the preſs, before I had ſeen it any other way;
" and it is no ſmall trouble to me, both upon your ſcore and my own, that I did not
" ſee the manuſcript before it came abroad. For if I had ſeaſonably ſeen what you
" wrote about miracles, I ſhould freely have diſſuaded you from publickly addreſſing
" to me, what I cannot but much diſſent from; and perhaps I ſhould have been able
" to prevail with you to omit all that part of your epiſtle. For beſides that ſince
Page 25. " you take notice yourſelf of the prejudice your former meddling with theological
" matters has done you, you can ſcarce doubt, but that it has made many perſons
" indiſpoſed to put the beſt conſtructions upon what you write; beſides this, I ſay, I
" confeſs I think you might have ſpared ſo much pains as you take in the former
" part of your letter, to ſhew, that Mr. *Greatraks*'s gift may be miraculous, ſince the
" latter part of it is employed to make out what he performs by natural means.
" For my part, though I be very backward to believe any ſtrange thing in particu-
" lar, though but purely natural, unleſs the teſtimonies that recommend it be pro-
" portionable to the extraordinarineſs of the thing propoſed; yet I remember not,
" that I have hitherto met with (no more than you have done) any, at leaſt any co-
" gent proof, that miracles were to ceaſe with the age of the apoſtles; and not only

[1] P. 10, 11.

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" the excellent *Grotius*, but *Tertullian*, *Justin Martyr*, *Cyprian*, and other ancients tell " us, that the power of ejecting devils out of possessed persons lasted long after that, " and was not unfrequent in the Christian church. And therefore if those relations " of Mr. *Greatraks*'s cures, that I have not yet seen, shall convince me, I shall not " scruple, since his belief and life give me no just suspicions to acknowledge my " conviction, and to rejoice in the appearing of a protestant, that is enabled and for- " ward to do good in such a way, especially in an age where so many do take upon " them to deride all that is supernatural; and, whilst they loudly cry up reason, make " no better use of it than to employ it, first to depose faith, and then to serve their " passions and interests. But by what hitherto appears to me of Mr. *Greatraks*'s " cures, I must take leave to think, that either they are not real miracles; or, if they " have any thing in them of a supernatural gift, it is so far short of the gifts of our " Saviour Christ and his apostles, that I presume your friends will think, that if it " were not the effect of your haste, it was rather to shew your wit than declare your " opinion, that you seem to make a parity between them. And for my part I should " in that case, reflecting upon the passage you cite, that *there are differences of admini-* " *strations, but the same Lord*, think it more fit to look upon this gift of Mr. *Grea-* " *traks*, as a distinct and inferior kind, than degrade the unquestionable miraculous " gifts of the apostles, to depress them to the same level with his. For whether or " no it may question the truth of his gifts being miraculous, yet certainly it will " lessen the degree of it, that there are many diseases which he will not so much as " meddle with; that there are others, which he attempts to cure, but cannot; that " there are others, wherein the good he does is not lasting, so that his patients are " rather relieved than recovered; and that also there are others, wherein so durable " a contact and friction is requisite, as makes a great resemblance betwixt the opera- " tions of his hand, and the actions of physical agents. As to what you say about " *Trophimus*, whom St. *Paul* left sick at *Miletus*, and your supposition, that he may " hence have left many so elsewhere; the latter part of it, if it be taken in a sense per- " tinent to the present case, is precarious; and as to the former, it concludes not, that " St. *Paul* could not cure him, unless you can make it appear, that he endeavoured " it, as you confess Mr. *Greatraks* did to cure the excellent lady *Conway* and others. " What you say, that you believe, that there wanted not at *Corinth* those that had " the gift of healing, nor an effectual anointing with oil; yet did the misdemeanors " of some draw upon several of them *irrecoverable* (a main circumstance, which I " read not in the text) sickness and death, is only affirmed. But if it were granted, " that persons so qualified were in that church, yet it will not follow, that they were " unable to cure those that died, unless you make it out that they endeavoured it. " For, to note this once for all such cases, there is no just cause to suppose, that God " did so vouchsafe the gift of miracles to all, that in those times had it, as that it " was to be exerted on all occasions; but that he who gave it, did likewise by some " way or other signify to them, when it was fit for his glory (for whose service, " and not their own or their friends outward advantages they were intrusted with " it) that their miraculous endowments should be exercised. And to argue from " their not performing what it appears not that they did attempt, to their disability " to perform what they attempted, will be inconsequent. St. *Peter* was able, when " it was requisite, to strike *Ananias* and *Sapphira* with sudden death, and St. *Paul* to " strike *Elymas* with blindness; yet these apostles never made use of this fatal power " for their own or their friends defence, when they suffered those rude violences, " from which the exercise of it would in all probability have protected them. And

In his annotations on the 16th of St. *Mark*.

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" our Saviour himself, though he could turn water into wine (as at the wedding in " *Cana* of *Galilee*) yet we read not, that ever he thought fit to do it but once; nor " can it be well argued from his sending his disciples to buy loaves, that he was not " able miraculously to multiply bread, or feed a greater number than his ordinary " retinue by extraordinary means. As to what you say, that there were some diseases " that the disciples could not cure; I remember not any such thing in the gospel, " unless you mean, as I suppose you may, the youth mentioned in the 17th of " *Matthew*: but he, the story informs us, was *possessed with the devil*; of which pos- " session the bodily distemper (whatever that were) was but an effect, and therefore
Luke ix. 39. v. 42. Chap. xvii. v. 18. " in the beloved physician's gospel, both the paroxysm is described by *a spirit taketh* " *him*, &c. and *the devil threw him down and tare him*; and in the narrative of the cure " it is said, that *Jesus rebuked the unclean spirit*, which in St. *Matthew* is called *the* " *devil*; and is said *to depart out of him*. And as to that part of this story in the gos- " pels, that relates to the *casting out of the devil*, I shall not spend time to discourse " upon it, nor to enquire what was the extent and conditions of the power Christ " gave his disciples, whilst they were, as his usual train, in their attendance upon " himself; yet it plainly appears by their divine master's rebuking them in the fore- " cited chapter of St. *Matthew* for their unbelief, as that which alone had hindered " the ejection of the evil spirit, that it was not want of power, but their culpable " neglecting to use the proper means that left the devil unejected. But when they " were sent forth and commissioned by Christ, not only we never read that they at- " tempted a cure which they could not effect, but their performances were divers of " them of a much higher nature than Mr. *Greatraks*'s. For not only *Peter* and *John* " cured instantly a man that had been lame from his mother's womb; *Peter* cured " in a trice at *Lydda*, one whom the palsy had kept bed-rid eight years together; " *Paul* in the same manner at *Lystra* made one leap and walk, that had been a cripple " from his mother's womb; but the same apostle had so exuberant and diffusive a
Acts xix. 12. " power of chasing, not only diseases, but devils, that he did it at a distance by the " intervention of handkerchiefs and aprons that were brought from his body to the " sick or possessed. To which I should add the cures intimated to be wrought by " St. *Peter*'s shadow, if I did not consider, that it may be said, that to come so near " as to have that pass over them, was to come within the atmosphere (if I may so " call it) which the effluvia of his body made miraculously sanative. To enume- " rate all the other effects of the disciple's supernatural gifts, would be to transcribe " a good part of the Acts of the Apostles. And if I could imagine, that any dis- " cerning men were in danger of thinking, that because you have made it plausible, " that some of your *Thaumaturgus*'s cures are performed by an ἰδιοσυγκρασία, or some " kind of complexional efficacy, I should add, that I think such a suspicion exceed- " ing ill-grounded, because there are divers phænomena in the miracles of our Saviour " and his apostles, that do not at all agree with so injurious an hypothesis as that " would be. For, to repeat nothing of what I have said already, our Saviour could " communicate the power of working miracles to others at his pleasure, (which I " think you do not believe Mr. *Greatraks* can do to you) as in the case of the seventy " disciples; and that many years after his death, as in the case of St. *Paul*; divers " of his miracles were done on absent persons, as that on the Centurion's dying " servant and others. The mute fishes obeyed him, and that so strangely, that " being at a distance one of them brought St. *Peter*, not only himself, but a deter- " minate piece of money. His power reached not only to living creatures, but in- " animate ones, as the sea and the wind; the former of which supported him when

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" he walked on it, and both the former and the latter obeyed him. His power, and " that of his apostles, (which will not, I suppose, be ascribed to a peculiar tempe- " rament, reached not only to the curing of the sick, but the raising of the dead, " (which if the devil could do, he would have far more followers than now he has, " though they be but too many) and divers of the apostles cures were done without " any contact at all but barely by their word; by which also they could do harm " as well as good, and bring diseases and death as well as chase them, which will not, " I presume, be ascribed to an exuberance of health (or *plusquam perfect* tincture, as " the chemists speak). Besides that, we may well be induced to believe, that even " those cures, wherein they did touch the sick, were miraculous, both by other " reasons that may be drawn from what has been already said, and by these two. " The one, that there is no ground to believe, that so many differing and un- " allied persons in one place, and at one time, should have that sanative ἰδιοσυγκρασία, " of which many ages have produced us so few unquestionable instances, if any other " than what your *Thaumaturgus*, supposing his such, affords; for the testimonies you " cite of *Rodericus a Castro* are not cogent, especially considering his religion and " that of his country; and his *creduntur* is an expression that rather argues his diffi- " dence than belief. And the second, that the apostles and their contemporary " workers of miracles, who professed themselves sent by God to promulgate a new " doctrine, had other indowments that were confessedly miraculous, (and therefore " needed not a complexional physical power of healing) as is evident in the gift of " tongues so illustriously poured out on the apostles at the feast of Pentecost. And " to this so much more to the same purpose might be added, that though I have " given some proof of my not being afraid to propose paradoxes if I think them " truths, yet I shall clearly profess to you, that as much as I ascribe to the Corpus- " cularian philosophy, (both in my other tracts, and professedly in a new one, that " this week comes out in favour of it) I am far from believing, that any mechanical " or physical hypothesis will make out those supernatural phænomena, without having " a recourse to the miraculous interposition of God. And when I had the curiosity to " consider the grand opinions that are entertained among men about religion in " general, I have long looked upon those enemies to Christianity as none of the wa- " riest and formidablest, that granting the truth of the historical part of the New Testa- " ment, (which relates to miracles) have gone about to give an account of it by " cœlestial influences, or natural (though peculiar) complexions, or such conceits, " which have quite lost them, in my thoughts, the title of knowing naturalists. But " I must not forget, that the opinion I have been opposing may possibly be dis- " claimed as well by you as me, though I wish some readers do you not the discour- " tesy to take a rise from your epistle to maintain it. And by this time I presume " you expect that I should say something to the historical and physical parts of your " letter, after having insisted so long on the theological. But though to comply with " that expectation and my own haste, I skip other points relating to divinity, and " what were not proper for me to meddle with; yet there are two things that " I must not leave untaken notice of. And first, as for the apology you conclude " with for recommending some physical directions after Mr. *Greatraks* stroking, I " think most of your readers, especially divines, will much more easily forgive a phy- " sician the prescribing of medicines, which is a main part of his profession, than " the irreverent mention you are pleased to make of the language of one of the pen- " men of the Old Testament, and all but one of the New; that mention neither be- " ing required of you as a physician, nor appearing at all necessary to be made in

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" that place; and, but that you wrote in haste, neither would you have published " that, whereby you will needlessly displease many, (and I am confident divers of " your own profession) nor would you of all men have addressed it to me, who have " publickly given cause to think it must be peculiarly unwelcome to me. The other " theological passage not to be overlooked, is that which contains an opinion, which " I think Christianity itself may be so much concerned in, that I cannot forbear, as " I pass along, to take some little notice of it, as by telling you, that in the opinion " you hold as an *undoubted truth, that God had permitted all religions to have their real* " *miracles*, I do very much dissent from you. I have not now time to examine how " far it may be said, that among a people or church, where God has already esta- " blished a doctrine by divine and acknowledged miracles, and has expressly foretold, " (or at least sufficiently intimated) that it is possible he may permit some seducers " to work strange things to try the people, to whom a divine doctrine attested by " miracles has been delivered; I must not, I say, now stay to examine, how far one " may grant, that in such a case, and after such a warning, false prophets may be " permitted to work miracles; though for my part I see no cogent reason, why the " τέρατα ψευδῆ of the man of sin may not by a construction, which we find exceeding " common in Hebrew writings, be rendered (as our English translation seems to in- " terpret) *false wonders*, i. e. miracles, that are as well false themselves, as are the " doctrine they are brought to confirm. But though, as I was saying, I must not " now insist on a debate of what may or may not be granted in the like cases, that I " have proposed; yet speaking as you do indefinitely of all religions, and taking real " miracles for such exertions of God's power, as are above the power of creatures, I " am so very unapt to believe the stories of the Turkish, Heathenish, and other " miracles pretended to by divers enemies of the Christian religion in confirmation of " theirs, and some enquiries I have made have so confirmed me in my diffidence, that " when I consider the nature and use of true miracles, (for I speak not of natural " prodigies, sorceries, or impostures) I confess I am so little convinced, that I ought " to believe the suspicious and unlikely reports that go of the miracles of the Turks " and Heathens, divers of which are said to have been done in times and places " when there were no miraculous gifts in the church to controul them, that perhaps " I should not believe those that I find recorded in the scripture itself, if the rela- " tions of them were not recommended by such concurrent characters of credibility, " as would make my rejecting them an obstinacy, and as are of another guess weight " than those that countenance those relations whereto I do not give credit. And I " little doubt, but if the pretended miracles of *Pyrrhus* and *Vespasian* had been " watched and considered by Mr. *Stubbes* as narrowly as those of Mr. *Greatraks*'s " had been, you would have found at least as much reason to ascribe their cures, as " his, to physical causes, if not to some mistake, collusion, or flattery, which the " persons on whom those wonders are fathered, render the more suspicious. And " though perhaps no body will more willingly grant than I, that the soundness of the " doctrine ought to go along with those miracles that bear witness to religion, yet I " doubt, whether the holiness of doctrine, which may be allowed to show that a reli- " gion may be instituted by God, will, without the assistance of miracles, sufficiently " assure men that it really is so.

" I COME now to the physical part of your letter, wherein many that will dissent " from you about the theological, will, I presume, confess, that divers things are plau- " sibly proposed. And as you will believe, that I am of their mind in the general, so " as to particulars I have not time to enter into a debate of it, especially since you " seem

" seem willing to refer that task to the Members of the Royal Society, who, if they " think fit to meddle with it, are the likeliest persons to give a fair account of it. " But in the mean while to keep scribbling as long as the time will permit, I shall " briefly take notice of some particulars in the casual order, wherein they offer them- " selves to my thoughts, premising only in general, that you write to one that is " almost as backward to acquiesce in the explications of strange things, as to believe " the narratives.

" I AM not yet fully convinced, that there is in what either you or the ingenious " Mr. *Foxcroft* relate upon your own observations, (of which in this whole letter I " do upon your testimony suppose the truth) any thing that is purely supernatural; " (unless in the way, wherein he was made to take notice of his gift, and exercise it, " there may be something of that kind) and therefore till the contrary doth appear; " I hold it not unlawful to endeavour to give a physical account of his cures, and " to enquire whether his touch be any more than a more noble specific, that reaches " not to all diseases, or to most, but to more than the generality of specificks, whose " operations are usually more confined.

" THOSE physicians, and other learned men, that think every disease must be over- " come by distinct and appropriated remedies, that are contrary to the particular dis- " ease upon the account of some known quality, as that a fever being a disease hot " and dry, must be cured by remedies cold and moist, will perhaps think it incre- " dible, that the application of one thing should work so many differing effects, and " cure so many differing distempers. But though I think it is strange and extraor- " dinary, yet I dare not say it is impossible to mere nature. This would be suffi- " ciently manifested, if the relations, that *Helmont* makes of the various and sudden " cures he saw performed by *Butler*'s drif either touched with the tip of the tongue, " or by outward application, were proper to be urged on this occasion. But without " having recourse to them, the strangeness may be lessened, if we consider these two " things; the one, that diseases are not always so differing in their nature and essence " as they are commonly thought, but the same morbifick matter for essence may " produce very differing symptoms, which may be taken for several diseases, accord- " ing to the condition of the parts that it resides in, or works upon, (in all or most " of which it may be subdued by the same remedies, which may destroy its texture, " giving it a more innocent one, or proscribe it) as we see in some metastases of the " morbific matter, which according as it successively affect differing parts has the " appearance of several diseases, as in some of the instances you give, and in hyste- " rical fits, which sometimes counterfeit almost all diseases, and in other examples, " which I have elsewhere taken notice of. The other, that some remedies are of that " noble nature, that their efficacy reaches to diseases that seem of contrary kinds, as " may be seen in the operations of the *Spa* waters, and those which I have in some " lucky seasons observed at our own wells near *Tunbridge*; and we see how many dif- " fering diseases are cured by an excellent air, which keeps me from wondering at that " sentence of *Hippocrates*, γῆν μεταμείβειν ξύμφορον ἐπὶ τῶσι μακρῶσι νεσήμασι. Hippocratis Lib. tertii, Sect. 5ta Popular.

" As to what seems your main hypothesis, that Mr. *Greatraks* performs his cures " by the strengthening and reinvigorating of nature, which being relieved and forti- " fied by the sanative effluvia that pass from his body into the body of the patient; " doth afterwards vanquish the disease herself, I doubt not, that you will have many " of your readers of that opinion with you; and particularly those Helmontians and " other chemists, that hope or plead for universal medicines, operating by way of " restoratives, will be glad to find you to countenance their tenet. For my part, " unless

" unless I could send you what I once drew up by way of disquisition about the " received notion of nature (wherein perhaps I do not acquiesce) I cannot think it " proper to mention to you my particular thoughts of the power of strengthened " nature. But some scrupulous person will not only deny, that nature alone, though " fortified with any thing, that is but a cordial, can cure all diseases (as for instance, " the stone in the bladder, or kidneys, when it is confirmed and grown too big to be " voided by the urinary passages) but will perhaps enquire, whether it does appear " that Mr. *Greatraks*'s touch is positively a cordial, and not barely or chiefly so, as " it relieves nature by freeing her from some distempers that oppressed her, as when " burned feathers remove the faintings of hysterical women, and others are freed " from lypothymias by being pinched, or having cold water thrown in their faces. " But these scrupulous persons (if you meet with any such) will perhaps be more " easily satisfied in other cases than about the cure of wens. For whereas Mr. " *Greatraks*'s touch must, according to the forementioned hypothesis, free the pa- " tient from them, by invigorating nature, and enabling her to discuss or prescribe " those preternatural collections of matter; I must inform you, that to omit *Hel-* " *mont*'s story, I was the other day visited by an eminent physician, who not long " since upon occasion of a fine pendulum clock, that I was taking notice of in his " chamber, told me, that it was presented him by such a one (a well known person, " that has the honour to be one of his Majesty's domestics) for having cured his " daughter of an almost monstrous wen by the lasting application of a dead man's hand. " And such another cure of a wen, or some such tumour, I remember our famous " *Harvey* related to me as performed (if I very much mistake not) by himself, by " stroking the wen, as *Helmont* prescribes, with a dead man's hand. But this advan- " tage you have, that however it will perhaps be very difficult for those, that upon " these, and the like grounds, do reject the strengthening of nature, to pitch, among " the hypotheses that are yet noted, upon a more plausible one, especially since your " great *Hippocrates* is supposed to be of the like opinion when he pronounced that " sentence, Νεοῦν φύσεις ἰητροί. But I hope you will allow me to add (what if I " forget not you yourself somewhere seem to intimate) that Mr. *Greatraks*'s touch " may work not only as a cordial or strengthener of nature, but as a proper remedy " of qualities opposite to those of the causes of the diseases he cures. For I do not see, " why it may not be possible for the sanative, and perhaps anodyne steams of his body " to be of such a texture, that they may both reinvigorate the spirits, and by appro- " priated qualities oppose and subdue the morbific matter or ferment, as we see, that " lemons (to name no other things) have, besides their cordial virtues, the power to " cool, incide, and resist putrefaction, and strengthen the stomach, and promote urine, " *&c.* And when I consider the strange effects that longing has produced in teeming " women, and the sudden and manifest operations I have sometimes known passions " to have upon the body, I am not sure, but that something else may be fit to be " taken into consideration about Mr. *Greatraks*'s cures, and that in some of his " patients the mind by exalted imagination, and by strong passions (which in so ex- " traordinary and supposedly supernatural as well as public way of healing as his, " there are several circumstances that may heighten) may have an interest in the " recovery, by occasioning usually a great and therefore probably sometimes a lucky " commotion in the blood and spirits, upon whose motion (and thereby texture) thus " altered, that the amendment, if not recovery, may sometimes ensue, the obvious, " though unheeded instances of those that are freed from the hiccough, by being told " of some feigned ill news, or even of some other thing, that but excites a great at-

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" tention of mind, and much more the examples, that have been divers times seen " of diseases frighted, or by fits of passion driven away, make me think it not absurd " to suspect.

" WHAT to mention of morbific ferments, I know divers ingenious readers will " approve, and they seem to be of good use in the explication of diseases. But " whether all diseases require ferments; and whether your doctrine about them " be as well applicable to the rest, as to some, is a disquisition, that I shall willingly " leave to those learned men of your faculty, that our age and country abounds " with.

" WHAT you mention of Mr. *Greatraks*'s excision of wens and immediately stanch- " ing the blood, as it much takes off from the appearance of a miracle, so as to the " physical cause of it, it may or may not have a great deal of difficulty, according to " some circumstances, which I would gladly be informed of. For if the wen be " such, or so placed, that it being cut out by a chirurgeon, no such vessels would be " cut as that, according to the common course of nature, the effusion of blood would " be great, it is no great wonder it should stop after the application of his finger, " that perhaps compresses the orifice of the vessels; and the marvel is confined to the " sudden cessation of the pain, which may be attributed to some anodyne effluvia " issuing from his hand. But if in the extirpating of the wen any great vessels be " cut asunder, the stanching of the blood seems to be more difficult to be accounted " for by supposing with you, that his cures are performed by restoring the tempera- " ment of the debilitated parts, and reinvigorating the blood. For though this be " granted, it will not be so clear, that the blood should by his touch be so quickly " stanched, since in a person, that were perfectly healthy, and where the blood " needed no invigoration, upon such a solution of continuity there would be a large " effusion of the continually circulating blood, unless appropriated means be used to " stay it, as is daily seen in the wounds received by healthy men. The observation " of stanching of blood out of *Platerus* I well remember, as having had occasion " to make use of it, together with an experiment of his own, that he annexes to it, " of a person, that he himself cured by the same means. But I doubt, whether " these will reach your case, till you have made out the cause of this operation. For, " that there may be very differing applications, that will stanch blood, may be learned " from him in the same place, where he relates how he made a chirurgeon stanch a " dangerous hæmorrhage, not by the application of a bloody part of a hen, but of " aqua fortis, which is corrosive instead of being balsamical. If indeed there be any " truth in the strange relations I have met with in some navigators of an Indian " simple, that worn by a great commander, kept his wounds from bleeding (which " what I elsewhere mention of the effect of the moss of a dead man's skull upon a " noble friend of mine, does, though not approach, make the less incredible) it may " be more plausibly argued, that Mr. *Greatraks*'s finger may in this case operate as a " natural agent; but then there will remain some little scruple, whether the effect be " wrought in the particular way you seem to pitch upon, till it appears, that these " things work either as restorers of nature, or as balsams, and not rather as medicines " (by some hidden virtue) appropriated to such particular effects, which seems to be " the case of the moss of dead men's skulls, and the holding of spiders to the nose of " bleeding persons. But perhaps a fuller information of circumstances will ease me " of this scruple, and therefore I shall not insist upon it.

" WHAT you say of the subtlety of the effluvia, and of the great efficacy they are " capable of, will not be much struck at by a Corpuscularian. And if I could think

" it proper here to add ſome of the inſtances of that kind, which I have lying by " me in my notes about Occult Qualities, (as they are commonly called) perhaps they " would afford no deſpicable confirmation both to what you here ſay, and to what I " have elſewhere written about the power of inviſible corpuſcles. And I am the " more perſuaded to think great matters performable by them, both by ſome odd " obſervations, that I have ſince met with of the efficacy of the even ſolid parts of " dead animals, and particularly of a ſea-horſe tooth outwardly applied to the " body, and by conſidering, that a ſanative temperament may reaſonably be ſuppoſed " capable of diffuſing its virtue by contacts more plentifully and more powerfully " in a living body of a ſanguine complexion, where the natural heat of the blood " and ſpirits, being vegete and active, inceſſantly emit ſo great a plenty of inſinuat- " ing ſteams, as living bodies, that tranſpire freely, appear to do by the notable " obſervations of *Sanctorius*'s ingenious *Medicina Statica* (not to mention my own " trials to the ſame purpoſe). I will only add in favour of the efficacy of ſteams, " that I have ſeveral times made a ſlight preparation of ſal armoniac (that I have im- " parted to divers ingenious men) by the odour of which more than once or twice, " ſome, that have ſuddenly fallen down and were taken for epileptic, have been in a " few minutes, after I had held a ſmall vial under their noſes, brought to them- " ſelves again; hyſterical vapours, and ſome painful dulneſſes of the head, have " been often, as the patients aſſured me, diſſipated for the time in a trice; and there " are at leaſt two or three able phyſicians (whereof but one is unknown to you) who " will aſſure you, they have (though not conſtantly) preſently cured the tooth-ach by " the ſteams of that ſame preparation of ſal armoniac, which they were pleaſed to " ſend to me for; and of which, if I had time, I could tell you ſome other feats " wrought without the contact of the viſible body.

" As to what you ſay about the poſſibility of freeing patients from the morbific " matter (whoſe bulk oftentimes is not near ſo great as the miſchief it does) without " purging, vomiting, and the like evacuations; I am glad to be able to confirm by " your teſtimony what I ventured to publiſh about three years ſince to the ſame " purpoſe, where I endeavoured to bring ſpecifics out of the diſeſteem of a ſort of " learned phyſicians of a neighbouring country, that were very ſevere to them; and " where I remember I attempted to confirm the opinion you now defend, by the " inſtances, among others, of thoſe, that are cured of agues and other diſeaſes " by ſudden frights, where there uſually intervenes no ſenſible evacuation of peccant " matter.

" WHAT you teach, that as to the pains and diſtempers, that are thought to fly " from him from one part of the body to another, they avoid not his hand; but his " touch and ſtroke ſo invigorateth the parts, that they reject the heterogeneous fer- " ment, till it be expelled the body at ſome of thoſe parts he is thought to ſtroke it " out at, is a handſome conjecture, and agrees very well with your hypotheſis. But " I perceive by your way of mentioning ſimilar attraction, you are (and that very " juſtly) diffident of its being granted you by me, who am, I confeſs, very ſhy of " admitting any thing as a principle in matters purely phyſical, that I cannot well " underſtand, and do not find well proved. But you are freed from further effects " of my ſcrupulouſneſs, by my being called upon by the late time of the day to take " horſe. I ſhall not make you any apology for taking the ſame liberty to diſſent from " you in ſome points, that you took in propoſing thoſe opinions to me, which the " knowledge of my principles might eaſily make you think I would diſſent from. " And ſince both your letter and this anſwer are written in haſte, as I am willing that

" my

" haste should be my excuse for any thing, that the leisure of reading over your letter " a second time, and of reviewing what I have confusedly scribbled might have made " me avoid or mend; so I would not be so unkind to you as to impute to your de- " sign, whatever some critical readers may plausibly enough infer to the prejudice of " religion from some unstudied expressions of your *raptim scripta*; nor shall I take " them for your deliberate tenets, unless upon a review you should surprise me with " declaring them to be so; but am very willing to leave you the liberty of explaining " yourself in any thing, wherein the impetus you tell me you writ with, may have " had an influence on your pen. And as for the physiological part of my letter, " though I have annexed it, lest you should in vain expect some of my thoughts of " the physical part of yours; yet not having a competent knowledge of the matters " of fact to ground an hypothesis upon, I have been obliged, for want of such an " information, and out of an unwillingness either to say nothing to you or speak " rashly, rather to discourse on some passages of your epistle, than to establish any " theory about the thing itself, which future relations may oblige me to alter. And " therefore, as I propose my sudden apprehensions but as conjectures, that amount " not to an opinion, so I shall not be concerned, especially being otherwise sufficiently " busied, to contend for them. And I the more willingly suffer my occasions to " keep me from troubling you with any more of these physical discourses, because " you have in some sort addressed your letter to the highly learned Dr. *Willis*, from " whose profession and abilities you may expect a better account of what is like to be " applauded or questioned in the physical passages of your letter, than from him, " whose parting haste obliges him to leave the historical part untaken notice of, " save by his wonder at it, and thanks for it, and to subscribe himself somewhat " abruptly,

" Sir, your very humble servant."

AT the end of the original of this letter of Mr. *Boyle*, I find in his hand-writing an account of some cures performed in his presence in April 1666, by Mr. *Greatraks*, who in the month preceding had been very severely attacked in a pamphlet, supposed to be written by Mr. *David Lloyd*, reader of the *Charter-house*, and printed in 4to, under the title of *Wonders no Miracles: or Mr.* Valentine Greatraks's *Gift of healing examined, upon occasion of a sad Effect of his stroking, March the 7th*, 1665, *at one Mr.* Cresset's *house in* Charter-house-yard. *In a letter to a reverend Divine living near that place.* This letter is dated March the 13th, 1665-6, and obliged Mr. *Greatraks* to vindicate himself from the imputation cast upon him in it by publishing at *London* in 1666, in 4to, *A brief Account of Mr.* Valentine Greatraks, *and divers of the strange cures by him lately performed. Written by himself, in a letter addressed to the honourable* Robert Boyle, *Esq; Whereunto are annexed the Testimonials of several eminent and worthy persons of the chief Matters of Fact therein related.* In this letter, which is dated at *London*, May the 8th, 1666, he observes, that though he had not the happiness formerly to have an acquaintance with Mr. *Boyle, yet I was*, says he, *no stranger to your worth and virtue, which have made you as much admired and reverenced abroad, as honoured and beloved at home. And therefore I have assumed the confidence to make this address to your honour, whose repute and testimony to the world will be so powerful (knowing your wisdom, devotion, and learning to be so great) that truth may find belief, God have glory, and his poor instrument be justified before men, who hath no further design in the distribution of that talent, which the all-healing God has intrusted him withal, than the*

the honour of his Maker, and the good of his poor fellow-creatures, whose distempers, many of them, neither art nor physic probably could reach. At the conclusion he intimates his design, when he had an opportunity, to answer all the queries, which Mr. *Boyle* had put to him, in a paper delivered into his hands, and which he was not then at leisure to satisfy. The testimonials subjoined to this letter are signed, among others, by Mr. *Boyle* himself, *William Denton*, M. D. *James Fairclough*, M.D. Sir *William Smith*, Bart. Sir *Nathaniel Hobart*, Knt. and Master in Chancery, Sir *John Godolphin*, Knt. *Albert Otto Faber*, M. D. Col. *George Weldon*, *William Knight*, alderman of the city of *London*, Sir *Charles Doe*, Sir *Abraham Cullen*, Bart. *John Wilkins*, D. D. afterwards bishop of *Chester*, *Benjamin Wichcot*, D. D. *Ralph Cudworth*, D. D. *George Rust*, D. D. and dean of *Connor*, *Simon Patrick*, afterwards bishop of *Ely*, &c. several of whom give the highest character of Mr. *Greatrakes*'s integrity and unblameable conversation in every respect.

IN the same year, 1666, Dr. *John Wallis* addressed to Mr. *Boyle* an *Hypothesis about the Flux* and *Reflux of the Sea*, by way of letter dated from *Oxford*, April 25, and printed in the *Philosophical Transactions*, N°. XVI. p. 264. which begins thus: " You " were earnest with me, when you last went from hence, that I would put in writing " somewhat of that, which at divers times these three or four years last past I have " been discoursing with yourself and others concerning the *common Center of Gravity* " *of the Earth and Moon*, in order to solving the phænomena, as well of the sea's " ebbing and flowing, as of some perplexities in astronomical observations of the " places of the cœlestial bodies. How much the world, and the great bodies therein, " are managed according to the laws of motion and static principles, and with how " much of clearness and satisfaction many of the more abstruse phænomena have " been solved on such principles within this last century of years, than formerly they " had been, I need not discourse to you, who are well versed in it. For since that " *Galileo*, and after him *Torricellio*, and others, have applied mechanic principles to " the solving of philosophical difficulties, natural philosophy is well known to have " been rendered more intelligible, and to have made a much greater progress in less " than an hundred years, than before for many ages."

DR. *Thomas Sydenham* likewise dedicated to him that year his *Methodus curandi Febres propriis observationibus superstructa*, printed at *London* in 8vo. This piece being very scarce, though a second edition of it appeared in 1668, and it not being reprinted among that great physician's works, I shall transcribe some passages of the dedication, which places Mr. *Boyle*'s character in a very amiable light, and inform us, that he used to attend Dr. *Sydenham* in visiting many of his patients.

Illustrissimo & excellentissimo Domino, Domino ROBERTO BOYLE.

" Usque adeo bonorum doctorumque animos, quicquid ad te, vir undiquaque " nobilissime, quoquomodo pertinere conspicitur, attentos reddit & experrectos, ut " eos prævideam statim cognoscendi avidos, quibus demum fretus rationibus nominis " tui longè celeberrimi authoritatem ad hujusce operis patrocinium ascisceere susti- " nuerim. Quid plerique in simili negotio pro more causari soleant, me neutiquam " fugit. Est sanè, quod ego (perinde ac faciunt alii) de multis magnique tuis erga " me meritis palam populo commentari possem; atque etiam grati animi specimen " daturus, honoris ac cultûs, quo te prosequor, duraturum aliquod monumentum, " multo verborum apparatu, architectari. Atqui ejusmodi consilium (ut libere " profitear)

" profitear) in te tam præsidenter interpellando ab animo meo procul abfuit. In " immensas laudes tuas descendere non institui, aut eximias animi dotes, quibus " decantatissima anteriorum ætatum nomina unus exæquas (uberrimam sane tum " mihimet ipsi, tum operi meo qualicunque honorificam præfandi materiam) edis- " serere. Enimverò id ipsum tractatus à te jampridem in lucem emissi plenissimè " loquuntur; nec alio tibi opus futurum est præcone, cujus subsidiaria vox nominis " tui claritatem amplificare satagat. Quocirca tritis hisce jamque tralatitiis præfandi " argumentis ultro repudiatis, me tractatum hunc ideo in clientelam tuam dedisse " profiteor, propterea quod, quemadmodum ego tuo suasu atque instinctu hanc pro- " vinciam suscepi, ita etiam eorum, quæ hic traduntur, veritatem & efficaciam tute " ipse, testis planè locupletissimus, experientiâ aliquoties comprobatam conspexisti, " dum ad illud obsequii identidem descenderis (quæ tua & quidem egregia est hu- " manitas) ut temet in adeundis ægris meis comitem mihi adjungeres; in quâ sanè " re benignum animi affectum ad honesta utcunque se demittentem, vel invito hujusce " sæculi genio, ostendisti."

THE same year Mr. *Boyle* published at *London* in 8vo, his *Hydrostatical Paradoxes made out by new Experiments, for the most part physical and easy*; which had been presented to the Royal Society, at whose request they had been made in May 1664. In this book, which was published likewise in Latin, he shews, that the lower parts of fluids are pressed by the upper: that a lighter may gravitate upon one that is more ponderous: that if a body contiguous to it be lower than the highest level of the water, the lower end of the body will be pressed upwards by the water beneath: that the weight of an external fluid is sufficient to raise the water in pumps: that the pressure of an external fluid is able to keep an heterogeneous liquor suspended at the same height in several pipes, though they are of different diameters: that when a body under water hath its upper surface parallel to the horizon, the direct pressure it sustains is no more than that of a column of water, which hath the abovementioned horizontal superficies for its basis; and if the incumbent water be contained in pipes open at both ends, the pressure is to be estimated by the weight of a pillar of water, the basis of which is equal to the lower orifice of the pipe (parallel to the horizon) and its height equal to a perpendicular, reaching to the top of the water, though the pipe be much inclined, irregularly shaped, and in some parts broader than the orifice: that a body in a fluid sustains a lateral pressure from it, which increases in proportion to the depth of the immersed body in the fluid: that water may be able to depress a body lighter than itself: that a parcel of oil lighter than water may be kept from ascending in it: that the cause of ascension of water in siphons may be explained without the notion of *abhorence* of a *vacuum*: that the heaviest body known will not sink of itself, without the assistance of the weight of the water upon it, when it is at a depth greater than twenty times its own thickness, though it will nearer the surface.

HIS *Origin of Forms and Qualities, according to the Corpuscular Philosophy, illustrated by Considerations and Experiments: written formerly by way of Notes upon an Essay about Nitre*, was printed likewise in 1666 at *Oxford*, in 4to, and reprinted the year following in 8vo, with the addition of a *Discourse of subordinate Forms*. In his *Origin of Forms* he delivers the minds of men from the imaginary and useless notions of the schools about them, which have no foundation in the nature of things, nor in the least promote knowledge, or assist mankind; but very much disserve those great interests, by setting the understanding at rest in general obscurities, or employing it in airy

ſubtilties and diſputes, and ſo hindering its purſuit of particular cauſes and experimental realities. He lays down the foundations, and delivers the principles of the mechanic philoſophy, which he confirms and illuſtrates by ſeveral agreeable and inſtructive experiments. He ſhews, that the moſt admirable things, which have been taken for the effects of *ſubſtantial Forms*, and are uſed as proofs of notional hypotheſes, may be the reſults of the mere texture and poſition of parts; ſince art is able to make vitriol, as well as nature, and bodies may be produced by human ſkill, the ſuppoſed forms of which have been deſtroyed. He gives us many very ingenious inſtances to prove, that the mechanic motions and order of the parts is ſufficient to yield an account of the difference of bodies, and their affections, without having recourſe to the forms and qualities of the ſchools; as in the reſtoration of camphire to its former ſmell and nature, after its diſſolution and ſeeming extinction; in the changes of the colour, conſiſtence, fuſibleneſs, and other qualities of ſilver and copper; in the odd phænomena of a certain anomalous ſalt, and thoſe of the ſea-ſalt, dried, powdered, and mixed with aqua fortis; in the ſal mirabilis, in the production of ſilver out of gold by his *menſtruum peracutum*, in the tranſmutation of water into earth in a certain diſtillation of ſpirit of wine and oil of vitriol: in ſhort, he makes it evident, that the internal motions, configuration, and poſition of the parts, are all that is neceſſary for alterations and diverſities of bodies; and conſequently, that ſubſtantial forms and real qualities are needleſs and precarious beings.

HE communicated alſo to Mr. *Oldenburgh* ſeveral pieces, which were publiſhed in 1666, in the *Philoſophical Tranſactions*, viz. *A Confirmation of the former Account*, [*Phil. Tranſ.* N°. X. p. 166.] *touching the late Earthquake near* Oxford, *and the Concomitants thereof:* N°. XI. p. 179. *Some Obſervations and Directions about the Barometer:* p. 181. *General Heads for a Natural Hiſtory of a Country, great or ſmall:* p. 186. *A Way of preſerving Birds taken out of the Egg, and other ſmall Fœtuſes:* N°. XII. p. 199. *An Account of a new kind of Baroſcope, which may be called Statical, and of ſome Advantages and Conveniences it hath above the Mercurial:* N°. XIV. p. 256. *A new frigorific Experiment, ſhewing how a conſiderable degree of cold may be ſuddenly produced without the help of ſnow, ice, hail, wind, or nitre; and that at any time of the year:* N°. XV. p. 256. *Other Inquiries concerning the Sea:* N°. XVIII. p. 315. *Promiſcuous Inquiries about Mines:* p. 334. *The Method obſerved by Dr.* Lower *in tranſfuſing* the blood of one live animal into another: *N°. XX. p.* 353. *Trials propoſed to Dr.* Lower, *to be made by him for the improvement of tranſfuſing blood out of one live animal into another:* N°. XXII. p. 385. *Propoſals to try the effect of the pneumatical Engine exhauſted, in plants, ſeeds, eggs of ſilk-worms:* No. XXIII. p. 424.

IN 1667 he publiſhed in the ſame *Tranſactions*, *A Confirmation of the Experiments mentioned in* N°. XXVII. *to have been made by Seignor* Fracaſſati *in* Italy, *by injecting acid liquors into the blood:* N°. XXIX. p. 551. *New Experiments concerning the relation between Light and Air in ſhining Wood and Fiſh*, N°. XXXI. p. 581. And *Obſervations and Trials about the Reſemblances and Differences between burning Coal and ſhining Wood*, N°. XXXII. p. 605.

THE ſame year Dr. *Walter Needham* dedicated to him his *Diſquiſitio Anatomica de formato Fœtu*, printed at *London* in 8vo, with this inſcription: *Nobiliſſimo, clariſſimo* Roberto Boyle *Arm. illuſtrium comitum de* Burlington & Orrery *fratri germano, philoſophorum principi, hanc ſuam diſquiſitionem anatomicam ſacram voluit* Gualterus Needham, *M. D.* and he owns in the Preface, that this treatiſe was drawn up and publiſhed at the ſolicitation of Mr. *Boyle*.

IN

IN the beginning of the year 1668, he wrote a letter of thanks to Dr. *Thomas Sydenham*, upon receiving from him a present of the second edition of his *Methodus curandi Febres*, dedicated to him, as the former had been; to which the doctor returned an answer, dated April 2, 1668, in which he confirms his own practice in the small-pox [k], to which Mr. *Locke* had been a witness, and with regard to which he declares, that he had found no cause from his best observation, to repent of any thing said by him in this tract *de Variolis*; though he greatly regretted, that he had not said, that considering the practices that obtained both amongst learned and ignorant physicians, it had been happy for mankind, that either the art of physic had never been exercised, or the notion of *malignity* never been stumbled upon. "As it is palpable, *says he*, to all the world, how fatal that disease proves to many of all ages, "so it is most clear to me from all the observations that I can possibly make, that "if no mischief be done, either by the physician or nurse, it is the most slight and "safe of all other diseases. If it shall be your hap to be seized of that disease (as "probably you never may) I should recommend to you, upon the word of a friend, "the practice mentioned in the 155th page of my book." He then takes notice of some accidents, which are incident to that disease, which he was never master of till the summer before; and of his own success in general. "I have the happiness, "adds he, of curing my patients; at least of having it said concerning me, that few "miscarry under me; but cannot brag of my correspondency with some others of my "faculty, who, notwithstanding my profoundness in palmistry and chemistry, im-"peach me of great insufficiency, as I shall likewise do my taylor, when he makes "my doublet like a hop-sack, and not before, let him adhere to what hypothesis he "will. Though yet in taking fire at my attempts to reduce practice to a greater "easiness and plainness, and in the mean time letting the mountebank at *Charing-"Cross* pass unrailed at, they contradict themselves, and would make the world be-"lieve I may prove more considerable than they would have me. But to let these "men alone to their books, I have again taken breath, and am pursuing my design "of *specifics*, which, if but a delusion, so closely haunts me, that I could not but "indulge the spending of a little money and time at it once more. I have made "a great progress in the thing, and have reason to hope not to be disappointed." But it does not appear, that this *Design of Specifics* was ever executed by Dr. *Sydenham*; at least, it was never communicated to the world.

THE Royal Society had from its first institution alarmed the zeal of the admirers of the old philosophy, who affected to represent the views of many of its Members to be the destruction, not only of true learning, but even of religion itself. This gave occasion to Dr *Sprat's History of the Royal Society*, printed in 1667, and to a discourse published at *London* in 1668, in 8vo, by Mr. *Joseph Glanvill*, rector of the church of *Bath*, and afterwards chaplain to King *Charles* II. and prebendary of *Worcester*, under the title of *Plus Ultra: or, the Progress and Advancement of Knowledge since the days of* Aristotle, *in an account of some of the most remarkable late Improvements of practical useful Learning, to encourage philosophical Endeavours. Occasioned by a Conference with one of the notional way.* This conference was with Mr. *Robert Crosse*, vicar of *Great Chew* in *Somersetshire*, who maintained, that "*Aristotle* had more ad-"vantages for knowledge than the *Royal Society*, or all the present age had, or could "have, because he did *totam peragrare Asiam*." Mr. *Glanvill* in his 12th chapter, which treats of *the Royal Society*, shews the reasons of their institution, and their de-

[k] Vol. VI. p. 649, 649.

[l] *Glanvill's Plus Ultra*, Ch. I.

ſigns; and returns an anſwer to the queſtion, *What have they done?* In the next chapter he gives *an account of what hath been done by the illuſtrious Mr.* Boyle, *for the promotion of uſeful knowledge*; and obſerves [m], that he alone had even then *done enough to oblige all mankind, and to erect an eternal monument to his memory; ſo that had this great perſon lived in thoſe days, when men godded their benefactors, he could not have miſſed one of the firſt places among their deified mortals*; and that in his writings are to be found *the greateſt ſtrength and the gentileſt ſmoothneſs, the moſt generous knowledge and the ſweeteſt modeſty, the nobleſt diſcoveries and the ſincereſt relations, the greateſt ſelf-denial and the greateſt love of men, the profoundeſt inſight into philoſophy and nature, and the moſt devout, affectionate ſenſe of God and of religion.* However this piece did not reſtrain Mr. *Henry Stubbe* the phyſician from publickly attacking the Royal Society with prodigious warmth and ſeverity, upon ſeveral chimerical pretences; and particularly, that the Members of it intended to bring contempt upon ancient and ſolid learning, eſpecially the Ariſtotelian philoſophy; to undermine the univerſities; to deſtroy the eſtabliſhed religion, and to introduce Popery in its ſtead. Theſe charges he maintains in various tracts of his, *viz. Legends no Hiſtories: or, a Specimen of ſome Animadverſions upon the* Hiſtory of the Royal Society, *London* 1670, in 4to. *The* Plus Ultra *reduced to a* Non Plus: *or a Specimen of ſome Animadverſions upon the* Plus Ultra *of Mr.* Joſeph Glanvill, *London* 1670, in 4to. *Cenſure of certain paſſages contained in* The Hiſtory of the Royal Society, *&c. Oxford* 1670, in 4to. *Campanella revived; or an Inquiry into* The Hiſtory of the Royal Society, *&c. London* 1670, in 4to. and *Reply unto the Letter written to Mr.* Henry Stubbe, *in a defence of* The Hiſtory of the Royal Society, *&c. Oxford* 1671, in 4to. But amidſt theſe outrages againſt that Society, he kept up a correſpondence with Mr. *Boyle*, who ſhewed a ſingular goodneſs of temper, and an uncommon zeal for the public ſervice, in bearing with ſo much pride, paſſion, and indecent treatment from a perſon, whom he had highly obliged, becauſe he thought him, with all his faults, capable of being uſeful to the world. The following letters will give a clear idea of ſo remarkable a man.

" Honoured Sir,

" I AM much obliged unto you for the trouble you gave yourſelf by imparting " the contents of my letter to the Members of the Royal Society. I wonder " they ſhould imagine this attempt of mine to be an action I was not provoked unto, " whereas Mr. *Glanvill*'s book [n] was enough to juſtify any ſeverity of proceedings, " it being written by a cabal, and not by him alone. And it is much more ſtrange " to me, that they ſhould think, that a man needed particular exaſperations to write " againſt that pernicious *Hiſtory* [o]. There is no man deſires to live more in quiet " than I do; and to purſue my practice without intermeddling with the affairs of the " nation. But if that *Hiſtory* take place, the whole education of this land and all " religion is ſubverted; and I profeſs I am not inclined ſilently to expect, till fire, " faggots, and exile be the common conſequences of being a proteſtant. I know " this great change was no part of the intentions of the King, and ſuch honourable " and pious perſons, as firſt aſſociated; but that the hiſtorian exactly complies with " *Campanella*, is evident; and it is in vain for you not to meddle with religion amongſt " yourſelves, if you thus overthrow it in your writings. Upon this account I ſevered " the caſe of your comical wits from that of the other illuſtrious perſonages; nor is " there any thing in my book, that reflects on the King or any perſon of learning or

[m] P. 92, 93. [n] *Plus Ultra.* [o] *Sprat*'s Hiſtory of the Royal Society.

quality.

" quality. I gave the licenser leave to blot out every thing, that he thought might " give just offence; and that he did too much, even to the disturbing of the sense, " practising the freedom I gave him so far, that I am forced to add a Review, thereby " to illustrate some passages, which he had, I know not how, nor upon what reason, " curtailed. It is certain and manifest, that the *History* must be written anew; and " I wonder, that any should scruple my amendments of it, whereof all that comes " out now is but a specimen; for I have much more behind.

" As for Mr. *Glanvill*, it is much, that the Society should now appoint him their " secretary (I have not yet seen the new model of correspondence) seeing that he is " a man, whom the universities hate and scorn, and that hath so notoriously disobliged " the physicians, and whose credit is now irrecoverably lost; for if my papers ever " come out, it will be in vain for any man to go about to excuse him from the " most gross ignorance, that ever any writer committed; and this action looks as if " the Society would own him as a man of worth. Besides his *Plus Ultra*, his Letter " against *Aristotle* is all mistakes; and it is evident he never read *Laertius*. My " reply to that Letter will be printed at *Oxford*, I hope before the end of the next " term; but the publishing of that depends upon this; not that they scruple at it, " being earnest for it, but that I would keep the business alive by printing one book " after another.

" AT the session of parliament you will see a *Censure* upon certain passages in the " *History*, with a defence of the *Censure* approved by as eminent divines as this na- " tion yields; which will convince you, how justly I say, that the *History* subverts " the protestant religion and church of *England*. I never did any thing more un- " willingly than to enter upon divinity, though now the thirty-nine articles, and ho- " milies, and the protestant religion, be the subject I defend against Popery and " Socinianism. I understood of the King's displeasure at me, and that he said some " severe things; but I was not troubled at it much, being content rather to serve " him, than to flatter him. I did expect to have been imprisoned, when I first " undertook it, and therefore contrived, how I might be freed again, never pur- " posing to decline the other trouble. But when I bring the case before the Com- " mons, I am sure I shall here be powerfully abetted, and the Society will have the " worse, and whatever they do before, will add to my plea then. You will see be- " fore Twelfth-day, that no inconsiderable persons will speak favourably of my " intentions, and avow the bravery and necessity of the performance. But I deter- " mined not to engage them to discourse their minds before the book came out.

" BESIDES that book of *Vander Mye*, there is Fonseca *de sanitate tuenda*, which I " sent to let you see, what mastic wood will do in the gout. I assure you, it is a wood " I have tried in bodies apt to catarrhs, and it doth not only work powerfully in " drying them, but purges twice or thrice in a day, as I found particularly in Mr. " *Garnet* (that married the lady *Boswell*) lately, to his great recovery out of a ma- " lignant fever, which ended with a perfect salivation of three weeks, and after that " endangered him of a consumption. But he was perfectly well and fat again in a " few weeks. It is not ill tasted, nor hot, but after it is swallowed a while, it creates " in the throat such a sense, as remains after drinking pepper-posset.

" IF there be any thing, wherein I may serve you, or any other books you need, " you may command them. Had not the Society thus gone about to injure the phy- " sicians, I had gone on with my collection of English receipts, illustrating every " experiment out of the experiments of the greatest physicians. But in this empi- " rical ignorant age such works are not to be published.

" I SUPPOSE you may have seen two pieces of *Tachenius:* there are in them many " experiments that are pretty, discovering the discrepancy of salts (a thing I have " made some inquiries into) and that gold is acid; which agrees with a way I learned " to make cheese and whey with gold colour, which was tried in a consumption by a " lady of my acquaintance.

" I HAVE no more to add, but that I am glad the house dismissed that foolish " impeachment of my lord *Orrery*. I am,

" SIR,

Warwick, December 17, 1669.

" your obliged humble servant,

" HENRY STUBBE."

" I HAD forgot to suggest one thing to the consideration of your Society, that " perhaps it may be manifested at the parliament, that the historian by his book is " chargeable with high-treason; and that there want not such as think, if your So- " ciety own it, they incur a præmunire; so that whatever they may imagine to draw " upon me (though I may suffer a while) will fall worse on them, when the world " takes notice of the ground of their actings against me, and that they do all this, " whom I had represented in my preface as unconcerned in the contest."

MR. *Stubbe* wrote likewise another letter to him from *Warwick* on the 18th of May, 1670, which was as follows:

" SIR,

" I NEVER doubted, but that Mr. *Boyle* would never swerve from the rules of " honour and strict virtue, whatever the other virtuosi might do. You are still " constant to yourself and worth; but so are not they. That you might hear several " persons of quality incense the king against me, and that his majesty let fall some " passages that were severe, it is not strange to me: I had an account thereof im- " mediately from many hands. I know the opportunities my enemies have, and " under how false representations princes discern things, who see but only with others " eyes, and glasses obtruded by the virtuosi. I am satisfied in the justness of my " attempt, and the integrity of my designs and sentiments. Had interest moved " me, and either desire of glory, or any other vulgar inducement moved me, to " have flattered the Royal Society, to have admired the present condition of our na- " tion, to have betrayed religion, had been my speedy course to effect all my de- " signs. But I needed not any new courses to be known to the world, or in this " country to advance my practice; and the first thoughts I assumed after the design " of opposing the Royal Society, were to desert the land, to encounter all manner of " censures and imprisonment, perhaps death; and amidst these perplexities I did " resolve to proceed, if that they would be so indiscrete, as not to disclaim those two " books. Nor have I yet received any intelligence, that proposeth to me any new " and unforeseen danger, and which a man, who is weary to have out-lived the pro- " sperity, honour, learning of the nation, and who hath no other prospect than the " apparent ruin of his country, would not willingly sink under. Yet I find amidst " these contrasts, that most sober persons commend my design, and that my perfor-

" mances,

" mances were neceſſary, and not mean; and I dare ſay, that except the miſcon-
" ſtructions, which my prince puts on my doings, my abettors are more numerous,
" judicious, and noble than my enemies. But the conduct of my fate I leave to that
" power, which irreſiſtibly put me upon theſe actings, and permitted me no reſt but
" in purſuing them. Whatever befalls me, the world will ſay it was malice in the
" virtuoſi, now that the quarrel is ſo boldly begun by me. If you conſider the writ-
" ing of *Glanvill*, you cannot ſay, that I proceed in too exaſperating a way; I did
" but write by his copy; nor had I equalled him but by out-doing him. Beſides it
" is *reſponſum, non dictum, quia laeſit prior*. And though your hiſtorian be the more
" civil in words, yet is his ſenſe parallel to that of *Glanvill*, as I have ſhewed in my
" dedication to both univerſities in my *Animadverſions* on the *Hiſtory*. And what
" reaſon the Royal Society had to deal ſo with the phyſicians, if it be true, that they
" had ſuch an eſteem for them, and that ſo many eminent perſons joined with them,
" I know not, except I accommodate all to a ſubſerviency unto *Campanella*; and then
" it is eaſy to divine. That the biſhop of *Ch.* [r] had made collections in order to the
" writing of my life, is a thing I am aſſured of from one, who gave me an account
" of the tendency of the papers, and of ſome particulars of them; as alſo of a letter of
" *Glanvill*'s, which was a piece of raillery, and paſſed through ſome hands, contain-
" ing nothing of truth, but like to him. They that put the ſtreſs of their defence
" on ſuch foundations, are welcome adverſaries; yet ſhall I ſay no more to them,
" than what I wrote a year ago, when I was firſt threatened with theſe actions, and
" which is like to be public in my preface to the *Animadverſions* on the *Hiſtory*. The
" diſingenuity with which I have been treated, admits of no apology; nor can my
" lord *Brounker* ever acquit himſelf unto the world, for having bragged and told
" the earl of *Aylesſbury*, that he would licenſe it, if I would ſend it unto him, and
" yet declined it. It is very falſe printed, nor is the catalogue perfect as to thoſe
" material. I did appoint ſome to be corrected with a pen; but orders at a diſtance
" are ill obſerved; and I durſt not come up to ſee it done, for fear of ſome mecha-
" nical philoſophers and their ſtratagems.

" I DO not yet underſtand by any hands but yours, that there is ſo great a corre-
" ſpondence betwixt the College of Phyſicians and Royal Society. That ſundry of
" thoſe few, that were entered in it, have deſerted and deteſted it, I am ſure; and
" ſo Dr. *Scarborough* and others inform me. Nor do I hear of four intelligent and
" eminent phyſicians, that heartily adhere to the Society; and it is evident, that they
" betray their faculty, who do it. But this point I have largely handled in my fourth
" part, where Dr. *Merret* is replied unto; which piece I do intend not to ſuffer to
" be publiſhed till Michaelmas. As to thoſe paſſages, wherein you ſeem concerned
" in this piece, I did not think fitting to give you a particular account, becauſe you
" were not directly concerned; for in the aërial column, and that each little altera-
" tion of the air may be diſcovered by the barometer, I only oppoſe *Glanvill*, who
" ſays it is *concluded* to be ſo; not you, who make no ſuch poſitive concluſion. As
" to the mixtures of liquors with blood, and their operating differently on the blood
" in a pottinger, and when injected into the veins, or aſſumed by the mouth, I did
" give you notice long ago thereof, and that I did not think your argument for Sp.
" C. C to be concluſive. But there is, I find, a miſtake in what is printed, where
" it is printed, as if I held the opinion, that ſuch mixtures, when injected, and when
" made in a pottinger, were the ſame; whereas I deny it totally, and my page 129

[r] *Chester*, Dr. *John Wilkins*.

" contradicts

" contradicts itself and the subsequent discourse, if it be taken so, as I hear you " did. I think I gave you an account of some *Errata* in my last. I did not think " I needed to mention, that in the *Animadversions* on the *History* I have laughed at " him, that told you for an arcanum the use of ivy-berries; and I have called into " suspicion the redintegration of nitre, it being manifest, I think, that you did not " use the best nitre in your calcination. Indeed I think I have there given a great " insight into the nature of salt-petre; yet could have added more, had I designed " an history, and not mere *Animadversions*. Besides when that was written, I did not " intend, unless forced unto it, to extend my papers beyond ten or twelve sheets. " But if any great difference appear betwixt you and me, it will be about the second " part of your *Usefulness in Experiments for Physic*; in which book I think we phy- " sicians do suffer much; and that book hath been the occasion of all the insolencies " we have received from *Dowde* to *Glanvill* and *Thompson*; wherein yet your candour " would unconcern you, but that others make too much use of it to our prejudice; " and our defence is very facile, as I have briefly shewed against Dr. *Merret*. It " was a great unhappiness, that several persons of honour like yourself should ever " mix with such insignificant talkers, as the generality are; for you could get no " credit by them, and their arrogance and folly would unavoidably run you all into " quarrels, if not contempt. And it is apparent, that the Royal Society hath not " managed things with that discretion, which was necessary; and being informed of " their errors, would rather be obstinate than amend them.

" As to my transferring the glory of some inventions from my countrymen to " others, I am sure I have injured no man thereby; for to deprive any man of undue " praises is no injury; for if I had said, that *Picquet* did suppose the air to be like " flocks of wool, I had said true; yet I did not, but I shall do it e'er long, but my " respects to you hindered me. And I do suspect, that the bishop of *London*'s chap- " lains, and some of the Society, in the dispunging did join against me, or else some " others had been charged for plagiaries, or men of no reading, who have escaped I " know not how. But I do think no man was ever used as I have been; and it is the " common concern of mankind, that such courses be not continued.

" I HAVE no more to add, but that I shall willingly allow many of the Society the " facility of improving past inventions; but I will not say, contrary to my knowledge, " that they did invent them, except they will on the other side grant unto me, that " they are men of no reading, and understand no more what hath been printed in " *Europe*, than what was invented in *China* before our commerce with the *East-* " *Indies*.

" THIS is all, besides those respects, which I shall ever pay unto you upon many " scores, and which are such, that except where the common utility of *England* " forceth me to the contrary, I shall ever be

" Your most humble servant,

" HEN. STUBBE."

I FIND

I FIND likewise a third letter of his dated the 4th of June following:

" SIR,

" I RECEIVED yours, as also the two books, and I cannot but begin my answer " with expressing my sorrow to find, that you still adhere to the R. S. Except " yourself, I do not hear of any sober person, but approves of my design against the " Society, and they that dissuaded me from it, thought the attempt necessary, not " dangerous. I beseech you, Sir, consider the mischiefs it hath occasioned in this " once flourishing kingdom; and if you have any sense, not only of the glory and " religion, but even the being of your native country, abandon that constitution. " It is too much, that you contributed to its advancement and repute; the only " reparation you can make for that fatal error, is to desert it betimes. Do not you " apprehend, that all the inconveniencies that have befallen the land, all the de- " bauchery of the gentry (which ariseth from that pious and prudent breeding, " which was and ought to have been continued) will be charged on your account? " Do you not hear, how much the serious and sober men do blame you? You are " much a stranger to the discourse and sentiments of men, if you know not, that " your name is frequently questioned, and that the integrity of Mr. *Boyle* is dis- " puted, as if you in the council of trade, and by some other engagements, had " shewed yourself less a patriot and public spirited than was imagined. It will be " impossible for you to preserve your esteem, but by a seasonable relinquishing of " these impertinents. I tender you this testimony of my sincere respects, that I am " thus free with you; and whilst others flatter you, there is an old servant of yours " informs you of the truth. What I believe you could never have apprehended, is " come to pass; the body of the nation, and all learned men abet me: the two uni- " versities (especially that of *Cambridge*, which was most inclined to novelty) avow " my quarrel; and they that favoured experimental philosophy, judge those virtuosi " unfit to prosecute it. The bishop of *Chichester* reads against them, and intends to " hold his lecture a year or two, and that for this reason, to overthrow the esteem of " them. The concurrence to hear him is such, as the university never saw before. " The elect of *Bath* is as much as any for me, and against *Glanvill*. In sum, all " men apprehend it now their common interest to oppose the R. S. I know not what " any physicians may, as the mode is, tell you to your face; but except it be such " as Dr. *Sydenham*, and young *Cox*, I believe not one lives, that doth not condemn " your experimental philosophy; and either the college, or Sir *Alexander Fraser*, and " the principal of them intend me a letter of thanks for what I have done, with the " liberty to print it; and perhaps others will follow the precedent; and how con- " sistent these things are with the repute of the Society you adhere unto, judge " you. . . . The bishop of *London* hath licensed that against *Sprat* once more, and " approved the preface and dedication to both universities, wherein the passages are " more severe than any yet published . . . being resolved to keep the controversy " hot, till the R. S. submit to the terms I propose, and then they had as good dissolve. " I acquaint you with these things, that you may consult your own ease, and let " these comediants stand or fall alone; and then whatever reply I shall at the request " of the physicians at *London* make to you, shall be as amicable as it is possible. And " if it please God to continue us a nation, perhaps you shall see a nobler project for " the advancement of useful knowledge in reference to physic, than ever your So- " ciety was capable of, and which shall add glory to our faculty. It was indeed the

" popiſh intereſt to depreſs the phyſicians, that ſo there might be nothing of learn-
" ing in the nation to oppreſs them. Civil law is gone, the number of learned di-
" vines is ſmall, and your R. S. hath deſtroyed the ſucceſſion. But there remains
" enough to retort the ſhame upon the authors, and cover you all with ignominy. I
" have no more to add, but that I ſhall never quit that reſpect, which becomes

" Your moſt humble ſervant,

Ragley, June 4, 1670.

" HEN. STUBBE."

SUCH was the impetuous genius of this learned man, of whom, as I ſhall have no other occaſion to mention him again, I ſhall add here this ſhort account, that he was ſon of a miniſter, and born at *Partney* near *Spilſbye* in *Lincolnſhire* on the 28th of February 1631, and at ten years of age ſent to *Weſtminſter* ſchool under the care of Dr. *Richard Buſby*, where by the intereſt of Sir *Henry Vane*, jun. he became one of the king's ſcholars, and in 1649 was elected ſtudent of *Chriſt Church* in *Oxford* [q]. In July 1653 he took the degree of Bachelor of Arts [r], and then went to *Scotland*, where he ſerved in the army till 1655; and in December 1656 took the degree of Maſter of Arts. In the beginning of the year following he was appointed ſecond library-keeper of the *Bodleian* library under Dr. *Tho. Barlow*, which poſt he held till the year 1659, when he was removed from it, as well as from his place of ſtudent of *Chriſt Church*, having the ſame year publiſhed a *Vindication* of his patron Sir *Henry Vane*, *An Eſſay on the good old Cauſe*, and a piece entitled, *Light ſhining out of Darkneſs*, with *an Apology for the Quakers*, in which he reflected upon the clergy and the univerſities. He retired then to *Stratford* upon *Avon*, in order to practiſe phyſic there, and after the reſtoration joined himſelf to the church of *England*, *not only*, ſays he in the dedication to biſhop *Morley* of his tranſlation of *Caſa*'s *Arts of Grandeur and Submiſſion*, *upon account of its being publickly impoſed (which in things indifferent is no ſmall conſideration, as I learned from the Scottiſh Tranſactions at* Perth) *but becauſe it is the leaſt defining, and conſequently the moſt comprehenſive and fitting to be national.* In 1661 he went to *Jamaica*, being honoured with the title of his majeſty's phyſician for that iſland; but that climate not agreeing with his conſtitution, he returned to *England*, and at laſt ſettled in *Warwick*, where he gained very conſiderable practice, as likewiſe at the *Bath*, which he frequented in the ſummer ſeaſon; but was unfortunately drowned in a river about two miles from that city in a journey to *Briſtol* on the 12th of July 1676, and was interred in the great church at *Bath*, his old antagoniſt Mr. *Glanvill* preaching his funeral ſermon.

BUT to return to the year 1669, in which Mr. *Boyle* gave the world his *Continuation of new Expriments phyſico-mechanical touching the Spring and Weight of the Air and their Effects. The firſt Part. Written by way of Letter to the right honourable the lord* Clifford *and* Dungarvan. *Whereto is annexed a ſhort Diſcourſe of the Atmoſpheres of conſiſtent Bodies; ſhewing, that even hard and ſolid Bodies (and ſome ſuch, as one would ſcarce ſuſpect) are capable of emitting Effluvia, and ſo of having Atmoſpheres:* Oxford, in 4to. *A Diſcourſe of abſolute reſt in Bodies*; added to a ſecond edition of his *Certain phyſiological Eſſays*, London, in 4to. *An Invention to eſtimate the Weight of Water in Water with ordinary Balances and Weights*; printed in the *Philoſophical Tranſactions*,

[q] Wood, Athen. Oxon. Vol. II.

[r] Col. Faſti Oxon. Vol. II.

N°. L. p. 1001. a tranſlation of which into Latin is annexed to the Latin edition of his *Hydroſtatic Paradoxes*; and a dilucidation of the experiment in an *Hydroſtatic Letter* printed among his tracts in 1672, in 8vo. He wrote alſo the ſame year *a Letter to Dr.* Peter du Moulin, prefixed to the 4th edition of a book, tranſlated by the doctor from French into Engliſh, and printed at *Oxford* 1669, in 12mo, under the title of, *The Devil of* Maſcon: *or, a true Relation of the chief things, which an unclean Spirit did and ſaid at* Maſcon *in* Burgundy, *in the houſe of one Mr.* Francis Perreaud, *miniſter of the reformed church in the ſame town. Publiſhed in French lately by himſelf, and now made Engliſh by one that hath a particular knowledge of the truth of the ſtory.* It was afterwards indeed reported, that Mr. *Boyle* had diſowned the hiſtory of that dæmon, as a clear impoſture; but he declares, that he never did this, in a letter to Mr. *Joſeph Glanvill*, dated at *London* Feb. 10, 1677-8, wherein he has theſe words: " Whatever I may have thought of ſome incidental paſſages in the printed " book, yet as to the main ſtory itſelf, the converſation I had with the author, when " the thing was freſh in men's memories, and the notoriety of divers of the circum- " ſtances, ſurmounted my indiſpoſition to believe ſuch things; and of late years I re- " member not, that I have had occaſion to diſcourſe much of it, much leſs to diſown it, " eſpecially after a learned and intelligent traveller, that ſome years ago paſſed by " that town, and afterwards viſited me, informed me, that he found upon the place " itſelf juſt motives to believe the truth of it." Dr. *du Moulin*, the tranſlator of the piece laſt mentioned, was ſon of *Peter du Moulin*, the celebrated French proteſtant divine, and had been tutor to the lord viſcount *Dungarvan*, and Mr. *Richard Boyle* his brother, nephews to Mr. *Boyle*; and after the reſtoration was preferred to be chaplain to the king, and prebendary of *Canterbury*. He had an uncommon genius for Latin poetry; and having collected his performances in that kind, he publiſhed them with a dedication to Mr. *Boyle* at *Cambridge* 1670, in 8vo, under the title of *Petri Molinæi P. F.* ΠΑΡΕΡΓΑ. *Poematum Libelli tres.* I. *Hymni in Symbolum Apoſtolorum.* II. *Eccleſiæ Gemitus.* III. *Sylva Variorum.* In the dedication the doctor highly commends Mr. *Boyle*'s excellent talents in verſe, and his particular reliſh for that kind of poetry, which is devoted to religion. *Cum vero nulla literaturæ pars extra eruditionis tuæ pomærium poſſit evagari, patere, quæſo, etiam huic poetico labori liberum aperiri apud te angulum. Cui ſanè poſtulationi ut te facilem fore mihimet ſpondeam, tria ſunt. Unum eſt, quod ſeveriora ſtudia poetico meile delenire tibi non eſt inſolens, & in carmine ita excellis, ut niſi excelſiores in te dotes eminerent, ad id unum natus poſſis exiſtimari. Alterum eſt, quod horum poematum nonnulla jamdudum in manibus habuiſti, approbâſti, & cum iis familiaritatem inire non aſpernatus es. Potiſſimum eſt, quod cum ingenii & eruditionis tuæ ſublimitatem immenſum quantum excedat tua erga Deum pietas, & in capaciſſimâ mentis arce regina ſedeat, non poterunt tibi non placere piæ ad Deum aſpirationes, quas tutemet poetico ſpiritu altiùs atque feliciùs attolli non ſemel apud me profeſſus, expertus apud expertum; adjectis hortamentis, quæ mihi ſunt mandatorum loco, ut iſtam aream ſeriò tererem.* The doctor in a letter to Mr. *Boyle*, dated at *Canterbury* December 28, 1669[1], takes notice of one circumſtance, which deſerves to be mentioned, as it ſhews how far the prejudices againſt the Royal Society had poſſeſſed the univerſities at that time; for Dr. *Peter Gunning*, afterwards biſhop of *Ely*, *one of the prime licenſers of the book*, would not ſuffer a copy of heroic verſes, which Dr. *du Moulin* had written in commendation of that Society, to be publiſhed with the reſt of his poems. " It grieves me (adds he, to ſee a feud between that noble Society and the univer-

[1] See Vol. VI. p. 579.

" ſities, to which Mr. *Glanvill*'s books have much contributed. Your great credit, " prudence, and moderation may ſtop that growing evil, if any thing in the world " can."

IN 1670, Mr. *Boyle* publiſhed his *Tracts about the coſmical qualities of things; coſmical ſuſpicions; the temperature of the ſubterraneal regions; the temperature of the ſubmarine regions; the bottom of the ſea*; to which is prefixed, *an introduction to the hiſtory of particular qualities:* printed at *Oxford*, in 8vo, and tranſlated into Latin. In the tract concerning the *coſmical qualities of things*, he ſhews, 1. That they depend partly upon the influence of external agents, as well as the primary affections of matter: ſo that there are many bodies, which in ſeveral caſes act not, unleſs acted upon, and ſome of them act either ſolely or chiefly, as they are acted on by general and unregarded agents. 2. That there are certain ſubtile bodies in the world, which are ready to inſinuate themſelves into the pores of any body diſpoſed to admit their action, or by ſome other way to effect it; eſpecially if they have the concurrence of other unobſerved cauſes, and the eſtabliſhed laws of the univerſe. 3. That a body, by a mechanical change of the texture, may acquire or loſe a fitneſs to be wrought upon by unheeded agents, and alſo to diverſify their operations on it, by a variation of its texture. He propoſes next the following *coſmical ſuſpicions*, 1. That beſides thoſe more numerous and uniform ſorts of minute corpuſcles, which are by ſome of the modern philoſophers thought to compoſe the æther, there may poſſibly be ſome other kind of corpuſcles fitted to produce conſiderable effects, when they find bodies to be wrought on by them. 2. That ſeveral perſons have diſcovered peſtilential ſteams in the air, before they acted as ſuch upon other bodies. 3. That conſiderable, though ſlow, changes in the internal parts of the earth may occaſion a variation of the mariner's needle. 4. That the ebbing and flowing of the ſea, and ſuch like phænomena, are occaſioned by ſome *coſmical* law of nature; or that the planetary vortex may be not a little concerned in producing ſuch effects. 5. That all endemical and epidemical diſtempers principally depend upon the influence of thoſe globes, which move about us, and the terreſtrial effluvia of our own globe. 6. That thoſe which are thought the grand rules, by which things corporeal are tranſacted, and which ſuppoſe the conſtancy of the preſent fabric of the world, and a regular courſe of things, are not altogether ſo uniformly complied with, as we preſume; at laſt, as to the lines, according to which the great mundane bodies move, and the boundaries of their motions. 7. That on the other hand we may perhaps take ſuch things for exorbitances and deviations from the ſettled courſe of nature, as, if long and attentively obſerved, will be periodical phænomena of very long intervals; but becauſe men have not ſufficient ſkill and curioſity to obſerve them, nor a life long enough to be able to take notice of a competent number of them, they readily conclude them to be but accidental extravagancies, which ſpring not from any ſettled and durable cauſes. In the diſcourſes concerning *the temperature of the ſubterraneal and ſubmarine regions*, he obſerves, that there are different regions below the earth; that the firſt region of the earth is very variable both as to bounds and temperature; that the ſecond ſeems to be generally cold in compariſon of the other two; that in ſeveral places, which by reaſon of their diſtance from the ſurface of the earth might be referred to the middle region of it, the temperature of the air is very different at the ſame ſeaſons of different years; that the third region of the earth has been obſerved to be conſtantly and ſenſibly, but not uniformly warm, being in ſome places conſiderably hot; that there are two different regions below the ſurface of the ſea, the one extended from the ſuperficies of the water as far downward, as the manifeſt ope-

ation of the ſun-beams or other cauſes penetrates, and the other from thence to the bottom; ſo that the upper region muſt vary as to its extent, according to the difference of the climate and the heat of the ſun, or the nature of the ſoils about the ſhore; but that the lower region is generally cold: he farther tells us, that the bottom of the ſea is very rough and unequal; that the water gravitates conſiderably upon bodies immerſed in it; and that the bottom of the ſea is not diſturbed with ſtorms, but that the water ſtagnates.

There were publiſhed alſo *the ſame year* in the *Philoſophical Tranſactions* two other pieces of his, viz. *New Pneumatical experiments about reſpiration*; No. LXII. and *A continuation* of the ſame *experiments*; No. LXIII.

AMIDST all theſe labours for the public he had been afflicted with a ſevere paralytic diſtemper; but the regimen which he made uſe of with ſucceſs for his relief, will appear from his letter to his friend *John Mallet*, Eſq;

" *London*, May 23, 1671.

" SIR,

" I AM much obliged to you for the favour of haſtening to me ſuch welcome " news, as that of my brother *Orrery*'s being already put to ſea with ſo fair a " wind, as promiſed he would get quickly to the other ſide of it. Since Monday, " when I received the favour of your letter, there came to my hands a deſire from " him relating to your kinſman, Mr. *Pitt*, which was very needleſs; the relation, that " gentleman has to you, and the concern you expreſs for him, being more than " ſufficient to make me forward to do him all the ſervice I am able. But this, I " fear, will not be very much; for though, through the goodneſs of God, my pa- " ralytical diſtemper is much leſſened, yet I am far from being fully cured of it; " and during the ſpace of an eleven months paſt ſince it firſt invaded me, I have " taken ſo many medicines, and found the relief they afforded me ſo very ſlow, that " it is not eaſy for me to tell you what I found moſt good by. The things, which to " me ſeem the fitteſt to be mentioned on this occaſion, are, that cordial medicines, " eſpecially ſuch as peculiarly befriend the *genus nervoſum*, were very frequently and " not unuſefully adminiſtered. That I uſed during this ſickneſs leſs purging phyſic, " and that gentle, than in many years before; and found cauſe to think ſuch evacua- " tions very weakening, and, when they are not very neceſſary, dangerous. That the " dried fleſh of vipers ſeemed to be one of the uſefulleſt cordials I took; but then I " perſevered in taking it daily for a great while. That I ſeldom miſſed a day with- " out taking the air, at leaſt once, and that even when I was at the weakeſt, and was " fain to be carried in men's arms from my chair into the coach. That the beſt thing " I found to ſtrengthen my feet and legs, and which I ſtill uſe, was ſack turned to a " brine with ſea-ſalt, and well rubbed upon the parts every morning and night with " a warm hand. That for my hands I uſe ſeveral things, and particularly palm oil, " which comes from *Guinea*, and a liquor ſomewhat like the *ſpiritus lavendulæ compoſ.* " of the diſpenſatory, and alſo fomentations with cephalic flowers and herbs, one " or other of which I yet daily continue. But yet I found nothing ſo available as " frequent exerciſe of my hands and feet, in which I continued as far as my ſtrength " would poſſibly allow me, getting ſometimes others to bend the joints of my arms and " hands for me. And though this courſe makes me every day ſore and weary, yet I " continue to undergo it, becauſe I think I find more benefit by it alone, than by " all the outward applications of phyſicians and chirurgeons. Theſe, Sir, are in

" brief the chief things that I have obſerved about my own diſtemper; but how " far they will be applicable to that of your friend, I do not know half ſo well, as " that in caſe he uſe them, they will prove very effectual, if any thing can be added " to their virtue by the good wiſhes of,

" SIR,

" Your moſt humble and affectionate ſervant,

" R. B."

THE ſame year he publiſhed *Some Conſiderations touching the uſefulneſs of experimental and natural Philoſophy, propoſed in a familiar Diſcourſe to a Friend, by way of invitation to the ſtudy of it. The ſecond tome, containing the latter ſection of the ſecond part:* printed at *Oxford* in 4to. and *Tracts: Of a Diſcovery of the admirable Rarefaction of the Air: New Obſervations about the Duration of the Spring of the Air: New Experiments touching the Condenſation of the Air by mere Cold, and its Compreſſion without mechanical Engines: The admirable differing Extenſion of the ſame Quantity of Air rarified and compreſſed: London*, in 4to; a few copies of which *Tracts* were reprinted in 1739, but with the former date, only the *errata* in the firſt edition were corrected in it. He communicated likewiſe in the *Philoſophical Tranſactions*, N° LXXIV. p. 2216. *An Obſervation of a Spot in the Sun, April* 27, 1660.

HIS next work was *An Eſſay about the Origin and Virtue of Gems; wherein are propoſed and hiſtorically illuſtrated ſome Conjectures about the Conſiſtence of the Matter of Precious Stones, and the ſubjects, wherein their chiefeſt Virtues reſide:* printed at *London*, 1672, in 8vo, and tranſlated into Latin. In this *Eſſay* he informs us, that gems were once fluid, and have their virtues from mineral matter; which he ſhews from their tranſparency, figuration, internal texture, their colours being probably adventitious; heterogeneous matter having been found in their ſubſtance, and metalline or mineral mixtures mixed with their ſmall parts. He obſerves further, that a greater ſpecific gravity in gems argues their metalline or mineral nature, and that metals or minerals may be extracted from them. He tells us, that mineral productions are exceedingly numerous in the bowels of the earth, and that various menſtrua are likewiſe to be found there; and ſhews the formation of gems from a petrifying juice or ſpirit mixing in a ſufficient proportion with the impregnated waters in the earth, ſo as to coagulate and unite with them. That ſome of their real virtues may be derived from hence; that whilſt they were in a fluid form, the petreſcent ſubſtance was mixed with mineral ſolution, tincture, or other impregnated liquor; and that theſe were afterwards coagulated or united, and hardened into a gem.

The ſame year he publiſhed his *Tracts, containing, New Experiments touching the Relation between Flame and Air; and about Exploſion: An hydroſtatical Diſcourſe, occaſioned by ſome Objections of Dr.* Henry More *againſt ſome Explications of new Experiments made by the Author of theſe Tracts. To which is annexed an hydroſtatical Letter, dilucidating an Experiment about a Way of weighing Water in Water: New Experiments of the poſitive or relative Levity of Bodies under Water: About the differing Preſſure of heavy Solids and Fluids: London*, in 8vo. *Some Obſervations about ſhining Fleſh, both of Veal and of Pullet, and that without any ſenſible putrefaction in thoſe bodies:* printed in

in the *Philos. Transact.* N° LXXXIX. p. 5108. and *A new Experiment concerning the Effect of the varying Weight of the Atmosphere upon some Bodies in the Water*; and *suggesting a conjecture, that the very Alteration of the Air in point of Weight may have considerable Operations even upon men's sickness or health: Philosophical Transactions*, N° XCI. p. 5156.

THE year 1673 produced to the world his *Essays of the strange Subtilty, great Efficacy, determinate Nature of Effluviums. To which are annexed New Experiments to make Fire and Flame stable and ponderable; with additional Experiments about arresting and weighing of igneous Corpuscles; together with a Discovery of the Perviousness of Glass to ponderable Parts of Flame:* printed at *London* in 8vo, and translated into Latin: and *A Letter concerning Ambergrease, and its being a vegetable production: Philos. Transact.* N° XCVII. p. 6113.

HE wrote likewise, in December that year, the following letter to a gentleman in the *Levant*, whom he desired to transmit to him such observations as he thought proper, concerning those countries, and to whom he now sent several copies of Dr. *Edward Pococke*'s Arabic translation of *Grotius*'s book *De Veritate Religionis Christianæ*, as a means of propagating Christianity there.

" SIR, *London*, Decem. 10, 1673.

" I WERE as unjust to you, as unkind to myself, if I should at all deceive you " in the expectation you had, that your letter would not be thought by me, either " to need an apology, or be an impertinence. That liberty of writing to me, which " you out of modesty are pleased to stile a privilege, is that, whose effects I shall " always look upon as favours; and the greater, if you give yourself the trouble, " and me the satisfaction, to let them convey to me what you call the stories and " observations of the country you are in; for it cannot but be welcome as well as " instructive to a person, that does as much desire as want good observations about " the *Levant*, to receive accounts of such remote places and things from so sure a " hand as yours.

" I HOPE I shall not allow myself the vanity of receiving the compliments you " make me in reference to *Grotius*, but as effects of your civility and charity; what " I have been able to do, having been but to hand over to others those lamps, which " that excellent person framed and lighted. But I shall confess to you, that I have " a great deal of satisfaction to find ground to hope, by what you tell me, that my " poor endeavours to promote the Gospel in those parts, are not like to prove altogether fruitless. And though they were first and chiefly designed for the conversion of infidels (to which end I hope you will in due time attempt to make them " serviceable) yet I must not think them misemployed, if they be blessed to inform or " confirm the poor and ill-instructed Christians in those parts, especially since the " same arguments that work on them, may, through his blessing, for whose truth " they militate, enable them in time to work on their Mahometan countrymen. I " have therefore by the merchant, to whom you directed me, sent you three dozen " more of those Arabic books, bound as the others were, being not solicitous to " exceed that number now, both because I had but short warning given me of the " departure of the ships, and because of the danger of the seas; but I hope hereafter to send a further supply; and in the mean time must return you my humble " thanks, both for your zeal and concern in a work, that aims at the glory of our

" common

" common master, and for those obliging prayers, the continuation of which is " earnestly begged of you by,

" SIR,

" Your most affectionate friend and very humble servant,

" R. B.

" A SMALL treatise of mine being lately ventured abroad, I presume the relish you " formerly had for philosophical studies, may keep that from being altogether unac- " ceptable to you."

THE same year *Anthony le Grand*, an eminent Cartesian Philosopher, dedicated to him his *Historia Naturæ variis Experimentis & Ratiociniis elucidata, secundum Principia stabilita in Institutione Philosophiæ edita ab eodem Authore*, printed at *London*, in 8vo. This gentleman was born at *Doway* in *Flanders*, and entered himself in the order of Dominican friars, and was afterwards sent to preside over the mission in *England*, where he resided many years, and in August 1695 was tutor to the eldest son and heir of Mr. *Farmer* of *Tusmore* in *Oxfordshire*[1]. In his dedication he does justice to Mr. *Boyle*'s universal reputation for extensive learning and amazing sagacity in every part of experimental philosophy; and applies to him what *Averroes* said of *Aristotle*, that nature had formed him as an exemplar of the highest perfection, to which mankind can attain; and observes, that the Royal Society paid the highest deference to his judgment: *Adeo diffusa est, per totam Angliam, imo per orbem universum, nominis tui gloria, ut vix inter eruditos annumeretur, qui sapientiam tuam non agnoscit, & cultu peculiari non amplectitur. Enimvero quis* unquam *te* doctior, *ut quondam de Aristotele Romanus orator*, quis acutior, quis in rebus vel inveniendis vel judicandis acrior fuit? *Ita enim in rebus adinveniendis solers es, ita in detegendis naturæ arcanis sagax, ita in promovendâ experimentis philosophiâ felix, ut non immerito de te, quod in Aristotelis laudes aliquando veluti lymphatus est Averroes, dici queat, quod* regula & exemplar sis, quod natura invenit ad demonstrandam ultimam perfectionem humanam. *Hæc omnium doctorum, qui te norunt, sententia, cui suffragatur celeberrima Londinensis Regia Societas, quæ tam præclara artis & industriæ suæ edidit specimina, dum in omnibus, quæ aggreditur, nihil te inconsulto molitur, te asserente, nunquam dissentit, te impugnante, nunquam obluctatur.*

IN 1674 Mr. *Boyle* published his *Tracts, consisting of observations about the saltness of the sea: An Account of a Statical Hygroscope and its uses, together with an Appendix about the Force of the Air's Moisture: A Fragment about the natural and preternatural state of Bodies. To all which is premised a sceptical Dialogue about the positive or privative nature of Cold: with some Experiments of Mr.* Boyle's *referred to in that discourse:* printed at *London*, in 8vo, and translated into Latin. In the *Observations concerning the saltness of the sea*, he tells us, that the true cause of it is from the salt, which is dissolved in it; which salt is supplied, not only from rocks and other masses of salt, which at the beginning were, or in some places may yet be found, either at the bottom of the sea, or at the sides, where the water can reach them; but also from the salt, which the rains, rivers, and other waters dissolve in their passage through

[1] *Wood. Athen. Oxon.* Vol. II. col. 819. 2d Edit.

many

many parts of the earth, and at length carry along with them into the sea. He informs us of a method to make sea-water fresh by distillation; and observes, that the taste of sea-water is not such a simple saline taste, as spring-water would receive from *sal-gemmæ*, or some other pure terrestrial salt dissolved in it, but a bitterish one: which must be derived from some peculiar cause, which authors commonly overlook; and is occasioned by the liquid and other bitumen, which is carried by springs and other waters into the sea; and he suspects, that the various motion of the sea, and its being exposed to the action of the air and sun, may contribute to give it a taste other than saline, since sea-salt by being barely exposed for many months to the air, and sometimes put into a gentle agitation by a digestive heat, acquired a very different taste from the simple solution of sea-salt in common-water. He remarks likewise, that there are various degrees of saltness in different parts of the sea. In his *Account of a Statical Hygroscope* he informs us, that it is designed to shew, 1. The different variations of weather in the same month, day, and hour. 2. How much one year and season is drier or moister than another. 3. To discover and compare the changes of the temperature of the air made by winds, strong or weak; frosty, snowy, or other weather. 4. To compare the temperature of different houses and different rooms in the same house. 5. To observe in a chamber the effects of the presence or absence of a fire. 6. To keep a chamber at the same degree, or at an assigned degree of dryness. In the *Appendix about the Force of the Air's Moisture* he gives us instances of that force at all seasons, upon animal substances, vegetables, metals, and minerals, with a description of a *cord hygrometer*.

THE second work, which he published the same year, was intitled, *The Excellency of Theology compared with Natural Philosophy, as both are the Objects of Mens Study; discoursed of in a Letter to a Friend. To which are annexed, some occasional Thoughts about the Excellency and Grounds of the mechanical Hypothesis:* printed at *London* in 8vo, and translated into Latin. This discourse was written in 1665, while Mr. *Boyle*, to avoid the great plague, which then raged in *London*, was obliged to retire into the country, and frequently to pass from place to place, unaccompanied with most of his books*.

NEXT followed his *Tracts: containing*, I. *Suspicions about some hidden Qualities of the Air; with an Appendix touching Celestial Magnets, and some other Particulars.* II. *Animadversions upon Mr.* Hobbes's Problemata de Vacuo. III. *A Discourse of the Cause of Attraction by Suction.* Printed at *London*, 1674, in 8vo, and translated into Latin. In the *Suspicions about some hidden Qualities of the Air*, he mentions some unheeded causes of the salubrity or insalubrity of the air, which may be comprised under the following propositions: 1. That it is probable, that in several places the salubrity or insalubrity of the air in general may in a great measure depend on subterraneous expirations. 2. That in several places endemical distempers chiefly or partly depend on subterraneous steams. 3. That epidemical distempers are in a great measure occasioned by subterraneous *effluvia*. 4. That most diseases called new ones, chiefly or concurrently, depend on those *effluvia*. He then points out the uses of his doctrine of subterraneous and celestial effluvia in the air; and tells us, 1. That we may take occasion from thence to consider, whether several changes of temperature and constitution in the air, both as to manifest and latent qualities, may not sometimes be derived from the scarcity or plenty, and peculiar nature of one or both of these sorts of *effluvia*. 2. That possibly some bodies, which we are conversant with, may have a peculiar nature, disposition, and fitness to be wrought upon by, or to be associated with some

* See the publisher's advertisement to the reader.

of

of those foreign *effluvia*, emitted by unknown bodies lodged under ground, or which proceed from some particular planet. For what we call *sympathies* and *antipathies* depending really on the peculiar textures and other modifications of the bodies, between which these friendships and hostilities are said to be exercised, I see not, says he, why it is impossible, that there should be an affinity between a body of a fit or convenient texture, especially as to the shape and size of its pores, and the *effluvia* of any other body, whether subterraneal or sidereal. 3. That it may be considered, whether among the bodies, which we are acquainted with here below, there are not some, which may prove receptacles, if not also the attractives of sidereal and other foreign *effluvia*, which rove up and down in the air. By attractives are meant such magnetical bodies, as are fitted to detain and join with *effluvia*, when by virtue of the various motions, which belong to the air as a fluid, these happen to accost them. He mentions likewise the great effect of the air in producing colours, and some *experiments* to manifest some *hidden qualities in the air*, and some *observations about the growth of metals in their ore exposed to the air.*

HE communicated also to the editor of the *Philosophical Transactions*, *An Account of the two sorts of the* Helmontian *Laudanum, together with the Way of the noble Baron* F. M. Van Helmont, *Son to the famous* Johannes Baptista, *of preparing his Laudanum.* No. CVII. p. 147.

IN 1675 he published at *London*, in 8vo, *Some Considerations about the Reconcileableness of Reason and Religion. By* T. E. *a Layman. To which is annexed by the Publisher a Discourse of Mr.* Boyle *about the Possibility of the Resurrection.* In the beginning of this treatise there is mention made of its being to consist of two parts; one to shew, that a Christian *need not* lay aside his reason; and the other, that he is *not commanded* to do so; but the author thought fit to keep that paper, which related to the latter, from accompanying the former, which seemed the most seasonable, and likeliest to make impressions on that sort of persons, whom he chiefly designed to persuade. Several pieces of his were likewise printed the same year in the *Philosophical Transactions*, viz. *A Conjecture concerning the Bladders of Air, that are found in Fishes, communicated by* A. J. *and illustrated by an Experiment suggested by the honourable Mr.* Boyle. No. CXIV. p. 310. *A new Essay-Instrument invented and described, together with the Uses thereof.* Ibid. No. CXV. p. 329. *Ten new Experiments about the weakened Spring, and some unobserved Effects of the Air; where occur not only several Trials to discover, whether the Spring of the Air, as it may divers ways be increased, so may not by other ways than Cold or Dilatation be weakened; but also some odd Experiments, to shew the Change of Colours producible in some solutions and precipitations by the Operation of the Air.* Ibid. No. CXX. p. 467. *An experimental Discourse of Quicksilver growing hot with Gold.* Ibid. No. CXXII. p. 516. In this last piece he observes, that he had many years before made trials, whether some of the heterogeneous particles, which he found reducible with mercury into a lasting mercurial flux, might not so alter it, as to dispose it to heat with gold. *And that there were such*, says he, *through God's blessing, my trials afforded me positive proof about the year* 1652. For when he was alone, that he might not be imposed upon by others, he took to one part of his prepared mercury sometimes half the weight, and sometimes an equal weight of refined gold reduced to a calx or subtle powder. This he put into the palm of his left hand, and putting the mercury upon it, stirred it and pressed it a little with the fingers of his right hand, by which the two ingredients were easily mingled, and grew not only sensibly but considerably hot, and that so nimbly, that the incalescence sometimes came to its height in about a minute of an hour by a minute clock. He found the experiment

experiment succeed, whether he took altogether, or but half as much gold as mercury; but the effect seemed to be much greater, when they were employed in equal weight. He tried also the same mercury with refined silver reduced to very fine powder; but he could not perceive any heat or warmth at all, though he was apt to think, with a sufficient quantity of leaf-silver it might have been sensible. He made trial oftner than once in the hands of others, who were not a little surprised and pleased with the event; and particularly having given the ingredients to Mr. *Oldenburg*, he desired him to make the experiment in and with his own hand, in which it proved successful within somewhat less than a minute of an hour. Mr. *Boyle* however tells us, that he will not hence determine, whether those that are *mercurii corporum*, and were made, as chemists presume, by extraction only from metals and minerals, will each of them grow hot with gold; " as (says he) if I much mistake not, I " found antimonial mercury to do. Nor will I affirm, that every metalline mercury, " though never so disposed to incalescence, or even that of silver and gold itself, is " the same with that, which the Chrysopœan writers mean by their *philosophic mercury*, " or is near so noble as this. Nay I will not so much as affirm, that every mercury " obtained by extraction, even from the perfect metals themselves, must needs be " more noble and fit, as alchemists speak, for the *philosophic work*, than that, which " common mercury skilfully freed from its recrementitious and heterogeneous parts, " and richly impregnated with the subtle and active ones of congruous metals or " minerals.—But if there be any truth in what some of the most approved Spagy- " rists have delivered about a solvent of gold, that seems of kin, and perhaps is not " much nobler than one that I had; it seems allowable to expect, that even ours " should be of more than ordinary use both in physic and alchemy." This discourse occasioned Mr. (afterwards Sir) *Isaac Newton* to write the following * letter to Mr. *Oldenburg* from *Cambridge*, dated April 26, 1676.

" SIR,

" * * * * Yesterday I reading the two last *Philosophical Transactions* had the op- " portunity to consider Mr. *Boyle*'s uncommon experiment about the *incalescence of* " *gold and mercury*. I believe the fingers of many will itch to be at the knowledge " of the preparation of such a mercury; and for that end some will not be wanting " to move for the publishing of it, by urging the good it may do in the world. " But, in my simple judgment, the noble author, since he has thought fit to reveal " himself so far, does prudently in being reserved in the rest. Not that I think any " great excellence in such a ☿, either for medical or chemical operations; for it " seems to me, that the metalline particles, with which that ☿ is impregnated, may " be grosser than the particles of the ☿, and be disposed to mix more readily with " the ☉ upon some other account than their subtilty; and then in so mixing, their " grossness may enable them to give the parts of the gold the greater shock, and so " put them into brisker motion than smaller particles could do, much after the " manner that the saline particles, wherewith corrosive liquors are impregnated, heat " many things, which they are put to dissolve, whilst the finer parts of common " water scarce heat any thing dissolved therein, be the dissolution never so quick. " And if they do heat any thing (as quick lime) one may suspect, that heat is pro- " duced by some saline particles lying hid in the body, which the water sets on work " upon the body, which they could not act on, whilst in a dry form. I would com-

* In the possession of *William Jones*, Esq.

"pare therefore this impregnated ☿ to some corrosive liquor as aqua fortis; the "mercurial part of the one to the watery or phlegmatic part of the other, and the "metallic particles with which the one is impregnated, to the saline particles with "which the other is impregnated; both which I suppose may be of a middle nature "between the liquor which they impregnate, and the bodies they dissolve, and so "enter those bodies more freely, and by their grossness shake the dissolved particles "more strongly than a subtiler agent would do. If this analogy of these two kinds "of liquor may be allowed, one may guess at the little use of the one, by the indispo-"sition of the other, either to medicine or vegetation. But yet because the way by "which ☿ may be so impregnated, has been thought fit to be concealed by others "that have known it, and therefore may possibly be an inlet to something more no-"ble, not to be communicated without immense damage to the world, if there should "be any verity in the Hermetic writers; therefore I question not, but that the great "wisdom of the noble author will sway him to high silence, till he shall be resolved "of what consequence the thing may be, either by his own experience, or the judge-"ment of some other that thoroughly understands what he speaks about; that is, of "a true Hermetic philosopher, whose judgment (if there be any such) would be more "to be regarded in this point, than that of all the world beside to the contrary, "there being other things beside the *transmutation of metals*, (if those great preten-"ders brag not) which none but they understand. Sir, because the author seems "desirous of the sense of others in this point, I have been so free as to shoot my bolt; "but pray keep this letter private to yourself.

"Your servant,

"ISAAC NEWTON."

IN 1676 Mr. *Boyle* gave the public his *Experiments, Notes, &c. about the mechanical origin or production of divers particular qualities: among which is inserted a Discourse of the Imperfection of the Chemists Doctrine of Qualities; together with some Reflections upon the Hypothesis of Alkali and Acidum:* and likewise discourses *of the mechanical origin of Heat and Cold: Experiments and Observations about the mechanical production of Tastes: Of Odours: Advertisements about the Experiments and Notes relating to chemical Qualities: Experiments and Notes about the mechanical Origin and production of Volatility:—Of Fixedness:—Of Corrosiveness and Corrosibility: Of the mechanical Causes of chemical Precipitation: Experiments and Notes about the mechanical Production of Magnetism, and of Electricity.* Printed at *London* in 8vo. In the *Mechanical Origin of Heat and Cold* he shews, how a great degree of coldness may be produced at any time of the year. That a coldness may be produced by the mixture of warm bodies, and heat by the mixture of cold bodies. That ebullition is no argument of heat. That the same body, which cools some liquors, may heat others. That heat is mechanically producible. That agitation, and various determinations, and smalness of the moving particles, are requisite to heat. And that not only weather-glasses, but likewise our senses, may give us misinformation about cold by several predispositions, and the temper of our sensories, as we find it colder in the air coming out of a hot bath, than when only out of a warm room. In his *Experiments and Observations about the mechanical Production of Tastes*, he defines taste, as belonging to the object, to be that quality, or whatever else it be, which enables a body, by its operation, to produce in us that sensation which we feel, or perceive, when we say we taste; and then shews, that this quality depends upon the shape, size, motion, and other mechanical affections of the small parts

parts of the tasted body, and results from the association of two or more of them, not excluding their congruity or incongruity to the organs of tasting. In his *Experiments and Notes about the mechanical Origin and Production of Volatility*, he informs us, that to make a body volatile, the parts must be very small, not too solid or heavy, but conveniently shaped for motion; and that they loosely adhere, or at least be united in such a manner, as does not much indispose them to be separated by the fire in the form of fumes or vapours. With regard to *Fixedness*, he supposes the qualifications that conduce most to it are, 1. The grossness or the bulk of corpuscles the body consists of. 2. The ponderousness or solidity of those corpuscles. 3. The inaptitude of them for avolation, by reason of their branchedness, irregular figures, crookedness, or other inconvenient shape, which entangles the particles among one another, and makes them difficult to be extracted. He explains the theory of *Corrosiveness*, by observing, 1. That it is requisite, that a corrosive medium should abound with corpuscles not too big to get in at the pores of the body to be dissolved, nor yet so minute as to pass through them, as the rays of light through glass; or to be unable, by reason of their great slenderness and flexibility, to disjoin the parts which they invade. 2. That these corpuscles be of a shape fit to insinuate themselves into the pores abovementioned, in order to dissociate the solid parts. 3. That they have a competent degree of solidity to disjoin the particles of the body to be dissolved; which solidity of solvent particles is somewhat distinct from their bulk, as may appear by comparing a stalk of wheat and a metalline wire of the same diameter. 4. That the corpuscles of the menstruum be such, as are fit to separate the parts of the invaded body, either by their shape, minuteness, or fitness to have their action befriended by assisting causes; such as, first, the pressure of the atmosphere, which may impel them into the pores of bodies, not filled with a substance so resisting as common air; and secondly, that the intruding corpuscles may be fitted to receive in those pores by the transcursion of subtile ætherial matter, or by the numerous pulses of the swimming corpuscles of the menstruum itself, whereby, like so many little wedges and levers, they may be enabled to force asunder the little parts between which they insinuate. This theory he confirms by experiments. He then explains that of *Corresibility*, which property appears to him to consist in three requisites: 1. That the body to be corroded have pores of such a magnitude and figure, that the corpuscles of the solvent may enter them, and agitate the solid parts that inclose them. 2. That its consistent corpuscles be of such a bulk and solidity, as does not render them incapable of being di joined by the action of the insinuating corpuscles of the menstruum. 3. This property consists in such a cohesion of the parts, as is not too strong to be superable by the action of the menstruum. This theory he likewise confirms by experiments. With respect to *Precipitation* he observes, that by it is meant such an agitation or motion of an heterogeneous liquor, as makes the parts of it subside in the form of a powder or other consistent body; and that it may be mechanically accounted for from the gravity and bulk of the dissolved body, from weakening the solvent, and by presenting another body to be wrought upon, *&c.* And with regard to *Electricity*, he tells us, that electrical bodies act mechanically; and that electricity is producible by mechanical changes.

He communicated to the public the same year, in the *Philosophical Transactions*, *New Experiments about the superficial Figures of Fluids, especially of Liquors contiguous to other Fluids* No. CXXXI. p. 775. and *A Continuation of the Experiments published in the next foregoing Tract about Fluids contiguous to other Fluids.* Ibid. No. CXXXII. p. 799.

He had been for many years a director of the *East-India* company, and greatly instrumental in procuring the charter of it; an abstract of which is extant among his papers. But his health now not permitting him to attend the committee of that company, he wrote the following letter to *Robert Thompson*, Esq; in order to recommend to them the propagation of the Gospel in those countries, where their commerce gave them an opportunity.

" Worthy SIR, *Pall-Mall*, March 5, 1676-7.

" THE continuance of my distemper not permitting me to wait on you, as I " gladly would, in *London*, you will, I doubt not, give me leave to present you " in this paper some of the things, that I would more fully have acquainted you with " by word of mouth. You may remember, that when my health, and the kindness " of the *East-India* company, allowed me to sit in your committee, I ventured to " make a motion, that some course might be thought on of doing some considerable " thing for the propagation of the gospel among the natives, in whose countries we " have flourishing factories. And indeed it seemed to me very fit, that we, whose " endeavours God had of late so signally prospered, should pay him some little ac- " knowledgment of his many blessings, and that remembering ourselves to be Chri- " stians, as well as merchants, we should attempt to bring those countries some spi- " ritual good things, whence we so frequently brought back temporal ones. And I " was somewhat the more concerned to succeed in the motion I made, because I wished " the company were enough to desire, that they in particular should have the honour " to silence the reproaches of those who I wish had less pretence to upbraid the Pro- " testants, and among them the English, with the neglect of making proselytes to the " Christian religion; to the advancement of which I endeavoured to excite you by what " hath been done by the Dutch company in *Batavia*. I must do and have done those " that were present at my proposal, the right to say, that they gave me a very favou- " rable hearing, and readily consented to take the matter into further consideration. " But as my sickness disabled me from going so far as the *East-India* house, so other " accidents have from time to time been able to produce such delays, that notwith- " standing the interposition of my lord *Berkeley*, (who uses not to be backward, when " good is to be done) the good intentions of the company have hitherto proved inef- " fectual. But I am glad to find, that now the consideration of that business is to " be seriously resumed among you, my lord *Berkeley* having yesterday done me the " honour to visit me, and discourse with me about this affair. And since I cannot " wait on you myself, and have acquainted him with some of my thoughts, I shall " desire you at your next meeting to confer with his lordship about the ways of pro- " moting so good a work. And I the rather solicit this now, because since the be- " ginning of this week I received the honour of a visit from the bishop of *Oxford*, " with whom discoursing of divers things relating to religion, his lordship seemed to " wonder, that the *East-India* company should do nothing towards the spreading of " it in the country they trade to, and seemed unwilling to send able men thither for " ministers. And though I took this occasion to right the company by shewing him " that he had been misinformed, and to relate to him the matter of fact, which I could " do in great part upon my own knowledge; yet his lordship having assured me, that " in case reasonable encouragement were given, sober and learned men should be fitted " in the university to be sent into *India*, and furnished not only with the Arabic tongue, " but, if it were desired, with arithmetic and other qualifications fit to recommend " them,

" them, and make them appear more considerable, and grow more useful in those " parts; I cannot but take this rise to solicit you to resume in good earnest the " thoughts of speedily doing something worthy of the famous *East-India* company of " *England*. The way I leave to your wisdoms, not despairing, that if but so much " be done at first as may be carried without considerable opposition, the goodness of " the work will procure a blessing on it that will make it prosperous; and the suc- " cess will invite perhaps many more than your own company to be co-operators " with the truth, and contributors to the enlarging the Pale of the Christian church. " If you should now ask me, what is to be done with any stock or income that may " be raised for such an use? I shall willingly at large acquaint you with my poor " thoughts the next time you pass this way; and in the mean time give me leave to " put you in mind of what is done in the corporation (whereof you are a member) " for gospellizing (as they phrase it) the natives of *New England*. For the ways we " employ to convert and instruct them, are chiefly these: First, We have caused " the holy scriptures and some few choice practical books to be translated into their " chiefest language. To which you may add the publishing of a solid but civilly " penned confutation of the authentic books wherein the Bramins religion is " contained.

" NEXT, we have caused some of ours to learn their tongue; and having convinced " them of their idolatry and the sinfulness of their courses, to preach to them, and to " catechise them in their own language.

" AND then we breed some of their hopeful forward youths to that knowledge of " the English tongue and European learning, that they may afterwards be able to con- " fute the idolatrous priests, and convert and instruct their own countrymen.

" OTHER methods we do on some occasions employ; but to particularise and insist " on them, would require more than a letter; and this, I fear, is grown too tedious " already, and therefore I shall here conclude it with my hearty wishes, that, espe- " cially at a time when the company wants not enemies, they may be directed to do " a thing so good in itself, and of so good a report as that which has been recom- " mended to you, I hope, by your own piety, as well as by the motion of,

" SIR,

" Your most affectionate friend, and most humble servant,

" ROBERT BOYLE."

His zeal for the interest of the Christian religion, appeared the year following in another remarkable instance; for he was at the expence of printing five hundred copies of the *four Gospels*, and *Acts of the Apostles*, in the Malayan tongue, under the direction of Mr. (afterwards Dr.) *Thomas Hyde*, keeper of the Bodleian library. This work was published at *Oxford* in 1677 in 4to, with this title: *Jang Ampat Evangelia derri tuan Kitu Jesu Christi duan Beorboutan derri jang Apostoli Borsaeti Bersalin dallam Bassa Malayo. That is, The four Gospels of our Lord Jesus Christ, and the Acts of the holy Apostles, translated into the Malayan tongue:* with a preface by Dr. *Thomas Marshal*, rector of *Lincoln* college in *Oxford*, and afterwards dean of *Gloucester*, and with the following dedication by Mr. *Hyde*, which I shall here insert from the original.

" *To*

" *To the honourable* Robert Boyle, *Esq; one of the directors of the* East-India " *company for trade, and governor of the corporation for the propagation of* " *the Gospel and the conversion of the American natives in* New England.

" SIR,

" YOUR pious design of printing the holy Gospels, and the Acts of the Apostles, " in the Malayan tongue, being by God's assistance finished at your cost and " charge, as correct as all my diligence could possible make it, the work now done " returns to you, offering itself to be employed to that good end and purpose for which " you intended it, *viz.* for God's glory, and the good of the souls of those poor " Indians, to whom the glorious rays of the Gospel have not yet shone, or at least in " a very obscure manner; that so by this means they who sat in darkness and the " shadow of death, may have great light risen. And the conversion of such miserable " infidels from their gross idolatry, and the gaining of souls to heaven, being a work " so very acceptable to both God and man; we well hope, that this your good ex- " ample will prove an incitement to others of our nation to procure and promote " such like laudable undertakings. But your charity is not limited only to the East- " Indians, for the poor souls of the West-Indians are also bound to bless you, you be- " ing the head of that corporation, which is established by his Majesty at *London* for " the receiving and disposing of the benefactions of well-minded Christians (to which " the said corporation do usually add of their own no small mites) to be transmitted " to the commissioners of the united colonies in *New England*, and there to be em- " ployed for the propagation of the Gospel. By the sole cost and care of this fore- " mentioned corporation it was, that the whole Bible, and some other books of piety, " were translated into the language of *New England* by the pains of the reverend Mr. " *Eliot*, who made a grammar, now also in print, for that language, and who daily " labours in the work of the Gospel there, he having an honorary stipend continued " to him by the abovementioned corporation. Wherefore, if charity is not grown " too cold in these northerly parts of the world, those things, which have been already " performed, may justly be expected to be sufficient incentives to kindle and actuate " the zeal of many good people, by whose pious endeavours, in the promoting of " the Gospel of our Lord Jesus Christ, all the ends of the earth may in time see the " salvation of God. All which I have now to add is, that God in his goodness would " be pleased to continue to you long life and happiness; that so you may chearfully go " on to finish the good works which you have begun, and which you farther intend; " which is the hearty prayer of,

" SIR,

Sept. 14, 1677, from the public library in *Oxford*.

" Your very obliged friend,

" and most humble servant,

" THOMAS HYDE."

THE same year there was published at *Geneva*, in 4to, a collection of some of our author's works, which had been translated into Latin, under this title, ROBERTI BOYLE, *nobilissimi Angli, & Societatis Regiæ dignissimi socii, opera varia*; of which collection Mr. *Oldenburg*, in the *Philosophical Transactions*, No. CXXX. p. 766, 767, gives the follow-

following account: 1. That is was published without the consent or knowledge of the author. 2. That the year in the frontispiece of it is one and the same, as if the several treatises contained in this volume had been published in one year; and that the enumeration of those treatises, made in the catalogue of this Latin edition, is not according to the time wherein they were first printed; for the *first* of the books mentioned in that catalogue was published in English in 1660, the *fifth* and *sixth* in 1661, the *second* in 1662, the *seventh* in 1664, the *fourth* in 1666, the *third* in 1670, the *eighth* in 1671, the *tenth* in 1672, and the *ninth* in 1673, "so preposterously are those "books ranged in this catalogue and volume; which the reader was to be informed "of, that by comparing the several true dates of the first edition of this author's "works with the books of others since printed, the priority of the experiments, and "considerations respectively contained in them, may be truly stated." 3. That there is no mention made in the general title, nor in any advertisement, that these books are all of them translations out of English, in which tongue the author wrote them all. 4. That the treatise of the *Origin of Forms and Qualities*, and that of *subordinate Forms*, are both omitted in this volume, though they were printed even in Latin, at *Oxford*, ever since the year 1669, as they had been printed in English in 1667.

IN 1678 Mr. *Boyle's Short Memorial of some Observations made upon an artificial Substance, that shines without any preceding Illustration*, was published in Mr. *Hook's Lectiones Cutler.* No. II. p. 57. and his *Historical Account of a Degradation of Gold made by an Anti-Elixir, a strange chemical Narrative*, was printed at *London* in 4to, and reprinted there in 1739 in 4to. In this piece, *Pyrophilus*, one of the interlocutors of the Dialogue, observes, that though he could not affirm, that "he had with his own "hands made *projection* (as chemists are wont to call the sudden transmutation made "by a small quantity of their admirable elixir) yet I can affirm, *says he*, much of "what hath been urged for the possibility of such a sudden change of a metalline body "by a way which, I presume, will surprise you. For to make it more credible, that "other metals are capable of being graduated or exalted into gold, by way of *pro-* "*jection*, I will relate to you, that *by the like way* gold has been degraded or imbased. "If, *continues he*, it can be made appear, that art has produced an *anti-elixir*, or "agent, that is able in a very short time to work a very notable though deteriorating "change upon a metal, in proportion to which its quantity is very inconsiderable; "I see not why it should be thought impossible, that art may also make a *true elixir*, "or powder, capable of speedily transmuting a great proportion of a baser metal into "silver or gold; especially if it be considered, that those that treat of these *arcana*, "confess, that it is not every matter which may be justly called the *Philosophers* "*Stone*, that is able to transmute other metals in vast quantities, since several of these "writers (and even *Lully* himself) make differing orders or degrees of *elixir*, and ac-"knowledge, that a medicine or tincture of the first or lowest order, will not transmute "above ten times its weight of an imperfect metal." However the celebrated chemist *Otto Tachenius* was of opinion, that notwithstanding what is related in this narrative, salt of tartar is capable of recovering gold thus debased to its original purity.

THE regard which the great *Newton* had for Mr. *Boyle*, will appear from a very curious letter which the former wrote to him, explaining his sentiments upon one of the most abstruse points of philosophy with respect to the ætherial medium, which in his *Optics* he proposes as the mechanical cause of gravitation. This letter having never before seen the light, will be proper to be inserted here.

"Honoured

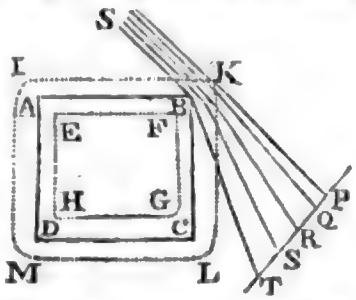
" Honoured SIR,

" I HAVE so long deferred to send you my thoughts about the physical qualities " we speak of, that did I not esteem myself obliged by promise, I think I should " be ashamed to send them at all. The truth is, my notions about things of this kind " are so indigested, that I am not well satisfied myself in them; and what I am not " satisfied in, I can scarce esteem fit to be communicated to others; especially in na- " tural philosophy, where there is no end of fancying. But because I am indebted " to you, and yesterday met with a friend, Mr. *Maulyverer*, who told me he was go- " ing to *London*, and intended to give you the trouble of a visit, I could not forbear " to take the opportunity of conveying this to you by him.

" IT being only an explication of qualities which you desire of me, I shall set down " my apprehensions in the form of suppositions, as follows. And first, I suppose, " that there is diffused through all places an æthereal substance, capable of contrac- " tion and dilatation, strongly elastic, and, in a word, much like air in all respects, " but far more subtile.

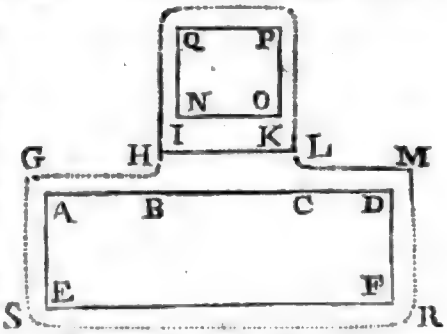
" 2. I SUPPOSE this æther pervades all gross bodies, but yet so as to stand rarer in " their pores than in free spaces, and so much the rarer, as their pores are less. And " this I suppose (with others) to be the cause why light incident on those bodies is " refracted towards the perpendicular; why two well polished metals cohere in a re- " ceiver exhausted of air; why ☿ stands sometimes up to the top of a glass pipe, " though much higher than 30 inches; and one of the main causes, why the parts " of all bodies cohere; also the cause of filtration, and of the rising of water in small " glass pipes above the surface of the stagnating water they are dipped into: for I " suspect the æther may stand rarer, not only in the insensible pores of bodies, but " even in the very sensible cavities of those pipes. And the same principle may cause " menstruums to pervade with violence the pores of the bodies they dissolve, the sur- " rounding æther, as well as the atmosphere, pressing them together.

" I SUPPOSE the rarer æther within bodies, and the denser without them, not to " be terminated in a mathematical superficies, but to grow gradually into one another; " the external æther beginning to grow rarer, and the internal to grow denser, at some " little distance from the superficies of the body, and running through all intermediate " degrees of density in the intermediate spaces. And this may be the cause why " light, in *Grimaldo*'s experiment, passing by the edge of a knife, or other opake " body, is turned aside, and as it were refracted, and by that refraction makes several " colours. Let ABCD be a dense body, whether " opake or transparent, EFGH the outside of the " uniform æther, which is within it, IKLM the inside " of the uniform æther, which is without it; and con- " ceive the æther, which is between EFGH and " IKLM, to run through all intermediate degrees of " density between that of the two uniform æthers on ei- " ther side. This being supposed, the rays of the sun " SB, SK, which pass by the edge of this body be- " tween B and K, ought in their passage through the " unequally dense æther there, to receive a ply from the denser æther, which is on " that side towards K, and that the more by how much they pass nearer to the body, " and thereby to be scattered through the space PQRST, as by experience they " are found to be. Now the space between the limits EFGH and IKLM, I shall " call the space of the æther's graduated rarity.



" 4. WHEN

" WHEN two bodies moving towards one another come near together, I suppose " the æther between them to grow rarer than before, and the spaces of its graduated " rarity to extend further from the superficies of " the bodies towards one another; and this, by " reason that the æther cannot move and play up " and down so freely in the strait passage be- " tween the bodies, as it could before they came so " near together. Thus, if the space of the æther's " graduated rarity reach from the body " ABCDFE only to the distance GHLMRS, " when no other body is near it, yet may it reach " farther, as to IK, when another body NOPQ " approaches: and as the other body approaches " more and more, I suppose the æther between them will grow rarer and rarer.



" THESE suppositions I have so described, as if I thought the spaces of graduated " æther had precise limits, as is expressed at IKLM in the first figure, and GMRS " in the second: for thus I thought I could better express myself. But really I do " not think they have such precise limits, but rather decay insensibly, and, in so decay- " ing, extend to a much greater distance than can easily be believed, or need be " supposed.

" 5. Now from the fourth supposition it follows, that when two bodies approach- " ing one another, come so near together as to make the æther between them begin " to rarefy, they will begin to have a reluctance from being brought nearer together, " and an endeavour to recede from one another: which reluctance and endeavour " will encrease, as they come nearer together, because thereby they cause the interja- " cent æther to rarefy more and more. But at length, when they come so near to- " gether, that the excess of pressure of the external æther, which surrounds the bodies, " above that of the rarefied æther, which is between them, is so great, as to overcome " the reluctance which the bodies have from being brought together; then will that " excess of pressure drive them with violence together, and make them adhere strongly " to one another, as was said in the second supposition. For instance, in the second " figure, when the bodies ED and NP are so near together, that the spaces of the " æther's graduated rarity begin to reach to one another, and meet the line IK; the " æther between them will have suffered much rarefaction, which rarefaction re- " quires much force, that is, much pressing of the bodies together: and the endea- " vour which the æther between them has to return to its former natural state of con- " densation, will cause the bodies to have an endeavour of receding from one another. " But, on the other hand, to counterpoise this endeavour, there will not yet be any " excess of density of the æther which surrounds the bodies, above that of the æther " which is between them at the line IK. But if the bodies come nearer together, so " as to make the æther in the mid-way line IK grow rarer than the surrounding " æther, there will arise from the excess of density of the surrounding æther a com- " pressure of the bodies towards one another: which when by the nearer approach " of the bodies it becomes so great, as to overcome the aforesaid endeavour the bodies " have to recede from one another, they will then go towards one another and adhere " together. And, on the contrary, if any power force them asunder to that distance, " where the endeavour to recede begins to overcome the endeavour to accede, they " will again leap from one another. Now hence I conceive it is chiefly, that a fly " walks on water without wetting her feet, and consequently without touching the

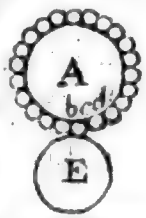
" water; that two polished pieces of glass are not without pressure brought to con-
" tact, no, not though the one be plain, the other a little convex; that the particles
" of dust cannot by pressing be made to cohere, as they would do, if they did but
" fully touch; that the particles of tinging substances and salts dissolved in water do
" not of their own accord concrete and fall to the bottom, but diffuse themselves all
" over the liquor, and expand still more, if you add more liquor to them. Also,
" that the particles of vapours, exhalations, and air, do stand at a distance from
" one another, and endeavour to recede as far from one another, as the pressure of
" the incumbent atmosphere will let them: for I conceive the confused mass of va-
" pours, air, and exhalations, which we call the atmosphere, to be nothing else but
" the particles of all sorts of bodies, of which the earth consists, separated from one
" another, and kept at a distance, by the said principle.

" FROM these principles the actions of menstruums upon bodies may be thus ex-
" plained. Suppose any tinging body, as cochineal, or logwood, be put into
" water; so soon as the water sinks into its pores and wets on all sides any particle,
" which adheres to the body only by the principle in the second supposition, it takes
" off, or at least much diminishes the efficacy of that principle to hold the particle to
" the body, because it makes the æther on all sides the particle to be of a more
" uniform density than before. And then the particle being shaken off, by any little
" motion, floats in the water, and with many such others makes a tincture; which
" tincture will be of some lively colour, if the particles be all of the same size and
" density; otherwise of a dirty one. For the colours of all natural bodies whatever
" seem to depend on nothing but the various sizes and densities of their particles;
" as I think you have seen described by me more at large in another paper. If the
" particles be very small (as are those of salts, vitriols, and gums) they are trans-
" parent; and as they are supposed bigger and bigger, they put on these colours in
" order, black, white, yellow, red; violet, blue, pale green, yellow, orange, red;
" purple, blue, green, yellow, orange, red, *&c.* as is discerned by the colours,
" which appear at the several thicknesses of very thin plates of transparent bodies.
" Whence, to know the causes of the changes of colours, which are often made by
" the mixtures of several liquors, it is to be considered, how the particles of any
" tincture may have their size or density altered by the infusion of another liquor.

" WHEN any metal is put into common water, the water cannot enter into its pores,
" to act on it and dissolve it. Not that water consists of too gross parts for this pur-
" pose, but because it is unsociable to metal. For there is a certain secret principle
" in nature, by which liquors are sociable to some things, and unsociable to others.
" Thus water will not mix with oil, but readily with spirit of wine, or with salts.
" It sinks also into wood, which quicksilver will not; but quicksilver sinks into
" metals, which, as I said, water will not. So aqua fortis dissolves ☽, not ☉, aqua
" regis ☉, not ☽, *&c.* But a liquor, which is of itself unsociable to a body, may,
" by the mixture of a convenient mediator, be made sociable. So molten lead,
" which alone will not mix with copper, or with regulus of Mars, by the addition
" of tin is made to mix with either. And water, by the mediation of saline spirits,
" will mix with metal. Now when any metal is put in water impregnated with such
" spirits, as into aqua fortis, aqua regis, spirit of vitriol, or the like, the particles of
" the spirits, as they, in floating in the water, strike on the metal, will by their
" sociableness enter into its pores, and gather round its outside particles, and, by ad-
" vantage of the continual tremor the particles of the metal are in, hitch themselves
" in by degrees between those particles and the body, and loosen them from it; and

" the water entering into the pores together with the saline spirits, the particles of " the metal will be thereby still more loosed, so as, by that motion the solution puts " them into, to be easily shaken off, and made to float in the water: the saline par- " ticles still encompassing the metallic ones as a coat or shell does a kernel, " after the manner expressed in the annexed figure. In which figure I " have made the particles round, though they may be cubical, or of any " other shape.

" If into a solution of metal thus made be poured a liquor, abounding with " particles, to which the former saline particles are more sociable than to the parti- " cles of the metal (suppose with particles of salt of tartar) then so soon as they strike " on one another in the liquor, the saline particles will adhere to those more firmly " than to the metalline ones, and by degrees be wrought off from those to enclose " these. Suppose A a metalline particle, enclosed with saline ones of spirit of nitre, " E a particle of salt of tartar, contiguous to two of the particles of " spirit of nitre b and c, and suppose the particle E is impelled by any " motion towards d, so as to roll about the particle c, till it touch the " particle d, the particle b adhering more firmly to E than to A, will be " forced off from A. And by the same means the particle E, as it rolls " about A, will tear off the rest of the saline particles from A, one after " another, till it has got them all, or almost all, about itself. And when " the metallic particles are thus divested of the nitrous ones, which, as a mediator " between them and the water, held them floating in it; the alcalizate ones crowd- " ing for the room the metallic ones took up before, will press these towards one " another, and make them come more easily together: so that by the motion they " continually have in the water, they shall be made to strike on one another, and " then, by means of the principle in the second supposition, they will cohere and " grow into clusters, and fall down by their weight to the bottom, which is called " precipitation.

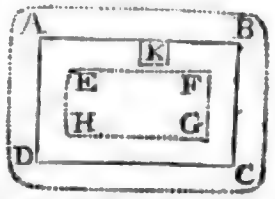


" In the solution of metals, when a particle is loosing from the body, so soon as " it gets to that distance from it, where the principle of receding described in the " fourth and fifth suppositions begins to overcome the principle of acceding, de- " scribed in the second supposition, the receding of the particle will be thereby ac- " celerated; so that the particle shall as it were with violence leap from the body, " and putting the liquor into a brisk agitation, beget and promote that heat we often " find to be caused in solutions of metals. And if any particle happen to leap off " thus from the body, before it be surrounded with water, or to leap off with that " smartness, as to get loose from the water; the water, by the principle in the fourth " and fifth suppositions will be kept off from the particle, and stand round about " it, like a spherically hollow arch, not being able to come to a full contact with it " any more. And several of these particles afterwards gathering into a cluster, so as " by the same principle to stand at a distance from one another, without any water " between them, will compose a bubble. Whence I suppose it is, that in brisk so- " lutions there usually happens an ebullition.

" This is one way of transmuting gross compact substances into aëreal ones. An- " other way is, by heat. For as fast as the motion of heat can shake off the par- " ticles of water from the surface of it, those particles, by the said principle, will " float up and down in the air, at a distance both from one another, and from the " particles of air, and make that substance we call vapour. Thus I suppose it is, " when the particles of a body are very small (as I suppose those of water are) so

" that the action of heat alone may be sufficient to shake them asunder. But if the " particles be much larger, they then require the greater force of dissolving men- " struums, to separate them, unless by any means the parties can be first broken " into smaller ones. For the most fixed bodies, even gold itself, some have said will " become volatile, only by breaking their parts smaller. Thus may the volatility " and fixedness of bodies depend on the different sizes of their parts.

" AND on the same difference of size may depend the more or less permanency of " aerial substances, in their state of rarefaction. To under- " stand this, let us suppose A B C D to be a large piece of " any metal, E F G H the limit of the interior uniform æther " and K a part of the metal at the superficies A B. If this " part of particle K be so little, that it reaches not to the " limit E F, it is plain, that the æther at its center must be " less rare, than if the particle were greater; for were it



" greater, its center would be further from the superficies A B, that is, in a place, " where the æther (by supposition) is rarer. The less the particle K therefore, the " denser the æther at its center, because its center comes nearer to the edge A B, " where the æther is denser than within the limit E F G H. And if the particle " were divided from the body, and removed to a distance from it, where the æther " is still denser, the æther within it must proportionally grow denser. If you con- " sider this, you may apprehend, how by diminishing the particle, the rarity of the " æther within it will be diminished, till between the density of the æther without, " and the density of the æther within it, there be little difference; that is, till the " cause be almost taken away, which should keep this and other such particles at a " distance from one another. For that cause explained in the fourth and fifth sup- " positions, was the excess of density of the external æther above that of the inter- " nal. This may be the reason then, why the small particles of vapours easily come " together, and are reduced back into water, unless the heat, which keeps them in " agitation, be so great as to dissipate them as fast as they come together: but the " grosser particles of exhalations raised by fermentation keep their aërial form more " obstinately, because the æther within them is rarer.

" NOR does the size only, but the density of the particles also, conduce to the " permanency of aërial substances. For the excess of density of the æther without " such particles above that of the æther within them is still greater. Which has " made me sometimes think, that the true permanent air may be of a metallic ori- " ginal; the particles of no substances being more dense than those of metals. This, " I think, is also favoured by experience, for I remember I once read in the Philo- " sophical Transactions, how M. *Huygens* at *Paris* found, that the air made by dis- " solving salt of tartar would in two or three days time condense and fall down again, " but the air made by dissolving a metal continued without condensing or relenting " in the least. If you consider then, how by the continual fermentations made in " the bowels of the earth there are aërial substances raised out of all kinds of bodies, " all which together make the atmosphere, and that of all these the metallic are the " most permanent, you will not, perhaps, think it absurd, that the most permanent " part of the atmosphere, which is the true air, should be constituted of these; espe- " cially since they are the heaviest of all other, and so must subside to the lower " parts of the atmosphere, and float upon the surface of the earth, and buoy up " the lighter exhalation and vapours to float in greatest plenty above them. Thus, " I say, it ought to be with the metallic exhalations raised in the bowels of the

" earth

" earth by the action of acid menstruums, and thus it is with the true permanent " air; for this as in reason it ought to be esteemed the most ponderous part of the " atmosphere, because the lowest, so it betrays its ponderosity, by making vapours " ascend readily in it, by sustaining mists and clouds of snow, and by buoying up " gross and ponderous smoke. The air also is the most gross unactive part of the " atmosphere, affording living things no nourishment, if deprived of the more " tender exhalations and spirits, that float in it: and what more unactive and remote " from nourishment than metallic bodies?

" I SHALL set down one conjecture more, which came into my mind now as I was " writing this letter. It is about the cause of gravity. For this end I will suppose " æther to consist of parts differing from one another in subtilty by indefinite degrees: " that in the pores of bodies there is less of the grosser æther, in proportion to the " finer, than in open spaces; and consequently, that in the great body of the earth " there is much less of the grosser æther, in proportion to the finer, than in the re- " gions of the air: and that yet the grosser æther in the air affects the upper regions " of the earth, and the finer æther in the earth the lower regions of the air, in such " a manner, that from the top of the air to the surface of the earth, and again from " the surface of the earth to the center thereof, the æther is insensibly finer and finer. " Imagine now any body suspended in the air, or lying on the earth: and the æther " being by the hypothesis grosser in the pores, which are in the upper parts of the " body, than in those which are in its lower parts, and that grosser æther being less " apt to be lodged in those pores, than the finer æther below, it will endeavour to " get out and give way to the finer æther below, which cannot be without the bodies " descending to make room above for it to go out into.

" FROM this supposed gradual subtilty of the parts of æther some things above " might be further illustrated, and made more intelligible; but by what has been " said, you will easily discern, whether in these conjectures there be any degree of " probability, which is all I aim at. For my own part, I have so little fancy to " things of this nature, that had not your encouragement moved me to it, I should " never, I think, have thus far set pen to paper about them. What is amiss there- " fore, I hope, you will the more easily pardon in

" Your most humble servant,

Cambridge, Feb. 28, 1678-9.

" and honourer,

" ISAAC NEWTON."

THIS letter of our incomparable *Newton* may perhaps receive some illustration from another [7], which he wrote a few years before to Mr. *Oldenburg*, and was as follows:

" SIR,

" I RECEIVED both yours, and thank you for your care in disposing those " things between me and Mr. *Linus*. I suppose his friends cannot blame you at " all for printing his first letter, it being written, I believe, for that end, and they

[7] In the possession of *William Jones*, Esq.

" never

" never complaining of the printing of that, but of the not printing that, which " followed, which I take myself to have been *per accidens* the occasion of, by re- " fusing to answer him. And though I think I may truly say, I was very little con- " cerned about it, yet I must look upon it as the result of your kindness to me, that " you was unwilling to print it without an answer.

" As to the paper of Observations, which you move in the name of the Society " to have printed, I cannot but return them my hearty thanks for the kind accep- " tance they meet with there, and know not how to deny any thing, which they desire " should be done. Only I think it will be best to suspend the printing of them for a " while, because I have some thoughts of writing such another set of Observations " for determining the manner of the productions of colours by the prism, which, " if done at all, ought to precede that now in your hands, and will do best to be " joined with it. But this I cannot do presently, by reason of some incumbrances " lately put upon me by some friends, and some other business of my own, which " at present almost take up my time and thoughts.

" THE additions, that I intended, I think I must, after putting you to so long " expectations, disappoint you in; for it puzzles me how to connect them with what " I sent you; and if I had those papers, yet I doubt the things I intended will not " come in so freely as I thought they might have done. I could send them described " without dependance on those papers; but I fear I have already troubled your So- " ciety and yourself too much with my scribbling, and so suppose it may do better " to defer them till another season. I have therefore at present only sent you two or " three alterations, though not of so great moment, that I need have staid you for " them; and they are these:

" WHERE I say, that *the frame of nature may be nothing but æther condensed by a fer-* " *mental principle*, instead of these words write, that it may be nothing but various " contextures of some certain ætherial spirits or vapours condensed, as it were, by " precipitation, much after the manner, that vapours are condensed into water, or " exhalations into grosser substances, though not so easily condensable; and after " condensation wrought into various forms, at first by the immediate hand of the " Creator, and ever since by the power of nature, who by virtue of the command, " *Increase and multiply*, became a complete imitator of the copies set her by the Pro- " toplast. Thus perhaps may all things be originated from æther, *&c.*

" A LITTLE after, when I say, the ætherial spirit may be *condensed in fermenting or* " *burning bodies, or otherwise inspissated in the pores of the earth to a tender matter,* " *which may be, as it were, the* succus nutritius *of the earth, or primary substance, out* " *of which things generable grow:* instead of this you may write, that that spirit may " be condensed in fermenting or burning bodies, or otherwise coagulated in the pores " of the earth and water into some kind of humid active matter, for the continual " uses of nature, adhering to the sides of those pores after the manner that vapours " condense on the sides of a vessel.

" IN the same paragraph there is, I think, a parenthesis, in which I mention " volatile salt-petre. Pray strike out that parenthesis, lest it should give offence to " somebody.

" ALSO where I relate the experiment of little papers made to move variously with " a glass rubbed, I would have all that struck out, which follows about trying the " experiment with leaf gold.

" SIR, I am interrupted by a visit, and so must in haste break off.

" Yours,

Jan. 25, 1675-6. " ISAAC NEWTON."

BUT to return to Mr. *Boyle*; in the year 1680, he gave the world the following tracts, *viz. The Aerial Noctiluca: or some new Phænomena, and a process of a factitious self-shining substance*; London, in 8vo. *A new Lamp*, printed in Mr. *Hooke's Philosophical Collections*, No. II. p. 33. and *Divers Experiments and Notes about the producibleness of chemical Principles*, subjoined to the second edition of his *Sceptical Chemist*, at *Oxford*, 1680, in 8vo.

THE Royal Society, of which he had been so long one of the greatest ornaments, now thought proper at their annual election on St. *Andrew's* day, November 30, this year, to choose him for their president. But after a mature consideration he excused himself from accepting that post, for reasons which shew his extreme tenderness and delicacy in all matters of conscience, and were represented by him in the following letter to Mr. *Hooke:*

SIR, *Pall-Mall*, Dec. 18, 1680.

" THOUGH since I last saw you, I met with a lawyer, who has been a member " of several parliaments, and found him of the same opinion with my council " in reference to the obligation to take the test and oaths you and I discoursed of; " yet not content with this, and hearing that an acquaintance of mine was come " to town, whose eminent skill in the law had made him a judge, if he himself had " not declined to be one, I desired his advice (which because he would not send me " till he had perused the Society's charter, I received not till late last night) and by " it I found, that he concurred in opinion with the two lawyers already mentioned, " and would not have me venture upon the supposition of my being unconcerned " in an act of parliament, to whose breach such heavy penalties are annexed. His " reasons I have not now time to tell you, but they are of such weight with me, " who have a great (and perhaps peculiar) tenderness in point of oaths, that I must " humbly beg the Royal Society to proceed to a new election, and do so easy a " thing, as among so many worthy persons, that compose that illustrious company, " to choose a president, that may be better qualified than I for so weighty an em- " ployment. You will oblige me also to assure them, that though I cannot now " receive the great honour they were pleased to design me, yet I have as much sense " of it, as if I actually enjoyed all the advantages belonging to it. And accord- " ingly, though I must not serve them in the honourable capacity they were pleased to " think, of for me, yet I hope, that God assisting, I shall not be an useless Member " of that learned Body, but shall manifest in that capacity both my zeal for their " work, and my sense of their favours. This you will oblige me to represent in such " a way, as may persuade the Virtuosi, that you will discourse with, how concerned " I am to retain the favourable opinion of persons, that have so great a share in his " esteem, who shall reckon your good offices on so important an occasion among the " welcomest favours you can ever do,

" SIR,

" your most affectionate friend and humble servant,

" ROBERT BOYLE."

" Superscribed,
" *These for my much respected friend Mr.*
" Robert Hooke, *professor of mathe-*
" *matics at* Gresham College."

ABOUT

ABOUT this time Dr. *Burnet* being employed in compiling his admirable *History of the Reformation*, Mr. *Boyle* contributed very largely to the expence of publishing it; which is acknowledged by the doctor in his *Preface* to the *second volume*, in these words: "The noble Mr. *Boyle*, as he employs both his time and wealth for the "good of mankind, (for which he considers himself as chiefly born, and which he "has promoted not only in his own excellent writings, that have made him so famous over all the world, but in many other designs that have been chiefly carried "on at his cost) so hath he renewed his kindness to me in largesses suitable to so "great a mind."

IN 1681 Mr. *Boyle* published his *Discourse of things above Reason; inquiring, whether a Philosopher should admit there are any such? To which are annexed by the Publisher, for the Affinity of the Subject, some Advices about judging of things said to transcend Reason:* London, in 8vo. as he did the year following his *New Experiments and Observations made upon the icy Noctiluca: to which is added, A chemical Paradox grounded upon new Experiments, making it probable, that chemical Principles are transmutable; so that out of one of them others may be produced:* London, in 8vo. and *A Continuation of new Experiments physico-mechanical touching the Spring and Weight of the Air and their Effects. The second part; wherein are contained divers Experiments made both in compressed and also in factitious Air, about Fire, Animals, &c. Together with a Description of the Engines, wherein they were made:* London, in 8vo.

IT was probably in the beginning of the year 1681, that he wrote a letter to Mr. *John Eliot* of *New England*, who was one of the first who preached the gospel among the Indians of that part of *America*, having learned their language to great perfection, and translated into it the Bible [a] and several books of piety. Mr. *Boyle*'s letter seems to be in answer to one from Mr. *Eliot*, dated Nov. 4, 1680 [b]; and as it is a strong evidence of his zeal against persecution on account of religious opinions, it deserves a place here.

"SIR,

"I AM very glad to find, by the favour of your very kind letter, that God is "pleased to continue you still an active and an useful instrument in the propagation of the Gospel of his Son among the poor Indians, whose having been so true "to Christianity, and serviceable to the English interest, may well prove matter of "rejoicing both to you and us. That little, which I have contributed to their good, "deserves not so advantageous a mention, as your letter makes of it; and duties "of that kind have such recompences apportioned to them by God, that the performers need not seek them from the acknowledgments of men. I am obliged to "you for what I perceive you have done for Mr. *Daniel*, whom I look upon both as "an ingenious man, and a friend to *New England*, as well by his good offices here, "as by the moderation, to which he endeavours to persuade his friends there, which "last clause I the rather employ, because of late I have, to my trouble, heard the "government of the Massachusets sharply censured for their great severity to some "dissenters, who, contrary to order, had convened at a meeting-house to worship "God. This severe proceeding seems to be the more strange, and the less defensible in those who having left their native country, and crossed the vast ocean to

[a] It was first printed in *Cambridge* in *New England* in 1663, in 4to.
[b] Inserted in the *Appendix* to this *Life*.

"settle

" ſettle in a wilderneſs, that they may there enjoy the liberty of worſhipping God " according to their own conſcience, ſeem to be more engaged than other men to " allow their brethren a ſhare in what they thought was ſo much all good men's due. " And, indeed, though perſecution for innocent, though perhaps erroneous opinions, " taken up for conſcience ſake, were not unſuitable to the equity and gentleneſs of " the Goſpel; yet many of your friends here think it would be a very improper " courſe to be taken by you at this time, and fear, that if your rigorous proceedings " againſt diſſenters ſhould be talked of here (as if you quickly forbear them not, " they will be) it would open men's mouths againſt your government, and furniſh " your enemies with objections, that your friends would not be able to anſwer; and " beſides may be of very bad conſequence to that ſort of men here, who do moſt " ſymbolize with you in point of opinion and worſhip. You will eaſily believe, that " I, who am never like to viſit your colony, have no private ends of my own in " what I have now written; and therefore I hope you will take it, as it is meant, for " a friendly (and perhaps not unſeaſonable) admonition, the deſpiſing of which may " probably be more prejudicial to your colony, than many among you ſeem to be " aware of. Our worthy friend, alderman *Aſhurſt* (though now, thanks be to God, " in a more hopeful condition) was on Wedneſday laſt ſo ill, that the corporation " could not meet at his houſe; and the preſence of that good man was much miſſed " amongſt us, and particularly in reference to your deſire of having the Old Teſta- " ment reprinted in the Indian language. In his abſence I read to the company that " part of your letter to me, which concerns that affair, and the buſineſs was diſ- " courſed of among us; but in regard we have had no letters from the commiſſioners " about it, and that the court thought they might hear further before the New " Teſtament and Pſalms would be printed off; they did not think fit to determine " any more about that buſineſs, till they ſhould have a particular account of the " progreſs and expence of the work already begun; by which account they expect " to be aſſiſted to take further meaſures. This I acquaint you with by order of the " company, divers of whoſe members on this occaſion manifeſted the kindneſs and " eſteem, which they had for Mr. *Eliot*. I briefly acquainted them alſo with what " you wrote to me about the * * * people, whoſe language is almoſt the ſame with " that of the Maſſachuſets. If any further diſcoveries be made of them, you will " oblige me to impart it to him, that in that and all your other endeavours to enlarge " the kingdom of Chriſt, wiſhes you moſt proſperous ſucceſs, and is unfeignedly,

" SIR,

" your moſt affectionate and humble ſervant."

IN 1683 he wrote *A Letter to the learned Dr.* John Beale, *F. R. S. concerning freſh Water made out of Sea-Water, printed at the deſire of the Patentees*, in a tract, intitled, *Salt Water ſweetened; or a true account of the great advantages of this new invention both by ſea and land: together with a full and ſatisfactory anſwer to all apparent difficulties. Alſo the approbation of the College of Phyſicians:* London, 1683, in 8vo. This piece was written by *R. Fitz-Gerald*, Eſq; who in his dedication to the King obſerves, " the ex- " periment was in a great degree owing to the eminent Mr. *Boyle*; and indeed well " worthy ſo ingenious a promoter, being ſo much more the favourite of his happy " genius, as it is univerſally uſeful to mankind."

THE year following produced his *Memoirs of the natural History of human Blood, especially the Spirit of that Liquor; with an Appendix:* printed at *London*, in 8vo, and translated into Latin; and his *Experiments and Considerations about the Porosity of Bodies, in two Essays:* printed likewise at *London*, in 8vo. The first essay treats of the porousness of *animal bodies*, and the second of that of *solar bodies*. A Latin translation of this work was also published there in 8vo, the same year, under the title *Tentamen Porologicum*.

HIS writings grew now so numerous, that Dr. *Ralph Cudworth*, the excellent author of *the true intellectual System of the Universe*, recommended to him in a letter dated October 16, 1684, the procuring of all his works to be translated into Latin, and published together. "Then, *adds he*, what you shall superadd, will be easily col-" lected and added afterwards. And I pray God continue your life and health, that " you may still inrich the world with more. The writers of hypotheses in natural " philosophy will be confuting one another a long time, before the world will ever " agree, if ever it do. But your pieces of natural history are unconfutable, and will " afford the best grounds to build hypotheses upon. You have much outdone Sir " *Francis Bacon* in your natural experiments; and you have not insinuated any thing, " as he is thought to have done, tending to irreligion, but the contrary."

IN 1685, he gave the world his *Short Memoirs for the natural experimental History of mineral Waters, addressed by way of Letter to a Friend:* printed at *London*, in 8vo. In this book he gives us heads for the natural history of those waters, with a great many curious experiments. With regard to the methods of trying them, he gives the following directions: 1. To observe the changes of the colours made by tinctures in a good light, in order to distinguish what mineral tinctures they are impregnated with. 2. To vary the shades of colours made by mineral waters, either by dropping such waters upon paper, whose pores are saturated with powder of vitriol, or tinged with a decoction of logwood. Several variations also of colours may also be made by dropping either medicinal liquors into mineral waters, or an infusion of galls either before or after. 3. He recommends for such experiments the astringent parts of plants or animals, or especially mineral substances.

THE next work, which he published that year, was, *An Essay of the great Effects of even languid and unheeded motion: whereunto is annexed, An experimental Discourse of some little observed Causes of the Insalubrity and Salubrity of the Air and its Effects: London*, in 8vo, and reprinted there in the same form in 1690. In this *essay* he proves, by several instances, 1. The great efficacy of celerity in bodies very small, especially when the space which they move through is but very small, as in lightning. 2. That the insensible motion of so soft bodies as fluids may have a sensible effect upon solid bodies, as in sounds, when they shake the windows in houses, *&c.* at a considerable distance. 3. That the number of the insensible parts of matter put into motion, enable them to perform several things. 4. That local motion may be propagated through several mediums, and even solid bodies. 5. That the effects of particular modifications of the invisible motions of fluids on animal bodies to be disposed to be worked upon by them, are very considerable. Thus a particular note of a musical instrument, hath a peculiar effect upon particular animals. 6. That the effects of fluids upon inanimate bodies, by a particular texture and modification of the agent and the patient, are also considerable. 7. That some bodies are thought to have their parts absolutely at rest, when they are only in a forced state, as of tension, compression, *&c.* 8. That one principal reason why such motions, as we speak of, are overlooked, is, that we scarce ever take notice but of those motions of solid bodies, where-

in

in one whole body drives away another, or at least visibly hits against it; whilst many effects proceed from the intestine motions produced by the external agent in and among the parts of the same body.

He published also, the same year, *An historical Account of a strangely self-moving Liquor*; printed in the *Philosophical Transactions*, No. CLXXVI. p. 1188; and a discourse, intitled, *Of the Reconcileableness of specific Medicines to the corpuscular Philosophy: to which is annexed, A Discourse about the Advantages of the Use of simple Medicines*: printed at *London*, in 8vo, and translated into Latin, in which language it was published there the year following in 8vo. In this book he observes, that there are three sorts of qualities mentioned in the writings of physicians, under the notion of specific virtues; for by some of them a medicine is said to have a specific quality, because it is eminently and peculiarly beneficial to a particular part of the body, as the heart, the brain, the eyes, *&c.* Others say, that a specific remedy attracts and evacuates some determinate humour, as choler, phlegm, *&c.* But the most usual account upon which a medicine is said to be specific, is, that by some hidden property it cures this or that particular disease, as a pleurisy, an asthma, the cholic, a dropsy, *&c.* This being the common sense in which the word is employed, Mr. *Boyle* makes use of it in that sense. He then urges several arguments for the existence of specific remedies; and enquires, whether the notion of them may be accommodated to the mechanical philosophy; and lays down the six following propositions: 1. A specific medicine may cure by discussion, or resolving the morbific matter, and thereby fitting it for expulsion through the greater outlets of the body and cuticular pores. 2. It may subdue the too great proportion of acid or other luxuriant particles in the mass of blood, and prevent their coagulating or other pernicious effects. 3. It may prove serviceable by precipitating the peccant matter out of the blood or other fluid of the body. 4. It may peculiarly strengthen and cherish the heart, and by that means, or otherwise, relieve the part affected. 5. It may have its effect, by producing in the mass of blood a disposition to enable nature, by correcting, expelling, *&c.* to surmount the cause of the disease. 6. It may unite its particles with those of the peccant matter, and thereby constitute some different substance to be easily thrown off, or safely continued in the body. He then encourages the use of *simple medicines*, from the advantage of foreseeing their effects, and their being more safe and commodious for exhibition, easily procurable, and likely to improve the knowledge of the *Materia Medica*. He observes likewise, that chemical remedies should be simple; and enquires, whether simple remedies may cure complex distempers.

Besides these philosophical tracts, he gave the world likewise, the same year, an excellent theological one, *Of the high Veneration Man's Intellect owes to God, peculiarly for his Wisdom and Power*: printed at *London*, in 8vo. and translated likewise into Latin. In the advertisement prefixed to this tract, we are informed, that "the abrupt "beginning of it will not be wondered at, when it is declared, that the whole excur- "sion is to be looked upon as a fragment of a discourse, from which, for certain rea- "sons, it has been separated in its present form; in which it ought to pass for a "rough draught; the nobleness, sublimity and sacredness of the subject not allow- "ing the author to presume, that the first thoughts he committed to paper about it, "might be for good and all parted with by him, till he should have heedfully revised "and corrected them, and left them as few faults, as the disproportion of so vast and "sublime a subject to his slender abilities would permit."

In 1685-6, he published his *Free Enquiry into the vulgarly received Notion of Nature; made in an Essay addressed to a Friend*: printed at *London*, in 8vo, and translated

into Latin, and printed there in 1687 in 12mo. In this discourse he observes, that this notion is prejudicial to religion and philosophy; and he advances a new notion of it, distinguishing between the *universal* and the *particular* nature of things. With regard to *universal nature*, he tells us, that nature is the aggregate of the bodies, which make up the world in its present state, considered as a principle, by virtue whereof they act and suffer, according to the laws of motion prescribed by the author of things. And that this makes way for the other subordinate notion, since the *particular nature* of an individual consists in the *general nature* applied to a distinct portion of the universe; or supposing that place, as it is, in a world framed by God, like ours, it must be a convention of the mechanical properties (such as magnitude, figure, order, situation and local motion) of parts convenient and sufficient to constitute or intitle to its particular species or denomination the particular body which they make up; the concourse of all these being considered as the principle of motion, rest, and changes in the body.

IN June the same year, Dr. *Burnet*, afterwards bishop of *Salisbury*, transmitted to him from the *Hague* the manuscript account of his travels [a], which he had drawn up in the form of letters addressed to Mr. *Boyle*; who in his answer to the doctor, dated the 14th of that month, observes, that he was glad to find, that "all men do not "travel, as most do, to observe buildings, and gardens, and modes, and other amuse-"ments of a superficial and almost insignificant curiosity; for your judicious remarks "and reflections, *says he*, may not a little improve both a statesman, a critic, and a "divine, as well as they will make the writer pass for all three."

THE next work of our great author which saw the light, was, *The martyrdom of Theodora and Didymus*, drawn up in his youth, and printed at *London*, 1687, in 8vo; and the year following he published *Receipts sent to a Friend in America: London*, in 12mo; which edition contains only the first five decads of *choice remedies*, and hath a preface before it, which was omitted in the second edition in 1692, containing the second five decads. He published likewise, in 1688, *A Disquisition about the final Causes of natural things; wherein it is enquired whether, and (if at all) with what caution a Naturalist should admit them. To which are subjoined, by way of Appendix, some uncommon Observations about vitiated Sight: London*, in 8vo. In this *Disquisition* he examines, whether these final causes are knowable by men; and observes, that they signify, 1. Some grand and general ends of the universe, such as exercising and displaying the Creator's wisdom, the communication of his goodness, and the admiration and thanks due to him from his intelligent creatures. 2. In a more restrained sense, the ends designed in the number, fabric, situation, and motion of great masses of matter which make large parts of the world; since it is very probable that these bodies, such as the sun, moon, fixed stars, and the terraqueous globe, were so framed and placed, as not only to persevere in their own present state, but also to conduce to the universal ends of the creation, and the good of the whole, whereof they are considerable parts. Upon which accounts these ends may be called *cosmical* or *systematical*, as they regard the symmetry of the great system of the world. 3. Ends, which more peculiarly concern the parts of animals, and perhaps plants too; or those to which the peculiar parts of animals are destined for the welfare of the whole creature, considered as an intire and distinct system of organized parts, designed to preserve himself, and propagate his species upon that stage on which his structure and circumstances determine him to act his part; which ends Mr. *Boyle* calls *animal ends*. 4. The same ex-

[a] See Vol. VI. p. 625, 626.

pression

preſſion may ſignify another ſort of ends, which, becauſe they relate particularly to man, may be called *human ends*, and are thoſe aimed at by nature, where ſhe is ſaid to frame animals, vegetables, *&c.* for the uſe of man. And theſe ends may be further diſtinguiſhed into *mental* and *corporeal*, not only as man is an animal, framed, like others, for his own preſervation, and the propagation of his ſpecies; but alſo as he is made to have dominion over other animals and works of nature, and fitted to make them ſubſervient to his purpoſes. This diſtinction being thus ſettled, Mr. *Boyle* declares his diſſent, as well from the vulgar notion of final cauſes, which allows of none but thoſe, which are above called human ones, as from theirs, who wholly reject them all. He obſerves, that chance is an imaginary being; that revelation allows us to ſpeak more poſitively of final cauſes than natural philoſophy; that it is often allowable, from the manifeſt and appoſite uſes of the parts of animal bodies to collect ſome of the particular ends for which the Creator deſigned them; and in ſome caſes, from the known nature and ſtructure of the parts, to draw probable conjectures about the particular offices of them; and that it is rational from the manifeſt fitneſs of ſome things to *coſmical* or *animal ends* to infer, that they were ordained to them by an intelligent agent; that we ought not to be haſty in concluding upon the particular uſe of a thing, or the motive which induced the author of nature to frame it in a peculiar manner; and that the naturaliſt ſhould not ſuffer the ſearch or diſcovery of final cauſes to make him undervalue or neglect the enquiry after their efficients. In the *Obſervations concerning vitiated Sight* he obſerves, that white objects are moſt viſible to a dim ſight; that both eyes are concerned in real viſion; that the parts of the eye are capable of great dilatation without prejudice; and that colours appear different to diſordered organs.

HE publiſhed likewiſe in May the ſame year 1688, at *London*, in two pages in folio, *An advertiſement about the loſs of many of his writings, addreſſed to* J. W. *to be communicated to thoſe of his friends that are virtuoſi; which may ſerve as a kind of preface to moſt of his mutilated and unfiniſhed writings.* And to the ſame purpoſe I find the following papers of his, which ſhew in what manner he had been treated by Plagiaries.

" IN the month of May 1688, I thought myſelf obliged to give notice to the pub-
" lic, that I had, partly by ſome men's fraud, and partly by miſchance, loſt ſo many
" of my eſſays and other tracts, and had ſo many of my remaining papers enda-
" maged by corroſive liquors, that the curious were not thenceforth to expect from
" me any thing but imperfect and mutilated. And yet ſince that time all my care
" and circumſpection has not hindered me from loſing ſix centuries of matters of fact
" in one parcel, beſides ſo many other papers of leſſer bulk, that I am reduced to
" remind the curious of my former advertiſement; not for any pleaſure I take in
" complaining, or troubling others with my misfortunes, but to render a reaſon of
" the courſe I am by theſe miſadventures driven to, though otherwiſe I ſhould not
" think it eligible. This is to ſecure the remaining part of my writings, eſpecially
" thoſe that contain moſt matters of fact, by ſending them maimed and unfiniſhed, as
" they come to hand, to the preſs."

" BEING wont, when I firſt turned a writer, to ſet down, as well as others uſe to
" do, my thoughts and obſervations on papers bound up into books, I was quite diſ-
" couraged from that practice, by the loſſes I made at ſeveral times of manuſcripts,
" which I ſtrongly ſuſpected to have been ſurreptitiouſly conveyed away by ſome, that
" though they expected to find valuabler things than I ſuppoſed they met with, would

" not probably have ſtolen away thoſe papers, if their bulk had not been a main " temptation to the theft.

" WHEREFORE I afterwards reſolved to write in ſingle ſheets, and other looſe " papers, that the ignorance of the coherence might keep men from thinking them " worth ſtealing. And though I could not, by ſo doing, prevent the loſing ſome- " times a paper or two by chance, or other men's fraud, yet I thought ſuch incon- " veniencies (which I could ſometimes eaſily repair out of my memory) much in- " ferior to thoſe of loſing an entire diſcourſe, or a whole diſcourſe at a time. Now " that it may ſeem the leſs ſtrange, that the particulars in the following manuſcript " appear to be huddled together, without any either method or connection, I am to " advertiſe, that when I had ſettled the number and titles of the ſubjects to be treated " of in theſe papers, I found myſelf both invaded with a ſickneſs threatening enough, " and likely to be hindered by other diſtracting avocations, from ſetting down the " particulars belonging to the enſuing treatiſe, in ſuch an uninterrupted ſeries as I " had propoſed to myſelf; which made me reſolve, rather than venture the loſs of " my obſervations, to ſet down under each title or ſection thoſe that did more par- " ticularly belong unto it, without any other order than that wherein I lighted on " them, contenting myſelf to leave competent blanks or intervals between the diſ- " tinct obſervations, notions, *&c.* which courſe I pitch upon, partly, that if God " ſhould be pleaſed to ſpare me life and leiſure, I might be able to fill them up in " the requiſite connexions, tranſitions, *&c.* partly that I might preſerve the materials, " ſome of which do not perhaps deſerve to periſh, but gave me ſome hopes, that " though I myſelf ſhould not complete with them the fabric I chiefly deſign them " for, yet perhaps a ſkilful builder may think fit to perform it; and that however " they may at laſt prove uſeful for other purpoſes.

" THAT the author may not be diſcouraged from continuing to impart clearly and " freely his experiments and notions to the commonwealth of letters, I think it ne- " ceſſary to advertiſe equitable readers, that he has been the worſe dealt with by " ſeveral writers, upon the very account of that candour and faithfulneſs he has ex- " erciſed in delivering matters of fact. For whereas when experiments and obſer- " vations are related by men whoſe faithfulneſs is dubious, the more cautious ſort " of plagiaries think themſelves obliged to mention the names of their authors, leſt " an experiment not proving true, its falſity ſhould be (as it juſtly may be) imputed to " them, they think they may ſo ſafely rely on the truth of what our author relates, " that their reputation runs no venture in making any experiment that he delivers, " paſs for their own.

" THIS hath emboldened ſeveral writers, both formerly and of late, to uſurp from " him a great many things whereof they were not the authors; ſometimes tranſcribing " this or that particular out of his book into theirs, and ſometimes transferring whole " ſets of experiments, if not reaſonings too, perhaps ſomewhat abridged or otherwiſe " diſguiſed. And this hath been done by ſome of them, without ſo much as naming " the true author, and ſometimes naming him indeed as it were incidentally, and per- " adventure reflectingly, for ſome inconſiderable part of what they took from him. " Of theſe ſeveral ſorts of plagiaries, it would not be difficult to give particular in- " ſtances: it will be done, if it be thought fit and deſired.

" IT is not unknown to a great part of the curious, that our author hath given emi- " nent proofs of his readineſs to acknowledge thoſe whoſe writings he tranſcribes into " his,

" his, insomuch, that some new experiments, that were never in the press till he sent " them thither, and so might easily have been adopted by him, were declared by him " not to be his, though he yet knew not whose they were. It will therefore become " ingenious readers, who would not discourage his candor and communicativeness, to " think, when they meet with the same experiment in his writings and in another's, " that a person that has been so conversant with nature, and uses a comprehensive " method in examining the subjects he solemnly treats of, may be at least as likely, " as most others have been, the author of what he delivers upon his own knowledge. " And particularly it is desired and hoped, that the equitable reader will so far con- " cern himself in the reputation of a person, who hazarded it only to serve the pub- " lic, that he will take notice of the time when our author's books came abroad, and " that wherein the first edition of the books, containing the same or the like things " with his, was published, it having been the subtile practice of several plagiaries, as " well as *Claudius Berigardus*, to assign to the second or other subsequent editions of " their books the same date with the first impression, and stealing into these after- " editions many passages, both of other authors and divers other experimental " writers, and particularly divers members of the Royal Society, as if they were as " ancient as the first edition of their books, wherein a due collation will discover them " not to be extant.

" THIS will, it is hoped, appear but a reasonable request, since the thing desired is " necessary to keep a man, that has long faithfully served the commonwealth of " learning, from being thought to have taken from others such things as others have " really taken from him.

" THOUGH many learned and ingenious foreign writers, especially in *Germany* and " *Italy*, have thought fit, when they made use of our author's experiments and obser- " vations, to do him justice, and sometimes even obligingly; yet divers others have " been far from imitating their laudable example. For they have made use, not only " of an experiment or two of his here and there, but some of them of whole sets of " experiments; and others have made bold with the historical part of whole chapters " out of his writings, without taking notice of them, or perhaps so much as naming " him. This way of proceeding offended many, and particularly the late learned se- " cretary of the Royal Society, Mr. *H. O.* who publicly complained of it in general " terms, and shewed the prejudice such plagiaries must naturally do to the common- " wealth of learning, especially by discouraging the industry of those many that do " not undervalue fame, and by encouraging lazineſs in those numerous pretenders " to the new philosophy, who will never take much pains to promote experimental " knowledge, whilst they find it far easier to usurp experiments than to make them, " and think they may securely, by turning plagiaries, pass for philosophers. But " though our author's strong inclination to serve the public, and gratify the curious, " has hitherto enabled him to surmount the abovementioned discouragement; yet " those virtuosi, that are earnestly desirous, that the more historical parts of his remain- " ing writings may not be exposed to the accidents by which so many of their com- " panions have been lost already, nor the curious kept from things that may be of " use or of delight to them, so long as till they may be published with less danger " of being usurped by foreigners; they have undertaken, for prevention of this dan- " ger, both to hasten the translation of the writings, that will, God granting him life " and health, from time to time come abroad; and whether he be alive or not, to " have now and then a list drawn up and printed, wherein the particular experiments

" that

" that may have been taken from Mr. *Boyle*'s writings without owning him for them, " will be mentioned, though without any severe reflections upon the writers. This " course is on this occasion the rather thought fit, not only that it may take off a dis- " couragement which might have too much influence upon another than Mr. *B.* but " because in a time where so many false, obscure, or imperfectly delivered experiments, " especially chemical ones, fly abroad, it cannot be indifferent to a wary reader, from " what hand he receives a matter of fact which one writer may deliver, not only " much more faithfully and perspicuously, but (what in some sorts of nice experi- " ments is very considerable) with greater skill than another."

THE decay of Mr. *Boyle*'s health began now to interrupt his communications to the Royal Society, as he observes himself in a letter to monsieur *Le Clerc*, dated May 30, 1689 [b]; and this, added to the ill situation of his affairs in *Ireland* upon the revolution, obliged him to resign his post of governor of the corporation for propagating the Gospel in *New England*, &c. which he did by the following letter to that corporation.

" Honoured Gentlemen, Aug. 22, 1689, *Pall-Mall.*

" AS it hath not been without great satisfaction, that I have for many years served " such worthy persons as I now write to, in promoting so excellent a work as " the propagation of the Gospel among savages, that were utter strangers to it, and " worshipped the grand enemy to it; so it is not without much trouble that I am " now obliged to resign so honourable, and to me grateful, an employment. And, " indeed, it is none of the least uneasy circumstances of the almost continual sick- " liness, that out of his justice, and, I hope, his goodness too, God has been pleased " to exercise me with of late years, that my infirmities have disabled me to perform " the conditions that the late laws require of those that would continue in that pub- " lick station, wherein I counted it a great honour, and such as deserved my most " humble thanks to God for it, that I was preferred to assist you so long in car- " rying on your pious and charitable work. But on the other side, it may very pro- " bably turn to the advantage of the honourable corporation, who, instead of a per- " son that wants his health, and is for the present deprived of all his estate in *Ireland*, " may make choice of a governor, vigorous, active, versed in letting, setting, and " other œconomical affairs, who, especially if he be a single man, may further your " pious endeavours, and contribute to the welfare of your society, not only by his " counsel and direction, but with his purse. I hope the necessity of changing an in- " strument, will not discourage you from chearfully prosecuting a design which you " may justly expect that God, whose glory it aims at, will bless and graciously both " prosper and reward. For my part, I have found among you so particular a re- " gard, and so much kindness to me, during the long continuance of my relation " to you, that gratitude obliges me, as well for your own sake as for that of the " work you are engaged in, to wish and pray that you may be directed in your " choice, and your next governor may be as faithful and affectionate as your last, " but much more capable and prosperous. And though I must cease to serve you " in my former station, yet you will not find me more backward than formerly to " serve you faithfully in my reduced capacity; and I hope you will do me the right

[b] Vol. VI. p. 61.

" to

" to believe, that to ſee the great and good work, you are purſuing, proſper in your " charitable hands, though I can have the honour to contribute but my good " wiſhes to it, will heartily rejoice,

" Honoured Gentlemen,

" Your moſt faithful and

" moſt humble ſervant."

IT was probably about this time, that he thought proper to publiſh the following advertiſement, in order to decline viſits on certain days, that he might have leiſure to finiſh ſome of his works, then lying by him.

" MR. *Boyle* finds himſelf obliged to intimate to thoſe of his friends and acquaint- " ance, that are wont to do him the honour and favour of viſiting him, 1. That he " has by ſome unlucky accidents (whereof he has given notice to the public) had " many of his writings corroded here and there, or otherwiſe ſo maimed, that with- " out he himſelf fill up the *lacunæ* out of his memory or invention, they will not be " intelligible. 2. That his age and ſicklineſs have for a good while admoniſhed him " to put his ſcattered, and partly defaced, writings into ſome kind of order, that " they may not remain quite uſeleſs. And, 3. that his ſkilful and friendly phyſi- " cian[2], ſeconded by Mr. *Boyle*'s beſt friends, has preſſingly adviſed him againſt " ſpeaking daily with ſo many perſons, as are wont to viſit him, repreſenting it as " that which cannot but much waſte his ſpirits, and by obliging him to ſit a great " deal too much for a perſon ſubject to the ſtone of the kidneys, and on ſeveral " other accounts impair his health, and diſable him for holding out long. And he " is alſo obliged further to intimate, that by theſe and other inducements he does at " length, though unwillingly, find himſelf reduced to deny himſelf part of the ſa- " tisfaction frequently brought him by the converſation of his friends and other " ingenious perſons, and to deſire to be excuſed from receiving viſits (unleſs upon " occaſions very extraordinary) two days in the week, namely on the forenoon of " Tueſdays and Fridays (both foreign poſt days) and on Wedneſdays and Saturdays " in the afternoon, that he may have ſome time, both to recruit his ſpirits, to range " his papers, and fill up the *lacunæ* of them, and to take ſome care of his affairs in " *Ireland*, which are very much diſordered, and have their face often changed by " the public calamities there."

HE ordered likewiſe a board to be placed over his door, with an inſcription ſignifying when he did and did not receive viſits.

AMONG the works, which he finiſhed during theſe intervals of retirement, was probably a collection of elaborate proceſſes in chemiſtry, which he ſent to a friend, with the following letter:

[2] Sir *Edmund King*.

 " SIR,

" SIR,

" I CONFESS you are not the only person among my friends, to whom it hath " seemed somewhat strange, that I, who have spent many of my thoughts, some " of my money, and, what I value far more, of my time too, upon chemistry, as " well as divers other parts of learning, have not been taken notice of to have found " any *particulars*, as chemists speak, or other lucriferous experiments upon metals " and minerals, nor have pretended to be possessor of those difficult and compounded " experiments, that are magnified by chemists as excellent *Hermetic Arcana*.

" BUT, Sir, since I find you in the list of those that have made the newly-men- " tioned reflection, I am content to give you such a summary account of my com- " portment, as may at least lessen your wonder at it. I must inform you then, that " when, among other studies, I applied myself to the cultivating of natural philoso- " phy, I soon perceived, that some insight into chemical operations was, though not " absolutely necessary, yet highly conducive to the true knowledge of nature, and " especially to the indagation of several of her most abstruse mysteries. On this score " I was induced to make a nearer inspection into chemistry than virtuosi are wont to " think it worth while to do; and I did not repent me of my labour. But as I " cultivated chemistry, not so much for itself, as for the sake of natural philosophy, " and in order to it, so most of the experiments I devised and pursued, were gene- " rally such as tended not to multiply processes, or gain the reputation of having " store of difficult and elaborate ones; but to serve for foundations, and other useful " materials for an experimental history of nature, on which a solid theory may in " process of time be superstructed. For this purpose I judged, that plain and easy " experiments, and as simple, or as little compounded as may be, would, *cæteris* " *paribus*, be the fittest, as being the most easy to be tried (and, if need be, repeat- " ed) and to be judged of, both in relation to their causes, and to their effects. And " for these reasons, though I had by me a not inconsiderable number of more com- " pounded and elaborate processes, some of which I had made, and others I received " as great secrets from noted artists; I purposely forbore to mention any number of " them in my writings about physics, being desirous rather to increase knowledge, " than make any ostentation of any that I thought would puzzle most readers more " than it would instruct them.

" THIS, Sir, I hope, will appear to you a fair account of your not finding my " physical discourses larded with long and intricate processes, some of which may, " I willingly grant, produce notable effects, and for that reason are valuable, but " are less fit than far more simple ones to discover the causes of things, which yet " is the chief scope of a naturalist, as such. And to those that think it strange, " that among my other experiments about metals and minerals, I have not produced " those gainful ones, that chemists call *particulars*, it may, I hope, suffice to repre- " sent, that being a bachelor, and through God's bounty furnished with a competent " estate for a younger brother, and freed from any ambition to leave my heirs rich, " I had no need to pursue lucriferous experiments, to which I so much preferred luci- " ferous ones, that I had a kind of ambition (which I now perceive to have been a " vanity) of being able to say, that I cultivated chemistry with a disinterested mind, " neither seeking nor scarce caring for any other advantages by it, than those of the " improvement of my own knowledge of nature, the gratifying the curious and " industrious, and the acquist of some useful helps to make good and uncommon " medicines.

" I

" If I may be allowed to judge of courses by the success, the entertainment, that " the public has been pleased to give my endeavours to serve it, will not make me " repent of the way I have made choice of to do it in. But, however, since I find " myself now grown old, I think it time to comply with my former intentions to " leave a kind of Hermetic legacy to the studious disciples of that art, and to deliver " candidly, in the annexed paper, some processes chemical and medicinal, that are " less simple and plain than those barely luciferous ones I have been wont to affect, " and of a more difficult and elaborate kind than those I have hitherto published, " and more of kin to the noblest Hermetic secrets, or, as *Helmont* stiles them, *arcana* " *majora*. Some of these I have made and tried; others I have (though not with- " out much difficulty) obtained, by exchange or otherwise, from those that affirm " they knew them to be real, and were themselves competent judges, as being some " of them disciples of true adepts, or otherwise admitted to their acquaintance and " conversation. Most of these processes are clearly enough delivered; and of the " rest there is plainly set down, without deceitful terms, as much as may serve to " make what is literally taught to be of great utility, though the full and complete " uses are not mentioned, partly because, in spite of my philanthropy, I was engaged " to secrecy, as to some of these uses, and partly because I must ingenuously confess " it, I am not yet, or perhaps ever shall be acquainted with them myself. The " knowledge I have of your great affection for the public good, and your particular " kindness for me, invites me, among the many virtuosi, in whose friendship I am " happy, to intrust the following papers in your hands, earnestly desiring you to im- " part them to the public faithfully, and without envy, *verbatim*, in my own expres- " sions, as a monument of my good affections to mankind, as well in my chemical " capacity, as in the others, wherein I have been solicitous to do it service. I am, " with sincere respect,

" SIR,

" Your most faithful and most humble servant,

" ROBERT BOYLE."

THIS collection of chemical processes is not now to be found among his manuscripts, though there are still extant among them a great number relating to chemistry; and it appears, that he left orders, that after his death all his papers upon that subject should be examined by three physicians nominated by him for that purpose[a]; being unwilling that they should be lost to the public. And indeed it is highly reasonable to suppose, that many important discoveries were contained in them, chemistry being his favourite study, and opening to him perpetually such a new scene of wonders, as easily persuaded him of the possibility of the transmutation of metals into gold. This persuasion of his is evident from several parts of his writings, and was avowed by himself to the great Dr. *Halley*, the late royal astronomer, who related to me his conversation with him upon that subject; and it was probably in consequence of this opinion, that Mr. *Boyle* procured by his interest the following *act*

[a] Letter of Mr. *John Warr* senior to his son Mr. *John Warr*, executor to Mr. *Boyle*, dated July 16, 1692.

to be passed in August 1689 *for the repeal of a statute made in the fifth year of King* Henry IV. *against the multiplying of gold and silver* [b].

" WHEREAS by a statute made and enacted in the parliament held in the fifth year " of the reign of King *Henry* the fourth, late King of *England*, it was, amongst " other things, enacted in these words, or to this effect, namely, *that none from* " *thenceforth should use to multiply gold or silver, or use the craft of multiplication; and* " *if any the same do, they should incur the pain of felony:* And whereas since the mak- " ing of the said statute divers persons have, by their study, industry, and learning, " arrived to great skill and perfection in the art of melting and refining of metals, " and otherwise improving them and their ores (which very much abound within this " realm) and extracting gold and silver out of the same; but dare not exercise their " said skill within this realm, for fear of falling under the penalty of the said sta- " tute, but exercise the said art in foreign parts, to the great loss and detriment of this " realm:

" BE it therefore enacted by the king's and queen's most excellent majesties, by " and with the advice and consent of the lords spiritual and temporal, and commons, " in this present parliament assembled, that from henceforth the aforesaid branch, " article, or sentence, contained in the said act, and every word, matter, and thing, " contained in the said branch or sentence, shall be repealed, annulled, revoked, and " for ever made void; any thing in the said act to the contrary in any wise whatsoever " notwithstanding.

" PROVIDED always, and be it enacted by the authority aforesaid, that all the gold " and silver, that shall be extracted by the aforesaid art of melting and refining of " metals, and otherwise improving of them and their ores, as before set forth, be " from henceforth employed for no other use or uses whatsoever, but for the increase " of monies; and that the place hereby appointed for the disposal thereof shall be " their majesties mint within the Tower of *London*; at which place they are to receive " the full and true value for their gold and silver so extracted from time to time, ac- " cording to the assay and fineness thereof; and so for any greater or lesser weight: " and that none of that metal of gold and silver, so refined and extracted, be per- " mitted to be used or disposed in any other place or places within their majesties " kingdoms and dominions.

" PROVIDED also, and be it further enacted by the authority aforesaid, that no " mine of copper, tin, iron, or lead, shall hereafter be adjudged, reputed, or " taken to be a royal mine, although gold or silver may be extracted out of the " same."

IN 1690 he gave the world his *Medicina Hydrostatica: or Hydrostatics applied to the* Materia Medica; *shewing how by the weight that divers bodies used in physic have in water, one may discover whether they be genuine or adulterate. To which is subjoined, A previous hydrostatical way of estimating Ores*; London, in 8vo. In the *Postscript* he " observes, that when he first sent to the press his *Medicina Hydrostatica*, he intended " it should, in the same book or volume, be accompanied by another help or two, " to explore and improve the *Materia Medica*. But when, *says he*, the Essay itself, " and the annexed Epistle about a previous Exploration of Ores had been printed " off, I could not but perceive, that the bulk of those two tracts so far exceed what

[b] Statutes at Large, Vol. II. p. 1545. cap 30, edit. *London*, 1706.

" I ex-

" I expected, that if I subjoined what at first I designed to add to it, it would prove " a mis-shapen book, and inconvenient to be opened. Wherefore it seemed expe- " dient to divide the whole intended work in two volumes or tomes, whereof what " had already past the press should make the first; which, that it might be the sooner " serviceable, should forthwith come abroad by itself; and the second should consist " partly of the other papers abovementioned, as relating to the *Materia Medica*, " and partly of a supplement to the first tome, containing divers historical paralipo- " mena, that by mistake were omitted, and are fit to be there supplied out of a fuller " copy than that, which by an oversight was made use of at the press." But this second tome never appeared. In the same year he published likewise another excellent work, entitled, *The Christian Virtuoso; shewing, that by being addicted to experimental philosophy a man is rather assisted than indisposed to be a good Christian. The first part. To which are subjoined,* I. *A Discourse about the Distinction that represents some things as above reason, but not contrary to reason.* II. *The first Chapters of a Discourse, entitled, Greatness of Mind promoted by Christianity.* Printed in the *Savoy*, in 8vo. In the *Advertisement* prefixed to the *Reflections upon a Theological Distinction, according to which it is said, that some Articles of Faith are above Reason, but not against Reason*, he observes, that after he had begun the *second part* of the *Christian Virtuoso*, and made some progress in it, which he designed to continue till he had completed it, he was obliged to leave the country, where he enjoyed some leisure, and to remove to *London*; where sickness, and business, and a multitude of visits he could not avoid receiving, did so distract him, that these remoras, added to the fertility of the subjects that remained to be treated of, which he found much greater than he was at first aware of, made him lay aside the materials he had prepared for the *second part* to a fitter opportunity, and comply with the occasions he had to publish some tracts that required more. He did not live to finish this *second part*; but the papers, which he left behind him for that purpose, as well as for an *Appendix* to the *first part*, are printed in the present edition.

THE year following he communicated to Monsieur *de la Crose*, author of *the History of Learning*, an *Account of some Observations made in the great Congregation of Waters, by lowering Bottles down into the sea six hundred feet deep from the surface, January the 2d*, 1677-8, which was printed in that work for the month of July 1691[c], with a short letter from Mr. *Boyle* to the author, in which he observes, that this " experiment made a great noise in the court of King *Charles* II. and will, *says he*, " resolve all the difficulties in the questions, which you proposed to me, concerning " the coldness of water. It was made by a captain of a ship, a man of very good " sense, and in the presence of a great many persons; insomuch that there can be no " manner of doubt concerning it."

THE last work, which he published himself, was his *Experimenta & Observationes Physicæ: wherein are briefly treated of several Subjects relating to Natural Philosophy in an experimental Way. To which is added, a small Collection of strange Reports.* Part I. *London* 1691, in 8vo. The *second part* never appeared.

BEING now sensible from the decays of his health, that his death could not be very remote, he determined upon drawing up his last will, which he signed and sealed upon the 18th of July 1691, and of which I shall subjoin a copy in the *Appendix* to this *Life*; but before he proceeded to the perfecting of it, he wrote down the following protestation.

[c] Page 57, 58.

" WHEREAS

" WHEREAS I am this day about to perfect my last will and testament, I do hereby, " to prevent and secure myself from all scruples, solemnly protest and declare, that " I do not intend by signing and sealing the said will, or any other will or codicil " that I may hereafter sign, to abridge myself of any power that law or equity, or " the nature of a will, do or can give me, to dispose freely of all or any of my tem- " poral concerns, even those assigned to pious or charitable uses therein mentioned; " and that I reserve to myself a full liberty, when this or any other testament shall " be perfected, to annul, revoke, or alter the whole will, or any part of it, and " dispose otherwise of my concerns, as freely as if the said will, or any part or " codicil of it, had never been signed and published, or so much as intended by me. " Witness my hand this 18th day of July 1691.

" ROBERT BOYLE.

" Witness
" JOHN WARR."

IN October following we find by a letter of his, dated the 8th of that month, to Dr. *Turberville* of *Salisbury* [4], that his sister the lady *Ranelagh* and himself were extremely ill; and he particularly complains of a distemper in his eyes, which greatly surprized and afflicted him, having continued with him for about a month. " The " case in short, *says he*, is this: in the day time, I see, thanks be to God, as I use to " do, and so till five o'clock in the afternoon; but then, as soon as candles are " brought in, I find a very sensible decay in my sight; so that though I can see all " the same gross objects as I did before, and could, if I durst, read printed books, " as I have often tried, yet the reflection from those objects is not vivid, as it was " wont to be; and if I look upon somewhat distant objects, methinks I see them " through a thin mist, or a little smoke; but when the candles are newly snuffed, " and so the light increased, I see far better for a little while, till it begin to have " more snuff. This distemper continues as long as I make use of candle-light; but " the next morning, by God's goodness, I find myself as before; only now and then " there seems to fall slowly down, sometimes in one eye, and sometimes in another, " a faintly shining vapour, which immediately disappears. I have such apparitions " of late, for these two or three years, without any bad consequence. What this " distemper may proceed from, I know not, though I remember I have heard you " more than once take notice of the narrowness of my pupil. Sight is a thing so " dear to all men, and especially to studious persons, that I earnestly beg you would " be pleased to consider my case deliberately, and acquaint me with your thoughts " of the cause; and more particularly to send prescriptions of the receipts you would " have me employ, and your directions what else you would have me do towards the " cure of it.—I forgot to tell you, that for some months last past I have been much " troubled with what they call vapours, or fumes of the spleen, and with some scor- " butic disaffections."

HIS sister *Ranelagh*'s indisposition at length terminated in her death, on the 23d of December following; and the loss of such a person was perhaps as fatal to him as it was important to the world. She had lived the longest on the most public scene, and made the greatest figure in all the revolutions of these kingdoms for above fifty

[4] Vol. VI. p. 61, 62.

years,

years, of any woman of that age. She employed her whole time, intereſt, and eſtate, in doing good to others; and as her great underſtanding, and the vaſt eſteem ſhe was in, made all perſons in their ſeveral turns of greatneſs deſire and value her friendſhip, ſo ſhe gave herſelf a clear title to uſe her intereſt with them for the ſervice of others, by this, that ſhe never made any advantage of it to any end or deſign of her own. She was contented with what ſhe had; and though ſhe was twice ſtript of it, ſhe never moved on her own account, but was the general interceſſor for all perſons of merit or in want. This had in her the better grace, and was both more Chriſtian and more effectual, becauſe it was not limited within any narrow compaſs of parties or relations. When any party was depreſſed, ſhe had credit and zeal enough to ſerve them; and ſhe employed that ſo effectually, that in the next turn ſhe had a new ſtock of credit, which ſhe laid out wholly in that *labour of love*, in which ſhe ſpent her life. And though ſome particular opinions might ſhut her up in a divided communion, yet her ſoul was never of a party. She divided her charities and friendſhips, her eſteem as well as her bounty, with the trueſt regard to merit and her own obligations, without any difference made upon the account of opinion. She had, with a vaſt reach both of knowledge and apprehenſion, an univerſal affability and eaſineſs of acceſs; an humility, that deſcended to the meaneſt perſons and concerns, an obliging kindneſs and readineſs to adviſe thoſe who had no occaſion for any further aſſiſtance from her. And with all theſe and many other excellent qualities ſhe had the deepeſt ſenſe of religion, and the moſt conſtant turning of her thoughts and diſcourſes that way, that was known perhaps in that age. Such a ſiſter became ſuch a brother; and it was but ſuitable to both their characters that they ſhould have improved the relation, under which they were born, to the more exalted and endearing one of friend [e]. And as they were *pleaſant in their lives, in their death they were not divided*; for as he had lived with her for the greateſt part of forty-ſeven years, ſo he did not ſurvive her above a week, for he died in the ſixty-fifth year of his age, on Wedneſday, December 30, 1691, at three quarters of an hour after twelve at night [f], and was interred on the 7th of January following, at the upper end of the ſouth ſide of the chancel of St. *Martin's in the Fields* in *Weſtminſter*, near the body of his ſiſter *Ranelagh*, his funeral ſermon being preached by Dr. *Gilbert Burnet*, biſhop of *Saliſbury*, upon this text, *Eccleſ.* ii. 26. *For God giveth to a man, that is good in his ſight, wiſdom, knowledge, and joy.* His funeral was decent; and though without pomp, yet honoured with a great appearance of perſons of the higheſt diſtinction, beſides his own numerous relations.

BEFORE I proceed to his character, I ſhall inſert an account of his poſthumous works, which were as follows:

I. *The General Hiſtory of the Air deſigned and begun.* Printed at *London*, 1692, in 4to. Mr. *Locke*, in a letter to *William Molyneux*, Eſq; dated December 26, 1692, obſerves, that "though this treatiſe was left imperfect, yet, I think, *ſays he*, the very "deſign of it will pleaſe you; and it is caſt into a method, that any one, "who pleaſes, may add to it under any of the ſeveral titles, as his reading or ob-"ſervation ſhall furniſh him with matter of fact. If ſuch men as you are, curious "and knowing, would join to what Mr. *Boyle* had collected and prepared, what "comes in their way, we might hope in ſome time to have a conſiderable hiſtory of "the air, than which I ſcarce know any part of natural philoſophy would yield more "variety and uſe. But it is a ſubject too large for the attempts of any one man,

[e] Biſhop *Burnet's* Funeral Sermon on Mr. *Boyle*, p. 33, 34.

[f] Mr. *Ware's* MSS.

"and

" and will require the assistance of many hands to make it a history very short of " complete." Mr. *Molyneux*, in his answer, dated March 2, 1692-3, writes thus: " I am extremely obliged to you for Mr. *Boyle*'s book of the air, which lately came " to my hands. It is a vast design, and not to be finished but by the united labours " of many heads, and indefatigably prosecuted for many years; so that I despair of " seeing any thing complete therein. However, if many will lend the same helping " hands that you have done, I should be in hopes; and certainly there is not a " chapter in all natural philosophy of greater use to mankind, than what is here pro- " posed."

II. *Medicinal Experiments: or a Collection of choice remedies, for the most part simple and easily prepared: London* 1692, 12mo. This is a *second edition* of the *Receipts sent to a Friend in America:* printed in 1688, with a new preface, and the addition of a *second part*, or the latter five decads. These were again reprinted in 1696, and called the *first* and *second* volume; and in 1698 a *third volume* was added to them.

III. *General Heads for the Natural History of a country great or small, drawn out for the Use of Travellers and Navigators.* To which are added, *Other Directions for Navigators, &c. with particular Observations of the most noted Countries in the world. By another Hand. London* 1692, in 12mo. These *General Heads* were first printed in the *Philosophical Transactions*, No. II. p. 186. No. XVIII. p. 315. No. XIX. p. 330. being drawn up by Mr. *Boyle*, at the request of the Royal Society. The other directions added in this edition were drawn up by various persons at divers times, by order of the *Royal Society*, and printed in different numbers of the *Philosophical Transactions*; but being in pursuance of the plan sketched out by Mr. *Boyle*, were very properly annexed to the preceding ones.

IV. *A paper of the honourable* Robert Boyle's, *deposited with the Secretaries of the Royal Society, October* 14, 1680, *and opened since his death; being an Account of his making the Phosphorus, &c. September* 30, 1680. Printed in the *Philosophical Transactions*, No. CXCVI. p. 583. anno 1692-3.

V. *An Account of a Way of examining Waters, as to freshness and saltness. To be subjoined as an Appendix to a lately printed Letter about sweetened Water: October* 30, 1683: published in the *Philos. Transact.* No. CXCVII. p. 627.

VI. *A Free Discourse against Customary Swearing, and a Dissuasive from Cursing. London* 1695, in 8vo.

VII. *Medicinal Experiments: or a Collection of choice remedies, chiefly simple and easily prepared; useful in Families, and fit for the Service of Country People. The third and last Volume, published from the Author's original MSS. Whereunto is added, several other useful notes, explicatory of the same. London* 1698, in 12mo. This and the two preceding volumes have been since several times reprinted all together.

MR. *Boyle* was tall of stature, but slender, and his countenance pale and emaciated[g]. His constitution was so tender and delicate, that he had divers sorts of cloaks to put on when he went abroad, according to the temperature of the air; and in this he governed himself by his thermometer[h]. He escaped indeed the small pox during his life[i]; but for almost forty years he laboured under such a feebleness of

[g] Letter of *John Evelyn*, Esq; to Dr. *Wotton*, March 29, 1696.
[h] Information of Sir *Hans Sloan*, Bart.
[i] Letter of the Rev. Mr. *Kirkwood* to Mr. *Boyle*, dated July 13, 1689.

body,

body, and ſuch lowneſs of ſtrength and ſpirits, that it was aſtoniſhing, how he could read, meditate, try experiments, and write as he did. He had likewiſe a weakneſs in his eyes, which made him very tender of them, and extremely apprehenſive of ſuch diſtempers as might affect them. He imagined alſo, that if ſickneſs ſhould confine him to his bed, it might raiſe the pains of the ſtone to a degree, which might be above his ſtrength to ſupport; ſo that he feared leſt his laſt minutes ſhould prove too hard for him. This was the ground of all the caution and apprehenſion, which he was obſerved to live in. But as to life itſelf, he had that juſt indifference to it, which became ſo true a Chriſtian. However, his ſight began not to grow dim above four hours before he died; and when death came upon him, he had not been above three hours in bed, before it made an end of him with ſo little pain, that it was plain the light went out merely for want of oil to maintain the flame [k]. The ſimplicity of his diet was in all appearance that which preſerved him ſo long beyond all men's expectation. This he practiſed ſo ſtrictly, that in a courſe of above thirty years he neither eat nor drank to gratify the varieties of appetite, but merely to ſupport nature; and was ſo regular in it, that he never once tranſgreſſed the rule, meaſure, and kind, which were preſcribed him [l].

IN his firſt addreſſes, when he was to ſpeak or anſwer, he ſometimes heſitated a little, rather than ſtammered, or repeated the ſame word; and this, as it rendered him ſlow and deliberate, ſo after the firſt effort he proceeded without the leaſt interruption in his diſcourſe [m].

HE was never married; but Mr. *Evelyn* was aſſured, that he courted the beautiful and ingenious daughter of *Cary*, earl of *Monmouth*; and that to this paſſion was owing his *Seraphic Love* [n]. But it does not appear from any of his papers, that he had ever entertained the leaſt thoughts of that kind; and he wrote the following letter, when he was very young, to the lady *Barrimore*, his niece, upon a report of his being actually married, of which that lady had informed him.

" * * * It is high time for me to haſten the payment of the thanks I owe your " ladyſhip for the joy you are pleaſed to wiſh me, and of which that wiſh poſſibly gives " me more than the occaſion of it would. You have certainly reaſon, Madam, to " ſuſpend your belief of a marriage celebrated by no prieſt but fame, and made un- " known to the ſuppoſed bridegroom. I may poſſibly ere long give you a fit of the " ſpleen upon this theme; but at preſent it were incongruous to blend ſuch pure " raillery, as I ever prate of matrimony and amours with, among things I am ſo " ſerious in as thoſe this ſcribble preſents you. I ſhall therefore only tell you, that " the little gentleman and I are ſtill at the old defiance. You have carried away too " many of the perfections of your ſex, to leave enough in this country for the re- " ducing ſo ſtubborn a heart as mine, whoſe conqueſt were a taſk of ſo much diffi- " culty, and is ſo little worth it, that the latter property is always likely to deter any, " that hath beauty and merit enough to overcome the former. But though this " untamed heart be thus inſenſible to the thing itſelf called love, it is yet very ac- " ceſſible to things very near of kin to that paſſion; and eſteem, friendſhip, reſpect, " and even admiration, are things, that their proper objects fail not proportionably

[k] *Burnet*'s Funeral Sermon, p. 31.
[l] Id. Ibid.
[m] Mr. *Evelyn*'s letter of March 29, 1696.
[n] Ibid.

" to exact of me, and consequently are qualities, which in their highest degrees are
" really and constantly paid my lady *Carrimore* by her,

" Most obliged humble servant,

" and affectionate uncle,

" ROBERT BOYLE."

In the memorandums of Mr. *Boyle*'s life, set down by bishop *Burnet*, it is remarked, that *he abstained from purposes of marriage, at first out of policy, afterwards more philosophically; and upon a general proposition with many advantages, he would not know the person's name.* And we find by a letter of Dr. *John Wallis* to him, dated at *Oxford*, July 17, 1699 [o], that he had an overture made to him with respect to the lady *Mary Hastings*, sister to the earl of *Huntingdon*, and eminent for her admirable temper, great piety, and uncommon understanding, with every other accomplishment proper to make him an excellent wife. But he still persisted in his first resolution of living single, though few men were more facetious and agreeable in conversation with the ladies, whenever he happened to be engaged among them [p]. And indeed sometimes, upon other occasions, he distinguished himself by so copious and lively a flow of wit, that Mr. *Cowley*, and Sir *William Davenant*, both thought him equal in that respect to the most celebrated geniuses of that age [q].

He had so profound a veneration for the Deity, that the very name of God was never mentioned by him without a pause and a visible stop in his discourse; in which Sir *Peter Pett* [r], who knew him for almost forty years, affirms, that he was so exact, that he did not remember to have observed him once to fail in it. He was very constant and serious in his secret addresses to God; and it appeared to those who conversed most with him in his enquiries into nature, that his main design in that, on which as he had his own eye most constantly, so he took care to put others often in mind of it, was to raise in himself and others more elevated thoughts of the greatness and glory, and of the wisdom and goodness of the Deity [s]. This was so deep in his mind, that he concludes the article of his will, which relates to the Royal Society, in these words: *Wishing them also a happy success in their laudable attempts to discover the true nature of the works of God, and praying, that they and all other searches into physical truths, may cordially refer their attainments to the glory of the great Author of Nature, and to the comfort of mankind.* For this purpose he founded his lecture in the city of *London*, charging, by a codicil annexed to his will, and dated July 28, 1691, his messuage or dwelling-house in St. *Michael's Crooked-Lane*, in that city, with the payment of the clear yearly rents and profits thereof to some learned divine in *London*, or within the bills of mortality, to be elected for a term not exceeding three years by Dr. *Tenison*, afterwards archbishop of *Canterbury*, Sir *Henry Ashurst*, Sir *John Rotherham*, and *John Evelyn*, Esq. The business which he appointed those lecturers, was, among others, *to be ready to satisfy real scruples, and to answer such new objections and difficulties, as might be started, to which good answers had not been made*; and also *to preach eight sermons in the year, the first Monday of January, February, March,*

[o] Vol. VI. p. 458, 459, 460. [p] Mr. *Evelyn*'s letter of March 29, 1696.
[q] Sir *Peter Pett*'s papers relating to Mr. *Boyle*. [r] Ibid. [s] *Burnet*'s Funeral Sermon, p. 25.

April, and May, and of September, October, and November. The subject of these sermons was to be *the proof of the Christian Religion against notorious Infidels, viz. Atheists, Theists, Pagans, Jews, and Mahometans, not descending lower to any controversies, that are among Christians.* But by reason the lectures were seldom continued above a year, and that the house sometimes stood empty, and tenants broke, or failed in due payment of their rent, therefore the salary sometimes remained long unpaid, or could not be gotten without some difficulty. To remedy which inconvenience, archbishop *Tenison* procured a yearly grant of 50 pounds to be paid quarterly for ever, charged upon a farm in the parish of *Brill*, in the county of *Bucks*; which stipend is accordingly very duly paid, when demanded, without fee or reward[t].

He was likewise at the charge of the translation and impression of the *Four Gospels* and *Acts of the Apostles* into the Malayan language, as I have remarked above; and this book he sent over all the *East-Indies*. He gave a noble reward to Dr. *Edward Pococke*, who translated *Grotius*'s incomparable treatise *of the Truth of the Christian Religion* into Arabic, and was at the charge of a whole impression, which was finished at *Oxford* in 1660, in 4to, and which he took care to order to have dispersed in all the countries, where that language is understood. He was resolved to have carried on the impression of the New Testament in the Turkish language; but the Company thought, that it became them to perform that work, and so suffered him only to give a large share towards it[u]. He was at 700 pounds charge in the edition of the *Irish* bible, which he ordered to be distributed in *Ireland*[v]; a particular account of which will appear from the *Appendix*, No. III.

He contributed largely also to the impressions both of the Welch bible, and of the Irish bible, for the use of the *High-Lands* in *Scotland*, as may be likewise seen in the *Appendix*, No. IV.

He gave, during his life, 300 pounds to advance the design of propagating the Christian religion in *America*; and his zeal and generosity in that respect are acknowledged in many letters of Mr. *Eliot*, of *New-England*; which are given in the *Appendix*, No. V.

He was no less a friend to that colony in their civil affairs, as appears by letters of thanks signed by the governor, *&c.* sent to him, and printed in the *Appendix*, No. VI.

He had the greatest regard for the clergy, of which he gave many instances. When he understood, what share he had in impropriations, he ordered very large gifts to be made to the incumbents in those parishes, and to the widows of such as died before he had resolved upon this charity. A person who was concerned in two distributions which were made, declared, that the sums upon those two occasions amounted to near 600 *l.* *And another very liberal one*, says bishop *Burnet*[w], *is also ordered by his will, but in an indefinite sum; I suppose, by reason of the present condition of estates in* Ireland; *so plentifully did he supply those who served at the altar out of that, which was once devoted to it, though it be now converted to a temporal estate.* It appears likewise, by a letter of his to one of his stewards in *Ireland*, dated January 22, 1684-5, that he had ordered a former steward to set aside every year about a fifth part (rather

[t] See the note upon Dr. *Will Derham*'s dedication of his *Physico-Theology*, fifth edit. *London* 1720, in 8vo.

[u] *Burnet*'s Funeral Sermon, p. 26. [v] Id. ibid. See likewise *Burnet*'s Life of Dr. *Will. Bedell*, bishop of *Kilmore*, p. 136, 137. Edit. *London* 1685. in 8vo. [w] Funeral Sermon, p. 27.

more than less) of the clear annual income of his tithes and impropriations, to be employed in pious uses. The orders to the new steward run thus: "I must desire "you at every half year's rent day, or when the rents then due shall be paid into "you, to lay aside, till you receive contrary orders, the full fifth part of what you "judge you receive for me *de claro* upon the account of tithes and impropria-"tions, to be disposed," *&c.* And in a letter of Sir *Robert Southwel* to him from *Kingsale*, March 20, 1675[1], are these words: *It has often come into my mind, and indeed I thought, that I had accordingly wrote lately of it, that concern of yours, which long since you were pleased to honour me with the trust and care of, I mean, that stock of money, which, out of your great bounty and charity, you were pleased to distribute amongst those ministers that had served the cures upon your impropriate estate in this kingdom, and of the distribution of which I did long since return you an account, and also what balance remained unto you, after satisfying all the concerned, which was forty-five pounds, &c.* I find also, by a list of names, which he had procured, together with several letters of thanks sent him out of *Wales*, that he extended his diffusive charity to such poor nonconformist ministers there, as were men of piety and good learning.

He was constant to the established church, and went to no separate assemblies, how charitably soever he might think of their persons, and how plentifully soever he might have relieved their necessities[7]. He had once indeed, before the restoration, as he told Sir *Peter Pett*[8], the curiosity to go to Sir *Henry Vane*'s house, and there heard him preach in a large thronged room a long sermon, on the text of *Dan.* xii. 2. *And many of them that sleep in the dust of the earth shall awake, some to everlasting life, and some to shame and everlasting contempt.* The whole scope of Sir *Henry*'s sermon was to shew, that many doctrines of religion, that had long been dead and buried in the world, should before the end of it be awakened into life; and that many false doctrines being then likewise revived, should, by the power of truth, be then doomed *to shame and everlasting contempt.* When Sir *Henry* had concluded his discourse, Mr. *Boyle* spoke to this effect to him before the people; That being informed, that in such private meetings it was not uncustomary for any one of the hearers, who was unsatisfied about any matters there uttered, to give in his objections against them, and to prevent any mistakes in the speakers or hearers; he thought himself obliged for the honour of God's truth to say, that this place in *Daniel* being the clearest one in all the Old Testament for the proof of the resurrection, we ought not to suffer the meaning of it to evaporate into allegory; and the rather, since that inference is made by our Saviour in the New Testament by way of asserting the resurrection from that place of *Daniel* in the old. And that if it should be denied, that the plain and genuine meaning of those words in the Prophet is to assert the resurrection of dead bodies, he was ready to prove it to be so, both out of the words of the text and context in the original language, and from the best expositors both Christian and Jewish. But that if this be not denied, and Sir *Henry*'s discourse of the resurrection of doctrines true and false was designed by him only in the way of occasional meditations from those words in *Daniel*, and not to enervate the literal sense as their genuine one, then he had nothing further to say. Mr. *Boyle* then sitting down, Sir *Henry* rose up and said, that his discourse was only in the way of such occasional meditations, which he thought edifying to the people; and declared, that he agreed, that the literal sense of the words was the resurrection of dead bodies; and so that meeting broke up. Mr. *Boyle* afterwards speaking of this conference to Sir *Peter Pett*, observed, that

[6] Vol. VI. p. 301. [7] *Burnet*'s Funeral Sermon, p. 29. [8] Sir *Peter Pett*'s papers relating to Mr. *Boyle*.

that Sir *Henry Vane* at that time being in the height of his authority in the ſtate, and his auditors at that meeting conſiſting chiefly of dependents on him and expectants from him, the fear of loſing his favour would probably have reſtrained them from contradicting any of his interpretations of ſcripture, how ridiculous ſoever. " But " I (ſaid Mr. *Boyle*) having no little awes of that kind upon me, thought myſelf " bound to enter the liſts with him, as I did, that the ſenſe of the ſcriptures might " not be depraved."

HE had poſſeſſed himſelf with ſuch an amiable view of Chriſtianity, ſeparated from either ſuperſtitious practices or the ſourneſs of parties, that as he was fully perſuaded of the truth of it, he rejoiced in every diſcovery, which nature furniſhed him with to illuſtrate it, or to take off the objections againſt any part of it. He always conſidered it as a ſyſtem of truths, which ought to purify the hearts, and govern the lives of thoſe who profeſs it. He loved no practice, which ſeemed to leſſen that, nor any nicety, which occaſioned diviſions amongſt Chriſtians. He thought, that pure and diſintereſted Chriſtianity was ſo bright and glorious a ſyſtem, that he was much troubled at the diſputes and diviſions which had riſen about ſome leſſer matters, while the great and the moſt important, as well as moſt univerſally acknowledged truths were by all ſides almoſt as generally neglected, as they were confeſſed [a]. He loved no narrow thoughts, no low or ſuperſtitious opinions in religion; and therefore as he did not ſhut himſelf within a party, ſo neither did he ſhut any party out from him [b]. His zeal was lively and effectual in the greateſt and trueſt concerns of religion; but he avoided to enter far into the unhappy breaches, which had long weakened as well as diſtracted Chriſtianity any otherwiſe, than to have a great averſion to all thoſe opinions and practices, which ſeemed to him to deſtroy morality and charity. He had a moſt particular zeal againſt all ſeverities and perſecution upon the account of religion; *and I have ſeldom*, ſays biſhop *Burnet* [c], *obſerved him to ſpeak with more heat and indignation, than when that came in his way. He did thoroughly agree with the doctrines of our church, and conform to our worſhip; and he approved of the main of our conſtitution; but he much lamented ſome abuſes, that he thought remained ſtill among us.* And Dr. *Thomas Dent*, prebendary of *Weſtminſter*, who was a particular friend of his, obſerves [d], that " he always heard him expreſs his judgment and " inclination to the church of *England*; but he was for moderation to thoſe who " diſſented from us, and not to force tender conſciences, for which he ſeemed to " expreſs great averſeneſs. He had frequent conferences on this ſubject with the " preſent archbiſhop [*Teniſon*], biſhop of *Sarum* [*Burnet*], but particularly the late " biſhop of *Worceſter*, the learned Dr. *Stillingfleet*, for whoſe depth of learning and " ſolid judgment he had always the greateſt value and eſteem." Sir *Peter Pett* likewiſe affirms, that he was peculiarly warm in his expreſſions againſt perſecution on account of religion; and relates, that ſoon after the reſtoration Mr. *Boyle* and he diſcourſing of the ſeverities practiſed by the biſhops towards the Puritans in the reign of King *Charles* I. and of thoſe which were returned upon the epiſcopal divines, during the following uſurpations; and being apprehenſive, that the reſtored clergy might be tempted by their late ſufferings to ſuch a vindictive retaliation, as would be contrary to the true meaſures of Chriſtianity and politics, they came at laſt to an agreement, that it would tend to the public good, to have ſomething written and publiſhed in defence of liberty of conſcience. Mr. *Boyle* undertook to engage Dr. *Thomas Barlow*,

[a] *Burnet*'s Funeral Sermon, p. 25, 26. [b] Ibid. p. 29. [c] Ibid p. 27.
[d] Letter to Dr. *William Wotton*, of May 20, 1699.

whoſe

whose judgment in that point he very well knew to treat of the theological part of the question; and desired Sir *Peter* to write of the political part; which the latter consented to, on condition that Mr. *Boyle* would let him read his manuscript before it was committed to the press, and give him his opinion about the whole. Mr. *Boyle* frankly promised this, and was willing to state the fact of the allowance of liberty of conscience in foreign parts. But afterwards considering, that Mr. *John Dury*, who had spent many years in his travels, especially in the northern parts of *Europe*, when he was engaged in his scheme for reconciling the Lutherans and Calvinists, was capable of writing on that subject with more extent and exactness than himself, he prevailed upon Mr. *Dury* to write upon it, and rewarded him for it, and delivered the treatise drawn by him to Sir *Peter*, who published it at the end of his own in 1660, (though the booksellers, according to their custom, antedated in the title page 1661) and inscribed both those treatises with the last letters only of the writers names. But he did not print Dr. *Barlow*'s discourse upon the same subject; because as, on the one hand, it would not, how strong soever its reasonings were, be sufficient to restrain the rigorous measures resolved upon against the Nonconformists, so, on the other, it might expose the doctor to the resentment of his brethren, whom he had offended by writing, just before the restoration, a letter to Mr. *John Tombes*, the famous Anabaptist, in which he had expressed some prejudice against the practice of infant-baptism, and by refusing, even after the restoration, to retract that letter, notwithstanding he was in danger of losing, by that refusal, his station in the university of *Oxford*, and all his hopes of future preferment. However his discourse abovementioned was published after his death, under the title of *The Case of a Toleration in Matters of Religion*, in a collection, intitled, *Several miscellaneous and weighty Cases of Conscience learnedly and judiciously resolved by the Right Reverend Father in God Dr.* Thomas Barlow, *late Lord Bishop of* Lincoln; printed at *London* 1692, in 8vo.

MR. *Boyle*'s charity to those who were in want, and his bounty to all learned men who were put to wrestle with necessities, were very extraordinary. Great sums went easily from him without the partialities of sect, country, or relations; for he considered himself as a part of the human nature, and a debtor to the whole race of men. He took care to do this so secretly, thal even those who knew all his other concerns, could never find out what he did that way. And indeed he was so strict to our Saviour's precept, that except the persons themselves, or some one whom he trusted to convey it to them, no body ever knew how that great share of his estate, which went away invisibly, was distributed; even he himself kept no account of it, for that he thought might fall into other hands[e]. "I speak (says bishop *Burnet*[f]) upon full "knowledge on this article, because I had the honour to be made use of by him in "it. If those that have fled hither from the persecutions of *France*, or from the cala-"mities of *Ireland*, feel a sensible sinking of their secret supplies, with which they "were often furnished, without knowing from whence they came, they will conclude, "that they have lost not only a purse, but an estate, that went so very liberally among "them, that I have reason to say, that for some years his charity went beyond a thou-"sand pounds a year."

HE was very plain, unaffected, and temperate in the manner of his life, and had about him all that neglect of pomp in clothes, lodging, furniture, and equipage, which agreed with his grave and serious course of life[g].

He was extremely candid and courteous in his conversation. His constitution indeed inclined him to be choleric; but he gained so perfect an ascendent over this passion,

[e] *Burnet*'s Funeral Sermon, p. 31. [f] Ibid. p. 32. [g] Ibid p. 30.

ſion, that it never appeared, except ſometimes in his countenance upon a very high provocation [h]. He had brought his mind to ſuch a freedom, that he was not apt to be impoſed upon; and his modeſty was ſuch, that he did not dictate to others, but propoſed his own ſenſe with a due and decent diſtruſt, and was ever ready to hearken to what was ſuggeſted to him by others. When he differed from any, he expreſſed himſelf in ſo humble and ſo obliging a way, that he never treated things or perſons with neglect; and he was never known to have offended any perſon in his whole life by any part of his deportment. For if at any time he ſaw cauſe to ſpeak roundly to any, it was never in paſſion, or with any reproachful or indecent expreſſions. And as he was careful to give thoſe who converſed with him no cauſe or colour for diſpleaſure; ſo he was yet more careful of thoſe who were abſent, never to ſpeak ill of any. If the diſcourſe began to be hard upon any perſon, he was immediately ſilent; and if the ſubject was too long dwelt upon, he would at laſt interpoſe, and between reproof and raillery divert it [i].

WHATEVER he was in the ſight of men, how pure and ſpotleſs ſoever his character appeared to the world, he was in reality the ſame in his moſt ſecret receſſes. He affected nothing which was ſolemn or ſupercilious, nor uſed any methods to make multitudes run after him, or depend upon him. It was never diſcovered, that there was any thing hid under all this appearance of goodneſs which was not truly ſo; for he concealed both his piety and charity all he could, and lived in the due methods of civility, and would never aſſume the authority which all the world was ready to pay him. He allowed himſelf a great deal of decent chearfulneſs, without the leaſt tincture of that moroſeneſs to which philoſophers think they have ſome right; nor of thoſe affectations which men of an extraordinary pitch of devotion ſometimes run into, without being well aware of them. He had indeed nothing of frolic and levity in him; he had no reliſh for the idle and extravagant madneſs of the men of pleaſure. He did not waſte his time, nor diſſipate his ſpirits in fooliſh mirth; but he poſſeſſed his own ſoul in patience, full of that ſolid joy which his goodneſs as well as his knowledge afforded him. He, who had neither deſigns nor paſſions, was capable of little trouble from any concerns of his own. He had about him all the tenderneſs of good-nature, as well as all the ſoftneſs of friendſhip. Theſe gave him a large ſhare of other men's concerns; for he had a quick ſenſe of the miſeries of mankind. He had alſo a feeble body, which needed to be looked to the more, becauſe his mind went faſter than his body could keep pace with it. Yet his great thoughts of God, and his contemplation of his works, were to him ſources of continual joy, which never could be exhauſted. The ſenſe of his own integrity, and of the good which he found it did, afforded him the trueſt of all pleaſure, *ſince they gave him*, ſays biſhop *Burnet* [k], *the certain proſpect of that fulneſs of joy, in the ſight of which he lived ſo long.*

HE ſpake of the government even in times which he diſliked, and upon occaſions which he ſpared not to condemn, with an exactneſs of reſpect [l]. King *Charles* II. King *James* II. and King *William*, were ſo highly pleaſed with his converſation, that they often uſed to diſcourſe with him with great familiarity. His four elder brothers being all noblemen, he was ſeveral times offered a peerage, which he conſtantly refuſed to accept [m]; but he procured a title, which, without derogating from the dignity of Kings, muſt be acknowledged to be beyond their prerogative [n]. He had too unblemiſhed a candor, to be capable of thoſe arts and practices which the world generally

[h] *Burnet*'s MS. papers. [i] *Burnet*'s Funeral Sermon, p. 29, 30. [k] Id. p. 38.
[l] Id. p. 35. [m] *Budgell*'s Life of the Earl of *Orrery*, p. 145. [n] *Burnet*'s Funeral Sermon, p. 23.

terms

terms wisdom. He could neither lie nor equivocate, but could well be silent; and by practising that much, he covered himself upon many uneasy occasions. He made true judgments of men and things; and his advices were solid and sound; and if caution and modesty gave too strong a bias, his invention was fruitful to suggest good expedients. He had great notions of what human nature might be brought to; but since he saw mankind not capable of them, he withdrew himself early from courts and affairs, *notwithstanding the distinction*, says bishop *Burnet* [o], *with which he was always used by our late Princes.* He had the principles of an Englishman, as well as of a protestant, too deep in him to be corrupted, or to be cheated out of them; and in these he studied to fortify all who conversed much with him. He had a very particular sagacity in observing what men were fit for; and had so vast a scheme of different performances, that he could easily furnish every man with work, who had leisure and capacity for it; and as soon as he saw him engaged in it, then an handsome present was made to enable him to go on with it [p].

THE reputation which he had acquired among foreign nations was so great, that no strangers, who came among us, and had any taste for learning and philosophy, left *England* without seeing him. He received them with a certain openness and humanity which were peculiar to him; and though these visits made a great waste of his time, yet as he was strict in not suffering himself to be denied when he was at home, so he said, that he knew the heart of a stranger, and how much eased his own had been, while he was travelling, if admitted to the conversation of those whom he desired to see; and therefore he thought that his obligation to strangers was more than mere civility, and that it was a point of religious charity in him [q].

HIS knowledge was of prodigious extent. He was a great master of the *Greek* language, and read the New Testament in the original with such attention, that he could have quoted it almost as readily as the English version [r]. He carried the study of the Hebrew tongue very far into the Rabbinical writings; and drew up a grammar in it for his own use. He learned likewise the Chaldee and Syriac, and would have gained a thorough knowledge of the Arabic, if the infirmity of his eyes had not interrupted his progress in it [s]. He had read so much of the Fathers, that he had formed out of it a clear judgment of all the eminent ones. He had read over a vast number of commentators on the scriptures, and had gone with great exactness through the whole controversies of religion, and had a just idea of the intire body of divinity. He run the whole compass of the mathematical sciences; and though he did not set up for an inventor in them, yet he knew even the abstrusest parts of geometry. Geography in the several branches of it, which related to navigation or travelling, history, and books of travels, were his diversions. He went very accurately through all the parts of physic; only the tenderness of his nature made him less able to endure the exactness of anatomical dissections, especially of living animals, though he knew these to be most instructing. But for the history of nature ancient and modern, of the productions of all countries, of the virtues and improvements of plants, ores, and minerals, and all the varieties which are in them in different climates, his knowledge was unrivalled in that age [t]. But his peculiar and favourite study was chemistry, "in which, *says bishop* Burnet [u], he engaged with "none of those ravenous and ambitious designs that drew many into them. His "design was only to find out nature, to see into what principles things might be

[o] *Burnet's* Funeral Sermon, p. 35, 36. [p] Ibid p. 36. [q] Ibid. p. 30. [r] *Burnet's* MS. papers. [s] Ibid. [t] *Burnet's* Funeral Sermon, p. 36. [u] Ibid p. 37.

"resolved,

" resolved, and of what they were compounded, and to prepare good medicaments for " the bodies of men. He spent neither his time nor his fortune upon the vain pur- " suits of high promises and pretensions. He always kept himself within the com- " pass, that his estate might well bear. And as he made chemistry much the better " for his dealing in it, so he never made himself the worse or the poorer for it. It " was a charity to others, as well as an entertainment to himself; for the produce of " it was distributed by his sister, and others, into whose hands he put it." His laboratory was constantly open to the curious, whom he permitted to see most of his processes. It is true, he found out some things in the course of his experiments, which he looked upon himself as obliged to conceal for the good of mankind. Of this nature were several sorts of poisons, and a certain liquor with which, he assures us, he could discharge all the writing of any deed upon paper and parchment, and leave nothing but the parties names who signed it; and that the place whence the writing had been discharged, would bear ink again as well as ever [v].

His merit as a writer in natural philosophy and chemistry, is universally acknowledged. Dr. *Herman Boerhaave* [x], after having declared lord *Bacon* to be the father of experimental philosophy, asserts, that " Mr. *Boyle*, the ornament of his age and " country, succeeded to the genius and inquiries of the great chancellor *Verulam*. " Which of Mr. *Boyle*'s writings shall I recommend? All of them. To him we owe " the secrets of fire, air, water, animals, vegetables, fossils; so that from his works " may be deduced the whole system of natural knowledge." Dr *Richard Bentley*, in his *fourth sermon*, preached at the lecture founded by Mr. *Boyle*, thus expresses himself: *The mechanical or corpuscular philosophy, though peradventure the oldest as well as best in the world, had lain buried for many ages in contempt and oblivion, till it was happily restored and cultivated anew by some excellent wits of the present age. But it principally owes its re-establishment and lustre to Mr.* Boyle, *that honourable person of ever-blessed memory, who hath not only shewn its usefulness in physiology, above the vulgar doctrines of real qualities and substantial forms, but likewise its great serviceableness to religion itself.* Mr. *John Hughes* likewise observing [y], that Mr. *Boyle* was born the same year in which lord *Bacon* died, tells us, that " he was the person, who seems to " have been designed by nature, to succeed to the labours and inquiries of that extra- " ordinary genius just mentioned. By innumerable experiments, he in a great mea- " sure filled up those plans and outlines of science, which his predecessors had sketched " out. His life was spent in the pursuit of nature, through a great variety of forms " and changes, and in the most rational as well as devout adoration of its divine " author." *Francesco Redi*, in one of his *letters* published in the *fourth volume* of his works at *Florence* 1724, in 4to, expresses the highest esteem and veneration for him, and asserts, that *he was the greatest man who ever was, and perhaps ever will be, for the discovery of natural causes.* But the justest and completest character of him is given us by the learned and ingenious Dr. *Shaw*, in the *General Preface* to his *Abridgment of Mr.* Boyle's *Philosophical Works*, where he tells us [z], that those works *have, from their first appearing in public, done an honour to his country, and procured him a general esteem in the world. The novelty, the variety, the dignity, and the usefulness of the several subjects he treats, with the easy and familiar manner wherein they are handled, recommend his performances to the whole body of mankind. Mr.* Boyle *accommodates himself as well to the unlearned and the novice, as to the philosopher and the scholar; his whole*

[v] *Budgell.* p 141, 142. [x] *Methodus discendi medicinam.* [y] Spectator, No. 54. Vol. VII.
[z] p 1. 2d edit. *London* 1738, in 4to.

ſcope and deſign being, with the utmoſt candor and ſimplicity, to communicate thoſe numerous and important diſcoveries, which coſt him infinite pains and application to make. He afterwards remarks [a], that Mr. *Boyle* takes up his reader at the elements or fundamental principles of things, and with exquiſite judgment conducts him through all the regions of nature, to furniſh him with objects whereon to exerciſe his faculties; and being firſt ſolicitous to make him a general philoſopher, leaves him prepared for any further inquiry he ſhall think fit to make into the works of nature or art. That as the beſt and only ſolid foundation for philoſophy is its uſefulneſs to mankind, and the only way to gain it the reputation it deſerves, muſt be to manifeſt the great advantages that attend the ſtudy thereof; ſo Mr. *Boyle* has been every where careful to ſhew, that nothing can be more ſerviceable and beneficial in life than experimental philoſophy in general. "The men of wit and learning have, *continues Dr.* Shaw, in "all ages, buſied themſelves in explaining nature by words; but it is Mr. *Boyle* alone "who has wholly laid himſelf out in ſhewing philoſophy in action. The ſingle point "he perpetually keeps in view, is, to render his reader not a talkative, or a ſpeculative, "but an actual and practical philoſopher. Himſelf ſets the example; he made all "the experiments he poſſibly could upon natural bodies, and communicates them "with all deſirable candour and fidelity." He then obſerves [b], that Mr. *Boyle* reſtrains not the buſineſs of experimenting to any particular branch of ſcience, but applies it, in full latitude, to all the elements, and all the bodies they mix with, or go to compoſe; and that the air, earth, fire, and water, are all ſcrutiniſed and tortured by experiments to confeſs their natures, offices, uſes, and the wiſdom and deſign of their creation. That he greatly opens and dilates the mind; gives us noble and generous thoughts of nature, and of our own abilities; manifeſts, that even deſperate things may be attempted with ſucceſs; and ſhews no quarter to the lazy, indolent temper of thoſe, who, from ſhallow notions, and a want of having ſeen the powers in natural bodies, are for diſcouraging all new and grand undertakings; and uniting the knowledge of the philoſopher with all the freedom and addreſs of the gentleman, he renders the moſt daring projects promiſing and advantageous. That he will never allow us to conſider the world as a rude heap of dull inactive matter; but convinces us, that it is a grand and noble machine, continually actuated, informed, and governed by a moſt wiſe and beneficent being, who keeps all the parts thereof in motion, and makes them act upon one another, according to certain laws. "Then bringing "us acquainted with theſe laws, he enables us to make uſe of the ſame ſtratagems "and contrivances which nature herſelf employs; which ſurely are capable of per-"forming the greateſt things when rightly applied; that is, when applied as Mr. "*Boyle*, by his own example and experience, directs us. But he ſhews alſo, that in "order to make the moſt advantageous uſe of theſe powers, a general knowledge "of nature is required. Without large and comprehenſive views, without being well "verſed in mechanical arts, and the ſeveral branches of natural knowledge, ſo as to "make one aſſiſting to another, and all in their turn conſpire to the ſame end, Mr. "*Boyle* encourages no man to be a projector [c]." He tells us [d], that Mr. *Boyle* is particularly cautious to guard his reader againſt every operation that might any ways prove dangerous or hurtful; and to prevent his impoſing upon himſelf by any ſuperficial obſervation, any fallacious or contingent experiment, whether made in his own perſon, or delivered by others. And from an attentive peruſal of his admirable writings, Dr. *Shaw* declares, that in general no author appears to have been more in-

[a] P. 4. 2d edit. *London* 1738, in 4to. [b] Ibid. p. 6. [c] Ibid. p. 8. [d] Ibid. p. 9.

quiſitive

quiſitive into the foundations and evidence of the narratives he receives from others, or to have ſhewn greater ſagacity and induſtry in diſcovering their weak ſides; always frankly propoſing them, as he received them, to be confirmed or overthrown by farther ſearch and future diligence. For proof of this he appeals to the manner wherein Mr. *Boyle* delivers himſelf upon what are or were eſteemed the moſt exceptionable things in his writings; the ſpecific virtue of the Peruvian bark, rue-leaved whitlow graſs, *ens veneris*, *oſteocolla*, the *ens primum* of balm, *Butler*'s ſtone, the ſympathetic powder, the weapon ſalve, the alkaheſt of *Paracelſus*, the *virgula divina*, the tranſmutation of metals, projection, or the philoſopher's ſtone, *&c.* and he deſires, that Mr. *Boyle* may be compared herein with the moſt approved and judicious authors who have treated of the virtues of remedies, the ſecrets in chemiſtry, and the powers of nature. No doubt (ſays he) Mr. *Boyle* may appear to give more into the belief of ſome extraordinary things than the vulgar, or perſons unacquainted with experimental philoſophy, and the immenſe powers and ſtrange operations of nature and art; but the queſtion is, whether he has here gone further than his evidence carried him. He relates alſo, upon his own knowledge, many things very aſtoniſhing and incredible to minds unprepared by philoſophy to entertain them. But he was ſo happy, as to have ſeen with his own eyes much more of the powers of nature and art, than can eaſily be imagined. And if we conſider the numerous phænomena, which, in a very long ſeries of years ſpent upon natural inquiries, moſt of them ſucceſsful, muſt have been preſented him, by the proceſs of his laboratory, the experiments of the pneumatic engine, his mechanical, optical, hydroſtatical, and other kinds of trials; the correſpondence he happily cultivated with the whole learned world, and particularly with the chemiſts of all nations; the various ſecrets he purpoſely kept to exchange for theirs; we ſhall ſee reaſon enough to perſuade us, that he might well be enabled to deliver things that ſhould ſound odd, ſtrange, and ſhocking to vulgar ears. To render the moſt innocent fluids poiſonous, without altering their colour, taſte, ſmell, or any obvious quality; utterly to eraſe the hand-writing in a ſheet of paper or parchment, without at all unfitting either for ink; to make a baſe metal paſs even upon goldſmiths for gold; not to mention the more ſurpriſing and ſhocking experiments of the like ſtamp, are things that can hardly be thought poſſible or practicable by the vulgar; yet ſome chemical philoſophers could perform them with eaſe. But there are ſome philoſophers, and even chemiſts, who, if you ſhould tell them, that Mr. *Boyle* had a certain liquor with which he could convert gold into ſilver; they would beg to be excuſed believing ſo ſtrange a thing. And why? Merely becauſe they never ſaw the like performed, and had tied themſelves down to a narrow ſtraitlaced philoſophy, that would not ſuffer them to look abroad into what nature and art can produce. *But if Mr.* Boyle (ſays Dr. *Shaw*) *has actually done the thing, and others, men of veracity, have aſſured him they had done it too; is he credulous for believing it? Thoſe rather were ſo who, without ſufficient evidence, raſhly concluded the thing impracticable.* The ſame writer then obſerves[e], that ſince Mr. *Boyle* aſſures us, and gives us ſufficient grounds to believe, that he has rendered ſilver, gold, fixed alkalies, *&c.* volatile; converted acids into alkalies; produced light from a deſpicable dark ſubſtance; immediately reſtored fetid and corrupted water, by precipitating from it a ſubſtance not fetid; diſſolved the hardeſt bodies by innocent and potable liquors without heat; produced cold by a mixture of warm bodies, heat by a mixture of cold ones; warmed cold liquors by ice; inſtantly froze ſpirit of wine; converted that ſpirit into water,

[e] Ibid. p. 10.

water and fixed alkalies into earth; and performed numberless other surprising things, that appear so many contradictions among the unskilful; may he not, with good reason, speak of them as feasible, and proper to be tried by others? Or may he not, upon the strength of human testimony, upon the relation of his friends and acquaintance, believe, that other men have done the same, or even greater things than these, though himself was not an eye-witness of them? If, after having made his liquid *phosphorus* in *England*, he should be assured, that another chemist had made a solid one in *Germany*; if, after having invented his air-pump, he should have heard, that many unsuspected discoveries were made by means of it; if having converted gold into silver, eye-witnesses should inform him, that silver has been converted into gold; or if having himself prepared a menstruum almost as strange as the alkahest, could he be reproached with credulity for believing, that *Paracelsus* or *Helmont* might possibly possess it in greater perfection; or that the other things were done by others? "This, "surely, *says Dr.* Shaw[f], is not unphilosophical or irrational. Mr. *Boyle* was too "wise to set any bounds to nature; he was not forward to say, that every strange "thing must needs be impossible, because he saw strange things every day; though "he always acknowledges, and it was a common saying with him, that *those who had* "*seen them, might better believe them than those who had not.* He modestly thought, "that all we have to do is to keep our eyes open, and expect what nature and art "will, upon due application, perform. He was well aware that there are powerful "agents in the world, whose laws and manner of acting we want to be acquainted "with. A man of a different cast, or an ignorant Indian, who had never looked "abroad, and seen the common phænomena, would scarce credit the most faithful "and exact relation that could possibly be made him of the the properties and effects "of the loadstone, or of gunpowder, by persons of the greatest veracity. But his "ignorance alone is the cause of this incredulity. Propose the same things to a man "who never heard of gunpowder or the loadstone, but is otherwise a general philo-"sopher, and he will readily and thankfully yield his assent, and solicit you to shew "him the experiments." The doctor then remarks[g], that we may certainly depend upon this, that what Mr. *Boyle* delivers as an experiment or observation of his own, is related in the precise manner wherein it appeared to him: no one ever yet denied, that he was a man of punctual veracity. And what he delivers as received from other hands, if it be any thing extraordinary, the reader will find that he does it with a manifest diffidence, and in a quite different manner from that wherein he gives us facts upon his own knowledge; and never entertains it for truth, without producing something analogous or parallel to countenance it from his own observations or experience; always reserving to himself the full liberty of believing no more than he sees reason for. And what can a man do more than this, to avoid the imputation of credulity? On the contrary, do not those who, without evidence, and merely from common report, believe Mr. *Boyle* to have been credulous, give us a remarkable instance of their own credulity? Wherever he believes more than other men, it is because he had more reason for it than they. It is the general way indeed, when a strange thing is related, either positively to deny it as impossible, or directly to entertain it as a truth. This is making short work of it. But Mr. *Boyle* possessed in a great degree that noble faculty of suspending his judgment, till by all the enquiries he could any way make he received fuller information; and then the thing he found would determine itself.

[f] Ibid. p. 11. [g] Ibid. p. 12.

DR.

DR. *Shaw* obſerves next[h], that Mr. *Boyle* is altogether taken up with the objects around us, and of which we are or may be put in poſſeſſion; though we remain ignorant of their properties, virtues, and uſes, till he calls them out, ſets them before us, and ſurprizes us with a ſight of our own ignorance of things ſo near us, ſo momentous in themſelves, and ſo neceſſary to our well-being and the true enjoyment of life. There is no profeſſion or condition of men but may be benefited by his diſcoveries. As he had a wonderful comprehenſive genius himſelf, he has improved every part of natural knowledge, and the world is more obliged to this ſingle man, than to a thouſand vulgar philoſophers taken together. It is certain, that he laid the foundations of almoſt all the improvements which have been made ſince his time in natural philoſophy; and actually himſelf performed abundance of thoſe very things, and perhaps in a much better manner too, whereby ſeveral famous men have gained a reputation in putting them off for their own diſcoveries. A very fine collection of uſeful knowledge, publiſhed as the works of a foreign Society, bears a remarkable teſtimony to this truth. The mechanic, the merchant, the ſcholar, the gentleman, all are benefited by Mr. *Boyle*. He ſhews us trades in a new light, and makes them, what they really are, a part of natural philoſophy; and conſidering them accordingly, reveals ſome of their myſteries; all along advancing proper means to encourage, promote, and multiply the arts themſelves. The goldſmith, the lapidary, the jeweller, the refiner, the ſtone-cutter, the dyer, the glaſs-maker, artizans of all kinds, will from him receive the beſt informations as to the working, managing, and employing to advantage their various commodities, materials, engines, and inſtruments. The huſbandman and the diver are here inſtructed in their arts; and the mineraliſt, the miner, and aſſayer, to find and ſeparate their ore to the greateſt profit; to increaſe the quantity; to meliorate, improve, and enrich their metals; to purify and fine them, and accurately to diſtinguiſh the genuine and pure from the adulterate, baſe, and counterfeit. The architect and builder are ſhewn how to chooſe the beſt materials for their ſeveral purpoſes; the painter to make, to mix, and improve his colours; and no part of mankind is neglected by Mr. *Boyle*. But he ſhews a more particular regard to thoſe profeſſions, wherein the health of the ſpecies is nearly concerned. The phyſician, the anatomiſt, the apothecary, and the chemiſt, are moſt highly indebted to him. He has conſidered and improved the art of medicine in all its branches. We owe to him the beſt ways we have of diſtinguiſhing genuine drugs from adulterate; the diſcovery and preparation of ſeveral valuable medicines, with the manner of applying abundance to good advantage. He has ſhewn us the way wherein ſpecifics may act; how to judge of the wholeſomeneſs and unwholeſomeneſs of the air, water, and of places; and how to examine and make choice of mineral ſprings. In a word, there is ſcarce an art, or natural production known, but he makes ſome uſeful diſcovery or improvement in it.

HIS temper, according to Dr. *Shaw*[i], was moſt open, candid, generous, and communicative. He endeavours to make all the things he treats of plain, eaſy, and familiar. There is no deep knowledge in mathematics or algebra previouſly requiſite to underſtand him fully; no tedious ſyſtems need be read, to prepare us for reaping all the advantages of his philoſophy. He brings us at an eaſy rate acquainted with the moſt uſeful things in nature; and this all the world agrees to be the higheſt excellence in a philoſophical writer. His memory will be dear to poſterity, whilſt that of myſtical and enigmatical writers, ſuch as ſtudy to make things difficult, and envy

[h] Ibid. p. 13. [i] Ibid. p. 14.

mankind

mankind their knowledge, will meet with the disregard and contempt they deserve. What principally recommends him, and distinguishes him from the vulgar herds of chemists, naturalists, and philosophers, is this humane temper, this open, candid, generous, and beneficent disposition. He was at immense pains and charge in making his enquiries; he spared no time, no money, no diligence, in pursuing discoveries for the public advantages, without any sordid view to increase his own fortune, which he happily thought sufficient of itself. His beneficent and public spirit made the world a generous present of all the fruits of his labours, without the least expectation of reward from them. His soul was as great and noble as his genius was comprehensive, or his invention fruitful. And what shews him in the most amiable point of light, he was far above the selfish pleasure of being admired for a genius, or raising a reputation by his discoveries. Though he wanted no capacity or abilities to have worked up a glorious system, and erected a more pompous, ostentatious, and perhaps a more durable structure of natural and chemical philosophy than had ever appeared in the world before, he nobly despised this poor satisfaction and mean gratification, telling us plainly and expressly, that, notwithstanding all he had done, all the labour, pains, and expence bestowed in a life of natural enquiries, notwithstanding the vastly numerous and important observations and discoveries he made, he saw nothing but the first dawnings of science; has drawn only the rudiments of natural knowledge, and leaves it in charge to posterity, for their own sake, to consider him but as a beginner, and to pursue philosophical enquiries in general, without stopping to raise petty systems by the way. "Here, *concludes Dr.* Shaw [k], was a noble soul! "This the desirable character! A true philosophical mind, well seasoned with humanity, beneficence, goodness! After he had led us through all the regions of nature; "considered her various productions; shewed us their uses, and the manner of converting them to our several purposes; convinced us, that we live in a world most "wisely contrived, wherein numberless grand designs are at once carried on with unceasing variety; and manifested, that all the beings, and all the bodies we know, "jointly conspire, as one whole, in bringing about the great ends of nature; he bids "us not stop here, but leaves us full of assurance, that the further we enquire into "the works of the universal architect, the more beauty and harmony, the greater use "and satisfaction we shall find among them; and this as long as the frame of the "world endures. These noble, manly, and generous notions, are what the reader "will find inculcated through the philosophical writings of Mr. *Boyle*; whose principal aim and great delight it was to benefit mankind by them."

[k] Ibid. p. 15.

APPENDIX

APPENDIX

TO THE

LIFE of the Honourable *Robert Boyle*.

NUMBER I.

The Charter of the Corporation for propagating the Gospel in *New England* and the Parts adjacent in *America*, of which Mr. *Boyle* was the first Governor. *Life*, p. lxviii.

CHARLES the Second by the grace of God, king of *England*, *Scotland*, *France* and *Ireland*, defender of the faith, *&c.* To all, to whom these presents shall come, greeting. Whereas by the several navigations, discoveries, and successful plantations of divers of our loving subjects of this our realm of *England*, the empire and dominion of us and our royal progenitors and predecessors, hath, by the blessing of Almighty God, been augmented and enlarged, as well upon the main land and continent of *America*, as upon several islands and promontories thereof, and the trade and commerce between *England* and those colonies and plantations hath of late years been very much increased, and by reason thereof, and of the pains and industry of certain *English* ministers of the Gospel, and others residing in or near our colonies and plantations in *New England*, who having attained to speak the language of the heathen natives in those parts, have, by their teaching and instructions, brought over many of them from the power of darkness, and the kingdom of Satan, to the knowledge of the true and only God, and to an owning and professing of the protestant religion (whereof we have of late been more fully informed by the humble petition of divers ministers and others our loving subjects now residing in this our kingdom of *England*). And although a large door of hope be hereby opened to us for the glorifying of the name of Jesus Christ, and the further enlargement of his church, yet unless some due and competent provision be made to lay a foundation for the educating, clothing, civilizing and instructing the poor natives; and also for the support and maintenance of such ministers of the Gospel, schoolmasters, and other instruments, as have been, are, or shall be set apart and employed for the carrying on of so pious and Christian a work, the same may be much retarded, and a work, so happily begun, discouraged; those planters, who first began, and contributed largely thereunto, being of themselves unable to bear the whole charge thereof. And whereas we are resolved, not only to seek the outward welfare and prosperity of those colonies by putting an industrious people into a way of trade and commerce, that

 they,

they may be employed and improved, for their own and the common benefit of these our kingdoms, but more especially to endeavour the good and salvation of their immortal souls, and the publishing the most glorious Gospel of Christ amongst them; and to the end that such our loving subjects, as either have already been aiding herein, or as shall hereafter be willing to contribute hereunto, may not be discouraged in their intended charity, for want of sufficient authority and patronage from us faithfully to order and dispose all and every sum and sums of money, goods, chattels, lands, tenements, or hereditaments, that have been, or shall or may be given for the purposes aforesaid; know ye, that we, of our princely piety, and for the further propagation of the Gospel of Jesus Christ, amongst the heathen natives in or near *New-England* and the parts adjacent in *America*; and for the better civilising, educating and instructing of the said heathen natives in learning, and in the knowledge of the true and only God, and in the protestant religion, already owned and publicly professed by divers of them; and for the better encouragement of such others of them as shall embrace the same, and of them and their posterity after them, to abide and continue in, and hold fast the said profession; of our especial grace, certain knowledge, and mere motion, and for divers other good and pious causes and considerations us thereunto especially moving, do, for us, our heirs and successors, will, ordain, constitute and declare, by these presents, that there be, and for ever hereafter shall be, within this our kingdom of *England*, a society, or company, for propagation of the Gospel in *New England*, and the parts adjacent in *America*. And further, we do, for us, our heirs, and successors, will, ordain, constitute, and appoint, that our right trusty and right well beloved cousin and counsellor, *Edward*, earl of *Clarendon*, lord chancellor of *England*, our right trusty and right well beloved cousin and counsellor, *Thomas*, earl of *Southampton*, lord high treasurer of *England*, our right trusty and well beloved counsellor, *John*, lord *Roberts*, lord privy seal, our right trusty and right well beloved cousin and counsellor, *George*, duke of *Albemarle*, our right trusty and right well beloved cousin and counsellor, *James*, duke of *Ormonde*, our right trusty and right well beloved cousin and counsellor, *Edward*, earl of *Manchester*, lord chamberlain of our houshold, our right trusty and right well beloved cousin and counsellor, *Arthur*, earl of *Anglesey*, our right trusty and well beloved counsellor, *William*, viscount *Say* and *Seale*, our well beloved *Francis Warner*, alderman of *London*, *Erasmus Smith*, Esq; *Henry Ashurst*, *Richard Hutchinson*, *Joshua Woolnough*, *George Clarke*, *Thomas Speed*, *Thomas Bell*, *John Rolfe*, citizens of *London*, our trusty and well beloved *Robert Boyle*, Esq; Sir *William Thompson*, Sir *William Bateman*, Sir *Anthony Bateman*, Sir *Theophilus Biddolph*, Sir *Lawrence Bromfield*, Knights, *Tempest Milner*, *William Love*, *William Peake*, alderman of *London*, *Thomas Foley*, Esq; *Thomas Cox*, *John Micklethwait*, *Edmund Trench*, doctors in physic, and our well beloved *Charles Doyley*, *Thomas Staynes*, *John Jurian*, *William Antrobus*, *John Bathurst*, *Harman Sheafe*, *Thomas Gillibrand*, *James Hayes*, *John Benbowe*, *Lawrence Brinsley*, *Barnabas Meares*, *John Acrod*, *John Dockett*, *Edward Boscowen*, and *Martin Noell*, citizens of *London*, to be the first members and persons, whereof the said company shall consist. And that they, the said *Edward*, earl of *Clarendon*, *Thomas*, earl of *Southampton*, *John*, lord *Roberts*, *George*, duke of *Albemarle*, *James*, duke of *Ormonde*, *Edward*, earl of *Manchester*, *Arthur*, earl of *Anglesey*, *William*, viscount *Say* and *Seale*, *Francis Warner*, *Erasmus Smith*, *Henry Ashurst*, *Richard Hutchinson*, *Joshua Woolnough*, *George Clarke*, *Thomas Speed*, *Thomas Bell*, *John Rolfe*, *Robert Boyle*, Sir *William Thompson*, Sir *William Bateman*, Sir *Anthony Bateman*, Sir *Theophilus Biddolph*, Sir *Lawrence Bromfield*, *Tempest Milner*, *William Love*, *William Peake*,

Peake, Thomas Foley, Thomas Cox, John Mickletbwait, Edmund Trench, Charles Doyley, Thomas Staynes, John Jurian, William Antrobus, John Batburſt, Harman Sheafe, Thomas Gillibrand, James Hayes, John Benbowe, Lawrence Brinſley, Barnabas Meares, John Acrod, John Dockett, Edward Boſcowen, and *Martin Noell*, and their ſucceſſors, to be hereafter choſen into the ſaid company, and in ſuch manner, as is hereafter in theſe preſents directed, for ever hereafter be, and ſhall be by virtue of theſe preſents, one body corporate and politic, in deed and in name, and ſhall have continuance for ever, by the name of the company for propagation of the Goſpel in *New England* and the parts adjacent in *America*. And them the ſaid *Edward*, earl of *Clarendon*, *Thomas*, earl of *Southampton*, *John*, lord *Roberts*, *George*, duke of *Albemarle*, *James*, duke of *Ormonde*, *Edward*, earl of *Manchester*, *Arthur*, earl of *Angleſey*, *William*, viſcount *Say* and *Seale*, *Francis Warner*, *Eraſmus Smith*, *Henry Aſhurſt*, *Richard Hutchenſon*, *Joſhua Woolnough*, *George Clarke*, *Thomas Speed*, *Thomas Bell*, *John Rolfe*, *Robert Boyle*, Sir *William Thompſon*, Sir *William Bateman*, Sir *Anthony Bateman*, Sir *Theophilus Biddolph*, Sir *Lawrence Bromfield*, *Tempeſt Milner*, *William Love*, *William Peake*, *Thomas Foley*, *Thomas Cox*, *John Mickletbwait*, *Edmund Trench*, *Charles Doyley*, *Thomas Staynes*, *John Juriam*, *William Antrobus*, *John Batburſt*, *Harman Sheafe*, *Thomas Gillibrand*, *James Hayes*, *John Benbowe*, *Lawrence Brinſley*, *Barnabas Meares*, *John Acrod*, *John Dockett*, *Edward Boſcowen*, and *Martin Noell*, and their ſucceſſors, by the name of the company for propagation of the Goſpel in *New England*, and the parts adjacent in *America*, we do, for us, our heirs and ſucceſſors, fully and really create, erect, make, ordain, eſtabliſh, conſtitute, and appoint, to be one body corporate and politic, to have continuance for ever to them and their ſucceſſors. And that by the ſame name they, and their ſucceſſors, ſhall and may have perpetual ſucceſſion, and from time to time (as occaſion ſhall require) aſſemble, and meet together, in ſome convenient place, within the city of *London*, for the ends aforeſaid. And that they and their ſucceſſors, by the name of the company for propagation of the Goſpel in *New England*, and the parts adjacent in *America*, be for ever hereafter perſons able and capable in the law to purchaſe, take, have, hold, receive, and enjoy any manors, lands, tenements, liberties, privileges, juriſdictions and hereditaments whatſoever, of what kind, quality, or nature ſoever they be, ſituate and being either within our kingdom of *England*, or elſewhere, within any other of our dominions and territories to them, and their ſucceſſors, in fee and perpetuity, or for term of life, or lives, or years, or otherwiſe, in what ſort ſoever; ſo as the ſame exceed not, in lands and hereditaments of inheritance, the clear yearly value of two thouſand pounds; the ſtatute for not putting of lands or tenements in mortmain, or any thing therein contained, or any other act, or ſtatute, to the contrary, notwithſtanding. And alſo all manner of goods and chattels, ſum and ſums of money, and other things whatſoever, of what nature, or quality, ſoever they be. And alſo to give and grant, demiſe, let, aſſign, alien and diſpoſe of all or any of the ſaid manors, lands, tenements, or hereditaments, goods or chattels. And alſo to do, perform, and execute, all and every other lawful act and acts, thing and things, whatſoever, by the name of the company for propagation of the Goſpel in *New-England*, and the parts adjacent in *America*. And further, that the ſaid company, and their ſucceſſors, by the name aforeſaid, ſhall and may be perſons able and capable, in the law, to plead, and to be impleaded, to anſwer, and to be anſwered unto, to defend, and to be defended, in what court, or courts, ſoever, and before any judges, or juſtices, and other perſons, and officers of us, our heirs and ſucceſſors whatſoever, in all and ſingular actions, pleas, ſuits, plaints, matters, and demands, of what kind, nature, or quality, ſoever

they be, or shall be, in the same, and in the like, and in as ample manner and form, as any other the people of this our kingdom of *England*, or any other body, corporate or politic, within the same our kingdom, may or can have, hold, take, purchase, possess, enjoy, retain, give, grant, demise, let, alien, dispose, assign, plead, and be impleaded, answer, and be answered unto, defend, or be defended, do, perform, or execute. And that they the said company for propagating of the Gospel in *New England* and the parts adjacent in *America*, and their successors, shall and may for ever hereafter have a common seal, to serve and use, for all causes, matters, things, and affairs whatsoever, of them and their successors. And that it shall and may be lawful to and for any thirteen, or more of them, whereof the governor of the said company, for the time being, to be one, to appoint, alter, and make new the said seal, from time to time, at their wills and pleasures, as they shall think fit. And further, we will and ordain, and by these presents, for us, our heirs and successors, do declare and appoint, that there be, for the better ordering and managing of the affairs and business of the said company, and their successors, and for ever shall be, one of the members of the said company, and their successors, who shall be, and shall be called the governor of the said company; and to hold the said office and place of governor of the said company and their successors, so long as he the said governor shall well demean himself in the said office and place, and that the said governor shall have hereby power, from time to time, to summon, or cause to be summoned, any courts or meetings, of the said company, as often as occasion shall require. And we do, for us, our heirs and successors, assign, name, constitute, and appoint the aforesaid *Robert Boyle*, to be the first and present governor of the said company and their successors, to hold and exercise the said office, for so long time as he the said *Robert Boyle* shall well behave himself therein. And therefore we will, and by these presents, for us, our heirs and successors, do give and grant unto the said company for propagation of the Gospel in *New-England* and the parts adjacent in *America*, and their successors, that upon the death or removal of the said governor, for the time being (whom for evil government, frequent neglect of attendance, or any other just and reasonable cause, we will, and by these presents do, for us, our heirs and successors, declare, shall, and may, from time to time, be removed by the said company, or any thirteen of them) it shall and may be lawful to and for the said company, or any thirteen of them, as aforesaid, at their wills and pleasures, to elect and chuse any other members of the said company, for the time being, into the place of such governor, as shall be dead, or removed as aforesaid, to hold and exercise the said office, or place, whereunto he shall be so elected and chosen as aforesaid, for so long time as he shall and do respectively well demean himself therein. And also, in case of the absence of the governor, for the time being, whereby either the meeting of the said company cannot well be summoned, or held, or being summoned, cannot proceed, or act therein; that then the treasurer of the said company, for the time being, to be chosen, as hereafter is expressed, shall, and hath hereby power and authority to summon such meetings; and then any five or more of the said company, so meeting together, shall and may have power to appoint one of the said members, so meeting, to supply the place and office of the governor, for the present. And to the end also the said company may have a continual succession, and may be the better supplied, from time to time, hereafter, with fit and able persons to be members of the said company, in the place and places of such of the present members of the said company herein above named, or any others that shall hereafter be chosen to be members of the said company, and shall hereafter happen to die, or, as shall, by reason of their own affairs,

or

or otherwise, desire to be discharged, or otherwise shall be removed from being any longer members of the said company, we do likewise, for us, our heirs and successors, by these presents, give and grant unto the said company for propagation of the Gospel in *New-England* and the parts adjacent in *America*, and their successors, that it shall and may be lawful to and for the said governor, for the time being, and company, and their successors, or any thirteen or more of them, whereof the governor, for the time being, to be one, to discharge or remove any such person or persons, from being any longer member or members, of the said company as aforesaid; and by like order, at their wills and pleasures, to admit into the said company, in the place of any of the said member or members, that shall be dead or removed, as aforesaid, any other, or so many, fit and discreet person and persons, as they shall see cause; so as the number of the said person and persons, to be at any time hereafter newly admitted, and to be members of the said company, together with the then remaining members of the said company, for the time being, do not in the whole exceed the number of five and forty persons. And further, we will and ordain, and, by these presents, for us, our heirs and successors, do give and grant unto the said company for propagating of the Gospel in *New-England* and the parts adjacent in *America*, and their successors, that they, or any thirteen or more of them (whereof the governor of the said company, for the time being, to be one) shall and may elect and chuse one or more fit and able person and persons, member or members, of the said company, to be treasurer or treasurers, of the said company, for the time being; and one fit person to be clerk of the said company, to write and serve for the affairs of the said company and their successors; and all and every such other fit person and persons, and officers, as they shall find meet and necessary, to be serviceable and useful to them, in such matters and affairs, touching and concerning the said company and their successors, as he, or they, shall be employed in, and as they shall see cause, to allow them such salaries and allowances, in execution of the said respective places, as they shall think meet. And the said treasurer and treasurers, clerk, and all other officers, as aforesaid, to be so chosen by them, for reasonable and just cause, to displace and remove out of their place or places, and other meet persons, in their or any of their places, at the pleasure and discretion of the said company, and their successors, or any nine or more of them, so met and assembled, to chuse and elect. And we do likewise, for us, our heirs and successors, by these presents, further give and grant unto the said company, and their successors, that the said company, and their successors, or any nine or more of them (whereof the governor of the said company, for the time being, to be always one) shall and may have full power and authority, by virtue of these presents, to make, ordain, constitute, appoint, and set down, from time to time, such reasonable acts, orders, instructions, and constitutions, in writing, as to them, or any nine, or more of them (whereof the governor of the said company, for the time being, to be always one) shall seem fit, good, wholesome, profitable, honest, necessary and convenient, according to their sound discretions, for the support of the said company and their successors; and for directing how and in what manner the said governor, treasurer, and treasurers, clerk, and other officers and persons, for the time being, trusted, and to be trusted and employed by the said company and their successors, shall and ought to demean, bear, and carry themselves in their offices, places, and trusts, respectively; and for and concerning the managing, ordering, and disposing of all and every the manors, lands, leases, tenements, hereditaments, goods, chattels, money, or stock of the said company and their successors, and for and concerning all and every other matters and things, in-

cident unto, or that shall or may concern the same, or any other the matters, or affairs, of the said company and their successors; and the same acts, orders, instructions, and constitutions, so made, and to be made, to revoke, alter, or change, and others, or new ones, to agree, make, ordain, constitute, appoint, and set down, in writing, as aforesaid, as they, or any nine or more of them, as aforesaid, shall see cause; so as the said acts, orders, instructions, and constitutions, or any of them, be not repugnant or contrary to the laws and statutes of *England*, but thereunto conformable and agreeable. Nevertheless, our intent and meaning is, that the governor of the said company, for the time being, with any five or more of the other members of the same, shall have power, from time to time, upon all emergent occasions, to allow of all incident charges, for and touching the meetings of the said company, and other necessary expences, for the better managing of the affairs of the said company. And to the end, that the yearly revenues, issues, and profits of all and every the manors, lands, leases, tenements, and hereditaments, and also the goods, chattels, money, and stock, of the said company, and their successors, may, from time to time, be faithfully laid out, disposed, employed, and applied, for the promoting and propagating of the Gospel of Christ unto and amongst the heathen natives, in or near *New-England* and parts adjacent in *America*; and also for civilizing, teaching and instructing the said heathen natives, and their children, not only in the principles and knowledge of the true religion, and in morality, and the knowledge of the *English* tongue, and in other liberal arts and sciences; but for the educating and placing of them, or their children, in some trade, mystery, or lawful calling. we do ordain, and for us, our heirs and successors, do, by these presents, give and grant full power and authority to the said governor, or any thirteen or more of the said company, for the time being, under the common seal of the said company and their successors, from time to time, to nominate, constitute, and appoint, such and so many fit and meet person and persons, residing in or near any of the colonies or plantations in *New-England* aforesaid, and parts adjacent in *America*, to be commissioners for and on the behalf of the said company and their successors, to treat, contract, and agree with such ministers, schoolmasters, and others, residing, and to reside in any the parts aforesaid, for such salaries, allowances, and recompences, to be from time to time made, given, and paid to them, and every of them, for their labour, pains, and industry, to be taken by them and every of them, in the duties and employments aforesaid. And also to treat, contract, and agree with any other person or persons, there, for clothes, books, tools, implements, and other necessaries, for the civilizing, employing, educating, or placing out any of the said natives, or their children, that shall own and profess the protestant religion, in English families, or with and under English masters there, or otherwise, in such manner, as they, the said commissioners, in their good discretions, shall, from time to time, think fit, and to content, pay, and satisfy all such contracts, bargains, and agreements, and all salaries, wages, and allowances, to such ministers, schoolmasters, and officers, as they shall so contract and agree with and for the clothing and apparelling of any of the said natives, or their children, and for books, tools, implements, and other necessaries, for them, and for educating or placing them, or any of them, with, or under, any English master or masters, there, in any trade, mystery, or lawful calling, out of such monies, goods, and chattels, as shall be, from time to time, sent, or made over, unto the said commissioners, so to be appointed as aforesaid, or any of them, out of *England*, or any other the dominions of us, our heirs and successors, by the said company and their successors, unto *New-England* aforesaid, or any the

the parts adjacent in *America*, as aforesaid; and also to do, perform and execute, all and every other act and acts, matters and things, which shall, or may, any way tend or conduce to the ends aforesaid, in such manner, and according to such orders and instructions, as the said commissioners, so to be employed and intrusted, shall, from time to time, receive from the said governor, or any other nine or more of the said company, for the time being; which commissioners, so to be appointed, shall, from time to time, give an account, in writing, of their proceedings herein, so often as they shall be thereunto required, by the said governor, or any other nine of the said company. And that a supply of foreign coin or monies, according to the abilities of the said company and their successors, may not be wanting, to be employed for and towards the ends and purposes aforesaid, we do, by these presents, for us, our heirs and successors, give and grant free licence, and full power and authority to the said company for propagating of the Gospel in *New-England*, and the parts adjacent in *America*, and their successors, by direction, from time to time, of the governor, and any five or more of the said company, for the time being, to ship and carry, or cause to be shipped and carried, in any English ship or vessel, within any the ports of this our kingdom of *England*, any quantity or quantities, of Spanish, or other foreign silver coin, not exceeding in the whole, in any one year, the value of one thousand pounds, without any charge or custom, for the same, so as, before such shipping, the same, or any part thereof, to be so carried as aforesaid, the quantity and quantities, and the value and values, of the said foreign coin, so to be shipped and carried, be first duly entered in the custom-house of us, our heirs and successors, of such our port or ports, of this our kingdom of *England*, where the said foreign coin shall be so shipped, to be carried to *New-England* and parts adjacent in *America*, for any the ends or purposes abovementioned; any law, statute, act, ordinance, provision, proclamation, or restraint, to the contrary, in any wise, notwithstanding. And to the end, that what the said company shall be seized, interested, or possessed of, may, from time to time, be faithfully improved, applied, and disposed, for the ends, intents and purposes herein before declared, and for the necessary affairs and business of the said company and their successors only, and no other, we will, and, by these presents, for us, our heirs and successors, do, require and ordain, that the said company and their successors do yearly, and every year (if they shall be thereunto required) by and upon the order and warrant of the chancellor, or keeper of the great seal of *England* of us, our heirs and successors, and of the treasurer of *England* of us, our heirs and successors, and of the chief baron of the court of exchequer of us, our heirs and successors, at *Westminster*, for the time being, or any two of them, make, deliver, and declare, a true and perfect account, before the said chancellor or keeper of the great seal, treasurer and chief baron, for the time being, as aforesaid, or any two of them, of all and every the goods, chattels, and stock of the said company and their successors, and also of the rents, issues and profits of all and every the manors, lands, leases, tenements, and hereditaments, and also of all and every sum and sums of money, received, issued and paid, by or for the use of the said company and their successors; which said account and accounts, as also the account of all and every person and persons heretofore employed for the receiving or disposing any rents, profits, or sums of money, limited or appointed for propagating the Gospel in *New England*; we do, for us, our heirs and successors, authorise and require the said chancellor, or lord keeper, treasurer, and chief baron, for the time being, or any two of them, to hear, determine, and declare; and, if they shall find just cause, fully to ratify, confirm, and allow of, all and every the receipts, payments,

payments, and disbursements, in every such account and accounts, to be continued, made, and applied to or for the uses, intents and purposes aforesaid; and after such declaration and allowance of every such account and accounts, to sign the same, under their or any two of their hands, and to deliver, or cause to be delivered, the said account and accounts, so to be declared, determined, and allowed of, unto the remembrancer of us, our heirs and successors, of the said exchequer, for the time being, to remain on record, in the custody of the said remembrancer, for our use and information in the premises, without any fee or reward, to be given, or paid, by the said company and their successors to the said remembrancer, or any other person or persons, for the receiving and safe keeping of the said account and accounts, or any of them, or otherwise than for the entry and writing thereof. And lastly, we will, and, by these presents, for us, our heirs and successors, do grant unto the said company for propagating the Gospel in *New-England* and the parts adjacent in *America*, and their successors, that these our letters patents, or the inrollment, or exemplification of the same, and all and every the matters and things, powers, licences, and authorities, in the same contained, shall be, from time to time, good, valid, sufficient, and effectual in the law, in and by all things, according to the true intent and meaning thereof; and shall be construed, adjudged and taken, as well in all our courts within *England*, or elsewhere, most beneficially and largely, and for the benefit and advantage of the said company and their successors, any law, statute, act, ordinance, provision, proclamation, or restraint, to the contrary heretofore had, made, ordained, provided, or published, or any other matter or thing whatsoever to the contrary in any wise notwithstanding. In witness whereof we have caused these our letters to be made patent; witness ourself, at *Westminster*, the seventh day of February, in the fourteenth year of our reign.

By writ of privy seal.

HOWARD.

NUMBER II.

An exact copy of the last will and testament of the honourable *Robert Boyle*. *Life*, p. cxxxiii.

IN the name of God, Amen. I *Robert Boyle*, of *Stalbridge*, in the county of *Dorset*, Esq; youngest son of the late right honourable *Richard*, earl of *Corke*, deceased, being, God be praised, of good and perfect memory, and taking into due and serious consideration the certainty of death, and the uncertainty both of the time and manner of it; being likewise desirous, when I come to die, to have nothing to do but to die Christianly, without being hindered by any avoidable distractions from employing the last hours of my life in sending up my desires and meditations before me to heaven, do, this eighteenth day of July in the third year of the reign of our sovereign lord and lady *William* and *Mary*, by the grace of God King and

Queen of *England*, *Scotland*, *France*, and *Ireland*, defenders of the faith, *&c.* and in the year of our Lord God one thousand six hundred ninety and one, make and ordain this my last will and testament in writing, in manner and form following.

FIRST and chiefly, I commend my soul to Almighty God, my Creator, with full confidence of the pardon of all my sins in and through the merits and mediation of my alone Saviour Jesus Christ; and my body I commit to the earth, to be decently buried within the cities of *London* or *Westminster*, in case I die in *England*, without escutcheons, or unnecessary pomp, and without any superfluous ceremonies, and without the expence of above two hundred and fifty pounds, and also without being unnecessarily dissected or disembowelled; and if it shall appear necessary, that my body be opened, then my will is, that it be performed very privately, and in the presence of none but the physician, or chirurgeon, and his attendants, and of my domestics, or very near relations.

AND as touching my temporal estate, wherewith God of his goodness hath been pleased to endow me, I dispose thereof in manner and form following; that is to say;

I GIVE and bequeath unto my dear sister, the lady *Catharine*, viscountess *Ranelagh*, a small ring, usually worn by me on my left hand, having in it two small diamonds, with an emerald in the middle; which ring being held by me, ever since my youth, in great esteem, and worn for many years for a particular reason, not unknown to my said sister, the lady *Ranelagh*, I do earnestly beseech her, my said sister, to wear it in remembrance of a brother, that truly honoured, and most dearly loved her.

ITEM, I give to the said lady *Ranelagh* all my manuscripts and collections of receipts, whether of my own hand-writing, or others, unless I do otherwise dispose of all or any of them before my decease; beseeching her to have a care, that they or any of them come not to the hands or perusal of any, to whom she thinks, that if I were alive, I should be unwilling to have them communicated. And I do likewise devise unto her the sum of 300 *l.* (and all interest) which I formerly lent her upon enlarging the dwelling-house in the *Pall-Mall*, which she now lives in.

ITEM, whereas by my decease the greatest part of my intailed lands in *Ireland*, will come to my dear and eldest brother, *Richard*, now earl of *Burlington* and *Corke*, I do hereby give and bequeath unto him a Sardonyx seal ring, which I usually wear on my little finger; beseeching him to accept of it, and wear it for my sake, as a testimony of my unfeigned affection, and of my sense of his great kindness and many favours towards me; and beg of him to believe, that it doth not afflict me, that I have not children of my own to inherit my intailed lands, since they are, by that defect, to return to him, the truly honourable head of our house and family.

ITEM, I give and bequeath unto my dear brother, the lord viscount *Shannon*, the best watch I shall die possessed of, to put him in mind of my constant kindness and affection, which I endeavoured to express by my voluntary yearly expence in keeping up the manor house of *Stalbridge*, without intending to live in it, for his sake.

ITEM, I give and devise unto my worthy friend *Richard Newman*, of *Westminster*, in the county of *Middlesex*, Esq; a piece of plate of the value of 10 *l.* sterling, as a testimony of my remembrance of his kindness, which I desire him to continue by affording his assistance, if there be occasion, to my executors hereafter named.

ITEM, I give and bequeath unto my faithful friend *John Nicholls*, Gent. steward of the courts of my manor of *Stalbridge*, a piece of plate of the value of 10 *l.* sterling,

to

to be within the ſpace of one year next after my deceaſe delivered unto him by my executors, as a teſtimony of my remembrance and affection; hereby making him the ſame earneſt requeſt, that I have hereby made to Mr. *Richard Newman*.

ITEM, I give and deviſe unto the right reverend father in God *Gilbert*, lord biſhop of *Saliſbury*, my great Hebrew bible with ſilver claſps, as a ſmall token of my great reſpect for him, and ſenſe of his favours to me.

ITEM, I give and deviſe unto *Thomas Smith*, now my ſervant, thirty pounds, in caſe he be with me, or in my ſervice, at the time of my death; but in caſe of his departure or removal from me, before that time, then my will is, that he ſhall have 20 *l.* and no more.

ITEM, whereas my ſervant *John Warr* the younger is indebted unto me in the ſum of 50 *l.* by bond or bill obligatory, my will is, that his ſaid debt be remitted and diſcharged.

ITEM, whereas I had ſet apart, among other things, the ſum of 400 *l.* for certain pious uſes; and whereas his late Majeſty King *Charles* the ſecond having, by his ſpecial grace and favour, without my ſeeking, or knowledge, been pleaſed to conſtitute me governor of the corporation for propagating of the Goſpel amongſt the heathen natives of *New-England* and other parts of *America*, hath thereby given me opportunity to diſcern that work to be unqueſtionably pious and charitable; and whereas I have given and paid the ſum of three hundred pounds towards that piety; I do hereby give and deviſe the ſum of one hundred pounds more to the ſaid corporation (though by reaſon of ſickneſs and infirmity I have reſigned the office of governor) to be ſet aſide and employed as a ſtock for the relief of poor Indian converts, which I hope will prove of good effect for the advancement of the pious work, for which they are conſtituted, and in which I heartily pray him, whoſe glory the work itſelf tends unto (and I hope the perſons intruſted with it aim at) to give them a proſperous ſucceſs.

ITEM, to the Royal and learned Society for the advancement of experimental knowledge, wont to meet at *Greſham* college, I give and bequeath all my raw and unprepared minerals, as ores, marchaſites, earths, ſtones (excepting jewels) *&c.* to be kept amongſt their collections of the like kind, as a teſtimony of my great reſpect for the illuſtrious Society and deſign, wiſhing them alſo a moſt happy ſucceſs in their laudable attempts to diſcover the true nature of the works of God, and praying, that they and all other ſearchers into phyſical truths may cordially refer their attainments to the glory of the Author of Nature, and the benefit of mankind.

ITEM, to my worthy friend and phyſician, Sir *Edmund King*, Knt. I give and bequeath a ſilver ſtandiſh of the value of 30 *l.* ſterl. as an unfeigned, though ſlight teſtimony of the juſt eſteem I have of his worth and ſkill, and the ſenſe I have of his particular care of me and kindneſs for me.

ITEM, I give to Mr. *Robert Hooke*, now profeſſor of mathematics in *Greſham* college, my beſt microſcope and my beſt loadſtone, which I ſhall have at the time of my death.

ITEM, I give and bequeath unto Mr. *John Dwight* and Mr. *John Whittane*, once my ſervants, each of them a ring of 5 *l.* price.

ITEM, I give and bequeath unto Mr. *Chriſtopher White*, once my ſervant, 5 *l.* ſterling.

ITEM, I give and bequeath unto Mr. *John Milne* and Mr. *Hugh Gregg*, once my ſervants, each of them a ring of 6 *l.* price.

ITEM,

ITEM, I give unto *Nicholas Watts*, my bailiff of the manor of *Stalbridge*, a piece of plate of the value of 10*l.* or 10*l.* sterl. in money, in lieu thereof, to be at his own choice.

ITEM, I give unto my aforesaid servant *John Warr*, 40*l.* and the one half of my wearing apparel and linen, in case he be with me at the time of my death; but otherwise to have no share of my apparel or linen.

AND I give to *Robert St. Claier*, my servant, the sum of 15*l.* and the other half of my apparel and linen, in case he continues to be my servant at the time of my death: if he doth not, then I give to him a ring of 6*l.* price only. And to Dr. *Frederick Slare*, late my servant, a ring of 8*l.*

AND I make and ordain my said dear brother, *Richard* earl of *Burlington* and *Corke*, and my said dear sister, the lady *Ranelagh*, and *John Warr* the younger, gent. my present servant, sole executors of this my last will and testament.

ITEM, I give and devise to my said executors all my estate, right, title and interest of, in, and to a certain lease, bearing date in or about the month of made by the corporation of mines royal, or their authority, unto *William* lord viscount *Brounker*, Sir *Robert Morey*, Knt. late lord *Brereton*, and myself, of a mine or mines in or near the parish of *Ashbury* in *Cheshire*, for 41 years, or thereabouts.

ITEM, Whereas the executors of *James Watson*, late alderman of the city of *Dublin*, and *Abraham Richens*, late of the same city, merchant, or some one of them, stand indebted to me in nine hundred and sixty pounds, or thereabout, towards satisfaction whereof I have received the sum of fourscore pounds, or somewhat more, for a house in *Dame-street*, *Dublin*, sold by me to my once worthy friend, Sir *John Temple*, Knt. deceased, formerly master of the rolls; which house was made over unto me, amongst other things, towards satisfaction of the said debt, upon a decree which I obtained in the then called the court for the administration of justice sitting at *Dublin* aforesaid. And whereas the said debt of 960*l.* or thereabouts, saving and except the 80*l.* or somewhat more, so as aforesaid beforementioned to be received, is still due and owing; I give and devise the same, with all writings of what kind soever thereunto belonging, and the proceed of composition for the said debt, whensoever it shall happen, to be made unto my executors; and I do hereby will and declare, that all and every of my bequests aforesaid unto the said *Richard* earl of *Burlington*, the lady *Ranelagh*, and *John Warr*, my servant, jointly, as executors, and in that capacity, shall not be for their own private use, but only in trust for and towards the payment of my debts and legacies hereby devised, and towards and for performance of this my last will and testament, and to and for no other use or uses whatsoever.

AND as touching the disposition of all and singular my castles, manors, lordships, messuages, lands, tenements, and hereditaments in the kingdom of *Ireland*, which I have power to dispose of, this is the last will and testament of me the said *Robert Boyle* made and declared the day and year first above written, *viz.*

I GIVE and devise unto the right honourable *Richard* earl of *Burlington* and *Corke*, my dear nephew the honourable *Henry Boyle*, Esq; of *Castle-Martyr* in the county of *Corke*, and Sir *Robert Southwell* of *Kingsale* in the kingdom of *Ireland*, all my estate, right, title, interest, and demand whatsoever of, in, and unto the castle, town, manor, and lands, being nine plow-lands and half, or thereabouts, of *Buttevant*, and the moiety of the lands called *Buttevant* and *Rices* lands in the county of *Corke*, several years since mortgaged to my father and unto me for the sum of 2000*l.* sterling, or thereabouts, and after my father's decease leased by me under certain covenants and conditions for thirty-one years unto lieutenant colonel *Agmondesham Muschampe*, for

about the sum of six score pounds *per Ann.* and now or lately in lease to *Denny Muschampe*, Esq; and also all my estate, right, title, interest, claim, and demand whatsoever, of, in, and to all and singular the towns, lands, tenements, and hereditaments of *Rathenege*, and four pounds chief rent issuing out of the lands of *Turmore* and *Ballytramly* by the name of Five Castles, all lying in the aforesaid county of *Corke*, and all my estate, right, title, interest, and demand whatsoever of, in, and to the impropriate rectory of *Adare* alias *Athdare*, in the county of *Limerick* in *Ireland*; and also all my estate, right, title, interest, and demand whatsoever of, in, and to the castles, towns, and lands, containing two plow-lands and half (be it more or less) of *Ballydangan*, *Killcroyne*, and *Ballybrittas*, heretofore mortgaged to my said father for the sum of 1000*l.* or thereabouts, to have and to hold all and singular the said castles, manors, lordships, messuages, lands, hereditaments, and premises of *Buttevant*, *Rices* lands, *Rathenege*, and the 4*l.* a year chief rent of impropriation of *Adare*, the two plow-lands and half of *Ballydangan*, *Killcroyne*, and *Ballybrittas*, and all other the premises and sum or sums of money therefore or thereout due and payable unto me, with all my estate, right, title, interest, claim, and demand whatsoever in and to the premises, and every part, member, and appurtenances of them, every or any of them; and also all deeds, evidences, and writings whatsoever for or concerning the premises or any part thereof, to them the said *Richard* earl of *Burlington*, the honourable *Henry Boyle*, Esq; and Sir *Robert Southwell*, and their heirs and assigns for ever, as fully and in as large and ample a manner, as the premises or any part thereof are or invested in, descended, come, given, devised or bequeathed unto me the said *Robert Boyle*, upon special trust and confidence notwithstanding, and to and for the only uses, intents, and purposes in and by this will and testament limited and declared, and to and for no other use, intent or purpose whatsoever: that is to say, that they the said *Richard* earl of *Burlington*, the honourable *Henry Boyle*, Esq; and Sir *Robert Southwell* and their heirs, shall, after my death, by and with the advice of my executors, sell the said respective premises, and pay the money thereby raised, as also the profits until sale, unto my said executors, to and for the uses herein expressed.

Item, I give unto my said executors all and every such sum and sums of money, as shall be raised out of or by sale of my said lands; and also all my printed books, goods, chattels, plate, monies, jewels, and credits whatsoever, and herein before bequeathed, limited, or appointed unto them, to and for the only uses, intents, and purposes hereafter also in and by these presents limited, expressed, and declared, and to and for no other use, intent, or purpose, whatsoever: that is to say, to the intent and purpose, that the said *Richard* earl of *Burlington*, lady *Ranelagh*, and *John Warr*, their heirs, executors, administrators, or assigns, shall with what convenient speed they can, after my decease, by sale or composition, (though upon terms advantageous to the buyers) sell and make money of all the said castles, lands, mortgages, goods, and chattels, cattle, plate, jewels, and credits by this my last will and testament bequeathed unto them, or to my said trustees, *Richard* earl of *Burlington*, the honourable *Henry Boyle*, Esq; and Sir *Robert Southwell*; and shall, in some convenient time after my decease, out of the proceed of the said sale or compositions duly and fully satisfy, pay, and discharge as well all the several debts and sums of money (at present not amounting to much) which I now do, or at the time of my decease shall owe to any person or persons whatsoever, and the charges of the funeral of my body; and also all such legal engagements, as I have undertaken to perform to any person or persons whatsoever; especially to my tenants of my manor of *Stalbridge*: and from and immediately after my debts and funeral charges, and all such legacies, as I have above by

this

this my will given and bequeathed, shall be discharged and paid, and my aforesaid engagements made good; then my will is, that the rest, residue, and remainder of the proceed of the sale of, or composition for all the said lands, mortgages, and chattels hereby devised and bequeathed unto the said *Richard* earl of *Burlington*, lady *Ranelagh*, and *John Warr*, my said executors, shall be by them summed up: and that in the first place they do distribute among the poor (having therein a special regard unto the poor of the parish of *Stalbridge*, and of the parish of *Fermoy*, in the county of *Corke*, in the kingdom of *Ireland*, and other parishes in that kingdom, where any of my lands do lie) the sum of 300*l.* sterl. and next after the distribution thereof the sum of 200*l.* sterl. more amongst the much distressed persons of all qualities that have been forced or frighted out of *Ireland*, and that shall happen to be in or near *London*, *Bristol*, *Chester*, or *Liverpool*, at the time of my decease; which sums with respect chiefly to some things that occurred in a gracious deliverance, that was vouchsafed me in great distress, I think fit to charge my executors with the payment of after my decease. And also, in the next place, that after the distribution of the sums of 300*l.* and 200*l.* as aforesaid, and also after the payment of the sum of 66*l.* 4*s.* to the abovementioned Sir *Robert Southwell*, to be distributed by him amongst the incumbents, residents, and widows and children of such as have been incumbents and residents in the several parishes of which I have the impropriations in the kingdom of *Ireland*, and to such of them, and in such proportion, as the said Sir *Robert Southwell* shall think most needing and deserving of the same; that then they pay to my friends and kindred, named in the schedule to this my will annexed, the respective sums therein expressed, in the whole amounting to the sum of four hundred pounds; and after payment thereof I will and devise all the residue and remainder of my goods and chattels, real and personal, and of the money to be raised by the sales aforesaid by this my last will above directed to my said dear sister, the lady *Ranelagh*, with this desire nevertheless, and my will is, that in case the said residuary part of my said estate shall amount (as I verily believe it will by far) to more than 800*l.* sterl. over and above the legacies and bequests above devised and disposed of, that then my said dear sister do retain and enjoy, for her own use, the sum of 200*l.* sterl. over and above the 300*l.* debt above remitted to her (my intention being, that she shall be benefited in the total to the value of 500*l.* sterl. by this my last will and testament) and this legacy being from the beforementioned residuary part of my estate, then my will is, that the overplus be laid out by my said dear sister, in case she survive me, and in case of her death, by my executors, in such manner, as by my codicil or other writing under my hand I shall hereafter direct; and for want of such direction, for charitable and other pious and good uses at her or their direction. But I do chiefly recommend unto her and them the laying of the greatest part of the same for the advance or propagation of the Christian religion amongst infidels; and I hope my relations and friends will believe my intentions to be as good towards them now, as when I thought I had estate more considerable to dispose of than now I have (especially having of late years lost, by the breaking of goldsmiths and others, above 1500*l.* and since that, by the unfaithfulness of my receiver in *Ireland*, above 900*l.* and since that also by the destructive insurrections and war that hath happened in *Ireland*, the whole income for above two years last past of my estate there) and will therefore accept in good part of what legacies shall be presented unto them by my executors, considering that it is the duty of every honest man to prefer doing acts of justice before those of kindness, when he is not sure he is able to do both at once.

And whereas divers lands and tenements, by this my will above directed to be sold, were originally mortgaged estates, to the end therefore that the heirs of the mortgagers may have no just cause to complain, nor the buyers be mistaken in their purchases, I do hereby declare, direct, and appoint, that in case my cousin , the heir of Sir *Piercy Smith*, long since deceased, shall desire, within the space of one year after notice given him by my executors, to redeem the impropriate rectory of *Adare*, which was mortgaged by the said Sir *Piercy Smith* for 1000*l.* and shall not do the same, that then, upon his releasing of all his right and equity of redemption, the sum of 66*l.* shall be paid to him out of the proceed and sale of the said rectory, as a gift or legacy from me to him. But my will is, that the said sum of 66*l.* shall not be paid unto him, unless he release as aforesaid within one year after the notice aforesaid. And whereas the castle, town, and lands of *Ballydangan*, *Killcroyne*, and *Ballybrittas* aforementioned were heretofore transferred by me to my honoured cousin *Michael*, archbishop of *Armagh*, and primate of all *Ireland*, to have and to hold to the said archbishop and his heirs, from and after the determination of divers years yet to come and expire: and whereas some years since, when the said lands were ready to come into my hands, I made a promise to lease the said premises to my abovenamed dear brother the earl of *Burlington*, for all my term and interest therein under the rent of 60*l. per Ann.* or thereabouts *de claro*, I do hereby ratify and confirm my said promise, and do will and appoint, that he shall hold and enjoy the same under the rent of 60*l. per Ann.* during all the residue of my said term and interest, which shall be to expire at the time of my death.

Lastly, I do hereby earnestly desire my said executors to see this my will, and my trusts in them reposed, faithfully performed, as they hope or desire to have their own wills faithfully performed. But for as much as by reason of the uncertainties of the times it may happen (which God forbid) that my estate may meet with some further misfortunes, whereby it shall be so weakened as not to be sufficient to discharge all the legacies by this my will bequeathed; then my will is, that after the payment of 300*l.* by me devised to the poor, and the sum of 200*l.* by me devised to the much distressed persons of all qualities, *&c.* as aforesaid, mentioned to be in respect of a deliverance from a distress (which I hereby will shall be observed) that then my other legatees shall each of them be respectively abated their legacies, in proportion to what my said estate shall fall short of my said other legacies, above by me devised.

In witness whereof, I the said *Robert Boyle*, revoking and disannulling all other and former wills, testaments, and bequests, have to every sheet of this my last will and testament, contained in seven sheets of paper, set my hand, and to the last of them my seal, the day and year first above written.

Robert Boyle.

Signed, sealed, published, and declared by the abovementioned *Robert Boyle*, as his last will and testament, the day and year first abovewritten in the presence of us,

Isaac Garnier,
John Caddick,
James Oglebey,
William Johnson.

A schedule

A ſchedule in my will referred unto, for the diſpoſition of 400*l.* which I will ſhall be paid to the perſons herein after ſet down, in ſuch proportion to each perſon, as is herein expreſſed: *viz.*

IMPRIMIS, to my honoured and dear nephews, the earl of *Barrimore*, 30*l.* the earl of *Ranelagh*, 30*l.* *Charles* lord *Clifford*, 30*l.* Capt. *Robert Fitz-gerald*, 30*l.* Capt. *Henry Boyle*, 30*l.*

ITEM, to my honoured and dear nieces, the counteſs dowager of *Thanet*, 30*l.* the counteſs dowager of *Clancarty*, 30*l.* the lady viſcounteſs of *Powers-Court*, 30*l.* the lady *Frances Shaen*, 30*l.* the lady *Catherine Fitz-gerald*, 30*l.* and to Mrs. *Elizabeth Melſter*, not to make a difference between her and my other nieces in my affection, no more than there is in their relation to me, but becauſe of her peculiar circumſtances, I give 100*l.*

Robert Boyle.

THIS is the ſchedule referred to and mentioned in the fifth ſheet of this the laſt will and teſtament of the honourable Mr. *Robert Boyle*, as a part thereof, and was by his appointment annexed hereunto, and, at and before the enſealing and publiſhing this his will, ſigned by him in the preſence of us,

James Garnier,
John Caddick,
James Oglebey,
William Johnſon.

WHEREAS an act of parliament in *Ireland*, intitled, *An Act for the better Execution of his Majeſty King Charles the Second's gracious Declaration of the Settlement of his Kingdom of Ireland, and Satisfaction of the ſeveral Intereſts of Adventurers and Soldiers and others the Subjects there*, it was enacted (amongſt other things) that I *Robert Boyle*, my executors, adminiſtrators, and aſſigns, ſhould and might, for and during the term of thirty-one years, have, hold, and enjoy all and ſingular the impropriations of or belonging to the reſpective abbies late diſſolved, monaſteries, religious houſes, priories or pariſhes of *Ballytubber*, in the county of *Mayo*, *Knockmoy-Kireleulta Iran*, *alias* St. *Mary's Athenree*, and *Dunmore* in the county of *Galway*, and *Tybone* in the county of *Tipperary*, or any of them, together with all the impropriate tythes and rectories and appurtenances of the ſaid impropriations, tythes, and rectories, or belonging thereunto, which belong unto, or by that act were veſted in his late Majeſty to any of the uſes therein before mentioned, according to the tenor and effect of ſuch grant or grants as had, or then were, or then after ſhould be paſſed unto me thereof by his late Majeſty's letters patents in that behalf, I or they paying yearly for the ſame double the exchequer or crown rent reſerved thereupon in the year 1641: now I the ſaid *Robert Boyle* do hereby give and deviſe to my dear brother *Richard* earl of *Burlington* and *Corke* in *Ireland*, the right honourable *John* lord viſcount *Maſſerine*, *Clotworthy Skeffington*, Eſq; in the county of *Antrim* in *Ireland*, and Sir *Peter Pett*, Knt. their executors, adminiſtrators, and aſſigns, the fourth part (the whole in four parts to be divided) of the ſaid impropriations and premiſes in and by the ſaid act granted, limited, or appointed by me to be held and enjoyed during the ſaid term of one and thirty years upon truſt, to be by them employed and diſpoſed for ſuch good and pious uſes, as I ſhall from time to time declare or appoint it, or in default of ſuch appointment,

tions. And I do hereby will and ordain this my codicil to be a part of my last will and testament. Witness my hand and seal, this 28th day of July in the year of our Lord 1691.

Robert Boyle.

SIGNED, sealed, published, and declared by the honourable *Robert Boyle*, to be a codicil to and part of his last will and testament, in presence of us,

Isaac Garnier,
Robert St. Claire,
James Oglebey,
William Johnson.

WHEREAS since the making and executing of my last will and testament, several persons are brought to my remembrance, whom I should have made legatees in my said will, had I thought upon them before my finishing thereof; and being unwilling, that they should be wholly deprived of my intended kindness to them; wherefore I have thought fit to insert their several names and legacies in this my codicil, which I hereby declare to be a part of my said will.

IMPRIMIS, I give and bequeath unto my cousin Mrs. *Catharine Molster*, daughter of my niece Mrs. *Elizabeth Molster*, the sum of 100 *l.* to be paid unto her, when she shall have attained to her age of 21 years, or upon the day of her marriage, which shall first happen.

ITEM, I give and bequeath unto Sir *Robert Southwell* the sum of twenty pounds.

ITEM, I give and bequeath unto Sir *Henry Ashurst*, Knt. and Bart. the sum of twenty pounds.

ITEM, I give and bequeath unto Sir *William Ashurst*, Knt. and alderman of the city of *London*, the sum of twenty pounds.

ITEM, I give and bequeath unto Sir *John Rotheram*, serjeant at law, the sum of twenty pounds.

ITEM, I give and bequeath unto my servant *William Johnson*, the sum of fifteen pounds.

ITEM, I give and bequeath unto such a number of the French protestant refugees inhabiting in and about the cities of *London* and *Westminster*, as my executors shall think fit to pitch upon, and shall judge to be most necessitous, the sum of 100 *l.* to be divided amongst them, according to the discretion of my said executors.

AND whereas *Nicholas Courtney*, in the *Inner-Temple*, Esq; is and hath been for several years indebted unto me in the sum of 150 *l.* principal money, besides interest for the same during the time he hath had the said money; now in consideration, that the said *Nicholas Courtney* hath done some business for me in my life-time, I do hereby remit and fully discharge him, his executors, and administrators of and from all the interest money, which now is or shall be due unto me from him or them at the time of my decease. And I do hereby declare, that this codicil shall be added to my last will and testament, and be a part thereof. In witness whereof I have hereunto set my hand and seal, the 30th day of July, in the third year of the reign of our sovereign lord and lady, King *William* and Queen *Mary*, over *England*, *&c. Annoque Domini* 1691.

Robert Boyle.

SIGNED,

SIGNED, sealed, published, and declared by the honourable *Robert Boyle*, Esq; as a codicil to be added to his last will and testament, and to be a part thereof, in the presence of us,

Isaac Garnier,
Robert St. Claire,
Jo. Caddick.

WHEREAS I have in and by my last will and testament impowered my most honoured and dearest brother the right honourable *Richard* earl of *Burlington* and *Corke*, the honourable *Henry Boyle*, Esq; and Sir *Robert Southwell*, my trustees, to grant, bargain, sell and dispose of to the best advantage, immediately after my decease, all that the manor and lands of *Buttevant* and *Rices* lands in the county of *Corke* within the kingdom of *Ireland*, with all and singular their rights, members, and appurtenances for and during so long time and term, as there should be then to come and unexpired in the said premises, in case the same should not be redeemed; and have therein declared, that the money to be raised by the sale or redemption thereof should be by my said Trustees paid unto my executors: and having for a long time determined to make some real expressions of the great respects and kindness, which I have for and bear to my abovesaid brother, and to the right honourable *Charles* lord *Clifford* (son and heir apparent of my said brother) my honourable nephew, by making some gift or present to the off-spring of my said brother: now in prosecution of my said determination, I do hereby publish and declare, that my executors shall make an even and equal dividend of the said purchase or redemption money, when paid to them by my said trustees as aforesaid, and one moiety or half part thereof, my will is, that my executors shall pay back unto my said brother the earl of *Burlington* and *Corke*, upon special trust and confidence, nevertheless, and to the use, intent, and purpose herein after mentioned and declared; that is to say, that he the said earl shall apply, lay out, pay, and employ the same moiety or half part of the said money within three months after his receipt thereof for the use and best advantage of such one or more of the younger children now unmarried of the said lord *Clifford*, and in such proportion, if to more than one, as to him the said earl, his executors, administrators, or assigns, shall seem fit; and under this further trust, that in case he the said earl, his executors, administrators, or assigns, shall fail to settle and dispose of the same, according to the said trust, within three months after the same is paid to him by my executors as aforesaid, that then he the said earl, his executors, administrators, and assigns, shall stand possessed of the said moiety or half part of the purchase or redemption money, so paid to him as aforesaid, in trust for all the younger children now unmarried as aforesaid, of him the said lord *Clifford*, share and share alike. And as for the other moiety or half part of the said purchase or redemption money, which will remain in the hands of my executors, I do hereby publish and declare shall be employed by them to the use or uses in my said last will declared and appointed. And this codicil I do hereby declare shall be added to my last will and testament, and be a part thereof. In witness whereof I have hereunto set my hand and seal the first day of August in the third year of the reign of our sovereign lord and lady King *William* and Queen *Mary* over *England*, *&c. Annoque Domini* 1691.

Robert Boyle.

SIGNED, sealed, published and declared by the honourable *Robert Boyle*, Esq; as a codicil to be added to, and be a part of his last will and testament in the presence of us,

Isaac Garnier,
Robert St. Claire,
William Johnson,
James Oglebey.

WHEREAS I have in and by my last will and testament settled and appointed the sum of 200 *l.* sterling to be paid and distributed by my executors to and amongst such distressed Irish protestants of all qualities, as have been either forced or affrighted out of the kingdom of *Ireland* since the late calamities there, and that should happen to be in or near *London*; *Bristol*, *Chester*, or *Liverpool*, at the time of my decease: now in regard of the present exigencies and pressing necessities of several of those persons, I have thought fit to allot and give out myself as a part of that my intended the sum of 50 *l.* to be divided and distributed amongst such of them, as I do judge most necessitous for their present relief; so that now my will is, that my executors shall be obliged to pay and distribute to and amongst the said distressed Irish protestants in my last will mentioned as aforesaid, no more than the sum of 150 *l.* (being the residue thereof) for the reason aforesaid. And this my codicil I do hereby signify and declare shall in order thereto be added to and be a part of my last will and testament. In witness whereof I have hereunto set my hand and seal this fifth day of August in the year of our Lord God 1691.

Robert Boyle.

SIGNED, sealed, published, and declared by the honourable *Robert Boyle*, Esq; for and as a codicil to be added to and be a part of his last will and testament, in the presence of us,

Robert St. Claire,
Thomas Smith,
William Johnson,
James Oglebey.

WHEREAS I am justly sensible of the great trouble, that I shall give my dearest and most honoured brother and sister, the earl of *Burlington* and *Corke*, and the lady viscountess *Ranelagh*, by my having constituted and ordained them to be two of the executors in trust of my last will and testament; which, considering their age and quality, they may be no way fit to undergo; to the intent therefore, that they may be eased, as much as in me lies, from all the avoidable trouble of that office, I do hereby declare, that I have engaged my other executor in trust, Mr. *John Warr*, jun. my present servant, to take upon him more immediately the troublesome part of the execution of my said last will and testament, which he is to prosecute by and according to the direction and appointment of my other two executors, and not absent himself from the same during the term or terms herein after mentioned, for which he shall happen to be employed in the execution of the said trust, by travelling above one day's journey from *London*, unless it shall be on extraordinary occasion, and with the consent of my other two executors. In consideration whereof I do hereby give, devise, bequeath, and settle upon him, the said *John Warr*, the yearly sum of 40 *l.* for his care and pains in the execution of the said trust; which, my will is, shall be continued to him as a yearly salary for the space of three years from the time of the proving my said will, provided the discharge of my said trust require so long time; other-

otherwise my will is, that this salary shall be continued but for the space of one year, or at most but in proportion for such further time, as shall be necessary for the discharge of the then remaining part of the said trust. And my further will is, that besides the salary abovementioned, my said executor *John Warr* shall be allowed all such necessary and reasonable charges, as he shall from time to time expend or be put unto in the performance and execution of the said trust. Lastly, my will is, that this codicil shall be added to and be a part of my last will and testament. In witness whereof I have hereunto set my hand and seal the eleventh of September in the third year of the reign of our sovereign Lord and Lady *William* and *Mary* by the grace of God of *England*, *Scotland*, *France*, and *Ireland*, King and Queen, defenders of the faith, *&c.* and in the year of our Lord God 1691.

Robert Boyle.

SEALED, published, and declared by the honourable *Robert Boyle*, Esq; for and as a part of his last will and testament, in the presence of us,

Robert St. Claire,
Thomas Smith,
Jo. Caddick,
William Johnson.

WHEREAS in and by my last will and testament bearing date on or about the 18th day of July last past I did constitute and appoint my dearest sister *Ranelagh* one of my executors in trust: and whereas it hath pleased God some few days since to take unto himself my dear sister; I do therefore in her stead and place hereby make, ordain, and appoint my very loving friend Sir *Henry Ashurst* of the parish of St. *Sepulchre*'s in the county of *Middlesex*, Knt. and Bart. to be one of the executors in trust of my said last will and testament. And I do hereby desire and impower my said executor, together with my other two surviving executors in trust in my said last will named and appointed, to take special care to discharge the trusts by me therein in them reposed, and more especially that part of my will, that I had particularly recommended to the care of my dearest sister *Ranelagh* deceased. And I do hereby give and bequeath unto the said Sir *Henry Ashurst* the sum of 20*l.* to be laid out by him in a piece of plate. And I do hereby confirm all the appointments of my said last will and testament, excepting only what therein relates to my said deceased sister *Ranelagh*, which I do hereby revoke. And I do hereby also confirm all other codicil or codicils, that have been made by me since the date of my abovesaid last will and testament. In witness whereof I have hereunto set my hand and seal the 29th day of December, in the third year of the reign of our sovereign Lord and Lady King *William* and Queen *Mary* over *England*, *&c.* *Annoque Domini* 1691.

Robert Boyle.

SIGNED, sealed, published, and declared by the honourable *Robert Boyle*, Esq; as a codicil to and a part of his last will and testament, in the presence of us,

Isaac Garnier,
Robert St. Claire,
Tho. Smith,
William Johnson,
James Ogleby.

NUMBER III.

Letters relating to the edition of the Irish Bible printed and distributed at the expence of Mr. *Boyle*. See *Life*, p. cxxxix. and Works, Vol. VI. p. 592——610.

I.

Bishop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, Aug. 4, 1680.

THE inclosed, from my good friend Dr. *Sall*, should have been (as you find) sooner with you, but that I made bold to reserve the passing it to you by my good lord the earl of *Ranelagh*, as now; designing in both, to be represented, and so better known to you, than by a bare letter from a stranger, I mean myself.

By Dr. *Sall* I understand, Sir, your zeal for having this poor ignorant people, the *Irish*, informed in the true knowledge of God in their own language, by reprinting the New Testament, and Common Prayer in Irish, these being now out of press, and the books of them extant hardly found.

In addition whereunto is herein offered, the publishing of the Old Testament also in Irish, together with the Psalms in metre fitted to our ordinary church tunes, translated by means and procurement of that learned and pious prelate, Dr. *Bedell*, sometime bishop of *Kilmore* in *Ireland*, and provost of *Trinity* college in *Dublin*; whose heart God had stirred up thereunto zealously. This was by that learned bishop himself compared with the original, and finished anno 1640. After whose death it came into mine hand, where it now continues in sheets, and MSS. This compleating the bible in Irish, and added to what is already printed, would be a work greatly to God's glory, in bringing, by his grace, many from darkness to light, and of deserved praise to the happy undertaker.

I had once thoughts of representing this to our next parliament here, hoping for public allowance, and supplies thereby, toward it. But in discourse with some concerning it, I found it almost a principle in their politics, to suppress that language utterly, rather than in so public a way to countenance it. This occasioned what I have sometime written of that, in an epistle to the earl of *Essex*, then lord lieutenant of *Ireland*, prefixed to a small tract, which is therefore herewithal for your perusal; accompanied with another on another occasion, both under, Sir, your censure.

As for the Irish letters stamped for the first printing here of the Common Prayer, and New Testament; they had passed from hand to hand of many his Majesty's printers in *Dublin* successively, until by covetousness of one, into whose hands they fell unhappily, they were by the Jesuits gotten away, and are now at *Doway*, for Irish prints; some of which I have seen, to my grief, sent hither, further corrupting the people. So as there is nothing left of what was formerly, towards the printing of Irish here, if it should be required.

I have dealt with our present provost of this college of *Dublin*, that he, according to what was sometimes by Dr. *Bedell*, his predecessor, practised, would encourage the reading

reading Irish; and that Irish prayers, *&c.* as others, might be publicly used in the college, for thereby fitting out labourers for that harvest of souls, which may, by God's blessing such endeavours, be hopefully expected. But even for that, are books wanting, or not sufficient to be found for it. Therefore the reprinting what is now out of press, if there be no more, will be for that good work necessary.

I REFER all, Sir, to your consideration, whose heart God hath, I find, enlarged in this, as otherwise eminently, to your lasting remembrance, and praise every way. And desiring God's blessing you in all your ways, I rest,

Honourable Sir,

Your most humble servant,

Henry Midensis.

II.

Mr. Boyle *to the bishop of* Meath.

My Lord,

I HOPE your lordship did long since receive the letter I had the honour to write to you, when I sent you the forms of subscription, successfully employed here, in carrying on the publication of the Welch bible. And though I have not, since I sent that packet, which was above two months ago, received any intimation, that it came safe to your hands, yet I thought it unfit to trouble your lordship with another letter since that time, because I could not give you in it any good account of the progress of our Irish testament; the press, that was to print it, having, as well as others here, been so taken up with pamphlets of several sizes and kinds, that till of late our work was almost quite laid aside; but now, for want of other employment, I hope the printer will resume it, and vigorously pursue it, partly because he has promised to do so, and partly because he is so far advanced, that he says, there is but five or six sheets remaining to be printed off. But I fear the preface will lose as much time, because the most intelligent divine I have advised with here about it is earnest to have the preface, that the Jansenists have premised to their translation of the new testament; which being a piece of great learning and piety, and much esteemed by the better sort of the Romanists themselves, it is judged, that if it be published in English with no more alterations and additions, than the exigency of our design requires, it may very much recommend the introduction of the Irish testament to the better sort of Papists themselves, for whose benefit it was chiefly made. Mr. *Kelly*, that corrects the press for the Irish testament, having been bred in *France*, undertakes the turning this preface into English; which, as soon as he has done, for it is no short one, I shall, God permitting, send it into *Ireland* to be perused by your lordship and my worthy friend Dr. *Sall*, and to be made such use of, as you shall upon the place think fit. I formerly gave notice, that I had ordered my agent in *Munster* to return up to *Dublin* 50 *l.* in lieu of a subscription towards the printing of the Old Testament in Irish; but Sir *John Champeny* having more than once taken notice to me, that the money lies in his hands, I now think it necessary to advertise your lordship of it, and that a direction to pay it in, when your lord-

ſhip ſhall call for it, for the uſe newly mentioned, is intended to be, by the next poſt, renewed to him by,

My Lord,

Your Lordſhip's moſt humble

[April 8, 1681.]

and moſt obedient ſervant.

III.

Mr. Boyle *to Sir* John Champanty.

SIR, *London*, April the 9th, 1681.

FINDING by my ſiſter *Ranelagh*, that you have been pleaſed to receive fifty pounds for my uſe, and are deſirous I ſhould diſpoſe of it as ſoon as there is occaſion; I think myſelf obliged to return you my humble thanks for the favour of letting it lie in ſuch ſafe hands as yours, and to inform you, that I intend to employ it by way of contribution to the printing of the Old Teſtament in the Iriſh tongue, which is carrying on by the right reverend the lord biſhop of *Meath*, and ſome other worthy perſons in your parts. And therefore if his lordſhip, or any duly authoriſed by him, ſhall call to you for the money to be employed to that purpoſe, I deſire you would be pleaſed upon his acquittance to let him forthwith have it; whereby you will further oblige,

SIR,

Your moſt humble ſervant,

Robert Boyle.

The ſuperſcription,
Theſe to my honoured Friend Sir John Champanty,
preſent at Dublin.

IV.

Biſhop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, May 3, 1681.

I HAVE now two of yours (of the 8th and 19th paſt) and thereby underſtand my loſs of a packet not received, which I greatly deſire may be retrieved. (There is nothing from you not deſirable.)

As to that preface of the Janſeniſts, premiſed to their tranſlation of the New Teſtament, propoſed to be prefixed to our Iriſh New Teſtament; conſidering their refined principles, and even that very deſign of their publiſhing the holy ſcripture in a vulgar tongue; and that that preface paſſeth among the better ſort of Romaniſts themſelves; ſo rendering all, probably, more acceptable to that ſide; it having alſo that approbation you mention of thoſe eminent among us; on all, although I have not yet ſeen it, yet can I not but well approve of your thoughts for tranſlating, printing, and affixing that to our Iriſh new teſtament alſo. Dr. *Sall* is now here with me, to whom I imparted your mind in this, in which is his concurrence alſo.

I HAD, from Dr. *Sall*, some time since, my first intimation of your setting yourself on that good work of reprinting our Irish New Testament, which I then understood to have been intended of both, the Common Prayer Book and New Testament also; both being necessary, and out of press, and hardly found. But now I find it from yourself (in yours of the 19th) that you had not then so much as knowledge of such a thing in being, as the Common Prayer in Irish, and published.

So as our work herein is thereby enlarged; both for printing the Old Testament in Irish, which hath not been done hitherto; and for reprinting the Common Prayer also; that so all (with what you are now doing) may be complete.

YOUR letters for the work in hand will so far ease that charge; and what you propose of subscription for printing the Old Testament will take in all.

THAT, which leads to this, would be the digesting that proposal for such a subscription: that it be not overcharging as to particular sums; and that it be sufficient in the total to answer the whole work. It may be, what hath been already ordered that way, as to the Welsh Bible (which I have not seen) may be for this also. In ordering which, as to *Ireland*, for procuring subscriptions, from our clergy in our several dioceses, and from others here, and in the several provinces, as it shall require;) this shall be (if you please) my charge herein; if some other be at leisure for furthering something that way there also. The sooner that the scheme for this is considered and prepared, the better; that by that time that the New Testament and preface to it (which will take up some time) may be perfected, your letters for that work may be free for this, to be done here; or this here to be sent thither, for being ordered by the same press and overseer, as is the New Testament at present: in which Mr. *Reily*, your overseer, well deserves.

As for your 50 *l.* left in Sir *John Champanty*'s hand, in way of subscription toward the printing the Old Testament, I desire, that this may be yet under your own ordering, until that work shall be under a fuller prospect, one way or other. In the mean time, and ever, you have your praise of piety, bounty, and beneficence, among all good men, in this, and other your good every way manifested, and not to be forgotten.

I HAVE communicated to the worthy provost of this our college, what you were pleased to declare of your intentions for encouraging our Irish reader and instructor here: which is thankfully accepted, and in your so taking notice of that work, will it be much advanced. Be pleased, that what in that kind is designed may be rather in money than otherwise; of which the quantum will be sufficient, as it is proposed: that also this be consigned to the provost to be ordered accordingly.

As to the teaching and learning Irish; that person hath read (I hear) a public lecture in the college hall: to which, of the students, about 80 already resort: divers also of the fellows, and chief of that society, frequent his chamber privately for instruction in that kind. And even the provost himself (not very many years since transplanted out of *Oxford* hither, and not many months since his promoting this good work within this his charge) is become so great a proficient already in the knowledge of the Irish, that to the teacher's admiration (from whom I have it) he writes the Irish perfectly; and is with his own hand transcribing the Irish grammar, intended for your perusal and farther consideration. Of our having such a grammar in our hand, some intimation hath been formerly given you. There is, I hear, an Irish dictionary in *Bodley*'s library, which the provost hath sent for, for present use, and to be restored.

I SHALL shut this up, with what I have in my last given you joyfully, and with what is since then, of the progress of Irish preaching in this college chapel. The first

first Sunday in each month is designed for that. The first of that (as you heard) was on Easter-day last (a day, you well observe, for that, and I hope, auspicious). The next was on that day month, May-day last (the second of that kind) when the auditory was so enlarged, that the whole area of the chapel, with rooms adjoining above and below, were unusually thronged. Among those were the lord viscount *Dillon*, and other eminent persons, and others of the Romish profession: which we take for a presage of yet greater good to the church and kingdom at large, from this happy seminary. The lord lieutenant intends to afford his presence at this exercise sometime, for farther countenance, and encouragement to it. Dr. *Sall* designs to preach there next in Irish. Let God's blessing be on this, and on all, whose hearts are stirred up by his spirit, in this great work; in which you have, I doubt not, a double portion, who first promoted it in what you are there doing happily. To his grace and goodness I leave you; and rest,

Honourable Sir,

Your most humble and real servant,

Henry Midensis.

HAVING some curiosity for observing the late comet, I gave what was on that collected to the provost here; from whom I send in return the inclosed paper, with which I present you, as well knowing what use to make of it. He was well fitted for such observation by instruments which he had, and none other that I know hereabout.

V.

Bishop of Meath *to Dr.* Narcissus Marsh, *provost of* Trinity *college in* Dublin.

Reverend Sir, *Osberston*, Aug. 1, 1681.

I RECEIVED yours of the 25th past, for which I thank you. My leaving *Dublin* was (I found) about the time of your greatest trouble, the commencement; and my retiring then into the country, where now I am, was by reason of some indisposition attending me in that city: both which occasioned my not waiting on you on my going thence, whereby we might have discoursed more freely on the matters in Mr. *Boyle*'s letter, which I sent you for your perusal; that occasioning yours to me mentioned. Nor could I well return him an answer to his, until I had first advised with you concerning it.

IN former letters from that worthy person, Mr. *Boyle* (like whom, in his way, the age affords not any) I found him zealously intent on prefixing the Jansenists late prefaces of the New Testament in French, to the Irish New Testament now under his hand in the press; hoping thereby (I conceive) to gain more in the world, in joining so far with those of that persuasion; they drawing toward us, although not closing with us. Hereunto, on that consideration, I then also inclined, although I had not yet seen what those prefaces imported.

SINCE then, I had, by the hand of Dr. *Sall*, the book of the New Testament in French, with those Jansenists prefaces prefixed. Which their prefaces, for my better information, I had translated, being myself a stranger to the French.

IN

In reading which I obſerved harſh reflections on ſome of the more eminent and leading proteſtant reformers; alſo other paſſages concerning the difficulty of tranſlations out of the original, on the reaſons therein mentioned: this taking up much paper, and more time and labour than could be now well ſpared in tranſlating that again out of French into Iriſh.

On which I preſume to offer mine own thoughts, that I know not how convenient would be our tacit owning ſuch reflections on ours as juſt; and whether that might not ſtick with ſome, and not pleaſe others? Or profeſſing to give the world a true tranſcript of thoſe the Janſeniſts prefaces, whether it would be ingenuous by picking and chuſing, to omit any part thereof, on a private account; I mean, the omitting (if that ſhould be) thoſe reflecting paſſages, which might offend? And whether ſuch omiſſions might not diſguſt the Janſeniſts themſelves, whom we would indulge? And whether, either in omitting or not omitting thoſe reflecting paſſages on ours, we ſhould not equally gratify the common adverſary?

In this, I would willingly adviſe with you, Sir, but know not how you can give any judgment herein at preſent, without the firſt peruſal of thoſe prefaces, which you cannot now compaſs, the book being locked up in my ſtudy in *Dublin*, and not to be there readily found but by myſelf: which yet ſhall not hinder, if you pleaſe to command my going hence to *Dublin*; which on that occaſion I ſhall do, on intimation by you therein given.

As for myſelf: conſidering what hath been before offered; as alſo the delays to the principal part of the work, the publiſhing of the New Teſtament in Iriſh; of which there is ſuch uſe and need at this time; on theſe conſiderations, I ſay, I confeſſed myſelf much biaſſed to the laying aſide the tranſlating thoſe French prefaces in Iriſh; at leaſt for the preſent. And if on farther conſideration it ſhould be after found notwithſtanding adviſable to take notice of them, that they be prefixed to the Old Teſtament, or to the Common-Prayer-Book in Iriſh, when publiſhed.

As to the printing of the Old Teſtament in Iriſh; that being a work of charge and time, toward which, not ſo much as a propoſal is yet prepared, but in its time expected; I ſhould offer, that this alſo be reſpited for a while; and that the Common-Prayer-Book in Iriſh, formerly printed, and now out of preſs, ſhould be firſt reprinted: which, with the New Teſtament, ſhortly expected, may well forward that your pious work, in which you have ſo merited of this church and nation.

In all which, I humbly ſubmit to better judgments and to yours in a ſpecial manner; deſiring to hear from you, if you pleaſe; that in my return to Mr. *Boyle*, the whole may be repreſented to him, and alſo left to his ordering.

Thus, deſiring God to bleſs you in that your great charge and work, I reſt,

Reverend Sir,

Your very affectionate

and humble ſervant,

Henry Midenſis.

VI.

Bishop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, Sept. 3, 1681.

THIS seems a tardy return to yours, two months past; but the inclosed shews me not so forgetful:

IN which you find my communicating yours to my good friend Dr. *March*, the worthy provost of the college here; whose hand in furthering that by you begun, for the good of the Irish, in their own language, hath been happy, and every day more successful.

HIS thoughts, with mine, you have, in the inclosed, concerning the Jansenists prefaces to the New Testament in French, designed for ours also in Irish; to which I refer, and submit all, howsoever, to what you shall judge of it. Dr. *Sall* speaks of preparing a preface, he hopes, fit for our purpose; which, when perused and allowed by you, shall be after translated into Irish.

Some of your copies of the New Testament reprinted would be very acceptable here, where they are wanting, and necessary. These, if you please, may be consigned to the provost for public use.

I AM on the way for procuring one of the Irish Common-Prayer-Books; and shall find the means for conveying it to you. I suppose your press will be fitter for reprinting that, than any here, strangers to that character and language. But whether there or here, and how to be carried on, I may after understand.

I AM surprised, in missing in that our Common-Prayer-Book in Irish the vulgar translation of the psalms; which until now I never observed; it having been a strange omission, that the psalms, of such daily use in reading, should have been neglected. But, I suppose, that what of the psalms we have in the body of the Old Testament in the Irish MS. (which I have) may supply that want, although it be not according to the vulgar, and may be added in the Common-Prayer-Book as the other.

I PERCEIVE by your last, that what of yours miscarried in sending, was the project concerning your Welsh Bible, intended as leading to ours of the Old Testament in Irish, when it should be considered. But that so lost may be (I doubt not) retrieved seasonably, before we can be ready for the other. For I understand by Dr. *Sall*, and others versed in the Irish, that it may be necessary, that the Irish version of the Old Testament in MS. should be first revised, before it be exposed to public view. This will take up time; and will require charge also, in procuring assistance, and labours in that kind of such as are of approved abilities for it: among whom I wish, if it might be, that Mr. *Reily* might be one. Of this you shall hear farther hereafter.

WHAT you had in my former concerning the late comet, was of Dr. *Marsh*, our provost's observations, which being by him sent me, I presented them to yourself, who are generally known to be in the recondits of nature, above others, critical.

You find also something of his in Dr. *Plott*'s Natural History of *Oxfordshire*, page 289——concerning music; to which I refer. This I mention, that the genius of that worthy person may be better known, his natural modesty giving him otherwise to lie hidden.

I SHALL

I SHALL no farther interrupt your more ferious thoughts at prefent, but wifhing you all happinefs, and the reward of your good to all, reft,

Honourable Sir,

Your affectionate and humble fervant,

Henry Midenfit.

VII.

Mr. Boyle *to Dr.* Henry Jones, *Bifhop of* Meath.

My Lord,

I MUST begin this letter with my humble thanks for the honour of your lordfhip's of the 3d of this month, which, together with the two inclofed papers, came the flowlier to my hands, becaufe they found me not in *London*. As to the verfion of the Janfenifts preface, I muft ingenuoufly confefs to your lordfhip, that the book itfelf, to which it is prefixed, having been got from me, by a perfon of quality, before I had read more than here and there fome paffages of the preface, I did, in recommending it, rely lefs upon my own judgment, than that of a very learned and famous divine, and fome other perfons of eminent parts. And though I was aware of fome of the objections, that I find in the two judicious papers, and had with Mr. *Reily* taken order to have them obviated in his tranflation into Irifh, yet I meet with fome weighty ones that I did not (and indeed could not) well know of; and therefore, as your lordfhip may remember, that in propofing this preface, I was ready to fubmit to the better judgment of your lordfhip and your learned friends upon the place, fo now, upon frefh accounts, I fully refer to you the prefixing what preface fhall be thought moft fuitable to the defign of the publication, and the particular circumftances of it and the verfion: in which work I am very glad your lordfhip will have fo good an affiftant as Dr. *Sall*, who is fo able a man, and fo well acquainted with the humour and opinions and prejudices of his countrymen, that I doubt not but he will fkilfully accommodate his difcourfe to them. Only, perhaps, it may be thought fit to make ufe of fome paffages of the Janfenifts preface, not as it is a preface, but as it contains the public avowed fenfe, and eloquently exhibits the reafons of famous and eminent divines of the Roman church for the tranflating and ftudying the New Teftament in vulgar tongues. I am, as well as your lordfhip, much furprifed, that the reading pfalms fhould be omitted in the Irifh Common-Prayer-Book; but perhaps it will not be for the worfe, fince it will be neceffary to fupply that omiffion out of your Irifh Old Teftament, in which it is hard if the pfalms be not better tranflated out of the original, than if they had been out of the vulgate. I perceive, that, God permitting, there will be time to retrieve the papers that I fent your lordfhip, and that mifcarried, about the method ufed in getting fubfcriptions for the Welfh Bible; but by reafon of the abfence of the manager of that impreffion, I cannot yet get copies of his papers. But I hope your lordfhip, and the other well-wifhers to the printing of the Old Teftament, will not ftay long for the revifal of the tranflation. For though it feems very fit that one fhould be heedfully made, yet I fee no neceffity that the work fhould be thereby retarded. For fome good progrefs may be made in the review, before it will be poffible to get the firft fheet printed; and then it cannot

be very difficult for the revifers to correct at leaft as faft as I fear the printer will work off. Whether the edition will be fitter to be made here (as your lordfhip feems inclined to think) or at *Dublin*, I wholly leave to be determined by your lordfhip and your friend: and whether here or there, the letters I caufed to be caft will be equally at your lordfhip's fervice. I lately expoftulated with Mr. *Reily* about the fhameful tardinefs of the edition of the New Teftament; for which he much excufes himfelf, laying the whole blame upon the printer, and promifing, that if the Old Teftament be to be wrought off here, he will tie him up very ftrictly to difpatch. All which, notwithftanding I know not, as I was lately intimating, whether *London* or *Dublin* be the fitteft to be pitched upon for this work; efpecially fince here, in cafe Mr. *Reily* fhould die, or fall fick, the whole work muft be at a ftand, there being none (that I can hear of) in this place fit to correct the prefs. Thofe ingenious papers about the comet, were communicated to a perfon that I hope will make good ufe of them; and I am glad to know, that I was not miftaken in thinking they came from Dr. *Marfh*, that I may upon the new ground rejoice, that the college of *Dublin* is happy in fo learned and worthy a provoft. Before the receipt of your laft packet, I was altogether of your lordfhip's mind, that it is not fit all the exemplars of the New Teftament fhould ftay for the preface, (whatever that be) and accordingly I caufed copies to be bound the ordinary way, befides much more handfomely bound, for the prefent ufe of the members of the college, and fome other perfons that your lordfhip or Dr. *Marfh* fhall think fit to fend any to. And becaufe to have them tranfported by long fea (though I were advifed to that, as much the cheapeft way) would certainly take up time, and might prove hazardous, I refolved to difpatch them by land to *Chefter*, to the poft-mafter of which place I got them particularly recommended by Mr. *Dowlin*, poft-mafter of *Dublin*; where, I hope, they will fpeedily come fafe, and be delivered to your lordfhip, whofe long life, health, and fuccefs in this good work, is heartily prayed for by,

My Lord,

Your lordfhip's moft faithful

and moft humble fervant.

VIII.

Bifhop of Meath *to Mr.* Boyle.

Honourable Sir, *Dublin*, Novemb. 5, 1681.

YOUR laft of the 27th of September, came to hand as I was then entering on a journey into the country, where my prefence was neceffary, which fo far occafions this delay in this return to yours.

THEREIN you mentioned the fending hither Irifh Teftaments, fome (about 50) bound, and others (about 350 in fheets) then on their way hither from *London*.

THEREUPON I attended our principal officers at the Cuftom-houfe here, defiring, when thofe books arrived, they might be fafely depofited under their care, until my return out of the country; the provoft alfo of the college being then for fome time in the county of *Kerry*, infpecting college affairs there, and thereabout.

ON

ON his and my return hither, we found those books here arrived, and laid up accordingly for us about six weeks, which, and the bringing them from the Custom-house to the college, (where they are laid) was with some, but little charge.

IN that your letter mentioned, was also intimated something farther expected from you for ordering that parcel of those books which were not bound; of which nothing hath hitherto come to my hand here, that I can understand. Therefore are they laid by, not delivered to any book-binder, until further orders concerning them appear. These are deposited in the college under the provost's care, which I conceived the place proper for their safe keeping, and after dispersing them on occasion.

AS to those books sent over bound, 40, according to your directions, are left with the provost for the present use of the college.

SOME also of the books of the better sort of binding, were presented as from yourself to those great persons in yours named. Only I presumed to present one of them to the lord lieutenant the duke of *Ormonde*, as from yourself (although not mentioned) and found it well accepted by his lordship. I conceived this necessary, for having his lordship's countenance in that great business when occasion should require, there being many adversaries.

THAT which gives greatest hopes of success in this, is our good provost's care and zeal in training up the present youth in the college in reading the Irish, which by the books from you now in their hands is much forwarded.

THESE may be a seed plot for the church; the harvest is great, and labourers few; therefore is the Lord of the harvest to be earnestly desired to prepare and send forth more labourers for the great harvest of souls, which we hope for.

WE have received your translation of the Jansenists prefaces, and you will shortly hear from Dr. *Sall* of his preface also. I desired him first to send you a copy of that in English for your approbation, before he entered on his translating it into Irish.

I SHALL forthwith put into his hand the Irish Old Testament in manuscript, to be by him revised. You will also have from him the Irish Common Prayer-Book for farther consideration.

AS for the Welsh model for subscriptions, which may direct in our proceeding and forwarding this great work, in that we depend on your procuring it, as you shall find your opportunities for it. God bless you in all. I rest,

Honoured Sir,

your most humble and real servant,

Henry Midensis.

I CONTINUE to mind you of what I have some time since written concerning my kinswoman Mrs. *Leane*, tenant to the earl of *Burlington*, for renewing her lease now expiring. I am just now solicited in this, which occasions your trouble in it.

IX.

Dr. Anthony Dopping *Bishop of* Meath, *to Mr.* Boyle.

Honoured Sir, Aug. 3, 1682.

I MUST needs acknowledge it as a great omission in myself, that I have so long delayed an answer to your kind and obliging letter, which I had the honour to receive a considerable time since: and as I desire not to make any apology for my neglect, so I hope

hope you will be inclined to pardon it, upon my promise of offending no more in this nature. As for the design on foot about the Irish Old Testament, it goes on, though by slow and leisurely steps; but I doubt not within a small time to procure (from some well affected persons to the work) such subscriptions as may add little more of life and briskness to the undertaking; and I hope we may effect this, without putting you to the trouble of sending the formularies of subscriptions for the Welsh Bible: but I presume our subscribers here will expect a book when it is printed; and if so, I am afraid that the printed copies designed may but answer the number of subscriptions, and so the publickness of the work be prejudiced by it. How to obviate this, I leave it to your prudence and judgment to consider, and contrive an expedient; to whom, as the glory of the undertaking is entirely due, so I should think myself very criminous, if you were not made acquainted with all the hazards that fœtus must run through, which owes its birth to your care and charity. Thus with my humble respects I remain,

Honoured Sir,

your most humble and obedient servant,

Anth. Midensis.

X.

Bishop of Meath *to* * * *

Reverend Sir, December 14, 1685.

YOUR letter, which I received yesterday, would have been very obscure to me, if you had not sent me Mr. *Boyle*'s inclosed in it, as a key to open the sense of some things in it. As to the title page, mentioned in his letter, I can say nothing to it, being a stranger to the transactions between him and you concerning it: as to the preface, if by it be meant that which I sent you, I am much of Mr. *Boyle*'s opinion, that it is improper to have it in English, and that I intended it to be translated into Irish, had not your letter to me imported that the press stayed for it. But since it is now out of my hands, and Mr. *Reily* hath obtained a deserved character for his knowledge in the language of his own country, I know of no person more fit to be employed in it than himself.

As for an historical account of the version, I dare not undertake to tell the world, that it is impartially done from the original, having no knowledge of the language of my own country: all that I can add concerning it, is only this, (which, if Mr. *Boyle* thinks fit, may be farther enlarged in its exterior dress, but very little as to the substance) that in the convocation held at *Dublin* 1634, there were no small debates about the version of the Bible and the liturgy of the church into the Irish tongue, for the benefit and instruction of the natives; Dr. *Bedell*, bishop of *Kilmore*, being for the affirmative, and Dr. *Bramhall*, bishop of *Derry*, opposing it. The reasons of the former were drawn from the principles of theology, and the good of souls; of the latter, from politicks and maxims of state, and especially from an act of parliament, passed in this kingdom in the reign of King *Henry* VIII. for obliging the natives to learn the English tongue. However, the reasons of bishop *Bedell* were thought so

ſatisfactory (eſpecially being countenanced by the authority of primate *Uſher*) that the convocation thought fit to paſs two canons concerning it; the one, that the miniſter ſhould read the liturgy in Iriſh, where moſt of the people were ſo, can. 8. the other, for the pariſh clerk to accompany the miniſter in reading his part of the ſervice in Iriſh, can. 66.

UPON theſe foundations the pious biſhop *Bedell* reſolved to make farther ſuperſtructures; and accordingly ſet himſelf to the verſion of the Old Teſtament into the Iriſh tongue (for the New had been done before at the expences of Sir *William Uſher*, clerk of the council of *Ireland*, and by the care of Dr. *Daniel*, who was after made archbiſhop of *Tuam* in *Ireland*) taking to his aſſiſtance one Mr. *King* and Mr. *Dennis Sheridan*, both Iriſhmen and clergymen, and excellently ſkilled in the language of their own country, whoſe office it was to tranſlate the then Engliſh verſion into Iriſh, whilſt the biſhop (who was excellently learned in the Hebrew and the Iriſh languages) reviſed the whole work, comparing it with the original, and either expunged or added, as he ſaw it nearer or more remote from the original. The work, thus happily finiſhed, was left by biſhop *Bedell* with Mr. *Sheridan* the tranſlator, (who ſurvived him) and was by him delivered to the late biſhop of *Meath*, Dr. *Henry Jones*, by him communicated to Dr. *Andrew Sall*, from whom I received it before his death, and gave it to your predeceſſor Dr. *Marſh*; and what fate it hath met with ſince, he and others, whoſe hands it hath paſſed, can beſt relate.

SOME part of this narrative I have read in the life of biſhop *Bedell*, lately publiſhed by one *Clogy*, who is ſomewhere beneficed in *England*, (if he be alive) and married the ſaid biſhop's daughter, and may be more particular if conſulted. If I have miſtaken any thing, (as perhaps I may, as to the name of *King*, for I have not the book by me) that book will rectify it: the remainder, relating to Mr. *Sheridan*, you may receive a more ample account of from the biſhop of *Kilmore*, who is his ſon. I have ſent you Mr. *Boyle*'s letter, conceiving, that it may import you more than me. I ſuppoſe the title page ought to be in Iriſh characters firſt, and then in Engliſh; with this inſcription in both: *The books of the Old Teſtament tranſlated into Iriſh, by the Care and Diligence of Dr.* William Bedell, *late Biſhop of* Kilmore *in* Ireland, *and now printed for the public good of that Nation.* London.

IF Mr. *Boyle*'s modeſty would permit, I judge it neceſſary, that at the end of the preface ſomething be inſerted concerning his bounty in the printing it at his own charges, that the nation may know their obligation to ſo worthy a benefactor. I remain,

SIR,

Your moſt humble ſervant,

Anthony Midenſis,

XI.

PREFACE to the IRISH NEW TESTAMENT.

To the Chriſtian people of *Ireland*.

HERE you have, dear countrymen, the New Teſtament of our bleſſed Lord and Saviour Jeſus Chriſt preſented to you, in your country garment, tranſlated unto the

the Irish tongue; and therein the most noble, the most important, and, to well disposed minds, the most pleasant reading that possibly can be offered to their view. I say the most noble, whether you consider the Author of it, being the very wisdom of God, his divine Son Jesus, and his holy disciples, taught by himself, and by the Holy Ghost guiding their pens; whether you look upon the subject of this sacred book, which is the knowledge of divine mysteries, and of the way leading to your everlasting happiness; in which consideration you find also the supreme importance of this sacred reading, as guiding you to the kingdom of heaven. And what availeth you to purchase all that the world can afford, if you should fall short of that glorious enjoyment? And if God hath commanded his people to read continually the law which he hath given them, and to meditate therein day and night; and if societies, aspiring to some peculiar degree of perfection, do believe they are obliged to read always the rules they have taken to that end; we that are by God's mercy *called unto the fellowship of his Son Jesus Christ our Lord*, how shall not we think it our most necessary duty to read, and continually to meditate in the law he has left us as a testament and pledge of his inheritance?

1 Corin. i. 9.

Finally, the unspeakable pleasure of this sacred reading, was well experienced and frequently attested by those godly souls who happily have exercised themselves in it. Them you may conceive speaking to the Author of this book, in the Psalm cxviii. 103. *How sweet are thy words unto my taste! yea sweeter than honey to my mouth*; and in that of *Canticles* iii. *Thy speech is comely*. St. *Clement Martyr* declares, that whatsoever may be desired for harmless pleasure or profit in profane books, the same may be found far more advantageously in the sacred scripture, of which he maketh himself a large and fair induction, running upon the contents of several books of the Holy Bible. And St. *Augustin* says, that *as the store of gold, silver and garments, which the Israelites brought out of Egypt, was but mean, in comparison of what they found after in Jerusalem, as may appear by the wealth of King Solomon: so any purchase of knowledge got in the books of the heathens, is nothing, being compared to the science of sacred scripture.*

Clement. lib. 1. constit. apostolic. c. 7.

Aug. lib. 2. de doctrin. Christian. c. 41.

And this being true enough, speaking of scripture in general, is singularly evinced in this divine volume of the New Testament of our Lord and Saviour Jesus. For if you are for the pleasure of history, relating wonderful successes, other records may represent to you men overcoming other men, cities, or wild beasts: but here you shall find a divine man subduing the world without worldly arms; the sea and winds, and all the elements, even the devils, obeying his commands; death itself, which trampleth all human power, overcome by him, raising others and himself from under the power of it, and mounting with his own proper power over the clouds and skies unto the highest heaven. In the Acts of the Apostles you shall meet with the like prodigies wrought by his disciples, illiterate simple men triumphing over the swelling science of the sages of this world, kingdoms and nations obeying their word. If you desire moral instructions for ruling your life, and steering the course of it to everlasting rest, you have in the sermons and precepts of our Saviour spread over the Gospel, and in the epistles of his blessed apostles, the very treasure of knowledge given to you by God for that purpose, and the most excellent system of ethics and theology that ever was seen in the world, all virtues heretofore known to men there commended and exalted, and others more sublime never before practised in the world, taught by our Saviour and his disciples in their precepts, and most powerfully by their examples. St. *Chrysostom*, glossing upon the admirable meekness of *David*, sparing his inveterate enemy in the cave of *Engeddi*, 1 *Sam.* xxiv. exhorteth all men to print in their hearts that passage; *for it is impossible*, saith he, *that a mind conversant in such kind*

Chrysost. homil. 1.

kind of histories should be overcome by passions. And is it possible, that a man, reading in the Gospel and in the Acts of the Apostles, so many prodigious examples of humility, meekness and patience, practised by our Saviour and his blessed disciples, should be overcome by pride, vanity, or impatience? When he finds the Son of God chearfully bearing affronts offered to him by vile men; when he sees the holy apostles rejoicing, that *they were counted worthy to suffer shame for his name*, Acts v. 41. shall he be startled at every small injury received? de David & Saul.

AND lastly, if you are for a sublime extatic contemplation of heavenly mysteries, you have in the venerable prophetic deepness of the Apocalyps sparkles of divine light, which in pious and humble souls do work a profound respect of God's infinite grandeur and majesty.

IF any should say, that this precious treasure should be reserved for the learned in the original or school languages, and not exposed to popular eyes in the vulgar ones; whatever be the weight of this consideration, it has been over-ruled already by all Christian nations, *Italy*, *Spain*, *France*, *Germany*, *England*, and all other kingdoms and countries of the Christian world, do enjoy the glorious light of divine scripture in their native idioms. The learned doctors in divinity of the university of *Paris*, have reprinted of late the New Testament in the French language, following therein the examples of the doctors of *Louvain* in the same faculty, who had set forth the whole Bible in the same language, and bemoaning in the preface of their edition the sad effects of the common neglect of reading the holy scripture in these latter times of Christianity, far otherwise than it was in the more holy primitive ages of it. All the writings of the holy and learned fathers of the church are replenished with marks of their respect to the word of God delivered to us in the holy scripture, exhorting all men to the reading and knowledge of it. The Gospel, say they, is the mouth of Christ: he sits in heaven, but speaks continually on earth. How then dares that man call himself servant of Christ, who cares not to know what he ordains him to do? Or how can he obey him, if he slights to hear him? The precepts of the Gospel, says St. *Cyprian*, are the foundation of our hope, and the nourishment of our heart. It is in this reading we find light to guide us, strength to uphold us, and remedy to heal us. This made *Cæsarius*, bishop of *Arles*, to say, that those very men, who are not able to read, may not be excused from attaining to a competent knowledge of the soul-healing doctrine of the Gospel; for if even such men do find means to learn idle and lewd songs and romances, with more reason they should apply themselves to learn the heavenly doctrine of scripture. And that they may the better do it, the foresaid learned doctors of *Paris* and *Louvain*, as also other pious and learned men in all nations of Christendom, have taken care to translate the holy scripture into the vulgar language of their countries, that the divine doctrine of it may more easily come to the knowledge of all sorts of men. And that *Ireland*, sometimes inferior to few of the nations in piety and learning, might not be deprived of the like happiness, has been the pious zeal and care of worthy patriots, whose names ought to be with praise continued in the memory of posterity. Such we find to have been *John Kearney*, sometime treasurer of St. *Patrick*'s church in *Dublin*; *Nicholas Wailsh*, bishop of *Ossory*; *Nehemiah Odonelan* and *William Odonel*, both archbishops of *Tuam*, by the help of others skilful in the *Irish* tongue, the province of *Conaught*, and Sir *William Usher*, clerk of the council, bearing the charge of the first edition of the New Testament in Irish.

AND now again, that edition being worn out in time, and few copies of it to be found, God has raised up the generous spirit of *Robert Boyle*, Esq; son to the right

honourable *Richard* earl of *Corke*, lord high treasurer of *Ireland*, renowned for his piety and learning, who hath caused the same book of the New Testament to be reprinted at his own proper cost. And as well for that purpose, as for printing the Old Testament, and what other pious books shall be thought convenient to be published in the Irish tongue, has caused a new set of fair Irish characters to be cast in *London*, by a skilful artisan, and an able printer to be instructed in the way of printing this language; and having appointed five hundred copies of this new edition to be bestowed on such as shall be thought to make the best use of them, has extended his charitable zeal towards the printing of the Old Testament in Irish, giving both the use of his characters for that purpose, and fifty pounds of his money for a beginning to subscriptions for the publishing it. We cannot but hope, that God will excite the spirits of other pious men to follow the heroic example of this nobleman, in furthering this holy work for the good and comfort of many poor souls, pitifully desolate and blinded, for want of the heavenly light of the word of God to shine among them.

If any other should further say, that holy scripture, translated unto common language, should not be generally permitted to all men's reading, because some, by the ill use, and wrong understanding of it, have raised schisms and many disturbances; it may be answered, first, that this argument proving too much proves nothing by the ordinary rules of arguing. It proves too much, because churchmen and the more learned should be prohibited the reading of scripture more carefully upon this account, whereas *Arius*, *Pelagius*, *Nestorius*, aad other prime heresiarchs, were churchmen, and of eminent learning. Secondly, that because children do cut their fingers with knives, and wicked men do murder others with swords, knives and swords must be taken from all upon the like account. The remedy therefore is to be, not to take away knives and swords, and less the scripture, but the evil use of them. To avoid this, and find the inestimable treasure enclosed in sacred scripture, let piety, humility, and discretion guiding both, be their companions in reading it. Let their piety appear in seeking sincerely the will of God, and the way to him in that holy reading. Let their humility shine in not relying too much upon their own judgment in the exposition of scripture, where it is obscure, but praying to God, and seeking the help of the most warrantable teachers for the right understanding of it; in the choice of which teachers, their discretion is to be practised, relying more upon the most ancient, most holy, and most generally approved in the Christian church.

And if, after using their best endeavour to find out the right understanding of scripture, some places of it should continue obscure to you, take from the learned, humble and holy doctor St. *Augustin*, this excellent lesson for judging of those sacred books: *Where I understand them*, says he, *not only nothing can be more wise, but also nothing seems to me more well spoken: and where I do not understand them, their eloquence is not so apparent to me; but, even then, I do not doubt but it is such, as where I do understand them.* God were not God, if to be comprehended by human wit. If any of his words seem to you not so agreeable to reason, make sure account the fault lies in your weak understanding; wait patiently, pray and read again, and hope in God's mercy, that he will open your eyes to a right understanding; and the more often you read, the more light you shall daily receive. Mark what the same great doctor St. *Augustin* declareth of his own experience. *I do publickly avouch*, says he, *the depth of Christian scriptures to be such, that I would daily have more and more to learn in them; though from my childhood to decrepit old age, I should with full leisure, most eager study,*

Aug. lib. vit. de doct. Chr. c. 6.

Aug. epist. 3.

and greater wit, endeavour to learn them. But the same holy and learned doctor giveth you for comfort what is certainly true, that in what is plain of scripture you have all that is necessary for your instruction, both in belief and in a good life, which made St. *Paul* writing to *Timothy* tell him, that *the holy scriptures, which from a child he had known, were able to make him wise unto salvation.* Whereof he giveth the reason, adding immediately, that *all scripture is given by inspiration of God, and is profitable for doctrine, for reproof, for correction, for instruction in righteousness; that the man of God may be perfect, thoroughly furnished unto all good works.* 2 Tim. iii. 15, 16, 17.

Now therefore the excellency of this holy book being so incomparable, and so vast the importance of it, it is the expectation of the pious and learned men, who have laboured in publishing it, and their earnest request even for Christ Jesus his sake, that all good Christians capable to make use of it, shall in their respective stations reverently frequent the reading of it, for the benefit of their own souls, and of others about them, or under their charge; and more especially, that all godly churchmen, who have the cure of souls, may read, or cause it to be read to their congregations, whereto understanders of the Irish tongue do resort, or upon this encouragement may resort more; considering, that no preaching is so effective to work upon souls, as the very *word* of *God*, which, as St. *Paul* says, *is quick and powerful, and sharper than any two-edged sword.* For the same reason, pious fathers of families should cause it to be read on fit seasons to their houshold, inuring them to a deserved love of this sacred reading, in lieu of romances and other idle or noisome divertisements. Foreigners also, who designing to continue their habitation in *Ireland*, will, by the rules of common prudence, desire to purchase a knowledge of the language, may attain it by reading this book, comparing each chapter of it with another of the same contents in the language they understand; for which purpose some short rules have been already, and more shall be published, for the right pronouncing and reading the Irish tongue. Heb. iv. 12.

FINALLY, students in schools and universities, who design to live by the cure of souls in *Ireland*, shall upon a serious consideration find it their precise duty to procure such knowledge in the language of the natives, as may enable them to help and instruct the souls committed to their charge, and of which they are to give account to God; for notwithstanding all the wise statutes and endeavours used to bring this whole nation to a knowledge of the English tongue, experience shews it could not be effected, too many being unable to give such teaching to their children, or to get it for themselves; and it is apparent, that in *Ireland* there are many parishes, baronies, and whole counties, in which the far greater number of the common people do understand no other language but the Irish. This being so, how can it be presumed of any godly pastor of souls in such places, that he shall not procure the spiritual welfare of those men by the sweat of whose brows he hath his bread, enabling himself to preach, read to, or converse with them in a language they can understand, that being the way to gain their good-will, and thereby to win their souls to God? For very true and experienced is what *Aristotle* said, *plurimas amicitias taciturnitas sola dissolvit*, that want of communication breeds want of love and union. And to gain their affection is the way to benefit their souls, and cure them of the most deplorable blindness they are in, sitting long time *in darkness and in the shadow of death*, for want of setting before them the heavenly light of the word of God, which his loving providence hath given as *a lanthorn to our feet*, in this our pilgrimage towards our desired home of heavenly glory. And now the Lord and giver of it, our dear Psal. cxix. 104.

Saviour Jesus, who hath inspired the thoughts of publishing this sacred book for the good of souls, be graciously pleased to grant all others concerned to make due use of it towards the glory of God, and the spiritual welfare both of others and their own. Amen.

NUMBER IV.

Letters and papers relating to the distributing of the Irish Bibles in the *Highlands* of *Scotland*, at the expence of Mr. *Boyle*. *Life*, p. cxxxix.

I.

Mr. Kirkwood's *letter to Dr.* Wotton, *containing a general account of Mr.* Boyle's *concern in the edition of those bibles.*

Reverend Sir, June 22, 1702.

I AM glad you are going to publish the life of the excellent Mr. *Boyle*, who was so great an ornament to his country and to our holy religion. I reckon it one of the blessings of my life, to have been acquainted with so extraordinary a person, whose company I found always very delightful and edifying. It was soon after our acquaintance was begun, that I had the opportunity of talking with him of the sad state of religion in the *Highlands* of *Scotland*, where they had neither bibles nor catechism in their own language. This gave him an occasion to tell me of his having caused five hundred bibles in Irish, in a quarto volume, in the Irish character, to be printed for the use of those in *Ireland*, who understood not the English. He then offered a few of those bibles to be sent into *Scotland*, to see what reception they might meet with there: a dozen of them was first sent, and afterwards two hundred; which made one for each parish. In such places, where these books were distributed, they had a very good effect, as you may perceive more fully by the papers relating to that affair, which I send you. After some time, I heard from some ministers in those parts, that it was the earnest desire of many who wished well to our religion, to have a new impression of the bible in the Latin character, in a small volume, for the use of such persons in the *Highland* parishes, as had been taught to read English, though they did not understand it, and for the advantage of such children, as should be sent to school, especially those of the poorer sort, who could not purchase books for themselves. To answer the pious desires of such persons, endeavours were used in *Scotland* to procure another impression; but in this attempt we met not with success. The first encouragement that was given me to go on with it in this kingdom, was by the worthy Mr. *Boyle*, who told me, he would subscribe for printing one hundred bibles. This example disposed others, whom I acquainted with the design, to subscribe very freely and largely. Most of the subscriptions you will see in the printed paper I send you relating to that affair. I need not mention other particulars, only in

in ſhort, after ſome few years, this work was happily finiſhed, the books were printed, tranſmitted into *Scotland*, and long ago the greateſt part of them was diſperſed in the ſeveral counties of the *Highlands*. We have had many accounts of the happy effects, which have attended our endeavours, in behalf of thoſe poor people, who have been ſo long neglected and ſuffered to remain in a ſtate of ignorance and barbarity. By the printed paper you will likewiſe ſee, that there were ſix thouſand catechiſms and prayer-books in Iriſh printed at the only charge of Mr. *Boyle*, for the uſe of the Highlanders, which were accordingly ſent down into *Scotland*; many of which have been likewiſe diſperſed among the Highlanders. The catechiſm and prayers were compoſed by Mr. *Charteris*, and were tranſlated into Iriſh by Sir *Hugh Campbell*, of *Caddel*, and afterwards reviſed and corrected by Mr. *Kirk*. There were added to the catechiſm ſome paſſages of ſcripture, containing the principal heads of the Chriſtian religion, to ſerve as a ſhort and plain tract of devotion and Chriſtian morality. It was this catechiſm and prayers, that are mentioned in the biſhop of *Roſs*'s letter. As for the papers I ſend you, which have ſome relation to that great man, I leave it to you to make uſe of them, or any part of them, in ſuch ſort, as you judge may beſt ſerve your deſign. When you have done with them, be pleaſed to return them to me again, to be left for me at Mr. *Milbourn*'s a Watchmaker at the *Blue Boar* in the *Old Baily*, near *Ludgate*, *London*. As for the letter from Mr. *Charteris*, I ſend it you, that you may ſee the high opinion, that pious and primitive perſon had of the famous Mr. *Boyle*. As for Mr. *Boyle*'s own letter, the chief reaſon why I ſend it, is upon the account of ſome few ſeaſonable expreſſions in it about education, which may be very well improved in ſome part or other of his life. As for the letter from Mr. *Kirk*, a Highland miniſter, who was corrector afterward of the preſs, when the Iriſh Bible was reprinted, perhaps it will furniſh you with ſome hints not unuſeful to your deſign. I need ſay no more as to this matter. I heartily wiſh you may have good ſucceſs in what you are about, that you may be able to ſet forth ſo great a pattern and example in ſuch a light, as may move others to have a higher regard for ſolid piety, and uſeful learning. Before I conclude this letter, I muſt acquaint you with another deſign, which has been ſet on foot, in behalf of the miniſters, ſchool-maſters and probationers, in the *Highlands*. The reaſons of this deſign you will ſee in the printed paper I ſend you. They, who are now in the government there, have ſo far eſpouſed it, as to recommend it to the ſeveral preſbyteries to promote it. I know your good affection to religion and learning will diſpoſe you to encourage a work of this tendency, which is likely to prove of very great uſe and advantage. Your neighbour, Mr. *Frank*, can acquaint you with ſome farther particulars relating to this affair. I am,

Reverend Sir,

Your affectionate brother,

and humble ſervant,

JA. KIRKWOOD.

An

II.

An account of the design of printing 3000 Bibles in Irish, for the use of the Highlanders.

THE inhabitants of the *Highlands* of *Scotland* have never had the bible in their own language, which is the Irish. Some endeavours have formerly been used in *Scotland*, to have it printed in that language, but hitherto they have proved unsuccessful. Of late the honourable and pious and learned Mr. *Boyle* hath, at great charge, caused to be printed 500 Bibles in Irish, which were designed for *Ireland*; and accordingly most part of them, especially the New Testaments, were by him sent thither. Of the books of the Old Testament which remained, he sent above two hundred into *Scotland*, which made one book for each parish in the *Highlands*, there being so many parishes in those parts, which are of very large extent, containing great numbers of people. The same excellent and worthy person was at the charge of printing, for the use of the Highlanders, 3000 catechisms and prayer-books in Irish, with some passages of scripture, containing the principal heads of the Christian religion, they never having had any such helps before. He hath also lately given money for to reprint as many more. Several of those bibles and catechisms have been distributed among such parishes in the *Highlands* as are near *Edinburgh*, and where the present commotions have not prevented their being sent. The rest will now be sent very speedily. In all those places, where they have been sent, the people express a wonderful joy, and a great desire to know the word of God; so that they, that can read, are at some pains to teach others to read likewise. And such is their zeal, that they send for the bible sometimes to one part of the parish, and sometimes to another, that they may read on the week days; and then they return it to the church on the Lord's day, that all may hear it read publickly. And it is very remarkable, that amidst the public commotions in that kingdom, scarce any of those Highlanders, who have received bibles and catechisms, and have been instructed in the knowledge of truth, have joined themselves to the adversaries of the present happy settlement. All sort of people among them express an earnest desire to have another impression of the bible in Irish for themselves and for their children, that by reading and hearing of that sacred book, they may be delivered from ignorance and barbarity. The money collected for carrying on the work is to be deposited with the reverend doctor *Horneck*, who hath been pleased charitably to accept of the trouble of receiving it. Several worthy persons both of *England* and *Scotland*, have promised their concurrence, and signified the proportions they intend to give, of whom also not a few have already sent their money to Dr. *Horneck*, at his house in *Exeter-street*, who hath a list of their names, with the sums of money they have subscribed or sent. They who have not yet sent their money, are desired to send it as soon as may be. The person employed for the printing this bible is *Robert Everingham* at the *Seven Stars* in *Ave-mary lane* nigh *Pater-noster-row*. Mr. *Robert Kirk*, minister of *Aberfoyle* in *Monteith* in the *Highlands* of *Scotland*, is come to *London* to attend the press. The work is now begun, and will be finished (God willing) before Christmas next. The books, which are subscribed for by some charitable persons of this kingdom, are to be distributed (by the advice and with the concurrence of persons of public authority in the kingdom of *Scotland*) amongst the poorer sort of Highlanders. As for the books, which are subscribed for by those of *Scotland*, they are likewise to be bestowed on

on poor people, so far as is needful; and as for the money, which shall be got from those Highlanders, who are able to buy books for themselves, it is to be kept in store, as a fund for another impression of the bible in Irish, or for printing some such useful treatise in that language, as shall afterwards be thought most proper. We have sufficient reason to expect, that schools will be settled in all parishes of the *Highlands*, that thereby this charitable design may be rendered effectual. This will be shortly recommended to the care of the parliament in *Scotland*. The paper and printing amounts to 275 *l.* at 1 *s.* 10 *d.* per book in quires, the binding (at 8 *d.* per book) amounts to 100 *l.* which in all makes 375 *l.* Towards which we have the following subscriptions, which amount to 244 *l.* 8 *s.* 8 *d.* So that there is wanting to complete the charge of paper and print 30 *l.* 11 *s.* 4 *d.* and for binding 100 *l.* in all wanting 130 *l.* 11 *s.* 4 *d.* There are also New Testaments to be printed, for the use of young children learning at school, the number of which will be proportioned to the money that shall be given.

	l.	*s.*	*d.*
Mr. *Boyle* gave for this charitable use	8	6	8
As also did several other persons the sum	236	2	0
Total amounting to, with Mr. *Boyle*'s,	244	8	8

III.

An answer to the objection against printing the bible in Irish.

IT is not to be doubted, that a great many who make this objection, do it without any bad design, but only through their not considering the matter sufficiently. And therefore to remove their mistake and prejudice, the following considerations are offered unto them, whereby these two things will appear. 1st, That the extirpation of the Irish language, out of the *Highlands* of *Scotland*, is not possible in this age. And secondly, That it is not probable to be effected in succeeding ages. As to the first, it may appear sufficiently to any who will consider the methods of doing it. As 1st, By colonies. 2dly, By scattering the Highlanders all over the nation. 3dly, By schools. 4thly, By sending their children to serve in other parts of the kingdom, and so to learn English. As to the first, if several colonies should be sent among them, what could they do towards the extirpating the language, unless we should suppose them more numerous than the inhabitants, which is impracticable? For neither could the rest of the nation spare so great a number of people, nor yet could the barren mountains of the *Highlands* afford them the conveniences of life. Besides, the Highlanders not living in towns, but scattered up and down in very small villages and single houses, the settling of colonies among them could not signify much to root out their language. As to the 2d of scattering the Highlanders all over the nation, it is an idle dream, not at all practicable; for neither would the generality of the Highlanders leave their native soil, nor yet would others readily receive them. And, 3dly, As for schools, neither can they do it: for first we may suppose, that there will be still too many, who go not to school through the poverty and negligence of the parents, and uncharitableness of richer neighbours: now those that tarry at home, must needs retain their own language. 2dly, We may very reasonably suppose, that many who send their children to school to learn to read English, are not able

able to keep them so long under the care of a school-master, as to have time to learn to speak it, though they may have time to read and understand it pretty well in books. 3dly, Suppose one of ten or five children, who are sent to school, should learn it perfectly to speak it, and understand it when spoken, yet what is this to the extirpating the language in this age? What must be done with the four or five, or the nine or ten children, not to say any thing of aged persons, who cannot speak English? Ought not care to be taken, that they perish not for lack of knowledge? Or shall the blessed means of heavenly light and comfort be with-held from them, as from a generation, that God hath cursed and forsaken? Far be it from all those that bear the honourable name of Christians, to harden themselves against their brethren, and under pretence of any human policy to fight against the kingdom of our God and Saviour. As to the fourth, to wit, sending children from the *Highlands* to serve in those parts, where they speak English, it can signify but very little; for there are not many (in comparison) who can be sent into other parts of the kingdom; and it is known that many who have continued for some years in those parts where they speak English, have never acquired so much skill in that language, as to be able to understand a sermon in it. As to the second thing, that it is not very likely to be effected in succeeding ages, may appear from the following particulars. 1st, Where is here an instance of any such thing, that has been done any where in the world, except in such places where the conquerors have been more numerous than the conquered? It is known to all, who are acquainted with the state of *Europe*, that in most kingdoms there are some provinces, which speak a different language from what is spoken in the rest of the provinces of the same kingdom. 2dly, Many thousand Highlanders are separated from the rest of the nation by islands, so that they have very little conversation or commerce with any but those of their own language; and therefore it is very hard to imagine, how their language should be extirpated. 3dly, It has not been known or heard of in this age, nor for any thing we can learn in some past ages, that any one parish, where they have been wont to preach in Irish, has learned so much English, as not still to need a preacher in the Irish language. 4thly, In several parishes in the borders of the *Highlands*, where they preach only in English, there remain still some corners, where the inhabitants go to have their children baptised, and to hear sermons in the adjacent parishes, where they preach in Irish, because they understand not those that preach in English; which plainly shews, that the English language does not prevail so much over the Irish, as some would make the world believe. 5thly, About one half of the ministers in the *Highlands* (according to our best information) preach only in Irish, which shews, that there is very little English understood in their parishes. And where they preach one half of the day in English, it is in many places of very little use to the generality of the people; of whom several can buy and sell in English, who do not understand a sermon in that language. As to the women and children, scarce one in twenty can speak English throughout the *Highlands*. 6thly, It is very considerable, that in *Kintyre* (a country in the *Highlands*) whence the Highlanders were expelled, and where others, who spoke English, were planted in their stead, in process of time, by frequent conversation with the neighbouring Highlanders, many of them, instead of propagating the English language, have learned Irish; so that now they preach once a day in Irish in the chief churches of the country. To which might be added, 7thly, That the Highlanders, as much as any people, are very much in love with their own ancient language, and jealous of all designs against it. After all that hath been said, it doth sufficiently appear, how little ground there is to think it likely, that the language

guage of two hundred thousand persons can be extirpated; for by a moderate computation, there can be no fewer in the *Highlands*, where there are about two hundred parishes, which makes above a fifth part of the parishes of the whole kingdom: and therefore how necessary it is to carry on and encourage this design for the further reformation of so considerable a part of the kingdom. When once the children are provided with Irish Bibles, and taught to read them, they will be means of instructing the families, to which they belong. And besides the great advantage, which people in general will reap by this charitable work, the ministers themselves, and students in divinity, will thereby receive unspeakable advantage. It will prevent the mistakes, which many have been guilty of in translating the holy scriptures; and by reading the Bible in Irish, which will be no hard matter to do, for those who can read English, this impression being in the Latin character, they will be enabled in an apt and uniform stile (becoming soon familiar by use) with greater advantage to express their thoughts in their addresses to the people, both in public and in private. The scriptures are the weapons of the Christian warfare; and shall we unchristianly and unmercifully deprive our brethren of that which they have so great need of in their defence and safeguard? How careful has the church been in all ages (except since Popery prevailed in the world) to translate the Holy Bible into the language of all nations, which were converted to Christianity! And, not to mention what was done in other countries, *Bede* tells us, that in this island the Bible was read in five dialects, then vulgarly used, to wit, of the Angles, the Britains, the Scots, the Picts, and the Latins. How just a prejudice against the Papists is their hindering people from reading the holy scriptures! They put that light under a bushel, which God hath appointed to be set on a candlestick.

IV.

Mr. Kirkwood *to Mr.* Boyle.

Honoured Sir, *Astwick*, Nov. 5, 1687.

THIS is only to shew you, that the bishop of *Ross*, with the clergy of his diocese synodically assembled, by one of their number desires, that thanks may be returned you in the most affectionate manner, for that very good work, the making the scriptures to be more known, when others have design to eclipse them. The number of the ministers of that diocese are 31. They propose this, that if you have so many Bibles as may serve them, each of them one, they are most willing to pay for them. As for the catechisms, they give this account, that the province is sufficiently furnished with them already in the Irish tongue. This is all that I have heard of late. I know not whether these catechisms, which they mention, are printed: if it is so, they are both translated and printed very lately. I hope to hear shortly from other parts of the *Highlands*, where there is still, for ought I know, need of such helps. I shall not fail to acquaint you, whenever I hear from those parts. When these ten or twelve bibles (which my friend Dr. *Denvane* told me you intended to give to the Highlanders) are bound, I have desired a friend of mine to send them carefully. If you think fit to send some dozens more unbound, I shall take care, that they be bound in *Scotland*, and distributed to the best advantage. I have wrote to those who divide them amongst the Highlanders, to give them chiefly to such ministers, whose parishes are in greatest want of them, with these two conditions: 1st, that

that the ministers read some chapters every Lord's day to the people who never heard a chapter read in their lives; and on other days, when they can have occasion, as at baptisms, burials, marriages, *&c.* The second is, that the ministers take care of the Bible, as being for the use of the parish, that it be not alienated from this design; so that if a minister be removed into another parish, or die, yet the Bible may still be kept and preserved for the use of the same parish. If there are more bibles than serve each parish, then some may be given to the more numerous and considerable families, for the benefit of the children and servants, who know not the English tongue. I thought fit to mention the sending a good many bibles together, because I hope they may be more useful now than before, having heard of the good success, which some zealous persons have had in preaching amongst them of late. As to the number of the parishes in the *Highlands*, I believe they are not above 200, if you think fit to send so many. I doubt not but it would be of singular advantage towards your design, if you wrote a letter, addressed in general to the bishops and clergy of the *Highlands* of *Scotland*, or to the ministers of the Gospel in the churches of the *Highlands* of *Scotland*, wherein might be represented to them your design in causing these bibles to be printed, and the desire you have, that they may contribute their assistance in making your design effectual for the purposes of the Gospel. I know your piety and zeal to be attended with so much prudence and discretion, that you are able to do this in that manner, which will greatly conduce towards the rendering your work, which you have done already, so much the more profitable. For the better dispersing of such a letter (which I very much wish) there might be 4 or 500 of them printed: or, if you are not for that, care shall be taken to transcribe several of them. I hope you will pardon the freedom I use in this matter. If I know how to be serviceable to you in any good design, you might expect to find me always ready to shew you, how much I am,

Honoured Sir,

Your most affectionate and most humble servant,

JAMES KIRKWOOD.

He whom I have desired to take care of the bibles, is called *Oswald*, a bookbinder, at the *Blue Posts* in *Air-street*, *Piccadilly*.

I HAVE wrote to *Scotland*, that if it will do well enough to print the little books in the English character, as I believe it will (for all who can read the one, as I am informed, can read the other) then they may print as many as they think fit at *Edinburgh*, where they can easily find an Highlander to oversee the press. This, if it succeed, will prevent a great deal of trouble. What expence they are at in printing them, may be sent by a bill of exchange.

V.

Mr. Kirkwood *to Mr.* Boyle.

SIR, *Astwick*, Dec. 15, 1687.

YESTERDAY I heard from Mr. *Oswald* (of whom I wrote to you) that there are 32 bibles ready to be sent with the first occasion. I have writ to the bishop of *Ross* this inclosed, which I thought fit to send to you, that if it is not wholly according

cording to your thoughts and design (which I have presumed to guess at in this manner) then you may either acquaint me more fully with your mind, or desire Mr. *Oswald* to write to the bishop, unless perhaps you may think good to write yourself. If you think fit to send my letter as it is, be pleased to seal it up, and let Mr. *Gregg* deliver it to Mr. *Oswald*, to whom I have written about the bibles, that they may be sent safely to *Scotland*. Since my last to you I have heard concerning the Irish translation of the prayer-books, *&c.* that they have a great mind, that they be printed in *Scotland*, if it can be got done. I heartily pray for your preservation, and that God may direct and assist you in all your ways. I am,

SIR,

Your most affectionate and

most humble servant,

JAMES KIRKWOOD.

VI.

Mr. Kirkwood *to the bishop of* Ross.

My Lord, *Astwick*, Dec. 14, 1687.

HAVING received a letter from one of my friends in *Scotland*, wherein I was told, that it was your lordship's desire, as also of all the clergy of your diocese, that thanks might be returned to the excellent Mr. *Boyle*, for his generous and charitable offer of some bibles for the use of the Highlanders; I did accordingly acquaint him therewith. Since which time he hath been pleased to cause 200 bibles in the Irish tongue to be bound, whereof one is for your lordship, and thirty-one for the ministers of your diocese. The rest are for the use of other parishes of the *Highlands*. I know Mr. *Boyle*'s design in causing these bibles to be printed; and now his bestowing them freely where they are needful, is, that he may thereby contribute his assistance towards the propagating the knowledge of the Gospel of Christ Jesus, that by these sacred writings, they who are in darkness, and ignorant of divine things, may be enlightened, and rescued from sin and folly. He desires no other reward for these books, but to hear they produce the effect, which he heartily wishes and prays for. I doubt not but that your lordship will recommend to your clergy such methods, as in Christian prudence you judge may be most proper and effectual for promoting so good a design. Mr. *Boyle* hopes, that the ministers will read every Lord's day some chapters to the people, or cause them to be read by fit persons, at that time, and at other times, when people come to church. These bibles are intended for the use of the parishes; and therefore you will be pleased to acquaint your clergy therewith, lest otherwise by the death of the ministers the bibles fall into the hands of those who will not take much care to serve this design. I am mighty glad, that it hath pleased God to raise up Mr. *Boyle* to persue so excellent a design, as the spreading abroad the knowledge of the holy scriptures, which he hath done likewise in some other languages: so that he hath not only afforded great light to the world by his excellent philosophical writings, but, which is far better, hath done his utmost to fill some of

the dark places of the world with the light of divine truth, by these useful translations of the bible into so many languages. How much may so great an example make many of our sacred order to blush and be ashamed, considering that we do so little good in the world, and concern ourselves so little for the honour of our great master; have so little love for the souls of men, and are so barren of good designs and works, although we are appointed by God for this very thing, to make it our business to save men's souls, to make them wise and good, to turn them from darkness to light, by the light of pure and holy doctrine, and of an heavenly conversation? And as at all times this ought to be our meat and drink, so especially when we see how great pains many are at to fill the world with darkness, to corrupt men's hearts and lives, to lead them into errors and delusions. I humbly beg your lordship's blessing and prayers. I am,

My Lord, *&c.*

JAMES KIRKWOOD.

VII.

Mr. Kirkwood *to Mr.* Boyle.

SIR, *Astwick*, Dec. 27, 1687.

AFTER a long and tedious expectation, I have at last received a letter from the Highland minister, to whom the translation of the small treatises was entrusted, giving me an account of his having finished that work, and his having made some considerable additions for the use of the Highlanders. The minister's name is Mr. *Kirk*, who is one of very great worth, a learned, pious, zealous man. The translation is at *Edinburgh*. I have wrote to some persons there to take care there be printed about 2000 of them in the Latin character, according to Mr. *Kirk*'s desire. He tells me, that both ministers and gentlemen rejoice mightily to think, that there are hopes of their seeing the Bible with catechisms and prayer-books in their own language; and that the ministers are very willing to read the scriptures publickly in their churches. He says, a great many ministers and gentlemen would buy the bibles, if they were sent down, and the price set on them; and that 200 might quickly be sold: but (he adds) the *Highlands* require 2000: and if the Bible were reprinted (which he says were fit to be done in the Latin character) they could sell them, and get the money very soon. He says (which is very just) it behoved to be well attended, if there were another edition, by a very skilful Irishman. He hath remarked several errata in this edition, one letter for another, and some words wanting, which he promises to send me in a sheet of paper, if ever there be need of it. He says, that generally all people in the *Highlands* are overjoyed with the thoughts of this, and are mighty eager to have bibles. He blesses the God of heaven and earth, who put this excellent design into your breast, and adds [who knows what a heavenly flame this spark may kindle?] He adds a great many pious and zealous expressions, which I have not at present time to transcribe. I would have sent you the letter itself, but that it is such as I am unwilling to lose, and I know not, if this shall come to your hands. Not many days since, I sent you a letter, with one inclosed to the bishop of *Ross*; I know not whether you have received them. Instead of this letter, I had a great desire to have come myself, and talked with you of this affair, but that I am not as yet fit to travel so far, especially in this season of the year. If you think fit to send what

what bibles you have, if you have 200 remaining, you need not be at the trouble to cause them to be bound; they may be sent in sheets, with those which Mr. *Oswald* hath received for the diocese of *Ross*. If you judge it proper to put any price upon them, let me know, and I shall take care, that the money be received at *Edinburgh*, by those who will send it by a bill of exchange faithfully. A part of the price may be employed in printing the little treatises. As for the reprinting the 2000, I wish it were done. It were a design worthy of the concurrence of several persons, who have money, charity and zeal together: if there were such an edition of so many bibles, I doubt not but the money might be recovered by the sale of them, if that were needful. For my own part, I would willingly contribute 10 *l.* towards such a work. I cannot but have a great compassion for many poor souls, which live under a sad night of ignorance, superstition, and profaneness in those barbarous places. Mr. *Kirk* tells me, he hath drawn up an Irish vocabulary, a grammar, and a collection of Irish proverbs, which makes him desirous of a small but competent number of Irish types, with the contractions, for which he will send money. If you think fit to let him have any of yours, they may be sent with the bibles, and the price of them (if you let me know it) shall be sent him. He writes of an Irish book or two lately printed at *Rome*, which I would buy for him. If you know any Irishman at *London*, who deals in books, I make bold to desire you to cause your man, Mr. *Gregg*, to enquire about them, and to buy them for me, that they may be sent, when other things are sent. He writes a long account of the reason of the great delay in his translation, which is too tedious to transcribe. At the same time, when I received this letter of Mr. *Kirk*, I received another from the excellent author of the catechism and prayer-books, Mr. *Charteris* (formerly professor of theology at *Edinburgh*, but hath lived privately since the test) who desires me to shew you, that among many others in *Scotland*, he does highly honour and admire you, for your ingenious and learned labours, and your fervent charity and zeal; and wishes, that the Lord may long preserve you, and may stir up others to imitate your excellent virtue and piety. I know you value not yourself one whit the more, because others do so, and therefore I am not afraid to let you know what this excellent person did desire me. I heartily join with my worthy friend in wishing, that the Lord may long preserve you, and that he may direct and assist you in every good word and work. Whenever I think I may venture upon a journey to *London*, I intend to come and wait upon you. I make bold to desire you to give my most humble service to your excellent sister, my lady *Ranelagh*. I am,

SIR,

Your most affectionate

and most humble servant,

JA. KIRKWOOD.

VIII.

Mr. Boyle *to Mr.* Kirkwood.

Worthy Sir, *London*, March 5, 1687-8.

WHEN I come to look upon the date of your letters, I cannot but be much troubled to find, how long I have been your debtor for an answer. And I should be as much ashamed, as troubled at it, but that it pleased God to afflict me

with a diſtemper, which, though his goodneſs made very ſupportable to me, yet it has made me ſo weak and tender, that I have not ſtirred out of the houſe theſe two months: during which time I was indiſpoſed to keep a literary correſpondence, even with my near relations; whereto I muſt add, that, during a good part of the time, your letter was unhappily miſlaid. The biſhop and preſbytery of *Roſs* put too high a value upon my weak endeavours in a work, that need not expect a retribution from men. But I am very glad to find by their proceedings, how well they reliſh thoſe things, that aim at the propagation of the Goſpel; and I wiſh, that all, that ought to be equally zealous to promote affairs of this kind, would ſhew themſelves equally concerned for the advancement of this. Some paſſages in your letter oblige me to take the freedom to tell you, that I perceive you have not been rightly informed of my circumſtances in reference to the publication of the Iriſh Bible. For at firſt my aim was, as I thought it became me, to do ſome ſervice to the country, wherein I was born, and have ſome little eſtate, though I have lived a great ſtranger to it; and eſpecially to contribute to the converſion and the inſtruction of thoſe Iriſh natives, that are moſt of them of the Romiſh religion. In purſuit of this deſign, I with much ado, procured a verſion of the New Teſtament, made, as I remember, in ; and finding it to have been many years out of print, the copies having, as I have been informed, been bought up, from time to time, by ſome Romiſh eccleſiaſtics; I cauſed a font of Iriſh letters to be caſt, and the book to be here reprinted; of which I ſent over ſome hundreds, ready bound, to be diſtributed gratis among thoſe to whom they ſhould upon the place be judged the moſt likely to do good. Theſe things I relate, that you may not think it ſtrange, that I have not ſtill by me any number of New Teſtaments in Iriſh; though I ſhould not have ſo much diſfurniſhed myſelf of them, if I had been then ſo informed, as now I am, that this language is ſtill that of a great part of the North of *Scotland*. And my main deſign being, as I ſaid above, to acquaint the native Iriſh with the divine oracles; as ſoon as I had with the ſame characters cauſed an edition to be made of an Iriſh verſion of the Old Teſtament, which the excellent biſhop *Bedell* had long ago procured, and which, after the lately mentioned publication of the New Teſtament, divers of the reverend Proteſtant clergy in *Ireland*, beſides ſome worthy prelates, intended to promote, but by various occurrences were hindered from accompliſhing their good deſires; as ſoon I ſay, as the preſs had diſpatched this book, I ſent a competent number of theſe books into *Ireland*, to be diſtributed gratis by ſome pious and judicious perſons there. By all which particulars you may eaſily diſcern, that it was not, nor well could be, my principal deſign, to provide for the Highlanders and other natives in *Scotland*, that ſpeak *Iriſh*; and that it never was my purpoſe, that any of thoſe, that were printed at my charge, ſhould be ſet to ſale. But yet becauſe it were great pity, that the pious intentions and endeavours of your reverend miniſters ſhould be altogether diſappointed, I ſhall acquaint you with what comes into my mind about it. Before I well knew the great extent of the Iriſh language in *Scotland*, I had cauſed ſeveral Old Teſtaments to be bound for the uſe of your nation, divers of which remain yet in my hands. So that as to the one and thirty you mention, I can ſpare each of them a book, which I think you very diſcreetly adviſe, ſhould not be diſpoſed of by the miniſter at his death, or removed, but be kept for the uſe of the pariſh. And theſe will be accompanied with one with a gilt back, to be preſented to the right reverend biſhop of *Roſs*; which I hope his lordſhip will accept, as an acknowledgment of my juſt ſenſe of his pious forwardneſs to promote the good work I aimed at. I ſhall alſo ſend an hundred more unbound, ſince you deſire to have them ſo, which, with the other

other thirty-two, ought to be disposed of gratis to such parishes as my lord bishop, who I perceive consults with his presbytery, shall think fit. With Irish New Testaments I formerly told you, that I was not furnished. And I did not think fit to reprint them with the Old, not only because it was judged needless, but because it was scarce possible to bind them together, without making the volume much too bulky and unwieldy. But having, upon the late perusal of your letters, caused enquiry to be made, I am informed, that a bookseller, having with my leave printed a somewhat greater number of New Testaments than I had contracted for, he has yet in his hands about fifty Old Testaments, and about one hundred and fifty New ones. He can afford each whole Bible for a mark, provided the whole fifty be taken off his hands; and for the supernumerary hundred, or thereabouts, of New Testaments, he can part with them at three shillings a piece in quires.

IX.

Presbytery of Ross *to Mr.* Kirkwood.

Reverend brother, Canonry of *Ross*, Oct. 15, 1688.

WE the presbyters of the diocese of *Ross* being this day synodically convened, our right reverend bishop caused read publickly a letter from you to his lordship, dated June last; wherein, to our great satisfaction, we heard an account of the pious endeavours of the honourable and excellent Mr. *Boyle*, and were surprised with the bountiful offer you were pleased in his name to make, of a Bible and considerable number of catechisms translated into the Irish language, for every parish within this diocese: for which we can return nothing more suitable, than our most hearty thanks to the excellent and famous Mr. *Boyle*, and our humble prayer, that God may reward himself, and bless all those his good designs for the benefit of mankind, and withal to return you also our thanks for the representing to him our need and desires for these books. We shall use our utmost endeavours to prosecute all his religious designs, by following those methods which he by you recommends, as far as the circumstances of our people can allow; for so our reverend bishop has, with a great deal of affection and respect to that worthy person, very earnestly laid upon us. We are also using all diligence to get that noble present of books brought among us; but the distance of the place, the unusual storminess of the weather, and the confusions that we are all put into by the march of our parishioners, who are commanded to repair unto the King's host under the severe penalties of the law, makes us hopeless to get them brought to this place for some time, till it pleases God to calm things, as well as the weather. But the zeal we have to enjoy the benefit of them, will not suffer us to be negligent about them. In the mean time, we shall be most ready to do all services that are in our power to the generous Mr. *Boyle*, and to you, to whom also we think ourselves much obliged, and shall never cease to remember you both in our prayers; for we are, and subscribe ourselves by the clerk of the meeting,

Reverend brother,

Your affectionate brethren to serve you,

James Ross, Cl. Synod.

Mr.

X.

Mr. Kirkwood *to Mr.* Boyle.

SIR, July 13, 1689.

I SHOULD have waited upon you before I went out of town, but I have been obliged to be with a person who had the small-pox, and is now dead of them; which makes me that I do not come to wait upon you myself, knowing that you have never had that disease. As for what you mentioned to me, when I was last with you, about giving some more bibles to the Highlanders, I have considered of it with Mr. *Kirk*, and other discreet persons, who do not think it necessary at this time, since each parish hath a Bible already sent to them, or designed for them; and as for others, this impression I hope will be sufficient for them, for some time. We cannot but be more and more sensible of your generous charity, for which God will reward you. As for the New Testaments, which are not yet distributed, if you please to bestow of them upon the parishes which are largest, and have greatest need of them, it will be a very great and excellent present to those poor people. Mr. *Everingham* says, he thinks he has but about thirty New Testaments belonging to you. If you think fit to bestow them, they may be sent with the bibles, which are now a printing, when Mr. *Kirk* returns to *Scotland*. As for the binding of them, we shall take care that it be done, without putting you to any more charge, which hath already been very extraordinary. Mr. *Everingham* tells us, that he is obliged to make use of two of your Irish bibles, which are necessary in correcting; which I know you will be very well satisfied with. I heartily wish you health and all sort of happiness. I hope God will, in his good time, rescue *Ireland* from the hands of wicked men. I am,

SIR,

Your most affectionate

and most humble servant,

James Kirkwood.

XI.

Mr. Spalding *to Mr.* Boyle.

SIR, *Edinburgh*, June 28, 1690.

I AM appointed by the general meeting of the ministers and elders of this church, as to shew the sense they have of your great pains in causing emit the Bible in the Irish language, so to desire, that these two hundred Irish bibles already at *Edinburgh*, may be at the disposal of the next general assembly of this church, to be held on the third Thursday of October next; and that they may not be dispersed, until the said general assembly order the viewing of the translation, and their being made use

use of publickly in congregations. This is desired of you, in the name and by the appointment of the said general meeting, by,

SIR,

Your honour's very humble servant, and their clerk,

Jo. Spalding.

DIRECT your return, *To* Thomas Edgar, *chirurgeon, at his shop, over-against the Cross, on the north side of the street*, Edinburgh; *for Mr.* John Spalding, *minister at* Kirkcudbright.

XII.

Mr. Boyle *to Mr.* Kirkwood.

SIR, *London*, October 18, 1690.

I CONFESS you have exercised a great deal of charity (which yet I shall not be surprised to find in you) if you have not censured me in your thoughts, for my not having sent you all this while an acknowledgment of your obliging letter, and the valuable present that accompanied it. But though my sister and I were justly satisfied with the good counsel and reflections contained in your pious and sensible book; yet, when I should have returned you my thanks for it, in an answer to your letter, I found the care I intended to take of it had not kept it from miscarrying, as, after a fruitless search, I presumed it had done. But on the Lord's day, turning over some papers, for which I had a peculiar regard, I happily lighted on that which I had so long missed. I need make no return to what you are pleased to say to my advantage and your own diminution, save that I look upon the former as a compliment, and the latter as a dictate of modesty. But I am altogether of your mind, of what you write about the education of youth; for I think the rectifying of that to be a thing so important, that, till it please God to awaken men to a greater sense than they yet have of the necessity and usefulness of that, I shall scarce expect any such reformation as I wish, either of men's principles or their manners. And I think it as much the interest, as duty, of those whom it concerns, to listen to the good advices you give them in your serious discourse. Not long after I received the favour of your letter, there came to my hands, out of *Scotland*, a short one (of a long date) from an unknown writer; to whom, because he was so, I did not think fit to return an answer, till I had consulted you about the contents. And during the time I could not find your letter; the other that I thought I had carefully put put up, was unhappily mislaid too. For which reason, I must beg you to assist me to apologize for the delay occasioned by that unlucky accident. But now, having recovered both your's and your countryman's, I must desire your thoughts, what will be fit for me to do upon the latter, whereof a copy is inclosed to you, by,

Sir, your's, *&c.*

Robert Boyle.

XIII.

Mr. Kirkwood *to Mr.* Boyle.

SIR, *Astwick*, Oct. 30, 1690.

YESTERNIGHT I received your very kind and obliging letter, for which I return you my most humble thanks. I am glad, that the little plain treatise I made bold to send you, has met with your approbation. This is the effect of your goodness and charity, and of your zeal for any thing, that seems to tend to the honour of our great master, who accepts of poor and weak endeavours, if they are sincere.

As to the affair of the bibles, I have not been unmindful of it. I have writ to several persons to do what they can to render the design effectual; particularly I have writ to one of the ministers of *Edinburgh*, Mr. *David Blair*, who corresponds with me at present about promoting that good work. In my letters to him (the last whereof was writ about a month ago) I gave him an account of the bibles and catechisms you sent to *Edinburgh*; I shewed him how your great charity, in causing so many bibles to be printed, and your bestowing 200 of them upon the *Highlands* of *Scotland*, has encouraged several pious and charitable persons, chiefly of this kingdom, to give very liberally towards printing 3000 more bibles and 1000 testaments, whereof the testaments are already bound, as also about 2000 of the bibles. I acquainted him likewise, that you have been at the charge of printing 3000 catechisms, besides the first 3000. I desired him therefore, earnestly to recommend this business to the general assembly, that they might think of the way, how to transport into *Scotland* the bibles, testaments and catechisms, which are now ready at *London*; and to consider how to distribute into the *Highlands* both the books you have given, and so many of the rest of the books, as at present are necessary to be distributed amongst the *Highlanders*. The great thing, which I desired him to recommend to the general assembly, was, the settling of schools in every parish of the *Highlands*, that the children may be taught to read, and to make use of the catechisms and bibles; by which means they may prove happy means of propagating the knowledge of God in the families to which they belong. Those things I have likewise recommended to some other ministers, who, I hope, have been careful to act their part faithfully in promoting this design in their general assembly. It is likely the ministers will desire the concurrence of the parliament in a matter of so great importance, which tends so much to the benefit both of church and state. I wrote lately to Dr. *Horneck* to propose it, by some fit person, to the King, that he would recommend the encouragement of this design to the parliament of *Scotland*, that they may promote it so far as they can at this time. I have heard since from the doctor, who approves the thing. There seems a great deal of reason, that the King should appear in a matter, which is so much for the honour of the great King. And though the present confusions in the *Highlands* do not permit to do all that we wish, yet somewhat may be done and projected, till there be an opportunity to do more.

I THOUGHT it would not be improper, in answer to some part of your letter, to give you this general account of the affair.

You are pleased to desire my thoughts what to do as to Mr. *Spalding*'s letter. It is your humility to propose this. Though I am not fit to give advice to a person of your wisdom, yet, in obedience to your commands, as a well-wisher to the design, I shall

ſhall give you my thoughts. If you yourſelf judge it convenient to write, it ſeems not amiſs, in my humble opinion, to ſignify to Mr. *Spalding* your being deſirous, that they, whom he mentions in his letter, would think of fit ways to diſtribute, and to render uſeful the bibles and catechiſms you have beſtowed for the uſe of the *Highlanders*; and that you and a great many more would be very glad to hear of their zealous endeavours to reſcue ſo great a part of the nation from that ſad ſtate of ignorance and barbarity, in which you hear they are. This, when ſuggeſted by you, may prove no ſmall ſpur to them. I know the principle of divine charity, which moved you at firſt to attempt the impreſſion of the Bible in Iriſh, will move you to do any thing you think will tend to render what you have done ſucceſsful and effectual.

WHEN Mr. *Kirk* went to *Scotland*, I wrote to the perſon, with whom your bibles, *&c.* are depoſited, not to give any more of them to any body, but to leave it wholly to the enſuing general aſſembly, to order the diſpoſal of them, according to your intention and deſign; of which I wrote fully to Mr. *Blair*, from whom I expect to hear very ſhortly what the general aſſembly has done in this important affair. I have nothing elſe to add, but my hearty prayers for your ſafety and preſervation. I am,

SIR,

Your moſt obliged and moſt affectionate humble ſervant,

JAMES KIRKWOOD.

XIV.

Biſhop of Roſs *to Mr.* Kirkwood.

Reverend Sir,

I HAD the favour of your diſcreet and obliging letter, dated June the 16th, upon the 24th of Auguſt. You muſt not think it was the fault of your correſpondent, that it came not ſooner to my hand, but the diſtance of the place from *Edinburgh*, and the uncertainty of a ſure bearer. However I thought myſelf extremely obliged by your letter; not only becauſe it gave me an account of the labours of that honourable and excellent perſon, Mr. *Boyle*, but becauſe it gave me alſo an opportunity of a new converſation (at leaſt by letters) with ſo worthy a perſon as yourſelf. I have cauſed tranſcribe your letter, and am to ſend a double thereof to every biſhop any ways concerned with people in his dioceſe that ſpeak Iriſh; judging, that it would better ſpeak forth the learned Mr. *Boyle*'s mind and your's, than any thing I could ſay; and if I cannot get theſe books brought hither before the meeting of my ſynod, I ſhall with them adviſe of the beſt way of doing it. I ſhall alſo recommend to my brethren, and do what is in my power, to proſecute the religious deſign of the author of ſo great a favour as you narrated; though I am much of your mind, that not a few even of the preſbyters muſt begin at the alphabet, that they may but read the Iriſh language in its proper character, there being ſo few books in that language and character paſſing in all this country; and we are removed at ſo great a diſtance from *Ireland* and the *Weſt-Iſles*, that it will oblige the preſbyters to paraphraſe many words in the Iriſh Bible before they be underſtood fully by their people: ſo that I cannot condemn the deſigns of ſome, to have that language quite worn out of this country; and if with it we could alſo put away the barbarity of the manners of ſome of our

Highlanders, I ſhould think it a very good work. I took care, a conſiderable while ago, to cauſe ſome of the preſbyters, who were beſt ſkilled in that language, to tranſlate into Iriſh a catechiſm, which is appointed to be made uſe of in all the dioceſe either in Engliſh or Iriſh, according to the capacities of the perſons to be catechiſed, as it is alſo in ſome other dioceſes; and in my opinion, it ſerves the ends of catechiſing better than that of the aſſembly. I ſend you herewith a double of it in the Engliſh, that at your conveniency I may have your opinion about it, and, if it be poſſible, the judicious ſentiments alſo of the pious Mr. *Boyle*. Care alſo was taken, that there be ſchools for teaching the youth to read in every pariſh; but I confeſs hitherto it has only been in the Engliſh language: and no leſs care is taken, that every miniſter read a portion of holy ſcripture; and in places, where moſt of the people underſtand only the Iriſh, he is at the pains to tranſlate it into that language, for which this noble preſent of the Iriſh Bible will be very uſeful to them, becauſe I have heard ſome of them complain, that they could not get words to expreſs in Iriſh ſome paſſages of holy writ. I cannot but with you regret, that ſo few intend ſo good and catholic a deſign as the worthy Mr. *Boyle* does; but this adds the greater luſtre to his fame. We have many excellent treatiſes written of late by the worthy guides and paſtors of the church of *England*, and ſome alſo by learned and pious laics; all which are very uſeful for aſſerting and improving our holy religion, for which we are extremely obliged to them: but if in this poor church and nation we make fewer public appearances, you are better acquainted with the reaſons of it, than that you need be told them by me. I ſhould think all was well enough with us, if we could truly uſe the antient motto, *non magna loquimur, ſed vivimus*; but that I do not weary you in my firſt letters, I ſhall only ſay, that I have ſeen moſt of the judicious writings of the learned Mr. *Boyle*; and they have raiſed in me, as I think they muſt do in all others, an high but juſt eſteem of him; and for this laſt bounty, by which he has obliged all my charge and me, I know his pious generoſity expects no other reward but our prayers for him, wherein ſhould we be wanting, we were much to blame before God and man. Nor can I cloſe before I return you my own hearty thanks, as well as thoſe of ſuch in theſe bounds, as I have ſpoken with, for your kindneſs in this affair to us all, and in your letter to myſelf. When my brethren meet with me in the ſynod, it is like they will expreſs their own ſenſe of it. Wherefore, intreating, that you may upon occaſion let me know of the health and other good works of Mr. *Boyle*, and what way I can beſt ſerve him or you, I heartily commend you to the grace of God, and beg your prayers for,

Reverend Sir,

Yours, *&c.*

J. Rossen.

NUM-

NUMBER V.

Letters from Mr. *John Eliot* of *New-England* to Mr. *Boyle*, relating to the ſervices of the latter for the propagation of the Goſpel in *America*. *Life*, p. cxxxix.

I.

Right honourable, *Roxburg*, Sept. 30, 1670.

YOUR conſtant care of, and ſtedfaſt affection unto this Indian work (which the Lord hath, in great undeſerved mercy to me, put under my hand, a weak and unworthy inſtrument therein) do greatly oblige my heart to honour you, and pray, that it may be remembered by the Lord in that great day, when he will ſay [*come ye bleſſed*] unto all the ſincere benefactors unto his people. You have alſo added no ſmall encouragement unto me, in that worthy gift, which your honour is pleaſed to beſtow upon me, *viz. Pool's Synopſis*, or *critica ſacra* upon the whole bible, which, though it be not yet come, is under the care and faithful hand of my worthy and true friend Mr. *Aſhurſt*; for which deſirable gift, I return unto your honour my humble thanks. Touching the preſent ſtate of this work with the Indians, I have written to our worſhipful commiſſioners, who will ſend it unto your honour, governor of the honourable corporation; and therefore I ſhall keep ſilence of that matter here. And whereas your honour will ſee, that I have undertaken and begun a kind of academical reading unto them, in their own language, thereby to teach the teachers and rulers, and all that are deſirous of learning, I find, by experience, that it will be very neceſſary to have ſome entertainment of food, for all the principal men at leaſt, which do come; for many are to come a great way, and had we but food to entertain them, when they come there, it would be ſome encouragement. And I have ſome thoughts, if God give life and means, to read medicine, and call for ſuch roots (for they altogether uſe the root, and not the herb) as they have experience of; eſpecially had I wherewith to recompenſe any, that bring in a deſirable experiment. There hath been a rare work of God this ſummer in a great pond at *Watertown*, where all the fiſh died, and were not willing to die in the waters, but as many as could thruſt themſelves on ſhore, and there died; not leſs than twenty cart load, by eſtimation, lying dead, all at once, round about the pond. An eel was found alive in the ſandy border of the pond, and being caſt into the water, ſhe wriggled out again, as faſt as ſhe could, and died on the ſhore. An inhabitant of the town, living by the pond, his cattle uſe daily to drink there; but then, for three days together, they refuſed there to drink; but after three days they drank of the pond, as they were wont to do. When the fiſh began to come aſhore, before they died, many were taken and eaten, both by Engliſh and Indians, without any hurt; and the fiſh were very good. Now the diſeaſe of the ſtone groweth frequent among the Engliſh, and beginneth among the Indians; which ſtirreth me to ſearch, and I clearly find, that a crude ſtomach provides the matter, and cold in and about the bladder and ureters is the efficient of the ſtone, eſpecially in thoſe whom I have converſed with, as may be demonſtrated. But I am over-bold to preſume to meddle ſo far unto your honour. I therefore ſhall

ceaſe to give you any further trouble at preſent. So committing you to the Lord, and to the word of his grace, I remain

Your honour's to ſerve you in the ſervice of the Lord Jeſus,

John Eliot.

II.

Right honourable nurſing fathers, *Roxburg*, Oct. 23, 1677.

THE poor praying Indians do thankfully acknowledge, that (under God our heavenly father, and under Jeſus Chriſt our redeemer, who redeemeth us out of all our troubles) you have been the means and inſtruments in his hand, to ſave and deliver us. God moved your hearts to own us, in that black day, when all were againſt us, and we were almoſt ready to be ſwallowed up in deſtruction; which dark time we ought not to forget, nor your owning kindneſs unto us in that dark day.

And ſince that, your charity hath greatly revived and refreſhed us. Many of our aged, decrepid, fatherleſs, and widows, ſtill wear the garments, not yet worn out, which your charity did, the laſt winter, clothe us withal. And although we yet know not what our honoured commiſſioners will do for us, whoſe favour we doubt not of; yet underſtanding, that ſome doubt is raiſed about your countenancing and encouraging our rulers, who are of us, and live among us, and without whoſe preſence and aſſiſtance, the Lord's work of ſoul-inſtruction and edification will ſoon faint, ſink, and come to nothing; our humble petition is, firſt to God, that he, who hath hitherunto, would ſtill move your hearts for our good and welfare; and next, our petition is unto yourſelves, that we may have the countenance of your favour, to countenance and own our rulers among us, without whoſe countenance our teachers will be of little power, eſpecially among our youth and riſing generations, who do not yet favour the things of the Goſpel of Jeſus Chriſt; and among ſtrangers, who have not yet taſted how good the Lord is, though for their protection and ſafety they have crowded in upon us.

Noble-hearted Sir, your gift I do ſtill religiouſly keep, for ſome ſpecial and eminent ſervice of the Lord, in the Lord's time. In our firſt war with the Indians, God pleaſed to ſhew us the vanity of our military ſkill, in managing our arms, after the European mode. Now we are glad to learn the ſkulking way of war. And what God's end is, in teaching us ſuch a way of diſcipline, I know not. By our late eaſtern war it hath pleaſed God to ſhew us our weakneſs by ſea, as formerly by land. The Indians took many of our fiſhing veſſels, and the men that belonged to them, and forced them to ſail whither they deſired: many of the men delivered themſelves and their veſſels; many Indians were ſlain, ſome Engliſh. The hiſtory of theſe actions I have not: others do attend that ſervice, to whom I leave it. The governor of *New-York* ſent a ſtrength this ſummer, and took poſſeſſion of a northern port, where they fixed and fortified themſelves: ſince whoſe coming thither, the Indians have not ſtirred much. Little action hath paſſed, but I hear not of any peace made. The Yorkers have taken in hand a chargeable deſign: what profit will come of it, I know not; time will diſcover that: whether their intention be to promote religion, or only trading, I know not. It pleaſed the Lord, very lately to permit a ſmall handful (not twenty) of the late ſcattered rod to make a ſore direption upon *Hatfield* and *Dearfield*, at *Connecticut*; where about twelve perſons were killed, more than twenty carried

carried away captive, or loſt; ſeven dwellings burned, and ſundry barns full of corn; and ſince they have appeared at *Hadly*, burned the mill. They had parly with them, treated about reſtoring the captives, agreed of a time and place of meeting; but the Indians failed to appear. Theſe laſt actions have very much diſcouraged our people from repairing the deſtroyed towns, which ſome were beginning to do. We had a Sachem of the greateſt blood in the country, ſubmitted to pray to God, a little before the wars: his name is *Wanalaunſet:* in the time of the wars he fled, by reaſon of the wicked actings of ſome Engliſh youth, who cauſeleſsly and baſely killed and wounded ſome of them. He was perſuaded to come in again. But the Engliſh having ploughed and ſown with rye all their lands, they had but little corn to ſubſiſt by. A party of French Indians (of whom ſome were of the kindred of this Sachem's wife) very lately fell upon this people, being but few and unarmed, and partly by perſuaſion, partly per force, carried them all away. One with his wife, child, and kinſwoman, who were of our praying Indians, made their eſcape, came into the Engliſh, and diſcovered what was done. Theſe things keep ſome in a continual diſguſt and jealouſy of all the Indians. I ſhall give your honour no farther trouble at preſent. We intreat your prayers, and commit you to the Lord, and reſt

Your honour's to ſerve you in the Lord Jeſus,

John Eliot.

III.

Roxburg, Nov. 4, 1680.

Right honourable, charitable, indefatigable, nurſing father,

WHEN good works of pure charity are ſown three hundred fold thick, and that by a living hand, Lord what a reaping time or harveſt will there be! Sir, you are eminently mindful of that Goſpel charge, 1 *Tim.* vi. 17, 18, 19. *Charge them, that be rich in this world, that they be not high-minded, nor truſt in uncertain riches, but in the living God, who giveth us richly all things to enjoy.* That they do good, that they be rich in good works, ready to diſtribute, willing to communicate, laying up in ſtore for themſelves a good foundation againſt the time to come; a foundation not of grace unto juſtification by way of merit, but a foundation of degrees of glorification, when God will in free mercy diſtribute his gifts of glory, according to our improvements of our talents in the exerciſe of grace: he that gained ten talents, ſhall have ten cities.

I KNOW it will pleaſe your charitable heart to hear how it fareth with thoſe that are your alumni. We are in great affliction by the Manquaoy Indians: more than 60 at ſeveral times have been killed or captived; a narrative whereof major *Gookins* preſented to lord *Culpepper*, who was affected with it. Alſo he preſented a copy thereof to Sir *Edmond Andros*, who was likewiſe affected with it, though it is ſaid, that he might have prevented it. We hope he will move in it, and our Mr. *Pinchon* is gone up to join with Sir *Edmond* to endeavour a peace. Major *Gookins* intendeth to preſent your honour with a copy of the ſame narrative. The Eaſtern Indians do offer to renew peace with us, and to ſubmit themſelves to be taught to pray unto God. A chief Sachem was here about it, a man of a grave and a diſcreet countenance. Our praying Indians, both in the iſlands, and on the main, are (conſidered together) numerous; thouſands of ſouls, of whom ſome true believers, ſome learners, and ſome are

are ſtill infant; and all of them beg, cry, intreat for bibles, having already enjoyed that bleſſing, but now are in great want. Your honour's liberality in Engliſh bibles is a great favour, which we all with thankfulneſs receive; but the bible in their own tongue muſt help them to underſtand it. We are at the 19th chapter of the Acts; and when we have impreſſed the New Teſtament, our commiſſioners approve of my preparing and impreſſing alſo the old. Your honour's bounty of 30 *l.* towards our ſending the Goſpel to thoſe remote Indians that ſpeak the language, wherêunto the bible is tranſlated, I do religiouſly keep it, to be improved to the ſame end, to which your honour gave it, of which ſervice I am ſtill in hope, having more intelligence that there is ſuch a people.

But by the immaturity of ſome occurrences, and the intentions of the ſhip's ſpeedy ſailing, I cannot give your honour any further diverſion at this time: entreating your prayers, I commit you to God, and reſt,

Your honour's to ſerve you in any ſervice of Jeſus Chriſt,

John Eliot.

IV.

Roxburg, March 15, 1682-3.

Right honourable, charitable, nurſing father,

THIS winter the worſhipful Mr. *Stoughton* (commiſſioner) delivered to Major *Gookins* (a pillar in our Indian work) and to me, the ſum of 6 *l.* as the product of your honour's gift of charity; which we did diligently diſtribute to Chriſtian Indians, two aged blind women, others lame in their limbs, others decrepid with age; all which do bleſs you the giver, and do praiſe God the fountain; and we, your diſpenſers of ſo great charity, do thankfully accept of ſo good an office, as to be the diſpoſers of ſo charitable gifts unto the poor ſervants of Jeſus Chriſt.

The Lord's work ſtill goeth on among them, and though many of the younger ſort, ſince the wars (where their ſouls received a wound) have declined, and too much miſcarried, yet now (through the grace of Chriſt) they are on the repenting and recovering hand; of which your honour may hear more, when the work is proſecuted, and brought unto a good effect.

The great work, that I travel about, is, the printing the Old Teſtament, that they may have the whole bible. They have had the whole in the firſt impreſſion, and ſome of the old they ſtill have, and know the worth and uſe of it; and therefore they are importunately deſirous of the whole. I deſire to ſee it done before I die, and I am ſo deep in years, that I cannot expect to live long: beſides, we have but one man (*viz.* the Indian printer) that is able to compoſe the ſheets, and correct the preſs, with underſtanding. For ſuch reaſons, ſo ſoon as I received the ſum of near 40 *l.* for the bible work, I preſently ſet the work on foot; and one tenth part or near is done: we are in *Leviticus.* I have added ſome part of my ſalary to keep up the work, and many more things I might add, as reaſons of my urgency in this matter. Touching thoſe remote Indians, to the North-Weſt, whoſe language agreeth with ours, ſo that they and we can ſpeak to each other's underſtanding, we have not, as yet, ſo full intelligence of them, as to make a report thereof. But I do both pray and wait for ſome information that way. And for the furtherance thereof, I do carefully reſerve your honour's gift of 30 *l.* to be improved in that ſervice, when the Lord

Lord ſhall pleaſe to open a door thereunto. The Manquaoy Indians have not ſtirred to fall upon us this laſt year; but we are not yet fully ſettled in peace, becauſe they declare the Eaſtern Indians to be their enemies; and the way unto them is thorough us; and our Wameſet Indians, who are our moſt northerly plantation, are in danger to be their thoroughfair. And this putteth us into many fears; but our hope and help is in God, our eyes are unto him; this world is a place and ſtate, wherein God's people muſt expect nothing ſtedfaſt, all things mutable and afflicting. But I ſhall ceaſe to give your honour any farther trouble at preſent; therefore, commending you to the Lord, and to the word of his grace, I reſt,

Your honour's to ſerve you in Chriſt Jeſus,

John Eliot.

V.

Boſton, June 21, 1683.

Right honourable nurſing father,

YOUR hungry alumns do ſtill cry unto your honour for the milk of the word in the whole book of God, and for the bread of life, which they have fed upon in the whole bible, and are very thankful for what they have, and importunately deſirous to enjoy the whole book of God. It is the greateſt charity in the world to provide for their ſouls. Should your honour pleaſe but to change the object of your bountiful charity, from their bodies to their ſouls, here is enough already ſent over to accompliſh the work; they only ſtay for that word from your honour, *fiat*. My age makes me importunate. I ſhall depart joyfully, may I but leave the bible among them, for it is the word of life; and there be ſome godly ſouls among them, that live thereby. The work is under great incumberments and diſcouragements. My heart hath much ado to hold up my head; but doth daily drive me to Chriſt; and I tell the Lord, that it is his word, and your hearts are in his hand. I do therefore commit the whole to the Lord, and leave both it and myſelf to the Lord, who hath not left me wholly deſtitute. But I ſhall give your honour at preſent no farther trouble, for I am ſurpriſed with this opportunity of writing: therefore, committing your honour to the Lord, I reſt,

Your honour's to ſerve you in the Lord,

John Eliot.

VI.

Roxburg, Nov. 27, 1683.

Right honourable, right charitable, and indefatigable, nurſing father,

ALTHOUGH my haſty venturing to begin the impreſſion of the Old Teſtament before I had your honour's *(fiat)* may have moved (as ſome intimate) ſome diſguſt, yet I ſee that your love, bounty and charity, doth ſtill breathe out encouragement unto the work, by ſupplies of 460 *l.* unto the work, for which I do return my humble thankfulneſs to your honour, and take boldneſs to intreat favour for two requeſts.

First, I pray, that you would please to accept an apology for my haste. I am deep in years, and sundry say, if I do not procure it printed while I live, it is not within the prospect of human reason, whether ever, or when, or how, it may be accomplished. It is Christ's work, and for the good of souls, which is my charge to attend, and run adventures to accomplish, especially when divine providence brought into my hand some small encouragement to begin. But if this apology be short (though capable of much enlargement) yet then,

My second humble request is, that you would please to draw a curtain of love over all my failures, because love will cover a multitude of transgressions. The work goeth on now, with more comfort, though we have had many impediments, partly by sickness of the workmen, for it is a very sickly and mortal time with us, as also the rigour of the winter doth now obstruct us. The work goeth on, I praise God; the sabbath is sanctified in many places, and they have still fragments of their old bibles, which they make constant use of.

I desire to take boldness to propose a request. A vessel carried away a great number of our surprised Indians, in the times of our wars, to sell them for slaves; but the nations, whither he went, would not buy them. Finally, he left them at *Tangier*; there they be, so many as live, or are born there. An Englishman, a mason, came thence to *Boston*, he told me, they desired I would use some means for their return home. I know not what to do in it; but now it is in my heart to move your honour, so to mediate, that they may have leave to get home, either from thence hither, or from thence to *England*, and so to get home. If the Lord shall please to move your charitable heart herein, I shall be obliged in great thankfulness, and am persuaded, that Christ will, at the great day, reckon it among your deeds of charity, done unto them, for his name's sake. But I shall give your honour no farther trouble at present, I humbly request your prayers for me. So, commending you to the Lord, and to the word of his grace, I rest,

Your honour's to serve you in our Lord Jesus,

John Eliot.

VII.

Roxburg, April 22, 1684.

Right honourable and indefatigable benefactors,

THIS last gift of 400 *l.* for the reimpression of the Indian Bible doth set a diadem of beauty upon all your former acts of pious charity, and commandeth us to return unto your honours all thankful acknowledgments, according to our abilities. It pleased the worshipful Mr. *Stoughton*, to give me an intimation, that your honours desired to know the particular present estate of the praying Indians; and also, when *Moses*'s pentateuch is printed, to have some copies sent over, to evidence the real and good progress of the work.

Your honours intimation hath the force of a command upon me, and therefore I shall briefly relate the religious walking and ways of the praying Indians. They do diligently observe and keep the sabbath, in all the places of their public meetings to worship God. The example of the English churches, and the authority of the English laws, which Major *Gookins* doth declare unto them, together with such mulcts as are inflicted upon transgressors; as also and especially, the clear and express

command of God, which they and their children learn and rehearse daily in their catechisms; these all together have fully possessed and convinced them of their duty, to keep holy the sabbath day. So that the sanctifying of the sabbath is a great and eminent part of their religion. And though some of the vain and carnal sort among them are not so girt to it, as were to be desired, yet the grave and religious sort do constantly worship God, every sabbath day, both morning and evening, as the English do.

THE acts of worship, which they perform in their public meetings, are as followeth.

THE officer beginneth with prayer, and prayeth for all men, rulers, ministers, people, young, old, sick, well, English or Indians, *&c.* according to that word, 1 *Tim.* ii. 12. *I will that first of all prayers be made*, &c. I say, the officer beginneth with prayer, *viz.* where they have an officer ordained, as it is almost in all the churches. But we have more public assemblies, that meet every Lord's day, to worship God, than we have churches. There is not yet a church gathered in every place, where they meet to worship God and keep the sabbath; but where it is so, they chuse some able godly man (the best they can) to manage the worship among them: him they call their teacher, and he beginneth with prayer, *&c.* When prayer is ended, they call forth such as are to answer the catechism; and though this is sometimes omitted in some places, yet that is the way they walk in, and it is often practised. When catechism is ended, a chapter is read, sometimes in the Old Testament, and sometimes in the New; and sundry of the young men are trained up, and called forth to this service, sometimes one, sometimes another.

WHEN the chapter is read, a psalm is sung, which service sundry are able to manage well.

THAT finished, the preacher first prayeth, then preacheth, and then prayeth again. If it be the day for the Lord's Supper to be celebrated, the church address themselves unto it, and the minister doth exactly perform it, according to the scriptures. When that service is done, they sing a psalm, according to the pattern of Christ; then he blesseth the church, and so finisheth the morning service.

IN the afternoon they meet again, and perform all the parts of worship, as they did in the morning; which done, if there be any infant to be baptised, they perform that service according to the scriptures; which done, the deacon calleth for contributions; which done, if there be any act of public discipline (as divers times there is, there being many failures among us) then the offender is called forth (being with care and diligence prepared) and is exhorted to give glory to God, and confess his sin; which, being penitent, they gladly accept him, forgive him, and receive him. If it be not a satisfactory confession, they shew him his defect, they admonish and exhort him to a more full confession; and so he is left to some other time. This finished, he blesseth the church, and so dismisseth the assembly.

MOREOVER, major *Gookins* hath dedicated his eldest son, Mr. *Daniel Gookins*, unto this service of Christ; he is a pious and learned young man, about thirty-three years old, hath been eight years a fellow of the college; he hath taught and trained up two classes of our young scholars, unto their commencement; he is a man whose abilities are above exception, though not above envy. His father, with his inclination, advised him to *Sherborn*, a small village near *Natik*, whose meeting-house is about three miles, more or less, from *Natik* meeting-house. Mr. *Gookins* holdeth a lecture in *Natik* meeting-house once a month; which lecture many English, especially of *Sherborn*, do frequent. He first preacheth in English, to the English audience, and

then the ſame matter is delivered to the Indians by an interpreter, whom, with much pains, Mr. *Gookins* hath fore-prepared. We apprehend, that this will (by God's bleſſing) be a means to enable the Indians to underſtand religion preached in the Engliſh tongue, and will much further Mr. *Gookins* in learning the Indian tongue. Likewiſe major *Gookins* holdeth and manageth his courts in the Engliſh tongue; which doth greatly further the Indians in learning law and government in the Engliſh tongue; which is a point of wiſdom, in civilizing them, that your honours have manifeſted your deſires that it might be attained.

The places where the Indians meet to worſhip God, and ſanctify the Sabbath, are many; the moſt are ſtated places, others are occaſional. The ſtated places, in the *Maſſachuſets*, ſince the wars, are contracted into four, *Natik*, *Poukipoy*, *Wameſut*, and *Chachaubunkkakowok*. The occaſional meetings are at places of fiſhing, hunting, gathering cheſnuts in their ſeaſons. Alſo ſince the wars, the Manquaoys, making incurſions upon the praying Indians, did cauſe to make divers forts, to live ſafely in, and then they did there meet to worſhip God, and keep the Sabbath.

In *Plymouth Patent* there are about ten places, where they meet to worſhip God.

An intelligent perſon, of *Martyn*'s vineyard, reckoned up unto me ten places, where God is worſhipped every Lord's day in that iſland.

At *Nantukket* there be about five places of prayer and keeping ſabbath.

The reaſon of this diſperſion of places of public meeting to worſhip God, is this; there is but here and there a ſpot of good land, fit for planting corn, with accommodation of fiſhing; theſe ſpots of good land lie at a great diſtance from each other; ſome four or five miles, ſome eight or nine miles, ſome ten or twelve miles: ſo that it is impoſſible for them, eſpecially with women and children, to meet at one place; therefore all that live together at one place, meet to worſhip God on the ſabbath day.

Thus I have briefly repreſented before you, right honourable, at your command, the preſent eſtate of the praying Indians, in reſpect of their religion. And what I have here expreſſed, for the ſubſtance of the things, I know them to be true, and I have often ſo practiſed among them.

By this it appeareth, that they are, in ſome good meaſure, able (by the light of the ſcriptures, and by the example of the churches of Chriſt, and by ſuch inſtruction as they have had) to practiſe and manage the whole inſtituted public worſhip of God among themſelves, without the preſence or inſpection of any Engliſh among them, which is no ſmall addition and advancement of the kingdom of Chriſt; and I doubt not but it ſhall add much comfort and joy to your ſouls here, and ſhall add much weight of glory to your ſouls hereafter, who have been ſo diligent, liberal and conſtant in your ſupplies for the encouragement of this work of Chriſt.

And it is no ſmall comfort to me, whom divine providence and grace hath made one of the poor inſtruments, to inſtruct and manage them unto this eſtate in Chriſt Jeſus, whereunto they have attained.

As for the ſending any numbers of *Moſes*'s Pentateuch, I beſeech your honours to ſpare us in that; becauſe ſo many as we ſend, ſo many Bibles are maimed and made incomplete, becauſe they want the five books of *Moſes*. We preſent your honours with one book, ſo far as we have gone in the work, and humbly beſeech, that it may be acceptable, until the whole be finiſhed; and then the whole impreſſion (which is two thouſand) is at your honours command. Our ſlow progreſs needeth an apology. We have been much hindered by the ſickneſs this year. Our workmen have been all ſick, and we have but few hands, one Engliſhman, and a boy, and one Indian; and many

many interruptions and diversions do befal us; and we could do but little this very hard winter. But I shall give your honours no further trouble at this time; only requesting the continuance of your prayers and protection. So I remain,

Your honours

to serve you in our Lord Jesus,

John Eliot.

POSTSCRIPT.

THE people of *Natik* have procured some friend of *Sherborn*, to draw up a letter to me, which I make bold to present to your honours view, being here inclosed. If I have been over-bold herein, I beseech your honours to pardon me.

IX.

Roxburg, August 29, 1686, in the third month of our overthrow.

Right honourable unweariable nursing father,

I HAVE nothing new to write but lamentations, and I am loth to grieve your loving and noble soul.

OUR Indian work yet liveth, praised be God; the Bible is come forth, many hundreds bound up, and dispersed to the Indians, whose thankfulness I intimate and testify to your honour. *The Practice of Piety* is also finished, and beginneth to be bound up. And my humble request to your honour is, that we may again reimpose the Primer and Catechism; for though the last impression be not quite spent, yet quickly they will; and I am old, ready to be gone, and desire to leave as many books as I can. I know not what to add in this distressing day of our overthrow; so I commit your honour to the Lord, and rest,

Your honour's

to serve you in Jesus Christ,

John Eliot.

X.

Roxburg, July 7, 1688.

Right honourable, deep learned, abundantly charitable, and constant nursing father,

SIR,

I AM drawing home, and am glad of an opportunity to take my leave of your honour with all thankfulness. Sir, many years since you were pleased to commit 30*l.* into my hand, upon a design for the promoting Christ his kingdom among the Indians; which gift of yours I have religiously kept, waiting for an opportunity so to improve it; but God hath not pleased yet to open such a door. I am old, and desire to finish that matter, and take the boldness to request your honour, that it may be thus disposed of. It being in the hand of major *Gookins*'s relict widow, and he

he died poor, though full of good works, and greatly beneficent to the Indians, and bewailed by them to this day; therefore let his widow have 10*l.* his eldest son, who holds up a lecture among the Indians and English 10*l.* and the third 10*l.* give it to Mr. *John Cotton*, who helped me much in the second edition of the Bible. And also I must commit to him the care and labour of the revisal of two other small treatises, *viz.* Mr. *Shegheard's Sincere Convert* and *Sound Believer*, which I translated into the Indian language many years since; and now I hope, that the honourable corporation will be at the charge to print them, by your honour's favour and countenance. But I cannot commit them to the press without a careful revisal, which none but Mr. *Cotton* is able to help me to perform.

The work in general seemeth to my soul to be in and well toward a reviving. Many churches of confessors of Christ are in motions to gather into church estates, who do carefully keep the sabbath. And out of these professors of religion, we do gather up and call in such as are willing to confess Jesus Christ, and seek salvation by him. Touching other matters, what our losses and changes be, and how trading, *&c.* are spoiled, I am silent; but my prayer to God is, *Isaiah* i. 25, 26. *And I will turn my hand upon thee, and purely purge away the dross, and take away all thy tin, and I will restore thy judges as at the first, and thy counsellors as at the beginning*, &c. So do, O Lord.

Sir, the Lord prolong your days, and fill you with all grace, until you arrive at the fulness of glory, where I leave you, and rest,

Your honour's

to serve you in Jesus Christ,

John Eliot.

NUMBER VI.

Letters of thanks from the Governor, *&c.* of *New England*, to Mr. *Boyle*, for his services to that Colony. *Life*, p. cxxxix.

I.

Governor and General Court of New-England *to Mr.* Boyle.

Honourable Sir,

The occasion of our giving you this trouble, is from the confidence we have of your favour and care of these his Majesty's colonies in *New England*, manifested by your continued endeavours, as in promoting that good work of the natives conversion, so in taking opportunities for ingratiating us with his Majesty and the right honourable the Lord Chancellor, as we understand by your letter to Mr. *Winthrop*, whereby you have given us that comfortable information of his Majesty's grace towards us, in expressing himself in a very favourable manner, and that the Lord

Lord Chancellor did assure you, (with giving you commission to assure our friends in the city) that the King intends not any injury to our charter, or the dissolution of our civil government, or the infringement of our liberty of conscience, and that the doing of those things is not the business of the commissioners; the truth whereof we believe (as we ought) having the word of so gracious a King. But alas! Sir, the commission impowering those commissioners to hear and determine all causes, whether military, civil, or criminal, (what they have further by instruction at present we know not) should this take place, what will become of our civil government, which hath been (under God) the hedge to that liberty for our consciences, for which the first adventurers passed through, and bore up against all difficulties and discouragements that encountered them, as in the way to, so in the continuance in this wilderness? Sir, we return unto you our true and hearty thanks for your former favours, and crave the continuance thereof as opportunity shall offer, and the great mover of hearts shall incline you in appearing our friend still, that, if possible, the commission may be recalled; for which end we have made our humble supplication to his Majesty, in whose eyes, if we may find favour, we and our posterity shall have cause to bless the Lord. But if the decree be past, so that it may not be recalled, we shall wait the Lord's issue with us; and whatever may be the conjectures of any rendering alterations here advisable, the issue will speak them to be the subversion of all that, which makes this place or our abode herein desirable; or if any of those that desire a dominion over us, (not to serve his Majesty's interest in advancing plantation work, with the countenance of godliness, but to serve themselves by his Majesty's authority, and our ruin) shall prevail, it will to posterity be rendered a disservice to his Majesty's honour, and such a damage, as the procurers will not be able to repair. We can sooner leave our place, and all our present outward enjoyments, than leave that, which was the first ground of our wandering from our native country; nor are we thereby made such strangers thereunto, but we can rather chuse to return, and take our lot with our brethren, than abide here under the deprivement of the ends of our travels. Our way is with the Lord. Craving your honour's pardon for this boldness, lifting up our best desires to the Lord for you, we remain,

SIR,

Your humble servants,

in the name and by order of the General Court,
held at *Boston* in *New-England*, Oct. 19, 1664.

Jo. Endecott, Governor.

II.

Mr. Boyle *to* Henry Ashurst, *Esq*;

SIR,

I HAD sooner sent you an account of your letter, but that, although I delivered the petition to my Lord Chancellor, soon after I received it, and have waited on his Lordship several times since for an answer, yet I found him always engaged in business, either with the King, or Duke, or such great persons, as made me think it improper

improper to desire to be admitted. But being there this morning somewhat early, some of his relations, that saw me there, would needs bring me in, before I expected it, though a great person intervening obliged us to discourse the most part aloud, till the Portugal resident surprizing my Lord Chancellor with a request to have audience, put an untimely end to our conference.

P. S. I FORGOT to tell you, that it is not all the New-Englanders, but only the Massachusets, that have disrespectfully used the commissioners, to whom the rest of the colonies have behaved themselves otherwise. And when I speak civilly to my Lord Chancellor of your friend Mr. *Winthrop*, his Lordship assented to the character I had given him.

III.

Governor and Deputy Governor, &c. *of* New-England *to Mr.* Boyle.

Honourable Sir,

AS an addition to your former kindness, touching the present of masts sent from this colony to his Majesty, we are lately informed, that you have been pleased to speak on our behalf in the ears of his most excellent Majesty, our gracious Sovereign, when our adversaries, by their hard speeches and false suggestions, have laboured to alienate his royal heart and affections from us; which favour of yours (to a people that are so great strangers to you, and so undeserving your love) calls for gratitude. And therefore, should we be silent in our most thankful acknowledgment thereof, first unto God, that hath so inclined your heart, and nextly, to yourself, as an instrument (and if we may presume to say) an advocate for this part of God's poor church in the wilderness, it would render us most unworthy of our profession.

SIR, we need not put you in mind, that the poor church of Christ, in all ages, even from righteous *Abel*'s time unto this day, hath not wanted adversaries: the ancient enmity, which God hath put between the two seeds, will never reconcile; that example in *Ezra*'s and *Nehemiah*'s time do sufficiently evince this; for although the people of God then had ample charters from those great princes, *Cyrus* and *Artaxerxes*, yet God was pleased, for the trial of his church, and the illustration of his own glory (in their salvations) to permit a *Sanballat*, a *Tobiah*, and others, falsely to accuse that people to those princes of disloyalty. Sir, we hear that our adversaries there are plotting and designing against our peace; so much the more cause have we to lie in the dust before the Lord, imploring his assistance and salvation, as the matter shall require. And also it is our duty not to neglect the use of that little means that is left us, in order to the preservation of our quietness and liberties; among which, this application to yourself, and by you to our most gracious King (whose royal heart the Lord hath graciously inclined hitherto to favour our righteous cause) is the principal.

SIR, we hear of several things against us, which we do not particularly understand; but so far as is intimated to us, we will make bold here briefly to mention them, with our answers to them.

I. OUR loyalty is questioned. To this we answer (in all humility, not boastingly) that the demonstrations of our loyalty are known to thousands; particularly, 1. We never proclaimed or acted in the name of the late power in *England* in his Majesty's absence, as all other remote colonies did. 2. It is known, that in our public prayers,

as well as in private families, we pray for our King. 3. When a ſquadron of his ſhips, under Sir *John Harman*, commander, were in the *Weſt-Indies* ſtreightened for proviſions, we freely and ſeaſonably ſent a ſhip laden with proviſions for their ſupply. 4. In that preſent of a ſhip laden with maſts, ſent for the ſupply of his royal navy. Thoſe two laſt things coſt this poor colony ſome thouſands of pounds; and we have not heard, that any of his Majeſty's colonies (though far exceeding us in riches) have given higher demonſtrations of their loyalty.

II. WE are ſaid to be factious in the principles of religion. Anſwer. If Mr. *Perkins*, and thoſe good old Puritans in King *Edward* the VIth and Queen *Elizabeth*'s time, did, in their principles of religion, teach evil doctrine (which we conceive no true Proteſtant will ſay) then may we be rendered ſuch; for our religion and principles are the ſame for ſubſtance with thoſe old Chriſtians and reformers called Puritans.

III. IT is ſaid, we are a divided people. We acknowledge it is a matter to be greatly bewailed, that the church of God, all the world over (by reaſon of man's weakneſs and infirmity) doth labour under diverſity of perſuaſions and apprehenſions in matters of religion, and conſequently do not live in that bleſſed and ſweet unity that God requires: but for our parts (ſome petty differences excepted) we bleſs God, we have much peace and tranquillity in church and ſtate.

IV. WE are charged with carrying it diſreſpectfully towards his Majeſty's commiſſioners. To this we ſay, that God and man can witneſs for us, that our treatment of them was with civility, according to our mean conditions. Indeed, as to yielding obedience unto their mandates, which were deſtructive to our royal charter; as that was contrary to his Majeſty's inſtructions and letter ſent to us by them, ſo we had no reaſon to ſubmit to them therein.

V. WE are blamed for a great omiſſion touching baptizing of infants. To this we anſwer, that our principles declared to the world in print, particularly that of the laſt ſynod held here, doth ſpeak our judgments to run parallel with other reformed churches, viz. That viſible confederate believers and their ſeed are ſubjects of baptiſm. Indeed in practice there hath been ſome omiſſions thereof, as to the largeſt extent, eſpecially in ſome places; but endeavours are daily uſed to reduce each one to the rule.

VI. WE are accuſed of rigidneſs to ſuch as differ from us in matters of religion. To this we ſay, that from the firſt ſettling this plantation, theſe heterodoxes of Familiſm, Anabaptiſm, and of late Quakeriſm, have been looked upon by the godly here as great errors, and the promoters of them diſturbers of peace and order. Thoſe awful and tremendous motions of that ſort of people in *Germany*, and elſewhere, hath ſufficiently alarmed all pious and prudent men to provide a defenſative againſt them. Hence, from our firſt times, laws have been made to ſecure us from that danger; which have, at ſome times, upon juſt occaſions, been executed upon ſome of that ſort of people who have exceeded the rules of moderation in matters of practice: but this we may ſay truly, that ſome peaceable Anabaptiſts, and ſome of other ſects, who have deported themſelves quietly, have and do live here, under the protection of this government, undiſturbedly.

LASTLY, we are accuſed for graſping after dominions, more than belongs to us, and in particular, for taking in a place called the Province of *Mayne*, belonging to Mr. *Gorge*. To this we anſwer, that our patent (which is of greater antiquity than his) doth take in that place; and this may be clearly demonſtrated. Again, Mr. *Gorge*'s predeceſſor, finding no profit, deſerted the government thereof, and left the people under ſuch confuſion and diſorder, that they were neceſſitated to petition earneſtly unto the Maſſachuſets, to take them under their government; which they

did, at their earneſt deſire, to prevent their devouring one another. The truth is, there is no profit or benefit doth accrue to our government by their addition, but cumber and trouble. They are generally a very poor people, and contribute nothing to us for the ſupport of government in this place. We may truly ſay, our main end in taking them under us hath been a deſire to do them good, outwardly and inwardly; eſpecially to encourage a pious and able miniſter to live among them, and to preach the Goſpel to them, which, through the favour of God, hath been in ſome meaſure attained. Before they came under us, we know not of one preaching and pious miniſter in five or ſix villages there, and ſince (through God's favour) they have been well provided therewith. Godly miniſters indeed were very ſhy to go among them to live, before they were ſettled under this government; and at ſuch time, when the commiſſioners took them off from us, and ſettled ſome juſtices among themſelves, it was but a little while after the commiſſioners were gone, but that people fell into ſuch diviſions and confuſions, that many of their miniſters left them; and the people again earneſtly ſued to us for protection and government, finding no benefit (as they alleged) by ſuch as the commiſſioners had appointed to rule them. Peradventure Mr. *Gorge* and ſome others may apprehend, they are deprived of honour and profit by us in this matter; but, we believe, as it hath, ſo it will be found, that neither the one nor the other would accrue to them, if they had it under their power, according to their deſire.

THUS, noble Sir, we have made bold to give you an account (as briefly as we could) of what we hear is objected againſt us, and our anſwers; committing all to your goodneſs and wiſdom, to make uſe of as you ſhall ſee occaſion.

So deſiring, in all humility, your pardon for our preſumption, in giving you this trouble, with our cordial prayers unto the God of all mercy and grace, to pour upon your head and heart his richeſt bleſſings; with our moſt humble ſervice and love to you preſented, we take leave, deſiring always to remain,

Your honour's

moſt affectionate friends and ſervants,

John Leverett, Governor.
Samuel Symonds, Deputy Governor.
Daniel Gookins, Aſſiſtant.
Richard Ruſſell.
Thomas Dauforth.
John Pynchon.
William Stoughton.
Edward Tyng.

THIS is a duplicate of a letter ſent in December laſt. Dated in *Boſton* in *New-England*, May 10, 1673.

PREFACES

PREFACES

OF

Mr. BOYLE

TO

BOOKS wrote by other persons, ADVERTISEMENTS, &c.

Mr. *Boyle*'s Preface to *A Tract touching the Skill of a better Way of Anatomy of Man's Body*, by *Yonker Louis de Bills*, Lord of *Koppensdam*, &c. translated from the *Low-Dutch*, by Mr. *Pell*, and printed at *London*, 1659, in 12mo.

To my much esteemed friend Samuel Hartlib *the elder, Esq;*

SIR, October 13, 1659.

YOUR desires were wont to tend so much to the public good, that your virtuosi would think I wanted philanthropy, as well as civility, if I should refuse to comply with such as your letter brings me. I send you therefore the propositions of Monsieur *de Bills*, englished out of Low-Dutch, at my request, by such a person, as you will readily think can translate very well, though he can better write things worthy to be translated, when I should have told you, that his name is Mr. *Pell*. The design avowed to be aimed at in the propositions, and much more the matter of fact, without which they might appear extravagant, made the print, I confess, very welcome to me. For though in some papers I formerly told you of, I had mentioned divers things I had intended to try, and some, that I had already observed, in order to the preservation of animal substances, and the making some of them more durable subjects for the anatomist to deal with; yet besides that most of the ways I proposed to myself were as yet little more than bare designs, I never aimed at (and scarce so much as hoped for) such strange things, as in this paper the ingenuous publisher of it tells us he hath already actually performed. And therefore I suppose, that it will not be unwelcome to you, if not having had the opportunity to see the effect of this gentleman's skill myself, and being sufficiently indisposed to believe or affirm any strange thing that I have not seen, I annex the other paper you lately desired of me; I mean that, wherein the proposer's Dutch print came inclosed

to me from the *Hague*. For besides that it contains something of particular, it comes from a French gentleman, whose testimony may well be considerable in matters of this nature; for he is one of that select society of Parisian philosophers, of which you and I have sometimes discoursed; and you will find a celebrating mention made of him in the life of *Gassendus*, whose friend and disciple he was. And his curiosity leading him to imitate divers of the old Greek philosophers, who travelled through many countries, only to enrich themselves with what knowledge they could meet with in them, he went purposely to visit Monsieur *de Bills*, soon after he had left *England*, which he lately traversed; and in his passage honoured me with several of his visits. I am so willing that you should be as wary as I in admitting unlikely things into your belief, that, in putting the French I received into English, I have been (purposely to avoid straining his expressions) more careful to do right to his words, than to his stile. And as for those things, that appear to have been left out by me, they are some of them such matters as concern not our proposer, and others but such compliments, as require rather my blushes, than my translating. The consent you desire of me to publish the proposals in our language I must not refuse you. For it is the least we owe to a person, that hath endeavoured to oblige mankind, and to those that are willing to encourage such endeavours, by acquainting the ingenious here with his propositions, to strive to procure him subscribers, and to afford them the opportunity of encouraging and assisting so useful a work, as this (supposing the truth of the historical part of the paper) is like to prove. It may also be hoped, that the noise of such discoveries abroad will rouse up our ingenious men at home, and excite them to endeavour to emulate, if not surpass them; and to keep up that reputation for anatomical discoveries and skill, that this nation hath of late deservedly enjoyed. Nor will it be amiss, by the same means, to give as many of those in authority, as may happen to read what the states of *Holland* have done, in favour of a stranger, occasion to take notice, what countenance neighbouring magistrates, that are not thought unpolitic neither, are forward to give to those designs, that aim at the advancement of real learning. Which may perhaps invite those that can do it, to give encouragement to the English wits; which I am groundedly confident want nothing but encouragement, to perform things in this kind, that would really advantage the public, as well as highly gratify such persons as Mr. *Hartlib*, and his

affectionate humble servant,

Robert Boyle.

A LETTER

A

LETTER

Prefixed to a Tract tranſlated from French into Engliſh,

BY

Dr. PETER du MOULIN,

ENTITLED,

The DEVIL of MASCON:

The fourth edition of which was printed at *Oxford*, 1669, in 12mo.

To my reverend and learned friend, Dr. Peter du Moulin.

SIR,

THOUGH I ſuppoſe you will look upon my ſending you Monſieur *Perreaud*'s French book, as a minding you of the promiſe you were the other day pleaſed to make me of putting it into an Engliſh dreſs; yet I hope you will do me the right to believe, that if the ſubject were not extraordinary, and if my own pen were not (as you know it is) pre-engaged to a theme of a very diſtant nature, I ſhould think it injurious to the public, and to you, to be acceſſary to his turning tranſlator of another's books, that hath already manifeſted in ſeveral languages, how able he is to write excellent ones of his own.

I MUST freely confeſs to you, that the powerful inclinations, which my courſe of life and ſtudies hath given me to diffidence and backwardneſs of aſſent, and the many fictions and ſuperſtitions, which (as far as I have hitherto obſerved) are wont to blemiſh the relation, where ſpirits and witches are concerned, would make me very backward to contribute any thing to your publiſhing, or any man's believing, a ſtory leſs ſtrange than this of Monſieur *Perreaud*.

BUT the converſation I had with that pious author during my ſtay at *Geneva*, and the preſent he was pleaſed to make me of this treatiſe before it was printed, in a place where I had opportunities to enquire both after the writer, and ſome

paſſages of the book, did at length overcome in me (as to this narrative) all my ſettled indiſpoſedneſs to believe ſtrange things. And ſince I find, that you have received an account both of Monſieur *Perreaud* himſelf, and ſeveral things relating to his book, from that great ſcholar and excellent perſon your father, I have no reaſon to doubt, but that as your ſkill in the tongues, out of which and into which this treatiſe is to be tranſlated, will bring it the greateſt advantages, that it can receive from a tranſlator's pen; ſo the reputation, which your and your learned father's names will give it, will prove as effectual as any thing of that nature can be, to make wary readers as much believe even the amazing paſſages of it, as I hope you do that great truth of my being in a high degree,

SIR,

your affectionate friend and humble ſervant,

Robert Boyle.

An Advertiſement of Mr. *BOYLE*'s,

About the loſs of many of his writings, addreſſed to *J. W.* to be communicated to thoſe of his friends, that are virtuoſi; which may ſerve as a kind of preface to moſt of his mutilated and unfiniſhed writings. *London*, printed in May, 1688 two pages, folio.

AS for the report, that doubtleſs has reached your ears, of the loſs of ſeveral of my manuſcripts, and the defacing of divers others, it is but too true; and I am very ſenſible of it. But yet it is not barely upon my own account, that I am ſo, but very much upon that of my inquiſitive friends, and Mr. *J. W.* in particular. For I cannot be but troubled, that I find myſelf diſabled to anſwer the expectation they had, that I ſhould gratify their curioſity, by entertaining them with ſeveral tracts upon philoſophical ſubjects; and that ſome unwelcome accidents, that have of late befallen me, oblige me to diſſuade them from expecting hence-forward, that I ſhould preſent them with almoſt any treatiſe, finiſhed and entire. For having been for many years afflicted with a weakneſs of ſight, that neceſſitated me, inſtead of writing myſelf, to dictate to others; and having been neceſſitated to make ſeveral removes, ſome of them with too much haſte to permit me to take an exact care of my papers, or keep all of them together, and take them along with myſelf, from place to place: when, not long ſince, I had occaſion to review and range them, I found, to my ſurprize, as well as trouble, that I wanted four or five centuries of experiments of my own, and other matters of fact, which from time to time I had committed

committed to paper, as they were made and obſerved, and had been, by way partly of diary, and partly of adverſaria, regiſtered and ſet down, one century after another, that I might have them in readineſs to be made uſe of in my deſigned treatiſes. And together with theſe matters of fact, I found miſſing ſeven or eight centuries of notions, remarks, explications and illuſtrations of divers things in philoſophy, which I had committed to writing, as they chanced to occur to my thoughts, and which might have place among the ſame papers with the above mentioned experiments. How all theſe ſhould come to be loſt, whilſt ſome other centuries of notes and ſhort memoirs, ſome of them ſpeculative, and others experimental, eſcaped, I can as little declare, as recover them. But to add to the misfortune (for ſuch it is to me, though perhaps not to the world) one, whom I had ordered to do ſomething with a bottle of oil of vitriol, unluckily broke the glaſs juſt over a flat cheſt of drawers, which I had purpoſely cauſed to be made for no other uſe, than to keep in it my own manuſcript papers, whereof it had then good ſtore. And though I happened to be at that time in the room, and made haſte to unlock the diſtinct drawers, and take them out; yet the highly corroſive liquor had made ſuch haſte and ſuch havock, that ſeveral manuſcripts, and among them ſome that I moſt valued, were quite ſpoiled; inſomuch that there remained not words enough undefaced to declare what ſubject they concerned; and that the other manuſcripts, that miſchievous liquor had reached to, had ſome of them their leaves half conſumed, and others a greater or leſſer part of them; and all that the menſtruum touched, it made ſo rotten, that, notwithſtanding all our diligence, what was once wetted, could never be retrieved.

IT was natural enough, that this concourſe of miſchances ſhould ſuggeſt to me, that I was to take new meaſures in reference to my deſigned but yet unpubliſhed writings. For firſt it ſeemed reaſonable, that either I ſhould wholly ſuppreſs ſome diſcourſes, wherein I had made a conſiderable progreſs, but had not finiſhed them, or elſe ſhould at leaſt acknowledge and give notice they are incomplete, and blemiſhed with divers chaſms, ſince a great many particulars are loſt, that ſhould have done more than fill up thoſe vacancies: which defects I cannot now ſupply, many of the experiments having been made, when I had by me ſome ſuch drugs and other materials, and ſuch exact inſtruments and ſkilful workmen, as I am not now furniſhed with, nor am able to retrieve. Beſides that, I was then alſo ſometimes befriended by opportunities and favourable circumſtances, that I cannot hope for again.

To this firſt reflection it was natural to add another; which was, that ſince I could neither diſcover nor imagine, how moſt of the writings lately mentioned (with ſome others about differing ſubjects) came to be loſt; the ſureſt courſe (if not the only ſure one) that I could take to prevent the like accidents for the future, would be to publiſh from time to time, as faſt as conveniently I could, thoſe remains and fragments, as well as leſs mutilated papers, that yet continued in my hands, premiſing to each diſtinct bundle of them an admonition to thoſe readers, that care for no books, that are not methodical and complete, that they were not only free, but deſired to paſs theſe by, as pieces both confuſed and unfiniſhed.

I MIGHT add, that perhaps it may be more prejudicial to the author than to the reader, that theſe papers come forth with ſuch diſadvantageous circumſtances; ſince, for the moſt part, the method of writings, that treat about experimental philoſophy, is not much minded and remembered by the reader, at leaſt after the firſt peruſal; the notions and experiments themſelves abſtracting from the order they were delivered in, being the things, that philoſophers uſe to take notice of, and permanently

retain in their memories. The introductory discourses and prefaces to be met with among some of the very incomplete papers that accompany this letter, may perhaps not be unwelcome nor altogether useless to some ingenious men, who will not be displeased to find themselves excited, and perchance somewhat assisted, to take particular notice of some subjects, that seem worthy of being more thoroughly considered and cultivated than yet they have been; and some perchance may think the designs I had upon some subjects, not unfit to be pursued by them in their own way and stile. And as for these heaps of fragments, that seem to be more of a chaotic nature (if I may so speak) since the particulars they mainly consist of, are matters of fact, their being huddled together, without method, (though not always without order) may not hinder them from being fit, if well disposed of, to have places somewhere or other in the history of nature; and to become not unserviceable materials in the structure, that is aimed at in this age, of an useful and well grounded philosophy.

A TABLE

A TABLE of CONTENTS of the SIX VOLUMES.

The CONTENTS of the FIRST VOLUME.

The

The CONTENTS of the SECOND VOLUME.

The CONTENTS of the THIRD VOLUME.

V. Trials

XV. Tracts

XXVII. New

The CONTENTS of the FOURTH VOLUME.

The CONTENTS of the FIFTH VOLUME.

1. Some

The CONTENTS of the SIXTH VOLUME.

In the FIRST VOLUME, after the *Life* and *Appendix*, and before the *New Experiments*, are to be inserted the following Pieces.

I. A Preface by Mr. *Boyle*, to a Tract touching the Skill of a better way of Anatomy of Man's Body, by *Yonker Louis de Bills*, Lord of *Koppensdam*, &c. translated from the *Low Dutch* by Mr. *Pell*. *London* 1649, in 12mo.

II. A Letter prefixed to the fourth Edition of a Tract translated from the *French* by Dr. *Peter du Moulin*, intitled, *The Devil of Mascon*. *Oxferden* 1669, in 12mo.

III. An Advertisement of Mr. *Boyle*'s about the Loss of many of his Writings, addressed to *J. W.* to be communicated to those of his Friends that are *Virtuosi*; which may serve as a kind of a Preface to most of his mutilated and unfinished Writings. *London* 1688, Fol.

A List of the Titles of the MSS. of Mr. BOYLE, not inserted in the Six Volumes, for the Reasons mentioned in the Preface.

N. B. Those marked with an * Asterisc are missing.

THEOLOGICAL.

1. Considerations about some of the Causes of Atheism.
2. Four Conferences about as many grand Objections made by Philosophical Wits against the Christian Faith.
3. An Invitation to endeavour the Conversion of Infidels, to which is annexed the good Offices Revelation does to Reason.
4. A short Discourse, shewing that the great Diversity of Religions ought not much to stagger a well-grounded Christian.
5. Some Considerations about Miracles, as made use of as Proofs for the Christian Religion.
6. Memorials to the introductory Preamble for Mr. *Boyle*'s Theological Tracts.
7. Imperfection of human Reason discovered by its own Light. A Dialogue.
8. Mutual good Offices, that Reason and Revelation do each other.
9. Whether Theology and Philosophy contradict each other.
10. Excellency of Christianity.

11. Essay

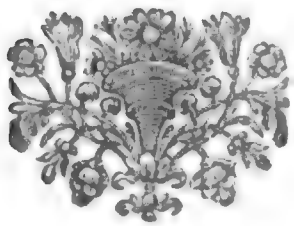
11. Essay on the Scripture. *N. B.* From this the Considerations touching the Style of the Scriptures were partly selected.
12. On Meditation.
13. Duty of a Mother to nurse her own Child.
14. The Daily Reflector, to Lady *Ranelagh*.
15. An Essay to shew in what sense Reason is, and in what it is not contrary to Revelation. A Dialogue.
*16. Whether true Miracles do so much repeal or overthrow the Course of Nature, as is wont to be believed.
17. Papers on the Martyrdom of *Theodora*, *&c.* never printed.
18. Essays on Ethicks, three Books. 1645.
19. Essays on Sin and Piety.

PHILOSOPHICAL.

1. Several Centuries of Experiments and Observations.
2. A second and third Part of *Experimenta & Observationes Physicæ.*
3. Chymistry, its Usefulness to human Life—to Trades, *&c.*
4. History of Colours.
5. History of human Blood, an Appendix to the published Tract.
6. A Philosophical Chaos.
7. Chymico-Medical Chaos.
8. Uses and Bounds of Experience in natural Philosophy.
9. The Utility of Experiments speculative and active.
10. *Cogitationes Physicæ Miscellaneæ.*
11. Of compiling a Natural History; the Imperfection of—the Usefulness of, *&c.*
12. Natural History of Tin, *&c.* of Air.
13. Of Fire and Flame, of the Fuel of the Sun's Fire, Mechanical Origin of Inflammability.
14. Elementary Bodies.
*15. Origin of Minerals and Metals—Conferences about Transmutation and Melioration of Metals.
16. Of Poisons, and of turning them into Antidotes.
17. Of Pores of Bodies, Observations and Figures of Corpuscles, *&c.*
18. Requisites of a good Hypothesis.
19. *Materia Medica*, a second Tome.
20. A practical Essay about parable Medicines.
*21. A Collection of parable Medicines.
*22. *for the Eyes.
23. Of Specifics, tending to shew they are not inconsistent with mechanical Philosophy.
24. An Invitation to Physicians to make use of simple Medicines.
25. A Chemical and Mechanical Examen of *Materia Medica.*
26. Paradoxes about Phlegms.
27. Seminal Principles.
28. Seeds of Plants, Animals, *&c.*
29. Hydrostatics, being an Appendix to Hydrostatic Paradoxes.
30. Examen of one Cause of the Magnetism of the Earth.
*31. The Sceptical Naturalist.
*32. Considerations touching occult Qualities.

*33. On ſpontaneous Generation.
34. Strange Reports; concerning the ſecond Sight, Relations communicated.
35. A large Collection of Things learned by converſing with Sea-captains, Pilots, *&c.* who have traded to the *Indies*, Eaſt and Weſt, to *Hudſon's Bay*, *&c.*
36. MS. Copies of many of his Tracts turned into *Latin*.
37. A great number of Letters from learned Perſons abroad in *Latin*, *French*, *Italian*, *Dutch*, *&c.* The Number of Letters of all ſorts, printed and unprinted, beſides many ſuppoſed to be loſt, amounts to 1500, and more.

N. B. There are many ſubordinate Titles of Tracts, comprehended under the more general ones above-mentioned, both Theological and Philoſophical, omitted for the ſake of Brevity.



Directions to the Binder.

The Sixteen Copper-Plates are to be placed at the End of the Volumes, to which they reſpectively belong.

NEW EXPERIMENTS PHYSICO-MECHANICAL,

TOUCHING

The SPRING of the AIR, and its EFFECTS;

Made, for the moſt Part, in a

NEW PNEUMATICAL ENGINE.

Written by Way of LETTER

To the Right Honourable CHARLES Lord Viſcount of DUNGARVAN, eldeſt Son to the Earl of CORKE.

To the Reader.

ALTHOUGH the following treatiſe being far more prolix than becomes a letter, and than I at firſt intended it, I am very unwilling to encreaſe the already exceſſive bulk of the book by a preface; yet there are ſome particulars, that I think myſelf obliged to take notice of to the reader, as things that will either concern him to know, or me to have known.

IN the firſt place then: If it be demanded why I publiſh to the world a letter, which, by its ſtyle and divers paſſages, appears to have been written as well for, as to a particular perſon; I have chiefly theſe two things to anſwer; the one, that the experiments therein related, having been many of them tried in the preſence of ingenious men, and by that means having made ſome noiſe among the Virtuoſi (inſomuch that ſome of them have been ſent into foreign countries, where they have had the luck not to be deſpiſed) I could not, without quite tiring more than one amanuenſis, give out half as many copies of them as were ſo earneſtly deſired, that I could not civilly refuſe them. The other, that intelligent perſons in matters of this kind perſuade me, that the publication of what I had obſerved touching the nature of the air, would not be uſeleſs to the world; and that in an age ſo taken with novelties as is ours, theſe new experiments would be grateful to the lovers of free and real learning: ſo that I might at once comply with my grand deſign of promoting experimental and uſeful philoſophy, and obtain the great ſatisfaction of giving ſome to ingenious men; the hope of which is, I confeſs, a temptation, that I cannot eaſily reſiſt.

OF my being ſomewhat prolix in many of my experiments, I have theſe reaſons to render: that ſome of them being altogether new, ſeemed to need the being circumſtantially related, to keep the reader from diſtruſting them: that divers circumſtances I did here and there ſet down for fear of forgetting them, when I may hereafter have occaſion to make uſe of them in my other writings: that in divers caſes I thought it

 neceſſary

neceſſary to deliver things circumſtantially, that the perſon I addreſſed them to might, without miſtake, and with as little trouble as is poſſible, be able to repeat ſuch unuſual experiments: and that after I conſented to let my obſervations be made public, the moſt ordinary reaſon of my prolixity was, that foreſeeing, that ſuch a trouble as I met with in making thoſe trials carefully, and the great expence of time that they neceſſarily require (not to mention the charges of making the engine, and employing a man to manage it) will probably keep moſt men from trying again theſe experiments, I thought I might do the generality of my readers no unacceptable piece of ſervice, by ſo punctually relating what I carefully obſerved, that they may look upon theſe narratives as ſtanding records in our new pneumatics, and need not reiterate themſelves an experiment to have as diſtinct an idea of it, as may ſuffice them to ground their reflexions and ſpeculations upon.

And becauſe ſometimes 'tis the diſcourſe made upon the experiment, that makes it appear prolix, I have commonly left a conſpicuous interval betwixt ſuch diſcourſes, and the experiments whereunto they belong, or are annexed; that they, who deſire only the hiſtorical part of the account we give of our engine, may read the narratives, without being put to the trouble of reading the reflexions too: which I here take notice of, for the ſake of thoſe, that are well verſed in the new philoſophy and in the mathematicks: that ſuch may ſkip what was deſigned but for ſuch perſons as may be leſs acquainted, even than I, with matters of this nature (ſcarce ſo much as mentioned by any writer in our language) and not for them, from whom I ſhall be much more forward to learn, than to pretend to teach them. Of my being wont to ſpeak rather doubtfully, or heſitantly, than reſolvedly, concerning matters wherein I apprehend ſome difficulty, I have in another treatiſe (which may, through God's aſſiſtance, come abroad ere long) given a particular, and, I hope, a ſatisfactory account: wherefore I ſhall now defend my practice but by the obſervation of *Ariſtotle*, who ſomewhere notes, that to ſeem to know all things certainly, and to ſpeak poſitively of them, is a trick of bold and young fellows; whereas thoſe, that are indeed intelligent and conſiderate, are wont to employ more wary or diffident expreſſions, or (as he ſpeaks) προτιθέασιν αἰεὶ τὸ ἴσως καὶ τὸ τάχα.

There are divers reflexions, and other paſſages in the following epiſtle, and even ſome experiments (occaſionally mentioned) which may ſeem either impertinent or ſuperfluous, but are not ſo; being purpoſely written, either to evince ſome truth oppoſed, or diſprove ſome erroneous conceit maintained by ſome eminent new philoſopher, or by ſome other ingenious men, who, I preſumed, would eaſily forgive me the having on ſuch occaſions purpoſely omitted their names; though an inquiſitive perſon will probably diſcover divers of them, by the mention of the opinions diſprov'd in the experiments I am excuſing.

Ever ſince I diſcerned the uſefulneſs of ſpeculative geometry to natural philoſophy, the unhappy diſtempers of my eyes have ſo far kept me from being much converſant in it, that I fear I ſhall need the pardon of my mathematical readers for ſome paſſages, which, if I had been deeply ſkill'd in geometry, I ſhould have treated more accurately.

And indeed, having, for reaſons elſewhere deduced, purpoſely kept myſelf a ſtranger to moſt of the new hypotheſes in philoſophy, I am ſenſible enough, that the engine I treat of hath prevailed with me to write of ſome ſubjects, which are ſufficiently remote from thoſe I have been moſt converſant in. And having been reduced to write the greateſt part of the enſuing letter at a diſtance, not only from my library, but from my own manuſcripts, I cannot but fear, that my diſcourſes do not only want many choice things, wherewith the learned writings of others might have enriched

riched or embellifhed them; but that partly for this reafon, and partly for that touched upon a little before, it is poffible I may have mentioned fome notions already publifhed by others, without taking notice of the authors, not out of any defign to defraud deferving men, but for want of knowing fuch particulars to have been already publifhed by them; efpecially the experiments of our engine being themfelves fufficient to hint fuch notions as we build upon them.

The order of the experiments every reader may alter, as fuits beft with his own defign in purfuing them: for not only all thofe, betwixt whom there is an affinity in nature (by belonging to one fubject) are not always placed one by another, but they are not ftill fet down fo much as in the order wherein they were made; but moft commonly in that cafual one, wherein my occafions induced me to difpatch them to the prefs. And, which is worfe, I did ufually fend quite away the former experiments, before the latter were written, or perhaps fo much as made: whereby I loft the advantage of correcting and fupplying the imperfections of what I had formerly written, by the light of my fubfequent trials and difcoveries.

Besides all this, the diftemper in my eyes forbidding me not only to write my felf fo much as one experiment, but even to read over myfelf what I dictated to others; I cannot but fear, that befides the author's miftakes, this edition may be blemifhed by many, that may be properly imputed to a very unfkilful writer (whom I was oftentimes by hafte reduced, againft my cuftom, to employ) and may have efcaped the diligence of that learned friend, that doth me the favour to overfee the prefs; efpecially, there being the diftance of two days journey betwixt it and me.

I need not, perhaps, reprefent to the equitable reader, how much the ftrange confufions of this unhappy nation, in the midft of which I have made and written thefe experiments, are apt to difturb that calmnefs of mind and undiftractednefs of thoughts, that are wont to be requifite to happy fpeculations. But I prefume, that by all thefe things put together, he will readily perceive, that I have been fo far from following the poet's prudent counfel touching the flow publication of books defigned to purchafe credit by,

——*nonumque prematur in annum*,

that I fuffer this treatife to come abroad into the world with a multitude of difadvantages.

But if it be demanded, why then I did not make it fitter for the prefs, before I fent it thither? my anfwer muft be, that not at firft imagining, that this fort of experiments would prove any thing near fo troublefome, either to make, or to record, as I afterwards found them, I did, to engage the printer to difpatch, promife him to fend him the whole epiftle in a very fhort time. So that although now and then the occafional vacations of the prefs, by reafon of feftivals, or the abfence of the corrector, gave me the leifure to expatiate upon fome fubject; yet being oftentimes called upon to difpatch the papers to the prefs, my promife, and many unexpected avocations, obliged me to a hafte, which, though it hath detracted nothing from the faithfulnefs of the hiftorical part of our book, hath (I fear) been difadvantageous enough to all the reft. And I made the lefs fcruple to let the following papers pafs out of my hands, with all their imperfections, becaufe, as the public affairs, and my own, were then circumftanced, I knew not when (if at all) I fhould be again in a condition to profecute experiments of this kind; efpecially, fince (to omit my being almoft weary of being, as it were, confined to one fort of experiments) I am pre-engaged (if it pleafe God to vouchfafe me life and health) to employ my firft leifure in the publication of fome other phyfiological papers, which I thought it would

make me much the fitter to take in hand, if I first dispatched all that I had at this time to write touching our engine.

I HAVE this farther to add, by way of excuse, that as it hath been my design in publishing these experiments to gratify ingenious men; so, if I have not been much flattered, I may hope, that the various hints to be met with in the following letter will (at least) somewhat awaken mens thoughts, and excite them to new speculations, (such as perhaps even inquisitive men would scarce else light upon;) and I need not despair, that even the examination of such new suspicions and enquiries will hence also, at least occasionally, be facilitated: I said occasionally, because it being, as it is proverbially said, *facile inventis addere*; it seems not irrational to expect, that our engine itself, and divers of our experiments, will be much promoted by the industry of inventive and mathematical wits, whose contrivances may easily either correct or supply, and consequently surpass many of those we have made use of. And, particularly, if men by skill and patience can arrive both to evacuate such receivers as ours, till there be no more air left in them, than there seems to have remained in the glasses made use of about the Magdeburgic experiment (hereafter to be mentioned) and to keep out the air for a competent while, the usefulness and discoveries of our engine will not be a little advanced. And perhaps that may belong to it, which I remember *Seneca* speaks of nature; *initiatos* (saith he) *nos credimus, in vestibulo ejus hæremus.* For being now in a place, where we are not quite destitute of moderately skilful artificers, we have, since the conclusion of the following letter, made some additions to our engine, by whose help we find (upon some new trials) that we may be able, without much of new trouble, to keep the ambient air out of the exhausted receiver for a whole day. And perhaps we should be able to keep it out much longer, if before we shall have dispatched some urgent affairs, and published some papers, for which a kind of promise is thought to make us debtors to the press, we could be at leisure to prosecute such experiments, as may possibly afford a supplement to the following treatise, from which I shall now no longer detain the reader.

Friendly Reader,

I KNOW all persons, that have a public spirit for the advancement of learning, will think much, that this piece came not out in a language of more general use, than this you see it now attired in; especially since the excellent noble person, who is the author, is known to be well able himself (being almost universally a linguist) to have given it either the old Latin, or the newer French dress.

BUT if it be an honour to a language to be preferred, and this honour breeds sometimes an emulation, as anciently it did between the Greeks and Romans, it cannot be thought unhandsome for an English nobleman to have preferred his own: and it may be a sufficient reason for the gentry of foreign parts to learn our speech, or keep interpreters, that they are sure to have for their requital, from many of our English writers (as here from this piece) much curiously ingenious and profitable learning.

BUT as to this particular, (give me leave to use words from a story;) since the mountain cannot come to *Mahomet*, *Mahomet* will go to the mountain: I mean thus; because many witty men, persons of honour and estate especially, may be supposed to be able to make a better account, by employing their studies and time on matter than words, and so are justly impeded from learning languages; and because (as I may judge)

THE

WORKS

OF THE HONOURABLE

ROBERT BOYLE.

In SIX VOLUMES.

To which is prefixed

The LIFE of the AUTHOR.

VOLUME THE FIRST.

A NEW EDITION.

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MDCCLXXII.

judge) the noble author is willing to oblige all men, he hath already provided, that this piece shall shortly be done into Latin, that so it may come home to divers worthy persons in its stream, who cannot travel to find it out in its first origin.

HAVING therefore leave so to do, I cannot forbear to give the world the advertisement of this Latin edition, lest some skilful artist should take needless pains about a work, which will ere long (by God's furtherance) be done to his hands. For such unprofitable expences of study have too frequently happened, and too much to the disadvantage of learning, for want of a sufficient correspondence and intercourse between such as are exercised in the mines of wisdom.

THIS is all the trouble I shall at present give you: nor shall I need mind you, if you have a true gust for the book you read, to have an honour and thankful regard to the person, that hath favoured us with the communication of these his trials, and is manifestly so great a patron and friend to experimental learning, and all true wisdom: for should you fail in this, you might deservedly be deprived of some other observations on the same subject, which the author, I hear, hath made since the finishing of this treatise.

I DESIRE to be excused, that I do not make excuses for the slowness of the publication, hoping that the long expectation you have had of it, will enhance, and not diminish your delight in the enjoyment of a piece like to be, amongst the students in accurate philosophy, of so general acceptance. Farewel.

R. Sh.

TO THE

LORD of *DUNGARVAN*,

my Honoured and Dear NEPHEW.

MY DEAR LORD,

RECEIVING in your last from *Paris* a desire, that I would add some more experiments to those I formerly sent you over; I could not be so much your servant as I am, without looking upon that desire as a command; and consequently, without thinking myself obliged to consider by what sort of experiments it might the most acceptably be obeyed. And at the same time, perceiving by letters from some other ingenious persons at *Paris*, that several of the Virtuosi there were very intent upon the examination of the interest of the air, in hindering the descent of the quicksilver, in the famous experiment touching a vacuum; I thought I could not comply with your desires in a more fit and seasonable manner, than by prosecuting and endeavouring to promote that noble experiment of *Torricellius*; and by presenting your Lordship an account of my attempts to illustrate a subject, about which its being so much discoursed of where you are, together with your inbred curiosity, and love of experimental learning, made me suppose you sufficiently inquisitive.

AND though I pretend not to acquaint you, on this occasion, with any store of new discoveries, yet possibly I shall be so happy, as to assist you to know some things, which you did formerly but suppose; and shall present you, if not with new theories, at least with new proofs of such as are not yet become unquestionable. And if what

I shall deliver hath the good fortune to encourage and assist you to prosecute the hints it will afford, I shall account myself, in paying of a duty to you, to have done a piece of service to the commonwealth of learning. Since it may highly conduce to the advancement of that experimental philosophy, the effectual pursuit of which requires as well a purse as a brain, to endear it to hopeful persons of your quality, who may accomplish many things, which others cannot but wish, or at most but design, by being able to employ the presents of fortune in the search of the mysteries of nature.

AND I am not faintly induced to make choice of this subject, rather than any of the expected chymical ones, to entertain your Lordship upon, by these two considerations: the one, that the air being so necessary to human life, that not only the generality of men, but most other creatures that breathe, cannot live many minutes without it, any considerable discovery of its nature seems likely to prove of moment to mankind. And the other is, that the ambient air being that, whereto both our own bodies, and most of the others we deal with here below, are almost perpetually contiguous, not only its alterations have a notable and manifest share in those obvious effects, that men have already been invited to ascribe thereunto, (such as are the various distempers incident to human bodies, especially if crazy in the spring, the autumn, and also on most of the great and sudden changes of weather;) but likewise, the further discovery of the nature of the air will probably discover to us, that it concurs more or less to the exhibiting of many phænomena, in which it hath hitherto scarce been suspected to have any interest. So that a true account of any experiment that is new concerning a thing, wherewith we have such constant and necessary intercourse, may not only prove of some advantage to human life, but gratify philosophers, by promoting their speculations on a subject, which hath so much opportunity to solicit their curiosity.

AND I should immediately proceed to the mention of my experiments, but that I like too well that worthy saying of the naturalist *Pliny*, *benignum est & plenum ingenui pudores, fateri per quos profeceris*, not to conform to it, by acquainting your Lordship, in the first place, with the hint I had of the engine I am to entertain you with. You may be pleased to remember, that a while before our separation in *England*, I told you of a book, that I had heard of, but not perused, published by the industrious Jesuit *Schottus*; wherein, it was said, he related how that ingenious gentleman, *Otto Gericke*, consul of *Magdeburg*, had lately practised in *Germany* a way of emptying glass vessels, by sucking out the air at the mouth of the vessel, plunged under water. And you may also perhaps remember, that I expressed myself much delighted with this experiment, since thereby the great force of the external air (either rushing in at the opened orifice of the emptied vessel, or violently forcing up the water into it) was rendered more obvious and conspicuous than in any experiment that I had formerly seen. And though it may appear by some of those writings I sometimes shewed your Lordship, that I had been solicitous to try things upon the same ground; yet in regard this gentleman was before-hand with me in producing such considerable effects by means of the exsuction of air, I think myself obliged to acknowledge the assistance and encouragement the report of his performances hath afforded me.

In Præfat. Lib. 1.

BUT, as few inventions happen to be at first so complete, as not to be either blemished with some deficiencies needful to be remedied, or otherwise capable of improvement; so when the engine, we have been speaking of, comes to be more attentively considered, there will appear two very considerable things to be desired in it. For first, the wind-pump (as somebody not improperly calls it) is so contrived, that

to

to evacuate the veſſel, there is required the continual labour of two ſtrong men for divers hours. And next (which is an imperfection of much greater moment) the receiver, or glaſs to be emptied, conſiſting of one entire and uninterrupted globe and neck of glaſs; the whole engine is ſo made, that things cannot be conveyed into it, whereon to try experiments: ſo that there ſeems but little (if any thing) more to be expected from it, than thoſe very few phænomena, that have been already obſerved by the author, and recorded by *Schottus*. Wherefore to remedy theſe inconveniences, I put both Mr. *G.* and *R. Hook* (who hath alſo the honour to be known to your Lordſhip, and was with me when I had theſe things under conſideration) to contrive ſome air-pump, that might not, like the other, need to be kept under water (which on divers occaſions is convenient) and might be more eaſily managed: and after an unſucceſsful trial or two of ways propoſed by others, the laſt-named perſon fitted me with a pump, anon to be deſcribed. And thus the firſt imperfection of the German engine was in good meaſure, though not perfectly remedied: and to ſupply the ſecond defect, it was conſidered, that it would not perhaps prove impoſſible to leave in the glaſs to be emptied a hole large enough to put in a man's arm cloathed; and conſequently other bodies, not bigger than it, or longer than the inſide of the veſſel. And this deſign ſeemed the more hopeful, becauſe I remembered, that having ſeveral years before often made the experiment *de vacuo* with my own hands; I had, to examine ſome conjectures that occurred to me about it, cauſed glaſſes to be made with a hole at that end, which uſes to be ſealed up, and had nevertheleſs been able, as occaſion required, to make uſe of ſuch tubes, as if no ſuch holes had been left in them, by deviſing ſtopples for them, made of the common plaiſter called diachylon; which, I rightly enough gueſſed, would, by reaſon of the exquiſite commixtion of its ſmall parts, and cloſeneſs of its texture, deny all acceſs to the external air. Wherefore, ſuppoſing that by the help of ſuch plaiſters carefully laid upon the commiſſures of the ſtopple and hole to be made in the receiver, the external air might be hindered from inſinuating itſelf between them into the veſſel, we cauſed ſeveral ſuch glaſſes, as you will find deſcribed a little lower, to be blown at the glaſs-houſe. And though we could not get the workmen to blow any of them ſo large, or of ſo convenient a ſhape as we would fain have had; yet finding one to be tolerably fit, and leſs unfit than any of the reſt, we were content to make uſe of it in that engine; of which, I ſuppoſe, you by this time expect a deſcription, in order to the recital of the phænomena exhibited by it.

To give your Lordſhip then, in the firſt place, ſome account of the engine itſelf; it conſiſts of two principal parts; a glaſs veſſel, and a pump to draw the air out of it.

THE former of theſe (which we, with the glaſs-men, ſhall often call a receiver, for its affinity to the large veſſels of that name, uſed by chymiſts) conſiſts of a glaſs with a wide hole at the top, of a cover to that hole, and of a ſtop-cock faſtened to the end of the neck, at the bottom.

THE ſhape of the glaſs, you will find expreſſed in the firſt figure of the annexed ſcheme. And for the ſize of it, it contained about 30 wine quarts, each of them containing near two pound (of 16 ounces to the pound) of water. We ſhould have been better pleaſed with a more capacious veſſel; but the glaſs-men profeſſed themſelves unable to blow a larger, of ſuch a thickneſs and ſhape as was requiſite to our purpoſe.

AT the very top of the veſſel A, you may obſerve a round hole, whoſe diameter B C is of about four inches; and whereof the oriſice is incircled with a lip of glaſs, almoſt an inch high: for the making of which lip, it was requiſite (to mention that upon

upon the by, in case your Lordship should have such another engine made for you) to have a hollow and tapering pipe of glass drawn out, whereof the orifice above mentioned was the basis; and then to have the cone cut off with an hot iron, within about an inch of the points B, C.

THE use of the lip is to sustain the cover delineated in the second figure; where D E points out a brass ring, so cast, as that it doth cover the lip B C of the first figure, and is cemented on, upon it, with a strong and close cement. To the inward tapering orifice of this ring (which is about three inches over) are exquisitely ground the sides of the brass stopple F G; so that the concave superficies of the one, and the convex of the other, may touch one another in so many places, as may leave as little access, as possible, to the external air. And in the midst of this cover is left a hole H I, of about half an inch over, invironed also with a ring or socket of the same metal, and fitted likewise with a brass stopple K, made in the form of the key of a stop-cock, and exactly ground into the hole H I it is to fill; so as that, though it be turned round in the cavity it possesses, it will not let in the air, and yet may be put in or taken out at pleasure, for uses to be hereafter mentioned. In order to some of which, it is perforated with a little hole 8, traversing the whole thickness of it at the lower end; through which, and a little brass ring L fastened to one side (no matter which) of the bottom of the stopple F G, a string 8, 9, 10, might pass, to be employed to move some things in the capacity of the emptied vessel, without any where unstopping it.

THE last thing belonging to our receiver is the stop-cock, designed in the first figure by N, for the better fastening of which to the neck, and exacter exclusion of the air, there was sodered on to the shank of the cock X, a plate of tin M T U W, long enough to cover the neck of the receiver. But because the cementing of this was a matter of some difficulty, it will not be amiss to mention here the manner of it; which was, that the cavity of the tin plate was filled with a melted cement, made of pitch, rosin, and wood-ashes, well incorporated; and to hinder this liquid mixture from getting into the orifice Z of the shank X, that hole was stopped with a cock, to which was fastened a string, whereby it might be pulled out at the upper orifice of the receiver; and then, the glass neck of the receiver being well warmed was thrust into this cement, and over the shank, whereby it was effected, that all the space betwixt the tin plate and the receiver, and betwixt the internal superficies of the receiver and the shank of the cock was filled with the cement. And so we have dispatched the first upper part of the engine.

THE undermost remaining part consists of a frame, and of a sucking-pump, or, as we formerly called it, an air-pump, supported by it. The frame is of wood, small but very strong, consisting of three legs, 1, 1, 1, so placed, that one side of it may stand perpendicular, that the free motion of the hand may not be hindered. In the midst of which frame is transversely nailed a board 2, 2, 2, which may not improperly be called a midriff; upon which rests, and to which is strongly fastened, the main part of the pump itself, which is the only thing remaining to be described.

THE pump consists of four parts, a hollow cylinder, a sucker, a handle to move that sucker, and a valve.

THE cylinder was (by a pattern) cast of brass; it is in length about 14 inches, thick enough to be very strong, notwithstanding the cylindrical cavity left within it: this cavity is about three inches diameter, and makes as exact a cylinder as the artificer was able to bore. This hollow cylinder is fitted with a sucker 4, 4, 5, 5, consisting of two parts; the one 4, 4, somewhat less in diameter than the cavity of the cylinder; upon which is nailed a good thick piece of tanned shoe-leather, which will

go

go ſo cloſe to the cylinder, that it will need to be very forcibly knocked and rammed in, if at any time it be taken out: which is therefore done, that it may the more exactly hinder the air from inſinuating itſelf betwixt it and the ſides of the cylinder whereon it is to move.

To the midſt of this former part of the ſucker is ſtrongly faſtened the other; namely, a thick and narrow plate of iron 5, 5, ſomewhat longer than the cylinder; one of whoſe edges is ſmooth, but at the other edge it is indented (as I may ſo ſpeak) with a row of teeth, delineated in the ſcheme, into whoſe intervals are to be fitted the teeth of a ſmall iron nut α β (as tradeſmen call it) which is faſtened by two ſtaples 2, 2, to the under ſide of the formerly mentioned tranſverſe board 2, 2, 2, on which the cylinder reſts, and is turned to and fro by the third piece of this pump, namely, the handle or *manubrium* 7, of which the figure gives a ſufficient deſcription.

THE fourth and laſt part of this cylinder is the valve R, conſiſting of a hole bored through at the top of the cylinder, a little tapering towards the cavity; into which hole is ground a tapering peg of braſs, to be thruſt in and taken out at pleaſure.

THE engine being thus deſcribed, it will be requiſite to add, that ſomething is wont to be done before it be ſet on work, for the more eaſy moving of the ſucker, and for the better excluſion of the outward air; which, when the veſſel begins to be exhauſted, is much more difficult to be kept out, than one would eaſily imagine.

THERE muſt then be firſt poured in at the top of the receiver a little ſallad oil, partly to fill up any ſmall intervals, that may happen to be betwixt the contiguous ſurfaces of the internal parts of the ſtop-cock; and partly that it may be the more eaſy to turn the key S, backwards and forwards. Pretty ſtore of oil muſt alſo be poured into the cylinder, both that the ſucker may ſlip up and down in it the more ſmoothly and freely, and that the air might be the better hindered from getting in between them. And for the like reaſons, a little oil is to be uſed alſo about the valve. Upon which occaſion, it would not be omitted (for it is ſtrange) that oftentimes, when neither the pouring in of water, nor even of oil alone, proved capable to make the ſucker move eaſily enough in the cylinder; a mixture of both theſe liquors would readily (ſometimes even to admiration) perform the deſired effect. And laſtly, the braſs cover of the receiver being put into the braſs ring formerly deſcribed, that no air may get between them, it will be very requiſite to plaiſter over very carefully the upper edges of both with the plaiſter formerly mentioned, or ſome other as cloſe, which is to be ſpread upon the edges with an hot iron; that, being melted, it may run into and fill up all the crannies, or other little cavities, at which the air might otherwiſe get entrance.

ALL things being thus fitted, and the lower ſhank O of the ſtop-cock being put into the upper orifice of the cylinder, and into which it was exactly ground; the experimenter is firſt, by turning the handle, to force the ſucker to the top of the cylinder, that there may be no air left in the upper part of it. Then ſhutting the valve with the plug, and turning the other way, he is to draw down the ſucker to the bottom of the cylinder; by which motion of the ſucker, the air that was formerly in the cylinder being thruſt out, and none being permitted to ſucceed in its room, it is manifeſt, that the cavity of the cylinder muſt be empty in reference to the air. So that if thereupon the key of the ſtop-cock be ſo turned, as that through the perforation of it a free paſſage be opened betwixt the cylinder and the receiver, part of the air formerly contained in the receiver will nimbly deſcend into the cylinder. And this air being, by the turning back of the key, hindered from the returning into the receiver, may, by the opening of the valve, and forcing up of the ſucker to the top of the cylinder again, be driven out into the open air. And thus by the repetition

of the motion of the sucker upward and downward, and by opportunely turning the key, and stopping the valve, as occasion requires, more or less air may be sucked out of the receiver, according to the exigency of the experiment, and the intention of him that makes it.

YOUR Lordship will perhaps think, that I have been unnecessarily prolix in this first part of my discourse: but if you had seen how many unexpected difficulties we found to keep out the external air, even for a little while, when some considerable part of the internal had been sucked out, you would peradventure allow, that I might have set down more circumstances than I have, without setting down any, whose knowledge, he that shall try the experiment, may not have need of. Which is so true, that, before we proceed any further, I cannot think it unseasonable to advertise your Lordship, that there are two chief sorts of experiments, which we designed in our engine to make trial of: the one, such as may be quickly dispatched, and therefore may be tried in our engine, though it leak a little, because the air may be faster drawn out by nimbly plying the pump, than it can get in at undiscerned leaks; I say, at undiscerned leaks, because such as are big enough to be discovered, can scarce be uneasy to be stopt. The other sort of experiments consist of those, that require, not only that the internal air be drawn out of the receiver, but that it be likewise for a long time kept out of it. Such are the preservation of animal and other bodies therein, the germination and growth of vegetables, and other trials of several sorts, which it is apparent cannot be well made, unless the external air can, for a competent while, be excluded: since, even at a very small leak, there may enough get in, to make the vacuum soon lose that name; by which I here declare once for all, that I understand not a space, wherein there is no body at all, but such as is either altogether, or almost totally devoid of air.

Now this distinction of experiments I thought fit to premise to the ensuing narratives, because, upon trial, we found it so exceeding (and scarce imaginably) difficult a matter, to keep out the air from getting at all in at any imperceptible hole or flaw whatsoever, (in a vessel immediately surrounded with the compressed atmosphere) that in spite of all our care and diligence we never were able totally to exhaust the receiver, or keep it, when it was almost empty, any considerable time, from leaking more or less: although (as we have lately intimated) by unwearied quickness in plying the pump, the internal air can be much faster drawn out than the external can get in, till the receiver come to be almost quite empty. And that is enough to enable men to discover hitherto unobserved phænomena of nature.

THE experiments therefore of the first sort will, I fear, prove the only ones, wherewith my avocations will allow me to entertain your Lordship in this letter. For till your further commands shall engage me to undertake, by God's permission, such an employment, and more leisure shall better fit me for it, I know not whether I shall be in a condition to try what may be done, to enable me to give you some account of the other sort of experiments also.

EXPERIMENT I.

TO proceed now to the phænomena exhibited to us by the engine above described; I hold it not unfit to begin with what doth constantly and regularly offer itself to our observation, as depending upon the fabric of the engine itself, and not upon the nature of this or that particular experiment, which it is employed to try.

FIRST then, upon the drawing down of the sucker (the valve being shut) the cylindrical space, deserted by the sucker, is left devoid of air; and therefore, upon the turning

turning of the key, the air contained in the receiver rusheth into the emptied cylinder, till the air in both those vessels be brought to about an equal measure of dilatation. And therefore, upon shutting the receiver by returning the key, if you open the valve, and force up the sucker again, you will find, that after this first exsuction you will drive out almost a whole cylinder full of air: but at the following exsuctions, you will draw less and less of air out of the receiver into the cylinder, because there will still remain less and less air in the receiver itself; and consequently, the particles of the remaining air, having more room to extend themselves in, will less press out one another. This you will easily perceive, by finding that you still force less and less air out of the cylinder; so that when the receiver is almost exhausted, you may force up the sucker almost to the top of the cylinder, before you will need to unstop the valve to let out any air. And if at such time, the valve being shut, you let go the handle of the pump, you will find the sucker forcibly carried up to the top of the cylinder, by the protrusion of the external air; which, being much less rarified than that within the cylinder, must have a more forcible pressure upon the sucker, than the internal is able to resist: and by this means you may know how far you have emptied the receiver. And to this we may add, on this occasion, that constantly upon the turning of the key to let out the air from the receiver into the emptied cylinder, there is immediately produced a considerably brisk noise, especially whilst there is any plenty of air in the receiver.

FOR the more easy understanding of the experiments triable by our engine, I thought it not superfluous nor unseasonable in the recital of this first of them, to insinuate that notion, by which it seems likely, that most, if not all of them, will prove explicable. Your Lordship will easily suppose, that the notion I speak of is, that there is a spring, or elastical power in the air we live in. By which ἐλατὴς or spring of the air, that which I mean is this; that our air either consists of, or at least abounds with, parts of such a nature, that in case they be bent or compressed by the weight of the incumbent part of the atmosphere, or by any other body, they do endeavour, as much as in them lieth, to free themselves from that pressure, by bearing against the contiguous bodies that keep them bent; and, as soon as those bodies are removed, or reduced to give them way, by presently unbending and stretching out themselves, either quite, or so far forth as the contiguous bodies that resist them will permit, and thereby expanding the whole parcel of air, these elastical bodies compose.

THIS notion may perhaps be somewhat further explained, by conceiving the air near the earth to be such a heap of little bodies, lying one upon another, as may be resembled to a fleece of wool. For this (to omit other likenesses betwixt them) consists of many slender and flexible hairs; each of which may indeed, like a little spring, be easily bent or rolled up; but will also, like a spring, be still endeavouring to stretch itself out again. For though both these hairs, and the aëreal corpuscles to which we liken them, do easily yield to external pressures; yet, each of them (by virtue of its structure) is endowed with a power or principle of self-dilatation; by virtue whereof, though the hairs may by a man's hand be bent and crouded closer together, and into a narrower room than suits best with the nature of the body; yet, whilst the compression lasts, there is in the fleece they compose an endeavour outwards, whereby it continually thrusts against the hand that opposes its expansion. And upon the removal of the external pressure, by opening the hand more or less, the compressed wool doth, as it were, spontaneously expand or display itself towards the recovery of its former more loose and free condition, till the fleece hath either regained

its former dimensions, or at least approached them as near as the compressing hand (perchance not quite opened) will permit. This power of self-dilatation is somewhat more conspicuous in a dry spunge compressed, than in a fleece of wool. But yet we rather chose to employ the latter on this occasion, because it is not, like a spunge, an entire body, but a number of slender and flexible bodies, loosely complicated, as the air itself seems to be.

THERE is yet another way to explicate the spring of the air; namely, by supposing with that most ingenious gentleman, Monsieur *Des Cartes*, that the air is nothing but a congeries or heap of small and (for the most part) of flexible particles, of several sizes, and of all kind of figures, which are raised by heat (especially that of the sun) into that fluid and subtle ethereal body that surrounds the earth; and by the restless agitation of that celestial matter, wherein those particles swim, are so whirled round, that each corpuscle endeavours to beat off all others from coming within the little sphere requisite to its motion about its own centre; and in case any, by intruding into that sphere, shall oppose its free rotation, to expel or drive it away: so that, according to this doctrine, it imports very little, whether the particles of the air have the structure requisite to springs, or be of any other form (how irregular soever) since their elastical power is not made to depend upon their shape or structure, but upon the vehement agitation, and (as it were) brandishing motion, which they receive from the fluid æther, that swiftly flows between them, and whirling about each of them (independently from the rest) not only keeps those slender aëreal bodies separated and stretched out (at least, as far as the neighbouring ones will permit) which otherwise, by reason of their flexibleness and weight, would flag or curl; but also makes them hit against, and knock away each other, and consequently require more room than that, which, if they were compressed, they would take up.

BY these two differing ways, my Lord, may the springs of the air be explicated. But though the former of them be that, which by reason of its seeming somewhat more easy, I shall for the most part make use of in the following discourse; yet am I not willing to declare peremptorily for either of them against the other. And indeed, though I have in another treatise endeavoured to make it probable, that the returning of elastical bodies (if I may so call them) forcibly bent, to their former position, may be mechanically explicated; yet I must confess, that to determine whether the motion of restitution in bodies proceed from this, that the parts of a body of a peculiar structure are put into motion by the bending of the spring, or from the endeavour of some subtle ambient body, whose passage may be opposed or obstructed, or else its pressure unequally resisted by reason of the new shape or magnitude, which the bending of a spring may give the pores of it: to determine this, I say, seems to me a matter of more difficulty, than at first sight one would easily imagine it. Wherefore I shall decline meddling with a subject, which is much more hard to be explicated than necessary to be so by him, whose business it is not, in this letter, to assign the adequate cause of the spring of the air, but only to manifest, that the air hath a spring, and to relate some of its effects.

I KNOW not whether I need annex, that though either of the above-mentioned hypotheses, and perhaps some others, may afford us an account plausible enough of the air's spring; yet I doubt, whether any of them gives us a sufficient account of its nature. And of this doubt I might here mention some reasons, but that, peradventure, I may (God permitting) have a fitter occasion to say something of it elsewhere. And therefore I should now proceed to the next experiment, but that I think it requisite, first, to suggest to your Lordship what comes into my thoughts, by way

of anſwer to a plauſible objection, which I foreſee you may make againſt our propoſed doctrine, touching the ſpring of the air. For it may be alledged, that though the air were granted to conſiſt of ſpringy particles (if I may ſo ſpeak) yet thereby we could only give an account of the dilatation of the air in wind-guns, and other pneumatical engines, wherein the air hath been compreſſed, and its ſprings violently bent by an apparent external force; upon the removal of which, it is no wonder, that the air ſhould, by the motion of reſtitution, expand itſelf till it hath recovered its more natural dimenſions: whereas, in our above-mentioned firſt experiment, and in almoſt all others triable in our engine, it appears not, that any compreſſion of the air preceded its ſpontaneous dilatation or expanſion of itſelf. To remove this difficulty, I muſt deſire your Lordſhip to take notice, that of whatever nature the air, very remote from the earth, may be, and whatever the ſchools may confidently teach to the contrary, yet we have divers experiments to evince, that the atmoſphere we live in is not (otherwiſe than comparatively to more ponderous bodies) light, but heavy. And did not their gravity hinder them, it appears not why the ſtreams of the terraqueous globe, of which our air in great part conſiſts, ſhould not riſe much higher, than the refractions of the ſun, and other ſtars, give men ground to think, that the atmoſphere, (even in the judgment of thoſe recent aſtronomers, who ſeem willing to enlarge its bounds as much as they dare,) doth reach.

BUT leſt you ſhould expect my ſeconding this reaſon by experience; and leſt you ſhould object, that moſt of the experiments, that have been propoſed to prove the gravity of the air, have been either barely propoſed, or perhaps not accurately tried; I am content, before I paſs further, to mention here, that I found a dry lamb's bladder containing near about two thirds of a pint, and compreſſed by a packthread tied about it, to loſe a grain and the eighth part of a grain of its former weight, by the receſs of the air upon my having prickt it: and this with a pair of ſcales, which, when the full bladder and the correſpondent weight were in it, would manifeſtly turn either way with the 32^{d} part of the grain. And if it be further objected, that the air in the bladder was violently compreſſed by the packthread and the ſides of the bladder, we might probably (to wave prolix anſwers) be furniſhed with a reply, by ſetting down the differing weight of our receiver, when emptied, and when full of uncompreſſed air, if we could here procure ſcales fit for ſo nice an experiment; ſince we are informed, that in the German experiment, commended at the beginning of this letter, the ingenious triers of it found, that their glaſs veſſel, of the capacity of 32 meaſures, was lighter when the air had been drawn out of it, than before, by no leſs than one ounce and $\frac{1}{12}$, that is, an ounce and very near a third. But of the gravity of the air, we may elſewhere have occaſion to make further mention.

TAKING it then for granted, that the air is not devoid of weight, it will not be uneaſy to conceive, that that part of the atmoſphere, wherein we live, being the lower part of it, the corpuſcles, that compoſe it, are very much compreſſed by the weight of all thoſe of the like nature, that are directly over them; that is, of all the particles of air, that being piled up upon them, reach to the top of the atmoſphere. And though the height of this atmoſphere, according to the famous *Kepler*, and ſome others, ſcarce exceeds eight common miles; yet other eminent and later aſtronomers would promote the confines of the atmoſphere to exceed ſix or ſeven times that number of miles. And the diligent and learned *Ricciolo* makes it probable, that the atmoſphere may, at leaſt in divers places, be at leaſt fifty miles high. So that, according to a moderate eſtimate of the thickneſs of the atmoſphere, we may well ſuppoſe, that a column of air, of many miles in height, leaning upon ſome ſpringy corpuſcles of air here below, may have weight enough to bend their little ſprings, and keep them

them bent: as, (to resume our former comparison,) if there were fleeces of wool piled up to a mountainous height one upon another, the hairs, that compose the lowermost locks, which support the rest, would, by the weight of all the wool above them, be as well strongly compressed, as if a man should squeeze them together in in his hands, or employ any such other moderate force to compress them. So that we need not wonder, that upon the taking off the incumbent air from any parcel of the atmosphere here below, the corpuscles, whereof that undermost air consists, should display themselves, and take up more room than before.

AND if it be objected, that in water, the weight of the upper and of the lower part is the same; I answer, that, (besides that it may be well doubted whether the observation, by reason of the great difficulty, hath been exactly made,) there is a manifest disparity betwixt the air and water: for I have not found, upon an experiment purposely made, (and in another treatise recorded) that water will suffer any considerable compression; whereas we may observe in wind-guns, (to mention now no other engines) that the air will suffer itself to be crouded into a comparatively very little room; insomuch, that a very diligent examiner of the phænomena of wind-guns would have us believe, that in one of them, by condensation, he reduced the air into a space at least eight times narrower than it before possessed. And to this, if we add a noble phænomenon of the experiment *de vacuo*; these things put together may for the present suffice to countenance our doctrine. For that noble experimenter, Monsieur *Pascal* (the son) had the commendable curiosity to cause the Torricellian experiment to be tried at the foot, about the middle, and at the top of that high mountoin (in *Auvergne*, if I mistake not) commonly called *Le Puy de Domme*; whereby it was found, that the mercury in the tube fell down lower, about three inches, at the top of the mountain, than at the bottom. And a learned man awhile since informed me, that a great Virtuoso, friend to us both, hath, with not unlike success, tried the same experiment in the lower and upper parts of a mountain in the west of *England*. Of which the reason seems manifestly enough to be this, that upon the tops of high mountains, the air, which bears against the restagnant quicksilver, is less pressed by the less ponderous incumbent air; and consequently is not able totally to hinder the descent of so tall and heavy a cylinder of quicksilver, as at the bottom of such mountains did but maintain an æquilibrium with the incumbent atmosphere.

AND if it be yet further objected against what hath been proposed touching the compactness and pressure of the inferiour air, that we find this very air to yield readily to the motion of little flies, and even to that of feathers, and such other light and weak bodies; which seems to argue, that the particles of our air are not so compressed as we have represented them, especially since, by our former experiment, it appears, that the air readily dilated itself downward, from the receiver into the pump, when it is plain, that it is not the incumbent atmosphere, but only the subjacent air in the brass cylinder that hath been removed: If this, I say, be objected, we may reply, that, when a man squeezeth a fleece of wool in his hand, he may feel, that the wool incessantly bears against his hand, as that which hinders the hairs it consists of, to recover their former and more natural extent. So each parcel of the air about the earth doth constantly endeavour to thrust away all those contiguous bodies, (whether aëreal or more gross,) that keep it bent, and hinder the expansion of its parts, which will dilate themselves, or fly abroad towards that part, (whether upwards or downwards,) where they find their attempted dilatation of themselves less resisted by the neighbouring bodies. Thus the corpuscles of that air we have been all this while speaking of, being unable, by reason of their weight, to ascend above the convexity

vexity of the atmosphere, and by reason of the resistance of the surface of the earth and water, to fall down lower, they are forced, by their own gravity and this resistance, to expand and diffuse themselves about the terrestrial globe; whereby it comes to pass, that they must as well press the contiguous corpuscles of air, that on either side oppose their dilatation, as they must press upon the surface of the earth; and, as it were recoiling thence, endeavour to thrust away those upper particles of air that lean upon them.

AND, as for the easy yielding of the air to the bodies that move in it, if we consider, that the corpuscles, whereof it consists, though of a springy nature, are yet so very small, as to make up (which it is manifest they do) a fluid body, it will not be difficult to conceive, that in the air, as in other bodies that are fluid, the little bodies it consists of, are in an almost restless motion, whereby they become (as we have more fully discoursed in another treatise) very much disposed to yield to other bodies, or easy to be displaced by them; and that the same corpuscles are likewise so variously moved, as they are entire corpuscles, that if some strive to push a body placed among them towards the right hand (for instance,) others, whose motion hath an opposite determination, as strongly thrust the same body towards the left; whereby neither of them proves able to move it out of its place, the pressure on all hands being reduced as it were to an æquilibrium: so that the corpuscles of the air must be as well sometimes considered under the notion of little springs, which remaining bent, are in their entire bulk transported from place to place; as under the notion of springs displaying themselves, whose parts fly abroad, whilst, as to their entire bulk, they scarce change place: as the two ends of a bow, shot off, fly from one another; whereas the bow itself may be held fast in the archer's hand. And that it is the equal pressure of the air on all sides upon the bodies that are in it, which causeth the easy cession of its parts, may be argued from hence; that if by the help of our engine the air be but in great part, though not totally, drawn away from one side of a body without being drawn away from the other, he that shall think to move that body to and fro, as easily as before, will find himself much mistaken.

In a discourse touching Fluidity and Firmness.

IN verification of which we will, to divert your Lordship a little, mention here a phænomenon of our engine, which even to divers ingenious persons hath at first sight seemed very wonderful.

EXPERIMENT II.

THE thing that is wont to be admired, and which may pass for our second experiment, is this; that if, when the receiver is almost empty, a by-stander be desired to lift up the brass key (formerly described as a stopple in the brass cover) he will find it a difficult thing to do so, if the vessel be well exhausted; and even when but a moderate quantity of air hath been drawn out, he will, when he hath lifted it up a little, so that it is somewhat loose from the sides of the lip or socket, which (with the help of a little oil) it exactly filled before; he will (I say) find it so difficult to be lifted up, that he will imagine there is some great weight fastened to the bottom of it. And if (as sometimes hath been done for merriment) only a bladder be tied to it, it is pleasant to see how men will marvel, that so light a body, filled at most but with air, should so forcibly draw down their hand, as if it were filled with some very ponderous thing: whereas the cause of this pretty phænomenon seems plainly enough to be only this: that the air in the receiver being very much dilated, its spring must be very much weakened, and consequently it can but faintly press up the lower end of the stopple; whereas the spring of the external air being no

no way debilitated, he that a little lifts up the ſtopple, muſt with his hand ſupport a preſſure equal to the diſproportion betwixt the force of the internal expanded air, and that of the atmoſphere incumbent upon the upper part of the ſame key or ſtopple: and ſo men being unuſed to find any reſiſtance, in lifting things up, from the free air above them, they are forward to conclude, that that which depreſſeth their hands muſt needs be ſome weight, though they know not where placed, drawing beneath it.

AND, that we have not miſ-aſſigned the cauſe of this phænomenon, ſeems evident enough by this, that as air is ſuffered by little and little to get into the receiver, the weight, that a man fancieth his hand ſupports, is manifeſtly felt to decreaſe more and more; the internal air by this recruit approaching more to an æquilibrium with the external, till at length the receiver growing again full of air, the ſtopple may be lifted up without any difficulty at all.

BY ſeveral other of the experiments afforded by our engine, the ſame notion of the great and equal preſſure of the free air upon the bodies it environs, might be here manifeſted, but that we think it not ſo fit to anticipate ſuch experiments: and therefore ſhall rather employ a few lines to clear up the difficulty touching this matter, which we have obſerved to have troubled ſome even of the philoſophical and mathematical ſpectators of our engine, who have wondered, that we ſhould talk of the air exquiſitely ſhut up in our receiver, as if it were all one with the preſſure of the atmoſphere; whereas the thick and cloſe body of the glaſs, wholly impervious to the air, doth manifeſtly keep the incumbent pillar of the atmoſphere from preſſing in the leaſt upon the air within the glaſs, which it can no where come to touch. To elucidate a little this matter, let us conſider, that if a man ſhould take a fleece of wool, and having firſt by compreſſing it in his hand reduced it into a narrower compaſs, ſhould nimbly convey and ſhut it cloſe up into a box juſt fit for it; though the force of his hand would then no longer bend thoſe numerous ſpringy bodies that compoſe the fleece, yet they would continue as ſtrongly bent as before, becauſe the box they are incloſed in would as much reſiſt their re-expanding of themſelves, as did the hand that put them in. For thus we may conceive, that the air being ſhut up, when its parts are bent by the whole weight of the incumbent atmoſphere, though that weight can no longer lean upon it, by reaſon it is kept off by the glaſs, yet the corpuſcles of the air within that glaſs continue as forcibly bent, as they were before their incluſion, becauſe the ſides of the glaſs hinder them from diſplaying or ſtretching out themſelves. And if it be objected, that this is unlikely, becauſe even glaſs-bubbles, ſuch as are wont to be blown at the flame of a lamp, exceeding thin, and hermetically ſealed, will not break; whereas it cannot be imagined, that ſo thin a priſon of glaſs could reſiſt the elaſtical force of all the included air, if that air were ſo compreſſed as we ſuppoſe; it may be eaſily replied, that the preſſure of the inward air againſt the glaſs is countervailed by the equal preſſure of the outward againſt the ſame glaſs. And we ſee in bubbles, that by reaſon of this, an exceeding thin film of water is often able, for a good while, to hinder the eruption of a pretty quantity of air. And this may be alſo more conſpicuous in thoſe great ſpherical bubbles, that boys ſometimes blow with water, to which ſoap hath given a tenacity. But that, if the preſſure of the ambient air were removed, the internal air may be able to break thicker glaſſes, than thoſe lately mentioned, will appear by ſome of the following experiments; to which we ſhall therefore now haſten, having, I fear, been but too prolix in this excurſion, though we thought it not amiſs to annex to our firſt experiments ſome general conſiderations touching the ſpring of the air, becauſe (this doctrine being yet a ſtranger to the ſchools) not only we find not the thing itſelf to be much taken notice of; but of

of those few that have heard of it, the greater part have been forward to reject it, upon a mistaken persuasion, that those phænomena are the effects of nature's abhorrency of a vacuum, which seem to be more fitly ascribable to the weight and spring of the air.

EXPERIMENT III.

WE will now proceed to observe, that though, by the help of the handle, the sucker be easily drawn down to the bottom of the cylinder; yet, without the help of that leaver, there would be required to the same effect a force or weight great enough to surmount the pressure of the whole atmosphere: since otherwise the air would not be driven out of its place, when none is permitted to succeed into the place deserted by the sucker. This seems evident from the known Torricellian experiment, in which, if the inverted tube of mercury be but 25 digits high, or somewhat more, the quicksilver will not fall, but remain suspended in the tube, because it cannot press the subjacent mercury with so great a force, as doth the incumbent cylinder of the air, reaching thence to the top of the atmosphere; whereas, if the cylinder of mercury were three or four digits longer, it would over-power that of the external air, and run out into the vessel'd mercury, till the two cylinders came to an æquilibrium, and no further. Hence we need not wonder, that though the sucker move easily enough up and down in the cylinder by the help of the manubrium; yet if the manubrium be taken off, it will require a considerable strength to move it either way. Nor will it seem strange, that if, when the valve and stop-cock are well shut, you draw down the sucker, and then let go the manubrium; the sucker will, as it were of itself, re-ascend to the top of the cylinder, since the spring of the external air findeth nothing to resist its pressing up the sucker. And for the same reason, when the receiver is almost evacuated, though, having drawn down the sucker you open the way from the receiver to the cylinder, and then intercept that way again by returning the key; the sucker will, upon the letting go the manubrium, be forcibly carried up almost to the top of the cylinder; because the air within the cylinder, being equally dilated and weakened with that of the glass, is unable to withstand the pressure of the external air, till it be driven into so little space, that there is an equilibrium betwixt its force and that of the air without. And congruously hereunto we find, that in this case the sucker is drawn down with little less difficulty, than if the cylinder, being devoid of air, and the stop-cock were exactly shut. We might take notice of some other things, that depend upon the fabric of our engine itself; but to shun prolixity, we will, in this place, content ourselves to mention one of them, which seems to be of greater moment than the rest, and it is this: that when the sucker hath been impelled to the top of the cylinder, and the valve is so carefully stopped, that there is no air left in the cylinder above the sucker; if then the sucker be drawn to the lower part of the cylinder, he that manageth the pump findeth not any sensibly greater difficulty to depress the sucker, when it is nearer the bottom of the cylinder, than when it is much farther off. Which circumstance we therefore think fit to take notice of, because an eminent modern naturalist hath taught, that when the air is sucked out of a body, the violence wherewith it is wont to rush into it again, as soon as it is allowed to re-enter, proceeds mainly from this; that the pressure of the ambient air is strengthened upon the accession of the air sucked out; which, to make itself room, forceth the neighbouring air to a violent subingression of its parts; which, if it were true, he that draweth down the sucker, would find the resistance of the external air increased as he drawed it lower, more of the dis-

 placed

placed air being thrust into it to compress it. But, by what hath been discoursed upon the first experiment, it seems more probable, that without any such strengthening of the pressure of the outward air, the taking quite away or the debilitating of the resistance from within, may suffice to produce the effects under consideration. But this will perhaps be illustrated by some or other of our future experiments, and therefore shall be no longer insisted on here.

EXPERIMENT IV.

HAVING thus taken notice of some of the constant phænomena of our engine itself, let us now proceed to the experiments triable in it.

WE took then a lamb's bladder large, well dried, and very limber, and leaving in it about half as much air as it could contain, we caused the neck of it to be strongly tied, so that none of the included air, though by pressure, could get out. This bladder being conveyed into the receiver, and the cover luted on, the pump was set on work, and after two or three exsuctions of the ambient air (whereby the spring of that which remained in the glass was weakened) the imprisoned air began to swell in the bladder; and, as more and more of the air in the receiver was, from time to time, drawn out; so did that in the bladder more and more expand itself, and display the folds of the formerly flaccid bladder: so that before we had exhausted the receiver near so much as we could, the bladder appeared as full and stretched, as if it had been blown up with a quill.

AND that it may appear, that this plumpness of the bladder proceeded from the surmounting of the debilitated spring of the ambient air remaining in the vessel, by the stronger spring of the air remaining in the bladder; we returned the key of the stop-cock, and by degrees allowed the external air to return into the receiver: whereupon it happened, as was expected, that as the air came in from without, the distended air in the bladder was proportionably compressed into a narrower room, and the sides of the bladder grew flaccid, till the receiver having re-admitted its wonted quantity of air, the bladder appeared as full of wrinkles and cavities as before.

THIS experiment is much of the same nature with that, which was, some years ago, said to be made by that eminent geometrician Monsieur *Roberval*, with a carp's bladder emptied and conveyed into a tube, wherein the experiment *de vacuo* was afterwards tried; which ingenious experiment of his justly deserveth the thanks of those, that have been, or shall be, sollicitous to discover the nature of the air.

BUT to return to our experiment; we may take notice of this circumstance in it, that after the receiver hath been in some measure emptied, the bladder doth, at each exsuction, swell much more conspicuously than it did at any of the first exsuctions; insomuch that towards the end of the pumping, not only a great fold or cavity, in the surface of the bladder may be made even, by the stretching of the inward self-expanding air; but we have sometimes seen, upon the turning of the key to let the ambient air pass out of the receiver into the cylinder; we have seen (I say) the air in the bladder suddenly expand itself so much and so briskly, that it manifestly lifted up some light bodies that leaned upon it, and seemed to lift up the bladder itself.

WE let down into the receiver with the forementioned bladder two other much smaller, and of the same kind of animal; the one of these was not tied up at the neck, that there might be liberty left to the air that was not squeezed out (which might amount to about a fifth part of what the bladder held before) to pass out into the receiver; the other had the sides of it stretched out and pressed together, almost

into,

into the form of a cup, that they might intercept the less air betwixt them, and then was strongly tied up to the neck. This done, and the air being in some measure sucked out of the pneumatical glass (if I may so call it) the bladder, mentioned at the beginning of our experiment, appeared extended every way to its full dimensions; whereas neither of the two others did remarkably swell, and that, whose neck was not tied, seemed very little, if at all less wrinkled, than when it was put in.

WE made likewise a strong ligature about the middle of a long bladder partly emptied, and, upon the drawing the air out of the receiver, could observe no such swelling betwixt the ligature and the neck of the bladder, which had been purposely left open, as betwixt the same ligature and the bottom of the bladder, whence the included air could no way get out.

BUT a farther and sufficient manifestation, whence the intumescence of the bladder proceeds, may be deduced from the following experiment.

EXPERIMENT V.

TO try then at once both what it was that expanded the bladder, and what a powerful spring there is even in the air we are wont to think uncompressed; we caused a bladder dry, well tied and blown moderately full, to be hung in the receiver by one end of a string, whose other end was fastened to the inside of the cover: and upon drawing out the ambient air that pressed on the bladder; the internal air not finding the wonted resistance, first swelled and distended the bladder, and then broke it, with so wide and crooked a rent, as if it had been forcibly torn asunder with hands. After which a second bladder being conveyed in, the experiment was repeated with like success: and I suppose it will not be imagined, that in this case the bladder was broken by its own fibres, rather than by the imprisoned air.

AND of this experiment these two phænomena may be taken notice of: the one, that the bladder at its breaking gave a great report, almost like a cracker; and the other, that the air contained in the bladder had the power to break it with the mentioned impetuosity, long before the ambient air was all, or near all, drawn out of the receiver.

BUT, to verify what we say in another discourse, where we show, that even true experiments may, by reason of the easy mistake of some unheeded circumstance, be unsuccessfully tried; we will advertise, on this occasion, that we did oftentimes in vain try the breaking of bladders, after the manner above mentioned; of which the cause appeared to be this, that the bladders we could not break, having been brought us already blown from those that sold them, were grown dry before they came to our hands: whence it came to pass, that, if we afterwards tied them very hard, they were apt to fret, and so become unserviceable; and if we tied them but moderately hard, their stiffness kept them from being closed so exactly, but that when the included air had in the exhausted receiver distended them as much as easily it could, it would in part get out between the little wrinkles of the sphincter of the neck. Whence also it usually happened, that upon the letting in the air from without, the bladders appeared more flaccid and empty than before they were put in; whereas when the bladders were brought us moist from the butchers, we could, without injuring them, tie their necks so close, that none of the air, once blown in, could get out of them, but by violently breaking them.

IT would not be amiss on this occasion to point at something, which may deserve a more deliberate speculation than we can now afford it; namely, that the elastical

power of the ſame quantity of air may be as well increaſed by the agitation of the aëreal particles (whether only moving them more ſwiftly and ſcattering them, or alſo extending or ſtretching them out, I determine not) within an every way incloſing and yet yielding body; as diſplayed by the withdrawing of the air that preſſed it without. For we found, that a bladder but moderately filled with air and ſtrongly tied, being a while held near the fire, not only grew exceeding turgid and hard, but afterwards, being brought nearer to the fire, ſuddenly broke with ſo loud and vehement a noiſe, as ſtunned thoſe that were by, and made us for a while after almoſt deaf.

EXPERIMENT VI.

HAVING thus ſeen, that the air hath an elaſtical power, we were next deſirous to know in ſome meaſure, how far a parcel of air might by this its own ſpring be dilated. And though we were not provided of inſtruments fit to meaſure the dilatation of the air any thing accurately, yet becauſe an imperfect meaſure of it was more deſirable than none at all, we deviſed the following method as very eaſily practicable.

WE took a limber lamb's bladder, which was thoroughly wetted in fair water, that the ſides of it being ſqueezed together, there might be no air left in its folds; (as indeed we could not afterwards upon trial diſcern any.) The neck of this bladder was ſtrongly tied about that of a ſmall glaſs, (capable of holding five full drachms of water) the bladder being firſt ſo compreſſed, that all the included air was only in the glaſs, without being preſſed there. Then the pump being ſet on work, after a few exſuctions, the air in the little phial began to dilate itſelf, and produce a ſmall tumour in the neck of the bladder; and as the ambient air was more and more drawn away, ſo the included air penetrated farther and farther into the bladder, and by degrees lifted up the ſides and diſplayed its folds, till at length it ſeemed to have blown it up to its full extent: whereupon the external air, being permitted to flow back into the receiver, repulſed the air that had filled the bladder into its former narrow receptacle, and brought the bladder to be again flaccid and wrinkled as before. Then taking out the bladder, but without ſevering it from the glaſs, we did, by a hole made at the top of the bladder, fill the veſſel, they both made up, with water, whoſe weight was five ounces five drachms and a half: five drachms whereof were above mentioned to be the contents of the bottle. So that in this experiment, when the air had moſt extended the bladder, it poſſeſſed in all above nine times as much room as it did when it was put into the receiver. And it would probably have much enlarged its bounds, but that the bladder by its weight, and the ſticking together of its ſides, did ſomewhat reſiſt its expanſion: and, which was more conſiderable, the bladder appeared tumid enough, whilſt yet a pretty deal of air was left in the receiver, whoſe exſuction would, according to our former obſervation, probably have given way to a farther expanſion of the air, eſpecially ſuppoſing the dilatation not to be reſtrained by the bladder.

SINCE we wrote the other day the former experiment, we have met with ſome glaſſes not very unfit for our purpoſe; by means of which we are now able, with a little more trouble, to meaſure the expanſion of the air a great deal more accurately than we could by the help of the above-mentioned bladder, which was much too narrow to allow the air its utmoſt diſtenſion.

WE took then firſt a cylindrical pipe of glaſs, whoſe bore was about a quarter of an inch in diameter; this pipe was ſo bent and doubled, that, notwithſtanding its being about two feet in length, it might have been ſhut up into a ſmaller receiver,

not a foot high; but by misfortune it cracked in the cooling, whereby we were reduced to make use of one part which was straight and entire, but exceeded not six or seven inches. This little tube was open at one end; and at the other, where it was hermetically sealed, had a small glass bubble to receive the air whose dilatation was to be measured.

ALONG the side of this tube was pasted a straight narrow piece of parchment, divided into twenty-six equal parts, marked with black lines and figures, that by them might be measured both the included air and its dilatation. Afterwards we filled the tube with water almost to the top; and stopping the open end with the finger, and inverting the tube, the air was permitted to ascend to the above-mentioned glass bubble. And by reason this ascent was very slow, it gave us the opportunity to mark how much more or less than one of the twenty-six divisions this air took up. By this means, after a trial or two, we were enabled to convey to the top of the glass a bubble of air equal enough, as to sight, to one of those divisions. Then the open end of the tube being put into a small phial, whose bottom was covered with water, about half an inch high; we included both glasses into a small and slender receiver, and caused the pump to be set on work. The event was, that at the first exsuction of the air there appeared not any expansion of the bubble, comparable to what appeared at the second, and that upon a very few exsuctions the bubble reaching as low as the surface of the subjacent water, gave us cause to think, that if our pipe had not been broken, it would have expanded itself much farther: wherefore we took out the little tube, and found, that besides the twenty-six divisions formerly mentioned, the glass bubble and some part of the pipe, to which the divided parchment did not reach, amounted to six divisions more. Whereby it appears, that the air hath taken up one and thirty times as much room as before, and yet seemed capable of a much greater expansion, if the glass would have permitted it. Wherefore after the former manner, we let in another bubble, that by our guess was but half as big as the former, and found, that upon the exsuction of the air from the receiver, this little bubble did not only fill up the whole tube, but (in part) break through the subjacent water in the phial, and thereby manifest itself to have possessed sixty and odd times its former room.

THESE two experiments are mentioned to make way for the more easy belief of that which is now to follow. Finding then that our tube was too short to serve our turn, we took a slender quill of glass, which happened to be at hand, though it were not so fit for our purpose as we could have wished, in regard it was three or four times as big at one end as the other. This pipe, which was thirty inches long, being hermetically sealed at the slender end, was almost filled with water; and after the above-related manner a bubble was conveyed to the top of it, and the open extreme was put into a phial that had a little fair water at the bottom: then the cover, by means of a small hole purposely made in it for the glass pipe to stand out at, was cemented on to the receiver, and the pump being set on work, after some exsuctions, not only the air manifestly appeared extended below the surface of the subjacent water, but one of the bystanders affirms, that he saw some bubbles come out at the bottom of the pipe, and break through the water. This done, we left off pumping, and observed how at the unperceived leaks of the receiver the air got in so fast, that it very quickly impelled up the water to the top of the tube, excepting a little space whereunto that bubble was repulsed, which had so lately possessed the whole tube. This air at the slender end appeared to be a cylinder of $\frac{5}{6}$ parts of an inch in length, but when the pipe was taken out and turned upside down, it appeared at the other end inferior in bulk to a pea.

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THESE things being thus done, we took (to make the experiment the more exactly) a small pair of scales, such as goldsmiths use to weigh gold coin in; and weighing the tube and water in it, we found them to amount to one ounce thirty grains and an half: then we poured in as much water as served to fill up the tube, wherein before we had left as much space unfilled up as was possessed by the bubble; and weighing again the pipe and water, we found the weight increased only by one grain. Lastly, pouring out the water, and carefully freeing the pipe from it, (which yet we could not perfectly do) we weighed the glass alone, and found it to want two drachms and thirty-two grains of its former weight: so that the bubble of air taking up the room but of one grain in weight of water, it appeared, that the air by its own ἐλατὴς was so rarified, as to take up one hundred fifty-two times as much room as it did before; though it were then compressed by nothing but the ordinary pressure of the contiguous air. I know not whether it be requisite to take notice, that this experiment was made indeed in a moist night, but in a room, in whose chimney there was burning a good fire, which did perhaps somewhat rarify the air, of which the bubble consisted.

IT hath seemed almost incredible, which is related by the industrious *Mersennus*, that the air, by the violence of the heat, though as great as our vessels can support without fusion, can be so dilated as to take up seventy times as much room as before: wherefore because we were willing to have a confirmation of so strange a phænomenon, we once more conveyed into the tube a bubble of the bigness of the former, and prosecuting the experiment as before with the same water, we observed, that the air did manifestly stretch itself so far, as to appear several times a good way below the surface of the water in the phial, and that too with a surface very convex toward the bottom of the pipe. Nay, the pump being plied a little longer, the air did manifestly reach to that place, where the bottom of the tube leaned upon the bottom of the phial, and seemed to knock upon it and rebound from it: which circumstances we add, partly that the phænomenon we have been relating may not be imputed to the bare subsiding of the water that filled the tube, upon the taking off the pressure of ambient air; and partly also, that it may appear, that if our experiments have not been so accurately made as with fitter instruments might perhaps be possible, yet the expansion of the air is likely to be rather greater than lesser than we have made it; since the air was able to press away the water at the bottom of the pipe, though that were about two inches below the surface of the water that was then in the phial, and would have been at least as high in the pipe, if the water had only subsided and not been depressed: so that it seems not unlikely, that if the experiment could be so made, as that the expansion of the air might not be resisted by the neighbouring bodies, it would yet enlarge its bounds, and perhaps stretch itself to two hundred times its former bulk, if not more. However, what we have now tried will, I hope, suffice to hinder divers of the phænomena of our engine from being distrusted: since in that part of the atmosphere we live in, that which we call the free air (and presume to be so uncompressed) is crouded into so very small a part of that space, which, if it were not hindered, it would possess. We would gladly have tried also, whether the air at its greatest expansion could be farther rarified by heat; but, do what we could, our receiver leaked too fast to let us give ourselves any satisfaction in that particular.

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EXPERIMENT VII.

TO discover likewise, by the means of that pressure of the air, both the strength of glass, and how much interest the figure of a body may have in its greater or lesser resistance to the pressure of other bodies, we made these farther trials.

WE caused to be blown with a lamp a round glass bubble, capable of containing, by guess, about five ounces of water, with a slender neck about the bigness of a swan's quill; and it was purposely blown very thin, as phials made with lamps are wont to be, that the thinness of the matter might keep the roundness of the figure from making the vessel too strong. Then having moderately emptied the receiver, and taken it out of the pump, we speedily applied to the orifice of the bottom of it, the neck of the newly mentioned glass, carefully stopping the crannies with melted plaister, that no air might get in at them: and after turning the key of the stop-cock, we made a free passage for the air to pass out of the bubble into the receiver; which it did with great celerity, leaving the bubble as empty as the receiver itself, as appeared to us by some circumstances not now to be insisted on. Notwithstanding all which, the vessel, continuing as entire as before, gave us cause to wonder that the bare roundness of the figure should enable a glass, almost as thin as paper, to resist so great a pressure as that of the whole incumbent atmosphere. And having reiterated the experiment, we found again, that the pressure of the ambient body, thrusting all the parts inwards, made them, by reason of their arched figure, so support one another, that the glass remained as whole as at first.

Now that the figure of the glass is of great moment in this matter, may be evinced by this other experiment.

EXPERIMENT VIII.

WE took a glass helmet or alembick (delineated by the seventh figure) such as chymists use in distillations, and containing by conjecture between two and three pints: the rostrum or rose of it marked with *c* was hermetically closed; and at the top of it was a hole, into which was fitted and cemented one of the shanks of a middle-sized stop-cock; so that the glass being turned upside-down, the wide orifice (which in common glass-helmets is the only one) was upwards; and to that wide orifice was fitted a cast cover of lead, which was carefully cemented on to the glass: then the other shank of the stop-cock being with cement likewise fastened into the upper part of the pump, the exsuction of the air was endeavoured. But it was not long before, the remaining air being made much too weak to balance the pressure of the ambient air, the glass was (not without a great noise) cracked almost half round, along that part of it where it began to bend inwards; as if in the figure the crack had been made according to the line *a b*. And upon an endeavour to pump out more of the air, the crack once begun, appeared to run on farther; though the glass where it was broken seemed to be (by conjecture) above ten, some thought above twenty times, as thick as the bubble mentioned in the foregoing experiment.

THIS will perhaps make it seem strange, that having taken another glass bubble blown at the same time, and like, for aught we discerned for size, thickness, and figure, to that thin one formerly mentioned; and having sealed it up hermetically, and suspended it in the receiver, the exsuction of the ambient air did not enable the imprisoned

imprisoned air to break, or in the least to crack the bubble; though the experiment were laboriously tried, and that several times with bubbles of other sizes: but that perhaps the heat of the candle or lamp wherewith such glasses are hermetically sealed, (not to mention the warmth of his hands that sealed it) might so rarify the contained air, as much to weaken its spring, may seem probable by the following experiments.

EXPERIMENT IX.

WE took a glass phial able to hold three or four ounces of water, and of the thickness usual in glasses of that size; into the neck of this was put a moderately slender pipe of glass, which was carefully fastened, with a mixture of equal parts of pitch and rosin, to the neck of the phial, and which reached almost to the bottom of it, as the sixth figure declareth.

THIS phial being, upon a particular design, filled with water, till that came up in it a pretty deal higher than the lower end of the pipe, was put into one of our small receivers, (containing between a pint and a quart) in such manner as that the glass pipe, passing through a hole made purposely for it in the leaden cover of the receiver, was for the most part of it without the vessel, which being exactly closed, the pump was set on work: but at the very first exsuction, and before the sucker was drawn to the bottom of the cylinder, there flew out of the phial a piece of glass half as broad as the palm of a man's hand, and it was thrown out with such violence, that hitting against the neighbouring side of the receiver, it not only dashed itself to pieces, but cracked the very receiver in many places, with a great noise that much surprised all that were in the room. But it seemed, that in so little a receiver, the air about the phial being suddenly drawn out, the air imprisoned in the vessel, having on it the whole pressure of the atmosphere (to which by the pipe open at both ends, it and the water were exposed) and not having on the other side the wonted pressure of the ambient air to balance that other pressure, the resistance of glass was finally surmounted, and the phial once beginning to break where it was weakest, the external air might rush in with violence enough to throw the cracked parcel so forcibly against the neighbouring side of the receiver, as to break that too.

AND this may be presumed sufficient to verify what we delivered in that part of our appendix to the first experiment, where we mentioned the almost equal pressure of the air on either side of a thin glass vessel, as the cause of its not being broken by the forcible spring of the contained air. But yet that it be not suspected, that chance had an interest in so odd an experiment as we have been relating, we will add, that, for farther satisfaction, we reiterated it in a round glass containing by guess about six ounces of water: this phial we put into such a small receiver as was lately mentioned, in such manner, as that the bottom of it rested upon the lower part of the pneumatical glass, and the neck came out through the leaden cover of the same at a hole made purposely for it. But being made circumspect by the foregoing mischance, we had put the phial into a bladder, before we put it into the receiver to hinder this last-named glass from being endangered by the breaking of the other. Then the pneumatical vessel being closed, so that no way was left for the outward air to get into it, but by breaking through the phial, into whose cavity it had free access by the mouth of it, (which was purposely left open,) the sucker being nimbly drawn down, the external air immediately pressed forcibly as well upon the leaden-cover as the phial; and the cover happening to be in one place a little narrower than the edge of the pneumatical glass, was depressed, and thrust into it so violently by the

the incumbent air, that getting a little within the tapering lip of the glaſs, it did like a kind of wedge) thruſt out that ſide where it was depreſſed, ſo as, (though the receiver was new) to ſplit it. This accident being thus mentioned upon the bye to confirm what we formerly ſaid touching the fitneſs or unfitneſs of glaſſes of ſome figures to reſiſt the preſſure of the atmoſphere; we will proceed to relate the remaining part of the experiment: namely, that having fitted on a wider cover to the ſame receiver, and cloſed both that and the crack with cement, we proſecuted the experiment in the manner above related, with this ſucceſs; that upon the quick depreſſing of the ſucker, the external air burſt the body of the phial into above an hundred pieces, many of them exceeding ſmall, and that with ſuch violence, that we found a wide rent, beſides many holes, made in the bladder itſelf.

AND to evince, that theſe phænomena were the effects of a limited and even moderate force, and not of ſuch an abhorrency of a vacuum, as that to avoid it, many have been pleaſed to think, that nature muſt, upon occaſion, exerciſe an almoſt boundleſs power; we afterwards purpoſely tried this experiment with ſeveral glaſſes ſomewhat thicker than thoſe phials, and found the event to verify our conjecture, that it would not ſucceed; for the glaſſes were taken out as entire as they were put in.

AND here, my Lord, I hold it not unfit, upon occaſion of the mention that hath been made of our having employed ſmall receivers, and one of them, notwithſtanding its being cracked, to annex theſe two advertiſements.

FIRST then, beſides the great pneumatical glaſs ſo often mentioned, and the proportionate ſtop-cock, we thought fit to provide ourſelves with ſome ſmall receivers, blown of cryſtalline glaſs, of ſeveral ſhapes, and furniſhed with ſmaller ſtop-cocks purpoſely made; and this we did upon hopes, that when we had ſurmounted the difficulties to be met with in cementing the glaſſes to the ſtop-cocks, and the pneumatical veſſels to the pump, ſo exquiſitely as is requiſite for our purpoſe, we ſhould from the ſmallneſs of our receivers receive a four-fold advantage. The firſt, that by reaſon of the ſlenderneſs of the veſſels, and their being made of much purer and clearer metal (as the glaſs-men ſpeak) than the great receiver, we might have a more perfect view of every thing happening within them. The next, that ſuch ſmall veſſels might be emptied with leſs labour and in much leſs time. The third, that this nimble exſuction of the ambient air would make many changes in the bodies ſhut up in theſe glaſſes more ſudden and conſpicuous, than otherwiſe they would prove. And the laſt, that we ſhould be able to draw and keep out the air much more perfectly from ſuch ſmall veſſels than from our large receiver: but though we were not much diſappointed in the expectation of the three firſt advantages, yet we were in our hopes of the fourth; for beſides the great difficulty we found in fitting together the glaſſes, the ſtop-cocks, and the covers; beſides this, I ſay, we found ourſelves ſeldom able to draw, and keep out the air ſo far as to make the remaining air in theſe receivers weaker than the remaining air in our great receiver. For though ſometimes the leaks of ſome of theſe little receivers may be much (either fewer or ſmaller) than thoſe of the larger veſſel; yet a little air getting into one of theſe, wherein it had but little room to expand and diſplay itſelf, might preſs as much upon all parts of the internal ſurface of the veſſel, and upon the included bodies, as a greater quantity of the air in a veſſel in whoſe capacity it might find more room to expand itſelf.

THE other thing, that we were to advertiſe, is, that, it is not every ſmall crack that can make ſuch a receiver as is of a roundiſh figure altogether uſeleſs to our experiment, in regard that upon the exſuction of the internal air, the ambient air on all

ſides preſſing the glaſs inwards or towards the middle, doth conſequently thruſt the lips of the crack cloſer, and ſo rather cloſe than increaſe it.

THIS I mention partly becauſe receivers fit for our turn are more eaſily cracked than procured, and therefore ought not to be unneceſſarily thrown away as unſerviceable; and partly, becauſe I think it becomes one, that profeſſeth himſelf a faithful relator of experiments not to conceal from your lordſhip, that after a few of the foregoing experiments were made, there happened in the great receiver a crack of about a ſpan long, beginning at the upper orifice, and occaſioned, as it ſeemed, by the exceſſive heat of too large an iron that was employed to melt the cement about that orifice. But having laid upon this crack a broad plaiſter, (which in one of our eſſays, written ſome years ſince, to your ingenious and hopeful couſin *Jones*, we extol for the mending of cracked receivers, and other chymical glaſſes;) and having afterwards thickly overlaid this plaiſter with diachylon, we neither could then, nor can yet perceive, that the veſſel leaks ſenſibly at that crack.

THE plaiſter was made of good quick lime finely poudered, and nimbly ground with a peſtle in a mortar, with a quantity (I know not how much preciſely, not having thoſe eſſays in this place) of ſcrapings of cheeſe and a little fair water, no more than is juſt neceſſary to bring the mixture to a ſomewhat ſoft paſte, which, when the ingredients are exquiſitely incorporated, will have a ſtrong and ſtinking ſmell: then it muſt be immediately ſpread upon a linen cloth of three or four fingers breadth, and preſently applied, leſt it begin to harden. But if your Lordſhip had ſeen, how we mended with it receivers even for the moſt ſubtle chymical ſpirits, you would ſcarce wonder at the ſervice it hath done in our pneumatical glaſs.

EXPERIMENT X.

WE took a tallow-candle of ſuch a ſize, that eight of them make about a pound; and having in a very commodious candleſtick let it down into the receiver, and ſo ſuſpended it, that the flame burnt almoſt in the middle of the veſſel, we did in ſome two minutes exactly cloſe it up: and upon pumping very nimbly, we found, that, within little more than half a minute after, the flame went out, though the ſnuff had been purpoſely left of that length we judged the moſt convenient for the laſting of the flame.

BUT the ſecond time having put in the ſame candle into the receiver (after it had by the blaſts of a pair of bellows been freed from fumes) the flame laſted about two minutes from the time the pumper began to draw out the air; upon the firſt exſuction whereof, the flame ſeemed to contract it ſelf in all its dimenſions. And theſe things were farther obſervable, that after the two or three firſt exſuctions of the air, the flame (except at the very top) appeared exceeding blue, and that the flame ſtill receded more and more from the tallow, till at length it appeared to poſſeſs only the very top of the wick, and there it went out.

THE ſame candle being lighted again was ſhut into the receiver, to try how it would laſt there without drawing forth the air, and we found, that it laſted much longer than formerly; and before it went out, receded from the tallow towards the top of the wick, but not near ſo much as in the former experiment.

AND having an intention to obſerve particularly, what the motion of the ſmoke would be in theſe experiments, we took notice, that when the air was not drawn out, there did, upon the extinction of the flame, a conſiderable part of the wick remain kindled,

kindled, which (probably by reason of the circulation of the air in the vessel, occasioned by the heat) emitted a steam, which ascended swiftly and directly upwards in a slender and uninterrupted cylinder of smoke, till it came to the top, whence it manifestly recoiled by the sides to the lower part of the vessel. Whereas when the flame went out upon the exsuction of the air one time (when the flame retired very leisurely to the top) we perceived it not to be followed by any smoke at all. And at another time the upper part of the wick remaining kindled after the extinction of the flame, the slender steam of fumes that did arise ascended but a very little way, and then after some uncertain motions this and that way, did, for the most part, soon fall downwards.

BEING desirous also to try, whether there would be any difference as well in our receiver, as there is wont to be elsewhere betwixt candles made of wax and those made of tallow, as to their duration; we took slender tapers of white wax, (commonly called virgins wax) that being found to burn with much less smoke than common yellow wax. Six of these of like bigness, and each of them of about the thickness of a swan's quill, we pressed together into one candle: and having lighted all the wicks, we let in the above-mentioned wax-candle into the receiver, and made what haste we could to close it up with cement. But, though in the mean while we left open the valve of the cylinder, the hole of the stop-cock, and that in the cover of the receiver, that some air might get in to cherish the flame, and the smoke might have a vent; yet for so great a flame the air sufficed not so much as till the cover should be perfectly luted on: so that before we were quite ready to imploy the pump, the candle was extinguished. Wherefore we took but one of the above-mentioned tapers, and having lighted it, closed it up in the receiver, to try how long a small flame with a proportionable smoke would continue in such a quantity of air: but we found upon two several trials, that from the beginning of pumping, the flame went out in about a minute of an hour. It appeared indeed to us, that the swinging of the wire to and fro (in the engine shaken by pumping) hastened the vanishing of the flame, which seemed by that motion to be cast sometimes on one side of the wick and sometimes on the other. But though once we purposely refrained pumping after a very few exsuctions of the air, that the flame might not be agitated, yet it lasted not much longer than the newly mentioned time. And lastly, closing up the same taper, lighted again, to discover how long it would last without drawing out of the air, we found that it burnt for a while vividly enough, but afterwards began to be lessened more and more in all its dimensions. And we observed, that the flame did not, as before, retire itself by little and little towards the top, but towards the bottom of the wick (from which yet it did a little withdraw upwards just before it went out) so that the upper part of the wick appeared for a pretty while manifestly above the top of the flame, which having lasted about five minutes, was succeeded by a directly ascending stream of smoke.

EXPERIMENT XI.

THERE was taken a wire, which being bent almost in the form of a screw, constituted such an instrument, to contain coals, and leave them every way accessible to the air, as the tenth figure declareth; the breadth of this vessel was no less than that it might with ease be conveyed into the receiver: and having filled it to the height of about five inches with thoroughly kindled wood-coals, we let it down into the glass; and speedily closing it, we caused the pumper to ply his work, and observed, that upon the very first exsuction of the air (though perhaps not because of that only)

the fire in the coals began to grow very dim, and though the agitation of the vessel did make them swing up and down (which in the free air would have retarded the extinction of the fire) yet when we could no longer discern any redness at all in any of them, casting our eyes upon a minute watch we kept by us on this occasion, we found, that from the beginning of the pumping (which might be about two minutes after the coals had been put in glowing) to the total disappearing of the fire, there had passed but three minutes.

WHEREUPON, to try the experiment a little farther, we presently took out the coals, in which it seems there had remained some little parcels of fire, rather covered than totally quenched: for in the open air the coals began to be re-kindled in several places, wherefore having by swinging them about in the wire, throughly lighted them the second time, we let them down again into the receiver, and closed it speedily as before; and then waiting till the fire seemed totally extinct, without meddling with the pump, we found that from the time the vessel was closed, till that no fire at all could be perceived, there had passed about four minutes: whereby it seemed to appear, that the drawing away of the ambient air made the fire go out sooner than otherwise it would have done; though that part of the air that we drew out, left the more room for the stifling streams of the coals to be received into.

LASTLY, having taken out the wire and put other coals into it, we did in the same room where the engine stood, let it hang quietly by a string in the open air, to try how long the fire would last without agitation, when no air was kept from it; and we found, that the fire began to go out first at the top and outside of the coals, but inwards and near the bottom the fire continued visible for above half an hour; a great part of the coals, especially those next the bottom, being burnt to ashes before the fire went out.

WE caused likewise a piece of iron to be forged, of the bigness of a middle-sized charcoal, and having made it red-hot throughout; we caused it in the lately mentioned wire, to be speedily conveyed and shut up into the receiver, being desirous to try what would become of a glowing body, by reason of its texture more vehemently hot than a burning coal of the same bigness, and yet unlike to send forth such copious and stifling fumes: but we could not observe any manifest change upon the exsuction of the air. The iron began indeed to lose its fiery redness at the top, but that seemed to be because it was at the upper end somewhat more slender than at the lower: the redness, though it were in the day-time, continued visible about four minutes; and then, before it did quite disappear, we turned the key of the stop-cock, but could not discern any change of the iron upon the rushing in of the air. Yet some little remainders of wax that stuck to the wire, and were turned into fumes by the heat of the neighbouring iron, seemed to afford a more plentiful, or at least a much more expanded smoke, when the air was sucked out, than afterwards, though allowance was made for the decreasing heat of the iron. And lastly, notwithstanding a considerable exsuction of the ambient air, though not by far so great a one as might have been made by the engine; and notwithstanding the inconsiderable dissipation of the parts of the iron; the surrounding sides of the receiver were sensibly, and almost offensively heated by it; insomuch that a pretty while after the iron was taken out, the sides of the glass manifestly retained a warmth: which would not be unfit to be considered by a person at more leisure than I am now.

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EXPERIMENT XII.

BEING willing to try after this something, that would not cherish much fire at once, and would keep fire much longer than a coal; we took a piece of match, such as soldiers use, of the thickness of a man's little finger, or somewhat thicker; and this being well lighted at one end, was by a string suspended with that end downwards in the cavity of the receiver, which was immediately closed: and yet by that time it could well be so, the copious fumes of the match had near filled and darkened the receiver. Wherefore, lest the vessel should be endangered, the pump was nimbly plied, and a great deal of air and smoke mixt together was drawn out, whereby the receiver growing more clear, we could discern the fire in the match to burn more and more languidly: and notwithstanding that by the diligence used in pumping, it seemed to have room enough allowed it to throw out fumes; yet, after no long time, it ceased from being discernible either by its light or its smoke. And, though by that we were invited to suppose it quite extinguished, yet we continued pumping a-while, in prosecution of another experiment we were trying at the same time: and this we did the more willingly, because of a suspicion the experiment about the coals might easily suggest, and which the event declared not to have been altogether groundless. For upon the admission of the external air, the fire, that seemed to have gone out a pretty while before, did presently revive; and being as it were refreshed by the new air, and blown by the wind made by that air in rushing in, it began again to shine and dissipate the neighbouring fuel into smoke as formerly.

EXPERIMENT XIII.

A WHILE after we let down into the receiver, together with a lighted piece of match, a great bladder well tied at the neck, but very lank, as not containing actually much (if any thing) above a pint of air, but being capable of containing ten or twelve times as much.

OUR scope in this experiment was partly to try, whether or no the smoke of the match, replenishing the receiver, would be able to hinder the dilatation of the inward air, upon the exsuction of the ambient; and partly to discover, whether the extinction of the fire in the match did proceed from want of air, or barely from the pressure of its own fumes, which, for want of room to expand themselves in, might be supposed to recoil upon the fire, and so to stifle it.

THE event of our trial was, that at the beginning of our pumping the match appeared well lighted, though it had almost filled the receiver with its plentiful fumes: but by degrees it burnt more and more dimly, notwithstanding, that by the nimble drawing out the air and smoke, the vessel were made less opacous, and less full of compressing matter; as appeared by this, that the longer we pumped, the less air and smoke came out of the cylinder at the opening of the valve, and consequently the less came into it before; yet the fire in the match went but slowly out. And when afterwards, to satisfy ourselves of its expiration, we had darkened the room, and in vain endeavoured to discover any spark of fire, as we could not for some time before by the help of candles discern the least rising of smoke) we yet continued pumping six or seven times; and after all that, letting in the air, the seemingly dead fire quickly revived, and manifested its recovery by light and store of smoke, with the latter

latter of which it quickly began to replenish the receiver. Then we fell to pumping afresh, and continued that labour so long, till the re-kindled match went out again: and thinking it then fit not to cease from pumping so soon as before, we found, that in less than half a quarter of an hour the fire was got out for good and all, and past the possibility of being recovered by the re-admitted air.

SOME circumstances besides those already mentioned, occurred in the making of the experiment; of which these are the principal.

FIRST, when the receiver was full of smoke, if the cylinder were emptied, immediately upon the turning of the stop-cock, the receiver would appear manifestly darkened to his eye that looked upon the light through it; and this darkness was much less when the receiver was much less filled with fumes: it was also instantaneous, and seemed to proceed from a sudden change of place and situation in the exhalations, upon the vent suddenly afforded them and the air they were mixt with, out of the receiver into the cylinder.

THE next thing we observed was a kind of halos that appeared a good while about the fire, and seemed to be produced by the surrounding exhalations.

AND lastly, it is remarkable, that even when the fumes seemed most to replenish the receiver, they did not sensibly hinder the air included in the bladder from dilating itself after the same manner (for aught we could discern) as it would have otherwise done: so that before the fire or the match was quite extinct, the bladder appeared swelled at least to six or seven times its former capacity.

SINCE the writing of these last lines, we took a small receiver, capable of containing (by guess) about a pound and a half of water; and in the midst of it we suspended a lighted match; but though within one minute of an hour (or thereabouts) from the putting in of the match, we had cemented on the cover, yet we could not make such haste, but that before we began to pump, the smoke had so filled that small receiver, as for aught we discerned, to choke the fire. And having again and again reiterated the experiment, it seemed still as at first, that we could not close up the vessel, and pump out all the fumes time enough to rescue the fire from extinction, whereupon we made use of this expedient. As soon as we had pumped once or twice, we suddenly turned the key, and thereby gave access to the excluded air, which rushing violently in, as if it had been forced thorow a pair of bellows, did both drive away the ashes, fill the glass with fresh air, and by blowing the almost extinguished fire, rekindled it, as appeared by the match's beginning again to smoke, which before it had ceased to do. We having by this means obtained a lighted match in the receiver, without being reduced to spend time to close it up, commanded the air to be immediately pumped out, and found, that upon the exsuction of it, the match quickly left smoking, as it seemed, by reason of the absence of the air; and yet if some urgent occasions had not hindered us, we would for greater security have tried, whether or no the match re-kindled as formerly, would smoke much longer, in case of no exsuction of the ambient air.

EXPERIMENT XIV.

TO try divers things at once, and particularly whether fire, though we found it would not long last, might not be produced in our evacuated receiver; we took a pistol of about a foot in length, and having firmly tied it to a stick almost as long as the cavity of the receiver, we very carefully primed it with well-dried gunpowder, and then cocking it, we tied to the tricker one end of a string, whose other end was fastened to the key formerly mentioned to belong to the cover of our receiver. This

done, we conveyed the piſtol, together with the annexed ſtaff, into the veſſel; which being cloſed up, and emptied after the uſual manner, we began to turn the key in the cover; and thereby ſhortening the ſtring that reached from it to the piſtol, we pulled aſide the tricker, and obſerved, that, according to our expectation, the force of the ſpring of the lock was not ſenſibly abated by the abſence of the air; (from whoſe *impetus* yet ſome modern naturaliſts would derive the cauſe of the motion of reſtitution in ſolid bodies.) For the cock falling with its wonted violence upon the ſteel, ſtruck out of it as many, and as conſpicuous parts of fire, as, for aught we could perceive, it would have done in the open air. Repeating this experiment divers times, we alſo obſerved, whether or no there would appear any conſiderable diverſity in the motion of the ſhining ſparks, in a place where the remaining air was ſo much rarified, but could not perceive but that they moved ſome of them upwards, as well as ſome of them downwards, and ſome of them ſide-ways, as they are wont to do, when upon ſuch colliſions they fly out in the open air.

WE likewiſe cauſed a piece of ſteel to be made of the form and bigneſs of the flint, in whoſe place we put it, and then the piſtol being cocked and conveyed into the receiver, the tricker was pulled after the air was drawn out; and though the place were purpoſely ſomewhat darkened, yet there appeared not upon the ſtriking of the two ſteels againſt each other the leaſt ſpark of fire; nor did we expect any (having before in vain attempted to ſtrike fire this way in the open air) though we thought fit to make the experiment, to undeceive thoſe, who fancy in rarified air, I know not what ſtrange diſpoſition, to take fire upon a much ſlighter occaſion than this experiment afforded. We have indeed found, that by the dextrous colliſion of two hardened pieces of ſteel, ſtore of ſparks may be ſtruck out: but that was done with ſuch vehement percuſſion of the edges of the two ſteels, as could not well be compaſſed in our receiver.

BUT the chief thing we deſigned to do with our piſtol, was, to obſerve, whether gunpowder would take fire in our emptied and cloſely ſtopped glaſs? whether the expanſion of the flame would be conſiderably varied by the abſence of ſo much of the ambient air as was drawn out of the receiver? and whether the flame would diffuſe itſelf upward, as it is wont, notwithſtanding its not having about it the uſual proportion of air to force it up? And though moſt of our attempts to fire the gunpowder in the pan of the piſtol ſucceeded not, becauſe we were fain to let it hang almoſt perpendicular in the receiver, whereby the powder was ſhaken down before the ſparks could reach it: yet once the experiment ſucceeded, and the kindled powder ſeemed to make a more expanded flame than it would have done in the open air, but mounted upwards according to its wont; whether by reaſon of that little portion of air, which in ſpight of our pumping remained in the receiver, or for any other cauſe, we have not now the leiſure to conſider. But we muſt not forget, that, upon the extinction of the flame, the receiver appeared darkened with ſmoak, which ſeemed to move freely up and down, and upon the letting in the air at the ſtop-cock, began to circulate much faſter than before. We would have made more obſervations concerning this flame, but that of two or three attempts we afterwards made to repeat the kindling of the powder, not any one ſucceeded; and we have not the leiſure to dwell long upon one kind of trials.

EXPERIMENT XV.

TO these experiments concerning fire we added another, which, though it succeeded not, may perhaps without impertinency be recorded; partly, because that (as we have in another treatise amply declared) it is useful to recite what experiments miscarry as well as succeed. And partly also, because it is very possible, that what we endeavoured in vain, may be performed by your Lordship, or some other Virtuoso, that shall have stancher vessels than we had, and more sunny days than the present winter allows us.

WE conveyed then into one of our small receivers a piece of matter combustible, dry and black, (experience declaring things of that colour to be most easy kindled;) and carefully closing the vessel, we brought it to a window, at which the sun, not very far from the meridian, shone in very freely: then drawing out the air with speed, we united the sun-beams with a burning-glass upon the combustible matter, which began immediately to send forth a smoak that quickly darkened the receiver; but notwithstanding all our care and diligence the external air got in so fast, that after divers trials we were fain to leave off the experiment in that glass, and induced to make trial of it in our great receiver.

HAVING then after some difficulty lodged the combustible matter in the cavity of this vessel in such manner, as that it was almost contiguous to that side thereof that was next the sun, we did endeavour with a pretty large burning-glass to kindle it; but found, as we feared, that, by reason of the thickness of the glass, (which was also of a less pure and less diaphanous matter than the other) the sun-beams thrown in by the burning-glass, were in their passage so dislocated and scattered, (not now to mention those many that, being reflected, could not pierce into the cavity of the receiver) that we could not possibly unite enough of them to kindle the matter, nor so much as to make it sensibly smoak. Yet we hope, that the seeing whether bodies (other than gun-powder) may be kindled, and what would happen to them, when set on fire in a place in great measure devoid of air, may prove so luciferous an experiment, that when the season is more favourable, we shall, God permitting, make farther trial of it, and acquaint your Lordship with the event, if it proves prosperous. In the mean time we shall pass on to other experiments, as soon as we have advertised your Lordship, that we have forborn to make such reflections upon the several experiments we have set down concerning fire, as the matter would have easily afforded, and your Lordship may perhaps have expected. But I made the less scruple to forbear the annexing of speculations to these recitals, because *Carneades* and *Eleutherius* have in some Dialogues concerning heat and flame, (which were last year seen by some friends, and may be, when you please, commanded by you) mentioned divers of my thoughts and experiments concerning fire.

EXPERIMENT XVI.

WE designed to try, whether or no divers magnetical experiments would exhibit any unusual phænomena, being made in our evacuated receiver instead of the open air: but for want of leisure and conveniency to prosecute such trials, we were induced to reserve the rest for another time, and to content ourselves with making that which follows. We conveyed into the receiver a little pedestal of wood, in

in the midſt of which was perpendicularly erected a ſlender iron, upon whoſe ſharp point an excited needle of ſteel purpoſely made, and of about five inches long, was ſo placed, that hanging in an æquilibrium it could move freely towards either hand. Then the air being after the uſual manner pumped out, we applied a load-ſtone moderately vigorous to the outſide of the glaſs, and found, that it attracted or repelled the ends of the needle, according to the laws magnetical, without any remarkable difference from what the ſame load-ſtone would have done, had none of the air been drawn away from about the needle; which, when the load-ſtone was removed, after ſome tremulous vibrations to and fro, reſted in a poſition, wherein it looked north and ſouth.

EXPERIMENT XVII.

PROCEED we now to the mention of that experiment, whereof the ſatisfactory trial was the principal fruit I promiſed myſelf from our engine, it being then ſufficiently known, that in the experiment *de vacuo*, the quickſilver in the tube is wont to remain elevated, above the ſurface of that whereon it leans, about 27 digits. I conſidered, that, if the true and only reaſon why the quickſilver falls no lower, be, that at that altitude the mercurial cylinder in the tube is in an æquilibrium with the cylinder of air ſuppoſed to reach from the adjacent mercury to the top of the atmoſphere; then if this experiment could be tried out of the atmoſphere, the quickſilver in the tube would fall down to a level with that in the veſſel, ſince then there would be no preſſure upon the ſubjacent, to reſiſt the weight of the incumbent mercury. Whence I inferred (as eaſily I might) that if the experiment could be tried in our engine, the quickſilver would ſubſide below 27 digits, in proportion to the exſuction of air, that ſhould be made out of the receiver. For, as when the air is ſhut into the receiver, it doth (according to what hath above been taught) continue there as ſtrongly compreſſed, as it did whilſt all the incumbent cylinder of the atmoſphere leaned immediately upon it; becauſe the glaſs, wherein it is penned up, hinders it to deliver itſelf, by an expanſion of its parts, from the preſſure wherewith it was ſhut up. So if we could perfectly draw the air out of the receiver, it would conduce as well to our purpoſe, as if we were allowed to try the experiment beyond the atmoſphere.

WHEREFORE (after having ſurmounted ſome little difficulties, which occurred at the beginning) the experiment was made after this manner: we took a ſlender and very curiouſly blown cylinder of glaſs, of near three foot in length, and whoſe bore had in diameter a quarter of an inch, wanting a hair's breadth: this pipe being hermetically ſealed at one end, was, at the other, filled with quickſilver, care being taken in the filling, that as few bubbles as was poſſible ſhould be left in the mercury. Then the tube being ſtopt with the finger and inverted, was opened, according to the manner of the experiment, into a ſomewhat long and ſlender cylindrical box (inſtead of which we now are wont to uſe a glaſs of the ſame form) half filled with quickſilver: and ſo, the liquid metal being ſuffered to ſubſide, and a piece of paper being paſted on level with its upper ſurface, the box and tube and all were by ſtrings carefully let down into the receiver: and then, by means of the hole formerly mentioned to be left in the cover, the ſaid cover was ſlipt along as much of the tube as reached above the top of the receiver; and the interval, left betwixt the ſides of the hole and thoſe of the tube, was very exquiſitely filled up with melted (but not over-hot) diachylon, and the round chink, betwixt the cover and the receiver, was likewiſe very carefully cloſed up: upon which cloſure there appeared not any change in the height

of the mercurial cylinder, no more than if the interposed glass-receiver did not hinder the immediate pressure of the ambient atmosphere upon the inclosed air; which hereby appears to bear upon the mercury, rather by virtue of its spring than of its weight; since its weight cannot be supposed to amount to above two or three ounces, which is inconsiderable in comparison to such a cylinder of mercury as it would keep from subsiding.

ALL things being thus in a readiness, the sucker was drawn down; and, immediately upon the egress of a cylinder of air out of the receiver, the quicksilver in the tube did, according to expectation, subside: and notice being carefully taken (by a mark fastened to the outside) of the place where it stopt, we caused him that managed the pump to pump again, and marked how low the quicksilver fell at the second exsuction; but continuing this work, we were quickly hindered from accurately marking the stages made by the mercury, in its descent, because it soon sunk below the top of the receiver, so that we could henceforward mark it no other ways than by the eye. And thus, continuing the labour of pumping for about a quarter of an hour, we found ourselves unable to bring the quicksilver in the tube totally to subside; because, when the receiver was considerably emptied of its air, and consequently that little that remained grown unable to resist the irruption of the external, that air would (in spight of whatever we could do) press in at some little avenue or other; and though much could not thereat get in, yet a little was sufficient to counterbalance the pressure of so small a cylinder of quicksilver, as then remained in the tube.

Now (to satisfy ourselves farther, that the falling of the quicksilver in the tube to a determinate height, proceedeth from the æquilibrium, wherein it is at that height with the external air, the one gravitating, the other pressing with equal force upon the subjacent mercury) we returned the key and let in some new air; upon which the mercury immediately began to ascend (or rather to be impelled upwards) in the tube, and continued ascending, till, having returned the key, it immediately rested at the height which it had then attained: and so, by turning and returning the key, we did several times at pleasure impel it upwards, and check its ascent. And lastly, having given a free egress at the stop-cock to as much of the external air as would come in, the quicksilver was impelled up almost to its first height: I say almost, because it stopt near a quarter of an inch beneath the paper-mark formerly mentioned; which we ascribed to this, that there was (as is usual in this experiment) some little particles of air engaged among those of the quicksilver; which particles, upon the descent of the quicksilver, did manifestly rise up in bubbles towards the top of the tube, and by their pressure, as well as by lessening the cylinder by as much room as they formerly took up in it, hindered the quicksilver from regaining its first height.

THIS experiment was a few days after repeated, in the presence of those excellent and deservedly famous Mathematic Professors, Dr. *Wallis*, Dr. *Ward*, and Mr. *Wren*, who were pleased to honour it with their presence; and whom I name, both as justly counting it an honour to be known to them, and as being glad of such judicious and illustrious witnesses of our experiment; and it was by their guess, that the top of the quicksilver in the tube was defined to be brought within an inch of the surface of that in the vessel.

AND here, for the illustration of the foregoing experiment, it will not be amiss to mention some other particulars relating to it.

FIRST then, when we endeavoured to make the experiment with the tube closed at one end with diachylon instead of an hermetical seal, we perceived, that upon the drawing of some of the air out of the receiver, the mercury did indeed begin to fall,

but continued afterwards to ſubſide, though we did not continue pumping. When it appeared, that though the diachylon, that ſtopt the end of the tube, were ſo thick and ſtrong, that the external air could not preſs it in, (as experience taught us that it would have done, if there had been but little of it;) yet the ſubtler parts of it were able (though ſlowly) to inſinuate themſelves through the very body of the plaiſter, which it ſeems was not of ſo cloſe a texture, as that which we mentioned ourſelves to have ſucceſsfully made uſe of, in the experiment *de vacuo* ſome years ago. So that now we begin to ſuſpect, that perhaps one reaſon, why we cannot perfectly pump out the air, may be, that when the veſſel is almoſt empty, ſome of of the ſubtler parts of the external air may, by the preſſure of the atmoſphere, be ſtrained through the very body of the diachylon into the receiver. But this is only conjecture.

ANOTHER circumſtance of our experiment was this, that if (when the quickſilver in the tube was fallen low) too much ingreſs were, at the hole of the ſtop-cock, ſuddenly permitted to the external air; it would ruſh in with that violence, and bear ſo forcibly upon the ſurface of the ſubjacent quickſilver, that it would impel it up into the tube rudely enough to endanger the breaking of the glaſs.

WE formerly mentioned, that the quickſilver did not, in its deſcent, fall as much at a time, after the two or three firſt exſuctions of the air, as at the beginning. For, having marked its ſeveral ſtages upon the tube, we found, that at the firſt ſuck it deſcended an inch and $\frac{1}{8}$, and at the ſecond an inch and $\frac{1}{8}$; and when the veſſel was almoſt emptied, it could ſcarce at one exſuction be drawn down above the breadth of a barley-corn. And indeed we found it very difficult to meaſure, in what proportion theſe decrements of the mercurial cylinder did proceed; partly, becauſe (as we have already intimated) the quickſilver was ſoon drawn below the top of the receiver; and partly becauſe, upon its deſcent at each exſuction, it would immediately reaſcend a little upwards; either by reaſon of the leaking of the veſſel at ſome imperceptible hole or other, or by reaſon of the motion of reſtitution in the air, which, being ſomewhat compreſſed by the fall as well as weight of the quickſilver, would repel it a little upwards, and make it vibrate a little up and down, before they could reduce each other to ſuch an æquilibrium as both might reſt in.

BUT though we could not hitherto make obſervations accurate enough, concerning the meaſures of the quickſilver's deſcent, to reduce them into any hypotheſis, yet would we not diſcourage any from attempting it; ſince, if it could be reduced to a certainty, it is probable, that the diſcovery would not be unuſeful.

AND, to illuſtrate this matter a little more, we will add, that we made a ſhift to try the experiment in one of our above mentioned ſmall receivers, not containing a quart; but (agreeably to what we formerly obſerved) we found it as difficult to bring this to be quite empty as to evacuate the greater; the leaſt external air that could get in (and we could not poſſibly keep it all perfectly out) ſufficing, in ſo ſmall a veſſel, to diſplay a conſiderable preſſure upon the ſurface of the mercury, and thereby hinder that in the tube from falling to a level with it. But this is remarkable, that having two or three times tried the experiment in a ſmall veſſel upon the very firſt cylinder of air that was drawn out of the receiver, the mercury fell in the tube 18 inches and a half, and another trial 19 inches and a half.

BUT on this occaſion, I hold it not unfit to give your Lordſhip notice, that I hoped, from the deſcent of the quickſilver in the tube, upon the firſt ſuck, to derive this advantage; that I ſhould thence be enabled to give a nearer gueſs at the proportion of force betwixt the preſſure of the air (according to its various ſtates, as to denſity and rarefaction) and the gravity of quickſilver, than hitherto hath been done. For

in our experiment there are divers things given, that may be made use of towards such a discovery. For first, we may know the capacity of the receiver wherein the experiment is made, since, by filling it with water, we may easily compute how many quarts, or measures of any other denomination, it containeth of air; which air, when shut up in the vessel, may be supposed to have a pressure equal to that of the atmosphere; since it is able to keep the quicksilver in the tube from falling any lower than it did in the free and open air. Next, here is given us the capacity of the brass cylinder, emptied by the drawing down of the sucker (its bore and height being mentioned in the description of our pump) whereby we may come to know how much of the air contained in the receiver is drawn out at the first suck. And we may also easily define, either in weight or cubic measures, the cylinder of quicksilver, that answers to the cylinder of air lately mentioned, (that mercurial cylinder being in our engine computable by deducting from the entire altitude of that cylinder of quicksilver, the altitude at which it rests upon the first exsuction.) But though, if this experiment were very watchfully tried in vessels of several sizes, and the various descents of the quicksilver compared among themselves, it is not improbable, that some such things, as we hoped for, may thereby be discovered. Yet, because not only the solid contents of as much of the glass-tube as remains within the concave surface of the receiver, and (which is more difficult) the varying contents of the vessel containing the mercury, and of as much of the mercury itself as is not in the tube, must be deducted out of the capacity of the receiver, but there must also an allowance be made for this, that the cylinder, that is emptied by the drawing down of the sucker, and comes to be filled upon the letting of the air out of the receiver into it, is not so replenished with air as the receiver itself at first was; because there passeth no more air out of the receiver into the cylinder, than is requisite to reduce the air in the cavity of the cylinder, and in that of the receiver to the same measure of dilatation. Because of these (I say) and some other difficulties, that require more skill in mathematics than I pretend to, and much more leisure than my present occasions would allow me, I was willing to refer the nicer consideration of this matter to some of our learned and accurate mathematicians, thinking it enough for me to have given the hint already suggested.

FOR farther confirmation of what hath been delivered, we likewise tried the experiment in a tube of less than two foot long: and, when there was so much air drawn out of the vessel, that the remaining air was not able to counterbalance the mercurial cylinder, the quicksilver in the tube subsided so visibly, that (the experiment being tried in the little vessel lately mentioned) at the first suck it fell above a span, and was afterwards drawn lower and lower for a little while; and the external air being let in upon it, impelled it up again almost to the top of the tube: so little matters it, how heavy or light the cylinder of quicksilver to subside is, provided its gravity overpower the pressure of as much external air as bears upon the surface of that mercury into which it is to fall.

LASTLY, we also observed, that if (when the mercury in the tube had been drawn down, and by an ingress permitted to the external air, impelled up again to its former height) there were some more air thrust up by the help of the pump into the receiver, the quicksilver in the tube would ascend much above the wonted height of 27 digits, and immediately upon the letting out of that air would fall again to the height it rested at before.

YOUR Lordship will here perhaps expect, that as those, who have treated of the Torricellian experiment, have for the most part maintained the affirmative, or the negative

(negative of that famous queſtion, whether or no that noble experiment infer a vacuum? ſo I ſhould on this occaſion interpoſe my opinion touching that controverſy; or at leaſt declare, whether or no, in our engine, the exſuction of the air do prove the place deſerted by the air ſucked out to be truly empty, that is, devoid of all corporeal ſubſtance. But beſides that I have neither the leiſure, nor the ability, to enter into a ſolemn debate of ſo nice a queſtion; your Lordſhip may, if you think it worth the trouble, in the Dialogues not long ſince referred to, find the difficulties on both ſides repreſented, which then made me yield but a very wavering aſſent to either of the parties contending about the queſtion: nor dare I yet take upon me to determine ſo difficult a controverſy.

FOR on the one ſide it appears, that notwithſtanding the exſuction of the air, our receiver may not be deſtitute of all bodies, ſince any thing placed in it, may be ſeen there; which would not be, if it were not pervious to thoſe beams of light, which rebounding from the ſeen object to our eyes, affect us with the ſenſe of it: and that either theſe beams are corporeal emanations from ſome lucid body, or elſe at leaſt the light they convey doth reſult from the briſk motion of ſome ſubtle matter, I could, if I miſtake not, ſufficiently manifeſt out of the Dialogues above-mentioned, if I thought your Lordſhip could ſeriouſly imagine that light could be conveyed without, at leaſt, having (if I may ſo ſpeak) a body for its vehicle.

BY the ſixteenth experiment, it alſo appears that the cloſeneſs of our receiver hinders it not from admitting the effluvia of the load-ſtone; which makes it very probable that it alſo freely admits the magnetical ſteams of the earth; concerning which, we have in another treatiſe endeavoured to manifeſt that numbers of them do always permeate our air.

BUT on the other ſide it may be ſaid, that as for the ſubtle matter which makes the objects encloſed in our evacuated receiver, viſible, and the magnetical effluvia of the earth that may be preſumed to paſs thorough it, though we ſhould grant our veſſel not to be quite devoid of them, yet we cannot ſo reaſonably affirm it to be repleniſhed with them, as we may ſuppoſe, that if they were gathered together into one place without intervals between them, they would fill but a ſmall part of the whole receiver. As in the thirteenth experiment, a piece of match was inconſiderable for its bulk, whilſt its parts lay cloſe together, that afterwards (when the fire had ſcattered them into ſmoke) ſeemed to repleniſh all the veſſel. For (as elſewhere our experiments have demonſtrated) both light and the effluvia of the load-ſtone may be readily admitted into a glaſs, hermetically ſealed, though before their admiſſion, as full of air as hollow bodies here below are wont to be; ſo that upon the exſuction of the air, the large ſpace deſerted by it, may remain empty, notwithſtanding the pretence of thoſe ſubtle corpuſcles, by which lucid and magnetical bodies produce their effects.

AND as for the allegations above-mentioned, they ſeemed to prove but that the receiver devoid of air, may be repleniſhed with ſome etherial matter, as ſome modern Naturaliſts write of, but not that it really is ſo. And indeed to me it yet ſeems, that as to thoſe ſpaces which the Vacuiſts would have to be empty, becauſe they are manifeſtly devoid of air and all groſſer bodies; the Pleniſts (if I may ſo call them) do not prove that ſuch ſpaces are repleniſhed with ſuch a ſubtle matter as they ſpeak of, by any ſenſible effects, or operations of it (of which divers new trials purpoſely made, have not yet ſhewn me any) but only conclude that there muſt be ſuch a body, becauſe there cannot be a void. And the reaſon why there cannot be a void, being by them taken, not from any experiments, or phænomena of nature, that clearly and particularly prove their hypotheſis, but from their notion of a body, whoſe nature,

ture, according to them, consisting only in extension (which indeed seems the property most essential to, because inseparable from a body) to say a space devoid of body, is, to speak in the schoolmen's phrase, a contradiction *in adjecto*. This reason, I say, being thus desumed, seems to make the controversy about a vacuum rather a metaphysical, than a physiological question; which therefore we shall here no longer debate, finding it very difficult either to satisfy Naturalists with this Cartesian notion of a body, or to manifest wherein it is erroneous, and substitute a better in its stead.

BUT though we are unwilling to examine any farther the inferences wont to be made from the Torricellian experiment, yet we think it not impertinent to present your Lordship with a couple of advertisements concerning it.

FIRST then, if in trying the experiment here or elsewhere, you make use of the English measures that mathematicians and tradesmen are here wont to employ, you will, unless you be forewarned of it, be apt to suspect that those that have written of the experiment have been mistaken. For whereas men are wont generally to talk of the quicksilver's remaining suspended at the height of between six or seven and twenty inches; we commonly observed, when divers years since we first were solicitous about this experiment, that the quicksilver in the tube rested at about 29 inches and a half above the surface of the restagnant quicksilver in the vessel, which did at first both amaze and perplex us, because though we held it not improbable that the difference of the grosser English air, and that of Italy and France, might keep the quicksilver from falling quite as low in this colder, as in those warmer climates; yet we could not believe that that difference in the air should alone be able to make so great an one in the heights of the mercurial cylinders; and accordingly upon enquiry we found, that though the various density of the air be not to be overlooked in this experiment, yet the main reason why we found the cylinder of mercury to consist of so many inches, was this, that our English inches are somewhat inferior in length to the digits made use of in foreign parts, by the writers of the experiment.

THE next thing I desire your Lordship to take notice of, is, that the height of the mercurial cylinder is not wont to be found altogether so great as really it might prove, by reason of the negligence or incogitancy of most that make the experiment. For oftentimes upon the opening of the inverted tube into the vesselled mercury, you may observe a bubble of air to ascend from the bottom of the tube through the subsiding quicksilver to the top; and almost always you may, if you look narrowly, take notice of a multitude of small bubbles all along the inside of the tube betwixt the quicksilver and the glass; (not now to mention the particles of air that lie concealed in the very body of the mercury:) many of which, upon the quicksilver's forsaking the upper part of the tube, do break into that deserted space where they find little or no resistance to their expanding of themselves. Whether this be the reason, that upon the application of warm bodies to the emptied part of the tube, the subjacent mercury would be depressed somewhat lower, we shall not determine; though it seem very probable, especially since we found, that, upon the application of linen cloths dipped in water, to the same part of the tube, the quicksilver would somewhat ascend; as if the cold had condensed the imprisoned air (that pressed upon it) into a lesser room. But that the deserted space is not wont to be totally devoid of air, we were induced to think by several circumstances: for when an eminent mathematician, and excellent experimenter, had taken great pains and spent much time in accurately filling up a tube of mercury, we found that yet there remained store of inconspicuous bubbles, by inverting the tube, letting the quicksilver fall to its wonted height; and by applying (by degrees) a red-hot iron to the outside of the tube, over

over againſt the upper part of the mercurial cylinder, (for hereby the little unheeded bubbles, being mightily expanded, aſcended in ſuch numbers, and ſo faſt to the deſerted ſpace, that the upper part of the quickſilver ſeemed, to our wonder, to boil.) We farther obſerved, that in the trials of the Torricellian experiment, we have ſeen made by others, and (one excepted) all our own, we never found that, upon the inclining of the tube, the quickſilver would fully reach to the very top of the ſealed end: which argued, that there was ſome air retreated thither that kept the mercury out of the unrepleniſhed ſpace.

IF your Lordſhip ſhould now demand what are the beſt expedients to hinder the intruſion of the air in this experiment; we muſt anſwer, that of thoſe which are eaſily intelligible without ocular demonſtration; we can at preſent ſuggeſt, upon our own trials, no better than theſe. Firſt, at the open end of the tube the glaſs muſt not only be made as even at the edges as you can, but it is very convenient (eſpecially if the tube be large) that the bottom be every way bent inwards, that ſo the orifice not much exceeding a quarter of an inch in diameter, may be the more eaſily and exactly ſtopped by the experimenter's finger; between which and the quickſilver, that there may be no air intercepted (as very often it happens that there is) it is requiſite that the tube be filled as full as poſſibly it can be, that the finger which is to ſtop it, preſſing upon the accumulated and protuberant mercury, may rather throw down ſome, than not find enough exactly to keep out the air. It is alſo an uſeful and compendious way not to fill the tube at firſt quite of mercury, but to leave near the top about a quarter of an inch empty; for if you then ſtop the open end with your finger, and invert the tube, that quarter of an inch of air will aſcend in a great bubble to the top, and in its paſſage thither, will gather up all the little bubbles, and unite them with itſelf into one great one; ſo that if by reinverting the tube, you let that bubble return to the open end of it, you will have a much cloſer mercurial cylinder than before, and need but to add a very little quickſilver more to fill up the tube exactly. And laſtly, as for thoſe leſſer and inconſpicuous parcels of air which cannot this way be gleaned up, you may endeavour, before you invert the tube, to free the quickſilver from them by ſhaking the tube, and gently knocking on the outſide of it, after every little parcel of quickſilver which you pour in; and afterwards, by forcing the ſmall latitant bubbles of air to diſcloſe themſelves and break, by imploying a hot iron in ſuch manner as we lately mentioned. I remember that by carefully filling the tube, though yet it were not quite free from air, we have made the mercurial cylinder reach to 30 inches and above an eighth, and this in a very ſhort tube: which we therefore mention, becauſe we have found, by experience, that in ſhort tubes a little air is more prejudicial to the experiment than in long ones, where the air having more room to expand itſelf, doth leſs potently preſs upon the ſubjacent mercury.

AND ſince we are fallen upon the conſideration of the altitude of the mercurial cylinder, I muſt not conceal from your Lordſhip an experiment relating thereunto, which perhaps will ſet both you and many of your friends, the Vertuoſi, on thinking; and, by diſcloſing ſome things about the air or atmoſphere, that have ſcarce hitherto been taken notice of, may afford you ſome hint conducive to a further diſcovery of the ſubject of this epiſtle.

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EXPERIMENT XVIII.

WE took a glass tube, which though it were not much above three foot long, we made choice of, because it was of a more than ordinarily even thickness. This we filled with mercury, though not with as much care as we could, yet with somewhat more than is wont to be used in making the Torricellian experiment. Then having, according to the manner, inverted the tube, and opened the mouth of it beneath the surface of some other quicksilver, that in the tube fell down to the wonted height, leaving, as is usual, some little particles of air in the space it deserted; as we guessed by observing, that upon the application of hot bodies to the upper part of the tube, the quicksilver would be a little depressed. Lastly, having put both the tube and the vessel it leaned on into a convenient wooden frame to keep them from mischances; we placed that frame in a window within my bed-chamber, that I might both keep the mercury from being stirred, and have opportunity to watch from time to time the phænomena it was to exhibit. For the better discovery of which, when the quicksilver both in the tube and subjacent vessel was perfectly at rest, we took notice, by a mark made on the outside of the glass, how high the included liquor then reached.

DURING several weeks that the tube was kept in that window (which was very rarely opened) I had the opportunity to observe, that the quicksilver did sometimes faintly imitate the liquor of a weather-glass, subsiding a little in warm, and rising a little in cold weather; which we ascribed to the greater or lesser pressure of that little air that remained at the top of the tube, expanded or condensed by the heat or cold that affected the ambient air. But that which I was chiefly careful to observe, was this, that oftentimes the quicksilver did rise and fall in the tube, and that very notably, without conforming itself to what is usual in weather-glasses, whose air is at the top, nay, quite contrary thereunto: for sometimes I observed it in very cold weather (such as this winter hath already afforded us good store of) to fall down much lower than at other times, when by reason of the absence of both frost, snow, and sharp winds, the air was comparatively much warmer. And I farther observed, that sometimes the quicksilver would for some days together rest almost at the same height; and at other times again it would in the compass of the same day considerably vary its altitude, though there appeared no change either in the air abroad, or in the temper of the air within the room (wherein was constantly kept a good fire) nor in any thing else, to which either I, or some eminently learned men, whom I then acquainted with the experiment, could reasonably impute such a change: especially considering that the space wherein the mercury wandered up and down, within about five weeks, amounted to full two inches, of which we found by our several marks whereby we had taken notice of its several removes, that it had descended about $\frac{9}{16}$ of an inch from the place where it first settled, and the other inch and $\frac{7}{16}$ it had ascended. And it seems probable that the height of the mercurial cylinder would have varied yet more, if the experiment had been made in the open air, and in a long tube, where the particles of the imprisoned air, by having more room to display themselves in, might not have had so strong a spring to work upon the quicksilver with. But for want both of time and of a competent quantity of mercury (which was not to be procured where we then happened to be) we were unable to make any farther trials: which therefore chiefly troubled us, because we would gladly have tried

tried an ingenious experiment which was ſuggeſted unto us by that excellent mathematician Mr. *Wren*, who being invited to name any thing he would have us try touching the preſſure of the air, deſired us to obſerve whether or no the quickſilver in a long tube would not a little vary its height according to the tides, eſpecially about the new and full moon; about which times mariners obſerve thoſe great flowings and ebbs of the ſea, that they call the ſpring tides. For he ſagaciouſly and plauſibly conjectured that ſuch obſervations, accurately made, would diſcover the truth or erroneouſneſs of the Carteſian hypotheſis concerning the ebbing and flowing of the ſea: which *Des Cartes* aſcribeth to the greater preſſure made upon the air by the moon, and the intercurrent ethereal ſubſtance at certain times (of the day, and of the lunary month) than at others. But in regard we found the quickſilver in the tube to move up and down ſo uncertainly, by reaſon, as it ſeems, of accidental mutation in the air; I ſomewhat doubt whether we ſhall find the altitude of the quickſilver to vary as regularly as the experiment is ingeniouſly propoſed. The ſucceſs we ſhall (God permitting us to make trial of it) acquaint your Lordſhip with; and in the mean time take notice, that when we had occaſion to take the tube out of the frame (after it had ſtaid there part of *November* and part of *December*) a good fire being then in the room, becauſe it was a ſnowy day, we found the quickſilver in the tube to be above the upper ſurface of the ſubjacent mercury 29 inches three quarters.

IF your Lordſhip ſhould now aſk me what are the true cauſes of this varying altitude of the mercurial cylinder; I ſhould not undertake to anſwer ſo difficult a queſtion, and ſhould venture to ſay no more, than that among divers poſſible cauſes to which it may be aſcribed, it would not be, perhaps, abſurd to reckon theſe that follow.

FIRST then, we may conſider, that the air in the upper part of the tube is much more rarefied, and therefore more weak than the external air, as may appear by this among other things, that upon the inclining of the tube, the quickſilver will readily aſcend almoſt to the very top of it, and ſo take up eight or nine tenth parts, and perhaps more of that ſpace which it deſerted before: which would not happen, if that whole ſpace had been full of unrarefied air, ſince that (as trial may eaſily ſatisfy you) would not have ſuffered itſelf to be thruſt into ſo narrow a room by ſo weak a preſſure. So that although in our tube when the included air was heated, the quickſilver was ſomewhat depreſſed; yet there is this difference betwixt ſuch a tube and common weather-glaſſes, that in theſe the included and the ambient air are in an æquilibrium as to preſſure, and the weight of the water that keeps them ſeparate is ſcarce conſiderable. Whereas in ſuch a tube as we are ſpeaking of, the air within is very much more dilated than that without. And it is not ſo much the ſpring or reſiſtance of the included air, as the weight of the mercurial cylinder itſelf, that hinders the quickſilver from aſcending higher: for if we ſhould ſuppoſe that deſerted part of the tube perfectly devoid of air, yet would the quickſilver riſe but a little higher in it, and be far from filling it; in regard the outward air would not be able to impel up ſuch a weight much higher: whereas it may, by our former experiments appear, that if all the air in the upper part of the weather-glaſs were away, the water would be impelled up to the very top of it, though the pipe were above thirty foot long.

WE may next conſider, that this rarefied air, at the upper part of our tube, being exactly ſhut up betwixt the glaſs and the quickſilver, it was ſcarce ſubject to any diſcernable alterations, ſave thoſe it received from heat and cold.

AND we may farther conſider, that yet the external air or atmoſphere is ſubject to many alterations, beſides thoſe that proceed from either of thoſe qualities.

FOR the experiment that occasioned this discourse, seems to make it probable enough, that there may be strange ebbings and flowings, as it were, in the atmosphere, or at least, that it may admit great and sudden mutations, either as to its altitude or its density, from causes, as well unknown to us, as the effects are unheeded by us. And that you may not think that there is nothing in nature but our experiment that agrees with this our conjecture, we might put your Lordship in mind of the pains and aches that are often complained of by those that have had great wounds or bruises, and that do presage great mutations in the air oftentimes, whilst to strong and healthy persons no sign of any such thing appears. And that is also very memorable to this purpose, which I remember I have somewhere read in a book of the ingenious *Kircherus*, who giving a pertinent admonition concerning the various refractions that may happen in the air, relateth, that during his stay in *Malta*, he often saw mount *Ætna*, though the next day, notwithstanding its being extremely clear, he could not see it; adding, that *Vintemillius*, a very learned person, did oftentimes, from a hill he names, behold the whole island he calls *Luprica* protuberant above the sea, though at other times, notwithstanding a clear sky, he could not see it. And though perhaps this may be in part ascribed to the various light and position of the sun, or to the various disposition of the spectator's eye, or peradventure to some other cause; yet the most probable cause seems to be the differing density of the air, occasioned by exhalations capable to increase the refraction, and consequently bring beams to the eye, which otherwise would not fall on it. We have likewise, in another treatise, mentioned our having often observed with telescopes a plenty of steams in the air, which without such a help would not be taken notice of, and which, as they were not at all times to be seen even through a telescope, so they did sometimes, especially after a shower of rain, hastily disappear: and when we have visited those places that abound with mines, we have several times been told by the diggers, that even when the sky seemed clear, there would, not seldom, suddenly arise, and sometimes long continue, a certain steam (which they usually call a damp) so gross and thick that it would oftentimes put out their very candles, if they did not seasonably prevent it. And I think it will easily be granted, that the ascension of such steams into this or that part of the air, and their mixing with it, are very like to thicken it; as on the other side either heat or the sudden condensation of the air in another part of the atmosphere (to mention now no other causes) are capable of rarefying it.

NOR will it very much import the main scope of our discourse, whether it be supposed that the copious steams the earth sends into the air, thicken that part of the atmosphere that receives them, and make it more heavy; or that sometimes the fumes may ascend with such celerity, that though the air be thickened, yet they rather diminish than encrease its gravitation, in regard that the quickness of their ascent not only keeps them from gravitating themselves, but may hinder the pressing downwards of many aërial corpuscles that they meet with in their way upwards. This, I say, is of no great importance to our present discourse, since either way the terrestrial steam may here and there considerably alter the gravity or pressure of the atmosphere.

YOUR Lordship may also be pleased to remember, that by our seventeenth experiment it appeared, that, as when the air in the receiver was expanded more than ordinarily, the quicksilver in the tube did proportionably subside; so when the air in the same receiver was a little more than ordinarily compressed, it did impel up the quicksilver in the tube, above the wonted height of betwixt six and seven and twenty digits.

AND

AND if to these things we annex, that, for aught we can find by trials purposely made, the degree of rarity or density of the air, shut up into our receiver, doth not sensibly alter its temperature as to cold or heat; it will not, I hope, appear absurd to conceive, that since the air, included in the tube, could but very faintly hinder the ascent of the quicksilver, or press it downwards; since too, that included air could scarce immediately receive any sensible alteration, save either by heat or cold; and since also, that according to the bare density or rarity of the air incumbent on the subjacent quicksilver in the vessel, that in the tube was impelled more or less high; such changes happening in the neighbouring part of the outward air, either by the ascension of gross or copious exhalations, or by any other cause (of which there may be divers) as were capable to make considerable alterations in the consistence of the air, as to rarity and density, may be able proportionably to alter the height of the quicksilver: I rather say, that such alterations may be, than that they are the causes of our phænomenon; because I think it sufficient, if I have proposed conjectures not altogether irrational about a new mystery of nature, touching which, the chief things I pretend to, is to give occasion to the curious to enquire farther into it, than I have been yet able to do.

EXPERIMENT XIX.

THE same reason that moved us to conclude, that by the drawing of the receiver, the mercury would descend in a tube shorter than six and twenty digits, induced us also to expect, that by the same means water might be brought to subside in glass-tubes of a moderate length: though by the noble experiment, said to have been accurately made in *France*, by Monsieur *Paschal*, we are informed that a tube of no less than about two and thirty foot, was found requisite to make the experiment *de vacuo* succeed with water instead of quicksilver; so tall a cylinder of that lighter liquor being, it seems, requisite to equal the weight of a mercurial cylinder of six or seven and twenty digits, and surmount the pressure of the atmosphere.

WE took then a tube of glass, hermetically sealed at one end, of about four foot in length, and not very slender: this, at the open end, we filled with common water, and then stopped that end till we had inverted the tube, and opened it beneath the surface of a quantity of the like water, contained in a somewhat deep and slender vessel. This vessel, with the tube in it, was let down into the receiver, and the receiver being closed up after the accustomed manner, the pump was set on work.

As much of the event as concerns our present purpose, was this, that till a considerable part of the air was drawn out of the receiver, the tube continued top full of water as when it was put in, it being requisite that a great part of the air, formerly contained in the receiver, should be drawn out, to bring the remaining air to an æquilibrium, with so short and light a cylinder of water. But when once the water began to fall in the tube, then each exsuction of air made it descend a little lower, though nothing near so much as the quicksilver at the beginning did in the experiment formerly mentioned. Nor did there appear so much inequality in the spaces transmitted by the water in its descent, as there did in those observed in the fall of the quicksilver; of which the cause will scarce seem abstruse to him that shall duly reflect upon what hath been already delivered. And whereas we drew down the quicksilver in the tube so far, as to bring it within an inch of the surface of the other quicksilver into which it was to fall; the lowest we were able to draw down the water

was, by our conjecture, to about a foot or more above the surface of that in the vessel; of which I know not whether it will be needful to assign so obvious a cause as that, though the little air remaining in the receiver could not hinder a cylinder of above an inch high of quicksilver from subsiding; yet it might be very well able, by its pressure, to countervail the weight of a cylinder of a foot long or more, of a liquor so much less ponderous than quicksilver, as water is. And in fine, to conclude our experiment, when the water was drawn down thus low, we found, that by letting in the outward air, it might be immediately impelled up again to the higher parts of the tube.

WE will add no more concerning this experiment, save that having tried it in one of our small receivers, we observed, that upon the first exsuction of the air the water did usually subside divers inches, and at the second (exsuction) fall down much lower, subsiding sometimes near two foot; as also that upon the letting in of the air from without, the water was impelled up with very great celerity.

EXPERIMENT XX.

THAT the air hath a notable elastical power (whencesoever that proceeds) we have, I suppose, abundantly evinced, and it begins to be acknowledged by the eminentest modern naturalists. But whether or no there be in water so much as a languid one, seems hitherto to have been scarce considered, nor hath been yet, for aught I know, determined either way by any writer; which invited us to make the following experiment.

THERE was taken a great glass-bubble, with a long neck, (such as chymists are wont to call a philosophical egg) which being filled with common water till the liquor reached about a span above the bubble, and a piece of paper being there pasted on, was put unstopped into the receiver, and then the air was sucked out after the wonted manner. The event was this, that a considerable part of the air, penned up in the receiver, was drawn out, before we discerned any expansion of the water: but, continuing the labour of pumping, the water manifestly began to ascend in the stem of the glass, and divers bubbles loosening themselves from the lower parts of the vessel, made their way through the body of the water, to the top of it, and there brake into the receiver: and after the water once appeared to swell, then at each time the stop cock was turned to let out the air from the receiver into the pump, the water in the neck of the glass did suddenly rise about the breadth of a barley-corn in the neck of the glass, and so attained by degrees to a considerable height above the mark formerly mentioned. And at length (to make the expansion of the water more evident) the outward air was suddenly let in, and the water immediately subsided, and deserted all the space it had newly gained in the glass.

AND, on this occasion, it will not perhaps be amiss to acquaint your Lordship here (though we have already mentioned it in another paper, to another purpose) with another expedient that we made use of two or three years ago, to try whether or no water had a spring in it. About that time then, that great and learned promoter of experimental philosophy Dr. *Wilkins*, doing me the honour to come himself, and bring some of his inquisitive friends to my lodging, we there had in readiness a round and hollow vessel of pewter, great enough to contain two pounds of water, and exactly close every where but at one little hole where it was to be filled; then partly by sucking out the air, and partly by injecting water with a syringe, it was (not without some difficulty) filled up to the top; and that hole being placed directly upwards, there

there was a little more water leisurely forced in by the syringe. Upon which, though the vessel were permitted to rest, and the hole kept in its former posture, yet the compressed wat r leisurely swelled above the orifice of the hole, and divers drops ran over along the sides of the vessel. After this we caused a skilful pewterer (who had made the globe) to close it up in our presence with soder so exquisitely, that none suspected there was any thing left in it besides water. And lastly, the vessel thus sodered up, was warily and often struck in divers places with a wooden mallet, and thereby was manifestly compressed, whereby the inclosed water was crouded into less room than it had before: and thereupon we took a needle, and with it and the mallet perforated the vessel, and drew out the needle again; the water (but in a very slender stream) was suddenly thrown after it into the air, to the height of two or three feet. As for the other phænomena of this experiment, since they belong not to our present purpose, and are partly mentioned in another of our papers, we shall, instead of recording them here, give this advertisement: that as evidently as this experiment, and that made in our receiver, seem to prove a power in the water to expand and restore itself after compression; yet for a reason to be met with ere long, I judged it not safe to infer that conclusion from these premises, till I had made some of the following trials, to the mention of which I will therefore hasten.

EXPERIMENT XXI.

TO discover whether the expansion of the water really proceeded from an elastical power in the parts of the water itself, we thought it requisite to try two things: the one, whether or no the atmosphere gravitates upon bodies under water; and the other, whether in case it do gravitate, the intumescence of the water may not be ascribed to some substance subtler than itself, residing in it. In order to the satisfying myself about the first of these, I intended to let down into the receiver a vessel of water, wherein should be immersed a very small oiled bladder, almost devoid of air, but strongly tied up at the neck with a string, and detained a little under water by such a weight fastened to that string, as should just be able to keep the bladder from swimming, and no more. For I supposed, that if when all things were thus ordered, the receiver were emptied, in case there were any such pressure of the atmosphere upon water, as I was inclined to believe, the air within the bladder, being, upon the exsuction of the air within the receiver, freed from that pressure, and being pressed only by the small weight of the incumbent water, would considerably expand itself; but whilst we were preparing bladders for this experiment, there occurred an easy way for the making at once both the discoveries I desired.

WE took then a glass phial, containing by guess a pound and some ounces of water; this we filled top full, and then we put into the neck of it a glass pipe a pretty deal bigger than a goose-quill, open at both ends, and of divers inches in length: one end of this pipe was so put into the neck of the phial, as to reach a little below it, and then was carefully cemented thereto, that no air might get into the phial, nor any water get out of it, otherwise than through the pipe; and then the pipe being warily filled, about half-way up to the top, with more water, and a mark being pasted over against the upper surface of the liquor, the phial thus fitted with the pipe was, by strings let down into the receiver, and according to the wonted manner exquisitely closed up in it.

THIS done, we began to pump out the air, and when a pretty quantity of it had been drawn away, the water in the pipe began to rise higher in the pipe, at the sides

of

of which some little bubbles discovered themselves. After a little while longer, the water still swelling, there appeared at the bottom of the pipe a bubble about the bigness of a small pea, which ascending through the pipe to the top of the water, stayed there a while and then broke; but the pump being nimbly plied, the expansion of the water so increased, that quickly, getting up to the top of the pipe, some drops of it began to run down along the outside of it; which obliged us to forbear pumping a while, and give the water leave to subside within less than two inches of the bottom of the pipe. After this, the pump being again set on work, the bubbles began to ascend from the bottom of the pipe, being not all of a size, but yet so big, that estimating one with another, they appeared to be of the size of the smaller sort of peas; and of these we reckoned about sixty which came up one after another, besides store of smaller ones, of which we made no reckoning. And at length, growing weary of reckoning and pumping too (because we found, that in spight of all our pains and industry, some undiscerned leak or other in the receiver hindered us from being able to empty it altogether) we thought fit to desist for that time, after trial made of what operation the external air, being let in upon the expanded water, would have; and accordingly turned the key to let in the air, we saw, as we expected, that the water in the pipe in a moment fell down almost to the bottom of it.

Now of this experiment there are two or three circumstances yet to be mentioned, which are, no less than those already recited, pertinent to our present purpose.

IN the first place then, when the greater part of the air had been pumped out of the receiver, the rising bubbles ascended so very slow in the pipe, that their progress was scarce discernible; which seemed to proceed from this, that their bigness was such, that they could not sufficiently extend themselves in the cavity of the glass, without pressing on both hands against the sides of it, whereby they became of more difficult extrusion to the water. And though it may seem strange that these bubbles should be of any considerable bulk, since it is like they consisted of lesser parcels of the air lurking in the water, than those that were vigorous enough to make their way through, long before them; yet they were commonly much larger than before, some of them being equal in quantity to four or five peas; whether this their increase of bulk proceeded from the greater decrement of the pressure of the air, or from the union of two or three of those numerous bubbles which were then generated below the bottom of the pipe, where we could not see what was done among them.

ANOTHER thing we noted in our bubbles was, that whereas in ordinary ones the air, together with the thin film of water that invests and detains it, is wont to swell above the surface of the water it swims on, and commonly to constitute hemispherical bodies with it, the little parcels of air that came up after the receiver was pretty well emptied, did not make protuberant bubbles, but such whose upper surface was either level with, or beneath that of the water; so that the upper surface being usually somewhat convex, the less protuberant parts of it had a pretty quantity of water remaining above them.

WE also farther observed, that whereas in the bubbles that first appeared in the pipe, the ascending air did, as in other common bubbles, make its way upwards by dividing the water through which it passed, in those bubbles that appeared at the latter end of our experiment, when the pressure of the little external air, remaining in the receiver, was grown inconsiderable; the ascending parcels of air having now little more than the weight of the incumbent water to surmount, were able both so to expand themselves as to fill up that part of the pipe which they pervaded, and by pressing every way against the sides of it, to lift upwards with them what water they

found above them, without letting any considerable quantity glide down along the sides of the glass: so that sometimes we could see a bubble thrust on before it a whole cylinder of water of perhaps an inch high, and carry it up to the top of the pipe; though, as we formerly noted, upon the letting in the external air, these tumid bubbles suddenly relapsed to their former inconspicuousness.

ALL these things laid together seemed sufficiently to confirm that, which the consideration of the thing itself would easily enough persuade; namely, that the air, and such like bodies being under water, may be pressed upon as well by the atmosphere, as by the weight of the incumbent water itself.

HENCE likewise we may verify what we observed at the close of the foregoing experiment; namely, that from the sole swelling of water there recorded, it cannot be so safely concluded, that water, when freed from compression, is endowed with an elastical power of expanding itself: since thereby it appears that the intumescence produced by that experiment, may (at least in great part) be ascribed to the numerous little bubbles which are wont to be produced in water, from which the pressure of the atmosphere is in great measure taken off. So apt are we to be misled, even by experiments themselves, into mistakes, when either we consider not that most effects may proceed from various causes, or mind only those circumstances of our experiment, which seem to comply with our preconceived hypothesis or conjectures.

AND hence it seems also probable, that in the pores or invisible little recesses of water itself there lie commonly interspersed many parcels of either air, or at least something analogous thereunto, although so very small, that they have not been hitherto so much as suspected to lurk there. But if it be demanded how it appears that there is interspersed through the body of water any substance thinner than itself, and why that which produced the bubbles above mentioned should not be resolutely said to be nothing else than a more active and spirituous part of the water; we shall, in order to the elucidation of this matter, subjoin, to what was formerly delivered, the following experiment.

EXPERIMENT XXII.

WE recited in our nineteenth experiment, how by drawing most of the air out of the receiver, we made the water subside by degrees in a glass not four foot long: we shall now add, that in the like experiment made in such a tube, or a greater, it may be observed, that when the water begins to fall, there will appear store of bubbles fastened all along to the sides of the glass; of which bubbles, by the agitation of the vessel consequent upon pumping, there will arise good numbers to the top of the water, and thereby break; and as the cylinder of water is brought to be lower and lower, so the bubbles will appear more numerous in that part of the tube which the water yet fills; and the nearer the surface of the water, in its descent, approacheth to these bubbles, the greater they will grow, because, having the less weight and pressure upon them, the expansion of that air which makes them, can be the less resisted by the pressure of the incumbent water and air; as seems probable from hence, that upon the letting in a little external air, those bubbles immediately shrink.

IT may indeed, as we lately intimated, be conjectured, that these bubbles proceed not so much from any air pre-existent in the water, and lurking in the pores of it, as from the more subtle parts of the water itself; which, by the expansion allowed them upon the diminished pressure of the ambient bodies, may generate such bubbles. And indeed, I am not yet so well satisfied that bubbles may not (at least sometimes) have

have such an origination: but that which makes me suspect that those in our trials contain real air, formerly latitant in the pores of the water, is this, that upon the inletting of the external air, the water was not again impelled to the very top of the tube whence it began to fall, but was stopped in its ascent near an inch beneath the top. And since, if the upper part of the tube had been devoid of any other than such ethereal matter as was subtle enough freely to penetrate the pores of the glass, the external air would have been able to impel the water to the top of a tube seven or eight times as long as ours was; the phænomenon under consideration seemed manifestly to argue, that the many bubbles that broke at the top of the water, did contain a real air, which being collected into one place, and hindered by the top of the glass from receding, was able to withstand the pressure of the outward air. As we see that if never so little air remain in the tube upon the making the experiment *de vacuo* with quicksilver, no inclining of the tube (though a long one) will enable a man to impel the mercury up to the very top, by reason (as we formerly noted) of the resistance of the included air, which will not be compressed beyond a certain degree.

But in order to a farther discovery what our bubbles were, we will, on this occasion, inform your Lordship, that we tried the XIXth experiment in one of our small receivers, and found, that upon the drawing down of the water, so many bubbles disclosed themselves, and broke into the upper part of the tube, that having afterwards let in the external air, the water was not thereby impelled to the top of the tube (three foot in length) within a little more than half an inch. And whether or no it were air that possessed that space at the top of the tube, which was not filled with water, we took this course to examine. We drew the second time the air out of the receiver, and found, that by reason of the body that possessed the top of the tube, we were able not only to make the water in the tube fall to a level with the surface of the water in the vessel, but also (by plying the pump a little longer) a great way beneath it; which, since it could not well be ascribed to the bare subsiding of the water, by reason of its own weight, argued that the water was depressed by the air: which was confirmed by the figure of the surface of the water in the tube, which was much more concave than that of water in tubes of that bigness useth to be. And this farther trial (to add that upon the bye) we made at the same time; that when the water in the pipe was drawn down almost as low as the water without it, we observed, that (though we desisted from pumping) by the bare application of a hand moderately warm to the deserted part of the tube, the remaining water would be speedily and notably depressed. And having for a while held a kindled coal to the outside of the tube, (the pump being still unimployed, because the vessel chanced to hold extraordinarily well) the air was by the heat so far expanded, that it quickly drove the water to the bottom of the tube, which was divers inches beneath the surface of the ambient water. Whereby it appears (by the same way by which we formerly measured the dilatation of the air) that the air, even when it is expanded to between 90 and 100 times its extent, will yet readily admit of a much farther rarefaction by heat.

I considered also that in case the bubbles we have been speaking of, were produced by the parcels of air latitant in the water, that air being now got together to the top of the tube, though the air were again drawn out of the receiver, the taking off its pressure would not disclose bubbles as before; and accordingly, the air being again pumped out, the water in the tube descended as formerly; but for a great while we scarce saw one bubble appear, only when the receiver had been very much exhausted, and the water was fallen very low, there appeared near the bottom of the tube, certain

tain little bubbles, which ſeemed to conſiſt of ſuch parcels of air as had not, by reaſon of their ſmallneſs, got up to the top of the water, with the more bulky and vigorous ones. And that which is not inconſiderable, is, that having, by letting in the air, forced up the water into the tube, we could not perceive that it aſcended near the top, though we permitted the engine to remain unimployed for two or three nights together, and watched whether the water would ſwell up and fill the tube. And on this occaſion I remember, that having tried ſuch an experiment as this with quickſilver inſtead of water, in a tube of about a foot and a half long, wherein it might ſeem more hopeful to eſcape bubbles; yet upon the drawing down the quickſilver as low as we could, and letting in the external air upon it, we found that ſome lurking particles of air were got up to the top of the tube, and hindered the quickſilver from being forced up again ſo high. And though the quickſilver were by this means brought to appear a very cloſe and lovely metalline cylinder, not interrupted by interſperſed bubbles as before; yet having cauſed the air to be again drawn out of the receiver, I could perceive ſeveral little bubbles to diſcloſe themſelves, faſtened to the inſide of the tube, near the bottom of it; and having purpoſely watched one or two of the chiefeſt, I had the pleaſure to obſerve, that though they grew bigger and bigger as the ſurface of the mercurial cylinder fell nearer and nearer to them, ſo as that at length they ſwelled into a conſpicuous bulk; yet upon the wary letting in the air upon them, they did not break, but preſently ſhrunk up into a littleneſs that rendered them inconſpicuous.

WHENCE it ſeems very probable, if not certain, that even in the cloſeſt and moſt ponderous liquors, and therefore much more in water, there may lurk undiſcernable parcels of air, capable, upon the removal of the preſſure of the ambient air (though but in part) and that of the liquor wherein it lurks, to produce conſpicuous bubbles. And conſequently, if it ſeem inconvenient to admit an elaſtical power in the water, it may be ſaid that the ſwelling of the compreſſed water in the pewter veſſel lately mentioned, and the ſpringing up of the water at the hole made by the needle, were not the effects of any internal *Elater* of the water, but of the ſpring of the many little particles of air diſperſed through that water, and acting upon it in their ſudden recovering themſelves to a greater extent, than that to which a violent compreſſion had reduced them.

BUT though, from all theſe particulars, it ſeems manifeſt that the bubbles we have been all this while treating of, were produced by ſuch a ſubſtance as may be properly enough called air; yet till we ſhall have had the opportunity of making ſome farther trials concerning the nature of the air, we ſhall not reſolutely determine whether or no air be a primogenial body (if I may ſo ſpeak) that cannot now be generated or turned either into water or any other body. Yet in the mean while (becauſe it is an important queſtion, and, if rightly determined, may much conduce to the knowledge of the nature of the air) we think it not unfit to make a brief mention of ſome of the particulars which at preſent occur to our thoughts in favour of either part of the queſtion.

FIRST then, divers Naturaliſts eſteem the air (as well as other elements) to be ingenerable and incorruptible. And reaſons plauſible enough may be drawn to countenance this opinion, from the conſideration of that permanency that ought to belong to the corporeal principles of other bodies.

NEXT, experience may be pleaded to the ſame purpoſe, for I have read of ſome who in vain attempted to turn air into water, or water into air.

Schottus Mechan. hydraulico-pneumat. Part. 1. Class. 1.

THE diligent *Schottus* tells us, that amongst other rarities to be met with in that great repository of them, the *Musæum Kircherianum*, there is a round glass with a tapering neck near half full (as one may guess by the scheme he annexeth) of ordinary spring water, which having been hermetically shut up there by *Clavius* the famous geometrician, the included water is to this day preserved, not only clear and pure, as if it were but newly put in; but (as it seems) without (in the least) turning into air, notwithstanding its having been kept there these fifty years: for he tells us, that the water hath continued there all this while without any diminution.

Nor doth it appear in those glasses, which for chymical experiments we usually close with *Hermes* his seal (as they call it) that the included air doth, during its long imprisonment, notwithstanding the alteration it receiveth from various degrees of heat, discernably alter its nature. Whereas we plainly perceive in our digestions and distillations, that though water may be rarefied into invisible vapours, yet it is not really changed into air, but only divided by heat, and scattered into very minute parts, which meeting together in the alembic or in the receiver, do presently return into such water as they constituted before. And we also see, that even spirit of wine, and other subtle and fugitive spirits, though they easily fly into the air, and mingle with it, do yet in the glasses of chymists easily lay aside the disguise of air, and resume the divested form of liquors. And so volatile salts, as of urine, hartshorn, *&c.* though they will readily disperse themselves through the air, and play up and down in the capacity of an alembic or a receiver; yet will they, after a while, fasten themselves to the insides of such glasses in the form of salts.

BESIDES, since air is confessedly endowed with an elastical power that probably proceeds from its texture, it appears not what it is, that in such light alterations of water, as are by many presumed capable of turning it into air, can be reasonably supposed so to contrive the particles of water, as to give them, (and that permanently) the structure requisite to a spring. I add the word permanently, because the newly mentioned observations seem to argue the corpuscles of air to be irreducible into water, whereas the aqueous particles may perhaps for a while be so vehemently agitated, as to press almost like springs upon other bodies; yet upon the ceasing of the agitation, they quickly, by relapsing into water, disclose themselves to have been nothing else whilst they counterfeited the air.

LASTLY, the experiment formerly made in our engine with a piece of match, seems to evince, that even those light and subtile fumes (for the most part not aqueous neither) into which the fire itself shatters dry bodies, have no such spring in them as the air, since they were unable to hinder or repress the expansion of the air included in the bladder they surrounded.

Natural and Moral Hist. of the Indies, Lib. 3. c. 9.

I REMEMBER indeed, that the learned *Josephus Acosta*, in his history of the *West Indies*, tells us, that he saw in those parts some grates of iron so rusted and consumed by the air, that the metal being pressed between the fingers, dissolved (to use his words) to powder, as if it had been hay or parched straw. And I remember too, that the accurate *Varenius* tells us, that in the islands commonly called *Azores*, the air (and wind) is so sharp, that in a short time it frets not only iron plates, but the very tiles upon the roofs of houses, and reduceth them to dust. And I have elsewhere mentioned some recent observations of this kind. But it may be said, that the above-mentioned authors ascribe the recited effects chiefly to the winds, and that, however the corrosion of the iron and the tiles may proceed not from the air itself, or any of its genuine parts, but from some saline corpuscles dispersed through the air, and driven by the winds against the bodies it is presumed to fret. And that such volatile salts

Georg. General. Lib. 1. cap. 19.

may copiouſly aſcend into the air, and yet retain their nature, as doth the more fixt ſalt in the ſea-water, the ſublimations of ſal-armoniac may ſufficiently evince. Not to mention, that I have ſhewn ſome friends a ſecret kind of ſaline ſubſtance incomparably ſubtler than ſal-armoniac, which did not only eaſily enough aſcend itſelf, but carried up with it (and that in a very great proportion) the ſolid and ponderous body even of uncalcined gold in the form of ſubtile exhalations, which did afterwards faſten themſelves to the upper parts of the veſſels, and yet manifeſt themſelves to continue gold. We remember alſo, that to try whether water could be turned into air, we once took an æolipile, into which we had before conveyed ſome water, and placing it upon kindled coals when the heat forced out a vehement ſtream of aqueous vapours; we tied about the neck of it, that of a bladder, which we had before emptied of air; and finding the æolipile after a while to blow up the bladder, we carefully tied it again, that the included ſubſtance might not get away. Then ſlipping it off from the æolipile, we conveyed it into our receiver, to try whether or no that, which in part diſtended the bladder, would appear by its ſpring to be true air: whereby we found that upon the exſuction of the ambient air, the included ſubſtance expanded itſelf and the bladder to a very much greater bulk than it was of before. And for farther ſatisfaction, having again taken out the bladder, we ſuffered it to remain tied up till next morning, to try whether time, and the coldneſs of the night, would make the contained ſubſtance relapſe into water: but the next morning we found it little leſs tumid than before. I remember, I ſay, that I once made this experiment; but I might ſay in anſwer to it, that the chief reaſon of my mentioning it, is, to let your Lordſhip ſee, how requiſite it is to be circumſpect and conſiderate, when we are to make and to build upon nice experiments. For though I may ſeem to have uſed ſufficient caution, yet afterward conſidering with myſelf that the æolipile I had imployed was a very large one, and that it required much more care than one that hath not tried it would imagine, to drive out all the air from a large æolipile, I eaſily ſuſpected that the diſtenſion of the bladder in our pneumatical veſſel, might proceed not from the watery ſteams that came out at the narrow mouth of the æolipile, and had very much wetted the bladder, but from the rarefied air which in that ſort of veſſels is wont for a good while together to come out with the rarefied water: and accordingly having reiterated the experiment, I found it very difficult (by reaſon of the ſhrinking of the bladders upon their being heated, and of other experiments) to make it ſo accurately as to deduce from it, that water may be rarefied into true air.

AGAINST the other four above-mentioned conſiderations, we cannot ſpend time to frame objections, but muſt forthwith proceed to the mention of thoſe things that ſeem to argue that air (at leaſt ſuch as produced our bubbles) may be generated of water and other bodies.

FIRST then, we have found by experience that a vapid air, or water rarefied into vapour, may at leaſt for a while emulate the elaſtical power of that which is generally acknowledged to be true air. For if you take a good æolipile, with a moderately ſtrong and ſlender neck, and filling it with water, lay it upon quick coals, you may, after a while, obſerve ſo great a preſſure by ſome of the parts contained in the æolipile upon others, that the water will ſometimes be thrown up into the air above three or four foot high; and if you then take the æolipile almoſt red-hot from off the fire, you may perceive that the water will, for a longer time than one would eaſily imagine, continue to be ſpouted out in a violent ſtream. And if there remains but little water in the æolipile when it is taken very hot from the fire, immerſing the neck of it into cold water, you will find, that after it begins to ſuck in ſome water, there will be made from time to time ſtore of large bubbles in that water wherein the neck was

plunged. Which bubbles seem manifestly to proceed from hence, that for a while the heat in the æolipile continues strong enough to rarefy part of the water that is sucked in, and expel it in the form of vapours through the water incumbent on the pipe. If also when the æolipile is almost full of water, and therefore can contain but little air; you hold a coal or brand in that stream of vapours that issues out of the narrow mouth of it, you will find this vapid or rorid air (if I may so call it) to blow the fire very strongly, and with a roaring noise. And that it be not said that it is by the external air which the aqueous steams drive before them, and not by the steams themselves, that the blast is made and the flame excited; it hath been observed, that by approaching the coal or brand almost to the mouth of the æolipile, the wind appeared more vehement, than if the body to be kindled were held some inches off.

BUT in regard the elastical power of the stream, issuing out of an æolipile, seems manifestly due to the heat that expands and agitates the aqueous particles whereof that stream consists, and that such rapid winds seem to be but water scattered into little parts and set a-moving; since we find, that holding a knife, or any solid, smooth, and close body against the stream that issues out of the æolipile, the vapours condensing upon it, will presently cover it with water: it will be very pertinent to subjoin a notable experiment that I remember I have met with, in the description given us by the industrious *Kircher*, of several musical engines. And (though it may seem somewhat prolix) we will recite what he delivers in his own words, which are these.

Kirch. Art. Mag. Con. & Diffon. lib 9. p. 300. See the fifteenth figure.

Cum eodem tempore quo hæc scripsi, summa Pont. Innocentii X^mi *mandato organi hydraulici in horto Quirinali constituendi cura mihi commendata esset. Æoliam cameram insigni sanè successu construi jussimus, eâ quæ sequitur ratione.*

Erat longitudo sive altitudo cameræ A H *5 pedum latitudine 3 ferè ex lateribus constructa; in medio duo tenebat diaphragmata* C D *&* E F *in modum cribri pluribus foraminibus pertusi. Paulo infra canalis* G *aquam advehens inserebatur in* H *eidem epistomium parabat exitum. Aqua itaque per canalem* G *maximo impetu ruens vehementissimum ventum mox intus excitabat; qui ventus nimiâ humiditate imbutus, ut purior exiret siccióque, diaphragmata illa in cribri modum pertusa, ordinata sunt. Intra hæc enim aquæ vehemens agitatio rupta fractáque aërem puriorem per* A *canalem subtiliorémque emittebat: verum cum postea inventum sit aërem plus æquo humidum interioribus organi meatibus maximum detrimentum inferre: hinc, ut aër aquosus siccissimam consistentiam acquireret, ordinavimus canalem plumbeum* Q R *in helicem contortum vasi* S *aliquantulum capaciori in modum urnæ efformato, insertum. Intra urnam enim plumbeam & canalem tortuosum illisus aër humidus, ita ab omni aquositate defæcabatur, ut ex furno in organum derivatus dici potuerit. Urna* S *canalis tortuosi* Q R *ultimum orificium* Z *inseritur anemothecæ organi. Et hanc modum organis hydraulicis omnium aptissimum reperi.*

Debet autem camera illa situari in loco quantum fieri potest sicciori, ita ut longo canali aqua intra eam derivetur, ne locus humiditate suâ organis officiat.

THUS far the ingenious *Kircherus*, whom I the rather cite, because although I have been informed of divers vendiducts (as they call them) by very knowing travellers that have observed them; yet this relation of our author being very punctual, and delivered upon his own particular experience, hath, I confess, made me wish I had had the good fortune when I was at *Rome*, to take notice of these organs; or that I had now the opportunity of examining of such an experiment. For, if upon a strict enquiry I should find that the breath that blows the organs doth not really upon the ceasing of its unusual agitation by little and little relapse into water, I should strongly suspect that it is possible for water to be easily turned into air. I remember indeed, that we have formerly taught, that there lurks an intersperſed air in the pores of

ordinary water, which may possibly be struck out by the breaking of the water in its fall into the Æolian chamber, (as he calls it.) But in regard the scheme seems to represent that chamber as closely shut, and thereby forbids us to suppose that any air is carried into it, but what is latitant in the water, it will scarce seem probable (to him who remembers how small a proportion of air that appeared to be, when its rarefication ceased, which was concealed in the water we freed from bubbles in our receiver) that so little air as is commonly dispersed through water, should be able, in so little water as was requisite for so small a room, to make so vehement a wind as our author here tells us of. I have sometime therefore suspected, that in this case the wind may be produced by small particles of the water itself, forcibly expelled out of the chamber into the organs. And to the objection, to which I foresaw this guess to be liable, namely, that, no heat intervening, there appeared nothing that should raise the water into exhalations, and give them an impulse; I thought it might be said, that motion alone, if vehement enough, may, without sensible heat, suffice to break water into very minute parts, and make them ascend upwards, if they can no where else more easily continue their agitation. For I remember that travelling betwixt *Lyons* and *Geneva*, I saw, not very far out of the way, a place where the river of *Rhone*, coming suddenly to be straitened betwixt two rocks, so near each other, that a man may, (if my memory fail me not) stand astride upon both at once; that rapid stream dashing with great impetuosity against its rocky boundaries, doth break part of its water into such minute corpuscles, and put them into such a motion, that passengers observe at a good distance off, as it were a mist arising from that place, and ascending a good way up into the air. Such, I say, was my suspicion touching the wind we have been considering; but it seems something odd that aqueous vapours should, like a dry wind, pass through so long and tortuous a pipe of lead, as that described by our author, since we see in the heads of stills, and the necks of æolipiles, how quickly such vapours are even by a very little cold recondensed into water. But to this also something may be speciously replied; wherefore contenting myself to have mentioned our author's experiment as a plausible, though not demonstrative proof, that water may be transmuted into air, we will pass on to mention in the third place another experiment, which we tried in order to the same enquiry.

WE took a clear glass bubble (capable of containing by guess about three ounces of water) with a neck somewhat long and wide, of a cylindrical form; this we filled with oil of vitriol and fair water, of each almost a like quantity, and casting in half a dozen small iron nails, we stopt the mouth of the glass, (which was top full of liquor) with a flat piece of diapalma provided for the purpose, that, accommodating itself to the surface of the water, the air might be exquisitely excluded: and speedily inverting the phial, we put the neck of it into a small wide-mouthed glass, that stood ready, with more of the same liquor in it, to receive it. As soon as the neck had reached the bottom of the liquor it was dipped into, there appeared at the upper part (which was before the bottom) of the phial a bubble, of about the bigness of a pea, which seemed rather to consist of small and recent bubbles, produced by the action of the dissolving liquor upon the iron, than any parcel of the external air that might be suspected to have got in upon the inversion of the glass, especially since we gave time to those little particles of air which were carried down with the nails into the liquor to fly up again. But whence this first bubble was produced, is not so material to our experiment, in regard it was so small: for soon after we perceived the bubbles produced by the action of the menstruum upon the metal, ascending copiously to the bubble

bubble named, and breaking into it, did ſoon exceedingly increaſe it, and by degrees depreſs the water lower and lower, till at length the ſubſtance contained in theſe bubbles poſſeſſed the whole cavity of the glaſs phial, and almoſt of its neck too, reaching much lower in the neck than the ſurface of the ambient liquor, wherewith the open-mouthed glaſs was by this means almoſt repleniſhed. And becauſe it might be ſuſpected that the depreſſion of the liquor might proceed from the agitation whereinto the exhaling and impriſoned ſteams were put, by that heat which is wont to reſult from that action of corroſive ſalts upon metals, we ſuffered both the phial and the open-mouthed glaſs to remain as they were, in a window, for three or four days and nights together; but looking upon them ſeveral times during that while, as well as at the expiration of it, the whole cavity of the glaſs bubble, and moſt of its neck, ſeemed to be poſſeſſed by air, ſince by its ſpring it was able for ſo long to hinder the expelled and ambient liquor from regaining its former place. And it was remarkable, that juſt before we took the glaſs bubble out of the other glaſs, upon the application of a warm hand to the convex part of the bubble; the impriſoned ſubſtance readily dilated itſelf like air, and broke through the liquor in divers bubbles ſucceeding one another.

HAVING alſo another time tried the like experiment with a ſmall phial, and with nails diſſolved in aqua fortis, we found nothing incongruous to what we have now delivered. And this circumſtance we obſerved, that the newly generated ſteams did not only poſſeſs almoſt all the whole cavity of the glaſs, but divers times without the aſſiſtance of the heat of my hand, broke away in large bubbles through the ambient liquor into the open air: ſo that theſe experiments with corroſive liquors, ſeemed manifeſtly enough to prove, though not that air may be generated out of the water, yet that in general air may be generated anew.

LASTLY, to the foregoing arguments from experience we might eaſily ſubjoin the authority of *Ariſtotle*, and of (his followers) the ſchools, who are known to have taught, that air and water being ſymbolizing elements, (in the quality of moiſture) are eaſily tranſmutable into one another. But we ſhall rather to the foregoing argument add this, drawn from reaſon, that if, as *Leucippus*, *Democritus*, *Epicurus* and others, followed by divers modern Naturaliſts, have taught, that the difference of bodies proceeds but from the various magnitudes, figures, motions, and textures of the ſmall parts they conſiſt of, (all the qualities that make them differ, being deducible from thence) there appears no reaſon why the minute parts of water, and other bodies, may not be ſo agitated or connected as to deſerve the name of air. For if we allow the Carteſian hypotheſis, according to which as we noted at the beginning of this letter, the air may conſiſt of any terrene or aqueous corpuſcles, provided they be kept ſwimming in the interfluent celeſtial matter; it is obvious that air may be as often generated as terreſtrial particles, minute enough to be carried up and down, by the celeſtial matter, aſcend into the atmoſphere. And if we will have the air to be a congeries of little ſlender ſprings, it ſeems not impoſſible, though it be difficult, that the ſmall parts of divers bodies may by a lucky concourſe of cauſes be ſo connected, as to conſtitute ſuch little ſprings, ſince (as we note in another treatiſe) water in the plants it nouriſheth is uſually contrived into ſpringy bodies, and even the bare altered poſition and connection of the parts of a body may ſuffice to give it a ſpring that it had not before, as may be ſeen in a thin and flexible plate of ſilver; unto which, by ſome ſtrokes of a hammer, you may give a ſpring, and by only heating it red-hot, you may make it again flexible as before.

THESE,

THESE, my Lord, are ſome of the conſiderations at preſent occurring to my thoughts, by which it may be made probable, that air may be generated anew. And though it be not impoſſible to propoſe objections againſt theſe, as well as againſt what hath been repreſented in favour of the contrary doctrine; yet having already almoſt tired myſelf, and I fear more than almoſt tired your Lordſhip with ſo troubleſome an inquiry after the nature of bubbles, I ſhall willingly leave your Lordſhip to judge of the arguments alledged on either ſide; and I ſhould ſcarce have ventured to entertain you ſo long concerning ſuch empty things as the bubbles, which have occaſioned all this diſcourſe, but that I am willing to invite you to take notice with me of the obſcurity of things, or the dimneſs of our created intellects (which yet of late too many ſo far preſume upon, as either to deny or cenſure the Almighty and Omniſcient Creator himſelf) and to learn hence this leſſon, that there are very many things in nature that we diſdainfully overlook as obvious or deſpicable, each of which would exerciſe our underſtandings, if not poſe them too, if we would but attentively enough conſider it, and not ſuperficially contemplate, but attempt ſatisfactorily to explicate the nature of it.

EXPERIMENT XXIII.

SINCE the writing of the twenty-firſt and twenty-ſecond experiments (and notwithſtanding all that hath been on their occaſion delivered concerning bubbles) we made ſome farther trials in proſecution of the ſame inquiry whereto they were deſigned.

WE choſe then, amongſt thoſe glaſſes which chymiſts are wont to call philoſophical eggs, one that containing about nine ounces of water, had a neck of half an inch in diameter at the top, and as we gueſſed, almoſt an inch at the bottom; which breadth we pitched upon for a reaſon that will by and by appear: then filling it up with common water to the height of about a foot and an half, ſo that the upper part remained empty, we ſhut it into the receiver, and watched what would follow upon pumping, which proved that a great part of the air being drawn out, the bubbles began to diſcover themſelves at the bottom and ſides of the glaſs; and increaſing, as the air was more and more drawn away, they did from time to time aſcend copiouſly enough to the top of the water, and there quickly break; but by reaſon that the wideneſs of the glaſs allowed them free paſſage through the water, they did not appear as in the former experiments to make it ſwell: the water ſcarce ever riſing at all above the mark affixed to its upper ſurface, when it was put in; and upon the return permitted to the outward air, and conſequently the ſhrinking in of the remaining bubbles, the water ſeemed to have loſt of its firſt extent, by the avolation of the formerly interſperſed air.

BEING willing likewiſe to try whether diſtilled water were, by having been divided into minute parts, and then reunited, more or leſs diſpoſed to expand itſelf than water not diſtilled; we took out of our laboratory ſome carefully diſtilled rain-water, and put about two ounces of it into a round glaſs-bubble, with a very ſmall neck (not exceeding the ſixth part of an inch in diameter) which we filled half way to the top, and then conveyed it into the receiver; the iſſue was, that though we drew out more air than ordinary, yet there appeared not the leaſt intumeſcence of the water, nor any aſcending bubbles.

BUT ſuſpecting that either the ſmall quantity of the water or the figure of the veſſel might have an intereſt in this odd phænomenon, we took the lately mentioned philoſo-

phical

phical egg, and another not much differing from it; the former we filled up with diſtilled rain-water to the old mark, and into the latter we put a long cylinder or rod of ſolid glaſs to ſtraiten the cavity of the neck by almoſt filling it up; and then pouring ſome diſtilled water into that alſo, till it reached within ſome fingers breadth of the top, the eggs were let down into the receiver. In this experiment the air was ſo far drawn forth, before there appeared any bubble in either of the glaſſes, that the diſparity betwixt this and common water was manifeſt enough. But at length, when the air was almoſt quite pumped out, the bubbles began to diſcloſe themſelves, and to increaſe as the preſſure of the air in the receiver decreaſed. But whereas in the firſt mentioned philoſophical egg the bubbles were very ſmall, and never able to ſwell the water, that we took notice of, at all above the mark; in the other, whoſe neck, as we lately ſaid, was ſtraitned, and their paſſage obſtructed, great numbers of them, and bigger, faſtened themſelves to the lower end of the glaſs-rammer (if we may ſo call it) and gathered in ſuch numbers between that and the ſides of the neck, that the water ſwelled about a finger's breadth above the mark, though upon the admitting of the external air it relapſed to the former mark, or rather fell ſomewhat below it. And although thereupon in the firſt named veſſel all the bubbles preſently diſappeared, yet in the other we obſerved, that divers remained faſtened to the lower part of the glaſs-rammer, and continued there, ſomewhat to our wonder, for above an hour after, but contracted in their dimenſions.

MOREOVER, having ſuffered the glaſſes to remain above twenty-four hours in the receiver, we afterwards repeated the experiment to try what change the exſuction of the external air would produce in the water, after the internal and latitant air had (as is above recited) in great meaſure got away in bubbles; and whether or no the water would, by ſtanding, readmit any new particles of air in the room of thoſe that had forſaken it. But though we exhauſted the receiver very diligently, yet we ſcarce ſaw a bubble in either of the glaſſes; notwithſtanding which, we perceived the water to riſe about the breadth of a barley-corn, or more, in the neck of that glaſs wherein the ſolid cylinder had been put; the liquor in the other glaſs not ſenſibly ſwelling.

AND laſtly, upon the letting in of the air, the water in the ſtraitened neck ſoon ſubſided to the mark above which it had ſwollen; which whether it ought to be aſcribed to the ſame ſmall expanſion of the parts of the water itſelf, or to the rarefaction of ſome yet latitant air broken into ſuch ſmall particles, as to eſcape our obſervation, ſeems not eaſily determinable, without ſuch farther trials, as would perhaps prove tedious to be recited as well as to be made; though I was content to ſet down thoſe already mentioned, that it might appear, how requiſite it is in nice experiments to conſider variety of circumſtances.

EXPERIMENT XXIV.

AFTER having thus diſcovered what operation the exſuction of the ambient air had upon water, we thought good to try alſo what changes would happen in other liquors upon the like taking off the preſſure of the external air. We took then a glaſs egg, ſomewhat bigger than a turkey's egg, which had a long neck or ſtem of about a third part of an inch in diameter; and filling it up with ſallad-oil, until it reached above half way to the top of the neck, we incloſed it in the receiver, together with common water in a reſembling veſſel, that we might the better compare together the operation of the exſuction of the air upon thoſe two liquors. The pump

pump being ſet on work, there began to appear bubbles in the oil much ſooner than in the water, and afterwards they alſo aſcended much more copiouſly in the former liquor than in the latter: nay, and when by having quite tired the pumper, and almoſt our own patience, we gave over, the bubbles roſe almoſt (if not altogether) in as great numbers as ever, inſomnch that none of the various liquors we tried either before or ſince, ſeemed to abound more with aërial particles than did this oil. In which it was farther remarkable, that between the time it was ſet into the receiver, and that at which we could get ready to pump, it ſubſided notably (by gueſs about half an inch) below the mark it reached before it was put in.

AFTER this expreſſed oil, we made trial of a diſtilled one; and for that purpoſe made choice of the common oil or ſpirit (for in the ſhops where it is ſold, the ſame liquor is promiſcuouſly called by either name) of turpentine, becauſe it was only of that chymical oil we had a ſufficient quantity: which being put into a ſmall glaſs-bubble with a ſlender neck, ſo as to fill it about two inches from the top, did, upon the evacuating of the receiver, preſent us with great ſtore of bubbles, moſt of which riſing from the bottom, expanded themſelves exceedingly in their aſcent, and made the liquor in the neck to ſwell ſo much by degrees, that at length it divers times ran over at the top: by which means we were hindered from being able to diſcern upon the letting in of the air, how much the ſubſidence of the oil below the firſt mark was due to the receſs of the bubbles.

HAVING likewiſe a mind to try, whether as ſtrong a ſolution of ſalt of tartar in fair water as could be made (we having then no oil of tartar *per deliquium* at hand) though it be accounted, quickſilver excepted, the heavieſt of liquors, would afford us any bubbles; we put in a glaſs-egg full of it at the ſame time with other liquors, and found, that they did long yield ſtore of bubbles before any diſcovered themſelves in the liquor of tartar; and having purſued the experiment, it appeared, that of all the liquors we made trial of, this afforded the feweſt and ſmalleſt bubbles.

SPIRIT of vinegar being tried after the ſame manner, exhibited a moderate number of bubbles, but ſcarce any thing elſe worth the mentioning.

NOR could we in red wine, tried in a glaſs-egg, take notice of any thing very obſervable. For though upon the exſuction of the air the bubbles aſcended in this liquor, as it were in ſhoals, and ſhifted places among themſelves in their aſcent; yet the intumeſcence of the whole bulk of the liquor was ſcarce at all ſenſible, the bubbles moſt commonly breaking very ſoon after their arrival at the top, where during their ſtay they compoſed a kind of ſhallow froth, which alone appeared higher in the neck of the glaſs than was the wine when it was let down. Neither yet did milk, conveyed into our pneumatical veſſel, preſent us with any thing memorable, ſave that (as it ſeemed by reaſon of ſome unctuouſneſs of the liquor) the bubbles not eaſily breaking at the top, and thruſting up one another, made the intumeſcence appear much greater than that of common water.

WE likewiſe conveyed hens eggs into the receiver, but after the exſuction of the air, took them out whole again. That which invited us to put them in, was, that (as perhaps we mention in other papers) we had among other experiments of cold, made eggs burſt, by freezing them within doors with ſnow and ſalt; the ice, into which the aqueous parts of the egg were turned by the cold, ſo diſtending (probably by reaſon of the numerous bubbles wont to be obſervable in ice) the outward parts of the egg, that it uſually cracked the ſhell, though the inner membrane, that involved the ſeveral liquors of the egg, becauſe it would ſtretch and yield, remained unbroken. And hereupon we imagined, that in our engine it might appear, whether

or no there were any considerable spring, either in any of the liquors, or in any other more spirituous substance included in the egg.

WE took also some spirit of urine, carelessly enough deflegmed, and put it into the same glass (first carefully scoured and cleansed) wherein we had put the oil olive above mentioned: we took also another glass, differing from a glass egg, only in that its bottom was flat, and filled it up to about $\frac{1}{7}$ of the neck (which was wider than that of the egg) with rectified spirit of wine.

WE took also another glass-egg; and having filled it with common water till it reached to the middle of the neck, we poured to it of the same spirit of wine, till it reached about an inch higher.

THESE three glasses having marks set on them, over against the edges of the contained liquors, were put into the r ceiver; and that beginning to be evacuated, the bubbles in all the three liquors began to appear. The mixture of the spirit of wine and water disclosed a great store of bubbles, especially towards the top; but scarce afforded us any thing worth the remembring. The spirit of urine appeared to swell near an inch and an half above the mark; and besides that, sent forth store of bubbles, which made a kind of froth at the upper part of it. And above that spume, there appeared eight or ten great bubbles one above another, in a very decent order, each of them constituting, as it were, a cylinder of about half an inch high, and as broad as the internal cavity of the neck: so that all the upper part part of the neck (for these bubbles reached to the top) seemed to be divided into almost equal parts by certain diaphragms, consisting of the coats of the bubbles, whose edges appeared like so many rings suspended one above another.

In the spirit of wine there did arise a great multitude of bubbles, even till weariness did make us give over the experiment. And in these bubbles two or three things were remarkable; as first, that they ascended with a very notable celerity; next, that being arrived at the top, they made no stay there: and yet, notwithstanding the great thinness and spiritousness of the liquor, did, before they broke, lift up the upper surface of it, and for a moment or two form thereof a thin film or skin, which appeared protuberant above the rest of the superficies like a small hemisphere. Thirdly, that they ascended streight up; whereas those produced at the lower part of the vessel, containing the mixture of the water and spirit of wine, ascended with a wavering or wriggling motion, whereby they described an indented line. Lastly, it was observable in the spirit of wine (and we took notice of the like in the oil of turpentine lately mentioned) that not only the bubbles seemed to rise from certain determinate places at the bottom of the glass, but that in their ascension they kept an almost equal distance from each other, and followed one another in a certain order, whereby they seemed part of small bracelets, consisting of equally little incontiguous beads, the lower end of each bracelet being, as it were, fastened to a certain point at the bottom of the glass.

THE air being sparingly let into the receiver, the great bubbles formerly mentioned as incumbent upon one another, in that glass that contained the spirit of urine, were by orderly degrees lessened till at length they wholly subsided. Notwithstanding the recess of so many bubbles, as broke on the top of the spirit of urine, during all the time of the experiment; yet it scarcely appeared at all to be sunk below the mark: nor did the mixture of spirit of wine and water considerably subside. But that is nothing to what we observed in the spirit of wine; for not only it conspicuously expanded itself in the neck of the vessel that contained it, notwithstanding the largeness of it, and that the bubbles were wont to break at the top of it almost as soon

as

as they arrived there; but upon the re-admiſſion of the external air, the ſpirit of wine retained its newly acquired expanſion. And though we let it alone for near an hour together, in expectation that it might ſubſide; yet when we took it out, we found it ſtill ſwelled between a quarter and half an inch above the mark; and although it was not eaſily imaginable, how this phænomenon could proceed from any miſtake in trying the experiment, yet the ſtrangeneſs of it invited me to repeat it with freſh ſpirit of wine. Which, ſwelling in the neck as formerly, I left all night in the receiver, allowing free acceſs to the external air at the ſtop-cock; and the next day found it ſtill expanded as before, ſave that it ſeemed a little lower: which decrement perhaps proceeded from the avolation of ſome of the fugitive parts of ſo volatile a liquor. And for better ſatisfaction having taken out the glaſs, and conſidered it in the open air, and at a window, I could not find, that there were any remaining bubbles, that could occaſion the perſevering and admired expanſion.

EXPERIMENT XXV.

BEING deſirous to diſcover what difference there might be, as to gravity and levity, between air expanded under water, and itſelf before ſuch expanſion; we took two very ſmall phials, ſuch as chymical eſſences (as they call them) are wont to be kept in, and of the ſize and ſhape expreſſed by the eighth figure. Into one of theſe we put ſo much of a certain ponderous mercurial mixture (happening to be then at hand) that the mouth being ſtopt with a little ſoft wax, the glaſs would juſt ſink in water and no more. This we let fall to the bottom of a wide-mouthed cryſtal jar, filled with about half a pint of common water, and into the ſame veſſel we ſunk the other eſſence-glaſs unſtopped, with as much water in it as was more than ſufficient to make it ſubſide. Both theſe ſunk with their mouths downwards, the former being about three quarters full of air, the latter containing in it a bubble of air, that was gueſſed to be of the bigneſs of half a pea: this done, the wide-mouthed glaſs was let down into the receiver, and the way of employing the engine was carefully made uſe of.

THE ſucceſs was, that having drawn out a pretty quantity of air, the bubbles began to diſcloſe themſelves in the water, as in the former experiments; and though for a good while after the bubbles aſcended in ſwarms from the lower parts of the water, and haſtily broke at the top; yet we proſecuted the experiment ſo long without ſeeing any effect wrought upon the eſſence-bottles, that we began to deſpair of ſeeing of them riſe. But continuing to ply the pump, that little glaſs, whoſe mouth was opened, came to the top of the water, being, as it were, buoyed up thither by a great number of bubbles, that had faſtened themſelves to the ſides of it; ſwimming thus with the mouth downward, we could eaſily perceive, that the internal air above mentioned had much dilated itſelf, and thereby ſeemed to have contributed to the emerging of the glaſs, which remained floating, notwithſtanding the breaking and vaniſhing of moſt of the contiguous bubbles. Being hereby encouraged to perſiſt in pumping, we obſerved, with ſome pleaſure, that at each time we turned the key, the air in the little glaſs did manifeſtly expand itſelf and thruſt out the water, generally retaining a very protuberant ſurface, where it was contiguous to the remaining water. And when after divers exſuctions of the air in the receiver, that in the phial ſo dilated itſelf as to expel almoſt all the water, it turned up its mouth towards the ſurface of the water in the jar, and there delivered a large bubble, and then relapſed

into its former floating posture. And this experiment taught us, among other things, that it was a work of more time and labour than we imagined, to exhaust our engine as much as it may be exhausted: for although before the emerging of the small phial, we did (as hath been touched already) think we had very considerably emptied the receiver, because there seemed to come out but very little or almost no sensible air at each exsuction into, and out of the cylinder; yet afterwards, at each drawing down the sucker, the air included in the phial did manifestly dilate itself so long, that it did no less than nine times turn its mouth upwards, and discharge a bubble by conjecture about the bignes of a pea, after the manner newly recited. But as for that phial, which had the weight in it, it rose not at all. So that being not able by quick pumping to gain another bubble from the air in the swimming glass, which proceeded from some small leak in the vessel, though it held in this experiment more stanch than was usual, we thought fit to let in leisurely the air from without, upon whose admission that within the phial shrinking into a very narrow compass, the glass did, as we expected, fall down to the bottom of the jar.

But being desirous, before we proceed to any new experiment, to try once more whether the little glass, that had the weight in it, might not also be raised; after we had suffered the engine to remain closed as it was, for five or six hours, the pump was again plied with so much obstinacy, that not only about the upper part of the jar there appeared a good number of bubbles (but very much smaller than those we saw the first time) but afterwards, there came from the bottom of the jar bubbles about the bignes of small peas; which, the pump being still kept going, followed one another to the number of forty, coming from the stopt phial; whose mouth, it seems, had not been shut so strongly and closely, but that the included air, dilating itself by its own spring, made itself some little passage betwixt the wax and the glass, and got away in these bubbles: after which, the unstopt glass began to float again, the air shut up in it being manifestly so dilated as to expel a good part of the water, but not so much as to break quite thorough. And at length, when our expectation of it was almost tired out, the heavier of the two phials began to come aloft, and immediately to subside again; which appeared to be occasioned by the air within it, whose bulk and spring being weakened by the recess of the forty bubbles before mentioned, it was no longer able, as formerly, to break forcibly through the incumbent water; but forming a bubble at the mouth of the glass, buoyed it up towards the top, and there getting away, left it to sink again, till the pressure of the air in the receiver being farther taken off, the air in the phial was permitted to expand itself farther, and to create another bubble, by which it was again for a while carried up. And it was remarkable, that though after having emptied the receiver as far as well we could, we ceased from pumping, yet the vessel continuing more stanch than it was wont, this assent and fall of the phial was repeated to the ninth time; the included air, by reason of the smallness of the vent at which it must pass out, being not able to get away otherwise than by little and little; and consequently, in divers such parcels as were able to constitute bubbles, each of them big enough to raise the phial, and keep it aloft until the avolation of that bubble. Whereby it may appear, that the grand rule in Hydrostaticks, that a body will swim in the water, in case it be lighter than as much of the water as equals it in bulk, will hold likewise, when the pressure of the atmosphere is in very great measure, if not when it is totally taken off from the liquor and the body: though it were worth inquiring, what it is, that so plentifully concurs to fill the bubbles made in our experiment by the so much expanded air. For to say with the old Peripatetic schools, that the air in rarefaction

tion may acquire a new extent, without the admiſſion of any new ſubſtance, would be an account of the phænomenon very much out of date, and which, I ſuppoſe, our modern naturaliſts would neither give nor acquieſce in.

I KNOW not whether it may be requiſite to add, that in this experiment, as in the former, the outward air, being let in, did ſoon precipitate the floating phial. But I think it will not be amiſs to note, that (congruouſly to what hath been above recorded of the vaſt expanſion of the air) the water, which in the heavier phial ſucceeded in the room of thoſe forty odd, if not fifty great bubbles of air, which at ſeveral times got out of it, amounted but to a very inconſiderable bigneſs.

EXPERIMENT XXVI.

IT having been obſerved by thoſe that have conſidered what belongs to pendulums (a ſpeculation that may, in my poor judgment, be highly uſeful to the Naturaliſts) that their vibrations are more ſlowly made, and that their motion laſts leſs in a thicker, than in a thinner medium; we thought it not amiſs to try, if a pendulum would ſwing faſter, or continue ſwinging longer in our receiver, in caſe of exſuction of the air, than otherwiſe. Wherefore we took a couple of round and poliſhed pendulums of iron or ſteel, of equal bigneſs, as near as we could get the artificer to make them; and weighing each of them twenty drachms, wanting as many grains: one of theſe we ſuſpended in the cavity of the receiver by a very ſlender ſilken ſtring, of about ſeven inches and a half in length, from the cover of the receiver to which it is faſtened. Then (by inclining the engine) we made the pendulum ſwing to and fro in it, and deſcribe as long arches, as in the capacity of ſo brittle a veſſel we thought ſafe and convenient. And one of the aſſiſtants telling the recurſions of the other pendulum hanging in the free air, by a ſtring of about the ſame length, we ſhortened and lengthened this other pendulum till it appeared to keep the ſame pace in its vibrations with that ſhut up in the receiver. Then having carefully drawn away the air, we did again ſet the pendulum in the receiver a vibrating; and giving the other ſuch a motion as made it deſcribe an arch, according to one's gueſs, equal to that of the included pendulum, we reckoned, one of us, the recurſions of that pendulum which was ſwinging within the receiver; and another of us that which was moving in (that which one would think a much more reſiſting medium) the air. But once, one of us reckoned near two and twenty recurſions of the included pendulum, whilſt the other reckoned but twenty of the pendulum that vibrated without. And another time alſo, the former of theſe pendula was reckoned to have made one and twenty recurſions, wherein the other made but twenty: yet this experiment ſeemed to teach us little, ſave that the difference betwixt the motion of ſuch a pendulum in the common air, and in one exceedingly rarefied, is ſcarce ſenſible in veſſels no bigger than our receiver; eſpecially, ſince though during this experiment it held very well, yet we could not ſuppoſe it to be altogether devoid of air. We obſerved alſo, that when the receiver was full of air, the included pendulum continued its recurſions about fifteen minutes (or a quarter of an hour) before it left off ſwinging; and that after the exſuction of the air, the vibration of the ſame pendulum (being freſh put into motion) appeared not (by a minute watch) to laſt ſenſibly longer. So that the event of this experiment being other than we expected, ſcarce afforded us any other ſatisfaction, than that of our not having omitted to try it. And whether, in caſe the trial be made with a pendulum much

less disproportionate to the air than steel is, the event will much better answer expectation, experience may be consulted.

EXPERIMENT XXVII.

THAT the air is the medium, whereby sounds are conveyed to the ear, hath been for many ages, and is yet the common doctrine of the schools. But this received opinion hath been of late opposed by some philosophers upon the account of an experiment made by the industrious *Kircher*, and other learned men; who have (as they assure us) observed, that if a bell, with a steel clapper, be so fastened to the to the inside of a tube, that upon the making the experiment *de vacuo* with that tube, the bell remained suspended in the deserted space at the upper end of the tube: and if also a vigorous load-stone be applied on the outside of the tube to the bell, it will attract the clapper, which, upon the removal of the load-stone falling back, will strike against the opposite side of the bell, and thereby produce a very audible sound; whence divers have concluded, that it is not the air, but some more subtle body, that is the medium of sounds. But because we conceived, that, to invalidate such a consequence from this ingenious experiment, (though the most luciferous that could well be made without some such engine as ours) some things might be speciously enough alledged; we thought fit to make a trial or two, in order to the discovery of what the air doth in conveying of sounds, reserving divers other experiments triable in our engine concerning sounds, till we can obtain more leisure to prosecute them. Conceiving it then the best way to make our trial with such a noise, as might not be loud enough to make it difficult to discern slighter variations in it, but rather might be, both lasting (that we might take notice by what degrees it decreased) and so small, that it could not grow much weaker without becoming imperceptible; we took a watch, whose case we opened, that the contained air might have free egress into that of the receiver. And this watch was suspended in the cavity of the vessel only by a pack-thread, as the unlikeliest thing to convey a sound to the top of the receiver; and then closing up the vessel with melted plaister, we listened near the sides of it, and plainly enough heard the noise made by the balance. Those also of us, that watched for that circumstance, observed, that the noise seemed to come directly in a streight line from the watch unto the ear. And it was observable to this purpose, that we found a manifest disparity of noise, by holding our ears near the sides of the receiver, and near the cover of it: which difference seemed to proceed from that of the texture of the glass, from the structure of the cover (and the cement) through which the sound was propagated from the watch to the ear. But let us prosecute our experiment. The pump after this being employed, it seemed, that from time to time the sound grew fainter and fainter; so that when the receiver was emptied as much as it used to be for the foregoing experiments, neither we, nor some strangers, that chanced to be then in the room, could, by applying our ears to the very sides, hear any noise from within; though we could easily perceive, that by the moving of the hand, which marked the second minutes, and by that of the balance, that the watch neither stood still, nor remarkably varied from its wonted motion. And to satisfy ourselves farther, that it was indeed the absence of the air about the watch, that hindered us from hearing it, we let in the external air at the stop-cock; and then though we turned the key and stopt the valve, yet we could plainly hear the noise made by the balance, though we held our ears sometimes at two foot di-

ſtance from the outſide of the receiver; and this experiment being reiterated into another place, ſucceeded after the like manner. Which ſeems to prove, that whether or no the air be the only, it is at leaſt the principal medium of ſounds. And by the way it is very well worth noting, that in a veſſel ſo well cloſed as our receiver, ſo weak a pulſe as that the balance of a watch, ſhould propagate a motion to the air in a phyſically ſtreight line, notwithſtanding the interpoſition of ſo cloſe a body as glaſs, eſpecially glaſs of ſuch thickneſs as that of our receiver; ſince by this it ſeems the air impriſoned in the glaſs muſt, by the motion of the balance, be made to beat againſt the concave part of the receiver, ſtrongly enough to make its convex part beat upon the contiguous air, and ſo propagate the motion to the liſtner's ears. I know this cannot but ſeem ſtrange to thoſe, who, with an eminent modern philoſopher, will not allow, that a ſound, made in the cavity of a room, or other place ſo cloſed, that there is no intercourſe betwixt the external and internal air, can be heard by thoſe without, unleſs the ſounding body do immediately ſtrike againſt ſome part of the incloſing body. But not having now time to handle controverſies, we ſhall only annex, that after the foregoing experiment, we took a bell of about two inches in diameter at the bottom, which was ſupported in the midſt of the cavity of the receiver by a bent ſtick, which by reaſon of its ſpring preſſed with its two ends againſt the oppoſite parts of the inſide of the veſſel: in which, when it was cloſed up, we obſerved, that the bell ſeemed to ſound more dead than it did when juſt before it ſounded in the open air. And yet, when afterwards we had (as formerly) emptied the receiver, we could not diſcern any conſiderable change (for ſome ſaid they obſerved a ſmall one) in the loudneſs of the ſound. Whereby it ſeemed, that though the air be the principal medium of ſound, yet either a more ſubtle matter may be alſo a medium of it, or elſe an ambient body, that contains but very few particles of air, in compariſon of thoſe it is eaſily capable of, is ſufficient for that purpoſe. And this, among other things, invited us to conſider, whether in the above-mentioned experiment made with the bell and the load-ſtone, there might not in the deſerted part of the tube remain air enough to produce a ſound; ſince the tubes for the experiment *de vacuo* (not to mention the uſual thinneſs of the glaſs) being ſeldom made greater than is requiſite, a little air might bear a not inconſiderable proportion to the deſerted ſpace: and that alſo, in the experiment *de vacuo*, as it is wont to be made, there is generally ſome little air, that gets in from without, or at leaſt ſtore of bubbles, that ariſe from the body of the quickſilver, or other liquor itſelf, obſervations heedfully made have frequently informed us; and it may alſo appear, by what hath been formerly delivered concerning the Torricellian experiment.

ON the occaſion of this experiment concerning ſounds, we may add in this place, that when we tried the experiment formerly mentioned, of firing gun powder with a piſtol in our evacuated receiver, the noiſe made by the ſtriking of the flint againſt the ſteel was exceeding languid, in compariſon of what it would have been in the open air. And on divers other occaſions it appeared, that the ſounds created within our exhauſted glaſs, if they were not loſt before they reached the ear, ſeemed at leaſt to arrive there very much weakened. We intended to try, whether or no the wire-ſtring of an inſtrument ſhut up into our receiver would, when the ambient air was ſucked out, at all tremble, if in another inſtrument held cloſe to it, but without the receiver, a ſtring tuned (as muſicians ſpeak, how properly I now examine not) to an uniſon with it, were briſkly touched, and ſet a vibrating. This, I ſay, we purpoſed to try, to ſee how the motion made in the air without would be propagated through the cavity of our evacuated receiver. But when the inſtrument, wherewith the trial

was

was to be made, came to be employed, it proved too big to go into the pneumatical vessel: and we have not now the conveniency to have a fitter made.

We thought likewise to convey into the receiver a long and slender pair of bellows, made after the fashion of those usually employed to blow organs, and furnished with a small musical instead of an ordinary pipe. For we hoped, that by means of a string fastened to the upper part of the bellows, and to the moveable stopple, that makes a part of the cover of our receiver, we should, by frequently turning round that stopple, and the annexed string, after the manner already often recited, be able to lift up and distend the bellows; and by the help of a competent weight fastened to the same upper part of the bellows, we should likewise be able at pleasure to compress them, and, by consequence, try whether, that subtler matter than air (which, according to those that deny a vacuum, must be supposed to fill the exhausted receiver) would be able to produce a sound in the musical pipe; or in a pipe like that of ordinary bellows, to beget a wind capable to turn or set on moving some very light matter, either shaped like the sails of a wind-mill, or of some other convenient form, and exposed to its orifice. This experiment, I say, we thought to make; but have not yet actually made it for want of an artificer to make us such a pair of bellows as it requires.

We had thoughts also of trying, whether or no, as sounds made by the bodies in our receiver become much more languid than ordinary, by reason of the want of air; so they would grow stronger, in case there were an unusual quantity of air crouded and shut up in the same vessel. Which may be done (though not without some difficulty) by the help of the pump, provided the cover and stopple be so firmly fastened (by binding and cement, or otherwise) to the glass and to each other, that there be no danger of the condensed air's blowing of either of them away, or its breaking through the junctures. These thoughts, my Lord, as I was saying, we entertained; but for want of leisure, as, of as good receivers as ours, to substitute in its place, in case we should break it before we learned the skill of condensing the air in it, we durst not put them in practice. Yet on this occasion give me leave to advertise your Lordship once for all, that though for the reasons newly intimated, we have only in the seventeenth experiment, taken notice, that by the help of our engine the air may be condensed as well as rarefied; yet there are divers other of our experiments, whose phænomena it were worth while to try to vary, by means of the compression of the air.

EXPERIMENT XXVIII.

We taught, among divers other things, when we discoursed of our first experiment, that the air shut up in our receiver presseth as strongly upon the bodies shut up with it, as if they were exposed to the pressure of the whole atmosphere. That this was not inconsiderately propounded, we hope your Lordship hath gathered from divers of the things already recited: but yet perhaps it will not be amiss to subjoin, by way of farther confirmation of the same truth, the following experiment; which would have accompanied the 20th, but the paper, wherein the one was written, chanced not to be at hand, when the other was sent away.

We conveyed into the receiver a new glass phial, capable of holding about 6 or 7 ounces of water, into which we had before put 2 or 3 spoonfuls of that liquor, and stopt it close with a fit cork. The pneumatical vessel being emptied, there appeared

not

not any change in the inclofed water, the air imprifoned with it, not having the force to blow out the ftopple. Which event, though it were no other than we expected, was differing from what we defired. For we would gladly have feen, what change would have appeared in the water upon the bottle's being fuddenly unftopped, in a place where the ambient body was fo differing from our common air. Wherefore we did again put in the phial, but lefs ftrongly clofed than formerly, though as ftrongly ftopt as feemed requifite on ordinary occafions: but when the air was pumped out of the receiver, that within the phial did quickly, as we expected, find or make itfelf little paffages to get out at: as we argued, from this, that whereas when the phial was put in the time before, the water remained all the while perfectly free from bubbles; at this time the bottom of the glafs appeared all covered with them, and they, upon the egrefs of the excluded air into the receiver, did prefently flag and fhrink up.

FROM thefe trials it feemed deducible enough, that whilft the phial continued to be well ftopped, the included water did, from the air fhut up with it, fuftain a preffure equal to that of the atmofphere; fince till the air could get out of the glafs, there appeared no bubbles in the water, notwithftanding the want of preffure in the ambient body.

BUT to be fure to reach the chief end of our experiment, we made ufe of this other expedient. We caufed a convenient quantity of water to be put, and hermetically fhut up into a glafs egg, whofe long neck (which was purpofely made of an unequal thicknefs) was faftened to one end of a ftring, whofe other end was tied to the cover of our receiver, after the manner elfewhere mentioned already. Then the egg being conveyed into the pneumatical veffel, and that being evacuated, we did, by turning the brafs-ftopple formerly defcribed amongft the parts of our engine, fo fhorten the ftring as to break the glafs; whereby liberty being given to the air imprifoned in the egg to pafs into the cavity of the receiver, the fudden recefs of the air made the bubbles in a trice appear fo numerous, and afcend fo fwiftly in the water, that their motion looked like that of a violent fhower of rain;-fave that the bubbles did not, like the drops of rain, tend downwards, but upwards. Which made me refemble this phænomenon to what I have feen happen in the diffolution of feed-pearl in fome acid menftruum; in which, if a good quantity of the little pearls be caft whole, they will at firft, if the menftruum be fharp enough, be carried in fwarms from the bottom to the top of the liquor. We will add, that without fealing up the glafs, this experiment may be tried in one of our fmalleft receivers. For there the exfuction of the ambient air may be performed fo nimbly, that immediately the bubbles lurking in the water are allowed to difplay themfelves, and afcend in throngs; infomuch, as having in fuch a receiver tried the experiment with wine (as a more fpirituous liquor) inftead of water, the red-wine appeared all covered with a copious, but vanifhing white froth, almoft as if a veffel full of bottled drink had been unwarily opened.

EXPERIMENT XXIX.

IT may not a little conduce to the clearer explication of divers points in the doctrine of meteors, and perhaps of fome other phyfiological difficulties, to difcover what the air doth to the motion of thofe ftreams or exhalations that afcend into it; namely, whether they mount upwards by virtue of any fuch pofitive levity (as fome Peripateticks fpeak) acquired together with their aërial nature, as enables them to

pierce through part of the atmosphere, and overcome its resistance. Or else, whether these steams being once raised above the earth by their agitation, have their ascent and sustentation aloft, rather promoted than hindered by the air: as the inferior parts of that, being thicker and heavier than the superior, the steams can more easily continue for a while their agitation upwards than downwards; and afterwards are by the same fluidity and thickness of the air carried to and fro in it, and kept from relapsing to the earth: as in the sea-water the saline parts are kept from subsiding by those aqueous ones, wherewith they are associated.

WE hoped to illustrate this matter, by observing the motion of the smoke, proceeding from kindled or flaming bodies in our exhausted receiver. But as we formerly noted, upon the exsuction of the air, the smoking of those bodies presently ceased. We had thoughts also of conveying into our pneumatical glass a hot iron, with some body easy to be dissipated into smoke set upon it; but considered, that neither was that way free from inconveniencies; especially this, that the hot body would make the imprisoned air circulate within the receiver, and consequently make it questionable, whether the ascent of the steams would not be due to the new and acquired motion of the air.

WHEREFORE I bethought myself of another way to satisfy in some measure my curiosity, to wit, by means of a certain liquor, which I called to mind, that some years ago I had (for a design that belongs not to our present purpose) prepared; which, I suppose, I shewed your Lordship, and which had the luck to be taken notice of by divers very ingenious and famous men. For this liquor, though most of its ingredients be metals, and all of them ponderous enough, is yet of that nature, that whilst the phial wherein it is kept is stopped (how slight a cover soever) both the liquor and the glass are transparent; and so is that upper half of the glass, to which the liquor reacheth not: but as soon as ever the stopple is taken out, and full access is given to the external air, both in the inward part of the cork, and the liquor itself, do presently send upwards, and scatter abroad a fume as thick and white, as if there were a quantity of alabaster-dust thrown up into the air. And this smoking of the liquor lasts till my unwillingness to waste it invites me to stop it again; and then the ascension of the fumes suddenly ceaseth, till the phial be again unstopped.

THIS fuming liquor then, I thought, would much conduce to the discovery I desired to make, since it saved me the need of conveying any hot body with it into the receiver, and would not darken it with fumes before the time. Wherefore having tied to the phial a great weight of lead, to keep it from being lifted up by the drawing out of the cork: and having tied to the stopple one end of a string, of which the other end was made fast to the cover of the pneumatical glass, the liquor was carefully closed up after the wonted manner: then the air being diligently pumped out, the phial was unstopped in the emptied receiver. And though immediately, upon the drawing out of the cork, there appeared to be as it were thrown up some white fumes, which seemed to proceed from the air before imprisoned in the phial, and diffusing itself suddenly into the capacity of the receiver; yet we afterward observed, as we expected, that the fumes did not mount and disperse themselves as they used to do in the open air; but that, when by reason of the agitation of the corpuscles of the liquor, which could not continue their motion in so narrow a space as the phial afforded them, and were therefore reduced to thrust one another out of it; when, I say, by these assistances the fumes were ascended to the lip of the phial, they mounted no higher, but ran down along the outside of the phial to the bottom of it; and thence along a long and inclining piece of lead, on which the phial rested,

rested, like a little stream (not very much bigger than a swan's quill) whose nature they seemed to emulate so well, that it quitted not the phial till it was come to the bottom of it, and then forsook it in such a manner, as a stream of water of the same bigness would have done. And this stream lasted a pretty while, and would probably have lasted longer, but that being loth to waste my liquor, I let in at the stopcock a pretty deal of the external air; notwithstanding which, finding after a while that the stream did run afresh, though as it seemed not altogether so copious as before, I let as much more air as would come in; and found (somewhat to my wonder) that though the stream formerly mentioned disappeared, yet there appeared not any white fumes to arise, either from the cork, or out of the phial itself, no not when the cover was removed from the receiver: though not only after a while there ascended white fumes from the receiver, but having forthwith taken out the phial into the open air, it emitted white exhalations as before. And having presently after unstopped it in an open window, we found both it and the cork immediately to send forth a yet much more plentiful smoke. Though it be now divers years since this numerical liquor was prepared, after the manner mentioned either by *Carneades* or *Eleutherius* (for I do not well remember which) in those *Dialogues concerning heat and flame* that have been above mentioned.

MORE circumstances concerning these fumes we might have observed, had we not been deterred by an indisposition in point of health, from having much to do with steams of so dangerous a nature, as by that of the ingredients of this liquor these seem likely to be of.

THE reflections, that may be made upon this experiment, we have not now the leisure to prosecute; and therefore shall content ourselves to recommend the several circumstances of it to your Lordship's serious consideration; and to take notice *(en passant)* that steams in an ambient body, or a medium thinner than themselves, may both tend downwards, and otherwise emulate the nature of a liquor: which I therefore point at, that it may appear the less strange, if we sometimes speak of the atmosphere as of a kind of liquor, in comparison of that more thin and subtile celestial matter, that surrounds it.

AND though it might perchance suffice to have on this occasion intimated thus much; yet, lest this way of speaking of the atmosphere should be thought too bold and extravagant, I am content to borrow an experiment of the discourse formerly mentioned (touching *fluidity and firmness*,) and subjoin it here with alterations suitable to the contrivance of our engine; and this the rather, because I hope it may conduce to the discovery of the nature of the atmosphere: for which reason it might have been annexed to what hath been noted either upon the first, or eighteenth experiment, but that when they were written and sent away, it came not into my mind. The experiment then, as we tried in our engine, was as follows:

EXPERIMENT XXX.

WE took one of the small receivers, often mentioned already, and into it we conveyed a piece of well-lighted match; and letting it remain there, till it had filled the receiver with smoke, we took it out and hastily closed again the receiver, that the smoke might not get away. Then staying a while to let these fumes leisurely subside, we found, as we expected, that after some time they settled themselves in the lower half of the receiver, in a darkish body, leaving the upper half of

the receiver transparent, and, as to sight, full of nought but clear air. Now to manifest, that this smoke thus settled emulated a liquor, we inclined the engine that contained it sometimes to one side, and sometimes to the other; and observed the smoke to keep its surface almost horizontal, notwithstanding the stooping of the vessel that held it, as water, or another liquor, would in the like case have done. And if by a quicker rocking of the engine the smoke were more swiftly shaken, it would, like water, either vibrate to and fro from one side to the other of the glass, or else have its surface manifestly curled with waves, but preserve itself in an entire and distinct body from the incumbent air; and being permitted to rest a while, would soon recover its former smooth and level superficies. If also the key were turned, and the valve unstopped, so that there was a free, though but a narrow passage opened betwixt the internal air and the cavity of the receiver, then would some of this smoke fall down, as it were, in a stream into the subjacent cylinder, and a proportionate quantity of the outward air would manifestly ascend through it into the incumbent air, much after the same manner as if you invert a phial with a long neck, and well filled with red-wine, into a glass full of fair water, you shall see the water and wine by degrees mingle with one another; the one falling down as it were in little coloured streams, and the other ascending in its room in the like curled streams, sometimes preceded by round parcels of water, which, by reason of their transparency, look almost like bubbles. The other circumstances of this experiment, belonging not all of them to our present purpose, we shall content ourselves with taking notice of one, which seems the most important, and may illustrate and confirm some things formerly delivered. And it was, that if, when the superficies of our smoke lay smooth and horizontal, a hot iron were held near the outside of the receiver, the neighbouring part of the included fumes (for the rest did not very much alter their former superficies) being rarefied by the heat, would readily ascend in a large pillar of smoke to the very top of the receiver, yet without seeming to lose a distinct superficies, or to be confounded with air; below which, upon the recess of the adventitious heat, that by agitating it impelled it upward, it would again subside.

ALL which being added to the late experiment of the smoking liquor, and to what may be from that, which hath been elsewhere said, gathered to the same purpose, will, I hope, keep it at least from appearing absurd; if (since we see, that there is so great an inequality in the density and weight of liquors, that water is near fourteen times thinner or lighter than quicksilver of the same bulk, and well dephlegmed spirit of wine, yet much lighter than water) we venture to speak sometimes of the atmosphere, as if it were a peculiar kind of thin and halitious liquor (if I may so call it) much lighter than spirit of wine.

To these things I know not whether it will be requisite to add, that as we lately took notice of conspicuous waves, that appeared upon the superficies of our agitated smoke, so some such thing may not absurdly be conjectured to happen on the superficies of the atmosphere, by those strange ruggednesses, that appear (especially in the spring and fall, when exhalations and vapours are wont to ascend most plentifully) upon the limb or edge of the rising and setting sun. I speak thus diffidently upon this occasion, because I know, that by the fluctuation or boiling of the sun's own superficies, divers eminent mathematicians have plausibly enough (but how truly, I leave your Lordship to judge) endeavoured to give an account of it. But if we will join with those, that have ascribed of late this phænomenon to the refraction the sunbeams suffer in our vapid air, we may, as hath been intimated, promote their doctrine,

trine, by deducing from it, that probably the ſurface of the atmoſphere is oftentimes (if not always) exceedingly curled or waved. And certainly it is ſomewhat wonderful, as well as very pleaſant to behold, how, to him that looks upon the ſetting ſun through a long and excellent teleſcope, there will not only appear ſtrange inequalities in the edge of it (inſomuch that I have often ſeen it more indented than a ſaw); but thoſe inequalities will vaniſh in one place, and preſently appear in another, and ſeem perfectly to move like waves ſucceeding and deſtroying one another, ſave that their motion oftentimes ſeems to be quickeſt, as if in that vaſt ſea they were carried on by a current, or at leaſt by a tide. And this (as we elſewhere note) appears to the eye, not only when it looks directly through the teleſcope upon the ſun, but alſo when a large and well-defined image of the ſun is by the ſame teleſcope brought into a room and caſt upon a ſheet of white paper. But to inſiſt on this were to digreſs; and therefore I will proceed to experiments of another kind.

EXPERIMENT XXXI.

IT hath been admired by very ingenious men, that if the exquiſitely poliſhed ſurfaces of two flat pieces of marble be ſo congruous to each other, that from their mutual application there will reſult an immediate contact, they will ſtick ſo faſt together, that he, that lifts up the uppermoſt, ſhall, if the undermoſt be not exceeding heavy, lift up that too, and ſuſtain it aloft in the free air. A probable cauſe of this ſo cloſe adheſion we have elſewhere endeavoured to deduce from the unequal preſſure of the air upon the undermoſt ſtone; for the lower ſuperficies of that ſtone being freely expoſed to the air, is preſſed upon by it, whereas the uppermoſt ſurface, being contiguous to the ſuperior ſtone, is thereby defended from the preſſure of the air; which conſequently preſſing the lower ſtone againſt the upper, hinders it from falling, as we have elſewhere more fully declared. Upon theſe grounds we conjectured, that in caſe we could procure two marbles exactly ground to one another, and in caſe we could alſo ſufficiently evacuate our receiver; the lower ſtone would, for want of the wonted and ſuſtaining preſſure of the air, fall from the upper. But the farther trial of this experiment we muſt, unleſs your Lordſhip think it worth your making at *Paris*, put off till a fitter opportunity. For where we now are, we cannot procure marbles ſo exactly ground, that they will ſuſtain one another in the air above a minute or two, which is a much ſhorter time than the emptying of our receiver requires. We did indeed try to make our marbles ſtick cloſe together, by moiſtening their poliſhed ſurfaces with rectified ſpirit of wine, in regard that liquor, by its ſudden avolation from marble, if poured thereon, without leaving it moiſt or leſs ſmooth, ſeemed unable to ſuſtain them together after the manner of a glutinous body, and yet ſeemed ſufficient to exclude and keep out the air. But this we tried to little purpoſe, for having conveyed into the receiver two black ſquare marbles (the one of two inches and a third in length or breadth, and ſomewhat more than half an inch in thickneſs; the other of the ſame extent, but not much above half ſo thick) faſtened together by the intervention of pure ſpirit of wine; and having ſuſpended the thicker by a ſtring from the cover, we found not, that the exſuction of the ambient air would ſeparate them, though a weight amounting to four ounces were faſtened to the lowermoſt marble to facilitate its falling off.

I WOULD gladly have the experiment tried with marble ſo well poliſhed, as to need no liquor whatſoever to make them cohere, and in a veſſel, out of which the air may be more perfectly drawn than it was out of ours. But in the mean time,

 though

though we will not determine whether the spirit of wine did contribute to the strong cohesion of these stones, otherwise than by keeping even the subtlest parts of the air from getting in between them; yet it seemed, that the not falling down of the lowermost marble might, without improbability, be ascribed to the pressure of the air remaining in the receiver: which, as we formerly noted, having been able to keep a cylinder of water, of above a foot in height, from falling to the bottom of the tube, may well enough be supposed capable of keeping so broad a flat marble from descending. And though this may seem a strange proof of the strength of the spring of the air, even when rarefied, yet it will scarce seem incredible to him, that hath observed, how exceeding strong a cohesion may be made betwixt broad bodies, only by their immediate touching one another. A notable instance of which I met with in this short narrative of the learned *Zucchius: Juveni* (saith he) *lacertorum suorum robur jactanti proposita semel est lamina ærea, per ansam in medio extantem apprehensam elevanda è tabula marmorea, cui optime congruebat: qui primo tanquam rem ludicram puero committendam contempsit; tum instantibus amicis manum utramque admovens, cum luctatus diu hærentem non removisset, excusavit impotentiam, objecta peregrini & potentissimi glutinis interpositione, quo fortissime copulante nequiret divelli; donec vidit ab alio per tabulam facillimè laminam deduci, & ad extrema productam, & actam in transversum inde deportari.* But that we may learn from our own engine, that two bodies, though they touch each other but in a small part of their surfaces, may be made to cohere very strongly, only by this, that the air presses much more forcibly upon the inferior superficies of the lowermost body, than upon the upper surface of the same; we will hereunto annex the following experiment, though out of the order, wherein they were made.

P. Nic. Zucchius apud Salv. part 1. Mec. Hydraulo-pneum.

EXPERIMENT XXXII.

I Remember I have, in a discourse touching fluidity and firmness, made mention of my having, by the exsuction of the air out of a glass vessel, made that vessel take up, or suck up (to speak in the common language) a body weighing divers ounces; but our engine affording us the opportunity of making considerabler experiments of that kind, we thought fit to make a further trial of the force of the atmosphere's pressure upwards, after the following manner:

THE receiver having been exquisitely closed, as we have often taught already, and the air being in a good measure drawn out of it, it was removed from off the pump: and to the lower branch of the stop-cock, there was speedily applied a tapering valve of brass, such as is described in the 9th figure, made fit to go with its narrower end into the cavity of the branch, and to fill the orifice of that cavity with its broader part. And that the air might not get in at the little intervals, left here and there between the convex surface of the stopple and the internal edge of the branch, those intervals were stopt with a little diachylon. And to the door, or (if you please) that part of the valve, which was to move to and fro, and in this experiment hung perpendicular to the horizon, there was, at a button of brass belonging to the valve, fastened a broad scale, wherein weights were to be put. This done, the key of the stop-cock was turned, and the external air beating like a forcible stream upon the valve to get in there, it did suddenly both shut the valve, and keep it shut so strongly, that we had time to cast in divers weights one after another into the scale; till at length the weight overpowering the pressure of the atmosphere, drew down the valve by the strings that tied the scale to it, and gave liberty to the outward air to rush

rush into the receiver. Though another time, when the valve had but little weight hanging at it, being, by I know not what accident, drawn down beneath its former place, it was by the impetuous current of the outward air suddenly impelled up into it again, and kept there. But in the former experiment it is remarkable, that though the receiver were not well exhausted, and though it leaked, whilst the rest of the experiment was in prosecution; and though the valve, whereon the cylinder of the atmosphere could press, were not above an inch and a half in diameter, yet the weight kept up by suction, or rather supported by the air, (namely the valve, the scale, and what was cast out of it) being sent to be weighed, amounted to about ten of our common pounds, consisting of sixteen ounces a piece; so that we doubted not, but that, had the experiment been made with favourable circumstances, the air endeavouring to press in at the orifice of the stop-cock, would have kept a very much greater weight from falling out of it; I say the air, because we found, by trial purposely made, that neither the imperfect contact of the valve and the stop-cock, nor the diachylon, that was employed to fill up the little crannies left betwixt them, were considerable in this experiment. By which it may, among other things, appear, that I did not without cause, in the above-named discourse touching fluidity and firmness, ascribe a great force, even to such pillars of air, as may be supposed to begin at the top of the atmosphere, and recoiling from the ground, to terminate on the bodies on which they press; since in the present experiment such a weight was supported by so slender a cylinder of air, rebounding from the earth to the valve, whereon it did bear.

EXPERIMENT XXXIII.

BUT in regard we have not yet been able to empty so great a vessel as our receiver, so well as we can the cylinder itself; our pump alone may afford us a nobler instance of the force of the air we live in, insomuch, that by help of this part of our engine, we may give a pretty near guess at the strength of the atmosphere, computed as a weight. And the way may be this; first, the sucker being brought to move easily up and down the cylinder, is to be impelled to the top of it: then the receiver must be taken off from the pump, that the upper orifice of the cylinder remaining open, the air may freely succeed the sucker, and therefore readily yield to its motion downwards. This done, there must be fastened to one of the iron teeth of the sucker such a weight as may just suffice to draw it to the bottom of the cylinder. And having thus examined, what weight is necessary to draw down the sucker, when the atmosphere makes no other than the ordinary resistance of the air against its descent; the sucker must be again forced to the top of the cylinder, whose upper orifice must now be exactly closed; and then (the first weight remaining) we easily may, by hanging a scale to the above mentioned iron (that makes part of the sucker) cast in known weights so long, till in spite of the reluctancy of the atmosphere the sucker may be drawn down. For to these weights in the scale that of the scale being added, the sum will give us the weight of a column of air, equal in diameter to the sucker, or to the cavity of the cylinder, and in length to the height of the atmosphere.

ACCORDING to this method we did, since the writing of the last experiment, attempt to measure the pressure of the atmosphere, but found it more difficult than we expected to perform it with any accurateness; for though by the help of the manubrium

brium the ſucker moved up and down with ſo much eaſe, that one would have thought, that both its convex ſurface, and the concave one of the cylinder, were exquiſitely ſmooth, and as it were ſlippery; yet when the ſucker came to be moved only with a dead weight or preſſure (that was not, like the force of him that pumped, intended as occaſion required) we found, that the little roughneſſes or other inequalities, and perhaps too the unequal preſſure of the leather againſt the cavity of the cylinder, were able, now and then, to put a ſtop to the deſcent or aſcent of the ſucker, though a very little external help would eaſily ſurmount that impediment; and then the ſucker would, for a while, continue its formerly interrupted motion, though that aſſiſtance were withdrawn. But this diſcouragement did not deter us from proſecuting our experiment, and endeavouring, by a careful trial, to make it as inſtructive as we could. We found then, that a leaden weight, of 28 pounds (each conſiſting of ſixteen ounces) being faſtened to one of the the teeth of the ſucker, drew it down cloſely enough, when the upper orifice of the cylinder was left open, though by the help of oil and water, and by the frequent moving the ſucker up and down with the manubrium, its motion in the cylinder had been before purpoſely facilitated. This done, the upper orifice of the cylinder was very carefully and cloſely ſtopt, the valve being likewiſe ſhut with its wonted ſtopple well oiled, after the ſucker had been again impelled up to the top of the cylinder. Then to the precedent twenty-eight pound we added a hundred and twelve pounds more; which forcing down the ſucker, though but leiſurely, we took off the twenty-eight pound weight; and being unable to procure juſt ſuch weights as we would have had, we hung on, inſtead of it, one of fourteen pound; but found that, with the reſt, unable to carry down the ſucker. And to ſatisfy ourſelves, and the ſpectators, that it was the reſiſtance of the ambient air, that hindered the deſcent of ſo great a weight, after that we had tried, that upon unſtopping the valve, and thereby opening an acceſs to the external air, the ſucker would be immediately drawn down; after this, I ſay, we made this farther experiment, that having by a man's ſtrength forcibly depreſſed the ſucker to the bottom of the cylinder, and then faſtened weights to the above-named iron, that makes part of that ſucker, the preſſure of the external air finding little or nothing in the cavity of the evacuated cylinder to reſiſt it, did preſently begin to impel the ſucker, with the weights that clogged it, towards the upper part of the cylinder; till ſome ſuch accidental impediment, as we formerly mentioned, checked its courſe. And when that rub (which eaſily might be) was taken out of the way, it would continue its aſcent to the top, to the no ſmall wonder of thoſe byſtanders, that could not comprehend, how ſuch a weight could aſcend, as it were, of itſelf; that is, without any inviſible force, or ſo much as ſuction to lift it up. And indeed it is very conſiderable, that though poſſibly there might remain ſome particles of air in the cylinder, after the drawing down of the ſucker; yet the preſſure of a cylinder of the atmoſphere, ſomewhat leſs than three inches in diameter (for, as it was ſaid in the deſcription of our engine, the cavity of the cylinder was no broader) was able, uncompreſſed, not only to ſuſtain, but even to drive up a weight of a hundred and odd pounds: for beſides the weight of the whole ſucker itſelf, which amounts to ſome pounds, the weights annexed to it made up an hundred and three pounds, beſides an iron bar, that by conjecture weighed two pounds more; and yet all theſe together fall ſomewhat ſhort of the weight, which we lately mentioned the reſiſtance of the air to have held ſuſpended in the cavity of the cylinder.

AND though (as hath been already acknowledged) we cannot peradventure obtain by the recited means ſo exact an account as were to be wiſhed, of what we would diſcover;

cover; yet, if it ſerve us to ground conjectures more approaching to the truth, than we have hitherto met with, I hope it will be conſidered (which a famous poet judiciouſly ſays,)

Eſt quoddam prodire tenus, ſi non datur ultra.

PERADVENTURE it will not be impertinent to annex to the other circumſtances, that have been already ſet down concerning this experiment, that it was made in winter, in weather neither froſty nor rainy, about the change of the moon, and at a place whoſe latitude is near about 51 degrees and a half; for perhaps the force or preſſure of the air may vary, according to the ſeaſons of the year, the temperature of the weather, the elevation of the pole, or the phaſes of the moon; all, or even any of them ſeeming capable to alter either the height or conſiſtence of the incumbent atmoſphere: and therefore it would not be amiſs, if this experiment were carefully tried at ſeveral times and places, with variety of circumſtances. It might alſo be tried with cylinders of ſeveral diameters, exquiſitely fitted with ſuckers, that we might know, what proportion ſeveral pillars of the atmoſphere bear to the weights they are able to ſuſtain or lift up; and conſequently, whether the increaſe or decrement of the reſiſtance of the ambient air can be reduced to any regular proportion to the diameters of the ſuckers. Theſe, and divers other ſuch things, which may be tried with this cylinder, might moſt of them be more exactly tried by the Torricellian experiments, if we could get tubes ſo accurately blown and drawn, that the cavity were perfectly cylindrical.

To dwell upon all the ſeveral reflexions, that a ſpeculative wit might make upon this and the foregoing experiment (I mean the thirty-third and thirty-ſecond) would require almoſt a volume; whereas our occaſions will ſcarce allow us time to touch upon three or four of the chief inferences, that ſeem deducible from them, and therefore we ſhall content ourſelves to point at thoſe few.

AND firſt, as many other phænomena of our engine, ſo eſpecially the two lately mentioned experiments ſeem very much to call in queſtion the received opinion of the nature or cauſe of ſuction. For it is true indeed, that when men ſuck, they commonly uſe ſome manifeſt endeavour, by a peculiar motion of their mouths, cheſts, and ſome other conſpiring parts, to convey to them the body to be ſucked in. And hence perhaps they have taken occaſion to think, that in all ſuction there muſt be ſome endeavour or motion in the ſucking to attract the ſucked body. But in our laſt experiment it appears not at all, how the upper part of the emptied cylinder, that remains moveleſs all the while, or any part of it, doth at all endeavour to draw to it the depreſſed ſucker and the annexed weights. And yet thoſe, that behold the aſcenſion of the ſucker, without ſeriouſly conſidering the cauſe of it, do readily conclude it to be raiſed by ſomething, that powerfully ſucks or attracts it, though they ſee not what that may be, or where it lurks. So that it ſeems not abſolutely neceſſary to ſuction, that there be in the body, which is ſaid to ſuck, an endeavour or motion in order thereunto; but rather that ſuction may be at leaſt for the moſt part reduced to pulſion, and its effects aſcribed to ſuch a preſſure of the neighbouring air upon thoſe bodies (whether aërial, or of other natures) that are contiguous to the body, that is ſaid to attract them, as is ſtronger, than that ſubſtance, which poſſeſſeth the cavity of that ſucking body, is able to reſiſt. To object here, that it was ſome particles of air remaining in the emptied cylinder, that attracted this weight to obviate a vacuum, will ſcarce be ſatisfactory; unleſs it can be clearly made

 out,

out, by what little hooks, or other grappling instruments, the internal air could take hold of the sucker; how so little of it obtained the force to lift up so great a weight; and why also, upon the letting in of a little more air into one of our evacuated vessels, the attraction is, instead of being strengthened, much weakened; though, if there were danger of a vacuum before, it would remain, notwithstanding this ingress of a little air. For that still there remained in the capacity of the exhausted cylinder store of little rooms, or spaces empty or devoid of air, may appear by the great violence, wherewith the air rusheth in, if any way be opened to it. And that it is not so much the decrement of the vacuum within the cavity of the vessel, that debilitates the attraction, as the spring of the included air (whose presence makes the decrement) that doth it by resisting the pressure of the external air, seems probable, partly from the disability of vacuities, whether greater or lesser, to resist the pressure of the air; and partly by some of the phænomena of our experiments, and particularly by this circumstance of the three and thirtieth, that the sucker was, by the pressure of the ambient air, impelled upwards with its weight hanging at it, not only when it was in the bottom of the cylinder, and consequently left a great vacuum in the cavity of it; but when the sucker had been already impelled almost to the top of the cylinder, and consequently, when the vacuum that remained was become very little in comparison of that, which preceded the beginning of the sucker's ascension.

IN the next place, these experiments may teach us, what to judge of the vulgar axiom received for so many ages as an undoubted truth in the Peripatetic schools, that nature abhors and flieth a vacuum, and that to such a degree, that no human power (to go no higher) is able to make one in the universe; wherein heaven and earth would change places, and all its other bodies rather act contrary to their own nature, than suffer it. For, if by a vacuum we will understand a place perfectly devoid of all corporeal substance, it may indeed then, as we formerly noted, be plausibly enough maintained, that there is no such thing in the world. But that the generality of the Plenists (especially till of late years some of them grew more wary) did not take a vacuum in so strict a sense, may appear by the experiments formerly, and even to this day employed by the denyers of a vacuum, to prove it impossible, that there can be any made. For when they alledge (for instance) that when a man sucks water through a long pipe, that heavy liquor, contrary to its nature, ascends into the sucker's mouth, only to fill up that room made by the dilatation of his breast and lungs, which otherwise will in part be empty: and when they tell us, that the reason, why if a long pipe exactly closed at one end be filled top-full of water, and then inverted, no liquor will fall out of the open orifice; or, to use a more familiar example, when they teach, that the cause, why in a gardener's watering pot shaped conically or like a sugar-loaf, filled with water, no liquor falls down through the numerous holes at the bottom, whilst the gardener keeps his thumb upon the orifice of the hole at the top, and no longer; must be, that if in the case proposed the water should descend, the air being unable to succeed it, there would be left at the upper and deserted part of the vessel a vacuum, that would be avoided, if the hole at the top were opened: When (I say) they alledge such experiments, the tendency of them seems plainly to import, that they mean, by a vacuum, any space here below, that is not filled with a visible body, or at least with air, though it be not quite devoid of all body whatsoever. For why should nature, out of her detestation of a vacuum, make bodies act contrary to their own tendency, that a place may be filled with air, if its being so were not necessary to the avoiding of a vacuum?

TAKING

TAKING then a vacuum in this vulgar and obvious ſenſe, the common opinion about it ſeems liable to ſeveral exceptions, whereof ſome of the chief are ſuggeſted to us by our engine.

IT will not eaſily then be intelligibly made out, how hatred or averſation, which is a paſſion of the ſoul, can either for a vacuum, or any other object, be ſuppoſed to be in water, or ſuch like inanimate body, which cannot be preſumed to know, when a vacuum would enſue, if they did not beſtir themſelves to prevent it; nor to be ſo generous as to act contrary to what is moſt conducive to their own particular preſervation for the public good of the univerſe. As much then of intelligible and probable truth, as is contained in this metaphorical expreſſion, ſeems to amount but to this; that by the wiſe author of nature (who is juſtly ſaid to have made all things in number, weight and meaſure) the univerſe, and the parts of it, are ſo contrived, that it is as hard to make a vacuum in it, as if they ſtudiouſly conſpired to prevent it. And how far this itſelf may be granted, deſerves to be farther conſidered.

FOR, in the next place, our experiments ſeem to teach, that the ſuppoſed averſation of nature to a vacuum is but accidental, or in conſequence, partly of the weight and fluidity, or, at leaſt, fluxility of the bodies here below; and partly, and perhaps principally, of the air, whoſe reſtleſs endeavour to expand itſelf every way makes it either ruſh in itſelf, or compel the interpoſed bodies into ſmall ſpaces, where it finds no greater reſiſtance than it can ſurmount. And that in thoſe motions which are made *ob fugam vacui* (as the common phraſe is) bodies act without ſuch generoſity and conſideration, as is wont to be aſcribed to them, is apparent enough in our 32d experiment, where the torrent of air, that ſeemed to ſtrive to get into the emptied receiver, did plainly prevent its own deſign, by ſo impelling the valve, as to make it ſhut the only orifice the air was to get out at. And if afterwards, either nature, or the internal air had a deſign the external air ſhould be attracted, they ſeemed to proſecute it very unwiſely by continuing to ſuck the valve ſo ſtrongly, when they found, that by that ſuction the valve itſelf could not be drawn in; whereas by forbearing to ſuck, the valve would by its own weight have fallen down, and ſuffered the excluded air to return freely, and to fill again the exhauſted veſſel.

AND this minds me to take notice of another deficiency, pointed at by our experiments in the common doctrine of thoſe Pleniſts we reaſon with; for many of thoſe unuſual motions in bodies, that are ſaid to be made to eſcape a vacuum, ſeem rather made to fill it. For why, to inſtance in our newly mentioned experiment, as ſoon as the valve was depreſſed by the weight we hung at it, ſhould the air ſo impetuouſly and copiouſly ruſh into the cavity of the receiver, if there were before no vacant room there to receive it? And if there were, then all the while the valve kept out the air, thoſe little ſpaces in the receiver, which the corpuſcles of that air afterwards filled, may be concluded to have remained empty. So that the ſeeming violence, employed by nature on the occaſion of the evacuating of the veſſel, ſeems to have come too late to hinder the making of vacuities in the receiver, and only to have, as ſoon as we permitted, filled up with air thoſe that were already made.

AND as for the care of the public good of the univerſe aſcribed to dead and ſtupid bodies, we ſhall only demand, why in our 19th experiment, upon the exſuction of the ambient air, the water deſerted the upper half of the glaſs tube; and did not aſcend to fill it up, till the external air was let in upon it. Whereas by its eaſy and ſudden regaining that upper part of the tube, it appeared both that there was there much ſpace devoid of air, and that the water might with ſmall or no reſiſtance have aſcended into it, if it could have done ſo without the impulſion of the re-admitted

air; which, it seems, was necessary to mind the water of its formerly neglected duty to the universe.

NAY, for aught appeareth, even when the excluded air, as soon as it was permitted, rushed violently into our exhausted receiver, that flowing in of the air proceeded rather from the determinate force of the spring of the neighbouring air, than from any endeavour to fill up, much less to prevent vacuities. For though, when as much air as will, is gotten into our receiver, our present opponents take it for granted, that it is full of air; yet if it be remembered, that when we made our 17th experiment, we crouded in more air to our receiver than it usually holds; and if we also consider (which is much more) that the air of the same consistence with that in our receiver may in wind-guns, as is known, and as we have tried, be compressed at least into half its wonted room (I say, at least, because some affirm, that the air may be thrust into an 8th, or a yet smaller part of its ordinary extent) it seems necessary to admit either a notion of condensation and rarefaction, that is not intelligible, or that in the capacity of our receiver, when presumed to be full of air, there yet remained as much of space as was taken up by all the aërial corpuscles, unpossessed by the air. Which seems plainly to infer, that the air, that rushed into our emptied vessel, did not do it precisely to fill up the vacuities of it, since it left so many unfilled, but rather was thrust in by the pressure of the contiguous air: which as it could not but be always ready to expand itself, where it found least resistance, so was it unable to fill the receiver any more, than until the air within was reduced to the same measure of compactness with that without.

WE may also from our two already often-mentioned experiments farther deduce, that (since nature's hatred of a vacuum is but metaphorical and accidental, being but a consequence or result of the pressure of the air and of the gravity, and partly also of the fluxility of some other bodies) the power she makes use of to hinder a vacuum, is not (as we have elsewhere also noted) any such boundless thing, as men have been pleased to imagine. And the reasons, why in the former experiments, mentioned in favour of the Plenists, bodies seem to forget their own natures to shun a vacuum, seems to be but this; that in the alledged cases the weight of that water, that was either kept from falling or impelled up, was not great enough to surmount the pressure of the contiguous air; which, if it had been, the water would have subsided, though no air could have succeeded. For not to repeat that experiment of Monsieur *Paschal* (formerly mentioned to have been tried in a glass exceeding 32 feet) wherein the inverted pipe being long enough to contain a competent weight of water, that liquor freely ran out at the lower orifice: not to mention this (I say) we saw in our nineteenth experiment, that when the pressure of the ambient air was sufficiently weakened, the water would fall out apace at the orifice even of a short pipe, though the air could not succeed into the room deserted by it. And it were not amiss, if trial were made on the tops of very high mountains, to discover with what ease a vacuum could be made near the confines of the atmosphere, where the air is probably but light in comparison of what it is here below. But our present (three and thirtieth) experiment seems to manifest, not only that the power, exercised by nature, to shun or replenish a vacuum is limited, but that it may be determined even to pounds and ounces: insomuch that we might say, such a weight nature will sustain or will lift up to resist a vacuum in our engine; but if an ounce more be added to that weight, it will surmount her so much magnified detestation of vacuities. And thus, my Lord, our experiments may not only answer those of the Plenists, but enable us to retort their arguments against themselves; since, if that be true, which they alledge, that when

when water falls not down according to its nature, in a body, wherein no air can succeed to fill up the place it must leave, the suspension of the liquor is made, *ne detur vacuum* (as they speak) it will follow, that if the water can be brought to subside in such a case, that deserted space may be deemed empty, according to their own doctrine; especially, since nature (as they would persuade us) bestirs herself so mightily to keep it from being deserted.

I HOPE I shall not need to remind your Lordship, that I have all this while been speaking of a vacuum, not in the strict and philosophical sense, but in that more obvious and familiar one, that hath been formerly declared.

AND therefore I shall now proceed to observe in the last place, that our 33d experiment affords us a notable proof of the unheeded strength of that pressure, which is sustained by the corpuscles of what we call the free air, and presume to be uncompressed. For, as fluid and yielding a body as it is, our experiment teacheth us, that even in our climate, and without any other compression than what is (at least here below) natural, or (to speak more properly) ordinary to it, it bears so strongly upon the bodies, whereunto it is contiguous, that a cylinder of this free air, not exceeding three inches in diameter, is able to raise and carry up a weight, amounting to between sixteen and seventeen hundred ounces. I said, even in our climate, because that is temperate enough; and as far as my observations assist me to conjecture, the air in many other more northern countries may be much thicker, and able to support a greater weight: which is not to be doubted of, if there be no mistake in what is recorded concerning the * Hollanders, that were forced by the ice to winter in *Nova Zembla*, namely, that they found there so condensed an air, that they could not make their clock go, even by a very great addition to the weights, that were wont to move it.

I SUPPOSE your Lordship will readily take notice, that I might very easily have discoursed much more fully and accurately than I have done, against the common opinion touching suction, and touching nature's hatred of a vacuum. But I was willing to keep myself to those considerations touching these matters, that might be verified by our engine itself; especially since, as I said at first, it would take up too much time to insist particularly upon all the reflections, that may be made even upon our two last experiments. And therefore passing to the next, I shall leave it to your Lordship to consider, how far these trials of ours will either confirm or disfavour the new doctrine of several eminent Naturalists, who teach, that in all motion there is necessarily a circle of bodies, as they speak, moving together; and whether the circles in such motion be an accidental or consequential thing or no.

EXPERIMENT XXXIV.

IT is a known thing to those, that are conversant in the hydrostatics, that two bodies, which in the air are of equal weight, but of unequal bulk, as gold for instance, and iron, being afterwards weighed in water, will lose their æquilibrium upon the change of the ambient body: so that the gold will sink lower than the iron; which, by reason of its greater bulk, hath more water to lift or displace, that it may sink. By analogy to this experiment, it seemed probable, that if two weights did

* *Aere frigido existente tardius moventur Automata quam aere calido, adeo quidem ut Automaton, quod Belgæ in Nova Zembla agentes in ædibus suis collocaverunt, omnino motu cessaverit etsi multo majus pondus ei addidissent quam antea ferre solebat.* Varenius Geog. General. Lib. III. Propo. 7. p. 648.

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in our engine balance each other, when the glass was full of air; upon the exsuction of a great part of that air, so notable a change in the consistence of the ambient body would make them lose their æquilibrium.

But being desirous at the same time to make a trial, for a certain design, that needs not here be mentioned, we took for one of our weights a dry bladder, strongly tied at the neck, and about half filled with air (that being a weight both slight, and that would expand itself in the evacuated glass) and fastening that to one part of our formerly mentioned exact balance (which turns with the 32d part of a grain) we put a metalline counterpoise into the opposite scale; and so the two weights being brought to an æquilibrium, the balance was conveyed into the receiver, and suspended from the cover of it.

But before we proceed farther, we must note, that presently after the laying on of the cover, the bladder appeared to preponderate; whereupon the scales being taken out, and reduced very near to an æquilibrium, yet so, that a little advantage remained on that side, to which the metalline weight belonged; they were again let down into the receiver, which was presently made fast with plaister, and a hot iron: soon after which, before the pump was employed, the bladder seemed again a little to preponderate. Afterwards the air in the glass being begun to be drawn out, the bladder began (according to the formerly mentioned observations) to expand itself and manifestly to outweigh the opposite weight, drawing down the scale, to which it was fastened, very much beneath the other, especially when the air had swelled to its full extent.

This done, we very leisurely let in the external air; and observed, that upon the flagging of the bladder, the scale, whereto it was fastened, not only by degrees returned to an æquilibrium with the other, but at length was a little outweighed by it.

But because we suspected there might have intervened some unheeded circumstance in this last part of the experiment, we would not presently take out the scales, nor meddle with the cover; but leaving things as they were, we perceived, that after a little while the bladder began again to preponderate, and by degrees to sink lower and lower for divers hours; wherefore, leaving the vessel closed up all night, we repaired to it next morning, and found the bladder fallen yet lower; as if the very substance of it had imbibed some of the moisture wherewith the air (the season being very rainy) did then abound; as lute-strings, which are made likewise of the membraneous parts of guts, strongly wreathed, are known to swell so much oftentimes, as to break in rainy and wet weather. Which conjecture is the more to be regarded, because congruously unto it one of the company having a little warmed the bladder, found it then lighter than the opposite weight. But this must be looked upon as a bare conjecture, till we can gain time to make farther trials about it. In the mean while, we shall add, that without removing the scales or the cover of the receiver, we again caused the air to be drawn out, (the weather continuing very moist) but found not any manifest alteration in the balance; whether because the æquilibrium was too far lost to let a small change appear, we determine not.

But to make the experiment with a body less apt to be altered by the temperature of the air, than was the bladder; we brought the scales again to an æquilibrium with two weights, whereof the one was of lead, the other of cork. And having evacuated the receiver, we observed, that both upon the exsuction, and after the return of the air, the cork did manifestly preponderate, and much more, a while after the air had been let in again, than whilst it was kept out. Wherefore, in the room of the cork, we substituted a piece of charcoal, as less likely to imbibe any moisture from

from the air, but the event proved much the ſame with that newly related: ſo that this experiment ſeems more liable to caſualties than any, excepting one we have made in our engine. And as it is difficult to prevent them, ſo it ſeems not very eaſy to diſcover the cauſes of them, whereof we ſhall therefore at preſent forbear mentioning our conjectures.

EXPERIMENT XXXV.

SOME learned Mathematicians have of late ingeniouſly endeavoured to reduce filtres to ſiphons; but ſtill the true cauſe of the aſcenſion of water, and other liquors both in ſiphons and in filtration, needing (for aught we have yet found) a clearer diſcovery and explication, we were deſirous to try, whether or no the preſſure of the air might reaſonably be ſuppoſed to have either the principal, or at leaſt a conſiderable intereſt in the raiſing of thoſe liquors. But becauſe we found, that we could not yet ſo evacuate our receiver, but that the remaining air, though but little in compariſon of the exhauſted, would be able to impel the water to a greater height than is uſual in ordinary filtrations; we reſolved, inſtead of a liſt of cotton, or the like filtre, to make uſe of a ſiphon of glaſs, delineated in the third figure; conſiſting of three pieces, two ſtraight, and the third crooked to join them together; whoſe junctures were diligently cloſed, that no air might find entrance at them. One of the legs of this ſiphon was (as it ſhould be) ſomewhat longer than the other, and was pervious at the bottom of it only, by a hole almoſt as ſlender as a hair, that the water might but very leiſurely drop out of it, leſt it ſhould all run out before the experiment were compleated. The other and ſhorter leg of the ſiphon was quite open at the end, and the ſame wideneſs with the reſt of the pipe, whoſe bore was about a quarter of an inch. The whole ſiphon made up of theſe ſeveral pieces put together was deſigned to be about a foot and a half long; that the remaining air, when the veſſel was exhauſted after the wonted manner, might not be able to impel the water to the top of the ſiphon; which being inverted, was filled with water, and of which the ſhorter leg being let down two or three inches deep into a glaſs veſſel full of water, and the upper parts of it being faſtened to the inſide of the cover of the receiver, we proceeded to cloſe firſt, and then to empty the veſſel.

THE effect of the trial was this, that till a pretty quantity of air had been drawn out, the water dropped freely out at the lower end of the lower leg of the ſiphon, as if the experiment had been performed in the free air. But afterwards, the bubbles (as had been apprehended) began to diſcloſe themſelves in the water, and aſcending to the top of the ſiphon, imbodied themſelves there into one, which was augmented by little and little, by the riſing of other bubbles, that from time to time broke into it, but much more by its own dilatation, which increaſed proportionably to the exſuction, that was made of the air out of the receiver. So that at length the water in the ſhorter leg of the ſiphon was reduced, partly by the extraction of the ambient air, and partly by the expanſion of the great bubble at the upper part of the ſiphon, to be about a foot high, if ſo much; whereby it came to paſs, that the courſe of the water in the ſiphon was interrupted, and that, which remained in the longer leg of it, continued ſuſpended there without dropping any longer. But upon the turning of the ſtop-cock, the outward air (being let into the receiver) got into the ſiphon, by the little hole, at which the water formerly dropped out; and traverſing all the incumbent cylinder of water, in the form of bubbles, joined itſelf with that air, that before poſſeſſed the top of the ſiphon.

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To prevent the inconveniences arising from these bubbles, two glass pipes, like the former, were so placed, as to terminate together in the midst of the belly of a glass phial, into whose neck they were carefully fastened with cement; and then both the phial and the pipes being (which was not done without difficulty) totally filled with water, the siphon described in the fifth figure was placed with its shorter leg in the glass of water, as formerly: and the experiment being prosecuted after the same manner, much more air than formerly was drawn out, before the bubbles, disclosing themselves in the water, were able to disturb the experiment; because that in the capacity of the phial there was room enough for them to stretch themselves, without depressing the water below the ends of the pipes: and during this time, the water continued to drop out of the propending leg of the siphon. But at length the receiver being very much emptied, the passage of the water through the siphon ceased, the upper ends of the pipes beginning to appear a little above the remaining water in the phial, whose dilated air appeared likewise to press down the water in the pipes, and fill the upper part of them.

And hereby the continuity of the water, and so the experiment itself being interrupted, we were invited to let in the air again, which, according to its various proportions of pressure to that of the air in the phial and the pipes, did for a good while exhibit a pleasing variety of phænomena, which we have not now the leisure to recite. And though upon the whole matter there seemed little or no cause to doubt, but that, if the bubbles had not disturbed the experiment, it would manifestly enough have appeared, that the course of water through siphons depends upon the pressure of the air; yet we resolved, at our next leisure and conveniency, to try the experiment again, with a quantity of water before freed from bubbles by the help of the same engine.

This occasion I have had to take notice of siphons, puts me in mind of an odd kind of siphon, that I caused to be made a pretty while ago; and which hath been since, by an ingenious man of your acquaintance, communicated to divers others. The occasion was this: an eminent Mathematician told me one day, that some inquisitive Frenchmen (whose names I know not) had observed, that in case one end of a slender and perforated pipe of glass be dipped in water, the liquor will ascend to some height in the pipe, though held perpendicular to the plain of the water. And, to satisfy me, that he misrelated not the experiment, he soon after brought two or three small pipes of glass, which gave me the opportunity of trying it; though I had the less reason to distrust it, because I remember I had often, in the long and slender pipes of some weather-glasses, which I had caused to be made after a somewhat peculiar fashion, taken notice of the like ascension of the liquor, though (presuming it might be casual) I had made but little reflection upon it. But after this trial, beginning to suppose, that though the water in these pipes, that were brought me, rise not above a quarter of an inch (if near so high) yet, if the pipes were made slender enough, the water might rise to a very much greater height; I caused several of them to be, by a dexterous hand, drawn out at the flame of a lamp, in one of which, that was almost incredibly slender, we found, that the water ascended (as it were of itself) five inches by measure, to the no small wonder of some famous Mathematicians, who were spectators of some of these experiments. And this height the water reached to, though the pipe were held in as erected a posture as we could; for if it were inclined, the water would fill a greater part of it, though not rise higher in it. And we also found, that when the inside of the pipe was wetted beforehand, the water would rise much better than otherways. But we caused not all our slender pipes to be made streight, but some of them crooked, like siphons: and having

having immerſed the ſhorter leg of one of theſe into a glaſs, that held ſome fair water, we found, as we expected, that the water ariſing to the top of the ſiphon, though that were high enough, did of itſelf run down the longer leg, and continue running like an ordinary ſiphon. The cauſe of this aſcenſion of the water appeared to all that were preſent ſo difficult, that I muſt not ſtay to enumerate the various conjectures that were made at it, much leſs to examine them; eſpecially having nothing but bare conjectures to ſubſtitute in the room of thoſe I do not approve. We tried indeed, by conveying a very ſlender pipe and a ſmall veſſel of water into our engine, whether or no the exſuction of the ambient air would aſſiſt us to find the cauſe of the aſcenſion we have been ſpeaking of: but though we employed red wine inſtead of water, yet we could ſcarce certainly perceive thorough ſo much glaſs, as was interpoſed betwixt our eyes and the liquor, what happened in a pipe ſo ſlender, that the redneſs of the wine was ſcarce ſenſible in it. But, as far as we could diſcern, there happened no great alteration to the liquor; which ſeemed the leſs ſtrange, becauſe the ſpring of that air, that might depreſs the water in the pipe, was equally debilitated with that, which remained to preſs upon the ſurface of the water in the little glaſs. Wherefore, in favour of his ingenious conjecture, who aſcribed the phænomenon under conſideration to the greater preſſure made upon the water by the air without the pipe, than by that within it, (where ſo much of the water, conſiſting perhaps of corpuſcles more pliant to the internal ſurfaces of the air, was contiguous to the glaſs) it was ſhown, that in caſe the little glaſs-veſſel, that held the water, of which a part aſcended into the ſlender pipe, were ſo cloſed, that a man might with his mouth ſuck the air out of it, the water would immediately ſubſide in the ſmall pipe. And this would indeed infer, that it aſcended before only by the preſſure of the incumbent air; but that it may (how juſtly I know not) be objected, that peradventure this would not happen, in caſe the upper end of the pipe were in a vacuum; and that it is very probable the water may ſubſide, not becauſe the preſſure of the internal air is taken off by the exſuction, but by reaſon of the ſpring of the external air, which impels the water, it finds in its way to the cavity deſerted by the other air, and would as well impel the ſame water upwards, as make it ſubſide, if it were not for the accidental poſture of the glaſſes. However, having not now leiſure to examine any farther this matter, I ſhall only mind your Lordſhip, that if you will proſecute this ſpeculation, it will be pertinent to find out likewiſe, why the ſurface of water (as is manifeſt in pipes) uſeth to be concave, being depreſſed in the middle, and higher on every ſide: and why in quickſilver, on the contrary, not only the ſurface is wont to be very convex, or ſwelling, in the middle; but if you dip the end of a ſlender pipe in it, the ſurface of the liquor (as it is called) will be lower within the pipe, than without. Which phænomena, whether, and how far, they may be deduced from the figure of the mercurial corpuſcles, and the ſhape of the ſpringy particles of the air, I willingly leave to be conſidered.

EXPERIMENT XXXVI.

SEVERAL ways we have met with propoſed, partly by the excellent *Galileo*, and partly by other ingenious writers, to manifeſt, that the air is not devoid of weight; ſome of theſe require the previous abſence of the air to be weighed; and others, the violent condenſation of it. But if we could lift a pair of ſcales above the atmoſphere, or place them in a vacuum, we might there weigh a parcel of air itſelf,

ſelf, as here we do other bodies in the air, becauſe it would there be heavier than that which ſurrounds it, as are groſſer bodies we commonly weigh, than the medium or ambient air. Wherefore, though we have above declined to affirm, that our receiver, when emptied, deſerves the name of a true vacuum; and though we cannot yet perfectly free it from air itſelf; yet we thought fit to try how far the air would manifeſt its gravity in ſo thin a medium, as we could make in our receiver, by evacuating it. We cauſed then to be blown at the flame of a lamp a glaſs bubble of about the bigneſs of a ſmall hen-heg, and of an oval form, ſave that at one end there was drawn out an exceeding ſlender pipe, that the bubble might be ſealed up, with as little rarefaction as might be of the air included in the great or oval cavity of it. This glaſs being ſealed, was faſtened to one of the ſcales of the exact pair of balances formerly mentioned; and being counterpoiſed with a weight of lead, was conveyed into the receiver, and cloſed up in it. The beam appearing to continue horizontal, the pump was ſet on work; and there ſcarce paſt above two or three exſuctions of the air, before the balance loſt its æquilibrium, and began to incline to that ſide on which the bubble was; which, as the air was farther and farther drawn out, did manifeſtly more and more preponderate, till he that pumped began to grow weary of his employment: after which the air being leiſurely let in again, the ſcales by degrees returned to their former æquilibrium. After that we took them out, and caſting into that ſcale to which the lead belonged three quarters of a grain, we conveyed the balance into the receiver, which being cloſed up, and exhauſted as before, we obſerved, that as the air was drawn out more and more, ſo the glaſs bubble came nearer and nearer to an æquilibrium with the other weight, till at length the beam was drawn to hang horizontal; which (as we had found by another trial) we could not bring it to do, when a quarter of a grain more was added to the ſcale, to which the lead belonged: though it ſeemed queſtionleſs, that if we could have perfectly emptied the receiver of the contained air, that included in the bubble would have weighed above a grain, notwithſtanding its having been probably ſomewhat rarefied by the flame, by the help of which the bubble was ſealed up. Let us add, that on the regreſs of the excluded air, the lead, and the weight caſt into the ſame ſcale, did again very much preponderate.

WE likewiſe conveyed into the receiver the ſame bubble, opened at the end of the ſlender pipe above mentioned; but having drawn out the air, after the accuſtomed manner, we found not, as before, the bubble to outweigh the oppoſite lead: ſo that by the help of our engine we can weigh the air, as we weigh other bodies, in its natural or ordinary conſiſtence, without at all condenſing it: nay, which is remarkable, having conveyed a lamb's bladder about half full of air into the receiver, we obſerved, that though upon the drawing out of the ambient air, the impriſoned air ſo expanded itſelf, as to diſtend the bladder ſo, as to ſeem ready to break it; yet this rarefied air did manifeſtly depreſs the ſcale, whereunto it was annexed.

ANOTHER thing we muſt not forget to mention, that happened to us, whilſt we were making trials concerning the weight of the air; namely, that having once cauſed the pump to be ſomewhat obſtinately plied, to diſcover the better what may be expected from the thinneſs of the medium in this experiment; the impriſoned air broke its brittle priſon, and throwing the greateſt part of it againſt the ſide of the receiver, daſhed it againſt that thick glaſs into a multitude of pieces. Which accident I mention, partly, that it may confirm what we delivered in our reflexions upon the firſt experiment, where we conſidered what would probably be done by the ſpring of the air impriſoned in ſuch glaſſes, in caſe the balancing preſſure of the ambient air were withdrawn; and partly, that we may thence diſcern of how cloſe a texture glaſs is, ſince ſo very thin a film of glaſs (if I may ſo call it) proved ſo impervious

to

to the air, that it could not get away through the pores, but was forced to break the glaſs in pieces to free itſelf; and this, notwithſtanding the time and advantage it had to try to get out at the pores. And this I mention, that neither our experiments, nor thoſe of divers learned men, might receive any prejudice from an experiment, which I happened to make divers years ago, and which, having been ſo much taken notice of by curious men, may be drawn to countenance their erroneous opinion, who would fain perſuade us, that glaſs is penetrable by air properly ſo called. Our experiment was briefly this: We were diſtilling a certain ſubſtance, that much abounded with ſubtile ſpirits and volatile ſalt, in a ſtrong earthen veſſel of an unuſual ſhape, to which was luted a large receiver, made of the coarſer ſort of glaſs, (which the tradeſmen are wont to call green glaſs;) but in our abſence, the fire, though it were to be very ſtrong, was, by the negligence or miſtake of thoſe we appointed to attend it, ſo exceſſively increaſed, that when we came back to the furnace, we found the ſpirituous and ſaline corpuſcles poured out (if I may ſo call it) ſo hot, and ſo copiouſly into the receiver, that they made it all opacous, and more likely to fly in pieces, than fit to be touched. Yet, being curious to obſerve the effects of a diſtillation, proſecuted with ſo intenſe and unuſual a degree of heat, we ventured to come near; and obſerved, among other things, that on the outſide of the receiver, at a great diſtance from the juncture, there was ſettled a round whitiſh ſpot or two, which at firſt we thought might be ſome ſtain upon the glaſs; but after, finding it to be in divers qualities like the oil and ſalt of the concrete we were diſtilling, we began to ſuſpect, that the moſt ſubtile and fugitive parts of the impetuouſly aſcending ſteams had penetrated the ſubſtance (as they ſpeak) of the glaſs, and by the cold of the ambient air were condenſed on the ſurface of it. And though we were very backward to credit this ſuſpicion, and therefore called in an ingenious perſon or two, both to aſſiſt us in the obſervation, and have witneſs of its event, we continued a while longer to watch the eſcape of ſuch unctuous fumes; and upon the whole matter unanimouſly concluded, that (all things conſidered) the ſubtile parts of the diſtilled matter being violently agitated by the exceſſive heat, had paſſed through the pores of the glaſs, widened by the ſame heat. But this having never happened but once in any of the diſtillations we have either made or ſeen, though theſe be not a few, it is much more reaſonable to ſuppoſe, that the perviouſneſs of our receiver to a body much more ſubtile than air proceeded partly from the looſer texture of that particular parcel of glaſs the receiver was made of (for experience hath taught us, that all glaſs is not of the ſame compactneſs and ſolidity) and partly from the enormous heat, which, together with the vehement agitation of the penetrant ſpirits, opened the pores of the glaſs; than to imagine that ſuch a ſubſtance as air ſhould be able to permeate the body of glaſs, contrary to the teſtimony of a thouſand chymical and mechanical experiments, and of many of thoſe made in our engine, eſpecially that newly recited: nay, by our fifth experiment it appears, that a thin bladder will not at its pores give paſſage even to rarefied air. And on this occaſion we will annex an experiment, which hath made ſome of thoſe we have acquainted with it doubt, whether the corpuſcles of the air be not leſs ſubtile than thoſe of water.

BUT without examining here the reaſonableneſs of that doubt, we will proceed to recite the experiment itſelf, which ſeems to teach, that though air, when ſufficiently compreſſed, may perchance get entrance into narrower holes and crannies than water; yet unleſs the air be forced in at ſuch very little holes, it will not get in at them, though they may be big enough to let water paſs through them.

THE experiment then was this: I took a fair glaſs ſiphon, the lower end of whoſe longeſt leg was drawn by degrees to ſuch a ſlenderneſs, that the orifice, at which the

water was to fall out, would hardly admit a very ſmall pin. This ſiphon being inverted, the matter was ſo ordered, that a little bubble of air was intercepted in the ſlendereſt part of the ſiphon, betwixt the little hole newly mentioned and the incumbent water; upon which it came to paſs, that the air being not to be forced through ſo narrow a paſſage, by ſo light a cylinder of water (though amounting to the length of divers inches) as leaned upon it, hindered the farther efflux of the water, as long as I pleaſed to let it ſtay in that narrow place: whereas, when by blowing a little at the wider end of the ſiphon, that little parcel of air was forced out with ſome water, the remaining water, that before continued ſuſpended, began freely to drop down again as formerly. And if you take a glaſs pipe, whether it be in the form of a ſiphon, or no, that being for the moſt part of the thickneſs of a man's finger, is yet towards one end ſo ſlender, as to terminate in a hole almoſt as ſmall as a horſe-hair; and if you fill this pipe with water, you will find that liquor to drop down freely enough thorough the ſlender extreme: but if you then invert the pipe, you will find, that the air will not eaſily get in at the ſame hole, through which the water paſſed. For in the ſharp end of the pipe, ſome inches of water will remain ſuſpended, which, it is probable, would not happen, if the air could get in to ſucceed it; ſince if the hole were a little wider, the water would immediately ſubſide. And though it be true, that if the pipe be of the length of many inches, a great part of the water will run down at the wider orifice; yet that ſeems to happen for ſome other reaſon, than becauſe the air ſucceeds it at the upper and narrow orifice, ſince all the ſlender part of the pipe, and perhaps ſome inches more, will continue full of water.

AND on this occaſion I remember, that whereas it appears by our fifth experiment, that the aërial corpuſcles (except perhaps ſome, that are extraordinary fine) will not paſs thorough the pores of a lamb's bladder, yet particles of water will, as we have long ſince obſerved, and as may be eaſily tried, by very cloſely tying a little alcalizate ſalt (we uſed the calx of tartar made with nitre) in a fine bladder, and dipping the lower end of the bladder in water: for if you hold it there for a competent while, you will find, that there will ſtrain thorough the pores of the bladder water enough to diſſolve the ſalt into a liquor.

BUT I ſee I am ſlipped into a digreſſion; wherefore I will not examine, whether the experiment I have related proceeded from hence, that the ſpringy texture of the corpuſcles of the air makes them leſs apt to yield and accommodate themſelves eaſily to the narrow pores of bodies than the more flexible particles of water; or whether it may more probably be aſcribed to ſome other cauſe. Nor will I ſtay to conſider, how far we may hence be aſſiſted to gueſs at the cauſe of the aſcenſion of water in the ſlender pipes and ſiphons formerly mentioned; but will return to our bubble, and take notice, that we thought fit alſo to endeavour to meaſure the capacity of the bubble we had made uſe of, by filling it with water, that we might the better know how much water anſwered in weight to $\frac{1}{4}$ of a grain of air. But notwithſtanding all the diligence, that was uſed to preſerve ſo brittle a veſſel, it broke before we could perfect that we were about, and we were not then provided of another bubble fit for our turn.

THE haſte I was in, my Lord, when I ſent away the laſt ſheet, made me forget to take notice to you of a problem, that occurred to my thoughts, upon the occaſion of the ſlow breaking of the glaſs bubble in our evacuated receiver. For it may ſeem ſtrange, ſince by our ſixth experiment it appears, that the air, when permitted, will by its own internal ſpring expand itſelf twice as much as *Merſennus* was able to expand it, by the heat even of a candent æolipile; yet the elater of the air was ſcarce able to break a very thin glaſs bubble, and utterly unable to break one ſomewhat thicker,

thicker, within whose cavity it was imprisoned. Whereas air penned up and agitated by heat is able to perform so much more considerable effects, that (not to mention those of rarefaction, that are more obvious) the learned Jesuit *Cabæus* (he that writ of the load-stone) relates, that he saw a marble pillar (so vast, that three men together with displayed arms could not embrace it, and that 1000 yoke of oxen, drawing it several ways with all their strength, could not have torn it asunder) quite broken off in the midst, by reason of some wood, which happening to be burnt just by the pillar, the heat proceeding from the neighbouring fire so rarefied some air or spirituous matter, which was shut up in the cavities of the marble, that it broke through the solid body of the stone to obtain room to expand itself.

P. Nicbol. Cab. lib. 4. Meteor. Aristot.

I REMEMBER I have taken notice, that probably the reason, why the included air did not break the hermetically sealed bubbles, that remained intire in our emptied receiver, was, that the air, being something rarefied by the flame employed to close the glass, its spring, upon the recess of the heat, grew weaker than before. But though we reject not that guess, yet it will not in the present case serve the turn; because that much smaller glass bubbles exactly closed will, by the included air (though agitated by the heat of a very moderate fire) be made to fly in pieces. Whether we may be assisted to solve this problem, by considering, that the heat doth from within vehemently agitate the corpuscles of the air, and add its assistance to the spring they had before, I shall not now examine; since I here but propose a problem, and that chiefly, that by this memorable story of *Cabæus* notice may be taken of the prodigious power of rarefaction, which hereby appears capable of performing stranger things, than any of our experiments have hitherto ascribed to it.

WE should hence, my Lord, immediately proceed to the next experiment, but that we think it fit, on this occasion, to acquaint you with what some former trials (though not made in our engine) have taught us, concerning what we would have discovered by the newly mentioned bubble that broke. And this the rather, because (a great part of this letter supposing the gravity of the air) it will not be impertinent to determine more particularly than hitherto we have done, what gravity we ascribe to it.

WE took then an æolipile made of copper, weighing six ounces, five drachms, and eight and forty grains: this being made as hot as we durst make it, (for fear of melting the metal, or at least the soder) was removed from the fire, and immediately stopped with hard wax, that no air at all might get in at the little hole, wont to be left in æolipiles for the fumes to issue out at: then the æolipile being suffered leisurely to cool, was again weighed together with the wax that stopt it, and was found to weigh (by reason of the additional weight of the wax) six ounces, six drachms, and 39 grains. Lastly, the wax being perforated without taking any of it out of the scale, the external air was suffered to rush in (which it did with some noise) and then the æolipile and wax, being again weighed, amounted to six ounces, six drachms, and 50 grains. So that the æolipile freed, as far as our fire could free it, from its air, weighed less than itself when replenished with air, full eleven grains: that is, the air containable within the cavity of the æolipile amounted to eleven grains and somewhat more; I say somewhat more, because of the particles of air, that were not driven by the fire out of the æolipile. And by the way (if there be no mistake in the observations of the diligent *Mersennus*) it may seem strange, that it should so much differ from 2 or 3 of ours; in none of which we could rarefy the air in our æolipile (though made red-hot almost all over, and so immediately plunged into cold water) to half that degree, which he mentions, namely to 70 times its natural extent, unless it were, that the æolipile he employed was able to sustain a more vehement heat

heat than ours (which yet we kept in so great an one, that once the soder melting, it fell asunder into the two hemispheres it consists of).

THE forementioned way of weighing the air by the help of an æolipile seems somewhat more exact than that, which *Mersennus* used, in that in ours the æolipile was not weighed till it was cold; whereas in his, being weighed red-hot, it is subject to lose of its substance in the cooling, for (as we have elsewhere noted on another occasion) copper heated red-hot is wont in the cooling to throw off little thin scales in such plenty, that having purposely watched a copper æolipile during its refrigeration, we have seen the place round about it almost covered with those little scales it had every way scattered: which, however they amount not to much, ought not to be overlooked, when it is so light a body as air that is to be weighed. We will not examine, whether the æolipile in cooling may not receive some little increment of weight, either from the vapid or saline steams, that wander up and down in the air; but we will rather mention, that (for the greater exactness) we employed to weigh our æolipile, both when filled only with air and when replenished with water, a pair of scales, that would turn (as they speak) with the fourth part of a grain.

As to the proportion of weight betwixt air and water, some learned men have attempted it by ways so unaccurate that they seem to have much mistaken it. For (not to mention the improbable accounts of *Kepler* and others) the learned and diligent *Ricciolus*, having purposely endeavoured to investigate this proportion by means of a thin bladder, estimates the weight of the air to that of the water to be as one to ten thousand, or thereabouts. And indeed I remember, that having formerly, on a certain occasion, weighed a large bladder full of air, and found it, when the air was all squeezed out, to have contained fourteen grains of air; I found the same bladder afterwards filled with water to contain very near 14 pound of that liquor: according to which account, the proportion of air to water was almost as a grain to a pound, that is, as 1 to above 7000. To this we may add, that on the other side, *Galileo* himself using another, (but an unaccurate way too) defined the air to be in weight to water but as one to 4 hundred. But the way formerly proposed of weighing the air by an æolipile seems by great odds more exact; and (as far as we could guess) seemed to agree well enough with the experiment made in our receiver. Wherefore it will be best to trust our æolipile in the inquiry we are about. And according to our observations, the water it contained amounting to one and twenty ounces and an half, and as much air as was requisite to fill it weighing eleven grains, the proportion in gravity of air to water of the same bulk will be as one to 938. And though we could not fill the æolipile with water so exactly as we would, yet in regard we could not neither as perfectly as we would drive the air out of it by heat, we think the proportion may well enough hold: but those, that are delighted with round numbers (as the phrase is) will not be much mistaken, if they reckon water to be near a thousand times heavier than air. And (for farther proof, that we have made the proportion betwixt these two bodies rather greater than lesser than indeed it is, and also to confirm our former observation of the weight of the air) we will add, that, having another time put some water into the æolipile before we set it on the fire, that the copious vapours of the rarefied liquor might the better drive out the air, we found, upon trial carefully made, that when the æolipile was refrigerated, and the included vapours were by the cold turned again into water (which could not have happened to the air, that the preceding steams expelled) the air, when it was let in, increased the weight of the æolipile as much as before, namely, eleven grains; though there were already in it twelve drachms and a half, besides a couple of grains of water, which remained of that we had formerly put into it to drive out the air.

Mersennus

Merſennus indeed tells us, that by his account air is in weight to water, as 1 to 1356. And adds, that we may, without any danger, believe, that the gravity of water to that of air of a like bulk is not leſs than of 1300 to 1. And conſequently, that the quantity of air to a quantity of water equiponderant thereto is as 1300 to 1. But why we ſhould relinquiſh our own carefully repeated trials, I ſee not. Yet I am unwilling to reject thoſe of ſo accurate and uſeful a writer: and therefore ſhall propoſe a way of reconciling our differing obſervations, by preſenting, that the diſcrepance between them may probably ariſe from the differing conſiſtence of the air at *London* and at *Paris*: for our air being more cold and moiſt than that which your Lordſhip now breathes, may be ſuppoſed alſo to be a fourth or fifth part more heavy. I leave it to be conſidered, whether it be of any moment, that our obſervations were made in the midſt of winter; whereas his were perhaps made in ſome warmer time of the year. But I think it were not amiſs, that, by the method formerly propoſed, the gravity of the air were obſerved both in ſeveral countries, and in the ſame country, in the ſeveral ſeaſons of the year and differing temperatures of the weather. And I would give ſomething of value to know the weight of ſuch an æolipile as ours full of air, in the midſt of winter in *Nova Zembla*; if that be true, which we formerly took notice of, namely, that the Hollanders, who wintered there, found that air ſo thick, that their clock would not go.

IF your Lordſhip ſhould now aſk me, if I could not by the help of theſe, and our other obſervations, decide the controverſies of our modern mathematicians about the height of the air or atmoſphere, by determining how high it doth indeed reach; I ſhould anſwer, that though it ſeems eaſy enough to ſhew, that divers famous and applauded writers have been miſtaken in aſſigning the height of the atmoſphere, yet it ſeems very difficult preciſely to define of what height it is. And becauſe we have hitherto but lightly touched upon a matter of ſuch importance, we preſume it will not be thought impertinent, upon this occaſion, to annex ſomething towards the elucidation of it.

WHAT we have already tried and newly ſet down allows us to take it for granted, that (at leaſt about *London*) the proportion of gravity betwixt water and air, of equal bulk, is as of a thouſand to one.

THE next thing therefore, that we are to inquire after, in order to our preſent deſign, is the difference in weight betwixt water and quickſilver: and though this hath been defined already by the illuſtrious *Verulam*, and ſome other inquiſitive perſons, that have compared the weight of ſeveral bodies, and caſt their obſervations into tables, yet we ſhall not ſcruple to annex our own trials about it; partly, becauſe we find authors conſiderably to diſagree; partly, becauſe we uſed exacter ſcales, and a ſomewhat more wary method than others ſeem to have done; and partly alſo, becauſe, having proſecuted our inquiry by two or three ſeveral ways, the ſmall difference between the events may aſſure us, that we were not much miſtaken.

WE took then a glaſs pipe, of the form of an inverted ſiphon, whoſe ſhape is delineated in the ſixteenth figure; and pouring into it a quantity of quickſilver, we held it ſo, that the ſuperficies of the liquor, both in the longer and ſhorter leg, lay in a horizontal line, denoted in the ſcheme by the pricked line E F: then pouring water into the longer leg of the ſiphon, till that was almoſt filled, we obſerved the ſurface of the quickſilver in that leg to be, by the weight of the water, depreſſed, as from E to B; and in the ſhorter leg, to be as much impelled upwards as from F to C. Whereupon having formerly ſtuck marks, as well at the point B, as at the oppoſite point D, we meaſured both the diſtance D C to have the height of the cylinder of quickſilver, which was raiſed above the point D (level with the ſurface of the

the quickſilver in the other leg) by the weight of the water, and the diſtance B A, which gave us the height of the cylinder of water. So that the diſtance D C amounting to $2\frac{13}{16}$ inches, and the height of the water amounting to $30\frac{43}{16}$ inches; and the whole numbers on both ſides, with the annexed fractions, being reduced to improper fractions of the ſame denomination, the proportion appeared to be (the denominators being left out as equal on both ſides) as 121 to 1665; or by reduction, as one to $\frac{91}{121}$.

BESIDES this unuſual way of determining the gravity of ſome things, we meaſured the proportion betwixt quickſilver and water, by the help of ſo exact a balance, as loſeth its æquilibrium by the hundredth part of a grain. But becauſe there is wont to be committed an overſight in weighing quickſilver and water, eſpecially if the orifice of the veſſel wherein they are put be any thing wide, in regard that men heed not that the ſurface of water in veſſels will be concave, but that of quickſilver notably convex or protuberant; to avoid this uſual overſight (I ſay) we made uſe of a glaſs bubble, blown very thin at the flame of a lamp, that it might not be too heavy for the balance, and terminating in a very ſlender neck wherein the concavity or convexity of a liquor could not be conſiderable; this glaſs, weighing $23\frac{4}{5}$ grains, we filled almoſt with quickſilver, and faſtening a mark over againſt the middle of the protuberant ſuperficies as near as our eyes could judge, we found that the quickſilver alone weighed $299\frac{7}{15}$ grains; then the quickſilver being poured out, and the ſame glaſs being filled as full of common water, we found the liquor to weigh $21\frac{7}{5}$ grains. Whereby it appeared the weight of water to quickſilver, is as one to $13\frac{19}{15}$; though our illuſtrious *Verulam* (queſtionleſs not for want of judgment or care, but of exact inſtruments) makes the proportion betwixt thoſe two liquors to be greater than of 1 to 17. And to add, that upon the bye, ſince quickſilver and well rectified ſpirit of wine, are (how juſtly I ſay not) accounted, the one the heavieſt, and the other the lighteſt of liquors; we thought to fill the ſame glaſs, and with the ſame ſcales to obſerve the difference betwixt them, which we found to be as of 1 to $16\frac{641}{1014}$; whereby it appeared, that the difference betwixt ſpirit of wine, that may be made to burn all away (ſuch as was ours) and common water, is as betwixt 1 and $1\frac{44}{171}$.

WE might here take occaſion to admire, that though water (as appeared by the experiment formerly mentioned of the pewter veſſel) ſeems not capable of any conſiderable condenſation, and ſeems not to have interſperſed into it any ſtore of air; yet quickſilver, of no greater bulk than water, ſhould weigh near fourteen times as much. But having only pointed at this as a thing worthy of conſideration, we will proceed in our inquiry after the height of the atmoſphere: and to avoid the trouble of fractions, we will aſſume, that quickſilver is fourteen times as heavy as water, ſince it wants ſo little of being ſo.

WHEREFORE, having now given us the proportion of air to water, and water to quickſilver, it will be very eaſy to find the proportion betwixt air and quickſilver, in caſe we will ſuppoſe the atmoſphere to be uniformly of ſuch a conſiſtence as the air we weighed here below. For ſince our engine hath ſufficiently manifeſted that it is the æquilibrium with the external air, that in the Torricellian experiment keeps the quickſilver from ſubſiding; and ſince, by our accurate experiment formerly mentioned, it appears that a cylinder of mercury, able to balance a cylinder of the whole atmoſphere, amounted to near about thirty inches; and ſince, conſequently we may aſſume the proportion of quickſilver to air to be as fourteen thouſand to one; it will follow, that a cylinder of air, capable to maintain an æquilibrium, with a mercurial cylinder of two feet and a half in height, muſt amount to 35,000 feet of our Engliſh meaſure;

measure; and consequently (reckoning five feet to a geometrical pace, and one thousand such paces to a mile) to seven full miles.

BUT this (as we lately intimated) proceeds upon the supposition, that the air is every where of the same consistence that we found it near the surface of the earth; but that cannot with any safety be concluded, not only for the reason I find to have been taken notice of by the ancients, and thus expressed in *Seneca: Omnis aër* (he says) *quo propior est terris, hoc crassior; quemadmodum in aqua & in omni humore fæx ima est, ita in aëre spississima quæque desidunt.* But much more, because the springy texture of the aërial corpuscles makes them capable of a very great compression, which the weight of the incumbent part of the atmosphere is very sufficient to give those that be undermost and near the surface of the earth. And if we recal to mind those former experiments, whereby we have manifested that air, much rarefied without heat, may easily admit a farther rarefaction from heat, and that the air, even without being expanded by heat, is capable of being rarefied to above one hundred and fifty times the extent it usually possesseth here below; how can it be demonstrated that the atmosphere may not, for aught we know, or at least for aught can be determined by our statical and mechanical experiments, rise to the height of five and twenty German leagues, if not of some hundred of common miles?

Senec. Nat. quæst. Lib. 4. cap. 10.

AND this conjecture it self may appear very injurious to the height whereunto exhalations may ascend, if we will allow that there was no mistake in that strange observation made at *Tolouse* in a clear night in *August*, by the diligent Mathematician *Emanuel Magnan*, and thus recorded by *Ricciolus*, for I have not at hand the author's own book: *Vidit* (says he) *ab hora undecima post meridiem usque ad mediam noctem lunâ infra horizontem positâ, nubeculam quandam lucidam prope meridianum fere usque ad Zenith diffusam, quæ consideratis omnibus non poterat nisi à sole illuminari; ideoque altior esse debuit tota umbra terræ. Addit* (continues *Ricciolus*) *simile quid evenisse Michaeli Angelo Riccio apud Sabinos versanti, nempe viro in mathesi eruditissimo.*

Ricciol. Almag. Nov. Tom. 2. lib. 10. sect. 6. prop. 50. Ex Magnan. lib. 1. Perspectivæ horariæ prop. 38.

VARIOUS observations made at the feet, tops, and interjacent parts of high mountains, might perchance somewhat assist us to make an estimate in what proportion, if in any certain one, the higher air is thicker than the lower, and guess at the difform consistence, as to laxity and compactness of the air, at several distances from us. And if the difficulties about the refractions of the celestial lights were satisfactorily determined, that might also much conduce to the placing due limits to the atmosphere (whose dimensions those observations about refractions seem hitherto much to contract). But for the present we dare not pronounce any thing peremptorily concerning the height of it, but leave it to farther enquiry: contenting ourselves to have manifested the mistakes of divers eminent modern writers, who will not allow the atmosphere to exceed above two or three miles in height (as the famous *Kepler* will not the *Aër refractivus*) and to have rendered a reason why, in the mention we made in the notes upon the first experiment touching the height of the atmosphere, we scrupled not to speak of it, as if it might be many miles high.

EXPERIMENT XXXVII.

WE will now proceed to recite a phænomenon, which, though made amongst the first, we thought fit not to mention till after many others, that we might have the opportunity to observe as many circumstances of it as we could, and so present your Lordship at once, most of what we at several times have taken notice of concerning so odd a phænomenon.

OUR engine had not been long finished, when, at the first leisure we could steal from our occasions to make trial of it, we caused the air to be pumped out of the receiver; and whilst I was busied in entertaining a learned friend that just then came to visit me, an ingenious bystander thought he perceived some new kind of light in the receiver, of which giving me hastily notice, my friend and I presently observed, that when the sucker was drawn down, immediately upon the turning of the key, there appeared a kind of light in the receiver, almost like a faint flash of lightning in the day-time, and almost as suddenly did it appear and vanish. Having, not without some amazement, observed divers of these apparitions of light, we took notice that the day was clear, the hour about ten in the morning, that the only window in the room faced the north; and also, that by interposing a cloke, or any opacous body between the receiver and the window, though the rest of the room were sufficiently enlightened, yet the flashes did not appear as before, unless the opacous body were removed. But not being able on all these circumstances to ground any firm conjecture at the cause of this surprizing phænomenon, as soon as night was come, we made the room very dark; and plying the pump, as in the morning, we could not, though we often tried, find, upon the turning of the key, so much as the least glimmering of light; whence we inferred, that the flash appearing in the receiver, did not proceed from any new light generated there, but from some reflections of the light of the sun, or other luminous bodies placed without it; though whence the reflection should proceed, it posed us to conjecture.

WHEREFORE the next morning, hoping to inform ourselves better, we went about to repeat the experiment; but though we could as well as formerly exhaust the receiver, though the place wherein we made the trial was the very same, and though other circumstances were resembling; yet we could not discover the least appearance of light all that day, nor on divers others, on which trial was again fruitlesly made; nor can we to this very time be sure a day before-hand that these flashes will be to be seen in our great receiver. Nay, having once found the engine in a good humour (if I may so speak) to shew this trick, and sent notice of it to our learned friend Dr. *Wallis*, who expressed a great desire to see this phænomenon, though he were not then above a bow-shoot off, and made haste to satisfy his curiosity; yet by that time he was come, the thing he came for was no longer to be seen: so that having vainly endeavoured to exhibit again the phænomenon in his presence, I began to apprehend what he might think of me, when unexpectedly the engine presented us a flash, and after that a second, and as many more as sufficed to satisfy him that we might very well confidently relate, that we have ourselves seen this phænomenon, though not confidently promise to shew it others.

AND this unsuccessfulness whereto our experiment is liable, being such, that by all our watchfulness and trials we could never reduce it to any certain rules or observations, since in all constitutions of the weather, times of the day, *&c.* it will sometimes answer, and sometimes disappoint our expectations; we are much discouraged from venturing to frame an hypothesis to give an account of it: which if the experiment did constantly succeed, might the more hopefully be attempted, by the help of the following phænomena laid together; some of them produced upon trials purposely made to examine the validity of the conjectures other trials had suggested.

FIRST then we observed, that the apparition of light may be made as well by candle-light, as by day-light; and in whatever position the candle be held, in reference to the receiver, as on this or that hand of it, above it, beneath it, or any other way, provided the beams of light be not hindered from falling upon the vessel.

NEXT,

NEXT, we noted that the flash appears immediately upon the turning of the key, to let the air out of the receiver into the emptied cylinder, insomuch that I remember not that when at any time in our great receiver, the stop-cock was opened before the cylinder was exhausted (whereby it came to pass that the air did rather descend, than rush into the cylinder) the often mentioned flash appeared to our eyes.

YET, we farther observed, that when instead of the great receiver we made use of a small glass, not containing above a pound and a half of water, the phænomenon might be exhibited though the stop-cock were open, provided the sucker were drawn nimbly down.

WE noted too, that when we began to empty the receiver, the appearances of light were much more conspicuous than towards the latter end, when little air at a time could pass out of the receiver.

WE observed also, that when the sucker had not been long before well oiled, and, instead of the great receiver, the smaller vessel above mentioned was emptied; we observed, I say, that then, upon the opening of the stop-cock, as the air descended out of the glass into the emptied cylinder, so at the same time there ascended out of the cylinder into the vessel a certain steam, which seemed to consist of very little bubbles, or other minute corpuscles thrown up from the oil, rarefied by the attrition it suffered in the cylinder. For at the same time that these steams ascended into the glass, some of the same kind manifestly issued out like a little pillar of smoke at the orifice of the valve, when that was occasionally opened. And these steams frequently enough presenting themselves to our view, we found, by exposing the glass to a clear light, that they were wont to play up and down in it, and so by their whitishness to emulate, in some measure, the apparition of light.

FOR we likewise sometimes found, by watchful observation, that when the flash was great, not only at the very instant the receiver lost of its transparency, by appearing full of some kind of whitish substance; but that for some short time after, the sides of the glass continued somewhat opacous, and seemed to be darkened, as if some whitish steam adhered to the inside of them.

HE that would render a reason of the phænomenon, whereof all these are not all the circumstances, must do two things; whereof the one is difficult, and the other little less than impossible: for he must give an account not only whence the appearing whiteness proceeds, but wherefore that whiteness doth sometimes appear, and sometimes not.

FOR our part, we freely confess ourselves at a loss about rendering a reason of the less difficult part of the problem; and though your Lordship should even press us to declare what conjecture it was, that the above-recited circumstance suggested to us, we should propose the thoughts we then had, no otherwise than as bare conjectures.

IN case then our phænomenon had constantly and uniformly appeared, we should have suspected it to have been produced after some such manner as follows:

FIRST, we observed that, though that which we saw in our receiver seemed to be some kind of light, yet it was indeed but a whiteness which did (as hath already been noted) opacate (as some speak) the inside of the glass.

NEXT, we considered, that our common air abounds with particles, or little bodies, capable to reflect the beams of light. Of this we might easily give divers proofs, but we shall name but two: the one, that vulgar observation of the motes that appear in multitudes swimming up and down in the air, when the sun-beams shooting into a room, or any other shady place, discover them, though otherwise the eye cannot distinguish them from the rest of the air; the other proof we will

take from what we (and no doubt very many others) have obſerved, touching the illumination of the air in the night. And we particularly remember, that, being at ſome diſtance from *London* one night, that the people, upon a very welcome occaſion, teſtified their joy by numerous bonfires; though, by reaſon of the interpoſition of the houſes, we could not ſee the fires themſelves, yet we could plainly ſee the air all enlightened over and near the city; which argued, that the lucid beams ſhot upwards from the fires, met in the air with corpuſcles opacous enough to reflect them to our eyes.

A THIRD thing that we conſidered, was, that white may be produced (without excluding other ways, or denying inviſible pores in the ſolideſt bodies) when the continuity of a diaphanous body happens to be interrupted by a great number of ſurfaces, which, like ſo many little looking-glaſſes, do confuſedly repreſent a multitude of little and ſeemingly contiguous images of the lucid body. We ſhall not inſiſt on the explanation of this, but refer you for it to what we have ſaid in another paper (touching colours). But the inſtances that ſeem to prove it are obvious: for water or whites of eggs beaten to froth, do loſe their tranſparency, and appear white. And having out of one of our leſſer receivers carefully drawn out the air, and ſo ordered it, that the hole by which the water was to get in, was exceeding ſmall, that the liquor might be the more broken in its paſſage thorough it, we obſerved with pleaſure, that the neck being held under water, and the little hole newly mentioned being opened, the water that ruſhed in was ſo broken, and acquired ſuch a multitude of new ſurfaces, that the receiver ſeemed to be full rather of milk than water. We have likewiſe found out, that by heating a lump of cryſtal to a certain degree, and quenching it in fair water, it would be diſcontinued by ſuch a multitude of cracks (which created new ſurfaces within it) that though it would not fall aſunder, but retain its former ſhape, yet it would loſe its tranſparency, and appear white.

UPON theſe conſiderations, my Lord, and ſome others, it ſeemed not abſurd to imagine, that upon the ruſhing of the air out of the receiver into the emptied cylinder, the air in the receiver being ſuddenly and vehemently expanded, the texture of it was as ſuddenly altered, and the parts made ſo to ſhift places (and perhaps ſome of them to change poſtures) as during their new and vehement motion, and their varied ſituation, to diſturb the wonted continuity, and ſo the diaphaneity of the air; which (as we have already noted) upon its ceaſing to be a tranſparent body, without the interpoſition of coloured things, muſt eaſily degenerate into white.

SEVERAL things there were that made this conjecture ſeem the leſs improbable. As firſt, that the whiteneſs always appeared greater when the exſuction began to be made, whilſt there was ſtore of air in the receiver, than when the air was in great part drawn out. And next, that, having exhauſted the receiver, and applied to the hole in the ſtop-cock a large bubble of clear glaſs, in ſuch a manner, that we could at pleaſure let the air paſs out at the ſmall glaſs into the great one, and eaſily fill the ſmall one with air again, we obſerved with pleaſure, that, upon the opening the paſſage betwixt the two glaſſes, the air in the ſmaller having ſo much room in the greater to receive it, the diſſilition of that air was ſo great, that the ſmall phial ſeemed to be full of milk; and this experiment we repeated ſeveral times. To which we may add, that, having provided a ſmall receiver, whoſe upper orifice was ſo narrow that I could ſtop it with my thumb, I obſerved, that when, upon the exſuction of the air, the capacity of the glaſs appeared white, if, by a ſudden removal of my thumb, I let in the outward air, that whiteneſs would immediately vaniſh. And whereas it may be objected, that in the inſtance formerly mentioned, water turning from perſpicuous to white, there intervenes the air, which is a body of a heterogeneous

neous nature, and must turn it into bubbles to make it lose its transparency; we may borrow an answer from an experiment we deliver in another treatise, where we teach, how to make two very volatile liquors, which being gently put together, are clear as rock-water, and yet will almost in a moment, without the subingression of air to turn them into bubbles, so alter the disposition of their insensible parts, as to become a white and consistent body. And this happens not as in the precipitation of benjamin, and some other resinous bodies, which being dissolved in spirit of wine, may, by the effusion of fair water, be turned into a seemingly milky substance. For this whiteness belongs not to the whole liquor, but to the corpuscles of the dissolved gum, which, after a while subsiding, leave the liquor transparent, themselves only remaining white; whereas in our case, it is from the varied texture of the whole formerly transparent fluid body, and not from this or that part, that this whiteness results: for the body is white throughout, and will long continue so; and yet may, in process of time, without any addition, be totally reduced into a transparent body as before.

BUT besides the conjecture insisted on all this while, we grounded another upon the following observation; which was, that having conveyed some smoke into our receiver placed against a window, we observed, that upon the exsuction of the air, the corpuscles that were swimming in it, did manifestly enough make the receiver seem more opacous at the very moment of the rushing out of the air: for considering that the whiteness, whose cause we inquire of, did but sometimes appear, it seemed not impossible but that at such times the air in the receiver might abound with particles, capable of reflecting the light in the manner requisite to exhibit a white colour, by their being put into a certain unusual motion: as may be in some measure illustrated by this, that the new motion of the freshly mentioned fumes made the inside of the receiver appear somewhat darker than before; and partly by the nature of our formerly mentioned smoking liquor, whose parts, though they seemed transparent whilst they composed a liquor, yet when the same corpuscles, upon the unstopping of the glass, were put into a new motion, and disposed after a new manner, they did opacate that part of the air they moved in, and exhibited a greater whiteness than that which sometimes appears in our pneumatical vessel. Nor should we content ourselves with this single instance, to manifest, that little bodies, which being ranged after one manner are diaphanous and colourless, may, by being barely agitated, dispersed, and consequently otherways ranged, exhibit a colour, if we were not unwilling to rob our collection of experiments concerning colours.

BUT, my Lord, I foresee you may make some objections against our proposed guess, which perhaps I shall scarce be able to answer; especially, if you insist upon having me render a reason why our phænomenon appears not constantly.

I MIGHT indeed answer, that probably it would do so, if instead of our great receiver we use such a small phial as we have lately mentioned, wherein the dissolution of the air being much greater, is like to be the more conspicuous: since I remember not that we ever made our trial with such small vessels, without finding the expected whiteness to appear. But it would remain to be explicated, why in our great receiver the phænomenon should sometimes be seen, and oftentimes not appear. And though that conjecture which we last made should not be rejected, yet if were farther pressed to assign a reason why the air should abound with such particles, as we there suppose, more at one time than another, we are not yet provided of any better answer than this general one, that the air about us (and much more that within the receiver) may be much altered by such causes as few are aware of: for, not to repeat those probable arguments of this assertion which we have occasionally

occasionally mentioned here and there in the former part of this epistle, we will here set down two or three instances to verify the same proposition. First, I find that the learned *Josephus Acosta*, among other judicious observations he made in *America*, hath this concerning the effects of some winds: *There are*, saith he, *winds which naturally trouble the water of the sea, and make it green, and black; others, clear as crystal.* Next, we have observed, that though we conveyed into the receiver our scales, and the pendula formerly mentioned, clean and bright; yet after the receiver had been emptied, and the air let in again, the gloss or lustre both of the one, and of the other, appeared tarnished by a beginning rust. And in the last place, we will subjoin an observation we made some years ago, which hath been heard of by divers ingenious men, and seen by some of them: we had, with pure spirit of wine, drawn a tincture out of a certain concrete, which useth to be reckoned among mineral bodies; and this tincture being very pure and transparent, we did, because we put a great value upon it, put into a crystal phial, which we carefully stopped, and locked up in a press among some other things that we specially prized. This liquor being a chymical rarity, and besides very defecate, and of a pleasing golden colour; we had often occasion to look upon it, and so to take notice, that one time it seemed to be very much troubled, and not clear as it was wont to be: whereupon we imagined, that though it would be something strange, yet it was not impossible that some precipitation of the mineral corpuscles was then happening, and that thence the liquor was opacated. But, finding after some days, that though the expected precipitation had not been made, yet the liquor, retaining its former vivid colour, was grown clear again as before, we somewhat wondered at it; and locking it up again in the same press, we resolved to observe, both whether the like changes would again appear in our tincture, and whether in case they should appear, they would be ascribable to the alterations of the weather. But though, during the greatest part of a winter and a spring, we took pleasure to observe, how the liquor would often grow turbid, and after a while clear again; yet we could not find that these mutations depended upon any that were manifest in the air, which would be often dark and clouded, when the tincture was clear and transparent; as, on the other side, in clear weather the liquor would appear sometimes troubled, and more opacous. So that being unable to give an account of these odd changes in our tincture (which we suppose we have not yet lost, though we know not whether it hath lost its fickle nature) either by those of the air, or any thing else that occurred to our thoughts; we could not but suspect, that there may be in divers bodies, as it were spontaneous mutations, that is, such changes as depend not upon manifest causes. But, my Lord, what hath beenall this while said concerning our phænomenon, is offered to you, not as containing a satisfactory account of it, but to assist you to give yourself one.

Joseph Acosta's Nat. and Mor. Hist. of the Indies, lib. 3. cap. 9.

EXPERIMENT XXXVIII.

WE took a glass vessel, open at the top, and into it we put a mixture of snow and common salt, (such a mixture as we have in another treatise largely discoursed of;) and into the midst of this mixture we set a glass of a cylindrical form, closely stopped at the lower end with plaister, and open at the upper, at which we filled it with common water. These things being let down into the receiver, and the pump being set on work, the snow began to melt somewhat faster than we expected; whether upon the account of the exsuction of the air, or because there was but little of the snow, or whether for any other reason, it appeared doubtful. But however, by

by that time the receiver had been considerably exhausted, which was done in less than $\frac{1}{4}$ of an hour, we perceived the water near the bottom of the glass cylinder to freeze, and the ice, by a little longer stay, seemed to increase, and to rise somewhat higher than the surface of the surrounding liquor, whereinto almost all the snow and salt were resolved. The glass being taken out, it appeared that the ice was as thick as the inside of the glass it filled, though into that I could put my thumb. The upper surface of the ice was very concave, which, whether it were due to any unheeded accident, or to the exsuction of the air, we leave to be determined by farther trial. And lastly, the ice held against the light, appeared not destitute of bubbles, though some bystanders thought they were fewer than would have been found if the water had been frozen in the open air. The like experiment we tried also another time in one of our small receivers, with not unlike success.

AND on this occasion, my Lord, give me leave to propose a problem, which shall be this: whence proceeds that strange force that we may sometimes observe in frozen water, to break the bodies that imprison it, though hard and solid? That there is such a force in water exposed to congelation, may be gathered not only from what may be often observed in winter, of the bursting of glasses too close stopped, filled with water or aqueous liquors, but by instances as much more considerable as less obvious. For I remember, that an ingenious stone-cutter not long since complained to me, that sometimes, through the negligence of servants, the rain being suffered to soak into marble stones, the supervening violent frosts would burst the stones, to the possessor's no small damage. And I remember another tradesman, in whose house I had lodgings, was last winter complaining, that even implements made of bell-metal, being carelessly exposed to the wet, have been broken and spoiled by the water; which, having gotten into the little cavities and crannies of the metal, was there afterwards frozen and expanded into ice. And to these relations, we can add one of the formerly mentioned *Cabæus*'s, whereby they not only may be confirmed, but are surpassed: for he tells us, that he saw a huge vessel of exceeding hard marble split asunder by congealed water, whose rarefaction, saith our author, proved so vehement, that the hardness of the stone yielded to it; and so a vessel was broken, which would not have been so by 100 yoke of oxen drawing it several ways. I know, my Lord, that to solve this problem, it will be said, that congelation doth not (as is commonly, but erroneously presumed) reduce water into less room than it possessed before, but rather makes it take up more. And I have elsewhere proved by particular experiments, that whether or no ice may be truly said to be water rarefied (for that seems questionable) it may be said to take up more room than the water did before glaciation. But though we grant that freezing makes water swell, yet, how cold (which in weather-glasses manifestly condenseth the air) should expand either the water, or the intercepted air so forcibly, as to perform such things as we have newly related, will yet remain a problem.

In lib. de gl. Mirac.

EXPERIMENT XXXIX.

WE took an oval glass, clear and (lest it should break) pretty strong, with a short neck at the obtuser end; through this neck, we thrust almost to the bottom, a pipe of glass, which was closely cemented to the newly mentioned neck; the upper part of which pipe was drawn in some places more slender than a crow's quill, that the changes of the air in that glass egg might be the more conspicuous.

Then

Then there was conveyed into the glass five or six spoonfuls of water, part of which, by blowing air into the egg, was raised into the abovementioned slender part of the pipe, so that the water was interposed between the external air, and that included in the egg. This weather glass (delineated in the fourteenth figure) was so placed, and closed up in the cavity of one of our small receivers, that only the slender part of the pipe, to the height of four or five inches, passing thorough a hole in the cover, remained exposed to the open air.

THE pump being set at work, upon the exsuction of the air, the water in the pipe descended about a quarter of an inch, and this upon two or three reiterated trials; which seemed sufficiently to argue, that there was no heat produced in the receiver upon the exsuction of the air: for even a little heat would probably have been discovered by that weather-glass, since upon the bare application of my hand to the outside of the receiver, the warmth having after some time been communicated or propagated through both the glasses, and the interval betwixt them, to the imprisoned air, did so rarefy that, as to enable it, by pressing upon the subjacent water, to impel that in the pipe very many times as far as it had fallen downwards upon the exsuction of the air.

YET shall not we conclude, that in the cavity of the receiver the cold was greater after the exsuction of the air than before.

FOR if it be demanded what then could cause the forementioned subsiding of the water? it may be answered, that probably it was the reaching of the glass egg, which, upon the exsuction of the ambient air, was unable to resist altogether as much as formerly the pressure of the included air, and of the atmosphere, which, by the intervention of the water, pressed upon its concave surface. Which seemed probable, as well by what was above delivered, in the experiment about the breaking of the glass by the force of the atmosphere, as by this notable circumstance (which we divers times observed) that when by drawing the air out of the receiver, the water in the pipe was subsided, upon the readmission of the external air, to press against the convex surface of the egg, the water was presently reimpelled to its former height: which would perhaps appear less strange to your Lordship, if you had yet seen, what we have heretofore taught in another treatise, concerning the spring that may be discovered in glass, as rigid and inflexible a body as it is generally esteemed. And in the mean while it may serve the turn, to cause a glass egg to be blown exceeding thin; and then, having broken it, try how far you can by degrees bend some narrow parts of it; and how readily, upon the removal of what kept it bent, it will restore itself to its former state or posture. But to return to our experiment: from thence it seems probable, either that there succeeds no body in the room of the air drawn out of our receiver; or that it is not every matter that is subtile enough readily to pass through the pores of glass, that is always agitated enough to produce heat wherever it is plentifully found. So that if no vacuum be to be admitted, this experiment seems to invite us to allow a great disparity, either as to bulk, or as to agitation, or as to both, betwixt some parts of the ethereal substance, and those that are wont here below to produce heat and fire.

WE tried also what operation the drawing out of the air would have upon camphire, that being a body, which, though not a liquor, consists of such volatile or fugitive parts, that without any greater agitation than that of the open air itself, they will copiously fly away. But we found not that even this loose body was sensibly altered by the exsuction of the ambient air.

EX-

EXPERIMENT XL.

IT may ſeem well worth trying, whether or no in our exhauſted glaſs the want of an ambient body, of the wonted thickneſs of air, would diſable even light and little animals, as bees, and other winged inſects, to fly. But though we eaſily foreſaw how difficult it would be to make ſuch an experiment, yet not to omit our endeavours, we procured a large fleſh-fly, which we conveyed into a ſmall receiver. We alſo another time ſhut into a great receiver a humming bee, that appeared ſtrong and lively, though we had rather have made the trial with a butterfly, if the cold ſeaſon would have permitted us to find any *. The fly, after ſome exſuctions of the air, dropped down from the ſide of the glaſs whereon ſhe was walking. But, that the experiment with the bee might be the more inſtructive, we conveyed in with her a bundle of flowers, which remained ſuſpended by a ſtring near the upper part of the receiver; and having provoked the bee, we excited her to fly up and down the capacity of the veſſel, till at length, as we deſired, ſhe lighted upon the flowers: whereupon we preſently began to draw out the air, and obſerved, that though for ſome time the bee ſeemed to take no notice of it, yet within a while after ſhe did not fly, but fall down from the flowers, without appearing to make any uſe of her wings to help herſelf. But whether this fall of the bee, and the other inſect, proceeded from the medium's being too thin for them to fly in, or barely from the weakneſs, and as it were ſwooning of the animals themſelves, you will eaſily gather from the following experiment.

EXPERIMENT XLI.

TO ſatisfy ourſelves in ſome meaſure about the account upon which reſpiration is ſo neceſſary to the animals that nature hath furniſhed with lungs, we took (being then unable to procure any other lively bird, ſmall enough to be put into the receiver) a lark, one of whoſe wings had been broken by a ſhot of a man that we had ſent to provide us ſome birds for our experiment; but notwithſtanding this hurt, the lark was very lively, and did, being put into the receiver, divers times ſpring up in it to a good height. The veſſel being haſtily, but carefully cloſed, the pump was diligently plied, and the bird for a while appeared lively enough; but upon a greater exſuction of the air, ſhe began manifeſtly to droop and appear ſick, and very ſoon after was taken with as violent and irregular convulſions, as are wont to be obſerved, in poultry, when their heads are wrung off: for the bird threw herſelf over and over two or three times, and died with her breaſt upward, her head downwards, and her neck awry. And though upon the appearing of theſe convulſions, we turned the ſtop-cock, and let in the air upon her, yet it came too late; whereupon caſting our eyes upon one of thoſe accurate dials that go with a pendulum, and were of late ingeniouſly invented by the noble and learned *Hugenius*, we found that the whole tragedy had been concluded within ten minutes of an hour, part of which time had been employed in cementing the cover to the receiver. Soon after we got a hen-

* Since the writing of this XLth experiment, we procured a white butterfly, and incloſed it one of our ſmaller receivers, where, though at firſt he fluttered up and down, yet preſently, upon the exſuction of the air, he fell down as in a ſwoon, retaining no other motion than ſome little trembling of the wings.

ſparrow, which being caught with bird-lime was not at all hurt; when we put her into the receiver, almoſt to the top of which ſhe would briſkly raiſe herſelf, the experiment being tried with this bird, as it was with the former, ſhe ſeemed to be dead within ſeven minutes, one of which were employed in cementing on the cover: but upon the ſpeedy turning of the key, the freſh air flowing in, began ſlowly to revive her, ſo that after ſome pantings ſhe opened her eyes, and regained her feet, and in about ¼ of an hour after, threatned to make an eſcape at the top of the glaſs, which had been unſtopped to let in the freſh air upon her: but the receiver being cloſed the ſecond time, ſhe was killed with violent convulſions within five minutes from the beginning of the pumping.

A WHILE after we put in a mouſe, newly taken, in ſuch a trap as had rather affrighted than hurt him; whilſt he was leaping up very high in the receiver, we faſtened the cover to it, expecting that an animal uſed to live in narrow holes with very little freſh air, would endure the want of it better than the lately mentioned birds: but though, for a while after the pump was ſet a work, he continued leaping up as before; yet, it was not long ere he began to appear ſick and giddy, and to ſtagger: after which he fell down as dead, but without ſuch violent convulſions as the bird died with. Whereupon, haſtily turning the key, we let in ſome freſh air upon him, by which he recovered, after a while, his ſenſes and his feet, but ſeemed to continue weak and ſick: but at length, growing able to ſkip as formerly, the pump was plied again for eight minutes, about the middle of which ſpace, if not before, a little air by a miſchance got in at the ſtop cock; and about two minutes after that, the mouſe divers times leaped up lively enough, though after about two minutes more he fell down quite dead, yet with convulſions far milder than thoſe wherewith the two birds expired. This alacrity ſo little before his death, and his not dying ſooner than at the end of the eighth minute, ſeemed aſcribable to the air (how little ſoever) that ſlipt into the receiver. For the firſt time, thoſe convulſions (that, if they had not been ſuddenly remedied, had immediately diſpatched him) ſeized on him in ſix minutes after the pump began to be ſet a work. Theſe experiments ſeemed the more ſtrange, in regard that during a great part of thoſe few minutes the engine could but inconſiderably rarefy the air (and that too, but by degrees) and at the end of them there remained in the receiver no inconſiderable quantity; as may appear by what we have formerly ſaid of our not being able to draw down water in a tube, within much leſs than a foot of the bottom: with which we likewiſe conſidered, that by the exſuction of the air and interſperſed vapours, there was left in the receiver a ſpace ſome hundreds of times exceeding the bigneſs of the animal, to receive the fuliginous ſteams, from which exſpiration diſcharges the lungs; and which, in the other caſes hitherto known, may be ſuſpected, for want of room, to ſtifle thoſe animals that are cloſely penned up in too narrow receptacles.

I FORGOT to mention, that having cauſed theſe three creatures to be opened, I could, in ſuch ſmall bodies, diſcover little of what we ſought for, and what we might poſſibly have found in larger animals; for though the lungs of the birds appeared very red, and as it were inflamed, yet that colour being uſual enough in the lungs of ſuch winged creatures, deſerves not ſo much our notice, as it doth, that in almoſt all the deſtructive experiments made in our engine, the animals appeared to die with violent convulſive motions: from which, whether phyſicians can gather any thing towards the diſcovery of the nature of convulſive diſtempers, I leave to them to conſid r.

HAVING proceeded thus far, though (as we have partly intimated already) there appeared not much cauſe to doubt, but that the death of the forementioned animals proceeded

proceeded rather from the want of air, than that the air was overclogged by the steams of their bodies, exquisitely penned up in the glass; yet I, that love not to believe any thing upon conjectures, when by a not over-difficult experiment I can try whether it be true or no, thought it the safest way to obviate objections, and remove scruples, by shutting up another mouse as close as I could in the receiver; wherein it lived about three quarters of an hour, and might probably have done so, much longer, had not a Virtuoso of quality, who in the mean while chanced to make me a visit, desired to see whether or no the mouse could be killed by the exsuction of the ambient air: whereupon we thought fit to open, for a little while, an intercourse betwixt the air in the receiver, and that without it, that the mouse might thereby (if it were needful for him) be refreshed; and yet we did this without uncementing the cover at the top, that it might not be objected, that perhaps the vessel was more closely stopped for the exsuction of the air than before.

THE experiment had this event, that after the mouse had lived ten minutes (which we ascribed to this, that the pump, for want of having been lately oiled, could move but slowly, and could not by him that managed it be made to work as nimbly as it was wont) at the end of that time he died with convulsive fits, wherein he made two or three bounds into the air, before he fell down dead.

NOR was I content with this, but for your Lordship's farther satisfaction, and my own, I caused a mouse, that was very hungry, to be shut in all night, with a bed of paper for him to rest upon: and to be sure that the receiver was well closed, I caused some air to be drawn out of it, whereby, perceiving that there was no sensible leak, I presently readmitted the air at the stop-cock, lest the want of it should harm the little animal; and then I caused the engine to be kept all night by the fire-side, to keep him from being destroyed by the immoderate cold of the frosty night. And this care succeeded so well, that the next morning I found that the mouse was not only alive, but had devoured a good part of the cheese that had been put in with him. And having thus kept him alive full twelve hours, or better, we did, by sucking out part of the air, bring him to droop, and to appear swelled; and by letting in the air again, we soon reduced him to his former livelinesS.

A Digression containing some Doubts touching RESPIRATION.

I FEAR your Lordship will now expect, that to these experiments I should add my reflections on them, and attempt, by their assistance, to resolve the difficulties that occur about respiration; since at the beginning I acknowledged a farther enquiry into the nature of that, to have been my design in the related trials. But I have yet, because of the inconvenient season of the year, made so few experiments, and have been so little satisfied by those I have been able to make, that they have hitherto made respiration appear to me rather a more, than a less mysterious thing, than it did before. But yet, since they have furnished me with some such new considerations, concerning the use of the air, as confirms me in my diffidence of the truth of what is commonly believed touching that matter; that I may not appear sullen or lazy, I am content not to decline employing a few hours in setting down my doubts, in presenting your Lordship some hints, and in considering whether the trials made in our engine will at least assist us to discover wherein the deficiency lies that needs to be supplied.

AND this, my Lord, being all my present design, I suppose you will not expect that (as if you knew not, or had forgotten what Anatomists are wont to teach) I

ſhould entertain you with a needleſs diſcourſe of the organs of reſpiration, and the variety of their ſtructure in ſeveral animals; though if it were neceſſary, and had not been performed by others, I ſhould think, with *Galen*, that by treating of the fabrics of living bodies, I might compoſe hymns to the wiſe author of nature, who, in the excellent contrivance of the lungs, and other parts of (thoſe admirable engines) animals, manifeſts himſelf to be indeed what the eloquent prophet moſt juſtly ſpeaks him, *wonderful in counſel, and excellent in working.*

NOR ſhall we any farther meddle with thoſe controverſies ſo much agitated among the moderns, namely, whether the motion of the lungs in reſpiration be their own, or but conſequent to the motion of the thorax, diaphragm, and (as ſome learned men would have it) the abdomen; and, whence it is that the air ſwells the lungs in inſpiration, any farther than they may receive light from our engine: but that it may appear what kind of ſervice it is that may be expected from it on this occaſion, we muſt premiſe a few words to ſhew wherein the ſtrength of the objection we are to anſwer, lies. In favour then of thoſe that would have the lungs rather paſſive than active in the buſineſs of reſpiration, it may againſt the common opinion be alledged, that as the lungs being deſtitute of muſcles and of fibres, are unfit to dilate themſelves; ſo it appears, that without the motion of the thorax they would not be filled with air. Since, as our learned friend Dr. *Highmore* hath well (and congruouſly to what ourſelves have purpoſely tried) obſerved, if a live dog have a great wound made in his cheſt, the lobes of the lungs on that ſide of the mediaſtinum will ſubſide and lie ſtill; the thorax and the lobes on the other ſide of the mediaſtinum continuing their former motion. And if ſuddenly at once the muſcles of the cheſt be on both ſides diſſected, upon the ingreſs of the air, the whole lungs, though untouched, will emain moveleſs, at leaſt, as to any expanſion or contraction of their ſubſtance.

To which we may add the obſervation of the diligent *Bartholinus*, who affirms the like of the diaphragm alſo; namely, that it being wounded, the lungs will fall together, and the reſpiration ceaſe, which my experiments oppoſe not, provided the wound be any thing great. And indeed the diaphragm ſeems the principal inſtrument of ordinary and gentle reſpiration, although to reſtrained reſpiration (if I may ſo call it) the intercoſtal muſcles, and perhaps ſome others, may be allowed eminently to concur. But the chief of the controverſies formerly pointed at, is not yet decided; namely, what it is that conveys the air into the lungs. For when, to counterbalance all that hath been alledged, thoſe that plead for the lungs demand what it is that ſhould bring the air into the lungs, if themſelves do not attract it, their antagoniſts diſagree about the reply. For when to this queſtion ſome of the beſt modern philoſophers anſwer, that by the dilatation of the cheſt the contiguous air is thruſt away, and that preſſing upon the next air to it, and ſo onwards, the propulſion is continued till the air be driven into the lungs, and ſo dilate them; when this (I ſay) as anſwered, it is objected even by *Bartholine* himſelf, as a convincing reply, that, according to this doctrine, a man could not fetch his breath from a great veſſel full of air, with a ſlender neck; becauſe, that when his mouth covers the orifice of the neck, the dilatation of his thorax could not propel the air in the veſſel into his lungs, by reaſon of its being ſeparated by the incloſing veſſel from the ambient air; and yet, ſay they, experience witneſſeth, that out of ſuch a veſſel a man may ſuck air. But of this difficulty our engine furniſheth us with an eaſy ſolution, ſince many of the former experiments have manifeſted, that in the caſe propoſed, there needs not be made any (though it is true that in ordinary reſpiration there is wont to be made ſome) propulſion of the air by the ſwelling thorax or abdomen into the lungs; ſince upon the bare dilatation of the thorax, the ſpring of that internal air, or halituous ſubſtance that is wont

wont to possess as much of the cavity of the chest as the lungs fill not up, being much weakened, the external and contiguous air must necessarily press in at the open wind-pipe into the lungs, as finding there less resistance than any where else about it.

AND hence (by the way) we may derive a new assistance to judge of that famous controversy disputed among Naturalists and Physicians, ever since *Galen*'s time, some maintaining that the chest, with the contained lungs, may be resembled to a pair of bellows, which comes therefore to be filled because it was dilated; and others pleading to have the comparison made to a bladder, which is therefore dilated because it is filled. For as to the thorax, it seems evident from what hath been lately said, that it, like a pair of bellows, happens to be partly filled with air, but because it was dilated: but as for the lungs themselves, who want fibres to distend them, they may fitly enough be compared to a bladder; since they are dilated by being filled, namely, by that air which rusheth into them upon the dilatation of the chest, in whose increased cavity it finds (as we freshly noted) less resistance to its spring than elsewhere. And this brings into my mind that strange observation of *Nicolaus Fontanus*, a physician at *Amsterdam*, who testifieth, that in a boy of the same town, four years old, there was found, instead of lungs, a certain membranous bladder; which being filled with wind, and furnished with little veins, had its origination from the wind-pipe itself: which being supposed true, how well it will agree with most of the opinions touching respiration, I leave to be considered.

AND thus may the grand objection of *Bartholine*, and others, be answered: but I leave to Anatomists to consider what is to be said to some observations that seem to contradict those anatomical experiments already mentioned: such was particularly that which I remember I have read in *Sennertus* (from the observation of his father-in-law *Schato*) of a melancholy student, who having stabbed himself, and pierced the diaphragm in the thinner or tendonous part (called by many the nervous circle) lived seven months after he had wounded himself, though after his death (preceded by violent vomitings) the wound (perchance dilated by those strainings) appeared so great, that the whole stomach was found to have got in by it into the left side of the thorax. And such also was the accident that happened to a nobleman, whom I remember I have seen, and who is yet alive, in whose chest there hath, for these many years, remained a hole so great, that the motion of his heart may be perceived by it. These (I say) and some other observations, I shall now forbear to insist on, because I hold it not unfit, before we come to consider the use of respiration, that we acquaint your Lordship with an ingenious conjecture, that was made at the cause of the hasty death of the animals our engine killed; namely, that it was not the want of air that destroyed them, but the pressure of the innate air in the cavity of the chest: as if the spring of the air being no longer counterbalanced by the ambient air, was thereby become so strong, that it kept the thorax forcibly distended, and hindered its wonted contraction; and so compressed the lungs and their vessels, as to obstruct the circulation of the blood. And this conjecture, as it is specious enough, so I might have admitted it for true; but that I considered, that (not to mention that one, especially of the animals killed in our engine, seemed manifestly for a pretty while, and not long before he died, to move his thorax, as if he exercised respiration) the diligent *Walleus* relates, that he divers times observed, in the dissection of live bodies, that the membrane that invests the lungs had pores in it as big as the larger sort of peas; which agreeth with the observations of chirurgeons and physicians, *viz.* that matter collected in the thorax hath penetrated into the lungs, and been discharged by coughing. And I remember too, that most of the animals we killed in our engine were birds

birds, of whose lungs *Harvey* somewhere informs us, that he observed them very manifestly to open at their extremities into the abdomen; and by such perforations we may well suppose the passage free betwixt the external air, and that in the abdomen: but this conjecture may be farther considered. Besides, to show that the animals that died in our glasses, need not be supposed to have been killed by the want of air, we foresee another argument, that we must deal so ingenuously with your Lordship, as not to conceal You very well know, that besides the generality of the schools, there are many new philosophers, who, though they dissent from the old Peripatetics in other things, do, as they, deny the possibility of a vacuum; and hold, that those spaces which are devoid of air, and other grosser bodies, are all of them exactly replenished with a certain ethereal matter, so thin and subtile, that it can freely permeate the pores of the compactedst and closest bodies, and even of glass itself. Now some of those Naturalists that are of this persuasion may object, that the animals that died in our receiver, did so, not so much for lack of air, as by reason that the air that was pumped out was necessarily succeeded by an etherial substance; which consisting of parts vehemently agitated, and so very small, as without resistance to pass in and out through the very pores of glass; it may well be supposed, that a considerable quantity of this restless and subtile matter meeting together in the receiver, with the excessive heat of it, may be quickly able to destroy a little animal, or at least make the air too intemperately hot to be fit for respiration.

BUT though this be a difficulty not so easily to be resolved without the assistance of our engine, yet I suppose we have already answered the objection of our 38th and 39th experiments; which, though we made partly for other purposes, yet we premised them only to clear up the difficulty proposed.

ANOTHER suspicion we should have entertained concerning the death of our animals; namely, that upon the sudden removal of the wonted pressure of the ambient air, the warm blood of those animals was brought to an effervescence or ebullition; or at least so vehemently expanded, as to disturb the circulation of the blood, and to disorder the whole œconomy of the body: this, I say, I should have had some suspicion of, but that animals of a hot constitution are not the sole ones that cannot in our exhausted engine exercise the function of life. But I must not now dwell upon matters of this nature, because I think it high time to proceed to the consideration of the principal subject of our engine, namely, the use of respiration; or rather, the use of the air in respiration. For whereas of the divers uses of it mentioned by Anatomists, the most, such as the production and modulation of the voice by the elision of the air, the larynx, *&c.* the expulsion of excrements by coughing, the conveying in of odours by inspiration, and some others, rather convenient for the well being of an animal, than absolutely necessary to his life; whereas (I say) the other uses are such as we have said, the great *Hippocrates* himself gives this notable testimony to the use of the air, as to animals endowed with lungs: *Mortalibus* (saith he) *hic* (spiritus) *tum vitæ, tum morborum ægrotis causa est. Tantaque corporibus omnibus spiritûs inest necessitas, ut siquidem aliis omnibus & cibis & potionibus, quis abstineat, duos tamen aut tres, vel plures dies possit vitam ducere: at si quis spiritus in corpus vias intercipiat, vel exiguâ diei parte, homini pereundum sit; adeo necessarius est usus spiritûs in corpore. Ad hæc quoque, quum omnibus aliis actionibus homines quiescant, quod mutationibus innumeris vita sit exposita, ab hâc tamen solâ actione unquam desistant animantia, quin aut spiritum adducant, aut reddant.*

BUT touching the account upon which the inspiration and exspiration of air (both which are comprehended in ἀναπνοὴ, respiration) is so necessary to life, both Naturalists

lists and Physicians do so disagree, that it will be very difficult either to reconcile their opinions, or determine their controversies.

FOR first, many there are, who think the chief, if not sole use of respiration, to be the cooling and tempering of that heat in the heart and blood, which otherwise would be immoderate; and this opinion not only seems to be most received amongst scholastic writers, but divers of the new philosophers, Cartesians and others, admitted with some variation; teaching, that the air is necessary, by its coldness, to condense the blood that passeth out of the right ventricle of the heart into the lungs, that thereby it may contain such a consistence as is requisite to make it fit fewel for the vital fire or flame, in the left ventricle of the heart. And this opinion seems favoured by this, that fishes, and other cold creatures, whose hearts have but one cavity, are also unprovided of lungs, and by some other considerations. But though it need not be denied, that the inspired air may sometimes be of use by refrigerating the heart, yet (against the opinion that makes this refrigeration the most genuine and constant use of the air) it may be objected, that divers cold creatures (some of which, as particularly frogs, live in the water) have yet need of respiration; which seems not likely to be needed for refrigeration by them that are destitute of any sensible heat, and besides, live in the cold water: that even decrepid old men, whose natural heat is made very languid, and almost extinguished by reason of age, have yet a necessity of frequent respiration: that a temperate air is fittest for the generality of breathing creatures; and as an air too hot, so also an air too cold may be inconvenient for them (especially if they be troubled with an immoderate degree of the same quality which is predominant in the air:) that in some diseases the natural heat is so weakened, that in case the use of respiration were too cool, it would be more hurtful than beneficial to breathe; and the suspending of the respiration may supply the place of those very hot medicines that are wont to be employed in such distempers: that nature might much better have given the heart but a moderate heat, than such an excessive one, as needs to be perpetually cooled, to keep it from growing destructive; which the gentle, and not the burning heat of an animal's heart, seems not intense enough so indispensably to require These, and other objections might be opposed, and pressed against the recited opinion; but we shall not insist on them, but only add to them, that it appears not by our foregoing experiments (I mean the 38th and 39th) that in our exhausted receiver, where yet animals die so suddenly for want of respiration, the ambient body is sensibly hotter than the common air.

OTHER learned men there are, who will have the very substance of the air to get in by the vessels of the lungs, to the left ventricle of the heart, not only to temper its heat, but to provide for the generation of spirits. And these alledge for themselves the authority of the ancients, among whom *Hippocrates* seems manifestly to favour their opinion; and both *Aristotle* and *Galen* do sometimes (for methinks they speak doubtfully enough) appear inclinable to it. But for aught ever I could see in dissections, it is very difficult to make out, how the air is conveyed into the left ventricle of the heart, especially the systole and diastole of the heart and lungs being very far from being synchronical: besides, that the spirits seeming to be but the most subtile and unctuous particles of the blood, appear to be of a very differing nature from that of the lean and incombustible corpuscles of air. Other objections against this opinion have been proposed, and pressed by that excellent Anatomist, and my industrious friend Dr. *Highmore*, to whom I shall therefore refer you.

ANOTHER opinion there is touching respiration, which makes the genuine use of it to be ventilation not of the heart, but of the blood, in its passage through the lungs; in which passage it is disburthened of those excrementitious steams proceeding, for

the moſt part, from the ſuperfluous ſeroſities of the blood (we may add) and of the chyle too, which (by thoſe new conduits of late very happily detected by the famous *Pecquet*) hath been newly mixed with it in the heart. And this opinion is that of the induſtrious *Mœbius*, and is ſaid to have been that of that excellent philoſopher *Gaſſendus*; and hath been, in part, an opinion almoſt vulgar. But this hypotheſis may be explicated two ways: for firſt, the neceſſity of the air in reſpiration may be ſuppoſed to proceed from hence; that as a flame cannot long burn in a narrow and cloſe place, becauſe the fuliginous ſteams it inceſſantly throws out, cannot be long received into the ambient body; which, after a while, growing too full of them to admit any more, ſtifles the flame: ſo that the vital fire in the heart requires an ambient body of a yielding nature, to receive into it the ſuperfluous ſeroſities, and other recrements of the blood, whoſe ſeaſonable expulſion is requiſite to depurate the maſs of blood and make it fit, both to circulate and to maintain the vital heat reſiding in the heart. The other way of explicating the above-mentioned hypotheſis is, by ſuppoſing, that the air doth not only, as a receptacle, admit into its pores the excrementitious vapours of the blood, when they are expelled through the wind-pipe, but doth alſo convey them out of the lungs, in regard that the inſpired air reaching to all the ends of the *aſpera arteria*, doth there aſſociate itſelf with the exhalations of the circulating blood, and when it is exploded, carries them away with itſelf: as we ſee that winds ſpeedily dry up the ſurfaces of wet bodies, not to ſay any thing of what we formerly obſerved touching our liquor, whoſe fumes were ſtrangely elevated upon the ingreſs of the air.

Now, of theſe two ways of explicating the uſe of reſpiration, our engine affords us this objection againſt the firſt; that upon the exſuction of the air, the animal dies a great deal ſooner than if it were left in the veſſel; though by that exſuction, the ambient ſpace is left much more free to receive the ſteams that are either breathed out of the lungs of the animal, or diſcharged by inſenſible tranſpiration through the pores of his ſkin.

But if the hypotheſis propoſed be taken in the other ſenſe, it ſeems congruous enough to that grand obſervation, which partly the phænomena of our engine, and partly the relations of travellers, have ſuggeſted to us; namely, that there is a certain conſiſtence of air requiſite to reſpiration: ſo that if it be too thick, and already over-charged with vapours, it will be unfit to unite with, and carry off thoſe of the blood, as water will diſſolve and aſſociate to itſelf but a certain proportion of ſaline corpuſcles; and if it be too thin or rarefied, the number or ſize of the aërial particles is too ſmall to be able to carry off the halituous excrements of the blood, in ſuch plenty as is requiſite.

Now that air too much thickened (and as it were clogged) with ſteams, is unfit for reſpiration, may appear by what is wont to happen in the lead mines of *Devonſhire* and for aught I know, in thoſe too of other countries, though I have ſeen mines where no ſuch thing was complained of) for I have been informed by more than one credible perſon (and particularly by an ingenious man that hath often, for curioſity, digged in thoſe mines and been employed about them) that there often riſeth damps (as retaining the German word by which we call them) which doth ſo thicken the air, that, unleſs the workmen ſpeedily make ſigns to them that are above, they would (which alſo ſometimes happens) be preſently ſtifled for want of breath: and though their companions do make haſte to draw them up, yet frequently, by that time they come to the free air, they are, as it were, in a ſwoon, and are a good while before they come to themſelves again. And that this ſwooning ſeems not to proceed from any arſenical or poiſonous exhalation contained in the damp, as from its over-much con-

condenſing the air, ſeems probable from hence; that the ſame damps oftentimes leiſurely extinguiſh the flames of their candles or lamps; and from hence alſo that it appears (by many relations of authentical authors) that in thoſe cellars where great ſtore of new wine is ſet to work, men have been ſuffocated by the too great plenty of the ſteams exhaling from the muſt, and too much thickning the air: as may be gathered from the cuſtom that is now uſed in ſome hot countries, where thoſe that have occaſion to go into ſuch cellars, carry with them a quantity of well-kindled coals, which they hold near their faces; whereby it comes to paſs, that the fire diſcuſſing the fumes, and rarefying the air, reduceth the ambient body to a conſiſtence fit for reſpiration.

WE will add (by way of confirmation) the following experiment: in ſuch a ſmall receiver, as thoſe wherein we killed divers birds, we carefully cloſed up one, who, though for a quarter of an hour he ſeemed not much prejudiced by the cloſeneſs of his priſon, afterwards began firſt to pant very vehemently, and keep his bill very open, and then to appear very ſick; and laſt of all, after ſome long and violent ſtrainings, to caſt up ſome little matter out of his ſtomach; which he did ſeveral times, till growing ſo ſick that he ſtaggered and gaſped, as being juſt ready to die. We perceived, that within about three quarters of an hour from the time that he was put in, he had ſo thickened and tainted the air with the ſteams of his body, that it was become altogether unfit for the uſe of reſpiration: which he will not much wonder at, who hath taken notice in *Sanctorius* his *Statica Medicina*, how much that part of our aliments which goeth off by inſenſible tranſpiration, exceeds in weight all the viſible and groſſer excrements both ſolid and liquid.

THAT (on the other ſide) an air too much dilated is not ſerviceable for the ends of reſpiration, the haſty death of the animal we killed in our exhauſted receiver ſeems ſufficiently to manifeſt. And it may not irrationally be doubted, whether or no, if a man were raiſed to the very top of the atmoſphere, he would be able to live many minutes, and would not quickly die for want of ſuch air as we are wont to breathe here below. And that this conjecture may not appear extravagant, I ſhall, on this occaſion, ſubjoin a memorable relation that I have met with in the learned *Joſephus Acoſta*, who tells us, that when he himſelf paſſed the high mountains of *Peru* (which they call *Pariacaca*) to which, he ſays, that the *Alps* themſelves ſeemed to them but as ordinary houſes in regard of high towers, he and his companions were ſurprized with ſuch extreme pangs of ſtraining and vomiting (not without caſting up blood too) and with ſo violent a diſtemper, that he concludes he ſhould undoubtedly have died, but that this laſted not above three or four hours, before they came into a more convenient and natural temperature of air: to which our learned author adds an inference, which being the principal thing I deſigned in mentioning the narrative, I ſhall ſet down in his own words: *I therefore* (ſays he) *perſuade myſelf, that the element of the air is there ſo ſubtile and delicate, as it is not proportionable with the breathing of man, which requires a more groſs and temperate air; and I believe it is the cauſe that doth ſo much alter the ſtomach, and trouble all the diſpoſition.* Thus far our author, whoſe words I mention, that we may gueſs, by what happens ſomewhat near the confines of the atmoſphere (though probably far from the ſurface of it) what would happen beyond the atmoſphere. That, which ſome of thoſe that treat of the height of mountains, relate out of *Ariſtotle*, namely, that thoſe that aſcend to the top of the mountain *Olympus*, could not keep themſelves alive, without carrying with them wet ſpunges, by whoſe aſſiſtance they could reſpire in that air, otherwiſe too thin for reſpiration (that relation, I ſay, concerning this mountain) would much confirm what

hath been newly recited out of *Acosta*, if we had sufficient reason to believe it. But I confess I am very diffident of the truth of it; partly, because when I passed the *Alps*, I took notice of no notable change betwixt the consistence of the air at the top and the bottom of the mountain; partly, because in a punctual relation made by an English gentleman, of his ascension to the top of the pike of *Tenariff* (which is by great odds higher than *Olympus*) I find no mention of any such difficulty of breathing; and partly also, because the same author tells us out of *Aristotle*, that upon the top of *Olympus* there is no motion of the air, insomuch that letters traced upon the dust, have been, after many years, found legible and not discomposed; whereas that inquisitive *Busbequius* (who was ambassador from the German to the Turkish emperor) in one of his eloquent Epistles, tells us, upon his own knowledge, that Olympus *may be seen from* Constantinople, *blanched with perpetual snow*; which seems to argue, that the top of that, as well as of divers other tall hills, is not above that region of the air wherein meteors are formed. Though otherwise, in that memorable narrative which *David Frælichius* made of his ascent to the top of the prodigiously high Hungarian mountain *Carpathus*, he tells us, *that when having passed through very thick clouds, he came to the very top of the hill, he found the air so calm and subtile, that not a hair of his head moved; whereas, in the lower stages of the mountain, he felt a vehement wind.* But this might well be casual, as was his, having a clear air where he was, though there were clouds, not only beneath him, but above him.

Epist. 3.

Frælichius apud Varen. Geogra. Gener. lib. 1. cap. 19.

BUT, though what hath been hitherto discoursed, incline us to look upon the ventilation and depuration of the blood, as one of the principal and constant uses of respiration; yet methinks it may be suspected that the air doth something more than barely help to carry off what is thrown out of the blood, in its passage through the lungs, from the right ventricle of the heart to the left. For we see, in phlegmatic constitutions and diseases, that the blood will circulate tolerably well, notwithstanding its being excessively serous: and in asthmatical persons, we often see, that though the lungs be very much stuffed with tough phlegm, yet the patient may live some months, if not some years. So that it seems scarce probable, that either the want of throwing out the superfluous serum of the blood for a few moments, or the detaining it, during so short a while, in the lungs, should be able to kill a perfectly sound and lively animal: I say, for a few moments, because, that having divers times tried the experiment of killing birds in a small receiver, we commonly found, that within half a minute of an hour, or thereabout, the bird would be surprised by mortal convulsions, and within about a minute more would be stark dead, beyond the recovery of the air, though never so hastily let in. Which sort of experiments seem so strange, that we were obliged to make it several times, which gained it the advantage of having persons of differing qualities, professions and sexes (as not only ladies and lords, but doctors and mathematicians) to witness it. And to satisfy your Lordship, that it was not the narrowness of the vessel, but the sudden exsuction of the air that dispatched these creatures so soon; we will add, that we once inclosed one of these birds in one of these small receivers, where, for a while, he was so little sensible of his imprisonment, that he eat very cheerfully certain seeds that were conveyed in with him, and not only lived ten minutes, but had probably lived much longer, had not a great person, that was spectator of some of these experiments, rescued him from the prosecution of the trial. Another bird being within about half a minute cast into violent convulsions, and reduced into a sprawling condition, upon the exsuction of the air, by the pity of some fair ladies, related to your Lordship, who made me hastily let in some air at the stop-cock, the gasping animal was pre-

ſently recovered, and in a condition to enjoy the benefit of the ladies compaſſion. And another time alſo, being reſolved not to be interrupted in our experiment, we did at night ſhut up a bird in one of our ſmall receivers, and obſerved that for a good while he ſo little felt the alteration of the air, that he fell aſleep with his head under his wing; and though he afterwards awaked ſick, yet he continued upon his legs between forty minutes and three quarters of an hour: after which, ſeeming ready to expire, we took him out, and ſoon found him able to make uſe of the liberty we gave him for a compenſation of his ſufferings.

IF to the foregoing inſtances of the ſudden deſtruction of animals, by the removal of the ambient air, we ſhould now annex ſome, that we think fitter to reſerve till anon; perhaps your Lordſhip would ſuſpect, with me, that there is ſome uſe of the air which we do not yet ſo well underſtand, that makes it ſo continually needful to the life of the animals. *Paracelſus*, indeed, tells us, *that as the ſtomach concocts meat, and makes part of it uſeful to the body, rejecting the other part; ſo the lungs conſume part of the air, and proſcribe the reſt.* So that, according to our Hermetic philoſopher (as his followers would have him ſtyled) it ſeems we may ſuppoſe, that there is in the air a little vital quinteſſence (if I may ſo call it) which ſerves to the refreſhment and reſtauration of our vital ſpirits, for which uſe the groſſer and incomparably greater part of the air being unſerviceable, it need not ſeem ſtrange, that an animal ſtands in need of almoſt inceſſantly drawing in freſh air. But though this opinion is not (as ſome of the ſame author) abſurd, yet beſides that it ſhould not be barely aſſerted, but explicated and proved; and beſides that ſome objections may be framed againſt it, out of what hath been already argued againſt the tranſmutation of air into vital ſpirits: beſides theſe things, it ſeems not probable, that the bare want of the generation of the wonted quantity of vital ſpirits, for leſs than one minute, ſhould, within that time, be able to kill a lively animal, without the help of any external violence at all.

BUT yet, on occaſion of this opinion of *Paracelſus*, perhaps it will not be impertinent if, before I proceed, I acquaint your Lordſhip with a conceit of that deſervedly famous Mechanician and Chymiſt, *Cornelius Drebell*, who, among other ſtrange things that he performed, is affirmed, by more than a few credible perſons, to have contrived, for the late learned King *James*, a veſſel to go under water; of which, trial was made in the *Thames*, with admired ſucceſs, the veſſel carrying twelve rowers, beſides paſſengers; one of which is yet alive, and related it to an excellent Mathematician that informed me of it. Now that for which I mention this ſtory is, that having had the curioſity and opportunity to make particular enquiries among the relations of *Drebell*, and eſpecially of an ingenious Phyſician that married his daughter, concerning the grounds upon which he conceived it feaſible to make men unaccuſtomed to continue ſo long under water without ſuffocation, or (as the lately mentioned perſon that went in the veſſel affirms) without inconvenience; I was anſwered, that *Drebell* conceived, that it is not the whole body of the air, but a certain quinteſſence (as Chymiſts ſpeak) or ſpirituous part of it, that makes it fit for reſpiration; which being ſpent, the remaining groſſer body, or carcaſe, if I may ſo call it, of the air, is unable to cheriſh the vital flame reſiding in the heart: ſo that, for aught I could gather, beſides the mechanical contrivance of his veſſel, he had a chymical liquor, which he accounted the chief ſecret of his ſubmarine navigation. For when, from time to time, he perceived that the finer and purer part of the air was conſumed, or overclogged by the reſpiration and ſteams of thoſe that went in his ſhip, he would, by unſtopping a veſſel full of this liquor, ſpeedily reſtore to the troubled air ſuch a proportion of vital parts, as would make it again, for a good while, fit for reſpi-

ration, whether by dissipating, or precipitating the grosser exhalations, or by some other intelligible way, I must not now stay to examine; contenting myself to add, that having had the opportunity to do some service to those of his relations that were most intimate with him, and having made it my business to learn what this strange liquor might be, they constantly affirmed that *Drebell* would never disclose the liquor unto any, nor so much as tell the matter whereof he had made it, to above one person, who himself assured me what it was.

This account of *Drebell*'s performance I mention, not that I any farther assent to opinion than I have already intimated, but because the man and the invention being extraordinary, I suppose your Lordship will not be displeased to know the utmost I could learn about it; especially not having found it mentioned by any writer. Wherefore I have been sometimes inclined to favourable thoughts of their opinion, who would have the air necessary to ventilate and cherish the vital flame, which they do suppose to be continually burning in the heart. For we see, that in our engine the flame of a lamp will last almost as little after the exsuction of the air, as the life of an animal: nay, I remember that though I devised a more promising way to make a fire last in our exhausted receiver, yet it would not succeed. We took a hard body made in the form of a clove, but twice as long, and proportionably thick; this body being made of such a composition, that if it be kindled at the upper end, it will most certainly burn away to the very bottom, much better than a match; we conveyed it divers times, kindled at the upper end, into one of our small receivers, but still found, that though presently upon the exsuction of the air, it would leave smoking, and seem quite gone out, and again begin to smoke as soon as the air was let in upon it; yet if the air were kept out but four or five minutes, the fire would be totally and irrevocably extinguished. To which we will add, that though we conveyed into a great receiver a small lamp with rectified spirit of wine, that being so pure as not to smut the cotton wick, or so much as a piece of white paper held over it; yet we could not by divers trials make the flame last a couple of minutes, after the air was begun to be drawn out. But though our engine thus shews us a new kind of resemblance betwixt fire and life, yet the opinion we have last mentioned is not free from difficulties. For, though in the hearts of many animals, blood be a warm liquor, and in some even a hot one; yet it is not easy to conceive either how the air (in substance) can get thither, or how, in case it could, it were able to increase the heat. Since, however, the air may increase the heat of a coal by blowing off the ashes, and making the active corpuscles pierce farther into the kindled body, and shatter it the more; yet we see hot liquors have their heat allayed, and not augmented, by having air blown on them. And whereas some eminent Naturalists think it not inconvenient to make the heat residing in the heart to be a true flame, provided they add, that it is such a temperate and almost insensible fire, as the flame of spirit of wine, which will long burn upon fine white linen or paper without consuming either; give me leave to wish that they had been more curious to make differing trials with that liquor. For (as we observe in another treatise) the reason why a linen cloth, dipped in common spirit of wine, is not burnt by the flame of it, is, because the phlegm of the liquor defends the cloth: and the flame of the spirit of wine is so far from being too weak to burn a piece of paper, or of linen, that I have used it in lamps to distil liquors out of tall cucurbites, and found that the spirit burned away indeed much faster than sallad oil, but gave at least as great a heat: nay, I have, for curiosity sake, melted crude gold, and that readily enough, with the bare flame of pure spirit of wine.

BUT

BUT not to press this any farther, we will, on this occasion, venture to subjoin an odd observation, which may perhaps invite to a farther inquiry into the opinion we have for discourse sake opposed. Our English *Democritus*, Dr. *Harvey*, proposeth this difficult and noble problem to Anatomists, *Why a fœtus, even out of the womb, if involved in the secundines, may live a good while without respiration; but in case after having once began to breathe, its respiration be stopped, it will presently die?* We are far from pretending to solve so hard a problem, but this we tried in relation to it: we took a bitch that was said to be almost ready to whelp, and having caused her to be hanged, we presently opened her abdomen, and found four puppies in her womb; one of these we took out, and having freed him from the teguments that involved him, and from the liquor he swam in, we observed that he quickly opened his mouth very wide, moved his tongue, and exercised respiration. Then we opened both his abdomen and his chest, and cut asunder the diaphragm, notwithstanding which, he seemed often to endeavour respiring, and moved in a notable manner, both the intercostal muscles, part of the diaphragm, the mouth and the tongue. But that which we mention this puppy for, was this, that being desirous to try whether the other young ones, that had not yet breathed at all, would long survive this or no; we took them also out of the womb, and having opened them, found none of them so much alive, as to have any perceptible motion in his heart; whereas the heart of that puppy which had once enjoyed the benefit of respiration continued beating so long, that we ourselves observed the auricle to beat, after five or six hours; and a servant that staid up and watched it after we were gone to bed, affirmed, that he saw the pulsation continue about two hours longer. I shall leave it to others to make reflexions upon this observation, compared with Dr. *Harvey*'s problem.

IT is much doubted, whether fishes breathe under water, and we shall not take upon us, as yet, to determine the question either way, because we have not yet been able to procure little fishes alive to make experiments upon. That such as are not setaceous (for such manifestly breathe) have not respiration, properly so called, such as is exercised by four-footed beasts, and birds, may be argued from their having no cavity in their hearts, and from their want of lungs, whence they are observed to be mute; unless we say, what is not altogether absurd, that their gills seem somewhat analogous (as to their use) to lungs. But that, on the other side, air is necessary to the lives even of fishes, and that therefore it is probable they have some obscure kind of respiration, seems manifest by two or three observations and experiments, mentioned by divers authors, who tell us, *that fishes soon die in ponds and glasses quite filled with water; if the one be so frozen over, and the other so closely stopped, that the fishes cannot enjoy the benefit of the air*; if we allow them to be true. But because these relations are not wont to be delivered by writers upon their own knowledge; as I shall not reject them, so I dare not build upon them, till I have opportunity to examine them by experience. In the mean time, we will add, that our engine hath taught us two things that may illustrate the matter in hand: the one, that there is wont to lurk in water many little parcels of interspersed air, whereof it seems not impossible that fishes may make some use, either by separating it, when they strain the matter thorow their gills, or by some other way; the other, what may be collected from the following experiment.

WE took a large eel (being able to procure no other fish alive) and removing it out of the vessel of water, wherein it was brought us, into our great receiver, we caused the air to be pumped out; and observed, that the eel, after some motion to and fro in the glass, seemed somewhat discomposed; and that when we had prosecuted the exsuction of the air somewhat obstinately, she turned up her belly, as dying fishes

are

are wont to do, and from thence forward lay altogether moveless, just as if she were stark dead; and though I did not think her so, yet the continuing in that posture, even after the cover of the receiver was taken off (whereby the air was let in) I should have been of the opinion of the by-standers, if the diffidence I am wont to exercise in trying experiments (especially such as are not usual) had not invited me to take the fish out of the receiver; upon which she shewed herself, by her vivid motions, as much alive as before.

But that is most strange which we observed of a great, grey house-snail (as they call it) which being closed up in one of our small receivers, did not only not fall down from the side of the glass, upon the drawing out of the air; (for that may be ascribed to the tenacity of the liquor wherewith snails use to stick themselves, even to the smoothest bodies:) but was not so much as deprived of progressive motion by the recess of the air; though, except this snail, we never put any living creature into our receiver, whom it did not kill, or at least reduce to seem ready to die. But as we shall not here examine what interest the glutinous, and uneasily dissipable nature of the juices of snails may have on this event; so whether this escape of our eel be ascribed to the particular and vivacious nature of this sort of fishes, or to this, that the air is not indeed necessary to the life of fishes; or finally to this, that though these animals need some air, yet they need so little, that that which could not be drawn out of the receiver might (at least for a while) suffice them, we will not now determine.

Nor are we at leisure to examine that paradox of *Hippocrates*, which some learned physicians have of late revived, namely, that the fœtus respires in the womb. For, on the one side, it seems very difficult to conceive, how air should traverse the body of the mother, and the teguments of the child: and since nature hath, in new-born babes, contrived peculiar and temporary vessels, that the blood may circulate thorough other passages, than it is wont to do in the same individuals when they come to have the free use of their lungs; it seems unlikely that infants in the womb do properly respire. But then since our experiments have manifested, that almost all kinds of liquors do, as well as water, abound with interspersed corpuscles of air, it seems not altogether absurd to say, that when the fœtus is grown big, he may (especially the upper part of the involving amnios being destitute of liquor, and filled only with an halituous substance) exercise some obscure respiration; especially, since it is not (as many wise men think it) a fable, that children have been heard to cry in the mother's womb; for though it happens exceeding rarely, yet sometimes it hath been observed. And I know a young lady, whose friends, when she was some years since with child, complained to me, that she was several times much frighted with the cries of her infant, which, till I disabused her, she and her friends looked upon as portentous. And such observations are the more credible, because not only housewives, but more judicious persons, mention it as no very unfrequent thing, to hear the chick pip and cry in the egg, before the shell be broken. But this I mention but as a probable, not a cogent argument, till I can discover whether an elision of an halituous substance, though no true air, may not at the top of the larynx produce a sound; since I find that the blade of a knife, held in several postures in the stream of vapours (or rarefied water) that issues out of an æolipile, will afford various and very audible sounds.

I had thoughts of conveying into our receiver young ones, ripped out of the womb of their dams, with their involving coats entire, but could not procure them. And I have also had thoughts of trying whether it be not practicable to make a receiver, though not of glass, yet with little glass windows, so placed, that one may freely look

look into it, capacious enough to hold a man, who may obſerve ſeveral things, both touching reſpiration, and divers other matters; and who, in caſe of fainting, may, by giving a ſign of his weakneſs, be immediately relieved, by having air let in upon him. And it ſeems not impoſſible, but that by accuſtomance, ſome men may bring themſelves to ſupport the want of air a pretty while, ſince we ſee that divers will live, ſo much longer than other men, under water; that thoſe that dive for pearls in the *Weſt Indies* are ſaid to be able to ſtay a whole hour under water: and *Cardan* tells us of one *Colanus* a diver in *Sicily*, who was able to continue (if *Cardan* neither miſtake nor impoſe upon us) three or four times as long. Not to mind your Lordſhip, that you have yourſelf oftentimes ſeen in *England* a corpulent man, who is wont to deſcend to the bottom of the *Thames*, and bring out of deep holes, at the bottom of the banks, large fiſhes alive in his hands. And *Acoſta* tells us, he ſaw in *Peru* the like manner of fiſhing, but more difficult, practiſed by the *Indians*.

I MADE mention of ſome men, and of accuſtomance; becauſe there are but very few, who, though they uſe themſelves to it by degrees, are fit to ſupport, for many minutes, the want of air. Inſomuch that an ingenious man of my acquaintance, who is very famous for the uſeful ſkill of drawing goods, and even ordnance, out of ſunk ſhips, being aſked by me how long he was able to continue at the depth of 50 or 60 foot under water, without the uſe of reſpiration, confeſſed to me, that he cannot continue above two minutes of an hour, without reſorting to the air, which he carries down with him in a certain engine (whereof I can ſhow your Lordſhip a deſcription). Another thing I alſo learned of him by enquiry, that was not deſpicable: for aſking him, whether he found any uſe of chawing little ſponges, dipt in oil, in his mouth, when he was perfectly under water, and at a diſtance from his engine; he told me, that by the help of theſe ſponges he could much longer ſupport the want of his wonted reſpiration, than he was able to do without them. The true cauſe of which would perhaps, if diſcovered, teach us ſome thing pertinent to the problem touching the reſpiration of fiſhes.

BUT the neceſſity of air to the moſt part of animals unaccuſtomed to the want of it, may beſt be judged of, by the following experiments, which we tried in our engine, to diſcover, whether inſects themſelves have not, either reſpiration, or ſome other uſe of the air equivalent thereunto.

WE took then an humble bee, one of thoſe common flies that are called fleſh-flies, and one of thoſe hairy worms that reſemble caterpillars; and are wont to be called palmer worms: theſe three we conveyed into one of our ſmall receivers, and obſerved, to the great wonder of the beholders, that not only the bee, and the fly fell down, and lay with their bellies upwards, but the worm itſelf ſeemed to be ſuddenly ſtruck dead; all of them being reduced to lie without motion, or any other diſcernable ſign of life, within ſomewhat leſs (if we miſtake not) than one minute of an hour; and this, notwithſtanding the ſmallneſs of the animals in proportion to the capacity of the veſſels. Which circumſtance we the rather mention, becauſe we found that the veſſel was not free from leaks. And to ſatisfy the ſpectators, that it was the abſence of the air that cauſed this great and ſudden change; we had no ſooner re-admitted the air at the ſtop-cock, than all the three inſects began to ſhew ſigns of life, and by little and little to recover. But when we had again drawn out the air, their motions preſently ceaſed, and they fell down ſeemingly dead as before, continuing moveleſs, as long as, by continuing to pump, the veſſel was kept exhauſted. This invited us thankfully to reflect upon the wiſe goodneſs of the creator, who, by giving the air a ſpring, hath made it ſo very difficult, as men find it, to exclude a thing ſo neceſſary to animals: and

and it gave us also occasion to suspect, that if insects have no lungs, nor any part analogous thereunto, the ambient air affects them, and relieves them at the pores of their skin; it not being irrational to extend to these creatures that of *Hippocrates*, who saith, that a living body is throughout perspirable; or, to use his expression, εἰσπνοὴ δὲ ἐκπνοὴ, disposed to admit and part with what is spirituous. Which may be somewhat illustrated by what we have elsewhere noted, that the moister parts of the air readily insinuate themselves into, and recede from the pores of the beards of wild oats, and those of divers other wild plants; which almost continually wreath and unwreath themselves according to, even, the light variations of the temperature of the ambient air.

THIS circumstance of our experiment we particularly took notice of, that when at any time, upon the ingress of the air, the bee began to recover, the first sign of life she gave, was a vehement panting, which appeared near the tail; which we therefore mention, because we have observed the like in bees drowned in water, when they first come to be revived by a convenient heat: as if the air were in the one case as proper to set the spirits and alimental juice moving, as heat is in the other; and this may, perchance, deserve a farther consideration.

WE may add, that we scarce ever saw any thing that seemed so much as this experiment to manifest, that even living creatures (man always excepted) are a kind of curious engines, framed and contrived by nature (or rather the author of it) much more skilfully than our gross tools and imperfect wits can reach to. For in our present instance we see animals, vivid and perfectly sound, deprived immediately of motion, and any discernable signs of life, and reduced to a condition that differs from death, but in that it is not absolutely irrecoverable. This (I say) we see performed without any, so much as the least external violence offered to the engine; unless it be such as is offered to a wind-mill, when the wind ceasing to blow on the sails, all the several parts remain moveless and useless, till a new breath put them into motion again.

AND this was farther very notable in this experiment; that whereas it is known that bees and flies will not only walk, but fly for a great while, after their heads are off; and sometimes one half of the body will, for divers hours, walk up and down, when it is severed from the other: yet, upon the exsuction of the air, not only the progressive motion of the whole body, but the very motions of the limbs do forthwith cease; as if the presence of the air were more necessary to these animals, than the presence of their own heads.

BUT, it seems, that in these insects, that fluid body (whether it be a juice or flame) wherein life chiefly resides, is nothing near so easy dissipable as in perfect animals. For whereas we have above recited, that the birds we conveyed into our small receiver were within two minutes brought to be past recovery, we were unable (though by tiring him that pumped) to kill our insects by the exsuction of the air: for though, as long as the pump was kept moving, they continued immovable, yet, when he desisted from pumping, the air that pressed in at the unperceived leaks did, though slowly, restore them to the free exercise of the functions of life.

BUT, my Lord, I grow troublesome, and therefore shall pass on to other experiments: yet without despairing of your pardon for having entertained you so long about the use of respiration, because it is a subject of that difficulty to be explained, and yet of that importance to human life, that I shall not regret the trouble my experiments have cost me, if they be found in any degree serviceable to the purposes to which they were designed. And though I despair not but that hereafter our engine may furnish us with divers phænomena useful to illustrate the doctrine of respiration; yet

yet having not, as yet, had the opportunity to make the other trials, of various kinds, that I judge requisite for my information, I must confess to your Lordship, that in what I have hitherto said, I pretend not so much to establish or overthrow this or that hypothesis, as to lay together divers of the particulars that occurred to me, in order to a future enquiry, I say, divers of the particulars, because I could add many others, but that I want time, and fear that I should need your Lordship's pardon for having been so prolix in writing; and that of Physicians (which perhaps I shall more easily obtain) for having invaded anatomy, a discipline which they challenge to themselves, and indeed have been the almost sole improvers of. Without denying then, that the inspired and exspired air may be sometimes very useful, by condensing and cooling the blood that passeth through the lungs; I hold that the depuration of the blood in that passage, is not only one of the ordinary, but one of the principal uses of respiration. But I am apt also to suspect, that the air doth something else in respiration, which hath not yet been sufficiently explained; and therefore, till I have examined the matter more deliberately, I shall not scruple to answer the questions that may be asked me, touching the genuine use of respiration, in the excellent words employed by the acute St. *Austin*, to one who asked him hard questions: *Mallem quidem* (says he) *eorum quæ à me quæsivisti, habere scientiam quam ignorantiam: sed quia id nondum potui, magis eligo cautam ignorantiam confiteri, quam falsam scientiam profiteri.*

EXPERIMENT XLII.

HAVING (partly upon the consideration of some of the foregoing experiments, and partly upon grounds not now to be insisted on) entertained a suspicion, that the action of corrosive liquors in the dissolving of bodies may be considerably varied by the gravitation or pressure of the incumbent air, and the removal of it; I thought fit to examine my conjecture by the following experiment:

I TOOK whole pieces of red coral, and cast them into as much spirit of vinegar, as sufficed to swim above an inch over them: these substances I made choice of, that the ebullition upon the solution might not be too great, and that the operation might last the longer.

HAVING then put about half a score sprigs of coral, together with the menstruum, into a somewhat long-necked phial, whereof they seemed scarce to fill a third part, we conveyed that phial into one of our small pneumatical glasses, containing by guess about a quart of water; and having fastened on the cover, after the accustomed manner, we suffered the liquor to remain unmoved a while, to observe whether the menstruum would work upon the coral otherwise than before. But finding there did only arise, as formerly, a pretty number of small bubbles, that made there no sensible froth upon the surface of the distilled vinegar, there were made two or three exsuctions of the air; upon which, there emerged from the coral such a multitude of bubbles, as made the whole body of the menstruum appear white; and soon after a froth, as big as all the rest of the liquor, was seen to swim upon it; and the menstruum plainly appeared to boil in the glass, like a seething-pot. And though, if we desisted but one minute from pumping, the decrement of the froth and ebullition, upon the getting in of a little air, at some leak or other, seemed to argue, that the removal of the pressure of the external air was the cause, or, at least, the occasion of this effervescence; yet to evince this the more clearly, we turned the key, and let in the external air at the stop-cock: immediately upon whose entrance, the froth vanished, and so many of the bubbles within the body of the liquor disappeared,

that it loſt its whiteneſs, and grew tranſparent again; the menſtruum alſo working as languidly upon the coral, as it did before they were put into the receiver. But when we had again drawn out the air, firſt the whiteneſs re-appeared, then the ebullition was renewed, which (the pumping being a while longer, and nimbly purſued) grew ſo great, that for three or four times one after another, whenever the air was let out of the receiver into the emptied cylinder, the frothy liquor overflowed the glaſs, and ran down by the ſides of it; and yet, upon the re-admitting of the excluded air, the boiling liquor grew immediately as calm and as tranſparent as at firſt: as if indeed the operation of it, upon the coral, had been facilitated by the exſuction of the incumbent air, which on its receſs left it more eaſy for the more active parts of the liquor to ſhew themſelves ſuch, than it was whilſt the wonted preſſure of the air continued unremoved. It may indeed be ſuſpected, that thoſe vaſt and numerous bubbles proceeded not from the action of the menſtruum upon the coral, but from the ſudden emerſion of thoſe many little parcels of air that (as we formerly obſerved) are wont to be diſperſed in liquors, without excluding ſpirit of vinegar: but having had this ſuſpicion before we tried the experiment, we conveyed our diſtilled vinegar alone into the receiver, and kept it a while there, to free it from its bubbles, (which were but very ſmall) before ever we put the coral into it. It may be ſuſpected likewiſe, that the agitation of the liquor, neceſſarily following upon the ſhaking of the glaſs, by pumping, might occaſion the recited ebullition; but upon trial made, there appeared not any notable change in the liquor, or its operation, though the containing veſſel were ſhaken, provided no air were ſucked out of it. The former experiment was another time tried in another ſmall receiver, with coral groſly powdered, and the ſucceſs was very much alike, ſcarce differing in any thing, but that the coral being reduced to ſmaller parts, upon the ebullition of the liquor, ſo many little lumps of coral would be carried and buoyed up by the emerging bubbles, as ſometimes to darken the phial, though the ſame coralline corpuſcles would be let fall again upon the letting in of the air.

SOMETHING alſo we tried in our great receiver, concerning the ſolution of metals in aqua fortis, and other corroſive liquors; but partly the ſtink, and partly ſome accidents, kept us from obſerving any thing peculiar and remarkable about thoſe ſolutions.

ONE thing we muſt not omit, that when the ſpirit of vinegar was boiling upon the coral, we took off the cover of the receiver, and took out the phial, but could not find, that, notwithſtanding ſo very late an ebullition, the liquor had any heat great enough to be at all ſenſible to our hands.

EXPERIMENT XLIII.

WE will now ſubjoin an experiment, which, if the former did not leſſen, the wonder of it would probably appear very ſtrange to your Lordſhip, as it did to the firſt ſpectators of it.

THE experiment was this: we cauſed water to be boiled a pretty while, that by the heat it might be freed from the latitant air, ſo often already taken notice of in common water. Then almoſt filling with it a glaſs phial, capable of containing near four ounces of that liquor; we conveyed it, whilſt the water was yet hot, into one of our ſmall receivers (big enough to hold about a pound of water) and having luted on the cover, we cauſed the air to be drawn out. Upon the two firſt exſuctions, there ſcarce appeared any change in the liquor, nor was there any notable alteration made

made by the third; but at the fourth, and afterwards, the water appeared to boil in the phial, as if it had ſtood over a very quick fire; for the bubbles were much greater than are uſually found upon the ebullition of very much more water than was contained in our phial. And this effervefcence was ſo great in the upper part of the water, that the liquor boiling over the top of the neck, a pretty deal of it ran down into the receiver, and ſometimes continued (though more languidly) boiling there. Profecuting this experiment, we obſerved, that ſometimes, after the firſt ebullition, we were reduced to make divers exſuctions of the air, before the liquor would be brought to boil again. But at other times, as often as the key was turned to let the air paſs from the receiver into the pump, the effervefcence would begin afreſh, though the pump were plied for a pretty while together; which ſeemed to argue, that the boiling of the water proceeded from hence, that upon the withdrawing the preſſure of the incumbent air, either the fiery corpuſcles, or rather the vapours agitated by the heat in the water (which laſt, what we have formerly noted touching the rarefied water of an æolipile, manifeſt to be capable of an elaſtical power) were permitted to expand themſelves mightily in the evacuated receiver; and did, in their tumultuous dilatation, lift up (as the air is wont to do) the uppermoſt part of the water, and turning it into bubbles, made the water appear boiling. This conjecture was farther confirmed by theſe additional circumſtances: firſt, the effervefcence was confined to the upper part of the water, the lower remaining quiet, unleſs the liquor were but ſhallow. Next, although ſometimes (as is already noted) the ebullition began again, after it had ceaſed a pretty while, which ſeemed to infer, that ſome concurrent cauſe (whatever that were) did a little modify the operation of heat; yet, when the water in the phial could by no pumping be brought to boil any more, the ſelf-ſame water, being in the very ſame phial warmed again and reconveyed into the pneumatical glaſs, was quickly brought to boil afreſh, and that vehemently and long enough; not to mention, that a new parcel, taken out of the ſame parcel of the boiled water, with the former, and put in cold, could by no pumping be brought to the leaſt ſhew of effervefcence. Beſides, having tried the experiment in hot ſallad-oil, being a much more tenacious liquor, and requiring a ſtronger heat to make it boil, it could not be brought to an effervefcence in our receiver; whereas the chymical oil of turpentine, being thinner and more volatile, was preſently made to boil up, till it reached four or five times the former height in the phial, in whoſe bottom it lay, and continued boiling till it was almoſt reduced to be but lukewarm. Wine alſo being a more thin and ſpirituous liquor than water, being conveyed in hot, inſtead of the oil, did, as I remember, at the very firſt exſuction begin to boil ſo vehemently, that, in a ſhort time that the pump was kept moving, four parts of five, by our gueſs, boiled over out of the phial, though it had a pretty long neck. On which occaſion we will add, that even the water itſelf, near one half, would ſometimes boil over into the receiver before it became lukewarm. And it was alſo remarkable, that once, when the air had been drawn out, the liquor did, upon a ſingle exſuction, boil ſo long with prodigiouſly vaſt bubbles, that the effervefcence laſted almoſt as long as was requiſite for the rehearſing of a *pater noſter*. Now the experiment having been tried more than once, and found to ſucceed as to the main, ſeems much to countenance the conjecture we made at the beginning of this letter, where we told your Lordſhip, that perhaps the preſſure of the air might have an intereſt in more phænomena than men have hitherto thought. For as we had not then made this experiment, ſo now we have made it, it ſeems to teach, that the air, by its ſtronger or weaker preſſure, may very much modify (as the ſchoolmen ſpeak) divers of the operations of that

vehement and tumultuous agitation of the ſmall parts of bodies, wherein the nature of heat ſeems chiefly, if not ſolely, to conſiſt. Inſomuch that if a heated body were conveyed above the atmoſphere, it is probable that the heat may have a differing operation, as to the power of diſſipating the parts of it, from what it hath here below.

To conclude; this experiment might have been farther proſecuted, but our want of leiſure makes us content ourſelves to add at preſent, that perhaps it would not be loſt labour if this were tried, not only with other liquors, but with variety of heated, and eſpecially ſoft or melted bodies: but in ſuch caſes the receiver ought to be ſhaped, as is moſt proper to preſerve the cement wherewith the cover muſt be faſtened on, from being melted by the heat of the included matter; the inconvenience to be hereby avoided, having befallen us in the uſe of a receiver too ſhallow, though otherwiſe capacious enough.

The CONCLUSION.

BEING come thus far, my dear Lord, not without thoughts of proceeding farther; the unwelcome importunity of my occaſions becomes ſo prevalent, that it quite hinders, for the preſent, my deſigned progreſs; and reduceth me, not only to reſerve for another opportunity that kind of experiments, which, at ſome diſtance, from the beginning of this letter, I called (as your Lordſhip may remember) experiments of the ſecond ſort; but to leave uneſſayed ſome of the firſt ſort, which I might try in the engine, as it now is, were it not that my avocations are grown ſo urgent, for my remove from the place where the engine was ſet up, that I am put to write your Lordſhip this excuſe: weary, and in an inn which I take in my way to my dearest brother *Corke*; who being at length arrived in *England*, after I have for divers years been deprived of his company, and wiſhed for it as long; whatever my other occaſions may be, my firſt buſineſs muſt be to wait on him and your excellent mother; in whoſe grateful company I may hope to forget a while thoſe public calamities that diſtreſs this too unhappy nation; ſince that is endeared to me, both by their perſonal merit; by the near relation which nature gives me to him, affinity to her, and friendſhip to both; and alſo by their many favours, eſpecially that of my owing them my *Lord of Dungarvan.* But I ſuffer myſelf to be tranſported too far with theſe delightful thoughts; to return therefore to our engine. Though I find this letter is beyond my expectation ſwelled, not only into a book, but almoſt into a volume; yet the experiments already mentioned in it, are ſo far from compriſing all thoſe that may be tried by the help of our engine, that I have not yet been able to try all thoſe, which, preſently occurring to my thoughts, upon my firſt ſeeing the working of it, I cauſed to be ſet down in a catalogue within leſs than half an hour. But I doubt I have but too much cauſe to apprehend that the affairs and other things I complain of, have made it needful for me to apologize, as well for the things I have ſet down, as for thoſe I am neceſſitated to omit. For as partial as men uſe to be to the children of their own brains, as well as to thoſe of their loins, I muſt not deny that the foregoing trials are not altogether free from ſuch unaccurateneſſes, nor the recital of them from ſuch imperfections, as I myſelf can now diſcern, and could, perhaps, partly mend, if I had the leiſure to repeat the experiments, with the circumſtances that have ſince offered themſelves to my thoughts, as things that might have been worth obſervation or inquiry. But the truth is, that I was reduced to make theſe experi-

experiments, when my thoughts had things that more concerned me to employ them, and the ſame avocations made me ſet them down, for the moſt part, as ſoon as I had made them, and in the ſame order, and that ſo faſt, that I had not over frequently the opportunity to mind any more than the bare truth of what I ſet down; without allowing it any of thoſe advantages that method, ſtyle, and decent embelliſhments, are wont to confer on the compoſures they are employed to adorn.

BUT, my Lord, though to invite and encourage you and your learned friends at *Paris*, to make a farther uſe of this engine, than I have yet been able to do, I am thus free to acknowledge the imperfections of the foregoing letter; yet, if ſome intelligent perſons miſtake not, by what hath been done, ſuch as it is, there is a way opened, whereby ſagacious wits will be aſſiſted to make ſuch farther diſcoveries in ſome points of natural philoſophy, as are yet ſcarce dreamed of. And I am the more deſirous to engage you to that employment, becauſe I am apt to think, that if the making and writing of ſuch experiments ſhall coſt you as much trouble as they have me, you will be inclined to excuſe me; and if the diſcoveries give you as much pleaſure as they gave me, you will (perhaps) be invited to thank me. However, I think (my Lord) I may juſtly pretend, that the things I have ſet down have been faithfully recorded, though not elaborately written: and, I ſuppoſe, my former papers may have long ſince ſatisfied you, that though many deviſe experiments better than your ſervant, none perhaps hath related them more carefully and more truly: and particularly of theſe, ſometimes one, ſometimes another, hath been performed in the preſence of perſons, divers of them eminent for their writings, and all for their learning. Wherefore, having in the foregoing narratives made it my buſineſs to ennoble them with the chief requiſites of hiſtorical compoſures, candour and truth, I cannot deſpair that you will either excuſe their imperfections, or, at leaſt, forgive them: eſpecially, conſidering that this unpoliſhed letter is as well a production of your Lordſhip's commands and my obedience, as a teſtimony of my deſire to make others beholden to my Lord of *Dungarvan*, by the ſame way which I endeavour to expreſs myſelf,

Beconsfield, this 20th of *December*, 1659.

His Lordſhip's

Moſt obedient Servant, and

Moſt affectionate Uncle,

ROBERT BOYLE.

A

DEFENCE

OF THE

DOCTRINE

TOUCHING THE

SPRING and WEIGHT of the AIR,

Proposed by Mr. *R. BOYLE*, in his NEW PHYSICO-MECHANICAL EXPERIMENTS;

AGAINST THE OBJECTIONS OF

FRANCISCUS LINUS.

Wherewith the OBJECTOR'S FUNICULAR HYPOTHESIS is also examined.

The PUBLISHER *to the* READER.

FRIENDLY READER,

YOU may possibly in this volume have expected the Appendix which the author heretofore promised, and has intended shall contain some additional experiments to those which were formerly published, and are here now reprinted in this second edition. These following answers to *Franciscus Linus* and Mr. *Hobbs* are presented in compensation of the delay, and for your forbearance of that Appendix, which ere long you may expect in kind. For the author having hinted the promise, seems thereby to acknowledge the debt, and to be content to continue the obligation to see it performed. And these ought the rather to be his excuse, because the writing these answers, and publishing the *Sceptical Chymist*, and some other discourses, have been the principal hindrances to that piece; which is really so near a readiness, that part of it has lain at the press these six months: but yet it being not all perfected, the Stationer was loth to delay any longer the publication of these, for which he has been so frequently called upon. And they (though a Latin edition is intended) appear now the rather in English, that they may accompany the second edition of the original experiments, which were printed first in that language in octavo;

and that, instead of the promised Appendix, they may complete the bulk of the quarto volume.

As for that part of this piece that concerns Mr. *Hobbs*, it might have been larger: but the informarion that the author had, that the learned Dr. *Wallis* was writing against some passages in Mr. *Hobbs* his Dialogues (as well that concerning the air, as the rest) was the occasion why his H. would make no animadversion on some passages therein, and thought it not fit to enlarge upon others. And for the Errata of the press, I hope they will not be many: however, the author as to these is to be excused, who never (in regard to his eyes and impediments on other occasions) gives himself the trouble of corrections and revises; neither could the publisher much attend the press, it being printed in a distant place from his usual abode. If, as I wish, you shall find this jealousy of mine to have been causeless, you will have reason to give the piece that is so kindly offered, and leads you such rare and untrodden paths in the best way of Natural Philosophy, the fairer entertainment and acceptance. Farewel.

RO. SH.

The AUTHOR'S PREFACE *and* DECLARATION.

THEY that know how indisposed I naturally am to contentiousness, will, I presume, wonder to see me publickly engaged in two controversies at once. But that I am still as averse as ever from entering into disputes that may handsomely be declined, the way wherein I have managed the following controversies will, I hope, evince. And the inducements I now have to appear in public are such, that it would be hard for me to resist the being prevailed on by them.

FOR, in the first place, I was (by name, as it were) challenged by a person, who undertook to disprove not one or two of my conjectures, but as much of the whole body of my treatise as concerned the spring of the air, which most of my explications suppose. And this being done by a learned man, who writes very confidently of the goodness of his hypothesis and arguments, and his book being soon after followed by another written by Mr. *Hobbs*, a man of name in the world; there seemed to be some danger that so early an opposition might oppress the doctrine I had proposed, before it was well understood and duly pondered. Wherefore I feared I might be wanting to the truth and myself, if I should at such a time be altogether silent; especially since I might probably divert many who would otherwise be forward to appear against us, by letting them see how defensible our doctrine is, even against such adversaries as those I have replied to. And this course I the rather chose, that in case I should henceforward comply with those who would have me forbear to write any further of these controversies, it might not be presently inferred from my silence, that a good cause cannot enable a pen no better than mine to defend it.

BUT I scarce doubt but that intelligent readers, especially those that are imbued with the principles of the Corpuscularian philosophy, will be much more apt to think that I had reason to write the following discourses, than to think that I had any to make them so prolix: and especially ingenious men, that are accustomed to admit nothing that either is not intelligible, or is precarious, will think divers of the objections I reply to, have needed no answers, or at least no solemn ones. But to these I have four things to represent,

AND first, that which not a little swells the bulk of the following treatises, is the inserting those passages of my adversaries that I examine in their own words: which being

being a practice that I expect from any that shall think fit to animadvert upon any opinion or argument of mine; I thought it but equitable to do what I desired to have done to me, though oftentimes I could not do it in a little room.

Next, I was the more willing to prosecute some of *Franciscus Linus* his objections, because the fear of being reduced to grant a vacuum has so prevailed with many eminent persons bred up in the received philosophy of the schools, that though they disagree both with him and among themselves about the particular manner of solving the phænomena of the Torricellian experiment; yet they agree in ascribing them to some extremely rarefied substance, that fills up the space deserted by the quicksilver. So that this opinion, as to the main, being approved of by many eminent scholars, especially of that most learned order of the Jesuits (to whom perhaps its congruity to some articles of their religion chiefly recommends it) I was willing to pay them that respect, as not to dissent from persons, divers of whom for their eminence in mathematics and other learning I much esteem, without shewing that I do it not but upon considerations that I think weighty.

Thirdly, though the examiner's hypothesis have but few, and not very considerable arguments, to countenance it; yet his objections against our doctrine (the reply to which takes up the first part of the following treatise) are such, as though they may be solidly answered by any that thoroughly understands our hypothesis, yet they may chance to puzzle such readers as do not, and these possibly will prove more than a few.

And lastly, because that sometimes when the argument objected did not perhaps deserve to be much insisted on, the argument treated of deserved to be considered; I thought it not amiss to make use, now and then, of some such opportunities to illustrate the matter itself under consideration: which I the rather did, for these two reasons:

First, because I find that, except by some able mathematicians and very few other contemplative men, the doctrine of the spring of the air, at least as I have proposed it, is not yet sufficiently apprehended (and therefore needs to be inculcated). Insomuch that through a great part of some late discourses of men otherwise eminently learned (written against other Elaterists, not me) there seems to run so great and clear a mistake, perhaps for want of skill in the Hydrostaticks, that I can scarce impute it to any thing but to their not thoroughly understanding the hypothesis they would confute.

And next, because I was willing to lay down in my answer to the objections I examined, the grounds of answering such other arguments as may be built upon the same or the like principles. And perhaps I may truly enough say, that in the following treatise I have already, in effect, answered several discourses, written some before and some since mine, by learned men, about the Torricellian and other new experiments relating to a vacuum, though I forebore to mention the names or words of the authors, because I found not that my writings or experiments were as yet known to them. To these things I may add, that I thought the discourses of *Linus* the fitter to be insisted on, because he seems to have more diligently than some others (who yet venture to dispute against it) inquired into our doctrine. And I shall not scruple to say thus much of an adversary (and one to whom I gave no provocation to be so) that though I dare not speak in general of those that have written, either about the weight of the air, or else for or against a vacuum, because (as I acknowledge in the first chapter following) I cannot yet procure the books of divers learned men, especially of those great personages, *Robervall*, *Balianus* and *Casatus*; yet among the writers I have hitherto met with, who have recourse to the Aristotelian rarefaction and conden-

condenſation in the controverſies under debate, ſcarce any ſeems to have contrived his hypotheſis better than our *Linus*. Not that I think his principle is either true or (at leaſt to ſuch as I) intelligible; but that the Funiculus he aſſumes being allowed him, he may, for a reaſon to be touched a little below, make out, though not all the phænomena of my experiments, yet many more of them than moſt other Pleniſts, that deny the ſpring of the air, can deduce from their hypotheſis if granted. And in regard that, whereas we aſcribe to the air a motion of reſtitution outwards, he attributes to it the like motion inwards, it cannot but happen that, though the principles cannot both be true, yet many of the phænomena may be explicable by which of them ſoever is granted: becauſe of this, I ſay, it is not ſo eaſy as many ingenious readers may be apt to think, to draw pertinent objections from experience againſt the adverſary I have to deal with. Which I repreſent, leſt, as ſome may think, I have employed more arguments than I needed, ſo others ſhould think I have omitted many, as indeed I have omitted ſome, that I might pertinently have employed.

But there is another ſort of perſons beſides thoſe I mentioned at the beginning of this preface, to whom I muſt addreſs the remaining part of it; namely, to thoſe who ſeem troubled, that I ſuffer myſelf to be diverted either by *Linus* or Mr. *Hobbs* from perfecting thoſe experimental treatiſes that are lying by me, almoſt promiſed by the learned publiſher of the Latin edition of my eſſays; and from proſecuting thoſe ways of inquiry into the nature of things, wherein they are pleaſed to think I may be more ſerviceable to real learning and the lovers of it. And I confeſs that theſe men's reaſons and perſuaſions have ſo far prevailed with me, that after what I have done in the two following treatiſes, to vindicate my writings from the objections made againſt them by two learned men of very differing hypotheſes, and thereby to ſhew in ſome meaſure, that I am not altogether unacquainted with the way of defending oppoſed truths, I have laid aſide the thoughts of writing any more diſtinct or entire polemical treatiſes about the ſubjects already diſputed of. And to this I am invited by ſeveral other reaſons (beſides what I have newly intimated).

About the hiſtory of flame, of heat, of colours, of the origin of qualities and forms, &c.

For firſt, as I elſewhere declare, it was not my chief deſign to eſtabliſh theories and principles, but to deviſe experiments, and to enrich the hiſtory of nature with obſervations faithfully made and delivered; that by theſe and the like contributions made by others, men may in time be furniſhed with a ſufficient ſtock of experiments, to ground hypotheſes and theories on. And though, in my *Phyſico-Mechanical Epiſtle* and my *Specimens* I have ventured ſome conjectures alſo as the cauſes of the phænomena I relate, leſt the diſcourſe ſhould appear to inquiſitive readers too jejune; yet (as I formerly ſaid) I propoſed my thoughts but as conjectures deſigned (though not only, yet chiefly) to excite the curioſity of the ingenious, and afford ſome hints and aſſiſtance to the diſquiſitions of the ſpeculative. And accordingly I have not forborn to mention divers things, which judicious readers may eaſily perceive I foreſaw, that many would think unfavourable to the opinions I inclined to. So that for me to leave experimental for controverſial ſtudies, were a courſe unſuitable to the principal ſcope of my writings.

Next, though I have adventured to improve the doctrine of the ſpring and weight of the air, by ſome ſupplements where I found it deficient, and to recommend it by ſome new illuſtrations and arguments deduced from my experiments; yet the hypotheſes themſelves (for the main) being the opinions alſo of far learneder men than I, it might be thought injurious both to them and to our common cauſe, if I ſhould needleſly go about to hinder them from the honour of vindicating the truths we agree in; eſpecially ſome of them being excellent Mathematicians, and others eminent

Naturalists, whose concern to maintain the hypotheses against objections, if any shall arise, is equal to mine, and whose leisure and abilities far exceed those of a person who both is sickly, and hath other employments enough, and who (if he were far better skilled in geometry than he pretends to be) hath such a weakness in his eyes, as makes him both unwilling and unfit to engage in any study where the conversing with mathematical schemes is necessary.

THIRDLY, nor do I see much cause to doubt that the things I have delivered will, notwithstanding my silence, be left undefended: the forwardness I have already observed in divers Virtuosi to vindicate those writings, which they are pleased to say have convinced them, and to save me the labour of penning the following treatises, scarce permitting me such an apprehension; especially since there are some things that will much facilitate their task, if not keep men from putting them upon it. For though Mr. *Hobbs* and *Linus* have examined my writings upon principles wherein they differ as much from each other as from me; yet neither have they seen cause to deny any thing that I deliver as experiment, nor have their objections been considerable, whether as to number or to weight, against the applications I have made of my principles to solve the phænomena. So that usually, without objecting any incongruity to my particular explications, they are fain to fall upon the hypotheses themselves: in whose defence I think I may with the more reason expect to be seconded, because not only I have endeavoured, as I formerly noted, to lay the grounds of answering such objections as I foresaw might arise; but I have also, to prevent or ease their labour, written the two first parts of my defence against *Linus*, without being obliged to do so for the vindicating of my explications, which are particularly maintained in the third part.

I KNOW not whether I may venture to add on this occasion, that those who have taken notice of the usefulness of experiments to true philosophy, and have observed that nevertheless the difficulty, trouble and charge of making them is such, that even in this learned age of ours, there are very few *Bacons* or *Mersennuse*'s to be met with, and those who have either made themselves, or at least seen others make experiments, even such as those I have published, with the care I am wont to think myself obliged to employ on such occasions, will perhaps not only believe that they cost me far more time and pains than they that have not made nor seen such trials, are apt to imagine; but will possibly think it enough for a person, that is not by profession a scholar, to make them carefully and set them down faithfully, and will allow him to let others vindicate the truths he may have the good fortune to discover; especially, when there are so many fitter for it than he, who have (as well as his adversaries) more leisure to write disputations than opportunity to prosecute experiments; the latter of which, to be performed as it ought to be, doth, in many cases, besides some dexterity scarce to be gained but by practice, require sometimes more diligence, and oftentimes too more cost than most are willing, or than many are able to bestow upon them.

To be short, though if any thing very worthy to be taken notice of by me be suggested against any of my chief opinions or explications, I may either take occasion to say somewhat to it elsewhere, or at least have an opportunity to consider it in a review, wherein I may alter, mend, supply, vindicate or retract divers passages of my other writings: yet I would not have it expected that I should exchange a book with every one that is at leisure to write one against a vacuum, or about the air. Which declaration I make, not that I think it will, or ought to hinder any man from making use of his liberty to express a dissent, if he sees cause; but for these two reasons:

THE

The one, that my silence might not injure either the truth or myself, by tempting men to think, that whatever I do not answer, I cannot; but might give unbiassed and judicious readers a caution to allow as little of advantage to the writings of my adversaries upon the account of their being unanswered by me, as if I were no longer in the world. And the other, that I may not hinder those who would reply to such adversaries by leaving them an apprehension that either I may prevent them, or they me. To conclude, I see no cause to despair, that whether or no my writings be protected, the truths they hold forth will, in time, in spite of opposition, establish themselves in the minds of men, as the circulation of the blood, and other, formerly, much contested truths, have already done. My humour has naturally made me too careful not to offend those I dissent from, to make it necessary for any man to be my adversary upon the account of personal injuries or provocations. And as for any whom either judgment or envy may invite to contend, that the things I have communicated to the world deserved not so much applause as they have had the luck to be entertained with; that shall make no quarrel betwixt us: for perhaps I am myself as much of that mind as he; and however I shall not scruple to profess myself one of those who is more desirous to spend his time usefully, than to have the glory of leaving nothing that was ever written against him unanswered; and who is more solicitous to pursue the ways of discovering truth, than to have it thought that he never was so much subject to human frailties as to miss it.

A Defence of Mr. Robert Boyle's Explications of his *Physico-Mechanical Experiments*,

AGAINST

FRANCISCUS LINUS.

PART I.

Wherein the Adversary's Objections against the Elaterists are examined.

CHAP. I.

A NEWLY-published treatise, *De corporum inseparabilitate*, being brought to my hands, I find several chapters of it employed to oppose the explications I ventured to give of some of my new experiments, touching the spring of the air. Wherefore, though I am very little delighted to be engaged in controversies, and though I be not at present without employments enough (of a private, and of a public nature) to make it unseasonable for me, to be by a work of this sort diverted

from them; yet for the reasons specified in the preface, I hold it not amiss to examine briefly what is objected against the thing I have delivered: and the rather, partly, because the learned author, whoever he be (for it is the title-page of his book that first acquainted me with the name of *Franciscus Linus*) having forborn provoking language in his objections, allows me in answering them to comply with my inclinations and custom of exercising civility, even where I most dissent in point of judgment. Besides, the author himself has somewhat facilitated my reply to him, by directing me in the ninth page to some books and passages that I had not, when I published my epistle, either seen or taken notice of. As indeed there are besides some of these several other discourses that treat of the Torricellian experiments, which, though by the names of their authors I guess to be learnedly written, I have not to this day had opportunity to peruse; my stay in the remoter part of *Ireland* (whither philosophical books were not, in that time of public confusion, brought) having kept me from hearing of divers of them, till they were all bought up. Which I here mention, to excuse myself if I have not taken notice of some things or passages to be met with in these writings, which their learned authors or inquisitive readers might justly perhaps expect I should take some notice of, in case those writings had fallen into my hands. But to digress no further.

It is true indeed, and it somewhat troubles me that it is so, that I can scarce promise myself to make my adversary a proselyte, since he without scruple assumes those very things as principles, that to me seem almost as great inconviences as I would desire to shew any opinion I dislike, to be liable unto. But since whatever operation the following discourse may have upon the person that occasioned it, I hope it may bring some satisfaction to those philosophers who can as little as I understand the Aristotelian rarefaction, and who will as well as I be backward to admit what they cannot understand; it shall suffice me to defend the truths I have delivered, if I cannot be so happy as to convince my acute adversary of them; and I shall not believe my labour lost, if this discourse can contribute to the establishment of some notions in philosophy that I think not inconsiderable in the minds of those whose clear principles make me the most respect their judgments, and for whose sakes I principally write.

Now though I be not in strictness obliged to defend any more than such of my own explications as the examiner has thought fit to question, and those particulars which I have added by way of improvement to the two hypotheses of the spring and weight of the air; yet that I may the more effectually prosecute what I lately intimated I aim at in this writing, and may as well illustrate my doctrine as defend it, I shall divide the ensuing treatise into three parts; whereof the first is designed to answer my adversary's objections against our principles; the second shall examine the funicular hypothesis he would substitute in their stead; and the third shall contain particular replies to what he alleges against some of my particular explications.

CHAP II.

ALTHOUGH our author confesses in his second chapter, that the air has a spring as well as a weight, yet he resolutely denies that spring to be near great enough to perform those things which his adversaries (whom for brevity sake we will venture to call Elaterists) ascribe to it. And his whole fourth chapter, as the title declares, is employed to prove that the spring of the air is unable in a close place to keep the mercury suspended in the Torricellian experiment. The proof of this assertion

assertion he says is easy: but alleges two or three arguments for it, which I think will be more easily answered than his assertion evinced.

IN the first he says, that those experiments concerning the adhesion of one's finger, *&c.* which he had mentioned in the foregoing chapter, *eodem modo se habent in loco clauso ac in aperto.* But the answering of this we shall suspend till anon; partly, because it may then be more conveniently examined; and partly, because our author seems not to build much upon it, his chief argument being that which he proposes in these words: *Cum tota vis hujus Elaterii pendeat à refutato jam aëris æquipondio cum* Page 20. *digitis* $29\frac{1}{2}$ *argenti vivi, ita ut nec plus, nec minus faciat hoc elaterium in loco occluso, quam fit per illud æquipondium in loco aperto; manifestum est, cum jam ostensum sit fictitium planè esse hujusmodi æquipondium, fictitium quoque esse tale elaterium.* 'Being the whole power 'of the spring of the air depends upon the æquilibrium of its weight with twenty 'nine inches and a half of quicksilver, so that this spring doth neither more nor 'less in a shut place, than is done by that æquilibrium in an open place; it is mani-'fest, seeing we have shewed the æquilibrium to be plainly fictitious and imaginary, 'that the spring ascribed to the air is so likewise.' Wherefore since all the validity of his objection against the spring of the air depends upon his former chapter, wherein he thinks he has disproved the weight of the air; it will behove us to look back into the former chapter, and examine the four arguments which he there proposes. But I must crave leave to vary from his method, and consider the third in the first place, because the removal of that objection will facilitate and shorten the answer to the rest. His third argument therefore is thus set down: *Nam si tubus* Page 160. *viginti tantum digitorum (quo usi sumus in primo argumento) non totus impleatur argento, ut prius, sed spacium aliquod inter digitum superiorem & argentum relinquatur in quo sit solus aër; videbimus subtracto inferiore digito superiorem non solum deorsum trahi, ut prius, sed etiam argentum jam descendere, idque notabiliter, quantum nimirum extendi potest exigua illa aëris particula à tali pondere descendente. Unde si loco illius aëris ponatur aqua, aliusve liquor qui non tam facilè extenditur, descensus nullus erit.*

Hinc, inquam, contra hanc sententiam formatur argumentum: nam si externus ille aër nequeat vel hos viginti digitos argenti à lapsu sustentare, uti jam vidimus, quomodo quæso sustentabit $29\frac{1}{2}$*? Certè hæc nullatenus reconciliari possunt.*

'FOR if a tube but twenty inches long (such as we used in our first argument) be 'not quite filled with quicksilver, as before, but a little space be left betwixt the 'mercury and the finger on the top of the tube, in which air only may abide; we 'shall find that the finger below being removed, the finger on the top will not only 'be drawn downwards, as before, but the quicksilver shall descend also, and that 'notably, *viz.* as much as so small a parcel of air can be extended by such a de-'scending weight. So that if, instead of air, water, or any other liquor which is 'not so easily extended, be put in its place, there will be no descent at all.

'HENCE, I say, against this opinion an argument is framed: for if the external 'air cannot keep up those twenty inches of quicksilver from descending, as we have 'proved; how shall it keep up twenty-nine inches and an half? Assuredly these can 'no way be reconciled.'

BUT to this argument, which he thinks so irreconcilable with his adversary's hypothesis, he has himself furnished them with an answer in these words; *Dices forte ideo argentum in hoc casu descendere, quia deorsum truditur ab aëre illo sese per suum Elaterium dilatante.* 'You will perchance say, that the quicksilver therefore doth in the 'alleged case descend, because it is thrust down by that parcel of air which dilates 'itself by its own spring.' Which answer I think sufficient for the objection, notwithstanding the two exceptions he takes at it.

FOR

For first, whereas he says, that *sic deberet digitus potius à tubo repelli, quam eidem affigi, cum non minus sursum quam deorsum fiat hujusmodi dilatatio:* 'So should the 'finger be rather thrust from the top of the tube, than thereby fastened to it; be-'cause this dilatation must be made as well upwards as downwards:' He considers not, that though the endeavour of the included air to expand itself be at first every way alike, yet the expansion itself in our case must necessarily be made downward, and not upward; because the finger that stops the tube being exposed on the upper parts and the sides to the external air, has the whole weight and pressure of the atmosphere upon it; and consequently cannot be thrust away but by a force capable to surmount that pressure: whereas on the lower side of the included air there is the weight of the whole mercurial cylinder to assist the spring of the air to surmount the weight of the atmosphere, that gravitates upon the restagnant mercury. So that the air included and endeavouring to expand itself, finding no assistance to expand itself upward, and a considerable one to expand itself downward, it is very natural that it should expand itself that way whence it finds less resistance. As accordingly it will happen, till the spring of the air be so far debilitated by its expansion, that its pressure, together with the weight of the mercury that remains suspended, will but counterbalance, not overcome, the pressure of the outward air upon the restagnant mercury. And this explication may be confirmed by this trial that I have purposely made; namely, that if instead of quicksilver you employ water, and leave as before in the tube an inch of air, and then inverting it, open it under water, you will perceive the included inch of air not to dilate itself any thing near (for I need not here define the proportion) half so far as it did when the tube was almost filled with mercury; because the weight of so short a cylinder of water does but equal that of between an inch and an inch and an half only of quicksilver; and consequently the inward air is far less assisted to dilate itself, and surmount the pressure of the outward air by the cylinder of water, than by that of mercury. And as for what our author says, that if instead of air, water or some other liquor be left at the top of the tube, the quicksilver will not descend; the Elaterists can readily solve that phænomenon, by saying that water has either no spring at all, or but an exceeding weak one; and so scarce presses but by its weight, which in so short a cylinder is inconsiderable. Now the same solution we have given of our examiner's objection, gives us also an account why the finger is so strongly fastened to the upper part of the tube it stops; for the included air being so far dilated, that an inch, for example, left at first in the upper part of the tube, reaches twice or thrice as far as it did before the descent of the quicksilver, its spring must be proportionably weakened. And consequently that part of the finger that is within the tube will have much less pressure against it from the dilated air within, than the upper part of the same finger will have from the unrarefied air without. By which means the pulp of the finger will be thrust in (which our author is pleased to call sucked in) as we shall ere long have occasion to declare in our answer to his second argument.

And having said thus much to our author's first exception against the solution he foresaw we would give of his third argument; we have not much to say at present to this second. For whereas he says, *Concipi non posse quomodo aër ille sic se dilatet, argentumque deorsum trudat, nisi occupando majorem locum: quod tamen hi authores quam maxime refugiunt, asserentes rarefactionem non aliter fieri, quam per corpuscula aut vacuitates.* 'It cannot be conceived how that air should dilate itself, or thrust down the 'mercury, unless by taking up a greater place; which thing these authors are much 'against, asserting that rarefaction can be made no other ways than by corpuscles or 'vacuities.' I wish he had more clearly expressed himself, since as his words are couched

Page 17.

couched I cannot guess what he means, and much less easily discern how they make an argument against his adversaries. For, sure he thinks them not so absurd, as to imagine that the air can dilate itself, and thrust down the mercury, without in some sense taking up more room than it did before: for the very word dilatation, and the effect they ascribe to the included air, clearly imply as much; so that I see not why he should say that they are so averse from granting the air to take up more place than before, especially since he takes notice in the former chapter, that we compare the expansion of the air to that of compressed wool; and since he here also annexes, that we explicate rarefaction either by corpuscles or vacuities. But this latter clause makes me suspect his meaning to be, that the Elaterists do not admit that the same air may adequately fill more of place at one time than at another; which I believe to be as true as that the self-same lock of compressed wool has no more hairs in it, nor does adequately fill more place with them, when it is permitted to expand itself, than whilst it remained compressed. But against this way of rarefaction our author here has not any objection, unless it be intimated in these words: *Concipi non potest:* which if it be, I shall need only to mind him in this place, that whereas many of the chiefest philosophers, both of ancient and our own times, have professed they thought not the Aristotelian way of rarefaction conceivable; and he acknowledges (as we shall see anon) that it is not clear; what the ablest of his party (the modern Plenists) are wont to object against the way of rarefaction he dislikes, is, that it is not true, not that it is not intelligible.

CHAP. III.

OUR author's second objection (for so I reckon it) is thus proposed by him: *Si sumatur tubus utrinque apertus sed longior, puta digitorum* 40. *argentoque impleatur,* Page 15. *eique digitus supernè applicatur ut prius, videbimus subtracto inferiore digito, argentum quidem descendere usque ad consuetam suam stationem; digitum autem superiorem fortiter intra tubum trahi, eique firmissime, ut prius, adhærere. Ex quo rursum evidenter concluditer, argentum, in sua statione constitutum, non ibidem sustentari ab externo aëre, sed à funiculo quodam interno suspendi, cujus superior extremitas, digito affixa, eum sic intra tubum trahit, eique affigit.* 'If you take a tube open at both ends of a good length, suppose forty 'inches long, and fill it with mercury, and place your finger on the top as before, 'taking away your lower finger, you will find the mercury to descend even to its 'wonted station, and your finger on the top to be strongly drawn within the tube, 'and to stick close unto it. Whence again it is evidently concluded that the mercury 'placed in its own station is not there upheld by the external air, but suspended by a 'certain internal cord, whose upper end being fastened to the finger, draws and fastens 'it after this manner into the tube.' But this argument being much of the same nature with that drawn from his third experiment, the answer made to that and to his first may be easily applied, and will be sufficient for this also; especially because in our present case there is less pressure against the pulp of the finger in the inside of the tube than in the third experiment (where some air is included, though much expanded and weakened;) the pressure of the atmosphere being in the present case kept off from it by the subjacent mercury: whereas there is nothing of that pressure abated against the other parts of the finger that kept it off from the deserted cavity of the tube, save only that from the pulp that is contiguous to the tube, there may be somewhat of that pressure taken off by the weight of the glass itself. But as for that part of the finger which immediately covers the hole, whether or no there be any spring

in its own fibres, or other conſtituent ſubſtances, which finding no reſiſtance in the place deſerted by the quickſilver, may contribute to its ſwelling (for that we will not now examine) he that has duly conſidered the account already given of this intruſion of the pulp into the glaſs, will find no need of our author's internal funiculus; which to ſome ſeems more difficult to conceive, than any of the phænomena in controverſy is to be explained without it.

CHAP. IV.

BY what we have already ſaid againſt our examiner's third argument, we may be aſſiſted to anſwer his firſt, though he propoſe it as a very clear demonſtration; and though it be indeed the principal thing in his book. *Sumatur* (ſays he) *tubus brevior digitis* 29½ *puta digitorum* 20. *non tamen clauſus altero extremo (ut hactenus) ſed utrinque apertus: hic tubus, immerſo ejus orificio argento reſtagnanti, ſuppoſitoque digito, ne effluat argentum tubo infundendum, impleatur argento vivo: aliuſque deinde digitus orificio quoque applicetur, illudque bene claudat. Quo facto, ſi ſubtrahatur inferior digitus, ſentietur ſuperior vehementer trahi ac ſugi intra tubum, tamque pertinaciter ei (vel argento potius, ut poſtea) adhærere, ut ipſum tubum cum toto argento incluſo facilè elevet teneatque in vaſe pendulum.*

Page 12.

Ex quo ſane experimento clariſſimè refellitur hæc ſententia: cum enim, juxta eam, argentum in tubo hujuſmodi 20 *tantum digitorum, ſurſum trudatur à præponderante aëre externo: nunquam profectò per eam explicabitur, quomodo digitus ille ſic trahatur deorſum, tuboque tam vehementer adhæreat; non enim à trudente ſurſum poteſt ſic deorſum trahi.*

'Take a tube ſhorter than twenty-nine inches and an half, for inſtance of twenty digits, not ſhut, as hitherto, at one end, but with both ends open: let this tube, its orifice being immerſed in reſtagnant mercury, and one finger being placed underneath, that the mercury to be poured in run not through, be filled with mercury; and then another finger be applied to its orifice, to cloſe it well: which being done, if you draw away your lower finger, the upper will be found to be ſtrongly drawn and ſucked into the tube, and ſo ſtiffly to adhere to it (or rather to the quickſilver, as I ſhall hereafter ſhew) that it will elevate the tube itſelf with all the quickſilver, and make it continue to hang pendulous in the veſſel.

'FROM which experiment this opinion is moſt clearly refuted: for ſeeing according to it the quickſilver in ſuch a tube but twenty inches long muſt be thruſt upwards by the preponderating air; it will never by it be explained how this finger is ſo drawn downwards, and made ſo ſtrongly to ſtick to the tube. For it cannot by the air thruſting upwards be thus drawn downwards.'

THUS far our author's objection, in anſwer whereunto I have divers things to repreſent, to ſhew that a good account may be given of this experiment in the hypotheſis of the Elateriſts; which is ſufficient to manifeſt how far the argument is from being ſo unanſwerable as the propoſer of it would perſuade his reader.

I DENY then that the finger is drawn downward, or made by ſuction to adhere to the tube; but I explicate that which he calls the ſuction of the finger, as I lately did in anſwer to his third argument, as we ſhall more particularly ſee anon.

HE ſays indeed, that the air which thruſt up the quickſilver cannot ſo ſtrongly draw down the finger. As if the air were not a fluid body, but a ſingle and intire pillar of ſome ſolid matter. But to ſhorten our reply to his objections, the beſt way perhaps will be briefly to explicate the phænomenon thus:

WHEN

WHEN the tube is filled with quickſilver, the finger that ſtops the upper orifice is almoſt equally preſſed above and at the ſides by the contiguous air; but when the lower finger is removed, then the cylinder of mercury, which before gravitated upon the finger, comes to gravitate upon the reſtagnant mercury, and by its intervention to preſs againſt the outward air: ſo that againſt thoſe parts of the finger that are contiguous to the air there is all the wonted preſſure of the outward air; whereas againſt that pulp that is contiguous to the mercury there is not ſo much preſſure as againſt the other parts of the finger, by two thirds. I ſay by two thirds, or thereabout, becauſe the mercurial cylinder in this experiment is ſuppoſed to be twenty inches high; and if it were but a little more than thirty inches high (which is a third more) then the weight of the quickſilver would take off not two thirds only, but the whole preſſure of the outward air, from the above-mentioned pulp of the finger. For in that caſe the quickſilver would quite deſert it, and ſettle beneath it. Wherefore ſince it has appeared by our anſwer to the examiner's third argument, that the preſſure of the outward air is taken off from the body that remains in the upper part of the tube, according to the weight of the liquor ſuſpended in the tube; and ſince in our hypotheſis the preſſure of the outward air is able to keep thirty inches of quickſilver, or two or three and thirty foot of water, ſuſpended in a tube; it need be no great wonder, if a preſſure of the ambient air, equal to the weight of a cylinder of water of near twenty two foot long, ſhould be able to thruſt in the pulp of the finger at the upper orifice of the tube, and make it ſtick cloſely enough to the lip of it.

I KNOW the examiner affirms, that no thruſting or preſſure from without can ever effect ſuch an adheſion of the finger to the tube. But this ſhould be as well proved as ſaid. But firſt, though I am willing to think the examiner would not knowingly relate any thing he is not perſuaded of; yet as far as I, and another perſon very well verſed in theſe experiments, have purpoſely tried, I could not find the adheſion of the finger to the tube to be near ſo ſtrong as our author hath related. Secondly, if you carefully endeavour by preſſure and otherwiſe to thruſt the pulp of your finger into the orifice of the tube, you may through the glaſs perceive it to be manifeſtly tumid in the cavity of the pipe. And if by preſſing your finger againſt the orifice of the tube, you ſhould not make the pulp adhere quite ſo ſtrongly to the tube, nor ſwell quite ſo much within it, as may happen in ſome mercurial experiments; it is to be conſidered, that the air being a fluid as well as a heavy body, it does not (as groſſer weights would) preſs only againſt the upper part of the finger, but preſſing as much of the finger as is expoſed to it almoſt every where, and almoſt uniformly, as well as ſtrongly, it does by its lateral preſſure on every ſide thruſt in the pulp of the finger into the hole, where there is not any reſiſtance at all, or at leaſt near ſo much preſſure againſt the pulp as that of the ambient air againſt the parts of the finger contiguous to it.

BY this it may appear that we need not borrow the objection our author offers to lend us; namely, that in the experiment under conſideration the quickſilver is preſſed downward by the ſpring of ſome air lurking betwixt it and the finger. (Though I am prone to think, that unleſs the experiment be made with a great deal of care, ſuch a thing may eaſily happen, and contribute to the ſtronger adheſion of the finger to the tube.) This I ſay may appear, notwithſtanding what our author objects, that the air expanding itſelf will thruſt away the finger upwards, ſince the contrary of that pretence we have lately manifeſted in the anſwer to his third argument. And as for what he adds, to confirm his argumentation, in theſe words; *Quod vel inde* Page 14.
confirmatur, quia cum præponderans ille aër ſuccedat (uti aſſeritur) loco ſublati inferioris

 digiti,

digiti, id est, eodem modo nunc sustentet argentum quo ante ab applicato digito inferiore sustentabatur; manifestum est, non debere, juxta hanc sententiam, magis deorsum trahi digitum superiorem post sublatam inferiorem quam ante. Cum itaque contrarium planè doceat experientia, satis liquet sententiam illam esse falsam: 'Which is thence confirmed, because 'if that preponderating air succeeds, as is asserted, in the place of the lower finger 'which was withdrawn, that is, if it uphold the quicksilver after the same manner 'which it was upheld by the lower finger applied under it; it is manifest according 'to this opinion, that the finger on the top ought not to be more drawn downwards 'after the lower finger is removed than before. Seeing then that experience teacheth 'the contrary, it is manifest that opinion must be false:' We must consider that the tube being supposed perfectly full of mercury, the finger that stops the lower orifice, is wont to be kept strongly pressed against it, lest any of that ponderous liquor should get out between the tube and the finger. So that although both the lower finger do indeed keep up the mercury in the tube, and the pressure of the outward air would do so too; yet there is this difference, that the pressure of the atmosphere depending upon its weight, cannot be intended and weakened as we please, as can that of the undermost finger. And therefore whereas the atmospherical cylinder will not keep up a cylinder of quicksilver of above thirty inches high, those that make the Torricellian experiment do often, upon one occasion or other, keep up with the finger a mercurial cylinder of perhaps forty or fifty inches, or far more: so that whereas in our case, before the removal of the undermost finger, the pulp of the uppermost must have about the same pressure against it where it is contiguous to the mercury, as there is against the other part of the same finger; after the removal of the undermost finger, there is as much of the atmospherical pressure, if I may so speak, taken off from the newly mentioned pulp, as counter-balances a cylinder of quicksilver of twenty inches long.

CHAP. V.

Page 18. THE examiner's fourth and last experiment is thus proposed: *Quarto denique* (says he) *impugnatur; quia ex eo sequetur, argentum vivum per similem tubum e vasculo exugi posse eâdem prorsus facilitate qua ex eodem exugeretur aqua: quod tamen expte rientiæ repugnat, quâ docemur aquam in os sugentis facillimè attrahi; quo tamen argentum vivum ne toto quidem adhibito conatu perduci queat, imo vix ad tubi medietatem.*

Sequelam autem sic ostendo: Quia cum in hac sententia nihil aliud agendum sit quam hoc, ut per tubum sic ascendat subjectus liquor, sive aqua fuerit, sive argentum, nisi ut sugendo sursum trahatur aër tubo inclusus, quo sic attracto ascendit illico subjectus liquor, protrusus nimirum ab externo aëre jam præponderante (uti docet Pecquettus *in dissertatione anatomica pag.* 63.) *manifestum est, eadem planè facilitate exugendum sic argentum vivum qua exugitur aqua: quod quum experientiæ tam aperte repugnat, necesse est sententiam ex qua sequitur falsam esse.*

'In the fourth place, it is impugned, because thence it would follow that quick-'silver through a like tube might be sucked with the same easiness out of a vessel 'that water is sucked out of the same. Which notwithstanding is contrary to ex-'perience, by which we are taught that water is easily drawn into the mouth of him 'that sucks; whereas quicksilver cannot be drawn thither by his utmost endeavour, 'nay scarce unto the middle of the tube.

'The sequel I thus manifest: because seeing, according to this opinion, that the 'liquor underneath, whether it be water or mercury, may so ascend, no more is 'required but that the air shut in the tube may be drawn upwards by sucking; which 'being

'being drawn up, the liquor underneath will immediately ascend, being thrust thither 'by the external air now preponderating (as *Pecquet* declares in his Anatomical Dis-'course, *p.* 63.) it is manifest that the mercury may be sucked out with the same 'easiness that water is sucked out with. Which being so evidently against experience, 'the opinion from whence it is deduced must needs be false.'

THIS experiment I remember I made some years ago; accordingly, it is alledged in the fourth essay of the treatise (I was then writing) to prove against the vulgar opinion, that liquors do not, to prevent a vacuum, spontaneously ascend; which I presume will be so far allowed of by our author, who would have liquors supposed to be raised by suction violently drawn up by the contraction of his funiculus. But to examine this experiment, as it concerns the present controversy, we may recall to mind that we formerly shewed in the answer to our author's third argument, that when the mercurial cylinder that leans upon the restagnant mercury has at the other end of it air, kept from any intercourse with the atmosphere, that included air has so much of the pressure of the external air taken off from it, as counterpoises the mercurial cylinder. And the finger that is exposed to the whole pressure of the ambient air in some of its parts, and in others but to the much fainter pressure of the included air, endures an unusual pressure from the preponderating power of the atmosphere.

WE may consider also, that there is against the thorax, and those muscles of the abdomen that are subservient to respiration, the pressure of the whole ambient air. Which pressure notwithstanding, the muscles designed for the use of respiration are able, without any considerable resistance, to dilate the thorax at pleasure; because, as fast as they open the chest, and by dilating it weaken the spring of that air which is then within the body, the external air by flowing in, for want of finding the usual resistance there, keeps that within the thorax in an æquilibrium of force with that without. These things premised, it is not difficult in our hypothesis to give an answer to our examiner's experiment. For we say when a cylinder of mercury is raised in the tube to any considerable height, the pressure of the air in the thorax is lessened by the whole weight of that mercurial cylinder; and consequently the respiratory muscles are thereby disabled to dilate the chest as freely as they were wont, by reason of the prevalency of the undiminished pressure of the external air against the weakened pressure of the internal. But if instead of mercury you substitute water, so short a cylinder of that comparatively light liquor takes off so little of the pressure of the included air, that it comes into the lungs with almost its usual strength, and consequently with almost as much force as the outward air presses with against the thorax.

AND on this occasion there occurs to my thoughts a noble experiment of the most ingenious Monsieur *Paschal*, which clearly shews, that if we could free the upper part of such a tube as we are now considering from the pressure of all internal air, it would follow, as the examiner says it should, that the quicksilver would by the pressure of the outward air be impelled up into the tube as well as water, till it had attained a height great enough to make its weight not inferior, but equal to that of the atmosphere. The experiment itself being so pertinent and considerable, we shall annex it in the same words wherein it is related by his countryman and acquaintance, the learned and candid *Gassendus: Neque hoc verò solum, sed insuper vitreo diabete clysteréve ea qua par fuerit longitudine confecto, & post embolum ad orificium usque compulsum, immisso ad normam in subjectum hydrargyrum, deprehendit, ubi embolum sensim deinde educitur, consequi hydrargyrum ascendereque ad eandem usque duorum pedum & digitorum trium cum semisse altitudinem.* 'And not only this, but over and above, if a glass diabetes

Gass. Phys. Sect. 1. lib. 2 p. 204. De supero Inanis experimento.

'or syringe be made of a sufficient length, and after that the sucker is thrust into 'the utmost orifice, it be placed according to use in the mercury underneath; he 'finds that as soon as the sucker is drawn out, the mercury follows, and ascends to 'the same height of two feet and three inches and an half.' To which he immediately subjoins a circumstance very considerable to the present controversy, in the following clause: *Ac ubi deinceps, adhibita licet non majore vi, embolum altius educitur, consistere hydrargyrum, neque amplius consequi, ac fieri interim Inane, quod spatium intercipitur ab ipso ad embolum usque.* 'And when afterwards, although no greater force 'be added, the sucker is drawn higher, he finds that the mercury stands, and fol-'lows no further; and so that space is made empty which remains between the mer-'cury and the sucker.' Thus far he. So that as to the examiner's experiment, we may well explicate it in our hypothesis, by saying, that, agreeably to it, it happens, that in a more forcible respiration the mercurial cylinder is raised higher than in a more languid; because, in the former case, the chest being more dilated, the included air is also more expanded; whereby its debilitated spring cannot as before enable the mercurial cylinder to counterpoise altogether the pressure of the ambient air. And that the reason why the quicksilver is not by respiration raised as high as it is kept suspended in the Torricellian experiment, is not, that the pressure of the outward air is unable to to raise it so high, but because, as we have already declared, the free dilatation of the thorax is opposed by the pressure of the ambient air: which pressure being against so great a superficies, and being but imperfectly resisted by the debilitated pressure of the air within the thorax, will be easily imagined to be very considerable, by him who considers that in our engine the pressure of the external air against the sucker of less than three inches diameter was, as we relate in the 33d experiment, able to thrust up a weight of above a hundred pound. And here we may observe upon the by, in confirmation of our former doctrine, that when we strongly suck up quicksilver in a glass tube, though the elevation of the quicksilver be, according to our author, performed likewise by his Funiculus contracting itself every way, and though there be a communication betwixt the internal surface of the lungs, and the cavity of the tube; yet we feel not in our lungs any endeavour of the shrinking Funiculus to tear off that membrane they are lined with.

And thus we have examined our author's four arguments, to prove that in the Torricellian experiment the quicksilver cannot be kept suspended by the counterpoise of the external air: against which opinion he tells us indeed, that other arguments
Page 19. might be alledged, but as it is not probable that if he had thought them better than those he has elected to insist on, he would have omitted them, so it is not unlikely that answers might be as well found for them as for the others; especially since that which he singles out for a specimen is, that from his adversaries hypothesis it would follow, that the quicksilver would descend much more (I suppose it is a mistake of the press, for much less) in cold weather than in hot, because the air is then thicker and heavier, and therefore ought to impel up the quicksilver higher. For besides that we shall in its due place question the validity of our author's consequence, it will be here sufficient to reply, that the observation on which he grounds it does not constantly hold, as his objection supposes: which may appear by that part of our 18th experiment whence the matter of fact is desumed, as we shall have occasion to take further notice of when we shall come to the defence of that experiment So that what has been hitherto discoursed on both sides being duly considered, the reader is left to judge what ground the examiner had for the ἐπιφώνημα wherewith he is pleased to conclude
Page 19. his third chapter; *Maneat igitur tot argumentis comprobatum, quorum quodlibet se solo sufficit, argentum (facto experimento in loco aperto) per externi aëris gravitatem à lapsu*

minime sustentari. 'Be it therefore confirmed by so many arguments, of which every 'one is sufficient in itself, that quicksilver (the experiment being made in an open 'place) is not upheld from falling by the weight of the external air.'

CHAP. VI.

HIS fourth chapter, wherein the title promises that he will prove, *argentum in loco occluso non sustentari à lapsu per ipsum aëris elaterium*, 'that quicksilver in a 'close space is not upheld from falling by the elater or spring of the air;' is very short, and does not require that we should dwell long upon it. For the proof he brings of his assertion being this, *Cum tota vis hujus elaterii pendeat à refutato jam aëris æquipondio cum digitis 29½ argenti vivi, ita ut nec plus nec minus faciat hoc elaterium in loco occluso quam fit per illud æquipondium in loco aperto; manifestum est, cum jam ostensum sit fictitium planè esse hujusmodi æquipondium, fictitium quoque esse tale elaterium:* 'Seeing the whole power of this spring depends upon the already-confuted æquilibrium 'of the air with 29 inches and an half of quicksilver, so that this spring does neither 'more nor less in a close place than is done by that æquilibrium in an open place; 'it is manifest, seeing this equilibrium is already shewn to be plainly fictitious and 'imaginary, that the spring of the air is so likewise.' This being no new argument, but an inference from those he had set down in the former chapter, by our answers to them it is become needless for us to make any distinct reply to this. We shall rather desire the reader to take notice, that whereas our author says, that according to his adversaries, *Nec plus nec minus faciat hoc elaterium in loco occluso quam fit per illud æquipondium in loco aperto:* 'And that this spring doth neither more nor less in a close place, than is done by that æquilibrium in an open place:' Whatever others may have written, we for our part allow of this opinion but in some cases; for in others we have performed much more by the spring of the air, which we can within certain limits increase at pleasure, than can be performed by the bare weight, which for aught we know remains always somewhat near the same. And of this advantage that the spring of the air may have in point of force above the weight of it, we have formerly given an instance in our 17th experiment (where, by compressing the air in the receiver, we impelled the mercurial cylinder higher than the station at which the counterpoise of the air is wont to sustain it) and shall hereafter have occasion to give yet more considerable proofs. To the lately recited words our examiner subjoins these; *Adde, cum allata jam capite præcedente experimenta de adhæsione digiti, &c. eodem modo se habent in loco clauso ac in aperto, necessarium esse facta ex eis argumenta contra æquipondium, eadem quoque contra elaterium vim habere.* 'Add, that seeing the experiments 'brought in the chapter above of the adhesion of the finger, &c. are alike in a close 'and an open place; it is necessary and certain, that the same arguments made 'against the æquilibrium have force against the spring of the air.' But though he propose this as a new argument, yet since it is built but upon the adhesion of the finger (of which we have already given an account in our hypothesis) I see not how it requires any new and particular answer. And whereas he says, that the experiments he had mentioned concerning the adhesion of one's finger, &c. *eodem modo se habent in loco clauso ac in aperto*; I could wish he had added what way he took to make the trials. For he gives no intimation that he did them any other ways than in ordinary rooms: and in such there scarce ever wants a communication betwixt the inward and outward air, either at the chimney, or window, or door not exactly shut, or

Page 20.

Page 21.

or at some hole or crevice or other, by means of which the weight of the atmosphere has its operation within the room.

To his second argument our author adds not a third, unless we take take that for an argument which he immediately annexes to his last recited words: *Et profectò* (says he) *si secum expenderent hi authores, quanta sit difficultas explicandi hujusmodi aëris elaterium, nisi idem aër se solo occupet majorem locum (ut paulo ante) credo eos sententiam facilè mutaturos.* 'And really if these authors would consider how great a difficulty 'there is in explaining this spring of the air, unless the same air by itself alone may 'take up a greater place, I believe they would readily alter their opinion.' But this being said *gratis*, does not exact an answer; and he must make it more intelligible than any man that I know of has yet done, how the same air can adequately fill more space at one time than at another, before he persuade me to change my opinion about the spring of the air: especially since he himself allows that the air has a spring, whereby it is able, when it has been violently compressed, to recover its due extension; the manner whereof if he will intelligibly explicate, his adversaries will have no great difficulty to make out the spring of the air. But whether his hypothesis, or ours, be the more intelligible, will be more properly considered in the second part of our discourse, to which we will therefore now proceed.

Page 21.

Page 11.

PART II.

Wherein the Adversaries Funicular Hypothesis *is examined.*

CHAP. I.

What is alleged to prove the Funiculus, *is considered; and some difficulties are proposed against the Hypothesis.*

THE hypothesis that the examiner would, as a better, substitute in the place of ours, is, if I mistake it not, briefly this; that the things we ascribe to the weight or spring of the air are really performed by neither, but by a certain Funiculus, or extremely thin substance, provided in such cases by nature, *ne detur vacuum*, which being exceedingly rarefied by a forcible distension, does perpetually and strongly endeavour to contract itself into dimensions more agreeable to the nature of the distended body; and consequently does violently attract all the bodies whereunto it is contiguous, if they be not too heavy to be removed by it.

BUT this hypothesis of our author's does to me, I confess, appear liable to such exceptions, that though I disliked that of his adversaries, yet I should not embrace his, but rather wait till time and further speculations or trials should suggest some other theory, fitter to be acquiesced in than this; which seems to be partly precarious, partly unintelligible, partly insufficient, and besides needless: though it will not be so convenient to prove each of these apart, because divers of my objections tend to prove the doctrine, against which they are alleged obnoxious to more than one of the imputed imperfections.

FIRST,

FIRST, then, the arguments by which our author endeavours to evince his Funiculus, are incompetent for that end. The arguments which he proposes in his sixth chapter (where he undertakes to make good his assertion) I there find to be three.

THE first he sets down in these words; *Constat hoc primò ex jam dictis capite præcedente: nequit enim argentum descendens sic digitum deorsum trahere, tuboque affigere, nisi à tali funiculo suspendatur, eumque suo pondere vehementer extendat, ut per se patet.* 'This 'appears from what has been already spoken in the preceding chapter: for the 'quicksilver descending, cannot so draw the finger downwards, and fasten it unto 'the tube, unless it be hung upon the finger by such a cord, which by its weight 'it vehemently stretches, as is manifest by itself.' But to this proof answer has been made already in the former part of this discourse: only whereas the author seems to refer us to the foregoing chapter, we will look back to it, and take notice of what I find there against the Vacuists. For though I neither am bound, nor intend, in this discourse, to declare myself for or against a vacuum; yet, since I am now writing against the funicular hypothesis, it will much conduce to shew that it is not firmly grounded, if I examine what he here alleges against the assertors of a vacuum. Page 24.

IN the next place therefore, I consider that, according to the examiner, there can be no vacuum; and that he makes to be the main reason why nature in the Torricellian and our experiments does act after so extraordinary a manner as is requisite to the production of his Funiculus. For, in the 47th page, having, in his adversaries name, demanded what need there is, at the descent of the quicksilver, that before it falls, a superficies should be separated from it, and extended; *Respondeo* (says he) *ideo hoc fieri, ne detur vacuum; cum nihil aliud ibi adsit quod loco argenti descendentis possit succedere,* 'I answer, that this comes to pass, that there may be no vacuity, seeing 'there is nothing else there that can succeed into the place of the descending quick-'silver.' To which he immediately subjoins (with what cogency I will not now examine) *Atque hinc planè confirmatur commune illud per tot jam elapsa secula usurpatum in scholis axioma, viz. Naturam à vacuo abhorrere.* 'And hence is confirmed that 'common axiom used in the schools for so many ages past, that nature doth abhor 'a vacuum.' And though he seem to make his Funiculus the immediate cause of the phænomena occurring in the Torricellian and our experiments; yet that, if you pursue the inquiry a little higher, he resolves them into nature's abhorrency of a vacuum, himself plainly informs us in the next page; *Nam licet* (says he) *immediata ratio cur aqua v. g. ex hydria hortulana superne clausa (quo exemplo utuntur) non descendat, non sit metus vacui, sed ea quam modo diximus, nempe quod non detur sufficiens pondus ad solvendum illum nexum quo adhæreat aqua clausæ hydriæ summitati; ad eam tamen rationem tandem necessario veniendum est.* 'For though the immediate cause why 'water (for instance) doth not descend from a gardener's watering-pot, (for that ex-'ample they use) stopt on the top, is not the fear of a vacuum, but the reason now 'mentioned; namely, that there is not weight sufficient to loose that conjuncture 'by which the water doth adhere to the top of the closed water-pot: nevertheless, in 'the end, we must of necessity come to that cause.' But though, as well our author's Funiculus as the other scarce conceivable hypotheses that learned men have devised, to account for the suspension of the quicksilver otherwise than by the resistance of the external air, seem to have been excogitated only to shun the necessity of admitting a vacuum; yet I see not how our examiner cogently proves, either that there can be none *in rerum naturâ*, or that *de facto* there is none produced in these experiments. For, in his fifth chapter (where he professedly undertakes that task) he has but these two incompetent arguments. The first is drawn from the attraction, as he supposes, of the finger into the deserted cavity of the tube in the Torricellian experiment: Page 48.

Page 22. riment: *Quæ quidem* (says he) *tam vehemens tractio & adhæsio, cum non nisi à reali aliquo corpore inter digitum & argentum constitutum queat provenire, manifestum est spatium illud vacuum non esse, sed verâ aliquâ substantiâ repletum.* 'Which traction and adhe-'sion, when it cannot proceed but from some real body placed between the finger 'and the mercury, it is manifest that that space is not empty, but filled with some 'true substance.' But to this argument having already given an answer, let us (without staying to urge, that the Vacuists will perhaps object, that they see not a necessity, though they should admit of traction in the case, that the internal substance must therefore perfectly replenish the deserted cavity; without pressing this, I say, let us) consider his other, which he draws from the diaphaneity of the deserted part of the tube, which space, he says, were it empty, would appear like a little black pillar, *Eo quod nullæ species visuales neque ab eo neque per illud possunt ad oculum pervenire:* 'Because no visual species could proceed either from it, or through it, unto 'the eye.' But (not to engage ourselves in optical speculations and controversies) if we grant him somewhat more than perhaps he can prove; yet as the experiment will not demonstrate that there is nothing of body in any part of the space deserted by the mercury, so neither will the argument conclude (as the proposer of it does twice in this chapter) that space *verâ aliquâ substantiâ repleri*, 'to be filled with any true 'substance.' For, according to the hypothesis of the Epicureans and other Atomists, who make Light to be a corporeal effluvium from lucid bodies, and to consist of atoms so minute as freely to get in at the narrow pores of glass, there will be no cause to deny interspersed vacuities in the upper part of the tube. For the corpuscles of light that permeate that space may be so numerous, as to leave no sensible part of it un-inlightened; and yet may have so many little empty intervals betwixt them, that if all that is corporeal in the space we speak of were united into one lump, it would not perhaps adequately fill the one half (not to say the tenth, or even the hundredth part) of the whole space: according to what we have noted in the 17th experiment, that a room may appear full of the smoke of a perfume, though if all the corpuscles that compose that smoke were re-united, they would again make up but a small pastil. To which purpose I remember I have taken camphire, of which a little will fill a room with its odour, and having in well-closed distillatory glasses caught the fumes driven over by heat, I thereby reduced them to re-conjoin into true camphire, whose bulk is very inconsiderable in comparison of the space it fills as to sense, when the odorous corpuscles are scattered through the free air.

To which I might add, that the Torricellian experiment being made in a dark night, or in a room perfectly darkened, if it succeed (as there is little cause to expect it will not) it may well be doubted whether our author's argument will there take place. For if he endeavour to prove that the place in question was full in the dark, because, upon the letting in of the day, or the bringing in of a candle, the light appears within it, the Vacuists may reply according to their hypothesis, that that light is a new one, flowing from the lucid body that darts its corporeal beams quite through the glass and space we dispute about, which, for want of such corpuscles, were not just before visible.

And supposing light not to be made by a trajection of atoms through diaphanous bodies, but a propagation of the impulse of lucid bodies through them; yet it will not thence necessarily follow, that the deserted part of the tube must be full: as in our 27th experiment (though many of those gross aërial particles that appeared necessary to convey a languid sound were drawn out of our receiver at the first and second exsuction: yet there remained so many of the like corpuscles, that those that were wanting, were not missed by the sense, though afterwards, when a far greater number

number was drawn out, they were) so there may be matter enough remaining to transmit the impulse of light; though betwixt the particles of that matter there should be store of vacuities intercepted. Whereas our author pretends to prove, not only that there is no coacervate vacuity in the space so often mentioned, but absolutely that there is none. For it is in this last sense as well as the other, that the schools, and our author who defends their opinion, deny a vacuum.

BUT notwithstanding what we have now discoursed, as in our 17th experiment, we declined determining whether there be a vacuum or no; so now, what we have said to the examiner's argument, has not been to declare our whole sense of the controversy, but only to shew, that though his hypothesis supposes there is no vacuum, yet his arguments do not sufficiently prove it; which may help to shew his doctrine to be precarious: for otherwise the Cartesians, though Plenists, may plausibly enough (whether truly or no, I now dispute not) decline the necessity of admitting a vacuum in the deserted space of the tube, by supposing it filled with their second and first element, whose particles they imagine to be mute enough freely to pass in and out through the pores of glass. But then they must allow the pressure of the outward air to be the cause of the suspension of the quicksilver: for though the *materia cælestis* may readily fill the spaces the mercury deserts; yet that within the tube cannot hinder so ponderous a liquor from subsiding as low as the restagnant mercury; since all the parts of the tube, as well the lowermost as the uppermost, being pervious to that subtile matter, it may with like facility succeed in whatever part of the tube shall be forsaken by the quicksilver.

The examiner's second argument in the same place is, that since the mercurial cylinder is not sustained by the outward air, it must necessarily be, that it be kept suspended by his internal string. But since, for the proof of this he is content to refer us to the third chapter, our having already examined that, allows us to proceed to his third argument, which is, that the mercurial cylinder resting in its wonted station, does not gravitate; as may appear by applying the finger to the immersed or lower orifice of the tube. Whence he infers, that it must of necessity be suspended from within the tube. And indeed if you dexterously apply your finger to the open end of the tube, when you have almost, but not quite, lifted it out of the restagnant mercury (which circumstance must not be neglected, though our author have omitted it) that so you may shut up no more quicksilver than the mercurial cylinder is wont to consist of, you will find the experiment to succeed well enough (which makes me somewhat wonder to find it affirmed, that the learned *Maignan* denies it) not but that you will feel upon your finger, a gravitation or pressure of the glass tube, and the contained mercury as of one body; but that you will not feel any sensible pressure of the mercury apart, as if it endeavoured to thrust away your finger from the tube. But the reason of this is not hard to give in our hypothesis; for according to that, the mercurial cylinder and the air counterpoising one another, the finger sustains not any sensibly differing pressure from the ambient air that presses against the nail and sides of it, and from the included quicksilver that presses against the pulp. But if the mercurial cylinder should exceed the usual length, then the finger would feel some pressure from that surplusage of quicksilver, which the air does not assist the finger to sustain. So that this pleasant phænomenon may be as well solved in our hypothesis as in the examiner's: in which, if we had time to clear an objection which we foresee might be made, but might be answered too, we would demand why, when the mercury included in the tube is but of a due altitude, it should run out upon the removal of the finger that stops it beneath, in case it be sustained only by the internal Funiculus, and do, according to his doctrine, when the Funiculus sustains it, emu-

late a ſolid body, if the preſſure of the external air has not (as our author teaches it not to have) any thing to do in this matter.

And if ſome inquiſitive perſon ſhall here object, that certainly the finger muſt feel much pain by being ſqueezed between two ſuch preſſures as that of a pillar of thirty inches of quickſilver on the one ſide, and an equivalent preſſure from the atmoſpherical pillar on the other, it may readily be repreſented, that in fluid bodies (ſuch as are thoſe concerned in our difficulty) a ſolid body has no ſuch ſenſe of preſſure from the ambient bodies as (unleſs experience had otherwiſe inſtructed us) we ſhould perhaps imagine. For, not to mention that having inquired of a famous diver, whether he found himſelf ſenſibly compreſſed by the water at the bottom of the ſea; he agreed with the generality of divers in the negative. I am informed that the learned *Maignan* did purpoſely try, that his hand being thruſt three or four palms deep into quickſilver, his fingers were not ſenſible, either of any weight from the incumbent, or of any preſſure from the ambient quickſilver. The reaſon of which (whether that inquiſitive man have given it or no) is not neceſſary, in our preſent controverſy, to be looked after.

Page 25. To theſe three arguments the examiner adds not a fourth, unleſs he deſigns to preſent it us in this concluding paſſage: *Huc etiam faciunt inſignes librationes, quibus argentum ſubito deſcendens agitatur: idem enim hic fit, quod in aliis pendulis & ab alto demiſſis fieri ſolet.* 'And to this purpoſe make thoſe conſiderable vibrations, with 'which quickſilver is ſtirred in its deſcent: for the ſame thing happens here, that 'befalls other pendula in their fall from on high.' But of this phænomenon alſo it is eaſy to give an account in our hypotheſis by two ſeveral ways; whereof the firſt (which is proper chiefly when the experiment is made in a cloſe place, as our receiver is) that the quickſilver by its ſudden deſcent acquires an impetus ſuper-added to the preſſure it has upon the ſcore of its wonted gravity; whereby it, for a while, falls below its ſtation, and thereby compreſſes the air, that leans upon the reſtagnant mercury: which air, by its own ſpring, again forcibly dilating itſelf to recover its former extenſion, and (as is uſual in ſprings) haſtily flying open, expands itſelf beyond it, and thereby impels up the quickſilver ſomewhat above its wonted ſtation; in its fall from whence it again acquires ſomewhat (though not ſo much as before) of impetus, or power, to force the corpuſcles of the air to a ſub-ingreſſion; and this reciprocation of preſſure betwixt the quickſilver and the outward air decreaſing by degrees, does at length wholly ceaſe, when the mercury has loſt the ſuper-added preſſure, which it acquired by its falling from parts of the tube higher than its due ſtation. But this firſt way of explicating theſe vibrations is not neceſſary in the free air: for if we conſider the ambient air only as a weight, and remember what we have newly ſaid of the impetus acquired by deſcent; this phænomenon may be eaſily enough explained, by taking notice of what happens in a balance, when one of the equiponderant ſcales chancing to be depreſſed, they do not till after many vibrations ſettle *in æquilibrio.*

And on this occaſion I ſhall add this experiment: I took a glaſs-pipe, whoſe two legs (very unequal in length) were parallel enough, and both perpendicular to that part of the pipe that connected them (ſuch a ſiphon is deſcribed in our 36th experiment, to find the proportion of the gravity of mercury and water) into this quickſilver was poured till it was ſome inches high, and equally high in both legs: then the pipe being inclined, till the moſt part of the quickſilver was fallen into one of the legs, I ſtopped the orifice of the other leg with my finger, and erecting again the pipe, though the quickſilver were forced to aſcend a little in that ſtopped leg; yet by reaſon my finger kept the air from getting away, the quickſilver was kept lower by a good

good deal in that ſtopped leg than in the other; but if, by ſuddenly removing my finger, I gave paſſage to the included and ſomewhat compreſſed air, the preponderant quickſilver in the other leg would, with the mercury in this unſtopped leg, make divers undulations before that liquor did in both legs come to reſt in an æquilibrium. Of which the reaſon may be eaſily deduced from what has been newly delivered; and yet in this caſe there is no pretence to be made of a Funiculus of violently diſtended air to effect the vibrations of the mercury.

CHAP. II.

Divers other difficulties are objected againſt the Funicular Hypotheſis.

THIRDLY, but though our examiner have not ſufficiently proved his hypotheſis, yet perhaps it may be in its own nature ſo like to be true, as to deſerve to be embraced as ſuch. Wherefore we will now take notice of ſome of thoſe many things, that to our apprehenſion render it very improbable.

AND firſt, whereas our author acknowledges, that quickſilver, water, wine and other liquors, will, as well one as another, deſcend in tubes exactly ſealed at the top, in caſe the cylinder of liquor exceed the weight of a mercurial cylinder of 29½ inches; and will ſubſide no longer than till it is come to equiponderate a cylinder of quickſilver of that height; whereas I ſay, the examiner is, by the ingenious Monſieur *Paſchal*'s, and other experiments, induced to admit this; it cannot but ſeem ſtrange, that, whatever the liquor be, there ſhould be juſt the ſame weight or ſtrength to extend them into a Funiculus: though water, for inſtance, and quickſilver be near fourteen times as heavy one as the other, and be otherwiſe of very diſtant natures; and though divers other liquors, as oil and water, be likewiſe of textures very differing. And this may ſomewhat the more be wondered at, becauſe our author (in his animadverſions upon our 31ſt experiment) is pleaſed to make ſo great a difference betwixt the diſpoſition of bodies of various conſiſtences, as fluid and firm, to be extenuated into a Funiculus, that he will not allow any human force to be able to produce one by the divulſion of two flat marbles, in caſe the contact of their ſurfaces were ſo exquiſite as quite to exclude all air; though in the ſame place his ratiocination plainly enough teaches (which experience however does) that adhering marbles, though with extraordinary difficulty, may be forcibly ſevered, and according to him, the ſuperficial parts may be diſtended into a Funiculus, that prevents a vacuum.

BUT now the hypotheſis of his adverſaries is not at all incumbered with this difficulty. For the weight of the outward air being that, which keeps liquors ſuſpended in tubes ſealed at the top; it matters not of what nature or texture the ſuſpended liquor is, provided its weight be the ſame with that of a mercurial cylinder equiponderant to the aërial one: as if there be a pound of lead in one ſcale, it will not deſtroy the æquilibrium, whether what be put in the other be gold, or quickſilver, or wool, or feathers, provided its weight be juſt a pound.

IN the next place we may take notice, that the account our examiners gives us of his Funiculus in the tenth chapter (where he takes upon him to explicate it) is much more ſtrange than ſatisfactory, and not made out by any ſuch parallel operations of nature, as his adverſaries will not (and may not well do it) diſpute the truth of. Whereas the weight and ſpring of the air may be inferred from ſuch unqueſtioned experiments, as are not concerned in our preſent controverſy. For the gravity of the air may be manifeſt by a pair of ſcales, and the ſpring of it diſcloſes itſelf ſo clearly

in wind-guns and other instruments, that our adversary (as we have already had occasion to inculcate) does not deny it. But to consider his explication of his Funiculus
Page 38. he would have us note two things: first, *Argentum, dum replet totum tubum, non mere tangere ejus summitatem (ut primo aspectu videtur) sed eidem quoque firmiter adhærere. Patet hoc* (subjoins he) *experimento illo in primo argumento capitis tertii de tubo utrinque aperto.* 'Quicksilver, while it fills the whole tube, doth not only touch its top (as you 'would think at the first sight) but doth firmly stick unto it also; as it is manifest 'from the experiment mentioned in the first argument of the third chapter, con-'cerning the tube open at both ends.' But what is to be answered to this proof may be easily gathered from what we have replied to that argument. And to what our author adds to prove, that the adhesion of the finger is to the subjacent mercury,
Page 38. not the tube; namely, that *licet illud tubi orificium oleo, aliâve materiâ adhæsionem impediente, inungatur, non minus tamen firmiter adhærebit digitus quàm priùs:* 'Though 'that orifice of the tube be anointed with oil, or any other matter that will hinder 'adhesion, nevertheless the finger will no less firmly stick than before.' An answer may be drawn from the same place: nor perhaps will his reasoning much satisfy those, who consider, that bodies by trusion may easily enough be made stick together, as much as in our case the tube and finger do, notwithstanding one of them is anointed with oil, and that this adhesion of the finger to the tube is to be met with in cases where the surface of the included quicksilver is not contiguous to the finger, but many inches below. As for what he adds concerning the reason, why water and quicksilver ascend by suction, we have already taught what is to be answered to it, by ascribing that ascension to the pressure of the external air; without any need of having recourse to a Funiculus: or imagining with him in this place, that because nothing besides the water or quicksilver can in such cases succeed the air (which yet
Page 40. is not easy to be proved in reference to a thin æthereal substance) therefore, *Partes ipsius aëris* (to use his expression) *sic tubo inclusæ (quæ aliàs tam facile separantur) nunc tam fortiter sibi invicem agglutinentur, ut validissimam (uti videmus) conficiunt catenam, qua non solum aqua, sed ponderosum illud argentum sic in altum trahatur:* 'That the parts 'of air itself so shut up in the tube (which otherwise are so easily severed) are now so 'firmly glued to one another, that they make (as we see) a strong chain, by which 'not only water but even weighty quicksilver is drawn on high.' Which way of wreathing a little rarefied air into so strong a rope, how probable it is, I will for a while leave the reader to judge, and advance to our author's second notandum, which he thus proposes:

Page 40, 41. *Rarefactionem sive extensionem corporis ad occupandum majorem locum fieri non solo calore, sed etiam distensione seu vi divulsivâ; sicut è contra condensatio non solo frigore perficitur, sed etiam compressione, uti innumera passim docent exempta:* 'That the rarefac-'tion or extension of a body, so as to make it take up more space, is not only made 'by heat, but by distension or a certain disjoining power; as, on the contrary, con-'densation is not only made by cold, but also by compression, as infinite examples 'bear us witness.' And it is true and obvious, that the condensation of bodies, taking that word in a large sense, may be made as well by compression as cold. But I wish he had more clearly expressed what he means in this place by that rarefaction, which he says is to be made by distension, or a *vis divulsiva*, whereof he tells us there are innumerable instances. For, as far as may be gathered from the three examples he subjoins, it is only the air, that is capable of being so extended as his hypothesis requires quicksilver and even stones must be. And I know not how it will be proved, that even air may be thus extended so far, as in the *Magdeburg* experiment, to fill a place more than two thousand times as big as that it filled before. For that the same air

air in this and his two foregoing instances does adequately fill more space at one time than another, he proves but by the rushing in of water into the evacuated glass, and filling it within a little quite full, which, he says, is done by the distended air, that contracting itself draws up the water with it. Which explication how much less likely it is, than that the water is in such cases impelled up by the pressure of the atmosphere, we shall anon (when we come to discuss his way of rarefaction and condensation) have occasion to examine. In the mean time let us consider with him the explication, which, after having promised the two above recited observations, he gives us of his Funiculus; *Cum per primum notandum argentum ita adhæreat tubi vertici, & per secundum, rarefactio fiat per meram corporis distensionem, ita rem se habere, ut argentum descendens à vertice tubi affixam ei relinquat superficiem suam extimam sive supremam, eamque eousque suo pondere extendat extenuetque, donec facilius sit aliam superficiem similiter relinquere quam priorem illam ulterius extendere: secundam igitur relinquit, eamque eodem modo descendendo extendit, donec facilius sit tertiam adhuc separari quam illam secundam extendere ulterius; & sic deinceps, donec tandem vires amplius non habeat superficies sic separandi & extendendi; nempe donec perveniat ad altitudinem digitorum duntaxat 29½ ubi quiescit, ut capite primo dictum est.* 'Seeing by the first note it is manifest, that 'the quicksilver doth so stick to the top of the tube, and by the second note the 'rarefaction is made only by the mere distension of the body; it so comes to pass, 'that the descending quicksilver leaves its external or upper superficies fixed unto the 'top of the tube, and by its weight doth so stretch and extenuate it, until it becomes 'easier to leave another superficies in like manner, than to extend that any further. 'It leaves therefore a second, and doth by its descent extend that a little further, 'until it becomes easier to separate a third than to extend that any further; and so 'forwards, until at length it hath no power to separate or extend any more super-'ficies, namely, until it comes unto the height of 29 inches and an half; where it 'acquiesces, as we have declared in the first chapter.' Thus far our examiner's explication: by which it is easy to discern, that he is fain to assign his Funiculus a way of being produced strange and unparallelled enough. For, not to repeat our animadversions upon the first of the two notandums, on which the explication is grounded, I must demand by what force, upon the bare separation of the quicksilver and the top of the tube, the new body he mentions comes to be produced; or at least how it appears, that the mercury leaves any such thing as he speaks of behind it. For the sense perceives no such matter at the top of the tube, nor is it necessary to explicate the phænomena as we have formerly seen. It may also be marvelled at, that the bare weight of the descending mercury should be able to extend a surface into a body. And besides, it seems precariously affirmed, that there is such a successive leaving behind of one surface after another as is here imagined: nor does it appear how, though some of the quicksilver were turned into a thin subtile substance, yet that substance comes to be contrived into a Funiculus of so strange a nature, that scarce any weight (for aught appears by his doctrine) can be able to break it; that contrary to all other strings it may be stretched without being made more slender; and that it has other very odd properties, some of which we shall anon have occasion to mention. As for what our author subjoins in these words, *Eodem itaque ferè modo separari videntur hæ superficies ab argento descendente, & in tenuissimum quendam funiculum per descendens pondus extendi, quo per calorem in accensa candela separantur hujusmodi superficies à subjecta cera aut sevo, & in subtilissimam flammam extenuantur. Ubi notatu dignum, quemadmodum flamma illa plusquam millies sine dubio majus spatium occupat, quam antea occupaverat pars illa ceræ, ex qua conficitur; ita prorsus & hic existimandum, Funiculum illum plusquam millies majus spatium occupare, quàm prius occupaverat illa argenti*

Page 41.

Page 43. 44.

argenti particula, ex qua fit exortus: uti etiam sine dubio contingit, quando talis particula à subjecto igne in vaporem convertitur. 'These surfaces seem to be separated from the 'quicksilver, and to be extended into a most slender string by the weight that falls 'down, after the same manner that in a lighted candle surfaces of like sort are se-'parated from the wax or tallow underneath by the heat above, and are extenuated 'into a most subtile flame. In which it is worth observation, that as that flame doth 'doubtless take up more than a thousand times a greater space than the part of the 'wax, of which the flame was made, took up; so is it here to be thought, that that 'string doth take up a space more than a thousand times as big as that, which the 'small particle of mercury, from whence it arose, did before take up. As also it 'doubtless happens, when such a particle by a fire underneath is turned into a va-'pour.' Though it be the only example, whereby he endeavours to illustrate the generation of his Funiculus, yet (I presume) he scarce expects we should think it an apposite one. For besides that there here intervenes a conspicuous and powerful agent, namely, an actual fire to sever and agitate the parts of the candle; and besides that there is a manifest wasting of the wax or tallow turned into flame; besides these things, I say, we must not admit, that the fuel, when turned into a flame, does really fill (I say, not, with our author) more than a thousand times, but so much as twice more of genuine space than the wax it was made of. For it may be said, that the flame is little or nothing else than an aggregate of those corpuscles, which before lay upon the upper superficies of the candle, and by the violent heat were divided into minuter particles, vehemently agitated and brought from lying as it were upon a flat to beat off one another, and make up about the wick such a figure, as is usual in the flame of candles burning in the free air. Nor will it necessarily follow, that the space, which the flame seems to take up, should contain neither air nor æther, nor any thing else, save the parts of that flame, because the eye cannot discern any other body there: for even the smoke ascending from the snuff of a newly-extinguished candle appears a dark pillar, which to the eye at some distance seems to consist of smoke; when as yet there are so many aërial and other invisible corpuscles mingled with it, as if all those parts of smoke, that make a great show in the air, were collected and contiguous, they would not perhaps amount to the bigness of a pin's head, as may appear by the great quantity of streams, that in chymical vessels are wont to go to the making up of one drop of spirit. And therefore it does not ill fall out for our turn, that the examiner, to inforce his former example, alleges the turning of a particle of quicksilver into vapour, by putting fire under it: for if such be the rarefaction of mercury, it is not at all like to make such a Funiculus as he talks of, since those mercurial fumes appear by divers experiments to be mercury divided and thrown abroad into minute parts, whereby though the body obtain more of surface than it had before, yet it really fills no more of true and genuine space; since if all the particular little spaces filled by these scattered corpuscles were reduced into one (as the corpuscles themselves often are in chymical operations) they would amount but to one total space, equal to that of the whole mercury before rarefaction. But these objections against this explication are not all that I have to say against our adversary's Funiculus itself.

For I farther demand, how the Funiculus comes by such hooks or grapple-irons, or parts of the like shape, to take fast hold of all contiguous bodies, and even the smoothest, such as glass, and the calm surface of quicksilver, water, oil, and other fluids: and how these slender and invisible hooks cannot only in the tersest bodies find an innumerable company of ears or loops to take hold on, but hold so strongly, that they are able not alone to lift up a tall cylinder of that very ponderous metal of quick-

quickſilver, but to draw inwards the ſides of ſtrong glaſſes ſo forcibly, as to break them all to pieces. And it is alſo ſomewhat ſtrange, that water and other fluid bodies (whoſe parts are wont to be ſo eaſily ſeparable) ſhould, when the Funiculus once lays hold on the ſuperficial corpuſcles, preſently emulate the nature of conſiſtent bodies, and be drawn up like maſſes each of them of an intire piece; though even in the exhauſted receiver they appear by their undulation (when they are ſtirred by bubbles that paſs freely through them) and many other ſigns to continue fluid bodies.

IT ſeems alſo very difficult to conceive, how this extenuated ſubſtance ſhould acquire ſo ſtrong a ſpring inward, as the examiner all along his books aſcribes to it. Nor will it ſerve his turn to require of us in exchange an explication of the air's ſpring outward, ſince he acknowledges, as well as we, that it has ſuch a ſpring. I know, that by calling this extenuated ſubſtance a Funiculus, he ſeems plainly to intimate, that it has its ſpring inward, upon the ſame account that lute-ſtrings and ropes forcibly ſtretched have theirs. But there is no ſmall diſparity betwixt them: for whereas in ſtrings there is required either wreathing, or ſome peculiar and artificial texture of the component parts; and a rarefaction of air (were it granted) does not include or infer any ſuch contrivance of parts as is requiſite to make bodies elaſtical. And if the Carteſian notion of the cauſe of ſpringineſs be admitted, then our extenuated ſubſtance having no pores to be pervaded by the *materia ſubtilis* (to which beſides our author alſo makes glaſs impervious) will be deſtitute of ſpringineſs. And however, ſince lute-ſtrings, ropes, *&c.* muſt, when they ſhrink inwards, either fill up or leſſen their pores, and increaſe in thickneſs as they diminiſh in length; our examiner's Funiculus muſt differ very much from them, ſince it has no pores to receive the ſhrinking parts, and contracts itſelf as to length, without increaſing its thickneſs. Nor can it well be pretended, that this ſelf-contraction is done *ob fugam vacui*, ſince though it ſhould not be made, a vacuum would not enſue. And if it be ſaid, that it is made, that the præternaturally ſtretched body might reſtore itſelf to its natural dimenſions; I anſwer, that I am not very forward to allow acting for ends to bodies inanimate, and conſequently devoid of knowledge; and therefore ſhould gladly ſee ſome unqueſtionable examples produced of operations of that nature. And however to me, who in phyſical inquiries of this nature look for efficient rather than final cauſes, it is not eaſy to conceive, how air, by being expanded (in which caſe its force, like that of other rarefied bodies, ſeems principally to tend outwards, as we ſee in fired gunpowder, in æolipiles, in warmed weather-glaſſes, *&c.*) ſhould acquire ſo prodigious a force of moving contiguous bodies inwards. Nor does it to me ſeem very probable, that, when for inſtance part of a poliſhed marble is extended into a Funiculus, that Funiculus does ſo ſtrongly aſpire to turn into marble again. I might likewiſe wiſh our author had more clearly explicated, how it comes to paſs (which he all along takes for granted) that the acceſs of the outward air does ſo much and ſo ſuddenly relax the tenſion of his Funiculus; ſince that being (according to him) a real and poreleſs body, it is not ſo obvious, how the preſence of another can ſo eaſily and to ſo ſtrange a degree make it ſhrink. But I will rather obſerve, that it is very unlikely, that the ſpace, which our adverſary would have repleniſhed with his funicular ſubſtance, ſhould be full of little highly ſtretched ſtrings, that lay faſt hold of the ſurfaces of all contiguous bodies, and always violently endeavour to pull them inwards. For we have related in our 26th experiment, that a pendulum being ſet a moving in our exhauſted receiver, did ſwing to and fro as freely, and with the ſtring ſtretched as ſtreight, as, for aught we could perceive, it would have done in the common air. Nay, the balance of a watch did there move freely and nimbly to and fro; which it is hard to conceive thoſe bodies could do, if they were to break

through a medium consisting of innumerable exceedingly stretched strings. On which occasion we might add, that it is somewhat strange, that these strings, thus cut or broken by the passage of these bodies through them, could so readily have their parts re-united, and without any more ado be made intire again. And we might also take notice of this as another strange peculiarity in our author's Funiculus, that in this case the two divided parts of each small string, that is broken, do not, like those of other broken strings, shrink and fly back from one another; but (as we just now said) immediately redintegrate themselves: whereas, when in the Torricellian experiment the tube and contained mercury is suddenly lifted up out of the restagnant quicksilver into the air, the Funiculus does so strangely contract itself, that it quite vanishes: insomuch that the ascending mercury may rise to the very top of the tube. These, I say, and divers other difficulties might on this occasion be insisted on; but that, supposing ourselves to have mentioned enough of them for once, we think it now more seasonable to proceed to the remaining part of our discourse.

CHAP. III.

The Aristotelian Rarefaction (proposed by the adversary) examined.

BUT this is not all, that renders the examiner's hypothesis improbable: for, besides those already mentioned particulars, upon whose score it is very difficult to be understood, it necessarily supposes such a rarefaction and condensation, as is, I confess, to me, as well as to many other considering persons, unintelligible.

For the better discernment of the force of this objection, we must briefly premise, that a body is commonly said to be rarefied or dilated, (for I take the word in a larger sense than I know, many others do, for a reason that will quickly appear) when it acquires greater dimensions than the same body had before; and to be condensed, when it is reduced into less dimensions, that is, into a lesser space than it contained before; as when a dry spunge being first dipped in water swells to a far greater bulk, and then being strongly squeezed and held compressed, is not only reduced into less room than it had before it was squeezed, but into less than it had even before it was wetted. And I must further premise, that rarefaction (as also condensation) being amongst the most obvious phænomena of nature, there are three (and for aught we know but three) ways of explicating it: for, either we must say with the Atomists and Vacuists, that the corpuscles, whereof the rarefied body consists, do so depart from each other, that no other substance comes in between them to fill up the deserted spaces that come to be left betwixt the incontiguous corpuscles; or else we say with divers of the ancient philosophers, and many of the moderns, especially the Cartesians, that these new intervals produced betwixt the particles of the rarefied body are but dilated pores, replenished, in like manner as those of the tumid spunge are, by the imbibed water, by some subtile æthereal substance, that insinuates itself betwixt the disjoined particles: or, lastly, we must imagine, with *Aristotle* and most of his followers, that the self-same body does not only obtain a greater space in rarefaction, and a lesser in condensation, but adequately and exactly filled it, and so when rarefied acquires larger dimensions without either leaving any vacuities betwixt its component corpuscles, or admitting between them any new or extraneous substance whatsoever.

Now it is to this last (and, as some call it, rigorous) way of rarefaction, that our adversary has recourse in his hypothesis; though this, I confess, appears to me so difficult

difficult to be conceived, that I make a doubt whether any phænomenon can be explained by it; since to explain a thing is to deduce it from something or other in nature more known than itself.

He that would meet with full discussions of this Aristotelian rarefaction, may resort to the learned writings of *Gassendus*, *Cartesius* and *Maignan*, who have accused it of divers great absurdities: but for my part, I shall at present content myself to make use, to my purpose, of two or three passages, that I meet with (though not together) in our author himself.

Let us then suppose, that in the *Magdeburgh* experiment he so often (though I think causelessly enough) urges to prove his hypothesis; let us (I say) for easier consideration's sake suppose, that the undilated air, which (as he tells us) possessed about half an inch of space, consisted of an hundred corpuscles, or (if that be in this case disliked) a hundred parts; (for it matters not what number we pitch upon) and it will not be denied, but as the whole parcel of air, or the aggregate of this hundred corpuscles, is adequate to the whole space it fills, so each of the hundred parts, that make it up, is likewise adequately commensurate to its peculiar space; which we here suppose to be a hundred part of the whole space. This premised, our author having elsewhere this passage, *Corpore occupante locum verbi gratia duplo majorem, necesse est, ut quælibet ejus pars locum quoque duplo majorem occupet*; 'A body 'taking up a place, for instance, twice as big as itself; it is of necessity that every 'part of it must likewise take up a place twice as big as itself:' prompts us to subjoin, that in the whole capacity of the globe (which according to him was two thousand times as great as the room possessed by the unexpanded air) there must likewise be two hundred thousand parts of space commensurate each of them to one of the forementioned hundredth parts of air; and consequently, when he affirms, that that half-inch of air possessed the whole cavity of the globe, if we will not admit (as he does not) either vacuities, or some intervening subtile substance in the interval of the aërial parts, he must give us leave to conclude, that each part of air does adequately fill two thousand parts of space. Now that this should be resolutely taught to be not only naturally possible (for we dispute not here of what the divine Omnipotence can do) but to be really and regularly done in this *Magdeburgh* experiment, will questionless appear very absurd to the Cartesians and those other philosophers, who take extension to be but notionally different from body, and consequently impossible to be acquired or lost without the addition or detraction of matter; and will, I doubt not, appear strange to those other readers, who consider how generally naturalists have looked upon extension as inseparable, and as immediately flowing from matter; and upon bodies, as having necessary relation to a commensurate space. Nor do I see, if one portion of air may so easily be brought exactly to fill up a space two thousand times as big as that, which it did but fill before without the addition of any new substance; I see not (I say) why the matter contained in every of these two thousand parts of space may not be further brought to fill two thousand more, and so onwards, since each of these newly-replenished spaces is presumed to be exactly filled with body, and no space, nor consequently that which the unrarefied air replenished, can be more than adequately full. And since, according to our adversary, not only fluid bodies, as air and quicksilver, but even solid and hard ones, as marble, are capable of such a distension as we speak of, why may not the world be made I know not how many thousand times bigger than it is, without either admitting any thing of vacuity betwixt its parts, or being increased with the addition of one atom of new matter? Which to me is so difficult to conceive, that I have sometimes doubted, whether, in case it could be proved, that in the exhausted globe we

Page 42.

Page 160.

we speak of, there were no vacuities within, nor any subtile matter permitted to enter from without, it were not more intelligible to suppose, that God had created a new matter to join with the air in filling up the cavity, than that the self-same air should adequately fill two thousand spaces, whereof one was exactly commensurate to it, even when it was uncompressed. For divers eminent Naturalists, both ancient and modern, believing, upon a physical account, the souls of men to be created and infused, will admit it as intelligible, that God does frequently create substances on certain emergent occasions. But I know that many of them will not likewise think it conceivable, that without his immediate interposition an accession of new, real dimensions should be had without either vacuities or accession of matter.

And indeed when I considered these difficulties and others, that attend the rarefaction our examiner throughout his whole book supposes, and when I found that ever and anon he remits us to what he teaches concerning rarefaction; I could not but with some greediness resort to the chapters he addressed me to. But when I had perused them, I found the difficulties remained such still, and that it was very hard even for a witty man to make more of a subject than the nature of it does bear. Which I say, that by professing myself unsatisfied with what he writes, I may not be thought to find fault with a man for not doing what perhaps is not to be done, and for not making such abstruse notions plain, as are scarcely (if at all) so much as intelligible. And indeed as he has handled this subject modestly enough, so in some places his expressions are to me somewhat dark; which I mention, not to impute it as a crime in him, that he wrote in a diffident and doubtful strain of so difficult a matter; but to excuse myself, if I have not always guessed aright at his meaning.

The things he alleges, in favour of the rarefaction he would persuade, are two: the one, that the phænomena of rarefaction cannot be explicated either by vacuities or the subingression of an æthereal substance; and the other, that there are two ways of explicating the rigorous rarefaction he contends for.

His objections against the Epicurean and Cartesian ways of making out rarefaction are some of them more plausible than most of those, that are wont to be urged against them; yet not such as are not capable enough of answers. But whilst some of the passages appeared easy to be replied to by the favourers of the hypothesis they oppose, before I had fully examined the rest, chancing to mention these chapters to an ingenious man, hereafter to be further mentioned in this treatise, he told me he had so far considered them more than the rest of the book, that he had thought upon some hypotheses, whereby the phænomena of rarefaction might be made out either according to the Vacuists, or according to the Cartesians, adding, that he had also examined the instance our adversary pretends to be afforded him of his rarefaction by what happens in the *Rota Aristotelica*. Wherefore being sufficiently distressed by avocations of several sorts, and being willing to reserve the declaration of my own thoughts concerning the manner of rarefaction and condensation for another treatise, I shall refer the reader to the ingenious conjectures about this subject, which the writer of them intends to annex to the present discourse; and only add in general, that whereas the examiner's argument on this occasion is, that his way of rarefaction must be admitted, because neither of the other two can be well made out; his adversaries may with the same reason argue, that one of theirs is to be allowed, since his is incumbered with such manifest difficulties. And they may enforce what they say by representing, that the inconveniences, that attend his hypothesis about rarefaction, are insuperable, arising from the unintelligible nature of the thing itself; whereas those, to which the other ways are obnoxious, may seem to spring but from men's not having yet discovered, what kind of figures and motions of the small

ſmall particles may beſt qualify them to make the body, that conſiſts of them, capable of a competent expanſion.

AFTER our author's objections againſt the two ways of rarefaction propoſed, the one by the Vacuiſts, and the other by the Carteſians and others, that admit the ſolideſt bodies, and even glaſs itſelf, to be pervious to an æthereal or ſubtile matter; he attempts to explicate the manner, by which that rigorous rarefaction he teaches is performed: and having premiſed, that the explication of the way, how each part of the rarefied body becomes extended, depends upon the quality of the parts, into which the body is ultimately reſolved; and having truly obſerved, that they muſt neceſſarily be either really indiviſible, or ſtill endleſsly diviſible; he endeavours to explicate the Ariſtotelian rarefacton according to thoſe two hypotheſes. But, though he thus propoſe two ways of making out his rarefaction; yet beſides that they are irreconcilable, he ſpeaks of them ſo darkly and doubtfully, that it ſeems leſs eaſy to diſcern, which of the two he would be content to ſtick to, than that he himſelf ſcarce acquieſces in either of them.

AND, firſt, having told us, how rarefaction may be explained, in caſe we admit bodies to be diviſible *in infinitum*, he does himſelf make ſuch an objection againſt the infinity of parts in a *continuum*, as he is fain to give ſo obſcure an anſwer to, that I confeſs I do not underſtand it; and preſume, that not only the moſt part of unprejudiced readers will as little acquieſce in the anſwer as I do; but even the author himſelf will not marvel at my confeſſion, ſince in the ſame place he acknowledges the anſwer to be ſomewhat obſcure, and endeavours to excuſe its being ſo, becauſe in that hypotheſis it can ſcarce be otherwiſe.

WHEREFORE I ſhall only add on this occaſion, that it is not clear to me, that even ſuch a diviſibility of a *continuum* as is here ſuppoſed would make out the rarefaction he contends for. For, let the integrant parts of a *continuum* be more or leſs finite or infinite in number, yet ſtill each part, being a corporeal ſubſtance, muſt have ſome particle of ſpace commenſurate to it; and if the whole body be rarefied, for inſtance, to twice its former bigneſs, then will each part be likewiſe extended to double its former dimenſions, and fill both the place it took up before, and another equal to it, and ſo two places.

THE ſecond argument alleged to recommend the hitherto-mentioned way of explicating rarefaction is, that many learned men, amongſt whom he names two, *Aquinas* and *Suarez*, have taught, that the ſame corporeal thing may naturally be, and *de facto* often is, in the ſouls of brutes really indiviſible and virtually extended. But, though I pay thoſe two authors a juſt reſpect for their great ſkill in ſcholaſtical and metaphyſical learning; yet the examiner cannot ignore, that I could make a long catalogue of writers, both ancient and modern, at leaſt as well verſed in natural philoſophy as Saint *Thomas* and *Suarez*, who have ſome of them in expreſs words denied this to be naturally poſſible; and others have declared themſelves of the ſame judgment by eſtabliſhing principles, with which this conceit of the virtual extenſion of the indiviſible corpuſcles is abſolutely inconſiſtent. And though no author had hitherto oppoſed it, yet I, that diſpute not what this or that man thought, but what it is rational to think, ſhould nevertheleſs not ſcruple to reject it now; and ſhould not doubt to find ſtore of the beſt Naturaliſts of the ſame opinion with me, and perhaps among them the examiner himſelf, who (however this acknowledgment may agree with the three following chapters of his book) tells us (pag. 160.) that *juxta probabiliorum ſententiam hujuſmodi virtualis extenſio rei corporeæ concedenda non eſt, utpotè ſoli rei ſpirituali propria:* 'According to the more probable opinion, ſuch a virtual ex-

'tension of a corporeal being is not to be granted, as being only proper to such as 'are spiritual.'

But to conclude at length this tedious inquiry into the Aristotelian way of rarefaction (which is of so obscure a nature, that it can scarce be either proposed or examined in few words) I will not take upon me resolutely to affirm, which of the two ways of explicating it (by atoms or by parts infinitely divisible) our author declares himself for. But which of them soever it be, I think I have shown, that he has not intelligibly made it out: and I make the less scruple to do so, because he himself is so ingenuous as (at the close of his discourse of the two ways) to speak thus of the opinion he prefers; *Præstat communi & receptæ hactenus in scholis sententiæ insistere, quæ licet difficultates quidem non clarè solvat, iis tamen apertè non succumbit:* 'It is better to continue in the common opinion, which hath been hitherto received 'in the schools; which although it doth not clearly resolve all difficulties, yet it doth 'not openly lie under them.' So that in this discourse of rarefaction, to which our author has so often in the foregoing part of the book referred us, as that which should make good what there seemed the most improbable; he has but, instead of a probable hypothesis needlessly rejected, substituted a doctrine, which himself dares not pretend capable of being well freed from the difficulties, with which it may be charged; though I doubt not but other readers, especially Naturalists, will think he has been very civil to this obscure doctrine, in saying that *difficultatibus non apertè succumbit.*

Page 169.

As for the other way of explicating rarefaction, namely, by supposing that a body is made up of parts indivisible; he will not, I presume, deny, but that the objections we formerly made against it are weighty. For according to this hypothesis, (which one would think he prefers, since he makes use of it in the three or four last chapters of his book) *Necessariò fatendum est* (says he) *unam eandemque partem poni in duplici loco adæquate: cum enim indivisibilis sit, locumque occupet majorem quam prius, necesse est, ut tota sit in quolibet puncto totius loci, sive ut per totum illud spatium virtualiter extendatur:* 'We must needs confess, that one and the same part must be in two 'places adequately. For seeing it is indivisible, and takes up a greater place than 'before, it must of necessity be all in every point of that place, or that be virtually 'extended through all that space.' So that when he in the very next page affirms, that by this virtual extension of the parts, the difficulties, that have for so many ages troubled philosophers, may be easily solved, he must give me leave (who love to speak intelligibly, and not to admit what I cannot understand) to desire he would explain to me what this *extensio virtualis* is, and how it will remove the difficulties that I formerly charged upon the Aristotelian rarefaction. For the easier consideration of this matter, let us resume what we lately supposed; namely, that in the Magdeburgic experiment the half-inch of undilated air consisted of a hundred corpuscles: I demand how the indivisibility of these corpuscles will qualify them to make out such a rarefaction as the author imagines. For what does their being indivisible do in this case, but make it the less intelligible, how they can fill above a hundred parts of space? It is easy to foresee he will answer, that they are virtually extended. But not here to question, how their indivisibility makes them capable of being so; I demand, whether by an atom's being virtually extended, its corporeal substance do really (I mean adequately) fill more space than it did before, or whether it do not: (for one of the two is necessary.) If it do, then it is a true and real, and not barely a virtual extension. And that such an extension will not serve the turn, what we have formerly argued against the Peripatetic rarefaction will evince; and our adversary seems to confess

Page 163.

confess as much, by devising this virtual extension to avoid the inconveniences, to which he saw his doctrine of rarefaction would otherwise plainly appear exposed. But if it be said, that when an atom is virtually extended, its corporeal substance fills no more space than before; this is but a verbal shift, that may perhaps amuse an unwary reader, but it will scarce satisfy a considering one. For I demand how that which is not a substance can fill place; and how this improper and but metaphorical extension will solve the phænomena of rarefaction: as how the half-inch of air at the top of the forementioned globe shall without a corporeal extension fill the whole globe of two thousand times its bigness, when the water is sucked out of it, and act at the lower part of the globe. Which last clause I therefore add, because not only our author teaches (pag. 91, and 92.) that the whole globe was filled with a certain thin substance, which by its contraction violently snatched up the water into which the neck of the glass was immersed; but in a parallel case he makes it his grand argument to prove, that there is no vacuum in the deserted part of the tube in the Torricellian experiment, that the attraction of the finger cannot be performed but by some real body. Wherefore till the examiner does intelligibly explain, how a virtual extension, as it is opposed to a corporeal, can make an atom fill twice, nay two thousand times more space than it did before; I suppose this device of virtual extension will appear to unbiassed Naturalists but a very unsatisfactory evasion. Chap. 5.

Two arguments indeed there are, which our adversary offers as proofs of what he teaches. The first is, that they commonly teach in the schools, that at least *divinitus* (as he speaks) such a thing as is pleaded for may be done, and that consequently it is not repugnant to the nature of a body. But, though they that either know me, or have read what I have written about matters theological, will, I hope, readily believe, that none is more willing to acknowledge and venerate Divine Omnipotence; yet in some famous schools they teach, that it is contrary to the nature of the thing. And that men, who think so, and consequently look not upon it as an object of Divine Omnipotence, may (whatever he here say) without impiety be of a differing mind from him about the possibility of such a rarefaction as he would here have, our author may, perchance, think fit to grant, if he remembers that he himself says a few pages after, *Cum tempus sit Ens essentialiter successivum, ita ut ne divinitus quidem possint duæ ejus partes simul existere, &c.* 'Seeing time is a being essentially successive, so that neither by divine power can two of its parts exist together.' But, not now to dispute of a power, that I am more willing to adore than question, I say, that our controversy is not what God can do, but about what can be done by natural agents, not elevated above the sphere of nature. For though God can both create and annihilate, yet nature can do neither: and in the judgment of true philosophers, I suppose our hypothesis would need no other advantage, to make it be preferred before our adversaries, than that in ours things are explicated by the ordinary course of nature, whereas in the other recourse must be had to miracles. Page 175.

But, though our author's way of explicating rarefaction be thus improbable, yet I must not here omit to take notice, that his Funiculus supposes a condensation, that to me appears incumbered with no less manifest difficulties. For, since he teaches, that a body may be condensed without either having any vacuities for the compress parts to retire into, or having pores filled with any subtile and yielding matter, that may be squeezed out of them; it will follow, that the parts of the body to be condensed do immediately touch each other: which supposed, I demand how bodies, that are already contiguous, can be brought to farther approximations without penetrating each other, at least in some of their parts. So that I see not how the examiner's condensation can be performed without penetration of dimensions; a thing

that philosophers of all ages have looked upon as by no means to be admitted in nature. And our author himself speaks somewhere at the same rate, where to the question, Why the walls that inclose fired gun-powder must be blown asunder? *Respondeo* (says he) *hæc omnia inde accidere, quod pulvis ille sic accensus & in flammam conversus, longe majus spatium nunc occupet quàm prius. Unde fit, ut cum totum cubiculum antea fuerit plenissimum, disrumpantur sic parietes, ne detur corporum penetratio.* 'I 'answer, that all these things happen because the gun-powder so kindled and turned 'into flame takes up a much greater space than before. Whence it comes to pass, 'that seeing the chamber was before quite full, by this means the walls are broken, 'that there may be no penetration of bodies.' In the Magdeburgic experiment he tells us (as we have heard already) that the whole capacity of the globe is filled with an extremely thin body. But not now to examine how properly he calls that a rare body, which according to him intercepts neither pores nor any heterogeneous substance, the greater or lesser absence of which makes men call a body more or less dense; not to insist on this, I say, let us consider, that before the admission of water into the exhausted globe, there was, according to him, two thousand half-inches of a substance, which, however it was produced or got thither, was a true and real body; and that after the admission of the water, there remained in the same globe, besides the water that came in, no more than one half inch of body. Since then our author does not pretend (which if he did, might be easily disproved) that one thousand nine hundred ninety-nine half-inches of matter, that now appear no more, traversed the body of water; since he will not allow, that it gets away through the pores of the glass, I demand, what becomes of so great a quantity of matter? For that it is annihilated, I suppose he is too rational a man to pretend (nor, if he should, would it be at all believed); and to say, that a thousand and so many hundred parts of matter should be retired into that one part of space, that contains the one half-inch of air, is little less incredible: for that space was supposed perfectly full of body before, and how a thing can be more than perfectly full, who can conceive? To dispatch: according to our author's way of condensation, two, or perhaps two thousand, bodies may be crouded into a space, that is adequately filled by one of them apart. And if this be not penetration of dimensions, I desire to be informed what is so; and till then I shall leave it to any unprepossessed Naturalist to judge, whether an hypothesis that needs suppose a thing so generally concluded to be impossible to nature, be probable or not; and whether to tell us, that the very same parcel of air, that is now without violence contained in half an inch of space, shall by and by fill two thousand times as much room, and presently after shrink again into the two thousandth part of the space it newly possessed, be not to turn a body into a spirit, and, confounding their notions, attribute to the former the discriminating and least easily conceivable properties of the latter. And this argument is, I confess, with me of that weight, that this alone would keep me from admitting the examiner's hypothesis: yet if any happier contemplator shall prove so sharp-sighted, as to devise and clearly propose a way of making the rarefaction and condensation, hitherto argued against, intelligible to me, he is not like to find me obstinate. Nor indeed is there sufficient cause, why his succeeding in that attempt should make our adversaries hypothesis preferable to ours, since that would not prove it either necessary, or so much as sufficient, but only answer some of the arguments, that tend to prove it is not intelligible. And that we have other arguments on our side than those that relate to rarefaction and condensation, may appear partly by what has been discoursed already, and partly by what we have now to subjoin.

Page 159.

CHAP.

CHAP IV.

A consideration (pertinent to the present controversy) of what happens in trying the Torricellian and other experiments, at the tops and feet of hills.

THERE remain then yet a couple of considerations to be opposed against the examiner's hypothesis, which, though the past discourse may make them be looked upon as needless, we must not pretermit; because they contain such arguments, as may not only be employed against our adversaries doctrine, but will very much tend to the confirmation of ours.

I CONSIDER then further, that the hypothesis I am opposing, being but a kind of inversion of ours, and supposing the spring or motion of restitution in the air to tend inwards, as according to us it tends outwards; it cannot be, that if the supposition itself were (what I think I have proved it is not) true, many of the phænomena would be plausibly enough explicable by it; the same motions in an intermediate body being in many cases producible alike, whether we suppose it to be thrust or drawn, provided both the endeavours tend the same way. But then we may be satisfied, whether the effect be to be ascribed to pulsion or traction (as they commonly speak, though indeed the latter seems reducible to the former) if we can find out an experiment, wherein there is reason such an effect should follow, in case pulsion be the cause inquired after, and not in case it be traction. And such an *experimentum crucis* (to speak with our illustrious *Verulam*) is afforded us by that noble observation of Monsieur *Paschal*, mentioned by the famous *Pecquet*, and out of him by our author: namely, that the Torricellian experiment being made at the foot and in divers places of a very high mountain (of the altitude of five hundred fathom or three thousand foot) he found, that after he had ascended a hundred and fifty fathom, the quicksilver was fallen two inches and a quarter below its station at the mountain's foot; and that at the very top of the hill it had descended above three inches below the same wonted station. Whence it appears, that the quicksilver being carried up towards the top of the atmosphere, falls down the lower, the higher the place is wherein the observation is made: of which the reason is plain in our hypothesis, namely, that the nearer we come to the top of the atmosphere, the shorter and lighter is the cylinder of air incumbent upon the restagnant mercury; and consequently the less weight of cylindrical mercury will that air be able to counterpoise and keep suspended. And since this notable phænomenon does thus clearly follow upon ours, and not upon our adversaries hypothesis; this experiment seems to determine the controversy betwixt them: because in this case the examiner cannot pretend, as he does in the seventeenth and divers other of our experiments, that the descent of the quicksilver in the tube is caused, not by the diminution of the external air's pressure, but from the preternatural rarefaction or distension of that external air (in the receiver) when by seeking to restore itself, it endeavours to draw up the restagnant mercury: for in our present case there appears no such forcible dilatation of that air, as in many of the phænomena of our engine he is pleased to imagine.

Page 66.

IT need therefore be no great wonder, if his adversaries do, as he observes, make a great account of this experiment, to prove, that the mercury is kept up in the tube by the resistance of the external air: nor do I think his answers to the argument drawn from hence will keep them from thinking it cogent. For to an objection, upon which

which he takes notice that they lay so much stress, he replies but two things; which neither singly nor together will near amount to a satisfactory answer.

And, first, he questions the truth of the observation itself; because having made trial in a low hill, the event did no ways answer his expectation. But though, instead of disapproving, I am willing to commend his curiosity, to make the experiment himself, and especially since it was both new and important; and though also I like his modesty, in rather suspecting some mistake in the manner of the observation, than that the experimenters did not sincerely deliver it: yet, since there must be an error somewhere, I must rather charge it upon the examiner's observation (I say, his observation, not his want of sincerity) than upon Monsieur *Paschal*'s. For besides the commendations, that the learned *Gassendus*, who relates the experiment, gives to that ingenious gentleman (Monsieur *Paschal*) by whose direction he supposes it to have been tried; the same *Gassendus* relates, that the like observation was five times repeated, *partim intra sacellum, partim aëre libero, & nunc quidem flante, nunc silente vento*: 'Sometimes within the chapel, sometimes in the open air; the wind sometimes blowing, and sometimes being still.' Which circumstances sufficiently argue the diligence wherewith the experiment was tried in *Auvergne*. Especially since I can confirm these observations by two more made on distant hills in *England*: the one of which I procured from that known Virtuoso Mr. *J. Ball*, whom I desired to make the experiment at a mountain in *Devonshire*, on the side whereof he dwelt; and the other made in *Lancashire* by that ingenious gentleman Mr. *Rich. Townley*. Both which observations, since I have mentioned them at large in the Appendix to the Physico-Mechanical treatise, I shall not now repeat; contenting myself to observe to our present purpose, that however the proportion of the descent of the quicksilver may vary, according to the differing consistence and other accidents of the neighbouring air, in the particular places and times of the experiments being made, yet all observations agree in this, that nearer the top of the atmosphere the quicksilver falls lower than it does further from it. To all this I shall add two things, that will very much confirm our hypothesis. The one is, that the freshly-named Mr. *Townley*, and divers ingenious persons that assisted at the trial, bethought themselves of so making the Torricellian experiment at the top of the hill, as to leave a determinate quantity of air in the tube, before the mouth of it was opened under the vessel'd mercury; and taking notice how low such a quantity of that air depressed the mercurial cylinder, they likewise observed, that at the mountain's foot the included air was not able to depress the quicksilver so much. Whence we infer, that the cylinder of air at the top of the hill being shorter and lighter, did not so strongly press against the included air, as did the ambient air at the bottom of the hill, where the aerial cylinder was longer and heavier.

Gassendus, Tom. 1. p. 211.

But because that though experiments made in very elevated places are noble ones, and of great importance in the controversies about the air, yet there are but very few of those, that are qualified to make experiments of that nature, who have the opportunity of making them upon high mountains; we did, with the assistance of an ingenious man, attempt a trial, wherein we hoped to find a sensibly differing weight of the atmosphere, in a far less height than that of an ordinary hill. But instead of a common tube, we made use of a kind of weather-glass, that the included air might help to make the event notable, for a reason to be mentioned ere long; and instead of quicksilver we employed common water in the pipe belonging to the weather-glass, that small changes in the weight or resistance of the atmosphere in opposition of the included air might be the more discernable. The instrument we made use of consisted only

only of a glaſs with a broad foot and a narrow neck A B, and a ſlender glaſs pipe C D, open at both ends: which pipe was ſo placed, that the bottom of it did almoſt, but not quite, reach to the bottom of the bigger glaſs A B, within whoſe neck A, it was faſtened with a cloſe cement, that both kept the pipe in its place, and hindered all communication betwixt the inward I I and outward K K air, ſave by the cavity of the pipe C D. Now we choſe this glaſs A B more than ordinary capacious, that the effect of the dilatation of the included air I I might be the more conſpicuous. Then conveying a convenient quantity of water H H into this glaſs, we carried it to the leads of the lofty abby-church at *Weſtminſter*, and there blew in a little air to raiſe the water to the upper part of the pipe, that being above the veſſel A B, we might more preciſely mark the ſeveral ſtations of the water than otherwiſe we could. Afterward having ſuffered the glaſs to reſt a pretty while upon the lead, that the air I I within might be reduced to the ſame ſtate, both as to coldneſs and as to preſſure, with K K that without, having marked the ſtation of the water F, we gently let down the veſſel by a long ſtring to the foot of the wall, where one attended to receive it; who having ſuffered it to reſt upon the ground, cried to us, that it was ſubſided about an inch below the mark F we had put. Whereupon having ordered him to put a mark at his ſecond ſtation of it E, we drew up the veſſel again; and ſuffering it to reſt a while, we obſerved the water to be re-aſcended to or near the firſt mark F, which was indeed about an inch above E, the other. And this we did that evening a ſecond time with almoſt a like ſucceſs: though two or three days after, the wind blowing ſtrongly upon the leads, we found not the experiment to ſucceed quite ſo regularly as before; yet the water always manifeſtly fell lower at the foot of the wall than it was at the top: which I ſee no cauſe to aſcribe barely to the differing temperature of the air above and below, as to heat and cold, ſince according to the general eſtimate, the more elevated region of the air is, *cæteris paribus*, colder than that below; which would rather check the greater expanſion of the included air at the top of the leads, than to promote it. But the better to avoid miſtakes and prevent objections, we thought fit to try the experiment within the church, and got into a gallery of the ſame height with the leads: but the upper part of the pipe being caſually broken off, we thought fit to order the matter ſo, that the ſurface G of the remaining water in the pipe, ſhould be about an inch higher than the ſurface of the water in the veſſel. And then my above-mentioned correſpondent letting down the glaſs, almoſt as ſoon as it was ſettled upon the pavement, kneeling down to ſee how far it was ſubſided, I found that not only it was fallen as low as the other water, but that the outward air depreſſed it ſo far, as whilſt I was looking on, to break in beneath the bottom of the pipe, and aſcend through the water in bubbles; after which, the glaſs being drawn up again, my correſpondent affirmed, that the water was very manifeſtly reaſcended. But becauſe by the unlucky breaking of a glaſs, we were hindered to obſerve, as we deſigned, what would happen as well in a weather-glaſs, ſo contrived, that the weight or preſſure of the atmoſphere ſhould make no change in it, as in another whoſe included air was at the top (whereas, in that we employed, the included air was in the lower part) and becauſe there happened in our trials a circumſtance or two that ſeemed not ſo devoid of difficulties, but that we think it may require further examination, we deſign to ſet down a more particular account of this experiment (as how it ſucceeds with quickſilver inſtead of water, together with the capacity of the veſſel A B and the bore of the pipe C D, with ſome other variety of circumſtances) together with the event of the curioſity we had (which ſeemed very ſucceſsful) to try the Torricellian experiment upon the above-mentioned leads; and then let down the tube,

 together

together with the restagnant mercury, to the ground, to observe the increasing altitude of the quicksilver, in the formerly-mentioned appendix to the epistle we have been defending. And it shall suffice us, in the mean time, that the trials already mentioned seem to make it evident enough, that the atmosphere gravitates more, *cæteris paribus*, near the surface of the earth, than in the more elevated parts of the air. For the leads, on which we made our trials, were found by measure to be in perpendicular height but threescore and fifteen foot from the ground. To which we shall only add this at present, that once being desirous to observe what we could touching the proportions of the subsidence of the water to the height of its several stations from the ground, purposely carrying down the vessel so as not considerably to heat it, from the leads down the stairs to a little window that we guess to be almost half way to the bottom, we there perceived the water to have already subsided about a barley-corn's length; notwithstanding that probably, in spite of our care, the vessel were a little warmed by the heat of his body that carried it, since by that time we were come to the foot of the wall, the water stood almost at the highest mark; but after the vessel was suffered to rest a while, it relapsed by degrees to the lowest. And thus much for the first of the things I had to represent in favour of our doctrine.

The other particular I shall mention for confirmation of our hypothesis, is that experiment (which, though it be needless, seems yet more cogent and proper to prevent evasions) made by the same Monsieur *Paschal*, of carrying a weakly-blown foot-ball from the bottom to the top of an high mountain. For that foot-ball swelled more and more, the higher it was carried, so that it appeared as if it were full blown at the top of the mountain, and gradually growing lank again, as it was carried downwards; so that at the foot of the hill it was flaccid as before. This, I say, having thus happened, we have here an experiment to prove our hypothesis, wherein recourse cannot be had to any forcibly and preternaturally distended body, such as that is pretended to be, which remains in the deserted space of the tube in the Torricellian experiment.

The other thing, which the examiner alleges against our argument from Monsieur *Paschal*'s trials, is, that supposing it to be true, yet it cannot thence be inferred, that the subsidence of the mercury at the top of the hill proceeded from the atmospherical
Page 63. cylinder's being there lighter and less able to sustain the quicksilver. *Sed dici potest* (says he) *ideo sic in vertice montis magis descendisse, quod ibidem esset aura frigidior, aut ex alio temperamento hujusmodi descensum causante.* 'But it may be said, that on the 'top of the mountain it therefore descended after that manner, because the air was 'more cold there, or of some other temperature, such as might cause this descent.' But this solution will not serve the turn: for the coldness of the ambient air (which yet the experiments take not notice of) would rather contract the rarefied substance within the tube, and so draw up the mercury higher; as our author himself teaches us, that it is from the shrinking of the Funiculus, occasioned by the cold, that the
Page 50. water in thermometers ascends in cold weather. And whereas the only proof he adds of so improbable an explication is taken from our eighteenth experiment, wherein we relate, that sometimes the quicksilver did sensibly fall lower in colder than in far less cold weather; I answer, that this eighteenth experiment will scarce make more for him than against him: for, as I there take notice, that the quicksilver descended in cold weather, so it sometimes descended likewise in hot weather, and rose in cold. And it is very strange, that in all the observations made in differing countries and at different times, it should still so happen, that the mercurial cylinder should be

ſhorter near the top of the atmoſphere than further from it; if the reſiſtance of the outward air have nothing to do with the keeping it ſuſpended. And it is yet more ſtrange, that the foot-ball ſhould in like manner grow turgid and flaccid, according as it is carried into places where it has a ſhorter or longer pillar of air incumbent on it.

I WAS going to proceed to what remains of this ſecond part of our treatiſe, but that, ſince I begun this chapter, caſually meeting with an experiment lately ſent in a letter to a very ingenious * acquaintance of his and mine, by a very induſtrious † phyſician (who is ſaid to have had the curioſity to try over again many of the experiments of our engine) and finding it very proper to confirm our newly related experiment made at *Weſtminſter*, and to be of ſuch a nature as we have not in this part of *England* the opportunity to try the like, for want of hills high enough; I ſhall (according to the permiſſion given me) inſert it in this place. And the rather, that as the mountains have, by the trials made on them of the Torricellian experiment, afforded us a noble proof of the weight of the air; ſo they may afford us one of its ſpring: wherein I hope the phænomenon of the water's deſcent will not be aſcribed to any attraction made of the water by the violently-diſtended outward air. And becauſe the experiment was not made by us, but by another, we will ſet it down in his words, which are theſe: *This fifteenth of* October 1661, *we took a weather-glaſs* A B, *of about two foot in length, and carrying it to the bottom of* Hallifax *hill, the water ſtood in the ſhank at thirteen inches above the water in the veſſel: thence carrying it thus filled, with the whole frame, immediately to the top of the ſaid hill, the water fell down to the point* D, viz. *an inch and a quarter lower than it was at the bottom of the ſaid hill; which* (as he rightly infers) *proves the elaſticity of the air. For the internal air* A C, *which was of the ſame power and extenſion with the external at the bottom of the hill, did manifeſt a greater elaſticity than the mountain-air there**, *and ſo extended itſelf further by* C D.

* *Mr.* Croon, one *of the learned profeſſors of* Greſham *college.*

† *Dr.* Hen. Power.

See the ſecond figure.

THE like experiment, I hear, the ſame ingenious doctor has very lately repeated, and found the deſcent of the water to be greater than before. And though ſome Virtuoſi have thought it ſtrange, that in an hill far inferior to the *Alps* and *Apennines*, ſo ſhort a cylinder of ſo light a liquor as water ſhould fall ſo much; yet I ſee not any reaſon to diſtruſt upon this ground either his experiment or ours (lately mentioned to have been made at *Weſtminſter*;) but rather to wonder the water fell no more (if the hill be conſiderably high.) For their ſuſpicion ſeems grounded upon a miſtake, as if becauſe the quickſilver in the Torricellian experiment, made without purpoſely leaving any air in the tube, would not, at the top of the mentioned hill, have ſubſided above an inch, if ſo much; the water, that is near fourteen times lighter, ſhould not fall above a fourteenth part of that ſpace: whereas in the Torricellian experiment, the upper and deſerted ſpace of the tube has little or no air left in it, but the correſpondent part of the water-glaſs was furniſhed with air, whoſe preſſure was little leſs than that of the atmoſphere at the bottom of the hill; and conſequently muſt be much greater than the preſſure of the atmoſphere at the top of the hill, where the atmoſpherical cylinder's gravity (upon whoſe account it preſſes) muſt be much diminiſhed by its being made much ſhorter, and by its conſiſting of an air leſs compreſt. And thus much for the firſt of the two conſiderations wherewith I promiſed to conclude this ſecond part of the preſent tract. Only before I proceed, I muſt in a word deſire the reader to take notice, that though I have here ſingled out but one of the

* Probably theſe or the like words, *did manifeſt preſſure*, are here omitted, for the mountain air there ſeems to have acted rather by its weight than elaſticity.

nine experiments which the examiner in the 11th and 12th chapters reckons up as urged by his adversaries; yet do not thereby declare my acquiescing in his explications of those phænomena, but only leave both them and some other things he delivers about siphons and the *Magdeburg* experiments, to be discoursed by those that are more concerned to examine them, contenting myself to have sufficiently disproved the Funiculus which his expositions suppose, and cleared the grounds of explicating such experiments aright.

CHAP. V.

Two new Experiments touching the measure of the force of the spring of air compressed and dilated.

Page 11. THE other thing, that I would have considered touching our adversary's hypothesis is, that it is needless. For whereas he denies not, that the air has some weight and spring, but affirms, that it is very insufficient to perform such great matters as the counterpoising of a mercurial cylinder of 29 inches, as we teach that it may; we shall now endeavour to manifest by experiments purposely made, that the spring of the air is capable of doing far more than it is necessary for us to ascribe to it, to solve the phænomena of the Torricellian experiment.

WE took then a long glass-tube, which, by a dexterous hand and the help of a lamp, was in such a manner crooked at the bottom, that the part turned up was almost parallel to the rest of the tube, and the orifice of this shorter leg of the siphon (if I may so call the whole instrument) being hermetically sealed, the length of it was divided into inches (each of which was subdivided into eight parts) by a streight list of paper, which containing those divisions, was carefully pasted all along it. Then putting in as much quicksilver as served to fill the arch or bended part of the siphon, that the mercury standing in a level might reach in the one leg to the bottom of the divided paper, and just to the same height or horizontal line in the other; we took care, by frequently inclining the tube, so that the air might freely pass from one leg into the other by the sides of the mercury (we took, I say, care) that the air at last included in the shorter cylinder should be of the same laxity with the rest of the air about it. This done, we began to pour quicksilver into the longer leg of the siphon, which by its weight pressing up that in the shorter leg, did by degrees streighten the included air: and continuing this pouring in of quicksilver till the air in the shorter leg was by condensation reduced to take up but half the space it possessed (I say, possessed, not filled) before; we cast our eyes upon the longer leg of the glass, on which was likewise pasted a list of paper carefully divided into inches and parts, and we observed, not without delight and satisfaction, that the quicksilver in that longer part of the tube was 29 inches higher than the other. Now that this observation does both very well agree with and confirm our hypothesis, will be easily discerned by him, that takes notice what we teach; and Monsieur *Pascbal* and our English friend's experiments prove, that the greater the weight is that leans upon the air, the more forcible is its endeavour of dilatation, and consequently its power of resistance (as other springs are stronger when bent by greater weights). For this being considered, it will appear to agree rarely-well with the hypothesis, that as according to it the air in that degree of density and correspondent measure of resistance, to which the weight of the incumbent atmosphere had brought it, was able to counterbalance and

and resist the pressure of a mercurial cylinder of about 29 inches, as we are taught by the Torricellian experiment; so here the same air being brought to a degree of density about twice as great as that it had before, obtains a spring twice as strong as formerly. As may appear by its being able to sustain or resist a cylinder of 29 inches in the longer tube, together with the weight of the atmospherical cylinder, that leaned upon those 29 inches of mercury; and, as we just now inferred from the Torricellian experiment, was equivalent to them.

WE were hindered from prosecuting the trial at that time by the casual breaking of the tube. But because an accurate experiment of this nature would be of great importance to the doctrine of the spring of the air, and has not yet been made (that I know) by any man; and because also it is more uneasy to be made than one would think, in regard of the difficulty as well of procuring crooked tubes fit for the purpose, as of making a just estimate of the true place of the protuberant mercury's surface; I suppose it will not be unwelcome to the reader, to be informed, that after some other trials, one of which we made in a tube whose longer leg was perpendicular, and the other, that contained the air, parallel to the horizon, we at last procured a tube of the figure exprest in the scheme; which tube, though of a pretty bigness, was so long, that the cylinder, whereof the shorter leg of it consisted, admitted a list of paper, which had before been divided into 12 inches and their quarters, and the longer leg admitted another list of paper of divers feet in length, and divided after the same manner. Then quicksilver being poured in to fill up the bended part of the glass, that the surface of it in either leg might rest in the same horizontal line, as we lately taught, there was more and more quicksilver poured into the longer tube; and notice being watchfully taken how far the mercury was risen in that longer tube, when it appeared to have ascended to any of the divisions in the shorter tube, the several observations, that were thus successively made, and as they were made set down, afforded us the ensuing table: *See Fig. 5.*

A table

A table of the condensation of the air.

A	*A*	*B*	*C*	*D*	*E*
48	12	00		$29\frac{2}{16}$	$29\frac{2}{16}$
46	$11\frac{1}{2}$	$01\frac{7}{16}$		$30\frac{9}{16}$	$33\frac{6}{16}$
44	11	$02\frac{13}{16}$		$31\frac{13}{16}$	$31\frac{12}{16}$
42	$10\frac{1}{2}$	$04\frac{6}{16}$		$33\frac{8}{16}$	$33\frac{1}{7}$
40	10	$06\frac{3}{16}$		$35\frac{3}{16}$	35- -
38	$9\frac{1}{2}$	$07\frac{14}{16}$		37	$36\frac{13}{19}$
36	9	$10\frac{2}{16}$		$39\frac{2}{16}$	$38\frac{7}{8}$
34	$8\frac{1}{2}$	$12\frac{8}{16}$		$41\frac{10}{16}$	$41\frac{2}{17}$
32	8	$15\frac{1}{16}$	Added to $27\frac{1}{2}$ makes	$44\frac{1}{16}$	$43\frac{11}{16}$
30	$7\frac{1}{2}$	$17\frac{12}{16}$		$47\frac{9}{16}$	$46\frac{3}{5}$
28	7	$21\frac{1}{16}$		$50\frac{9}{16}$	50- -
26	$6\frac{1}{2}$	$25\frac{3}{16}$		$54\frac{5}{16}$	$53\frac{10}{13}$
24	6	$29\frac{11}{16}$		$58\frac{13}{16}$	$58\frac{2}{9}$
23	$5\frac{3}{4}$	$32\frac{3}{16}$		$61\frac{9}{16}$	$60\frac{18}{23}$
22	$5\frac{1}{2}$	$34\frac{12}{16}$		$64\frac{3}{16}$	$63\frac{6}{11}$
21	$5\frac{1}{4}$	$37\frac{13}{16}$		$67\frac{1}{16}$	$66\frac{4}{7}$
20	5	$41\frac{9}{16}$		$70\frac{11}{16}$	70- -
19	$4\frac{3}{4}$	45- -		$74\frac{2}{16}$	$73\frac{11}{19}$
18	$4\frac{1}{2}$	$48\frac{12}{16}$		$77\frac{14}{16}$	$77\frac{2}{3}$
17	$4\frac{1}{4}$	$53\frac{11}{16}$		$82\frac{12}{16}$	$82\frac{4}{17}$
16	4	$58\frac{2}{16}$		$87\frac{14}{16}$	$87\frac{1}{8}$
15	$3\frac{3}{4}$	$63\frac{12}{16}$		$93\frac{1}{16}$	$93\frac{1}{5}$
14	$3\frac{1}{2}$	$71\frac{5}{16}$		$100\frac{7}{16}$	$99\frac{6}{7}$
13	$3\frac{1}{4}$	$78\frac{11}{16}$		$107\frac{13}{16}$	$107\frac{7}{13}$
12	3	$88\frac{7}{16}$		$117\frac{9}{16}$	$116\frac{4}{9}$

AA. The number of equal spaces in the shorter leg, that contained the same parcel of air diversly extended.

B. The height of the mercurial cylinder in the longer leg, that compressed the air into those dimensions.

C. The height of the mercurial cylinder, that counterbalanced the pressure of the atmosphere.

D. The aggregate of the two last columns *B* and *C*, exhibiting the pressure sustained by the included air.

E. What that pressure should be according to the hypothesis, that supposes the pressures and expansions to be in reciprocal proportion.

For the better understanding of this experiment, it may not be amiss to take notice of the following particulars:

1. That the tube being so tall, that we could not conveniently make use of it in a chamber, we were fain to use it on a pair of stairs, which yet were very lightsome, the tube being for preservation's sake by strings so suspended, that it did scarce touch the box presently to be mentioned.

2. The lower and crooked part of the pipe was placed in a square wooden box, of a good largeness and depth, to prevent the loss of the quicksilver, that might fall aside in the transfusion from the vessel into the pipe, and to receive the whole quicksilver in case the tube should break.

3. That we were two to make the observation together, the one to take notice at the bottom, how the quicksilver rose in the shorter cylinder, and the other to pour in at the top of the longer; it being very hard and troublesome for one man alone to do both accurately.

4. That the quicksilver was poured in but by little and little, according to the direction of him that observed below; it being far easier to pour in more, than to take out any, in case too much at once had been poured in.

5. That

5. THAT at the beginning of the operation, that we might the more truly discern where the quicksilver rested from time to time, we made use of a small looking-glass, held in a convenient posture to reflect to the eye what we desired to discern.

6. THAT when the air was so compressed, as to be crouded into less than a quarter of the space it possessed before, we tried whether the cold of a linen cloth dipped in water would then condense it. And it sometimes seemed a little to shrink, but not so manifestly as that we dare build any thing upon it. We then tried likewise, whether heat would, notwithstanding so forcible a compressure, dilate it; and approaching the flame of a candle to that part where the air was pent up, the heat had a more sensible operation than the cold had before; so that we scarce doubted, but that the expansion of the air would, notwithstanding the weight that oppreſt it, have been made conspicuous, if the fear of unseasonably breaking the glass had not kept us from increasing the heat.

Now although we deny not, but that in our table some particulars do not so exactly answer to what our formerly mentioned hypothesis might perchance invite the reader to expect; yet the variations are not so considerable, but that they may probably enough be ascribed to some such want of exactness as in such nice experiments is scarce avoidable. But for all that, till further trial hath more clearly informed me, I shall not venture to determine, whether or no the intimated theory will hold universally and precisely, either in condensation of air, or rarefaction: all that I shall now urge being, that however the trial already made sufficiently proves the main thing, for which I here allege it; since by it, it is evident, that as common air, when reduced to half its wonted extent, obtained near about twice as forcible a spring as it had before; so this thus compreſt air being further thrust into half this narrow room, obtained thereby a spring about as strong again as that it last had, and consequently four times as strong as that of the common air. And there is no cause to doubt, that if we had been here furnished with a greater quantity of quicksilver and a very strong tube, we might, by a further compression of the included air, have made it counterbalance the pressure of a far taller and heavier cylinder of mercury. For no man perhaps yet knows, how near to an infinite compression the air may be capable of, if the compressing force be competently increased. So that here our adversary may plainly see, that the spring of the air, which he makes so light of, may not only be able to resist the weight of 29 inches, but in some cases of above a hundred inches of quicksilver, and that without the assistance of his Funiculus, which in our present case has nothing to do. And to let you see, that we did not (as a little above) inconsiderately mention the weight of the incumbent atmospherical cylinder as a part of the weight resisted by the imprisoned air, we will here annex, that we took care, when the mercurial cylinder in the longer leg of the pipe was about an hundred inches high, to cause one to suck at the open orifice; whereupon (as we expected) the mercury in the tube did notably ascend. Which considerable phænomenon cannot be ascribed to our examiner's Funiculus, since by his own confession that cannot pull up the mercury, if the mercurial cylinder be above 29 or 30 inches of mercury. And therefore we shall render this reason of it, that the pressure of the incumbent air being in part taken off by its expanding itself into the sucker's dilated chest; the imprisoned air was thereby enabled to dilate itself manifestly, and repel the mercury, that compreſt it, till there was an equality of force betwixt the strong spring of that compreſt air on the one part, and the tall mercurial cylinder, together with the contiguous dilated air, on the other part.

Now, if to what we have thus delivered concerning the compression of the air, we add some observations concerning its spontaneous expansion, it will the better appear, how much the phænomena of these mercurial experiments depend upon the differing mea-

measures of strength to be met with in the air's spring, according to its various degrees of compression and laxity. But, before I enter upon this subject, I shall readily acknowledge, that I had not reduced the trials I had made about measuring the expansion of the air to any certain hypothesis, when that ingenious gentleman Mr. *Richard Townley* was pleased to inform me, that having by the perusal of my physico-mechanical experiments been satisfied that the spring of the air was the cause of it, he endeavoured (and I wish in such attempts other ingenious men would follow his example) to supply what I had omitted concerning the reducing to a precise estimate, how much air dilated of itself loses of its elastical force, according to the measures of its dilatation. He added, that he had begun to set down what occurred to him to this purpose in a short discourse, whereof he afterwards did me the favour to shew me the beginning, which gives me a just curiosity to see it perfected. But, because I neither know, nor (by reason of the great distance betwixt our places of residence) have at present the opportunity to inquire, whether he will think fit to annex his discourse to our appendix, or to publish it by itself, or at all; and because he hath not yet, for aught I know, met with fit glasses to make an any-thing-accurate table of the decrement of the force of the dilated air; our present design invites us to present the reader with that which follows, wherein I had the assistance of the same person, that I took notice of in the former chapter, as having written something about rarefaction: whom I the rather make mention of on this occasion, because when he first heard me speak of Mr. *Townley*'s suppositions about the proportion, wherein air loses of its spring by dilatation, he told me he had the year before (and not long after the publication of my pneumatical treatise) made observations to the same purpose, which he acknowledged to agree well enough with Mr. *Townley*'s theory: and so did (as their author was pleased to tell me) some trials made about the same time by that noble virtuoso and eminent mathematician the Lord *Brouncker*, from whose further enquiries into this mattter, if his occasions will allow him to make them, the curious may well hope for something very accurate.

A table of the rarefaction of the air.

A. The number of equal spaces at the top of the tube, that contained the same parcel of air.

B. The height of the mercurial cylinder, that together with the spring of the included, air counterbalanced the pressure of the atmosphere.

C. The pressure of the atmosphere.

D. The complement of *B* to *C*, exhibiting the pressure sustained by the included air.

E. What that pressure should be, according to the hypothesis.

A	*B*	*C*	*D*	*E*
1	$00\frac{0}{0}$	Subtracted from $29\frac{3}{4}$ leaves	$29\frac{3}{4}$	$29\frac{1}{4}$
$1\frac{1}{2}$	$10\frac{1}{8}$		$19\frac{1}{8}$	$19\frac{5}{8}$
2	$15\frac{1}{8}$		$14\frac{1}{8}$	$14\frac{7}{8}$
3	$20\frac{5}{8}$		$9\frac{4}{8}$	$9\frac{15}{16}$
4	$22\frac{5}{8}$		$7\frac{1}{8}$	$7\frac{7}{16}$
5	$24\frac{1}{8}$		$5\frac{5}{8}$	$5\frac{19}{16}$
6	$24\frac{7}{8}$		$4\frac{7}{8}$	$4\frac{27}{16}$
7	$25\frac{4}{8}$		$4\frac{2}{8}$	$4\frac{1}{4}$
8	$6\frac{0}{0}$		$3\frac{6}{8}$	$3\frac{23}{16}$
9	$26\frac{1}{8}$		$3\frac{1}{8}$	$3\frac{11}{16}$
10	$26\frac{6}{8}$		$3\frac{0}{0}$	$2\frac{39}{40}$
12	$27\frac{1}{8}$		$2\frac{5}{8}$	$2\frac{13}{16}$
14	$27\frac{4}{8}$		$2\frac{2}{8}$	$2\frac{1}{8}$
16	$27\frac{6}{8}$		$2\frac{0}{0}$	$1\frac{31}{64}$
18	$27\frac{7}{8}$		$1\frac{7}{8}$	$1\frac{47}{128}$
20	$28\frac{0}{0}$		$1\frac{6}{8}$	$1\frac{9}{80}$
24	$28\frac{2}{8}$		$1\frac{4}{8}$	$1\frac{21}{96}$
28	$28\frac{3}{8}$		$1\frac{3}{8}$	$1\frac{1}{16}$
32	$28\frac{4}{8}$		$1\frac{2}{8}$	$0\frac{119}{128}$

To

To make the experiment of the debilitated force of expanded air the plainer, it will not be amiss to note some particulars, especially touching the manner of making the trial; which (for the reasons lately mentioned) we made on a lightsome pair of stairs, and with a box also lined with paper to receive the mercury that might be spilt. And in regard it would require a vast, and in few places procurable quantity of quicksilver, to imploy vessels of such kind as are ordinary in the Torricellian experiment, we made use of a glass-tube of about six feet long; for that being hermetically sealed at one end, served our turn as well as if we could have made the experiment in a tub or pond of seventy inches deep.

SECONDLY, We also provided a slender glass-pipe of about the bigness of a swan's quill, and open at both ends; all along which was pasted a narrow list of paper, divided into inches and half quarters.

THIRDLY, This slender pipe being thrust down into the greater tube almost filled with quicksilver, the glass helped to make it swell to the top of the tube; and the quicksilver getting in at the lower orifice of the pipe, filled it up till the mercury included in that was near about a level with the surface of the surrounding mercury in the tube.

FOURTHLY, There being, as near as we could guess, little more than an inch of the slender pipe left above the surface of the restagnant mercury, and consequently unfilled therewith, the prominent orifice was carefully closed with sealing-wax melted; after which the pipe was let alone for a while, that the air dilated a little by the heat of the wax, might, upon refrigeration, be reduced to its wonted density. And then we observed by the help of the above-mentioned list of paper, whether we had not included somewhat more or somewhat less than an inch of air; and in either case we were fain to rectify the error by a small hole made (with a heated pin) in the wax, and afterwards closed up again.

FIFTHLY, Having thus included a just inch of air, we lifted up the slender pipe by degrees, till the air was dilated to an inch, an inch and an half, two inches, *&c.* and observed in inches and eighths the length of the mercurial cylinder, which at each degree of the air's expansion was impelled above the surface of the restagnant mercury in the tube.

SIXTHLY, The observations being ended, we presently made the Torricellian experiment with the above-mentioned great tube of six feet long, that we might know the height of the mercurial cylinder, for that particular day and hour; which height we found to be 29¾ inches.

SEVENTHLY, Our observations made after this manner furnished us with the preceding table, in which there would not probably have been found the difference here set down betwixt the force of the air, when expanded to double its former dimensions, and what that force should have been precisely according to the theory, but that the included inch of air received some little accession during the trial; which this newly mentioned difference making us suspect, we found by replunging the pipe into the quicksilver, that the included air had gained about half an eighth, which we guessed to have come from some little aërial bubbles in the quicksilver, contained in the pipe (so easy is it in such nice experiments to miss of exactness). We tried also with 12 inches of air shut up to be dilated; but being then hindered by some unwelcome avocations to prosecute those experiments, we shall elsewhere, out of other notes and trials (God permitting) set down some other accurate tables concerning this matter. By which possibly we may be assisted to resolve, whether the atmosphere should be looked upon (as it usually is) as a limited and bounded portion of the air; or whether we should, in a stricter sense than we did before, use the atmosphere and aërial

part of the world for almost equivalent terms; or else whether we should allow the word atmosphere some other notion in relation to its extent and limits; (for as to its spring and weight, these experiments do not question, but evince them.) But we are willing, as we said, to refer the matters to our Appendix, and till then to retain our wonted manner of speaking of the air and atmosphere. In the mean time (to return to our last-mentioned experiments) besides that so little a variation may be in great part imputed to the difficulty of making experiments of this nature exactly, and perhaps a good part of it to something of inequality in the cavity of the pipe, or even in the thickness of the glass; besides this, I say, the proportion betwixt the several pressures of the included air undilated and expanded, especially when the dilatation was great (for when the air swelled but to four times its first extent, the mercurial cylinder, though of near 23 inches, differed not a quarter of an inch from what it should have been according to mathematical exactness) the proportion, I say, was suitable enough to what might be expected, to allow us to make this reflection upon the whole; that whether or no the intimated theory will hold exactly (for about that, as I said above, I dare determine nothing resolutely till I have further considered the matter) yet since the inch of air, when it was first included, was shut up with no other pressure than that, which it had from the weight of the incumbent air, and was no more compressed than the rest of the air we breathed and moved in; and since also this inch of air, when expanded to twice its former dimensions, was able with the help of a mercurial cylinder of about 15 inches to counterpoise the weight of the atmosphere, which the weight of the external air gravitating upon the restagnant mercury was able to impel up into the pipe, and sustain above twenty-eight inches of mercury, when the internal air, by its great expansion, had its spring too far debilitated to make any considerable (I say considerable, for it was not yet so dilated as not to make some) resistance: since, I say, these things are so, the free air here below appears to be almost as strongly compressed by the weight of the incumbent air, as it would be by the weight of a mercurial cylinder of twenty eight or thirty inches; and consequently is not in such a state of laxity and freedom as men are wont to imagine; and acts like some mechanical agent, the decrement of whose force holds a stricter proportion to its increase of dimension, than has been hitherto taken notice of.

I MUST not now stand to propose the several reflections, that may be made upon the foregoing observations touching the compression and expansion of air; partly because we could scarce avoid making the historical part somewhat prolix; and partly because I suppose we have already said enough to shew what was intended: namely, that to solve the phænomena there is not of our adversary's hypothesis any need: the evincing of which will appear to be of no small moment in our present controversy to him that considers, that the two main things, that induced the learned examiner to reject our hypothesis, are, that nature abhors a vacuum; and that though the air have some weight and spring, yet, these are insufficient to make out the known phænomena; for which we must therefore have recourse to his Funiculus. Now as we have formerly seen, that he has not so satisfactorily disproved as resolutely rejected a vacuum, so we have now manifested, that the spring of the air may suffice to perform greater things than what our explication of the Torricellian experiments and those of our engine obliges us to ascribe to it. Wherefore since besides the several difficulties, that incumber the hypothesis we oppose, and especially its being scarce, if at all, intelligible, we can add that it is unnecessary; we dare expect, that such readers as are not biassed by their reverence for *Aristotle*, or the Peripatetick schools, will hardly reject an hypothesis, which, besides that it is very intelligible, is

now proved to be ſufficient, only to imbrace a doctrine, that ſuppoſes ſuch a rarefaction and condenſation, as many famous Naturaliſts rejected for its not being comprehenſible, even when they knew of no other way (that was probable) of ſolving the phænomena wont to be explicated by it.

PART III.

Wherein what is objected againſt Mr. BOYLE'S *Explications of particular Experiments, is anſwered.*

AND now we are come to the third and laſt part of our defence; wherein we are to conſider, what our examiner is pleaſed to object againſt ſome paſſages of our Phyſico-Mechanical Treatiſe. But though this may ſeem the only part, wherein I am particularly concerned; yet perhaps we ſhall find it, if not the ſhorteſt, at leaſt the eaſieſt, part of our taſk. Partly, becauſe our author takes no exceptions at the experiments themſelves, as we have recorded them (which from an adverſary, who in ſome places ſpeaks of them as an eye-witneſs, is no contemptible teſtimony, that the matters of fact have been rightly delivered): and partly, becauſe there are divers experiments which, together with their explications, the examiner has thought fit to leave untouched, and thereby allows us to do ſo too: and partly alſo, becauſe that (as to divers of thoſe experiments, upon which he animadverts) he does not pretend to ſhew, that our explications are ill deduced or incongruous to our principles; but only that the phænomena may be explained either better, or as well, by his hypotheſis; whereof he ſuppoſes himſelf to have demonſtrated the truth, together with the erroneouſneſs of ours, in the other parts of his book, eſpecially the third, fourth and fifth chapters. So that after what we have ſaid to vindicate the hypotheſis we maintain, and take away our author's imaginary Funiculus; it will not be requiſite for us, on ſuch occaſions, to examine his particular aſſertions and explications. Which advertiſement we hope the reader will be pleaſed to bear in mind, and thereby ſave himſelf and us the trouble of a great deal of unneceſſary repetition. Wherefore, preſuming he will do ſo, we ſhall not ſtay to examine the firſt and ſecond corollaries, which in this 17th chapter he annexes to the manner of emptying our receiver by our pump. Neither ſhould we ſay any thing as to his third and laſt corollary, but that we think fit to deſire the reader to take notice, that according to what he teaches in that place, the more the air is rarefied, the more forcibly it is able to contract itſelf.

A defence of our firſt and ſecond EXPERIMENTS.

AND to proceed now to his 18th chapter, which he intitles *De experimentis Boylianis*, we ſhall find, according to what we lately noted, that againſt the firſt experiment he objects nothing, ſave that if one of the fingers be applied to the orifice of the valve, when the pump is freed from air, the experimenter ſhall feel to his pain, that the ſucker is not thruſt inward by the external air, but, as the finger,

drawn inward by the internal. But this phænomenon of the intrusion of the finger into a cavity, where it finds no resistance, having been formerly accounted for according to our hypothesis, we shall not need to repeat our explication of it; though this mistaken phænomenon supplies our adversary with divers of his following animadversions, and indeed with a great part of his book. And accordingly his objection against our second experiment being of the same nature with that against the first, requires but the same answer: for it will not alter the case, that he adds upon this experiment, *hoc esse discrimen manifestum inter pressionem & suctionem, quod suctio efficiat hujusmodi adhæsionem, pressio autem minimè:* 'That this is the difference 'between pression and suction, that suction makes such an adhesion, and pression 'doth not:' Since to say so, is but to affirm, not to prove.

EXPERIMENT III.

WHAT our author would except against the 3d experiment he ought to have more intelligibly expressed: for whereas of a discourse, wherein I deliver several particulars, he only says, that *nullatenus satisfacit, ut legenti constabit:* I would not do the reader the injury to suspect him of taking this proofless assertion for a rational confutation: especially since, upon the review of that third experiment, I find nothing, that agrees not with my hypothesis, however it may disagree with the examiner's. But, to consider the explication he substitutes in the room of our doctrine, which he rejects, he gives it us in these words: *Hoc quoque experimentum principis nostris optime convenit: cum enim per illam emboli depressionem aër in cavitate brachii inclusus separetur ab eodem brachio, descendatque simul cum embolo (uti de aqua simul cum argento vivo descendente capite decimo tertio vidimus) fit ut in tota illa depressione, novæ semper ab aëre illo descendente superficies diripiantur simul & extendantur, ut ibidem de aqua est explicatum: cum itaque æque facile diripiantur & extendantur hujusmodi superficies in fine depressionis ac initio, mirum non est, quod eadem utrobique sentiatur deprimendi difficultas:* 'And even this experiment doth very well agree with our prin-'ciples: for seeing by this depression of the sucker, the air shut up in the cavity of 'the cylinder is separated from the cylinder, and doth descend together with the 'sucker (as we have, chap. 13. observed of water descending together with quick-'silver) it comes to pass, that in that whole depression new surfaces are taken from 'that descending air, and stretched out, as we have there explained it in the case of 'descending water. Since therefore such surfaces are as easily slipped off and ex-'tended in the end of the depression as in the beginning; it is no wonder, that there 'is found the same difficulty of depressing it at both times.'

By which thought he seems to intend an opposition to that part of the third experiment, which I opposed not against his opinion, but that of some learned vacuists; yet (not to mention, that he seems to have somewhat mistaken my sense) he offers nothing at all to invalidate my inference against them; but, instead of that, proposes a defence of his own opinion, which supposes the truth of this disproved hypothesis, and is either unsatisfactory even according to that, or else disagrees with what himself hath taught us but a little before. For it is evident, that the more the sucker is depressed, the more the cylinder is exhausted of air. And in his third corollary (which we lately desired the reader to observe) speaking of the air in the receiver (and the case is the same with the air in the cylinder) he affirms more than once, *Eo magis extendi ac rarefieri aërem relictum, quo plus inde exhauritur, majoremque proinde acquirere vim sese contrahendi:* 'That the air is so much the more

7 'extended

'extended and rarefied, by how much the more is thence exhausted, and so doth 'acquire a greater force of contracting itself.' Whereas here he would have us believe, that the little internal air, that was in the cavity of the shank of the stop-cock, does as strongly retract the sucker, or, which in our case is all one, resists its depression, when the sucker is yet near the top of the cylinder (and consequently when the included air is but a little dilated) as when the same sucker being forced down to the lower part of the cylinder, the same portion of remaining air must be exceedingly more distended.

EXPERIMENT IV.

IN the fourth experiment, touching the swelling of a bladder upon the removal of the ambient air, and proportionably to that removal, our author objects nothing against the explication we gave of it by the spring of the air included in the bladder, and distending it according as the pressure of the ambient air is weakened. But he endeavours also to explicate it his way, to which he says this circumstance does excellently agree, that upon the regress of the external air into the receiver, the tumid bladder immediately shrinks, because (saith he) by such ingress of the external air, the air in the receiver, which drew the sides of the bladder outward from the middle of it, is relaxed. Which explication, whether it be more natural than ours (that ascribes the shrinking of the bladder to the pressure of the air, that is let into the receiver) let the reader judge, who has considered what we have formerly objected against the examiner's Funiculus, and the relaxation of it upon the admission of air.

As for the reason likewise he adds, why a perforated bladder does not also swell, namely, that by the hole, how little soever, the included air is sucked out by the rarefied ambient, we leave it to the impartial reader to consider, whether is the more genuine explication, either ours (against which he has nothing to object) or his, which to make clearly out he ought (according to what we formerly noted disputing against his Funiculus) to shew us what kind of strings they are; which, though, according to him, strongly fastened to the inside of the receiver and the superficies of the Bladder, must draw just as forcibly one as another, how long soever they be without the bladder in comparison of those, that within the bladder draw so as to hinder the deduction of its sides. For experience shews, that in a perforated bladder the wrinkles continue, as if there were no drawing at all.

AND though he could describe, how such a string may be context, yet our explication will have this advantage in point of probability above his, that whereas he denies not, that the air has spring and weight, as we deny his Funiculus to have any other than an imaginary existence; and whereas he acknowledges, that by the instrument the air about the bladder is exhausted; to shew, that there needs no more than that, and consequently no Funiculus, to draw asunder the sides of the bladder, we can confirm our explication by the formerly-mentioned experiment of the ingenious *Peschal*, who carrying a flaccid foot-ball from the bottom to the top of an high mountain, found it to swell proportionably as he ascended, and as the weight and pressure of the ambient air decreased, and likewise to shrink again as he descended. And yet in this case there is no recourse to be had to a Funiculus of violently-rarefied air, to draw asunder every way the sides of the foot-ball. But however the examiner will be able to defend his explication, it may suffice us, that he has objected nothing against ours.

EXPERI-

EXPERIMENT V.

AGAINST the cause we assign of the fifth experiment he likewise objects nothing, but only ascribes the breaking of the bladder to the self-contraction of the rarefied air in the receiver. And therefore referring the reader to what we have newly said about the last experiment, we will with our author pass over the sixth and seventh, to which he has no quarrel, and proceed to the eighth.

EXPERIMENT VIII.

THIS is that, wherein we mention our having broke a glass-receiver, which was not globular, by the exhaustion of most of the inward air, whereby its debilitated pressure became unable to resist the unweakened pressure of the outward air. But this explication the examiner confidently rejects in these words: *At profecto non videtur credibile, mollissimum hunc aërem tam vehementer vitrum (tantæ præsertim crassitudinis, quantæ ibidem dicitur) undique sic comprimere, ut illud perfringat:* 'But truly 'it seems not credible, that this most soft air should so vehemently compress a glass 'on all sides (especially one of that thickness there mentioned) as to break it.' As if it were more credible, that the little air within (which, according to him, is so much thinner than common air) should be able to act more powerfully upon the glass than the air without, which himself confesses to be a heavy body, and which not only reaches from the surface of the earth to the top of the highest mountains, but which (as may not improbably be argued from what we have elsewhere delivered) may, for aught we know to the contrary, be heaped upon the receiver to the height of some hundreds of miles, nay, to I know not how many thousands, in case the atmosphere be not a bounded portion of the air, but reach as high as it.

See part 2. c. 5.

As for the explication he substitutes in these words, *Verius itaque respondetur, ideo sic fractum esse illud vitrum, quia per exhaustionem illam latera ejus vehementiùs introrsum sint attracta, quam ut (ob figuram illam resistendo minus idoneam) resistere potuerint. Cum enim inclusus aër lateribus vitri firmissimè adhæreat, nihil aliud erit aërem illum sic exhaurire, quam satagere latera vitri introrsum flectere:* 'It is therefore more truly 'answered, that the glass is therefore so broken, because by that exsuction its sides 'are more vehemently drawn inwards than (by reason of the figure unfit for re-'sistency) they were able to resist. For seeing the included air doth most firmly 'stick to the sides of the glass, to draw out the air will be nothing else, but to 'endeavour to bend the sides of the glass inwards.' By what we have already discoursed about the Funiculus, the reader may easily discern what is to be answered. Nor does our author here shew us any way, by which his imaginary strings should take such fast hold of the sides of the glass, as to be able to draw them together, notwithstanding the resistance they find from the close texture of the body to be broken.

EXPERI-

EXPERIMENT IX.

OUR explication of the ninth experiment he handles very severely: fer having briefly recited it, he proposes his objection against it thus; *Sed profecto nimis longè videtur hoc à veritate recedere: potestque vel inde solum satis refutari; quia si tanta sit pressura aëris sic per tubum illum in phialam descendentis, ut ipsam phialam perfringat, deberet profecto inclusam, cui immergitur ille tubus, valde quoque ante fractionem phialæ commovere, bullulasque in eadem excitare, &c. ut constat, siquis, insufflando per illum tubulum, aquam vel mediocriter sic premat. At certum est aquam, antequam frangatur sic phiala, nec tantillum moveri; ut experienti constabit.* 'But truly this seems too 'far removed from truth, and may be by this alone sufficiently refuted. Because if 'the pressure of the air, which descends by that tube into the phial, be so great as 'to break the phial itself, it ought certainly, before the breaking of the phial, very 'much to move the water, in which the tube is immersed, and to excite bubbles in 'it, *&c.* as appears, if any one blowing through that tube doth make but an ordi-'nary pressure upon the water. But it is sure, that the water, before the phial is 'broken, doth not move at all; as the experimenter will find.'

BUT I do confess, I do for all this think our explication more true, than well considered by our author. For the putting of water into the phial, that was broken, was done (as clearly intimated in the beginning of our narrative) upon a particular design (as indeed we tried divers other things with our engine, not so much with immediate reference to the spring of the air, as to make use of such trials in some other of our writings). And accordingly in the second trial mentioned in the same experiment, the water was omitted. But, notwithstanding this water, the sides of the glass being exposed to the pressure of the atmosphere, had that whole pressure against them before the exhaustion of the receiver; so that there needed no such blowing in of the air afresh as our author imagines, to effect the breaking of the phial, it being sufficient for that purpose, that the pressure against the convex superficies of it was taken off by the exhaustion of the receiver, the pressure against the concave superficies remaining as great as ever. And therefore we need not altogether deny what the examiner says, that *licet clausus superne fuisset tubulus ille, eodem tamen modo fracta sine dubio fuisset phiala:* 'Though the tube had been shut at the top, the phial had doubtless been 'broken after the same manner.' For, since in such cases the air (as we have often taught) is shut up with the whole pressure of the atmosphere upon it, it may almost as easily break the glass as if it were unstopt. And accordingly we mention, in the 36th experiment, the breaking of a thin glass hermetically sealed, upon the recess of the ambient air. But, how confidently soever our author speaks, I thought fit to add the word *almost*, because we observed in the 39th experiment, that such thin phials (and thick ones will not break) are subject upon the withdrawing of the ambient air to retch a little, whereby the spring of the air within the phial might in some cases (I say, in some) be so far weakened, as not to be able to break it, unless assisted by the pressure of the atmosphere, wherewith it communicates, and which leans upon it. And when the phial does actually begin to break, then the pursuing pressure of the outward air upon the yielding air within the phial may help to throw the parts of the glass more forcibly asunder.

ALL the experiments from the 9th to the 17th exclusively our examiner leaving uncensured, we may with him advance to the consideration of the 17th.

The

The 17th EXPERIMENT *defended.*

IN this we relate how, when we made the Torricellian experiment, we shut up the restagnant mercury together with the tube and the suspended mercurial cylinder (of about 29 inches) in our receiver, that by drawing off and letting in the air at pleasure upon the restagnant mercury, and consequently weakening and increasing its pressure, we might make it more clearly appear than hitherto had been done by experiment, that the suspension of the mercurial cylinder, and the height of it, depended upon the greater or lesser pressure of the air. But against our explication of this experiment (which has had the good fortune to convince and satisfy many ingenious men) the examiner objects nothing in particular, contenting himself to have recourse here also to his Funiculus. Yet two observations of ours he is pleased to take notice of.

THE first is, that though the quicksilver were exactly shut up into our receiver after the manner newly declared, yet the suspended quicksilver did not descend: whence having said, that I argue, that it is now sustained not by the counterpoise of the atmosphere, but by the spring of the air shut up in the receiver, he subjoins only this; *Sed rectius sane infertur, cylindrum illum nihil ibidem antea præstitisse:* 'But it is 'more rightly thence inferred, that that cylinder did nothing there before.' But whether this be not *gratis dictum*, we leave the reader to collect from what we have formerly discoursed in the second part of this defence of the spring of the air, especially from that experiment, by which it appears, that spring may sustain a far higher cylinder of quicksilver.

IN the second observation he mentions of ours, he summarily recites our explication of the descent and ascent of the mercury in the tube, by the debilitated and strengthened spring of the air. But without finding fault with our application of that principle to the phænomena, he says, that he has sufficiently refuted the principle itself in the fourth chapter (which how well he has done, we have already seen); and therefore explicates the matter thus: *Dico igitur* (says he) *argentum per illam exhaustionem sic in tubo descendere, quod deorsum trahatur ab aëre, qui incumbit argento restagnanti: siquidem incumbens ille aër jam per exhaustionem valde rarefactus & extensus sese vehementer contrahit, & contrahendo conatur etiam subjectum sibi argentum restagnans e suo vasculo elevare; unde fit ut (argento illo restagnante minus jam gravitante in fundum sui vasculi) argentum quod est in tubo descendat; ut per se patet. Adeoque mirum non est quod, ingrediente postea aëre externo, rursum argentum ascendat, cum per illum ingressum vis illa sic elevans argentum restagnans debilitetur:* 'I say then, that the quicksilver 'doth by that exhaustion so descend in the tube, because it is drawn downwards by 'the air incumbent upon the restagnant quicksilver. For that incumbent air, being 'by its exhaustion greatly rarefied and extended, vehemently contracts itself, and by 'this contraction doth endeavour to lift the restagnant mercury out of its vessel; 'whence it comes to pass, that (the restagnant mercury now less gravitating upon the 'bottom of its vessel) the quicksilver in the tube must descend, as is manifest in it-'self: so that it is no wonder, that the external air afterwards entering, the quick-'silver again ascends, seeing by that ingress the force, which elevates the restagnant 'quicksilver, is weakened.' But this explication supposing such a Funiculus as we have already shewn to be but fictitious, the reader will easily gather what is to be judged of it from what has been already delivered. Wherefore I shall only subjoin, that by this explication, were it admitted, there is only an account given of that

that part of our ſeventeenth experiment, which relates to the deſcent of the mercury below its wonted height, and its reaſcent to it. But as for our having, by the forcing in ſome more air into the receiver, impelled the quickſilver to a conſiderably greater height than it is wont to be ſuſtained at in the Torricellian experiment, I confeſs I underſtand not how the examiner gives an account of it in the following words (which are immediately annexed to thoſe we laſt recited of his, and which are all that he employs to explicate this notable phænomenon:) *Atque hinc etiam redditur ratio alterius, quod ibidem quoque notatur, nempe quod per violentam intruſionem aëris externi in recipientem aſcenderit argentum notabiliter ſupra digitos* 29½. *Nam ſicut per extractionem aëris argentum infra ſtationem detrahitur, ſic etiam per intruſionem novi ſupra eandem elevabitur:* 'And hence is a reaſon alſo given of another thing, which is there noted, namely, 'that by the violent intruſion of the external air into the receiver the quickſilver 'aſcended conſiderably above 29 inches and an half. For as by the extraction 'of the air, the quickſilver is depreſt below its ſtation, ſo by the intruſion of 'new air it is elevated above it.' For in this paſſage I ſee not how he himſelf does not rather repeat the matter of fact, than give any account how it is performed. And if it be alleged on his behalf, that according to his principles it may be ſaid, that, upon the preſſure of adventitious air upon the reſtagnant mercury, the Funiculus in the tube, that was not able before to draw it up above 29½ inches, is now enabled to draw it up higher; I demand upon what account this new air does thus preſs againſt the reſtagnant mercury, and impel up and ſuſtain that in the tube. It will not be ſaid, that it is by its weight; for as much mercury as may be thus impelled up above the uſual ſtation will weigh a great many times more than the air forced into the receiver. And therefore it remains, that the additional air counterpoiſes the additional mercury by its ſpring. And if we conſider withal, that there is no reaſon to doubt (eſpecially conſidering what we have formerly delivered upon trial touching the power of compreſt air to impel up quickſilver) but that, had we not been afraid of breaking our veſſel, we might by forcing more air into the receiver have impelled it up to the top of the tube, and kept it there; we ſhall ſcarce deny, but that, ſuppoſing there could be no ſuch Funiculus as our examiner's *in rerum natura*, the preſſure of the incumbent air alone might ſuffice to keep a correſpondent cylinder of mercury ſuſpended; and that without any attraction of the reſtagnant mercury by a Funiculus of violently-diſtended air in the receiver, the quickſilver in the tube may be made to reſt at any height greater or leſſer, provided it exceed not 30 inches, only becauſe its weight is juſt able to counterbalance the preſſure of the contiguous air.

I KNOW not whether I may not add (to expreſs an unwillingneſs to omit what ſome may think proper to do my adverſary right) that it may be ſaid for the examiner, that he in the 11th page acknowledging with us a power in the air to recover its due extenſion, if it be crouded into leſs room than its diſpoſition requires; a man may from that principle ſolve the phænomena in queſtion, by ſaying, that the air in the receiver being forcibly compreſſed by the intruſion of freſh air into the ſame veſſel, does by its endeavour to recover its due expanſion preſs upon the reſtagnant mercury, and force up ſome of it into the tube. But this explication, though it agree with what the author teaches in a place very diſtant from his notes upon our 17th experiment, now under debate; yet ſtill it is not clear to me, how, by what he ſays in theſe notes, the phænomenon is accounted for as the word *hinc* imports it to be. But otherwiſe I need not quarrel with the explication, ſince without recurring to the Funiculus for the ſuſtaining of the additional mercury, the ſolution of the phænomenon is given upon the ſame principle that I employ.

EXPERIMENT XVIII.

OUR examiner in his animadversion upon the 18th experiment, having recited my conjecture as the cause, why a cylinder of mercury did in winter rise and fall in the tube sometimes as water is wont to do in a weather-glass, according to the laws of heat and cold, and sometimes quite contrary thereunto; adds, that this experiment does strongly enough overthrow our hypothesis of the atmospherical cylinder, and clearly shew, that the quicksilver is not sustained by it: *Nam* (says he) *si hoc ab eo sustentatum fuisset, debuisset potius frigidiore tempore ascendere quam descendere, eo quod aër tunc multo densior esset & gravior. Itaque non sustentatur argentum ab aëris æquipondio, ut asseritur:* 'For if it were kept up by that, it ought rather to ascend 'than descend in colder weather, because the air then would be more dense and 'heavy. Therefore the quicksilver is not upheld by the æquilibrium of air, as is 'asserted.' And by the same argument he concludes against the mercury's being sustained by the spring of the air. But in his animadversions upon this experiment he seems to have been too forward to reprehend; for he neither well confutes my conjecture, nor substitutes so much as a plausible one in the stead of it. And as to his objection, I answer,

FIRST, That it doth not conclude; because that as sometimes the quicksilver in the tube did rise in warmer, and fall in colder weather; so at other times it did rather emulate the ascent and descent of water in a weather-glass.

SECONDLY, Though it be true, that cold is wont to condense this or that parcel of air, and that a parcel of air may be made heavier by condensation; yet that is in regard of the ambient air, that retains its wonted laxity, in which the condensed air is weighed. But our author has not yet proved, that in case the cold of the winter should condense the whole incumbent atmosphere, it would then gravitate sensibly more upon the restagnant quicksilver than before. As a pound of wool will not sensibly vary its weight, though the hairs, whereof it is composed, be made to lie sometimes in a looser, sometimes in a closer order.

AND, thirdly, his objection does as little agree with his doctrine as with my conjecture: for in the 50th page, where he gives us an account, according to his principles, of the rising and falling of water in a weather-glass, and compares it with the suspension of quicksilver, he tells us, *Hinc fit quod, contracto hoc Funiculo per frigus, aqua illa tempore frigido ascendat, descendat autem tempore calido, eo quod per calorem Funiculus ille dilatetur:* 'Hence it comes to pass, that this Funicle being con-'tracted by the cold, the water doth ascend in cold weather; but doth descend in 'hot, because by heat the Funicle is dilated.' So that, according to the examiner himself, the quicksilver ought to have ascended in colder, and descended in warmer weather. Now, although I proposed my thoughts of the difficult phænomenon under consideration but as a conjecture, and therefore shall be ready to alter them, either upon further discovery or better information; yet I see not why it should be postponed to the examiner's, who, though he rejects our explication, substitutes no other than what may be gathered from these words; *Ego certè non dubito, quin dentur hujusmodi occultæ causæ, quibus Funiculus ille subtilis, quo in tubo suspenditur argentum (ut dictum est capite decimo) modo producatur, modo abbrevietur, &c. sicque argentum nunc demittat, nunc elevet:* 'I truly do not doubt, but there are some such occult causes, by which the slender Funicle, that suspends (as we mentioned in the tenth chapter)

'the quickſilver in the tube, is ſometimes lengthened, ſometimes ſhortened, and ſo 'doth ſometimes let down, and ſometimes lift up the quickſilver.' For, ſince we have made it probable, that the copious fumes ſometimes ſuddenly aſcending into the air, and rolling up and down in it, ſometimes ſenſibly altering (if good authors may be credited) the refraction of it, and ſince ſome other cauſes, mentioned in our eighteenth experiment, may alter the denſity and gravity of the air, that leans upon the reſtagnant mercury; I ſuppoſe the reader will think it more intelligible and probable, that alterations, other than thoſe produced by heat and cold, may happen to the incumbent atmoſphere, which freely communicates with the neighbouring air, and may thereby become ſometimes more ſtuffi, and ſometimes more deſtitute of adventitious exhalations, than that ſuch changes ſhould happen to a Funiculus included in glaſs; which, according to our author, is impervious to the ſubtileſt ſteams that are, and concerning which he offers not ſo much as a conjecture, upon what other account it can happen to be ſometimes contracted, and ſometimes ſtretched.

EXPERIMENT XIX.

UPON this the examiner has only this ſhort animadverſion; *In decimo nono oſtendit aquam eodem modo per exhauſtionem recipientis deſcendere, quo in præcedente deſcendere oſtenderat argentum vivum; cujus cum eadem ſit ratio non eſt cur amplius ei inſiſtamus.*' 'In the 19th he ſhews, that water doth in the ſame manner deſcend upon 'the exhauſting the receiver, as he had ſhewn quickſilver in the foregoing chapter to 'deſcend. Of both which, ſeeing there is the ſame cauſe, there is no reaſon we ſhould 'any longer inſiſt on this.' In which words, ſince he offers nothing new or peculiar to ſhew any incongruity in our explication to our principles, which agree very well with the new phænomena of the experiment; we are content to leave the reader to judge of the hypotheſes themſelves, which of the two is more probable, either ours, that only requires, that the air in the receiver ſhould equally reſiſt a cylinder of water and of quickſilver, when their weight is but the ſame, though their altitudes be not; or the examiner's, which exacts, that (according to what we formerly elſewhere noted) bodies of ſuch differing nature and texture as quickſilver and water ſhould need but juſt the ſame weight or ſtrength to rarefy them into a Funiculus.

EXPERIMENT XX.

IN his examen of this experiment our author makes me infer from the phænomena he repeats, that not only the air, but the water alſo has a ſpring. But though I ſuſpect not, that he does wilfully miſtake my ſenſe, yet by what I write in this and the following experiments the reader may well enough perceive, that I ſpoke but very doubtfully of a ſpring in the water; nay, and that I did in the 58th page expreſsly teach, that the intumeſcence of it might (at leaſt in great part) proceed from that of the ſmall parcels of air, which I thought to be uſually harboured in the body of that liquor.

BUT whereas I aſcribe the appearance of the bubbles in the water to this, that upon the exhauſtion of ſome of the air incumbent on the water, the preſſure of what remains is much debilitated, whereby the little particles of air lurking in the water are allowed to expand themſelves into bubbles; he rejects this explication as manifeſtly

Sed contra manifesta.

false: *Nam* (says he) *si ita fieret, deberent profectò hujusmodi bullulæ, non è fundo vasis sic ascendere (uti tam in hac quam in sequentibus experimentis, in quibus de istis bullis semper agitur, asseritur) sed è superiore parte aquæ, ubi minus premuntur, ut per se est manifestum.* 'For if it were done so, these bubbles ought not so to have ascended 'from the bottom of the vessel (as it is asserted they did, both in this and the follow-'ing experiments that treat of bubbles) but from the upper part of the water, where 'they are less compreſt; as it is apparently manifest.' But why he should be here so peremptory, I confess I do not, for all this objection, yet see: for in the bottom of the next page he says, he will not deny, but that aërial particles latitant in the other parts of the water (he had before spoken of the bottom of it) may be extended into bubbles by his way of rarefaction. And that we particularly mentioned the rising of bubbles, even from the bottom of the water, was because that circumstance seemed to deserve a peculiar note; and not (as he seems to imagine) as if the bubbles did not also rise from the superior parts of the liquor, since we did take notice of it about the middle of the 56th page *. And we often in this and the following experiments observed, that the ascending bubbles grew bigger the nearer they came to the top. Which agrees more clearly with our hypothesis, wherein their conspicuous swelling, as they ascend, is attributed more to the lessening of the pressure of the incumbent air, than to the decrement of the weight of the incumbent water (since when the surface of this liquor is leaned upon by the atmosphere, the ascending bubbles scarce sensibly increase in vessels no deeper than ours) than with the explication, which the examiner gives in these words; *Respondeo, aquam per illam aëris exhaustionem non sponte sic ascendere, sed sursum violenter trahi, ac elevari à rarefacto illo aëre sese contrahente. Quemadmodum enim aqua aliqualem patitur compressionem (ut experientiâ constat) ita & aliqualem quoque hic patitur distensionem. Atque hinc clarè patet, cur potius à fundo vasis quam à parte aquæ superiore oriantur hujusmodi bullæ. Cum enim vehemens illa suctio conetur aquam à fundo phialæ elevare, nascitur ibidem subtilis quædam materia, quæ in bullas conversa sic ascendit, uti capite decimo quinto in quarto experimento dictum est.* 'I answer, that the water, upon exhaustion of the air, doth not so 'ascend of its own accord, but is violently drawn or lifted upwards by that rarefied 'air contracting itself. For as water doth suffer some compression (as appears by 'experience) so here also it suffers some distension. And hence it is clearly manifest, 'why these bubbles should arise rather from the bottom of the vessel, than from the 'upper part of the water. For when that vehement suction doth endeavour to ele-'vate the water from the bottom of the phial, there arises there a certain subtile 'matter, which being turned into bubbles, doth so ascend, as is mentioned in the '15th chapter and the 4th experiment.' For, whatever he may think, it does not hence so clearly appear, how the endeavour only of the Funiculus to draw up the water from the bottom of the phial, to which, that endeavour notwithstanding, it remains contiguous, should generate in some parts of the bottom of the glass, and not in others, such a subtile matter as he tells us of. And I suppose the reader will, as well as I, wish he had more intelligibly declared, how this strange generation of subtile matter comes to be effected. And I presume it will likewise be expected, that he also declare, why both in our case and in the Torricellian experiment the bubbles grow so much larger by being nearer the top of the liquor; if, as he rejects our explication of this circumstance, the effect of the suction he speaks of be greater upon

* See also in the 13d Experim. these passages,—*And this effervescence was so great in the upper part of the water, &c.* As also,—*Effervescence was confined to the upper part of the water, unless, &c.*

the

the lower part of the liquor than the upper, to which alone nevertheless his Funiculus, that is said so to draw the liquor, is contiguous.

Our author making no particular objection against the ten following experiments, we also shall pass them by, and fall with him upon the consideration of the 31st experiment.

EXPERIMENT XXXI.

UPON this our author having recited our conjecture, as the cause why two very flat and smooth marbles stick so closely together, that by lifting up the uppermost you may take up also the lowermost, approves my way of examining that conjecture. But whereas I say, that the reason, why, though the marbles were kept together by the pressure of the ambient air, yet they did not fall asunder in our exhausted receiver, no not though a weight of four ounces were hung at the lower stone, might be, that by reason of some small leak in the receiver the air could not be sufficiently drawn out; yet he tells us, with his wonted confidence, *Certum esse sententiam illam vel hoc solo experimento satis refelli:* 'It is certain, that that opinion is sufficiently 'refuted by this single experiment.' But possibly he would have spoken less resolutely, if he had made all the trials about the adhesion of marbles, that we relate ourselves to have made in the short history we have published of *fluidity and firmness*. For our examiner speaks, as if all that we ascribe to the air in such experiments were to sustain the lower marble with the weight perhaps of a few ounces; whereas in case the air be kept from getting in at all between the stones, it may (according to our hypothesis) sustain a weight either altogether, or well-nigh equal to that of a pillar of air as broad as the basis of the lower marble, and as long as the atmosphere is high, or to the weight of a pillar of quicksilver of the same thickness, and about thirty inches long; these two pillars appearing by the Torricellian experiment to counterpoise each other. And therefore since in the seventeenth experiment, when we had exhausted our receiver as far as we could, there remained air enough to keep up in the tube a cylinder of about an inch long of quicksilver; and since the broader the contiguous marbles are, the greater weight fastened to the lowermost may be sustained by the resistance of the air (as is obvious to him, that considers the hypothesis, and as we have proved by experiment in the forementioned tract) it need be no wonder, that the air remaining in the receiver should be able to support the lowermost marble, whose diameter was near two inches, and a weight of four ounces, those two weights being inferior to that of a mercurial cylinder of that diameter and an inch in length. And though it were not, yet we are not sure, that the receiver was as well emptied when we made the 31st experiment, as when we made the 17th. And (if my memory does not much misinform me) it was with the same pair of marbles, that in the presence of an illustrious assembly of virtuosi (who were spectators of the experiment) the uppermost marble drew up the lowermost, though that was clogged with a weight of above 430 ounces.

As for the account the examiner substitutes of our phænomenon, I know not whether many readers will acquiesce in it. For, not to insist upon the objection, which himself takes notice of, that, according to him, the distended air in the receiver should draw asunder the adhering marbles; his explication supposes, that there cannot naturally be a vacuum: whence he infers that, *Necesse erat, ut lapis ille non aliter descenderet, quàm relinquendo post se tenuem hujusmodi substantiam, qualis ab argento vivo aut aquâ sic descendentibus relinqui solet.* 'It must needs be, that that stone could not 'otherwise descend, than by leaving behind it such a thin substance, as is left by quick-

'silver or water descending in like manner.' But whereas he adds, that the cause of the obstinate adhesion we meet with in our case is, that such a substance is far more difficult to be separated from marble than from quicksilver, or any other kind of body; that assertion is precarious. And though I have tried experiments of this nature with stones of several sizes, perhaps an hundred times, yet I never could find, that by their cohesion they would sustain a weight greater than that of a pillar of the atmosphere that prest against the lowermost: which is a considerable circumstance, that much better agrees with our explication than our adversary's. And whereas he further says, *Unde existimo planè, si perfectè complanata fuerint duo marmora sic conjuncta, ita ut nullus omnino aër inter utrumque mediaret, non posse ea ullis humanis viribus ab invicem divelli.* 'Whence I plainly conceive, that if two perfectly polished marbles 'were so joined, that no air at all were left between them, they could not be drawn 'asunder by all the power of man.' I hope I need not tell the reader, that whether or no this agree with what he had immediately before taught of the separableness of a subtile substance, even from marble, so bold and improbable an assertion requires the being countenanced with a much better proof, than the only one he subjoins in these words; *Uti etiam confirmat exemplum, quod ibidem adducit author de lamina ænea, tabulæ cuidam marmoreæ ita adhærente, ut à lacertoso juvene, de suis viribus gloriante, non potuerit per annulum centro ejus affixum inde elevari.* 'Which also is confirmed by the 'example the author there brings of a brass plate sticking so close to a marble table, 'that by a lusty youth, who boasted of his own strength, it could not be lifted off 'by a ring fixed to its center.' For sure there is great odds betwixt the strength of a man unassisted by an engine, and the utmost extent of human power. And indeed, according to our hypothesis, and without having recourse to nature's dreading of a vacuum, the case is clear enough. For, supposing the plate to be of any considerable breadth, the pillar of the atmosphere, that leaned upon it, and must at the instant of its deserting the superficies of the table all at once be lifted up with it, may well exceed the force of a single man, especially in an inconvenient posture; since by the cohesion of a pair of marbles of about three inches diameter, I did, with my own hands, take up above a thousand and three hundred ounces.

EXPERIMENTS XXXII, *and* XXXIII.

AGAINST our explication of these two, which our author examines together, he objects nothing peculiar, but contents himself to explicate them by his Funiculus. Wherefore neither shall we need to frame any peculiar defence for it, especially if the reader will be pleased to refer hither as much of what we opposed to his animadversion on the third experiment, as is justly applicable to our present controversy. Our author indeed endeavours to prove his explication by saying, that the distended air in the exhausted cylinder draws up the sucker with the annexed weight: *Eodem ferè modo, quo videmus in cucurbitulis dorso ægrotantis applicatis, in quibus, extincta jam flamma, rarefactus aër se contrahens carnem tam vehementer, uti videmus, elevat attrahitque intra cucurbitulam.* 'Almost in the same manner as we see in cupping-'glasses applied to a patient's back, in which the flame being extinct, the rarefied air 'contracting itself doth so vehemently (as we see) lift up, and draw the flesh within 'the glass.' But that phænomenon is easily enough explicable in our hypothesis, by saying, that upon the vanishing of that heat, which strengthened the pressure of the included air, the spring of it grows too weak to resist any longer the pressure of the ambient air; which thereupon thrusts the flesh and neighbouring blood of the patient

tient into the cupping-glaſs, almoſt after the ſame manner, as we formerly taught the pulp of the finger to be thruſt into the deſerted cavity of the glaſs-tube in the Torricellian experiment.

EXPERIMENTS XXXIV, XXXV, *and* XXXVI.

TO theſe our author ſaying nothing but this, *In his tribus nihil peculiariter occurrit hic explicandum, cujus ratio ex jam dictis non facile pateat:* 'In theſe three 'there is nothing occurs to be peculiarly here explicated, the account of which is 'not eaſy from what is already delivered:' we alſo may be allowed to pretermit them, and paſs on to

EXPERIMENT XXXVII.

OF the appearance of light or whiteneſs, mentioned in this experiment, the examiner confeſſes, that we have aſſigned a cauſe probable enough, by referring it to the vehement and ſudden commotion of the included air. And indeed though I do ſtill look upon ſome of the things; that I heſitantly propoſed about this difficult phænomenon, but as mere conjectures, and though he annexes his explication of it, yet I ſee not but that it is coincident with ours, or not better than it. For, to what I ſaid of the commotion of the parts of the air, he adds only in two or three ſeveral places their being violently diſtended; which how it improves the explication of the phænomenon, I do not readily ſee. And whereas he ſubjoins, *Exiſtimo autem dicendum potius candorem illum eſſe lumen quoddam reflexum, quam innatum, eo quòd (uc teſtatur author) in tenebris non appareat, ſed ſolum de die aut accenſa candela:* 'But I think, 'that whiteneſs ſhould be rather called a reflex than an innate light, becauſe, as the 'author bears witneſs, it appears not in the dark, but only in the day, or by candle-'light:' I preſume the attentive reader will eaſily diſcern, that his opinion is much what the ſame that I propoſed and grounded on the ſame reaſon. But the chief difficulty in this abſtruſe phænomenon, namely, why we meet with it but ſometimes, our examiner's explication leaves untouched.

EXPERIMENTS XXXVIII, *and* XXXIX.

AGAINST theſe our author makes no peculiar objections.

EXPERIMENTS XL, *and* XLI.

BUT in his animadverſions upon theſe, having told the reader, that I ſeem to aſcribe the ſudden extinction of the included animals to the exceſſive thinneſs of the air remaining in the receiver, made by the receſs of what was drawn out, unfit for reſpiration; he adds reſolutely enough, *Verum impoſſibile videtur, ut hujuſmodi animalcula ob ſolum defectum craſſioris aëris tam cito moriantur:* 'But it ſeems impoſſible, 'that ſuch animals ſhould die ſo ſoon only for want of a thicker air.' But gives no other reaſon, than that they die ſo ſoon, which is no more than what he ſaid in the newly-cited words, and beſides is grounded upon ſomething of miſtake. For the creatures

creatures he mentions were a bee, a fly, and a caterpillar, and those included too in a small receiver, which could be suddenly exhausted: and these indeed became moveless within a minute of an hour; but that minute was not (as the word is often used to signify in English) a moment, but the sixtieth part of an hour. And though these insects did in so short a time grow moveless, yet they were not so soon killed; as appears by the narrative. The sanguineous animals, that did indeed die, were killed more slowly. And I remember, that having purposely inquired of a man (used to go under water by the help of an engine, wherein he could carry air with him to the bottom of the sea) how long he could endure, before he was accustomed to dive, without breathing or the use of a spunge*? he told me, that at first he could hold out about two or three minutes at a time: which made me think, that divers become able to continue under water so long, either by a peculiarly-convenient constitution of body, or by a gradual exercise. And I am apt to think, that he did, as men are wont to do, when he said two or three minutes, mean what is indeed a much shorter time than that, when exactly measured, amounts to. For, having purposely made trial upon a couple of moles, that were brought me together alive, one of them included in a small, though not very small, receiver, was between two and three minutes in killing; whereas the other being immediately after detained under water, did not there continue full a minute and a quarter, before it finally ceased from giving any sign at all of life. By which trial it may appear, that it is not impossible, that the want of respiration should dispatch an animal in as little time as is mentioned in the experiment I am now defending. And indeed our author either should have proved, that it is not possible for the want of air to destroy animals so soon, or should have given us some better account of the phænomenon. For whereas he teaches us, that, according to his doctrine, the little animals above-mentioned were so soon killed; *Quia per rarefactam illum aërem sese contrahentem extractus fit eorum halitus:* 'Because by the self-contraction of the rarefied air their breath is drawn out of their 'bodies:' I see not, that hereby, if he explicate the phænomenon otherwise than we, he explains it better; for he seems to speak, as if he thought, his halitus to be some peculiar part of the animal, in which his life resides. And besides, he seems not to consider, that whereas, according to me as well as according to him, the air contained in the lungs (supposing these animalcula have any) must in great part pass thence into the receiver; (for whether that be done by the spring of the air itself, that was harboured in the lungs, or the traction of the more rarefied air in the receiver, is not material in our present case;) the examiner must, as well as I, render a reason, why the extenuation or recess of the halitus should cause the hasty death of the included animals; and condemning my conjecture, he ought to have substituted another reason. And though he subjoins these words, and concludes with them; *Atque hinc quoque ortæ sunt vehementes illæ convulsiones, quas ante mortem passas esse aviculas quasdam memorat ibidem author:* 'And thence also arose those vehement convulsions, which 'the author there mentions certain small birds to have endured before their 'death:' yet I doubt not but the reader will think it had not been amiss, that the author had more intelligibly reduced these tragic symptoms from his assumption, for the sake of those, that are not anatomists and physicians enough to discern how his Funiculus could produce these effects.

For my part, as in the 41st experiment I tendered my thoughts concerning respiration but doubtingly, so I am yet unwilling to determine resolvedly in a matter of that difficulty.

* See more concerning this objection in the answer to it, as it is proposed by Mr. *Hobbes*.

E X.

EXPERIMENTS XLII, *and* XLIII.

IN his examen of these two last of our physico-mechanical experiments, the author contents himself to endeavour to explicate the phænomena recited in them by the contraction of the rarefied air; which, according to him, endeavours to draw up the subjacent water out of the phial, whereby it vehemently distends the parts of that water, as he taught in the like case upon the 20th experiment. But since we have already considered his animadversion upon that, although this presumed distension of the water is not visible, that we have observed, when cold water, that has been at first freed from his interspersed air, is put into the receiver, notwithstanding that the Funiculus should in that case also distend it; we are so afraid of tiring out the reader's patience by the frequent repetition of the same things, that we will leave it to him to judge, which of the two explications, the examiner's or ours, is to be preferred, without troubling him and ourselves with defence of accounts, against which our adversary does not here make any peculiar objections.

AND thus have we by God's assistance considered what the examiner hath been pleased to oppose, either against our particular explications, or against the hypotheses, that divers of them suppose: wherein I have been the more particular and prolix, because I would willingly excuse myself and others from the trouble of any more disputes of this kind. I hope there is not in my answers any thing of asperity to be met with; for I have no quarrel to the person of the author, or his just reputation; nor did I intend to use any more freedom of speech in the answering his objections, than his resolute way of proposing divers of them made it, on those occasions, needful for the caution of readers, who are not acquainted with our differing ways of writing, and perhaps have not observed that some men are wont to consider as much what they propose but with a perhaps, or some such expression of diffidence, as others do what they deliver far more resolutely. And though being very far from being wedded to my opinions, I am still ready to exchange them for better, if they shall be duly made out to me (which I think it possible enough they may hereafter be); yet peradventure the reader will think with me, that the examiner has not given me cause to renounce any of them, since the objections he has proposed against me have been sufficiently answered, and since the hypothesis he would substitute in the room of ours (besides that it is partly precarious) supposes things, which divers of the eminentest wits of our age (otherwise of differing opinions) profess they cannot admit, or so much as understand: whereas the weight and spring of the air are not denied by our author himself, and are demonstrable by experiments, that are not controverted betwixt us. Which things I represent for the defence of what I think the truth, and not to offend my learned adversary, who shall have my free consent to be thought to have failed rather in the choice than in the management of the controversy. Though, since this passes for his first book, and since, consequently, he is not like to have been provoked, or engaged in point of reputation, to challenge me or any of those far more eminent persons he has named among his adversaries, I am induced by the severity wherewith I have known eminent Virtuosi speak of his attempts, and particularly of his Funiculus, to fear, that some of those he has needlessly opposed, will be apt to apply to him that of St. *Austin* against some of his adversaries, that had disputed against him with much more subtility than reason, *In mala causa non possunt aliter, at malam causam quis eos coëgit habere?* 'In a bad 'cause they can do no other; but who compelled them to undertake a bad cause?'

 But

But this notwithstanding, I am, as I was going to say, content, my adversary should be thought to have said for his principles as much as the subject will bear; nor would I have it made his disparagement, that I have declared that his whole book has not made me depart from any of my opinions or explications, since his hypothesis and mine being inconsistent, it may be looked upon as a sign rather that each of us have, than that either of us have not, reasoned closely to his own principles, that the things we infer from our contrary suppositions do so generally disagree.

An Explication of RAREFACTION.

THE chief arguments of the author of a certain treatise *De corporum inseparabilitate*, whereby he endeavours to invalidate the hypothesis of the weight and spring of the air, and to set up and establish instead thereof an unintelligible hypothesis of attraction, performed by I know not what strange imaginary Funiculus, are only five; two against the former, and three for the latter. The first of which is, that the weight and spring of the air are not sufficient to perform the effects ascribed to them: the second, that could they be performed by that hypothesis granted, yet the way of this strange spring itself is not intelligibly explained or explicable by the defenders of it. Now the former of these being little else but a bare affirmation, and the latter bearing some shew of demonstration, I shall endeavour to examine it as I find it set down in his 20th, 21st, 22d, 23d, and 24th chapters, to which (especially the 23d) he very often in his book refers the reader for satisfaction, pretending there to evince, that rarefaction cannot be made out any otherwise than by supposing a body to be in 2, 3, 4, 10, 100, 1000, 1000000 of places at the same instant, and adequately to fill all and every one of those places.

FIRST therefore, we will examine his negative, and next his affirmative, arguments for this strange hypothesis.

HIS negative I find in the 20th chapter, where he endeavours to confute the two ways of explicating the rarefaction and spring of the air, namely, that of the Vacuists and that of the Plenists.

CONCERNING the first of these, we find him conclude it impossible, first, because he had before proved, that there can be no vacuum, which being done by a circle (*viz.* there is no vacuum in the tube, because nature abhors a vacuum, and we see nature abhors a vacuum, because she will not suffer a vacuum in the tube above the mercury, but, to prevent it, will continually spin the quicksilver into superficies, and never diminish the body of it) will suffer me to pass to his next; which is, that this way is false, because in the experiment of the carp's bladder the air is rarefied 1000 times bigger; nay, in respect of the body of gold, it has 1000000 times less matter in equal spaces. And this, says he, is a phænomenon, that is impossible ever to be made out by interspersed vacuities. Now that the Vacuists cannot presently, by so bold an assertion as this, be made to forsake their principles, he may perceive by these following solutions, which I shall give of all the phænomena he recites, flowing naturally from an hypothesis, that I shall for the present assume. Let us suppose then the particles of bodies, at least those of the air, to be of the form of a piece of ribbon, that is, to be very long, slender, thin and flexible *laminæ*, coyled or wound up together as a cable, piece of ribbon, spring of a watch, hoop, or the like, are: we will suppose these to have all of them the same length, but some to have a stronger, others a weaker spring. We will further suppose each of these so coyled

coyled up to have ſuch an innate circular motion, as that thereby they may deſcribe a ſphere equal in diameter to their own, much after the manner that a meridian turned about the poles of a globe will deſcribe by its revolution a ſphere of the ſame diameter with its own in the air. By this circular motion the parts of the *laminæ* endeavouring to recede from the center or axis of their motion, acquire a ſpringineſs outward like that of a watch-ſpring, and would naturally fly abroad until they were ſtretched out at length; but that being incompaſſed with the like on every ſide, they cannot do it without the removal of them, as not having room ſufficient for ſuch a motion. And the faſter this circular motion is, the more do the parts endeavour to recede from the axis, and conſequently the ſtronger is their ſpring or endeavour outward. Theſe ſpringy bodies thus ſhaped and thus moved are ſufficient to produce all the phænomena he names as impoſſible to be explicated. And, firſt, for the buſineſs of expanſion, it will very naturally be explained by it. As let us ſuppoſe, for inſtance, the diameter of theſe ſmall coyled particles of the air, which being next the earth, are preſſed upon by all theſe numerous incumbent particles, that make up the atmoſphere, and are thereby ſo crouded, that they can but very little untwiſt themſelves; let us ſuppoſe, I ſay, the diameter of theſe particles to be $\frac{1}{10000000000000}$ of an inch; and then to be much of the form of thoſe repreſented in the 4th figure by ABCD: and that theſe particles, when a conſiderable quantity of the preſſure of the ambient air is taken away, will fly abroad into a coyle or zone ten times as big in diameter as before; that is, they will now be $\frac{1}{1000000000000}$ of an inch in diameter, and appear in the form of thoſe in the figure expreſſed by EFGH. Theſe zones, whirled round as the former, will deſcribe a ſphere 1000 times as big in bulk, and thereby fence that ſpace from being entered by any of the like zones. This it would do, ſuppoſing thoſe ſpheres did immediately always touch each other: but becauſe of their circular motion, whenever they meet, they muſt neceſſarily be beaten, and fly off from one another, and ſo require a yet greater ſpace to perform their motion in. This ſuppoſed, there are no phænomena of rarefaction (which is enough at preſent to anſwer what he objects) but may be naturally and intelligibly made out. As firſt, for that of the ſwelling of a carp's bladder, if we ſuppoſe ſome ſmall parcels of the former compreſſed *laminæ* to lie latitant within the folds of it, and being much coyled up together ſcarce to take any ſenſible room, this bladder in the air will appear to contain very little or nothing within it; whereas when the preſſure of the air is taken off in good part from the outſides of it, then thoſe formerly latitant particles diſcloſe themſelves by flying open into much bigger zones, ſo as perhaps to be able to defend a thouſand times bigger ſpace from being entered into by their like or any other groſs particles, ſuch as thoſe of the bladder. Now becauſe the pores of a bladder are ſuch as are not eaſily permeable by the particles of air, therefore theſe lurking particles ſo expanding themſelves muſt neceſſarily plump out the ſides of the bladder, and ſo keep them turgid, until the preſſure of the air, that at firſt coyled them, be re-admitted to do the ſame thing for them again.

NEXT, as for rarefaction by heat, that will as naturally follow as the former from this hypotheſis. For the atoms of fire flowing in, in great numbers, and paſſing through with a very rapid motion, muſt needs accelerate the motion of theſe particles; from which acceleration their ſpring, or endeavour outward, will be augmented; that is, thoſe zones will have a ſtrong nitency to fly wider open (for we know, that the ſwifter any body is moved circularly, the more do the parts of it endeavour to recede from the center of that motion) from whence, if it has room, will follow a rarefaction. As for the conveyance of light, that being, according to

Epicurus, performed by the local motion of peculiar atoms, their motions to and fro through this medium will be less impeded by the rarefied air than by the condensed; as indeed upon experiment we shall really find them.

As for his third objection, drawn from his supposed attractive virtue of the thus rarefied air, that is quickly answered, by denying it to have any power at all of attraction; and by shewing (which is already done) that what effects he would have to be performed by the attraction of the included, are really done by the pressure of the ambient air.

AND, lastly, the phænomena of my Lord *Bacon*'s experiment are sufficiently obvious, and easy to be deduced.

So then, by granting *Epicurus* his principles, that the atoms or particles of bodies have an innate motion; and granting our supposition of the determinate motion and figure of the aërial particles, all the phænomena of rarefaction and condensation, of light, sound, heat, *&c.* will naturally and necessarily follow: and the author's objections against this first way of rarefaction will signify very little.

As to the second way of rarefaction by the intrusion or intervention of some subtile matter or æther, into the spaces deserted by the rarefying particles, which is that proposed by the assertors of a *Plenum*, this also is by the author condemned, and branded with impossibility. And why? first, because 'tis (he says) impossible, that the above-mentioned phænomena of the carp's bladder can be explained by it. Secondly, because it is impossible to give a reason from it of the impetuous ascent of water admitted into an exhausted receiver. And, thirdly, because it is impossible to explicate the phænomena of gun-powder. His reasons to confirm which three impossibilities, because drawn from a mere mistake, or ignorance of those hypotheses which have been invented by the assertors of that opinion, I shall pass over, and content myself to explain a way how these impossibilities may become possibilities, if not probabilities.

AND the way, that I shall take, shall be that of the most acute modern philosopher Monsieur *Des Cartes*, published in his Philosophical Works: which is this, That the air is a body consisting of long, slender, flexible particles, agitated or whirled round by the rapid motion of the *globuli cælestes*, and the subtile matter of his first element, whereby they are each of them enabled to drive or force out of their vortice all such other agitated particles. Now the swifter these bodies are whirled round, the more do their flexible parts fly asunder and stretch themselves out, and the more forcibly do they resist the ingress of any other so agitated particles into their vortice; and consequently the slower their motion is, the less will be their resistance. And because there is a vast number of these whirled particles lying one above another, and each particle having its peculiar gravity; it will necessarily follow, that the undermost (which to maintain their vortice must resist so great a pressure) must very much be hindered from expanding themselves so far as otherwise they would, were there none of those incompassing agitated particles, that lay in their way: and that those being by any means removed, or they themselves by a more rapid motion of the particles of their vehicles, the first and second element (which is according to that hypothesis an effect of heat) more swiftly and strongly whirled round, they presently begin to expand themselves, and maintain a bigger vortice than before. Now to perform what I just now promised, I shall endeavour to give a possible, if not a probable, cause of the objected phænomena. And, first, for that of the carp's bladder, where the air is rarefied (says the author) 1000 times, it will easily be explained by supposing the few particles of the air, which (whilst they sustain the pressure of all the incumbent atmosphere) inconspicuously lurk within the bladder, (each

(each of them being able to maintain but a very ſmall vortice) to be by the ſubſiding mercury in the Torricellian experiment freed from the preſſure of the air, and their motion continuing the ſame (by reaſon that the tranſcurſion of their vehicles is not at all, or very little, hindered, either by the glaſs or bladder) their parts having room to expand themſelves, will fly abroad to ſuch extenſions, as may perhaps make a vortice 1000 times as big in bulk as what they were not able juſt before to exceed. Hence the particles of the air (being ſo groſs as not eaſily to pervade the pores of the bladder) muſt neceſſarily drive out the ſides of the bladder to its utmoſt extent, and ſerve to fill the receiver in the Magdeburgick experiment. Now, whereas theſe particles will by the ſame preſſure of the air be reduced to the ſame ſtate they were in at firſt, that is, to be thronged into a very little room, and thereby be able to maintain a very ſmall vortice; the air let in, in the Torricellian experiment, reduces the air in the bladder to its former inconſpicuouſneſs, as the admiſſion of the water in the *Magdeburg* experiment does that receiver full of rarefied air into the bigneſs of a hazel-nut. Now the water in this laſt-mentioned experiment enters with a great impetuoſity, becauſe driven on with the whole preſſure of the atmoſphere, and reſiſted only by the ſmall force of the ſo far rarefied air.

As for the author's objection againſt this way of rarefaction drawn from the phænomena of gun-powder, I ſhall endeavour to anſwer it by ſhewing them poſſibly explicable by a Carteſian hypotheſis. For ſuppoſing thoſe terreſtrial parts of the gun-powder to be firſt at reſt, and afterwards agitated by the rapid motion of its firſt element, there will be ſufficient difference of the former and latter condition in reſpect of extenſion; and ſuppoſing the particular conſtitution of gun-powder (ariſing partly from the ſpecific forms of the particles of its ingredients, nitre, ſulphur and charcoal, and partly from their proportionate commiſſion) to be ſuch as will readily yield to the motion of his *materia ſubtilis*, ſo ſoon as an ingreſs is admitted to it by the firing of any particular parcel of it, the expanſion will be ſpeedy enough.

So then let us ſuppoſe a barrel of gun-powder placed in ſome cloſe room, to ſome grains of which we will ſuppoſe ſome actual fire to be applied, by which actual fire (the texture of the powder being ſuch) thoſe grains are ſuddenly fired; that is, many millions of parts, which before lay ſtill and at reſt, are by the action of the burning-coals ſhattered, as it were, and put into a poſture ready to be agitated by the rapid motion of the *materia ſubtilis:* into which poſture they are no ſooner put, than agitated and whirled ſufficiently by it; whence follows a vaſt expanſion of that part of gun-powder ſo fired. For each of its parts being thus whirled and hurried round, expel and beat off with great violence all the contiguous particles, ſo as that each particle takes up now 1000 times as much elbow-room (if I may ſo ſpeak) as juſt before ſerved its turn; and conſequently thoſe that are outermoſt, take every one their way directly from the parcel or corn they had lain quiet in, being hurried away by the ſudden expanſion of the particles that lay next within them: ſo that whatever grain or parcel of gun-powder they chance to meet with, before they have loſt their motion, they preſently ſhiver, and put into ſuch a motion, as makes them fit to receive the action of the *materia ſubtilis.* Which ſubtile matter being every where preſent, and nothing ſlow in performing its office, immediately agitates thoſe alſo like the former; ſo that in a trice the particles of the whole barrel of gun-powder are thus diſordered, and by the motion the *materia ſubtilis* muſt needs be hurried away with ſo great an impetuoſity on all ſides, as not only to break in pieces its ſlight wooden priſon, and remove the lighter particles of the ambient air, but huge beams, nay, vaſt accumulated maſſes of the moſt compacted ſtructures of ſtone, and even ſhake the very earth itſelf, or whatever elſe ſtands in its way; whoſe texture is ſo cloſe, as not to give

give its particles free paſſage through its pores. This underſtood, I ſee not, firſt, what the author's three arguments, brought to prove his objection, ſignify; for there are no more corpuſcles in the room before the gun powder is fired than after, nor is there any more matter or ſubſtance, before the ſides of the room, by yielding, give place for the external fluid bodies to ſucceed, and the only change is this, that the *globuli ſecundi elementi* (as he calls them) are expelled out of the room, and the *materia primi elementi* ſucceeds in the place of it. Nor do I ſee, ſecondly, what great reaſon he had for his grand concluſion. *Hæc abundè demonſtrant, rarefactionem per hujuſmodi corpuſcula nullatenus poſſe explicari.*

HAVING thus examined the author's firſt arguments, that rarefaction cannot be made out by any other way than his; we ſhall find his other, which he brings to eſtabliſh his own hypotheſis, much of the ſame kind. As, firſt, that his way of rarefaction implies no contradiction: for if the affirming a body to be really and totally in this place, and at the ſame time to be really and wholly in another, that is, to be in this place, and not to be in this place, be not a contradiction, I know not what is. Next, that ſome learned ſchoolmen have thought ſo; to which I anſwer, more learned men have thought otherwiſe. And, laſtly, that there are very plain examples of the like nature to be found in other things; of which he only brings one, *viz.* that of the *Rota Ariſtotelica*, which upon examination we ſhall find to make as little to the purpoſe as any of the other.

An Explication of the Rota Ariſtotelica.

THE great problem of the *Rota Ariſtotelica*, by his explication of which he pretends, not only to ſolve all the difficulties concerning local motion, *quæ philoſophorum ingenia hactenus valde exercuerunt*, but to give an inſtance for the confirmation of his unintelligible hypotheſis of rarefaction, wherein there is *extenſio ſeu correſpondentia ejuſdem rei ad locum nunc majorem, nunc minorem*; We may upon examination find to be either a paralogiſm, or elſe nothing but what thoſe philoſophers ſaid, whom he accounts gravelled with it. Of this ſubject he begins in his 25th chapter, where, after he has ſet down a deſcription of it, he makes an inſtance in a cart-wheel; *Rem ante oculos ponit rota alicujus currus, ejuſque umbo ſeu lignum illud craſſum & rotundum, cui infiguntur radii; ſiquidem dum progrediente curru ipſa rota circumducta deſcribit in ſubjecta terra orbitam ſibi æqualem, umbo ille deſcribit in ſubjecto aëre orbitam* (I ſuppoſe both here and before he means *lineam*) *ſe multo longiorem, utpote æqualem orbitæ totius rotæ, licet ipſe non niſi ſemel quoque fuerit circumvolutus.* (As for what he ſays, that the nave muſt be ſuppoſed to paſs through the air, and not to touch the ſolid plain, I do not yet underſtand the force of his reaſon, nor why he ſets it down, making nothing to his preſent purpoſe, unleſs it were becauſe he did not well underſtand the thing.) In which, ſays he, the great difficulty is to explain how the nave ſhould be ſo turned about its axis, *ut partes ſuas ſucceſſivè applicet lineæ duplo plures partes habenti, idque motu perpetuo ac uniformi nè vel ad oculum inſtar interrupto.* Which how true, and what great occaſion he had to wonder at the ſolution of that problem by the example of a man ſtanding ſtill and another walking, we ſhall find by and by, when we come to explain the problem: but firſt I ſhall examine his hypotheſis and explication. And firſt, he ſuppoſes time to conſiſt of a determinate number of indiviſibles (that is, ſuch as have neither *prius* nor *poſterius* included in them) which he calls inſtants. And next he ſuppoſes the *præſentiam localem ſeu ubicationem cujuſlibet partis indiviſibilis & virtualiter extenſæ eſſe quoque indiviſibilem & virtualiter extenſam.* Which ſuppoſition, ſo ſtrangely expreſſed, is no more than this; that the extenſion or ſpace of his indiviſibles is alſo indiviſible.

5 But

But as for his virtual extenſion, I confeſs I underſtand as little what it is, as I verily believe he did: and therefore I will proceed to his following ſuppoſition. His third therefore is, that by how much more rare a body is, by ſo much the more are its indiviſibles virtually extended. Hence his fourth is, that though theſe indiviſibles be really indiviſible, yet they are virtually *in quotvis partes diviſibiles*. Whence he deduces his fifth principle, that ſince theſe indiviſibles are really indiviſible and virtually extended, they muſt neceſſarily be moved after the ſame manner that other indiviſible and virtually-extended things are. His inſtances are in the motions of an angel and an indiviſible piece of wood, which, he ſays, are both of the ſame kind. As for that of angels, having no immediate revelation, and a ſpirit and its actions not falling under ſenſe, and not having any third way by which to be informed. I ſhall leave him there to enjoy his fancies. But as for that of his piece of wood, we ſhall find it ſufficiently full of abſurdities and contradictions. And firſt, he calls it indiviſible, but why I know not, for it is neither really nor yet mentally ſo: not mentally ſo, by his fourth principle, where he ſays that it is *virtualiter in quotvis partes diviſibiles*, by which word *virtualiter* he means the ſame thing with *mentaliter*, or nothing. Nor, ſecondly, is it really ſo: for then (according to the main buſineſs of his book, as may be gathered from the firſt words of his title-page, *Tractatus de corporum inſeparabilitate*) it would be impoſſible, that any thing in the world ſhould be diviſible; for he making an ſeparable continuity, and that bodies will rather be (I cannot tell how) ſtretched beyond their own dimenſion *in infinitum*, than part from one another; a body may as ſoon paſs through the dimenſions of any one indiviſible, as paſs between two. Next, he grants in the ſtrange ſtretching or rarefaction of theſe indiviſibles a temporary motion of the condenſed dimenſion; whence there will follow, that there muſt be diſtinct places or *ubi's*, it muſt be *terminus à quo*, *terminus ad quem*, *& medium*. And next, it were impoſſible to divide a line into two parts, ſuppoſing it conſiſted of an unequal number of indiviſibles; as if 101 indiviſibles of exceedingly-rarefied air ſhould be extended in length an inch, it were impoſſible to divide that inch into two equal parts. I might run over many more, but it would be too tedious to be here recited. As for his indiviſible parts of time, thoſe alſo muſt neceſſarily be *in quotvis partes diviſibiles*; for elſe the ſame body or indiviſible muſt neceſſarily be in divers places at the ſame inſtant. But becauſe he can ſwallow, nay confidently affirm, this and many other ſuch like contradictions and abſurdities, I am not willing to mention them; and I think it would have made more for the author's reputation, if he had done ſo too. As for his laſt chapter, where he applies theſe principles to the explication of the *Rota Ariſtotelica*, I have not here time to ſet down all the abſurdities, that any one that has but a ſmattering in the mathematicks may obſerve: as, ſometimes half an indiviſible part of a circumference may touch an indiviſible of a line, ſometimes one may touch half, a quarter, a hundredth part, a whole one, two, ten, a hundred, *&c.* at the ſame inſtant; nay, an indiviſible of a circle may be all of it in a thouſand places together, and the like. And this he brings as a great argument to eſtabliſh his hypotheſis of rarefaction, pretending it to compriſe many ænigmas and very great difficulties; whereas the thing is very plain and eaſy, and contains no ſuch obſcurities. For if, for example, we ſuppoſe a wheel A B C D to be moved in a direct motion from A I C to K L M, every point of it retaining the ſame poſition to that line, that they had at the beginning of their motion, each of the points A E I G C will on a plain, or in the medium it pervades, paſs through or deſcribe a line equal to the line I L, and not only all the points lying in the line A I C, but all and every point of the whole area of the circle: this muſt neceſſarily happen, if the diameter A I C be moved parallel to itſelf: but if whilſt it be thus moved

moved with an equal progreſſion, it be likewiſe moved with an equal circulation, the caſe will be altered. For then, firſt, each point will by this compound motion deſcribe on the plain or medium either a perfect cycloid, as when the wheel makes one perfect revolution, whilſt the whole is progreſſively moved from I to L; or ſome piece, as when the wheel has not perfected its revolution; or more than a whole one, as when the circle has made more than one whole revolution, whilſt it is moved in its determinate length. I ſhall here only conſider the firſt, as pertaining more eſpecially to my preſent purpoſe, and in regard the two latter on occaſion may be eaſily explicated by it. Next, each point of this circle acquires from its compounded motion various degrees of celerity as to its progreſſion, according to its various poſition to a point, which is always found in ſome part of the line drawn through the center of the circular motion perpendicular to the progreſſive. And it is found thus, As the circumference to the radius, ſo is the line of the progreſſive motion to the diſtance of the point from the centre. And this happens, becauſe the line of progreſſion is equal to the circle deſcribed on that diſtance as radius; each point therefore of this ſmaller circle, when it comes to touch the perpendicular, muſt, as to its progreſſive motion, ſtand ſtill: this point therefore will be the centre of this compounded motion. Now becauſe for the explication of the *Rota Ariſtotelica* we need not conſider any other than thoſe points, which are tranſient through or croſs the perpendicular line, we ſhall only examine them. Let then in our example A be the centre or immoveable point; the circumference therefore A B C D will be equal to I L or A K by our hypotheſis. Now becauſe the point I, which is the centre of the rotation, has only one motion, *viz.* that of lation, its celerity will be equal to the ſingle celerity of the lation; we will therefore put it to have one degree C, becauſe it is moved with two motions, both tending the ſame way, and each equal to the velocity of I, muſt needs have two degrees of velocity. The point F, becauſe moved with two motions, both tending the ſame way, the one (*viz.* its lation) being equal to that of I, and the other (becauſe it is but half as far diſtant from the centre of rotation as C, and therefore is moved but with half the celerity of C, which was equal to that of I) but half as quick, we will put to have one degree and an half. By the like method we might find the velocity of all the points in the perpendicular, *viz.* ſuch as we have there marked ſome of them; but it would be too tedious, we needing not to conſider more than the two points A and E. The point at E being moved forward by its progreſſion with the ſame velocity that I, but by its rotation (which is but half as ſwift as that of the circle ABCD that is double the circle E F G H) being moved the contrary way or backwards with half the velocity, loſeth half of its progreſſion forwards. The point in A being by its progreſſion moved forwards equally ſwift with I, and by its rotation (the circle A B C D being equal to the line I L) being carried backwards with equal velocity, muſt neceſſarily ſtand ſtill as to its progreſſion. Now having ſhewn, that the point A (being by reaſon of its two equal oppoſite motions at reſt) does only touch a point of the line A K, and is not at all moved on it; and that the point E (being carried forward twice as faſt by its progreſſion as it is carried backward by its rotation, and thereby moved half as faſt as the point I) does not only touch the line E K, but whilſt it touches it, is moved on it with a progreſſive motion half as ſwift as that of I: it will neceſſarily follow, that each point ſituate in E muſt neceſſarily deſcribe a ſmall line, which is a part of the whole E C. Now both the contact of the former, and the contact and progreſſion of the latter, being performed by an infinite ſucceſſion of points in the ſpace of an infinite ſucceſſion of inſtants; I ſee not any one difficulty of this problem, but may ſatisfactorily be given an account of by it, without having recourſe to the hypotheſis of the determinate

determinate number of indivisibles of space and time, which at best will only come to this, that *in such a determinate moment or minute space of time* (which consists of an infinite consecution of instants, and has *prius* and *posterius* in it; though yet he will call it an instant, and have it to have the same proprieties with an instant used in the common philosophical sense) *such a determinate minute corpuscle* (which though it have extension in length, breadth and thickness, yet will he not admit it to be divisible or have parts, no not though, according to his hypothesis, the indivisible of one body may be rarefied to be as big in bulk as a million of the indivisibles of another, but will have it to be called and to be a real indivisible) *will successively pass over such a determinate space or length* (which yet he will not admit to be divisible, though according to his principles it may equalize the length of millions of his other indivisibles, nor admit a successive motion, but instantaneous, though that does necessarily put a a body into two, three, ten, a hundred, *&c.* places at once; but will have these also to be indivisible.) Haste makes me pass over the absurdities about the contact of a circle and a line, and to comprise in short all that great explication he has given of this and other intricate (as he calls them) problems; which is this, that the reason of the celerity of the motion of some one of these indivisibles above another is, that it passes through a greater part of an indivisible in the same instant than the slower; that is, in plain sense, no more than this, One body is swifter than another because it is moved faster. From whence he draws several corollaries, as that, Hence may be given a reason, why an eagle is swifter than a tortoise, *viz.* because it moves faster. I should have solved several objections, which may be brought against the divisibility of quantity *in infinitum*; but that as all the scholastic writers are full of them, so it is a subject, which we are least able to dispute of, having very little information of the nature of infinity from the senses.

AN

EXAMEN

OF

Mr. *T. HOBBES* his

Dialogus Physicus de Natura Aëris;

As far as it concerns Mr. BOYLE's Book of New Experiments touching the SPRING of the AIR, *&c.*

With an APPENDIX touching Mr. HOBBES's Doctrine of Fluidity and Firmness.

The PREFACE.

HAVING soon after I had begun the following examen of Mr. *Hobbes*'s dialogue, been diverted for a good while by divers urgent avocations from pursuing it; I was in the mean time informed by learned men (some of whom keep great correspondences with the virtuosi abroad) that my publishing any thing against his objections would not be necessary, nor was much expected. Whereupon I should perhaps have declined resuming an employment, that to a person of my humour could not be delightful; but that besides those inducements mentioned at the beginning of the following treatise, it came into my mind, that my adversary, not content to fall upon the explications of my experiments, has (by an attempt, for aught I know, unexampled) endeavoured to disparage unobvious experiments themselves, and to discourage others from making them. Which if he could by his dialogue effect, I dare be bold to say, he would far more prejudice philosophy by this one tract, than he (and that it may not seem said to undervalue him, I shall add, or any man else) can promote it by all his other writings. Wherefore, though his disparaging of experiments would probably have much more authority (especially with considering men) if he had been the author of considerable ones, or did appear to be more than ordinarily skilled in them: yet lest, for all this, his fame and confident way of writing might prejudice experimental philosophy in the minds of those, who are yet strangers to it, I thought it might not be amiss, both to go on with the discourse I had begun, and to enlarge it beyond what I first designed; and accordingly, to the intended vindication of the main points of our doctrine, *The weight and spring of the air*,

air, which (if I mistake not) we have firmly established, we have added an Examen, that otherwise we should scarce have made, of the greatest part of the physiological passages in Mr. *Hobbes*'s book, most of which I thought might be rationally questioned, and many of them clearly disproved. And in pursuance of this, though I did not perhaps always think myself obliged to prosecute things further than the nature of my design required, or to forget, that the matters in dispute were not all of an equal weight; yet the reflections I have employed will, I presume, be found sufficient to shew, both that it is easy even for a great wit frequently enough to mistake, and much more frequently to miss of clearly demonstrating what he pretends in matters physical, for want of having sufficiently considered the experiments he would be thought to despise; and that Mr. *Hobbes*'s adversaries need not be much ashamed of the name he is pleased to give them, of experimentarian philosophers. It was also suggested to me, that the dangerous opinions about some important, if not fundamental, articles of religion, I had met with in his *Leviathan*, and some other of his writings, having made but too great impressions upon divers persons (who, though said to be for the most part either of greater quality, or of greater wit than learning, do yet divers of them deserve better principles) these errors being chiefly recommended by the opinion they had of Mr. *Hobbes*'s demonstrative way of philosophy; it might possibly prove some service to higher truths than those in controversy between him and me, to shew, that in the Physics themselves, his opinions, and even his ratiocinations, have no such great advantage over those, of some orthodox Christian Naturalists. But for all this, as little as I could grudge to write a much longer than the following discourse to do religion the least service; yet thinking it fit to leave controversies of this kind to those, whom they more particularly concern, I should scarce, in the introduction to a dispute about the air, have at all mentioned any thing of this nature, but that Mr. *Hobbes*, in the preface to his dialogue, is pleased (though I know not to what purpose in that place) to speak without limitation or distinction (and consequently unwarily enough at least) of the things said in the books of naturalists concerning immaterial substances (and sure some things true, at least that there are such beings, as well as some things erroneous, are there said) though he hath been, by the learned Dr. *More*, and others, publickly accused to have taught, that it is absurd to believe, that there either are or can be any. Which yet, methinks, he could not do, since elsewhere, and in this very dialogue, he builds several things in his * philosophy upon the creation of the world, and an infinite power: and how a thing material can create matter, and have an infinite power, I confess I do not understand.

I DOUBT not but critical readers will think I might have excepted against many more particulars in Mr. *Hobbes*'s book than I have examined; and indeed, about this, I dare not contend with them. For besides that I may, through haste and indisposedness to quarrel, have overseen several things, which an eye either severer or more attentive would have observed; I purposely past by divers things I did not altogether overlook; partly, because I thought it needless to question them (having no want of other objections) and partly, because I could not do so in few words, and was loth to engage in needless and long-winded disputes: and, perhaps, I was too weary of my employment, to be willing to spend many words, when I could safely spare them. And though others will possibly think it strange, that a member of the

*— *Neque hominis philosophi esse censeo corporum quorundam, ut solis & stellarum, mirabiles supponere magnitudines, contra vero mirabiles exiguitates non admittere, cum virtutis ejusdem infinitæ sit utraque creare tam maxima quam minima.* Mr. *Hobbes* in his Dialogue of the Air, page 11. See also page 5. and elsewhere.

ſociety he is ſo ſevere to, ſhould not take notice of ſuch paſſages as theſe; *Nam conveniant* (ſays he, ſpeaking of the virtuoſi, that meet at *Greſham* college) *ſtudia conferant, experimenta faciant quantum volunt, niſi & principiis utantur meis, nihil proficient.* And again, *Næ illi quæ dicerent non videntur cogitaſſe, ſed ſortitos eſſe.* And elſewhere, *Conjicere hinc licet, quam ſint boni ratiocinatores, & quæ ſit ab illis expectanda philoſophia naturalis.* And (to trouble you no more) *Ad cauſas autem, propter quas proficere ne paululum quidem potuiſtis nec poteritis, accedunt etiam alia, ut odium Hobbii, quia nimium liberè ſcripſerat de academiis veritatem: nam ex eo tempore irati phyſici & mathematici veritatem ab eo venientem non recepturos ſe palam profeſſi ſunt*; though, as I ſaid, ſome may wonder I ſhould ſilently pretermit ſuch paſſages as theſe; yet beſides what I elſewhere ſay by way of account of my ſo doing, I ſhall here tell them, that I preſume ſome ſorts of readers will more eaſily pardon me for neglecting ſuch expreſſions, than they will Mr. *Hobbes* for uſing them. And I confeſs, I thought I ſhould find it more eaſy to ſay nothing at all to ſuch paſſages, than ſay any thing without ſaying ſomewhat, that would offend a perſon, that could allow himſelf to ſay ſuch things. Though I ignore not, that divers readers will much the leſs reliſh the following diſcourſe, for my having, perchance not altogether for want of knowing how to write otherwiſe, forborn to furniſh it with quick and ſmart expreſſions, which are wont to be employed in diſputes, to expoſe or depreciate an adverſary's perſon or cauſe, and which are uſually not the leaſt things, that ſerve to amuſe ſuch readers, and engage their attention. But I fear I have much leſs need to make excuſes for my omiſſions, than for having, in the following examen, been reduced by the nature of my taſk, to ſay ſo many things which intelligent readers need not be taught by me. And therefore ſuch ſhall have my conſent to ſkip, if they pleaſe, the whole diſcourſe: which though I could ſcarce upon ſuch an occaſion make a very inſtructive one; yet if they will be pleaſed to forgive me its barrenneſs, I hope hereafter to avoid the like temptations of writing again at the like rate.

And having ſaid thus much as to the reaſons of my penning the following diſcourſe, I muſt add ſomething, though but little, touching the manner of it; wherein I hope I have not much, if at all, ſwerved from what I propoſed to myſelf; namely, to give an example of diſputing in print againſt a provoking, though unprovoked, adverſary, without bitterneſs and incivility, and without purſuing thoſe things, which how much more ſoever they belong to the perſon of an antagoniſt than to his cauſe, are wont to make up a great part (if not the greateſt) of books divulged on ſuch occaſions. But ſince I intend what I write for intelligent and ingenious readers, I dare expect, that my forbearing to inſiſt on ſuch things, as I judged wholly extrinſical to the opinions and arguments I examine, will be aſcribed to the true cauſe, that my diſcourſe will not be thought to have the leſs of reaſon for having the leſs of paſſion; and (eſpecially) that my ſilence as to thoſe things, that are ſpoken to the diſparagement of the illuſtrious company, that meets at *Greſham* College, will be looked upon only as an effect of my judging it fit to leave them the full liberty to right themſelves, if they think it worth while, by ſome better pen than mine. And if Mr. *Hobbes* thinks fit to ſay any thing to the following diſcourſe, it will not be amiſs, that his reply be as inoffenſive as I have endeavoured to make my examen. For having diſpatched as much as I think requiſite to ſay of this controverſy myſelf, and having other (and I hope better) employments for my leiſure hours, if I can get any; I muſt leave the further diſputes, if any ſhall ariſe, to be managed by others, who, if Mr. *Hobbes* refuſe to imitate my way of writing, will poſſibly make no ſcruple to imitate his, and put him in mind of that law of *Veſpaſian*, upon which himſelf would be thought to ground

ground that heap of ſtrange titles he beſtows upon the two learned *Savilian* Profeſſors *, *That it is unlawful to give ill language firſt, but civil and lawful to return it.* I have but one thing more to add; which is, that I would not be ſo far miſtaken, as to be, upon the account of what I have written againſt my two adverſaries, looked upon as a perſon wedded to his opinions: for not having hitherto learned, that either of their books has yet made proſelytes; I preſume it will not be wondered at, that they have not made me one. And though the two learned authors I have anſwered, have given me no cauſe to retract any of my opinions; yet as it is not improbable, that others reaſoning upon better principles may do what theſe have not done, ſo I am ſtill of the ſame temper I was of, when I uſed to propoſe my thoughts but as conjectures.

AN

EXAMEN

Of the greateſt part of Mr. *HOBBES*'s

Dialogus Phyſicus de Natura Aëris.

CHAP. I.

The occaſion and ſcope of the preſent Treatiſe.

MEETING the other day with a treatiſe then newly publiſhed by Mr. *Hobbes*, and intitled *Dialogus phyſicus de naturâ aëris*: the name of the author, the ſubject of the book, and the information I had a good while before received from his friends, that he was writing againſt me, invited me to peruſe it, as a diſcourſe, wherein I might probably find myſelf concerned: nor was I deceived in my expectation. For having curſorily paſſed through it, I readily found, that though I be not expreſsly named there, and though ſome things in the title-page, and ſome others in the book itſelf, ſeem to make the chief deſign of it to be the diſparagement of the ſociety, that is wont to meet at *Greſham* College; yet the arguments are for the moſt part levelled at ſome writings of mine, publiſhed, ſome of them, the year before, and ſome of them this laſt ſpring; as the Experiments, whoſe explications he is pleaſed to cenſure, do all along declare. I confeſs I was ſomewhat ſurprized to find, that Mr. *Hobbes*, whom if my books have at all mentioned, it has been with reſpect, ſhould fall upon a perſon, that had not provoked him; whilſt ſuch mathematicians as Dr. *Wallis*, Dr. *Ward*, *Tacquet* and *Moranus* (men much too famous to be deſpicable adverſaries) having a good while ſince profeſſedly and unchallenged written againſt

* So go your ways (ſpeaking to Dr. *J. Wallis* and Dr. *S. Ward*) you uncivil eccleſiaſtics, inhumane divines, dedoctors of morality, unanimous collegues, egregious pair of *Iſſachars*, moſt wretched *Vindices* and *Indices Academiarum*; and remember *Veſpaſian*'s Law, &c. Mr. *Hobbes*'s Leſſon 6. p. 64.

him,

him, he hath yet the whole discourses of some, and so great a part of the objections of the others, to reply to. And it somewhat added to my wonder, that a writer of politics should causelessly and needlessly, for aught I can learn, fall upon a society, whereof, besides many other persons of quality and men of parts, his own great patron, and my highly honoured and learned friend, the Earl of *Devonshire* himself, is an illustrious member. And as for me, I shall not scruple to confess, that I could have been well enough contented Mr. *Hobbes* had spared this dialogue, partly because I have a natural indisposedness to contention, partly because I am at present distracted by store of other employments both of a public and a private nature (and particularly by the publishing of three or four books of differing subjects, and printed in several places;) partly because Mr. *Hobbes*'s objections are of such a nature, that perhaps my replies, though as short as (my design mentioned in the preface considered) I can conveniently make them, will amount to a longer discourse, than most readers will think the objections needed; and partly too, because Mr. *Hobbes* is pleased to write of divers worthy and learned men in so depreciating, and of himself in so differing a way, that I fear I shall find it somewhat uneasy to retain (under such provocations to decline it) the civility I am wont, and am desirous to write with; and that I must almost despair of dissenting without an absolute rupture from a person, whose way of discoursing is such, that though I shall not give it any epithet, yet I confess it leaves me but little hope, that I can oppose him without angering him.

But however, because if I can (as I intend to do) so far comply with my inclinations and my custom, as to wave personal and intrinsic matters, and restrain myself to the examen of the argumentative part of his discourse, my reply will not need to be prolix; and because he has vouchsafed rather to single out a young writer, whose books (at least of matters philosophical) do but begin to appear in the world, than to defend himself against those illustrious enemies, upon whom he might expect to gain much more honour; and because Mr. *Hobbes*'s name may with some readers give his arguments an efficacy, which their own nature could not confer on them; I must resolve to submit to what he and my concern for the truths he rejects impose upon me. But to shorten as much as I can a work, to which I can allow but very little time, it will be expedient, before I descend to the examination of particulars, to premise three or four advertisements touching the occasion and the nature of the controversy, that I might not be reduced to a frequent and unwelcome inculcation of the same things.

CHAP. II.

Of some mistakes of Hobbes *touching matters of fact, and the author's doctrine.*

AND first, whereas Mr. *Hobbes* is pleased to write, as if the explications and experiments to be met with in the physico-mechanical treatise he censures, were those of the whole society at *Gresham* College; I must do them that right to declare, that this way of proceeding is manifestly grounded upon a mistake, I will not affirm, that the mistake was wilful, that Mr. *Hobbes* might give himself a pretence to quarrel with them (who have hitherto suspended the declaring themselves as a Society) in the controverted points. But there are some, that think Mr. *Hobbes* might very easily have avoided this mistake, since the book he censures was published (and perhaps taken notice of by most of the virtuosi here) some months before the society was begun. And the experiments themselves had been, long before the book came forth,

forth, not only seen and discoursed of by divers learned men and illustrious persons, but had the honour to have our great monarch of the virtuosi, as well as of *Great Britain*, for a spectator. And though possibly divers of the learned members of our assembly may have no unfavourable opinion of what I have delivered in that book; yet the assembly, as such, has been so far from adopting or owning my opinions as theirs, that it has with approbation been proposed among them, to repeat the experiments, and take a review of the explications, that upon a strict examen of the several opinions, and the objections that could be brought in against them, they might see, what judgment will be fit to be passed on them. And although there be very few philosophers, whose parts may make their judgment more formidable to me; yet to comply with their design, whatsoever the event might be, I presented them the engine itself, I had made use of and described in my book; chusing rather to undergo their censures, than want their instructions. By which it may appear, upon how little ground Mr. *Hobbes* has thought fit to impute to the society those opinions, which (how erroneous soever he is pleased to think them) I must own to be mine. And this justice I the rather do it, because it is all, that I am to do in this treatise on their behalf, not only for the reasons above intimated, but because the vindication of such an assembly against Mr. *Hobbes* deserves a better pen than mine, though it doth not need it.

Secondly, Undertaking then the defence of my own cause, without interesting them in my quarrel, I must next admonish the reader, that whereas Mr. *Hobbes* writes, as if the new experiments were devised, or at least employed, to prove a vacuum; he is in this likewise mistaken. For neither has the society declared either for or against a vacuum, nor have I: nay, I have not only forborn to profess myself a Vacuist, or a Plenist, but I have in a fit place of my epistle expressly said, that I reserved the declaring of my own opinion touching that point to another discourse (which as yet is not published). Wherefore Mr. *Hobbes* either injures or mistakes those, whom he will needs make his adversaries, when he represents the new experiments as demonstrations alleged by professed Vacuists to disprove the fulness of the world. And though I shall be obliged in the following discourse to reject Mr. *Hobbes*'s supposition of a plenum; yet I intend not thereby to declare, whether or no I do absolutely allow a vacuum. But that, which I drive at, and which alone my present work exacts, is to shew, that I may reasonably oppose the hypothesis of a plenum, as it is stated by Mr. *Hobbes:* and consequently, unless he had better proved it, I may very well refuse to let him take it for demonstrated. But I intend not to question, whether or no, other Plenists may not have better arguments than his principles have suggested to him; nor to deny but that the Cartesians may, without granting a vacuum, give a more plausible account (whether true or no) of divers of the phænomena of our engine, if they will add, as some of them of late have done, the spring of the air to their hypothesis, that the celestial matter, of which the air does in great part consist, is suitable enough freely to pass through the pores of the closest bodies, and even glass itself.

As for the assertion *non dari vacuum*, though, as I said, I need not in this place declare myself either for or against it, yet I confess I do not find, that Mr. *Hobbes*, though all along this discourse he argues from this principle against those he thinks Vacuists, has demonstrated it. For in his book *De corpore* (though a main part of it depends upon the plenitude of the world) he has, that I remember, but one positive argument (indeed he thinks that unanswerable) to evince it. And that is drawn from this experiment: 'That if a gardener's watering-pot be filled with water, the hole 'at the top being stopped, the water will not flow out at any of the holes in the 'bottom;

Cap. 26. sect. 2.

'bottom; but if the finger be removed to let in the air above, it will run out at 'them all; and, as soon as the finger is applied to it again, the water will suddenly 'and totally be stayed again from running out. The cause whereof (subjoins he) 'seems to be no other but this, that the water cannot by its natural endeavour to 'descend, drive down the air below it, because there is no place for it to go into; 'unless either by thrusting away the next contiguous air it proceed by continual en-'deavour to the hole at the top, where it may enter and succeed in the place of the 'water that floweth out; or else by resisting the endeavour of the water downwards, 'penetrate the same, and pass up through it.'

BUT this experiment, as an obvious one, and without dreaming that Mr. *Hobbes* had laid such stress upon it, I have incidentally answered in what I say in two or three passages on the thirty-third experiment of my epistle; but after found, that it had been more fully answered (but upon grounds, some of which I do not need) by my learned friend Dr. *Ward*, with whom I thus take Mr. *Hobbes*'s argument to pieces. The cause, according to Mr. *Hobbes*, of the suspension of the water in the vessel is, that the water cannot thrust away the air. 2. And it cannot thrust that away, unless air succeed in its place. 3. But air cannot succeed in its place, unless either by getting in at the upper orifice, or at the holes that perforate the bottom. By which view of the argument it appears, that the main force of it lies in the second proposition: but neither doth he demonstrate that (which omission might excuse us from any further answer) nor indeed do I think it true. For if the watering-pot were tall enough, what reason is there why the water should not run out at the holes of it? as Monsieur *Pascball*'s experiment mentioned in my epistle manifests; that though in a glass-tube hermetically sealed at one end, and several times as long as a watering-pot, the water will not fall down, yet it will, if the tube exceed two or three and thirty foot, or thereabouts. And indeed the suspension or descent of the water depends upon the proportion betwixt the weight of the aqueous pillar, that tends downwards, and the resistance or pressure of the air, that can come to bear against it. For as, on the one side, when the height of that pillar is so increased, as that it can outweigh the atmospherical cylinder, that opposes its descent, it will flow out till those two cylinders come to an æquilibrium; so, on the other side, if instead of increasing the length of the cylinder of the water, you lessen the pressure and resistance of the air, the water will likewise descend, though the pillar be very short, as I have shown in the nineteenth experiment; where, by withdrawing some of the air in the receiver, and thereby weakening its spring, the water, in a tube hermetically sealed at one end, of but about four foot long, subsided about three foot; though that the space relinquished by the water was not full of air, as Mr. *Hobbes*'s argumentation requires it should be, may be proved by what is there added, 'that by letting in the outward 'air, when the water was sunk so low, it was immediately impelled up again to the 'higher parts of the tube.'

In some Dialogues of heat and flame.

AND indeed (as I elsewhere discourse) it seems to me a difficult matter for those, that reject, as Mr. *Hobbes* justly does, that conceit of nature's abhorring a vacuum, and making it, as it were, her business to hinder it, to prove there can be no vacuum at all by any particular experiment. For if the fulness of the world be not made necessary, either by the nature of body in general, or by the design of the author of the universe, it can scarce be easy to prove by a particular experiment, that no human force or art can contrive a way of overcoming, at least for some time, and as to some space, either the gravity of fluid bodies, or whatever other quality of the air or water it is, by which the contiguity of the neighbouring parts of the world is wont to be maintained. As we see the water, that will not descend even in a tube of thirty foot,

foot (and thereby has made men think it will never descend whilst the air is not permitted to succeed it) may, by our engine, be brought to subside in a tube of about a foot long. And I shall here add this out of my (yet unpublished) Dialogues of flame and heat; that whilst only particular experiments are brought to assert the impossibility of a vacuum, perhaps the Vacuists will have the advantage on their side. For a thousand experiments are not of that force, to prove universally, that a thing cannot be effected, as one, that shews it may be, is to prove the contrary. And the Vacuists have as well as the Plenists store of experiments on their side, that seem to favour their hypothesis; according to which, were it true, I see not why they may not solve the objections drawn from either the ascension of liquors upon suction, or the non-descension of liquors in watering-pots closed at the top, or from any of the like experiments I have yet met with, in case the weight and spring of the air be taken in to solve the phænomena. And the Vacuists will have this advantage, that if Mr. *Hobbes* shall say, that it is as lawful for him to assume a plenum, as for others to assume a vacuum; not only it may be answered, it is also as lawful for them to assume the contrary; and he but barely assuming, not proving a plenum, his doctrine will still remain questionable. But I think I could say more in favour of the Vacuists experiments; namely, that whereas in some phænomena of the Torricellian experiment, and in many of those of our engine, Mr. *Hobbes* proves the space deserted by the quicksilver or the air to have no vacuity, because according to his supposition the world is full; (and not by any sensible phænomena, that prove the space in question to be perfectly full:) for no less fulness is requisite to the truth of his hypothesis: the Vacuists on the other side need not go about to prove, that those spaces are not full by their hypothesis: but they prove it by this, that it appears by sensible phænomena, that the quicksilver deserts the upper part of the tube; and that much air is pumped out of our receiver. (The first of which is evident to the eye; and so is the other too, when the pump is kept under water.) But it does not appear by the like phænomena, that the air (as Mr. *Hobbes* would have it in his elements) does succeed to fill, I say, perfectly to fill the deserted space; which also they will confirm hence, that in the Torricellian experiment, by inclining the tube, the relinquished space may be again readily filled with mercury; and if our exhausted receiver be plunged under water, that liquor, when access is given it to the cavity, violently rushes into it, and almost fills it up.

From all which it seems probably deducible, that it is a very hard thing, by Mr. *Hobbes*'s way of managing the controversy, to prove, that there can be no vacuum. But as for the Cartesians more subtile and plausible way of asserting a plenum, it concerns me not here to dispute against it, or declare for it.

I will add this, and but this, on the occasion of Mr. *Hobbes*'s building a great part of his philosophy upon no surer a ground, that we may hence learn, how little reason there is to blame me, as he is pleased to do, for making elaborate experiments; and that though (as I have elsewhere purposely and amply discoursed) obvious experiments are by no means to be despised, yet it is not safe in all cases to content one's self with such; especially when there is reason to suspect, that the phænomenon they exhibit may proceed from more causes than one, and to expect, that a more artificial trial may determine, which of them is the true.

Thirdly, whereas Mr. *Hobbes* is pleased to find much fault with the society, and me, for not assigning the cause of springs in general; that omission seeming to him very unworthy of philosophers: I answer, that the society having hitherto, for weighty reasons, forborn to determine the particular causes of things, there was no

 reason

reason they should alter their method, for experiments, that were not made or published by them or by their order. And as for me, the title of my book promises some experiments touching the spring of the air and its effects, not speculations of the causes of springs in general. My avowed intention was candidly to communicate with the curious some experiments which, I thought, their novelty would render acceptable to them, wherein I have the good luck not to have been mistaken; nor can I be justly censured for not performing what I did not undertake, nor was obliged to. And perhaps Mr. *Hobbes* would more prejudice the commonwealth of learning by his severity, than he has yet advantaged it by any other way, if he could obtain, that none should publish an experiment or observation that cannot, by deduction from the first and catholic principles of philosophy, assign the true cause of it. But when I take upon me to write, as Mr. *Hobbes* has done, elements of philosophy, then perhaps I shall be able to give an account of springs, not much more unsatisfactory than others think his. For though he refers us to his explication given of the motion of restitution in his book *De corpore*; yet in the 22d chapter, and 30th section, which professedly contains his theory of it, after having premised (what rightly interpreted may be true enough) that the cause of the restitution proceeds not from the taking away the force, by which they were compressed or extended ('the 'removing of impediments not having the efficacy of a cause') that which follows to the end of the section is only this: 'The cause therefore of their restitution is some 'motion either of the parts of the ambient, or of the parts of the body compressed 'or extended. But the parts of the ambient have no endeavour, which contributes 'to their compression or extension, nor to the setting them at liberty or restitution. 'It remains therefore, that from the time of their compression or extension, there be 'left some endeavour (or motion) by which the impediment being removed, every 'part resumes its former place; that is to say, the whole restores itself.' Now this notwithstanding, I am so dull or so wary, that though I had met with this passage, and all the praises the author in his Dialogues gives it, yet I should have made some scruple to undertake the assigning the true cause of springs in general. For first, the learned *Gassendus*, and the Epicureans both ancient and modern, together with divers other Naturalists, do not admit what Mr. *Hobbes* supposes a few lines before, that 'that which is at rest cannot be moved, but by a moved and contiguous mo-'vent.' For they think motion, or at least *conatus ad motum*, an unloosable property, congenit to matter. And, by the way, whatever exceptions I have to this opinion, yet I am not satisfied with that principle of Mr. *Hobbes*, though it be the fundamental one of his philosophy, unless it be more warily proposed. For to assert universally and without exception, as he does in his Elements, that nothing can be moved but by a body contiguous and moved; I do not take to be true, nor consistent with his other assumptions. This I elsewhere (in a discourse against another than Mr. *Hobbes*, about the Christian religion) prove more at large: but now it will suffice to represent, that Mr. *Hobbes* not only admitting, but making use in his philosophy of the creation of the world, either he must allow, that motion is natural to some, if not all parts of matter; or that God put them into a motion not included in their nature. From which it will necessarily follow, that at least some bodies may have motion, though it be not given them by any body contiguous and moved, as an attentive considerer may easily discern. But to return to the cause of springs.

SECONDLY, whereas Mr. *Hobbes* assumes, that the parts of the ambient have no endeavour, which contributes to their compression or extension, nor to the setting of them at liberty or restitution; he says this indeed, but does not go about to prove it.

it. And I should the less have made this precarious assertion, because that after the celebrated *Des Cartes* himself, the Cartesian philosophers generally ascribed the motion of restitution to the passage of a subtile æthereal substance (and an æther Mr. *Hobbes* also admits) through the pores of the springy body, which striving to obtain its wonted freedom of passage, restores them to the shape and bigness, from which they were forced. Nay, I shall have occasion to shew anon, that Mr. *Hobbes* himself, whatever he says in this place, does elsewhere ascribe a motion of their own to multitudes of terrestrial corpuscles. And I might add, that elsewhere he speaks of the rekindling of the fire taken out of the receiver after this manner: *Quanquam vis illius motus in recipiente (ut loquimini) evacuato diminuta sit, oppressa ab aëris intus commoti consistentia, non tamen extinguitur; & propterea levata oppressione, satis habebit virium ad excitandum phantasiam lucis quanquam debiliorem:* 'Although the force of that motion in the evacuated receiver be diminished, being oppressed by the consistence of the air moved within, yet it is not extinguished: and therefore that oppression being taken off, will have strength enough to excite an appearance of light, though somewhat weaker than ordinary.' But I shall rather subjoin, that yet, thirdly, I do not think it improbable what the learned *Gassendus* had taught, and what Mr. *Hobbes* here teaches, that the restitution of bent springs may proceed from a certain endeavour or motion in their internal parts (left from the time of their compression or extension) which when the impediment is removed, makes every part resume its former place, and thereby makes the whole restore itself. But this notwithstanding, I feared others might be as inquisitive as myself, and might expect from him, that would undertake to settle a general theory of the motion of restitution, the clear and distinct explication of several phænomena, that I had met with, which are not touched, nor perhaps were, some of them, thought upon by Mr. *Hobbes*. As first, why such a determinate temper of iron and steel is requisite to make it elastical; so that if after having been hardened and gradually heated, it be suddenly cooled, at an inconvenient point of time, it will be brittle, and fit to make gravers and other rigid tools, not springs? Next, why bows and other elastical bodies, if they be kept too long bent, lose in process of time almost all their elastical power, and continue crooked? Thirdly, why, not only divers solid bodies as well as lead and gold, which before trial one would think as likely, as many springy ones, to have their parts put into a due motion by the force that bends them, should be devoid of an elastical power? Fourthly, what kind of motion, and what kind of texture it is, by virtue whereof, the parts of a body being for a very short time put into motion, do some months, perhaps some years, retain in great part a smart motion, without in so long a time communicating it to the ambient bodies, to some or other of which multitudes of them are perpetually contiguous, and thereby losing it themselves? Why upon such a bare and inartificial change made in the texture of a body, as is scarce at all discernable to the eye, it should acquire a strong spring, that it had not before (as I have tried upon silver and copper, which though flexible before they were hammered, yet being beaten into thin plates obtained a notable spring:) and why (which may seem more strange, upon another light change of texture) the acquired spring may presently be lost again; as I have tried in silver, that, chymists teach us, loses nothing in the fire, which having by being hammered acquired a strong spring, we have presently made flexible again as before, by only heating it red-hot, without so much as melting it; which argues, that in springs texture is as well to be considered as motion? To these I might add other particulars, that I had either made or observed (and mention in another treatise) concerning springs; all which phænomena perhaps

Page 18.

every one, that has read what we have lately recited out of Mr. *Hobbes*, will not presently be able satisfactorily to explicate. So that I hope the equitable reader will not think it a fault, that (contenting myself to propose the two explications of springs, I saw most liked among the curious; to which I should have added Mr. *Hobbes*'s, if I had found it as much esteemed) I declined engaging myself in controversies about the origin of motion, and such other high speculations, as, had my abilities enabled me, neither my design exacted, nor my leisure permitted that I should prosecute. And though Mr. *Hobbes* be pleased to speak thus of his notion concerning the restitution of bodies; *Sine qua hypothesi quantuscunque labor, ars, sumptus, ad rerum naturalium invisibiles causas inveniendas adhibetur, frustra erit:* 'Without which hypothesis, let never so much labour, art, cost be bestowed for the finding out of the invisible causes of natural things, all will be in vain.' Yet whether that bold assertion should pass for an argument, for an hyperbole, or for a compliment to himself, I am content to let the reader judge.

Page 8.

FOURTHLY, Mr. *Hobbes* in divers passages, wherein he disputes against me, seems to have misapprehended my notion of the air. For when I say, that the air has gravity and an elastical power, or that the air is, in great part, pumped out of the receiver, it is plain enough, that I take the air in the obvious acception of the word, for part of the atmosphere, which we breathe, and wherein we move. Nor do I find, that any other of my readers do otherwise understand me. But Mr. *Hobbes* seems to think he has sufficiently confuted me, if in some cases he have proved (which whether he have done well or no, is not here to be examined) that there is a subtile substance, which he calls æther (but which I wish he had better explained) in some places, which I take not to be filled with air; and that the æther has or has not some accidents, which I deny or ascribe to the air. Whereas I deny not, but that the atmosphere or fluid body, that surrounds the terraqueous globe, may, besides the grosser and more solid corpuscles wherewith it abounds, consist of a thinner matter, which for distinction-sake I also now and then call ethereal. And therefore though I did not think myself obliged to declare against either the atomical or the Cartesian hypothesis, touching the nature of the air, yet I proposed the latter too as probable (which as it excludes a vacuum, so it makes the air consist in great part of a celestial matter). And my incidental explications of the rarefaction and condensation of the air, together with my comparing it to a fleece of wool, sufficiently declare, that I take it not to be a homogeneous body; and though there be air intercepted betwixt the hairs of wool, yet in case I should prove, that a box were not so full of wool as before, because the most part of the hairs had been taken out, I should not think he argued well against me, that should only prove, that the box contained as much of matter, consisting of air and wool together, afterwards as before. Nor do I think Mr. *Hobbes* has in divers passages, wherein he supposes he disputes against me, much more directly contradicted what I teach concerning the air, if that word be rightly and in my sense understood. And on this occasion I must crave leave to add, that whereas he is pleased to intimate, that I misrepresent the Cartesian hypothesis, ascribing that to the air, which *Des Cartes* does to water; if the reader think it worth while to compare the summary account I give of that hypothesis, with what *Des Cartes* himself has taught in his 45th, 46th, and 47th articles of the fourth part of his Principles, wherein that author comprises his doctrine of the nature of the air; he will quickly find, that whether or no Mr. *Hobbes* be mistaken, I am not, unless it be in estimating his hypothesis by what he teaches in his principles, which were published after his meteors, and more elaborately written. And as for that particular, which alone

alone Mr. *Hobbes* alleges, namely, that he makes not the parts of air but of water so flexible; *Des Cartes*'s words in the 46th article are these: *Cum ejus particulæ ferè omnes sint flexiles instar mollium plumularum vel tenuium funiculorum, &c.* 'Seeing almost all its parts are flexile, like little soft feathers or fine threads.' And as for what Mr. *Hobbes* subjoins, *Sed quisquis talis suppositionis author fuit, parum refert. Nam ipsa hypothesis, in qua motus supponitur materiæ subtilis sine causa velocissimus, & præterea corpusculorum innumerabiles vertigines diversæ ab illius materiæ unico motu generatæ, vix sani hominis est.* 'But it matters not, who was the author of that supposition. For 'the very hypothesis itself, wherein is supposed a motion of subtile matter, which is 'swift without any cause assigned, and hath moreover divers innumerable circulations 'of corpuscles generated from the single motion of that matter, is not the conceit 'of a man of wit or sense.' I cannot but in gratitude to such a personage declare my dislike, to find him upon so slight an occasion so coarsely used for an opinion the censurer of it does no better confute, and which is thought to be in some particulars not so unlike his own. And perhaps I should be afraid, that Mr. *Hobbes*'s speaking so severely of one, that was at least a famous geometrician, might reflect upon the English civility in the opinion of strangers, if I did not hope, that those, who have read Dr. *Ward*'s exercitation, will look upon this censure of the Cartesian doctrine by Mr. *Hobbes*, as provoked by that severe judgment of *Des Cartes* mentioned by the doctor in these words: *Nempe hoc est, quod alicubi admiratus est magnus Cartesius; nusquam eum, sive verum, sive falsum posuerit, rectè aliquid ex suppositionibus ratiocinando inferre:* 'This is the thing, that the great *Des Cartes* somewhere admired, that he, 'whether his positions are true and false, doth never in argumentation make any right 'inference from his suppositions.'

Wardi Exer. in Philosph. Hobbian. p. 188.

CHAP. III.

Wherein the weight and spring of the air are asserted, against Mr. Hobbes.

HAVING thus dispatched those general considerations, I thought expedient to premise, my proposed method leads me, in the next place, to consider, that Mr. *Hobbes* does not, that I remember, deny the truth of any of the matters of fact I have delivered. Nor does he, if my memory fail me not, labour to prove, that the explications I have given of my experiments are not agreeable to the doctrine I proposed: but rather thinks fit to reject our two grand hypotheses themselves, the weight, and the spring of the air. And therefore it will suffice us in this chapter briefly, but not slightly, to prove what he is unwilling to grant.

And first, that the air (in the sense wherein we take the word) is not devoid of weight, we have proved by divers experiments: which having more fully delivered in the book itself, it may in this place suffice now to name them.

One then of these experiments, that prove the air's gravity, is, that we found a blown bladder carefully weighed in an exact pair of scales manifestly heavier, when full of air, than when the air was let out.

Next, it has been observed in our 36th experiment, that an æolipile, being well heated, and the little hole left at the top of the pipe being stopt, when it was thus hot; upon the opening of that hole, when the æolipile was grown cold again, the external air rushing in with a whistling noise at the forementioned orifice, made the æolipile weigh so much more than it did just before the external air got in, that it amounted, by computation, to near a thousandth part of the weight of an equal bulk

bulk of water. And though some difficulty may perhaps be moved touching the accurateness of the proportion this way found out, betwixt the gravities of those bodies; yet that the one as well as the other is actually heavy (which is all that we here need contend for) the experiment sufficiently manifests.

Exper. Magdeburgicum apud Schotum, fo. 446.

THIRDLY, in the Magdeburgic experiment (mentioned at the beginning of our epistle) the ingenious makers of it found, that, having before weighed the great receiver they were to exhaust, and having done the like after the extraction of the air, they found it to weigh one whole ounce and $\frac{1}{16}$; *quod sane* (says the learned publisher, though a Peripatetic) *luculentissimum est argumentum gravitatis aëris*; 'which is indeed 'a most evident argument of the weight of the air.'

FOURTHLY, in our 36th experiment we relate our having weighed the air, and that shut up in bodies in our exhausted receiver, wherein of two bodies of differing natures (the one a blown bladder, and the other a glass bubble) that were æquiponderant each to a more solid weight before the air was pumpt out, that which included a good quantity of air did manifestly preponderate after the exhaustion.

AND to these four we might add other proofs to the same purpose; but that these contain in them such a variety of cases, that I think it would be superfluous.

Page 15.

BUT now let us see, what Mr. *Hobbes* objects against the newly mentioned experiment of the bladder weighed in the exhausted receiver for the others he quarrels not with:) *Quod quidem lanx* (saith he) *in qua est vesica, magis deprimitur quam altera, certi esse possunt, oculis testibus: quod autem id à gravitate aëris naturali accidit, certi esse non possunt; præsertim si quæ sit gravitatis causa efficiens nesciunt:* 'that the scale, in which 'the bladder is, is more depreſt than the other, they may be certain, their eyes 'bearing them witness; but that this comes from the natural gravity of the air he 'cannot be assured; especially if they are ignorant what is the efficient cause of 'gravity.' But I know not whom Mr. *Hobbes* will persuade, that a man cannot be sure, that lead is *in specie* heavier than cork, unless he knows what is the efficient cause of gravity. And Mr. *Hobbes* speaks in his 30th chapter (where he expressly treats of that subject) as if that had not been explained by any man, and consequently not by any writer of statics; (and perhaps I am therein somewhat of his mind) and yet sure all these writers, treating of the proportion of heavy bodies, did not write they knew not what. And, though he mentions his own hypothesis, as that *than which nothing is more likely*; yet I think I could frame objections against it, that would not easily be answered, if my present task required it; or if I found his opinion, in this point, embraced, as yet, by men of note. Wherefore I shall now say no more of it than he himself doth, namely, that according to his doctrine, 'It may well be 'thought to determine (for it is a certain consequent) that heavy bodies descend 'with less and less velocity, as they are more and more remote from the æquator; 'and that at the poles themselves they will either not descend at all, or not descend 'by the axis: which whether it be true or false, experience must determine.' Which till it have done in his favour (an event I do not expect) I hope he will allow me to distrust his hypothesis.

Elem. of Phil. Ch. 30. Sect. 4.

Page 16.

BUT to return to our experiment. The account he gives, why the bladder does propend (for so he loves to speak) is this; *Quod vesica sive follibus sive flatu oris distenta sit, gravior sit quam eadem vesica non distenta, negare nolo, propter majorem quantitatem atomorum follibus vel corpusculorum fuligineorum ab halitu inflatorum. Ab experimento autem quod fit à vesica inflata nihil colligunt quod sit satis certum. Oportuit lancibus imponere duo vasa pendere æqualia, quorum alterum esset accuratè clausum, alterum apertum: sic enim non inflatus sed inclusus tantum aër ponderatus esset. Quando igitur aërem sic ponderatum videbis, meditabimur postea quid dicendum sit de phænomeno quod retuleris:* 'that 'the

'the bladder, whether it be blown up with a pair of bellows, or with the breath of 'one's mouth, is heavier than when it is not blown up, I will not deny, because of 'the greater quantity of atoms from the bellows, or of fuliginous corpuscles, that 'are blown in from the breath. But notwithstanding they gather nothing of sufficient 'certainty from this experiment of a blown bladder; they ought to have put into 'the scales two vessels of equal weight, whereof one should be shut and the other 'open: for by this means air not blown in, but only inclosed, had been weighed. 'When therefore you shall see air so weighed, we will afterwards consider 'what may be said concerning the phænomenon you bring.' But as to the first part of this passage, it does not deny the gravity of what we call the air; but only endeavours to shew what parts they are that make it heavy. And as to the second, he seems to mistake the present case. For, there is no need, that the air in the bladder be, before the exhaustion of the receiver (in which the foregoing, fifteenth page, he declares he supposes the experiment to be made) heavier than the outward air. Wherefore when he subjoins, that from this experiment we collect nothing *quod sit satis certum*, the affirmation is not an inference, but precarious. And as for the annexed way, whereby he would wish to have an experiment made fit to infer the gravity of the air, if he had not overlooked what I have delivered in the beginning of the 36th experiment, he would easily have perceived, that we did make a trial much of the same nature with that he desires. For we weighed in our receiver the air, in a glass hermetically sealed; wherein it was not (to use his expression) *inflated*, but only included. This is what he here objects against the gravity of the air in the other place (pag. 8, and 9.) where he says something to this controversy: he inculcates also, that we should first explicate what is gravity, and then adds, *Quod atmosphæræ insunt permistæ corpori æthereo multæ tum aquæ tum etiam terræ particulæ, facile persuadeor; sed quod in medio æthere, sursum, deorsum, quaquaversum motæ, nec semper alteræ alteris innitentes gravitent, inconceptibile est:* 'That many particles, both 'of earth and water, mingled with the æthereal body, are in the atmosphere, I am 'easily persuaded: but that in the middle of the æther they should move upwards, 'downwards, every way, and that one leaning on the back of another they should 'not gravitate, is a thing utterly unconceivable.' To which he adds two or three reflections, whose examen being here unnecessary, would require more time than perhaps it would (in reference to the present controversy) deserve: for we are now enquiring, not how the air comes to gravitate, but whether or no it have gravity. And since in his Elements of Philosophy he grants, and gives his reason for it, that 'if 'air be blown into a hollow cylinder, or into a bladder, it will increase the weight of 'either of them a little:' and since here he likewise confesses (as we have just now seen) that there are mingled with the æther many aqueous and earthly (and consequently heavy) particles: he confesses that, which we labour to evince; namely, that the air is not devoid of weight. And it concerns us no more than himself, to shew how the corpuscles, upon whose account the air is heavy, make it so. And this being what Mr. *Hobbes*, in several places, thinks fit to object against the gravity of the air; the reader will, I suppose, easily take notice, that he has left the experiment of the æolipile, and some others, unanswered. Though these alone prove, that the air is a manifest weight even when it is not compress't, but retains its laxity. Having said thus much to evince against Mr. *Hobbes* the gravity of the air, let us now examine, whether it have not also a spring (in the sense we take that word in *.)

Page 9.

Chap. 30. Sect. 10.

* *Utraque enim illa phantasia, tum gravitatis atmosphæræ, tum vis elasticæ sive antitupiæ aëris, somnium erat.* Dial. pag. 21.

This,

This, though Mr. *Hobbes* be pleased to call (as he also does the weight of the air) *a Dream*; yet he does himself grant, in effect, as much as is requisite to prove the spring of the air, in the sense I contend for it. For taking upon him to give account (how good an one we shall see anon) of that known experiment, wherein the air is compreſt in a glass-bottle by the forcible injection of water; which water, when the glass is unstopt, the air does again throw out in recovering its former dimensions; of this experiment (I say) he gives this account (pag. 24.) *Aër, quo ab initio sphæra plenus erat à corpusculis illis terreis motus motu circulari simplice, vi injectionis coactus, qui quidem purus est, exit (aquam injectam penetrans) in aërem extrinsecum, locum relinquens aquæ; sequitur ergo corpusculis illis terreis minus relinqui loci, in quo motum suum naturalem exercere possint: itaque in se mutuo impingentes aquam urgent ad egressum; egredientem aër externus (quia universum supponitur esse plenum) penetrat, locumque egredientis aëris successivè occupat, donec corpuscula, quantitate aëris eadem restituta, libertatem motui suo naturalem recipiant.* 'The air, with which in the beginning the spherical glass 'was full, being moved by those earthy corpuscles in a simple circular motion, and 'being compreſt by the force of the injection, that of it which is pure (penetrating 'the injected water) gets out into the open air, and gives place to the water. It 'follows therefore, that those earthy corpuscles have less place left, in which they 'can exercise their natural motion: therefore beating one upon another, they force 'the water to go out; it thus going out, the external air (because the universe is 'supposed to be full) penetrates it, and successively takes up the place of the air, 'that goes out, until the corpuscles, the same quantity of air being restored, regain 'a liberty natural to their motion.'

BUT how little this comes short of granting as much spring to the air as the Cartesians do, and as I need require, may easily be judged by divers passages in our book; and particularly by our proposing, as not improbable, the Cartesian way of explicating the spring of the air; according to which, the corpuscles, that swim in the æther, being each hindered by the neighbouring ones from the freedom of its motion, they beat off one another (which Mr. *Hobbes* would have them do:) whence it comes to pass that, in any assigned portion of air here below, the corpuscles, that compose that portion, beaten off by one another, do make the whole portion tend to obtain (though not exactly to fill up) more room, and consequently to emulate a spring, like that which we scruple not to ascribe to a compreſt fleece of wool, because of a like endeavour to expand itself.

WE may inforce this by another passage of Mr. *Hobbes*'s, that speaks expressly enough to our present purpose, where he gives this reason of one of the phænomena of our engine: *Quoniam per suctoris retractionem aër purus impulsus erat, partes autem terreæ impulsæ non erant; major erat ratio particularum terrearum, quæ extra cylindrum suctori contiguæ erant, ad aërem purum, in quo motum suum exercebant, post revulsionem quam ante: quare particulæ illæ motæ minus habentes loci ad motum suum naturalem exercendum, aliæ aliis impingebant, & propellebant: necesse ergo erat, ut particulæ, quæ suctoris superficiei contiguæ erant, suctorem propellerent**: 'because by the drawing 'back of the sucker the pure air was thrust in, but the earthy parts were not thrust 'in; there was a greater proportion of earthy particles, which, without the cylinder,

* The motions therefore of those small bodies (speaking of the earthy particles in the air) will be less and less free, by how much the quantity of the injected water is greater and greater: so that by their motion falling upon one another, the same bodies will mutually compress each other, and have a perpetual endeavour of regaining their liberty, and of depressing the water that hinders them. *Elem. chap.* 30. *sect.* 9.

'were near the sucker, unto the pure air, in which they exercised their motion as 'well after this revulsion as before. Wherefore these particles so moved having less 'place to exercise their natural motion in, some of them fell foul and beat upon the 'rest. So that of necessity the particles, that were near the surface of the sucker, 'must drive it upwards.' To which we may add, that Mr. *Hobbes* himself seems rather to reject other men's ways of proposing the spring of the air, than resolutely to deny the thing itself. For, *Vidisti* (says he) *jam elastrum illud aëris, quod supponunt, aut impossibile esse, aut recurrendum esse ad hypothesin Hobbianam:* 'you see now, that 'the spring of the air, which they suppose, is either an impossible thing; or they 'must (for its defence) have recourse to the hypothesis of Mr. *Hobbes*.'

But besides Mr. *Hobbes*'s concessions in the passages newly recited, and some others, we can prove the spring of the air by many of the phænomena of our engine, which we have deduced from it, and of which he does not offer any other way of explication. Wherefore we shall now content ourselves to prove the spring of the air by two experiments; the one not mentioned in our epistle, and the other much opposed by Mr. *Hobbes*.

And first, if you make the Torricellian experiment in a tube of between two foot and a half and three foot in length; and if, when the mercury rests at its wonted station, you dextrously stop the orifice of the tube with your finger (that orifice being lifted up as near the surface of the restagnant mercury as it can be, without giving admission to the external air) and if then you quite lift up the tube thus stopt into the free air; you shall feel, upon your finger, little or no gravitation or pressure from the weight of the mercurial cylinder, distinct from the weight of the tube: because (as we have more fully explicated this phænomenon elsewhere) the gravity of the quicksilver is balanced by that of the outward air, that thrusts the finger against it. But if you invert the tube, and having let in the air at the orifice, stop it again with your finger, and again let the mercurial cylinder lean upon that finger; you shall then find your finger strongly prest, and endeavoured to be thrust away: which new pressure, since it cannot come from the mercury, that being the very same, that was in the tube before, nor from the weight of the admitted air, which perhaps may not amount to so much as a grain, to what can we rationally ascribe it, but to the spring of the included air, whose force will be as well manifest to the eye as the finger, if the tube be unstopt under the surface of the restagnant mercury; for then that in the glass will not rest, as before, at the usual station, but be depreſt beneath it a good way, perhaps some inches? And if you make the Torricellian experiment in a short tube sealed at neither end, but stopt above and below with your fingers, you shall find, upon the unstopping of the upper orifice a new and forcible pressure upon the finger, that keeps the lower orifice stopt, made by the gravitation of the external air, which was before kept off from leaning upon the mercurial cylinder by the upper finger; the pulp of which finger, by that gravitating air, was before thrust into the deserted cavity of the tube (as we have elsewhere discoursed, in a fuller measure, of these experiments). Which will evince, against Mr. *Hobbes*, both the spring of the air and gravity of the atmosphere; since he is, as little as I, for ascribing these phænomena to the efficacy or absence of my other antagonist's imaginary Funiculus.

In our Defence, against Linus.

The other experiment I shall mention is the fourth in our epistle; namely, that of the swelling and shrinking of a bladder hung in our receiver, according as the ambient air, and consequently its pressure, is withdrawn or suffered to return. But though this experiment be so congruous to our hypothesis, that it is generally acquiesced in by those ingenious men, that have hitherto seen it; yet Mr. *Hobbes* is

pleased to reject our explication, and substitute another in these words, which is all
Page 19. he has concerning this matter: *Quia cuticula omnis ex filiculis constat, quæ propter figuras contactum per omnia puncta accuratum habere non possunt, pervia ergo est vesica, cum sit cuticula, nec aëri tantum, sed etiam aquæ, qualis est sudor. Eadem ergo aëris per vim incussi est compressio intra vesicam, quæ extra, cujus conatus, propter viam motuum undiquaque decussatum, tendit undiquaque ad superficiem vesicæ concavam. Quare necessarium est, ut undiquaque intumescat, & crescente conatûs vehementiâ tandem laceretur.* 'Because every skin is made up of small threads or filaments, which by reason of their 'figures cannot accurately touch in all points; the bladder therefore, being a skin, 'must be pervious not only to air, but to water also, as to sweat: therefore of the 'air beat in by force, there is the same compression within the bladder, that there is 'without. The endeavour of which, the way of its motions being every way cross, 'tends every way to the concave superficies of the bladder. Wherefore it is of necessity, that it must swell every way, and the vehemency of the endeavour increasing, be torn at last.' But, if this be a sufficient answer to such an experiment, I confess I fear it will be harder than we are yet aware of, to prove any thing by experiments.

FOR, first, how unlike is it to be true what he affirms, and what his reply supposes, namely, that such bladders as we used are readily pervious to the air; when easy experience shews us, that by leisurely compressing such blown bladders betwixt our hands, we shall rather break them (as we have tried) than squeeze out the air at the pores? So that the rest of his answer being built upon what is so repugnant to common experience, will not need a particular confutation: but however, *ex abundanti* we will add, that in our 36th experiment we relate, that by the exhaustion of the air we likewise broke a glass hermetically sealed; and to say that glass also is pervious to air, were to affirm what the greatest part of his book supposes to be false. Besides, whereas there is not any sensible and unquestionable phænomenon to prove, that the receiver is full of any such *aër per vim incussus* as he would have, we see plainly, that when the air does manifestly get into the receiver, the bladder is not thereby made to swell, but strangely to shrink. Moreover (since according to Mr. *Hobbes*) the bladder is pervious to the air, and the air within the receiver is universally compreſt, as well that which is within the bladder as that which is without it; how comes it to pass, that the air, that bears against the convex surface of the bladder, does not resist that which is contiguous to the concave superficies of the same; and at least, how comes the bladder to be broken by the air; which, according to Mr. *Hobbes*, can get in and out at pleasure? And lastly, to shew, that to the swelling of the bladder there needs nothing but the spring of the included air, and not such vehement agitation of the ambient air, as Mr. *Hobbes* supposes to be made in our engine; it appears by the elsewhere-mentioned experiment of Monsieur *Paschal*, that in the free and ordinary air a foot-ball half blown up will swell more and more, the nearer it is carried to the top of an high mountain; where the incumbent cylinder of the atmosphere is shorter, and its weight lighter; and will, for the contrary reason, grow more and more flagged, the nearer it approaches again to the foot of the mountain.

THOUGH I doubt not, but the arguments employed in this chapter will be sufficient to convince impartial readers; yet I shall add, by way of inforcement, that whereas Mr. *Hobbes* ascribes the weight of the air in bladders to the earthy corpuscles intruded by him, that blows them up; and attributes the spring of the air in the wind-gun, and in the phænomena of our engine, to the violent motion the air is put into by the vehement

vehement impulses of the rammer or sucker; our doctrine may be evinced by experiment, wherein the air in its natural and wonted state operates, without being forcibly compreſt or put into motion by us. This may appear by the two sorts of experiments to be made upon high mountains, which we have mentioned and urged in the *second part* of our *defence against the learned* Linus. Wherefore referring the reader thither, we shall now only, in very few words, mention the substance of them.

The first experiment is, That it has been found upon trial, both formerly in *France* and since in *England*, that the quicksilver in the Torricellian experiment falls notably lower at the top of a mountain than at the foot, (by Monsieur *Paschal*'s observation upon a hill, far higher than those the experiment was tried on here, the difference was so great, as to amount, as the most ingenious *Pecquet*, a happy promoter of experimental learning, informs us, to above three inches) which we say is caused by this, that the atmospherical cylinder is much lighter, as well as shorter at the top of the mountain than at the bottom. And Mr. *Hobbes* disallows not the experiment, but yet gives only this account of it: *Sed & particulæ illæ, quæ interspersæ aëri ita moventur, ut supposuimus, magis confertæ sunt ad radicem montis quàm in summo, nam hoc quoque supposuimus.* But what then? How does the plenty of these interspersed particles hinder the mercurial cylinder from descending at the bottom of the hill as much as at the top, unless by their gravity or pressure? And it is very unlikely, that the earthy atoms, contiguous to the restagnant mercury at the bottom of the hill, should be able, by their weight, to keep suspended a cylinder of mercury of above three inches, unless the contiguous air were gravitated upon by the weight of other incumbent parts of the atmosphere. Page 12.

The other of the two mentioned experiments is briefly this, That a thermoscope being carried from the bottom to the top of a hill, the included air, instead of shrinking in that colder region, manifestly dilated itself, and notably depressed the water. An effect which I see not to what it can well be attributed but to the spring of the included air, which having not near so great a pressure against it from the atmosphere incumbent on the restagnant and suspended water, was able to make itself more room than before it could; and since that pressure of the atmosphere depends, for aught appears, upon its gravity, the same experiment may argue both the spring of the air and its weight.

And this may suffice for our third chapter, wherein having evinced against Mr. *Hobbes* our grand hypothesis of the weight and spring of the air, I hope we have dispatched the chief part of our work; since as for the particular explications we deduce from these hypotheses, there are but very few, if any, that he endeavours to prove incongruous to them. Yet after we shall have (in the following chapter) considered upon what grounds he prefers his doctrine before ours, we shall (God permitting) in two or three other chapters gather up the things, that he objects against some particular opinions and explications by us delivered, and examine them.

CHAP. IV.

Wherein Mr. Hobbes's *principal explications of the phænomena of the author's engine are examined.*

OF the hypotheses, that Mr. *Hobbes* assumes, to explicate the phænomena of our engine, himself gives us a summary in this passage, (*pag.* 10.) *Intellexti ergo hypotheses meas?* 1. *Quod aëri interspersæ sunt particulæ multæ terreæ præditæ motu circulari simplice, naturæ congenito.* 2. *Quod major est quantitas earum particularum in aëre propè ad terram, quàm in aëre à terra remotiore:* 'Have you understood my hypothe-'ses? 1. That there are with the air interspersed many earthy particles, endued with 'a simple circular motion, congenite to its nature. 2. That there is a greater 'quantity of these particles in the air that is near the earth, than in that which is 'more remote from it.'

Now here I might at the beginning take notice, that there are other things, which he takes for granted. As, first, *Non dari Vacuum*, which, as we have already seen he has not well evinced, nor I think easily will, upon the grounds he proceeds on. Next, that our common air is chiefly composed of an æthereal substance, which methinks he should have proved; since for the most part the Vacuists (and such he will needs have his adversaries to be) admit not that pure air of his. Thirdly, that the air, at least the pure air, is easily divisible into parts always fluid and always air. Indeed he says of this assumption, *Nec suppono tantùm, sed credo*; but neither to suppose nor to believe, is to prove. And what he adds, *Neque est qui hactenus ullam adduxit rationem, quare ita esse non potest:* 'Nor is there any one, that hitherto has brought 'any reason why it may not be so:' if it were true, would conclude little, since many things have not been, and perhaps cannot be, proved to be true; of whose not being possible no proof has been given. We might, I say, mention and examine these other assumptions of our author, but for brevity sake we will consider those two lately recited from him.

Pag. 4. *Ibid.*

AND as for the second of them, bating the peculiar motion he is pleased to ascribe to the earthy particles, I shall not contend with him about that hypothesis; and therefore shall now only consider the other. The *Motus circularis simplex* itself, which he imagines, in the sun and the terrestrial globe, I shall not need to examine, since Dr. *Ward* (a person whom, without disparagement to a famous man, I may affirm to be at least as esteemed for astronomy as Mr. *Hobbes*) has expresly endeavoured to confute it, and that not without some derision (which yet I willingly forbear to imitate) by arguments, that I cannot learn Mr. *Hobbes* has yet answered. And I am informed, that the learned Dr. *Wallis*, and others, intend some animadversions on this motion. But restraining our present consideration to what this Dialogue suggests to me, this assumption to me seems very precarious, since I know not any unquestionable example or experiment, whereby it can be made out, that any small parcel of matter has such a *motus circularis simplex*, as he ascribes to each of these innumerable earthy, and (as himself adds in the same page) aqueous particles. The only argument he brings in that page to prove, that each atom would have this motion, if all the rest of the earth were annihilated, does not to me seem clear. For, not to mention, that it is still by many learned men doubted, whether the terrestrial globe itself have it; nor to examine, whether or no he assigns a good natural cause of it; it is not always true,

true, that each minute part of an homogeneous body (which yet it will be hard to prove the terrestrial globe to be) has in every respect the same qualities with the whole; as the roundness, which a small drop of water or quicksilver is commonly observed to have when it leans upon a dry or greasy plain, is not to be met with in great portions of either of those liquors, though placed upon the same plain. And Mr. *Hobbes*, as well as we, makes the terrene atoms in the air to have gravity, which yet is a quality, that does not properly belong to the whole globe of the earth; nor is it manifest, why, because the terrestrial globe moves in a vast circle about the sun, each particular atom of it must describe a small circle in the air about I know not what center. And since he teaches in his second hypothesis, and a few lines before it, that the air, near the earth, abounds with such terrene corpuscles, it is not likely they should be permitted to exercise such a regular motion as he attributes to them; but hitting against one another, they must in probability be put into almost as various and confused a motion, as *Des Cartes* ascribes to his terrestrial particles swimming in the atmosphere. That which some will, I doubt not, peculiarly wonder at in Mr. *Hobbes*'s hypothesis, is, that he makes this regular motion of each atom *naturæ suæ congenitus*: for philosophers, that are known to wish very well to religion, and to have done it good service, have been very shy of having recourse, as he has, to creation, for the explaining of particular phænomena. And the Cartesians will think it at least as allowable for them to suppose the motion he will not grant in their *materia subtilis*, as for Mr. *Hobbes* to assume it in his *particulæ terreæ*; especially since he seems to make each such atom put into and kept in a regular motion; whereas they assume but the having of one general impulse given to the whole mass of matter. Those likewise, that fancy a spring properly so called in particular aërial corpuscles, will hence perhaps take occasion to think they may suppose an ingenite motion fit for their turn, as well as he an ingenite *motus circularis simplex*. How well likewise this hypothesis will agree with his fundamental doctrine, that *Nihil movetur nisi à corpore contiguo & motu*: 'Nothing is moved but by a contiguous body that is in motion:' I leave to him to consider. As also whether or no *Gassendus*, and those other Atomists that admit creation, may not hence countenance their grand supposition of the congenite motion of atoms, which granted, would destroy the best part of Mr. *Hobbes*'s philosophy. But whatever becomes of this *motus circularis simplex*, I need not be much sollicitous, having formerly shewn, that the admission of it would not disprove what I have delivered concerning the spring of the air: and therefore leaving Mr. *Hobbes* to dispute it out, if he think fit, with his other adversaries, I will proceed to the main explications, wherein Mr. *Hobbes* endeavours to prefer his doctrine about the phænomena of our engine before ours. And these I find to be the four that ensue.

The first and principal of these is that, wherein he strives to prove, that by the exhaustion of our cylinder no vacuum is produced, and to give of the experiment itself a very differing account from ours. This he does in the following passage; which, by reason of its importance in our present controversy, we shall set down *verbatim*: *Dum suctor* (says he) *retrahitur, quanto relictus locus major fit, tanto minus loci relinquitur aëri externo, qui retrusus à suctore moto versus externa, proximum sibi aërem similiter movet, & hic alium, & sic continuè, ita ut necesse sit aërem tandem compelli in locum desertum à suctore, & intrare inter superficiem suctoris convexam & cylindri concavam: supposito enim aëris partes esse infinitè subtiles, impossibile est, ut vis illi, qua retrahitur suctor, illæ non se insinuent. Primo enim, contactus superficierum istarum per omnia puncta perfectus esse non potest, quia ipsæ superficies fieri infinitè lævis non possunt.* page 5.

Deinde

Deinde vis illa, quæ ad ſuctorem revellendum adhibetur, cavitatem cylindri aliquantulum diſtendit. Poſtremo, ſi in confinio duarum dictarum ſuperficierum ingrediatur una tantum atomus dura, aër purus eâ viâ ingreditur conatu quantumvis debili. Poteram etiam computaſſe aërem illum, qui propter eandem cauſam inſinuaſſet ſe per cylindri valvulam. Sublatam ergo vides conſequentiam à retractione ſuctoris ad locum vacuum. Sequuturum hoc quoque eſt, aërem illum, qui eſt in locum à ſuctore deſertum impulſus, quia magnâ vi impulſus eſt, motu valde celeri & per circuitum inter ſummum & imum in cylindro moveri; cum nondum ſit, quod motum ejus poſſit debilitare: ſcis autem nihil eſſe, quod ſibi motum aut impertiri poſſit aut diminuere. 'While the ſucker is drawn back, by how much a greater 'place is left (within) by ſo much a leſſer place is left to the external air; which 'being thruſt backwards by the motion of the ſucker towards the outmoſt parts, doth 'move in like manner the air that is next itſelf, and that air the next, and ſo for-'wards: ſo that it is of neceſſity at laſt, that the air muſt be compelled into the ſpace 'deſerted by the ſucker, and to enter between the convex and ſurface of the ſucker, 'and the concave of the cylinder. For it being ſuppoſed, that the parts of the air 'are infinitely ſubtile, it is impoſſible but they ſhould inſinuate themſelves that way, 'by which the ſucker is drawn down. For firſt, the contact of thoſe ſurfaces cannot 'be perfect in all points, becauſe the ſurfaces themſelves cannot be made infinitely 'ſmooth. Then, that force, which is applied to draw back the ſucker, doth di-'ſtend in ſome meaſure the cavity of the cylinder. Laſtly, if in the confines of '(that is, betwixt) the two ſurfaces, any one ſingle hard atom ſhould enter, pure air 'will enter at the ſame way, although with a weak endeavour. I might alſo have 'accounted that air, which for the ſame cauſe inſinuates itſelf through the valve of 'the cylinder. You ſee therefore the conſequence from the retraction of the ſucker, 'to the being of an empty place, is taken away. It will follow alſo, that the air, 'which is driven up into the place deſerted by the ſucker, becauſe it is driven up 'thither by a great force, is moved with a very ſwift and circular motion betwixt the 'top and the bottom in the cylinder, becauſe there is nothing there, that can weaken 'its motion: and you know, that there is nothing, that can give motion to its own-'ſelf, or diminiſh it.' But this ratiocination containeth divers things liable to exceptions: and in order to the examining of it I muſt premiſe, that I know not, why Mr. *Hobbes* ſhould here confine his diſcourſe to the pump, without taking notice of the glaſs, for whoſe evacuation it was deſigned. Wherefore, for eaſier conſideration ſake we will conſider, how this diſcourſe will account for the exhauſtion of the receiver, as well as for the cylinder; for we uſually empty them both in the ſame trials. And he being obliged to explicate the exhauſtion of the one as well as the other, it will be convenient to take into conſideration the receiver alſo; becauſe that being of glaſs and tranſparent, we can better ſee what happens in it than in the opacous cylinder. This premiſed, we may now proceed to the exceptions themſelves. And, firſt, I do not clearly ſee by this explication how he avoids a vacuum: for, according to his firſt words, the external air is diſplaced by the motion of the ſucker outward, and this diſplaced air muſt move that which is next to it, and that the next, and ſo onward (whether *in infinitum* or no, he declares not) ſo that at length *(tandem)* the air muſt be compelled into the place deſerted by the ſucker: ſo that till this returning air get in betwixt the ſucker and the cylinder, how appears it from this diſcourſe, that the deſerted ſpace was not empty for ſome little while? For, certainly, all theſe motions of the air forward and backward could not be performed in an inſtant; as may appear by the motion of ſounds and echoes, whoſe velocity is reducible to meaſure. Secondly, though he takes his adverſaries to be Vacuiſts, yet (to give an account of theſe phænomena)

phænomena) he supposes the plenitude of the world; as may appear both by express passages in his dialogue, and by his here rendering no other probable cause of the air's getting into the room relinquished by the sucker. But, because I have not here taken upon me the person of a Vacuist, I shall offer some other considerations. I wish then, thirdly, that Mr. *Hobbes* had declared from whence the regress of the air's impulsion should begin; for that may well be required from one, that, making the world full, and for aught appears (the celestial globes excepted) fluid, allows us to believe it infinite, if the magistrate shall please to enjoin us that belief. Fourthly, I demand what necessity there is, there should be such a forcible return of the impulse as is requisite to thrust in the air at so narrow a passage, as that between the sucker and cylinder. For, why may not that impulse, when diffused in the vast ambient medium, be so communicated and blended among the differing motions of the other parts of it, as not to return again from whence it begun? As we see that a voice, though strong, will not move the air beyond a certain distance, smartly enough to be reflected in an echo to the speaker: and a stone cast into a lake will have the waves it makes diverted from returning to the place they began at. Fifthly, I do not likewise see, that it is proved, or probable, what Mr. *Hobbes* affirms of so thick a cylinder as ours, that it should be distended by the depressing of the sucker. But this I insist not on; for the main thing, that is peculiar in Mr. *Hobbes*'s explication, is, that as much air as is driven away by the sucker, gets presently in again betwixt it and the cylinder: wherefore let us examine that a little. I say then, that by the air, which is so supposed to get in, he either means, in the usual sense and in ours, the common air, such as we live and breathe in, or he does not.

If he do speak of such air, I can plainly prove by several experiments, that our engine is in great part devoid of it.

For first, if there be a contrivance made, whereby the whole pump may be covered with water, one may, as we have tried, plainly see the air, that is drawn out of the receiver, at each reciprocation of the sucker, pass in great bubbles out of the valve through the water.

Next, it appears by the *Magdeburg* experiment formerly mentioned, that by reason of the recess of the air, the globe of glass, whence it went out, was diminished in weight above an ounce. Thirdly, the same truth may be proved by the experiments formerly mentioned of the swelling of a bladder, and the breaking of an hermetically-sealed glass upon the recess of the ambient air; these experiments having been already vindicated from Mr. *Hobbes*'s very improbable explications of them. Fourthly, the same may be proved by the breaking of weak or ill-figured receivers inwards; of which in our hypothesis the reason is clear, but not in Mr. *Hobbes*'s. But, fifthly, (not to multiply instances, though that were easy for me) what I contend for may be sufficiently proved by this one phænomenon, that though, if the receiver being full of common air the key be turned under water, the water will not at all be spurted up at the open orifice: yet the like being done after the exhaustion of the receiver, we have had divers gallons of water violently impelled into the cavity of the glass; which could not happen if it were full of air, both in regard there can be no probable cause assigned, why the water should be thus spurted up; and because the receiver being already full of air, either two bodies must be contained in one place, and so we must allow penetration of dimensions; or else common air, to which glass is impervious, must pass through the water, which we conclude it does not, because no such bubbles are made in the external water as would appear if common air passed through it. Nay, so little of this common air was sometimes left in the globe used at *Magdeburg*,

burg, that when the water was suffered to rush in, it reduced the air into less by the beholder's estimate, than the thousandth part of the capacity of the globe: and even if our receiver be unstopped, not under water, but in the open air, the ambient air will violently press in, with a noise great and lasting enough to argue, that the glass was far from being full of such air before.

AND thus we may argue against Mr. *Hobbes*, if he would have the engine, when we call it exhausted, filled with common air, as his words in the recited passage (where he talks of the external air, and that impelled into the cylinder, without differencing them) seem to intimate. But because by some other passages of this Dialogue he may be favourably thought to mean, that the pure air (as he speaks) is that, which gets in by the sides of the sucker into the pump, and so into the receiver; let us consider his explication in this sense also. And not to urge, that it had not been amiss, if, to avoid ambiguity, he had more clearly expressed himself, and named that other here, as well as he elsewhere calls it so; not to urge this, I say, I desire it may be taken notice of, that if Mr. *Hobbes* take the air in this second notion, he opposes not what I have delivered; the air I pretend to be pumped out of the receiver, being the common air, which consists in great part of grosser corpuscles than the æthereal substance: and therefore I might safely pass on to another subject. But I consider further, that even this explication of Mr. *Hobbes*'s will be liable to the two first inconveniences lately objected against the other in favour of the Vacuists; and to divers of those things besides, that are objected in the following parts of that discourse. Next I observe again, that though the pump be all the while kept under water, yet the exhaustion of the cylinder and receiver will be made as well as in the open air. I demand then of Mr. *Hobbes*, how the pure air gets in by the sides of the sucker that is immersed in water? I presume, that for want of a more plausible answer he will here say (as he elsewhere does in an almost parallel case) that the air passes through the body of the water to fill up that deserted space, that must otherwise be void: but then I appeal to any rational man, whether I am obliged to believe so unlikely a thing upon Mr. *Hobbes*'s bare affirmation. If I be, I must almost despair to prove things by experiments; and if he will allow me to expect from him as much as he seems to do from me, I shall scarce despair to maintain almost any hypothesis I please: for, besides that he does not so much as pretend by any phænomenon to countenance this bold assertion, there are phænomena, that make against it. For I know not how many experiments shew us, that when air passes through water, it makes bubbles there, which in our case do not appear. And besides, I see not, why the outward air should not rather impel the water (as we see it frequently does in such cases) than be supposed to dive so strangely and unperceivedly through it. When also the diligently-exhausted receiver is unstopped under water, he that observes, how the water rushes in with a stream as big as the passage will give leave, will hardly imagine, that at the self-same time, as much air, as there gets in water, can pass through the same hole without being perceived. But it may by Plenists be said in Mr. *Hobbes*'s behalf, and it seems the most that can be said, that either his explication, or a vacuum must be admitted. To which I reply, first, that he has not evinced there can be no vacuum; having endeavoured to prove it but by a single experiment, which at best does not more strongly plead against a vacuum than this does for it. Next, that we have lately made it probable, that by his explication he does not avoid the necessity of a vacuum And thirdly, that a Plenist, without having recourse to Mr. *Hobbes*'s precarious *diving* of the air, may more probably decline the necessity of yielding a vacuum by saying, according to the principles of the Cartesians (the

subtilest and wariest champions for a plenum I have yet met with) that the æther is, by the impulse of the depressed sucker, and the resistance of the ambient bodies, squeezed in at the pores of the glass or cylinder into the cavity of the vessel, as fast as room is there made for it. And I confess, I somewhat wonder at Mr. *Hobbes*'s being averse to this way of solving the objected difficulty, since (a little above the middle of that passage of his we have so long been examining) he supposes the parts of the air to be infinitely subtile; which if they are, I know not what pores can be too narrow for them to insinuate themselves into. But, to press this no farther, I must here take notice, that whether the cavity of the receiver be resolved to be (totally or in part) empty or full of Mr. *Hobbes*'s æthereal body, or the Cartesians celestial matter; the violent rushing in of the water, when the vessel is unstopped under that liquor, and divers other phænomena, which will not be ascribed to the subtile matter within (to which they attribute not any attraction) sufficiently argue, that there is in the external air a far greater power of pressing inwards, than there is within of resisting; and consequently such a weight or spring in that air as my epistle challenges to it. I had almost forgot to answer the last lines of Mr. *Hobbes*'s so often mentioned passage, where he would have the air, that he supposes to be impelled into the sucker, to move very swiftly betwixt the top and bottom of it. And so elsewhere he would have the same air, when it gets into the cavity of the receiver. But having tired myself, as I fear I have you, by dwelling so long upon one passage; I will skip somewhat, that I here approve not in the ratiocination, and only say, that when a light bladder is suspended in the cavity of the receiver, it betrays no such motion as is here imagined; nay, the flame of a taper, as our epistle mentions, was not blown out, nor (for aught appeared) stirred by this supposed wind: to which I shall add, that smoke produced in the receiver, whilst it remained exhausted, was not by this vehement motion of the air blown about the receiver, as is particularly set down in our Appendix, promised by the translator of the newly-mentioned treatise. But if you let in the common external air at the stop-cock, that indeed will rush in with noise and violence, and whirl about the bladder, that hung quietly enough before.

Having thus examined Mr. *Hobbes*'s first solemn explication, I now proceed to the next; wherein he undertakes to give an account by his hypothesis of the reason, why in our engine, if the manubrium of the depreſt sucker happens to slip out of the pumper's hand, the sucker is carried up again towards the top of the cylinder. But since this explication is such, that though he mentions it as his first, presently after the recital of his two hypotheses, he himself is pleased to confess in the last page of his book, that it is erroneous; I shall forbear to vex it, thinking such acknowledgments more fit to be imitated, whenever there is the like occasion, than to be discouraged. But as for the explication, which at the end of his Dialogues he substitutes for his retracted one, I confess to me it is so obscure, that I know not well what to make of it. But, as far as upon consideration I can understand it, it is coincident with that, which in our method will be called his fourth explication; with which, that it may the better agree, seems one of the chief reasons of his altering it from what we had proposed at first. Wherefore we should presently fall upon examining them both together; but that between them I meet with an explication (which in our reckoning is the third) of the Torricellian experiment.

And here he spends many words to prove the opinion he had, whether proposed or adopted, in his Elements of Philosophy; namely, that the place deserted by the suspended mercury is not empty, but full of air. But because this exposition assumes what he has not yet demonstrated, *viz. non dari vacuum*, and because the Torricel-

lian experiment, as it is wont to be made, is none of the phænomena of our engine; I shall refer you to what those learned men Dr. *Ward* and *Moranus* have professedly, and the first of them largely enough, written against Mr. *Hobbes*'s Explication (yet without making all that either of them teaches mine:) because, for my part, it will suffice me to argue, as I did before, that if he takes the air in the common sense of the word (and that wherein his readers generally understand him) his conceit is manifestly repugnant to several such phænomena as these. That if the experiment be very well made, we may, by inclining the tube, impel the mercury from its wonted station to the top of the tube; which will not happen, in case the air were before inclination let into that deserted space. That if, when the mercury is settled at its wonted station, the tube be lifted up out of the restagnant quicksilver, the outward air will drive up the heavy mercurial cylinder oftentimes with force enough to beat out the sealed end. To which we shall add only this experiment: the quicksilver resting at its wonted station, if you carefully stop the lower orifice under the surface of the restagnant quicksilver, and then lifting up the tube (that which we used was about three or four foot long) into the air, keep it well stopped; if, I say, you first depress one end and then the other, you shall find the quicksilver fall against the depressed extreme of the tube with such swiftness and force, as will perhaps surprize you, and make you apprehend, that the tube will be either beaten out of your hand or broken: whereas, if unstopping the tube, whilst the self-same quantity of mercury remains in it, you let the outward air into the cavity unpossessed by the mercury, and then if you again stop the orifice with your finger, and proceed as formerly, you shall perceive the motion of the included liquor to be very much slower and less violent than formerly, by reason of the resistance of the admitted air: which will also manifestly disclose itself by the conflict and bubbles, that will be produced betwixt the air and quicksilver in their hasty passing by one another to the opposite end of the tube.

If any friend of Mr. *Hobbes*'s, seeing the manifest inconveniences of this opinion, shall on his behalf pretend, that it is what he calls the pure air, that passes through the body of the quicksilver to the deserted part of the glass-cane; the answer is ready, that Mr. *Hobbes*'s expressions look so much another way, that his readers (for aught I have found) do generally understand him of such common air as is displaced by the descent of the mercury. And therefore I had reason to argue against what he wrote, as I have newly done; and however, this assertion is clearly precarious, and liable to the objections formerly alleged against the passing of the air through the water. To which we may add this circumstance, that in our present case it must descend into a far heavier and closer liquor than water. But perhaps it will be thought, I have already said more than needed against an opinion, which has been rejected as well by Plenists as Vacuists; and though mentioned as to the main by several writers, as well before Mr. *Hobbes* asserted it as afterwards, has been thought so unlikely, as not to have been (that I know of) approved by any man, even before the discovery of the phænomena of our engine. Which last words I add, because that Mr. *Hobbes* not pretending, that any attraction intervenes in the case, I see not how he can possibly make out, to omit other phænomena, the descent of the mercury, in the tube further and further beneath its wonted station, upon the exhaustion of the receiver, and the reascension of the same mercury in the same tube, as we please to let in more or less of the outward air; without admitting as much of spring or pressure in the air, as I need contend to have here allowed me. The weight of the terrene particles, by which, at the end of the third exposition, he is reduced to endeavour the solution

solution of the quicksilver's falling lower at the top than at the bottom of a hill (for I am willing to think, that is his meaning, and that it is by the transcriber's fault rather than his, that he resolutely affirms the quite contrary) will by no means serve his turn; it being utterly improbable to imagine, that the air contained in so little a vessel as one of our receivers can by its weight counterbalance so ponderous a cylinder of quicksilver: whence we may be allowed to argue that the air sustains it by such a pressure or spring as we plead for, whether that proceed from the texture of the aërial particles, or from their motion, or from both.

The fourth and last of Mr. *Hobbes*'s principal expositions is of that experiment of ours, wherein 100 and odd pound weight being hung at the depressed sucker, the sucker was notwithstanding impelled up again by the air to the top of the cylinder. Of this phænomenon (which has not hitherto proved unwelcome to the virtuosi) Mr. *Hobbes* gives us the following account.

Hærent hic nostri: (which, why he says, I know not) *quomodo hæc expedies tu?* Page 22.

A. Expedivi ante. Aër enim à retractione suctoris retrò pulsus, nec locum in mundo (ut supponimus pleno) quò se recipiat inveniens, nisi quem ipse, corpora contigua suis locis pellens, sibi faceret, perpetuâ pulsione in cylindrum tandem cogitur, tantâ velocitate inter cylindri concavam & suctoris convexam superficiem, quanta respondere solet viribus illis magnis, quas ad suctorem revellendum necessarias experti estis. Aër autem ille, quâ velocitate ingreditur, eandem ingressus retinet, simulque latera cylindri ænei (vi elasticâ præditi) undiquaque distinet. Conatur ergo aër in cylindro vehementer motus contra omnes partes superficiei cylindri concavæ; frustra quidem dum suctor retrahitur: sed quamprimum suctor manu emissus aërem impellere cessat, aër ille, qui ante incussus erat, propter conatum in omne punctum superficiei cylindri internæ & vim aëris elasticam, insinuabit se inter easdem superficies eâdem velocitate, quâ impulsus fuerat, id est, eâ velocitate, quæ respondet viribus impulsionis. Si ergo tanta ponderis vis suctor inappendatur, quanta manuum vis erat, quâ impellebatur, velocitas, quâ idem aër è cylindro exit, locum in mundo pleno nullum habens quò se recipiat, suctorem rursus ad cylindri summitatem impellet, propter eandem causam quæ effecit, ut suctor paulo ante impulerit aërem.

'Here our men are at a stand: how will you expedite this difficulty?

'*A.* I have done it already: for the air being beaten back by the retraction of the 'sucker, and finding no place in the world (which we suppose to be full) where it 'might dispose itself, besides that, which by driving out other bodies from their places 'it may make for itself, is by perpetual pulsion at length forced in the cylinder with 'so great swiftness, between the concave surface of the cylinder and the convex sur'face of the sucker, as may answer that store of power, which you found necessary 'to the drawing back of the sucker. Now the air, with what swiftness it enters, re'tains the same within, and then distends every way the sides of the brass cylinder, 'which is *(of itself)* elastical. Therefore the air in the cylinder being vehemently 'moved, endeavours or thrusts against all parts of the concave surface of the cylin'der; but in vain, until the sucker is drawn back. But as soon as the sucker, 'having slipt the hand, ceases to make its impulse upon the air, that air, which was 'before driven in, by reason of its endeavour against every point of the internal 'superficies of the cylinder and of the elastical force of the air, will insinuate itself 'between the same surfaces with the same swiftness, as that by which it was impelled, 'that is, with that velocity, which answers the strength of the impulsion. If there'fore so great a power of weight be hung upon the sucker, as may answer the power 'of the hands by which it was impulsed; the swiftness, with which the same air goes 'out of the cylinder, finding no place in the world (which is full) where to dispose

'itself, will again impel the sucker to the top of the cylinder, for the same reason, 'that the sucker a little before made an impulse upon the air.' Thus far our author's passage; against whose solution it is easy to draw divers arguments, from what we have discoursed against the first of his four explications. But though we refer you thither, yet we will here also observe, that this whole conceit of the air's running in and out with strange velocity between the sucker and the cylinder is precarious; nor does he propose any one phænomenon to countenance it. To which general advertisement I shall add the three following particulars. First, that in an engine, so contrived, that the pump lay covered with water, when the sucker was retracted, the atmosphere would strongly press the water against it; and, if the manubrium were let go, would swiftly enough repel up the sucker into the deserted cavity of the cylinder. Which being a case parallel to that under consideration, let any unbiassed person judge, how likely it is, that the air could perform all these excursions without exciting bubbles, notwithstanding the water's constant interposition betwixt it and the cylinder. Secondly, that there is as little probability in what our author teaches in those words *conatur ergo aër*, &c. I might here repeat what we formerly mentioned of the breaking of our receivers inwards, not outwards; and I might add, that I see no reason, why the conatus of the included air, if its conatus were granted, should be frustraneous, when the sucker is depressed. But I will rather demand, why, if the air within have so strong an endeavour outwards, as to stretch the thick sides of the brass cylinder, as Mr. *Hobbes* (with what probability, let any man judge) would have it; I demand, I say, why this air does not throw out the wooden peg or valve, which we have often to our trouble seen thrown out with great force and noise, when the depreſt sucker being thrust up again, while there was air in the cylinder, we forgot to leave the valve open; though in this case the air, that drove out the peg, was far enough from stretching the cylinder. And I further demand, how it comes to pass, that, if having stopt the hole of the cylinder with your finger instead of the peg, you swiftly depress the sucker, you shall be so far from feeling a pressure outwards against the pulp of the finger from any thing contained in the cavity of the cylinder, that your finger will be strongly, and perhaps not without some pain, pressed in by the ambient air; in so much that it was this phænomenon, and one somewhat like it in the Torricellian experiment, that seem to have engaged my other adversary, the learned *Linus*, to maintain a conceit quite contrary to Mr. *Hobbes*'s, and imagine in the deserted cavity of the cylinder, not a distending, but violently contracting substance. Thirdly, that as to the last part of the passage under consideration, beginning at *si ergo*, &c. which seems to me somewhat intricate; I do not so clearly understand, why the air, that is impelled in so swiftly betwixt the cylinder and the sucker, should not resist the swift egress Mr. *Hobbes* ascribes to the included air by the same passage: nor why this impelled air, that has so strong an endeavour outward, should never depress the sucker (against whose upper part it must bear as well as against the cylinder) as well as the same air, diffusing its motion through the vast ambient medium, can enable the external air to thrust up the sucker again; especially, since during such a depression of the sucker (as we have mentioned not to happen) made by the rebound of the air, forcibly impelled in from the close bottom of the cylinder, the air from without may all the while, with congruity enough to Mr. *Hobbes*'s principles, get in between the said sucker and the cylinder. But not to insist upon these niceties; I say, that the lifting up of the sucker either is not necessary to prevent a vacuum; or that in some cases it will be hard to shew, how a vacuum can by Mr. *Hobbes* be avoided. For when the depreſt sucker is ready to be thrust up again, if

you hang a somewhat greater weight at it than an hundred and odd pounds, it shall not be lifted up at all. And I shall make this further improvement of the experiment under consideration, that whereas this progress and regression of the impulse of the external air cannot reasonably be supposed to be very lasting, you may, by a competent weight, detain the sucker depreſt, till the ambient air is as quiet as it uses to be; and yet if then you take off the overplus of weight, and perhaps a little more, in case the pump have been very stanch, the sucker and the great weight appended will, notwithstanding, be carried up; which it is no way likely it could be by the impulse of the outward air, which had time to decay and be confounded. And as for the inward air, beside that we have proved, that it has no such conatus outwards as Mr. *Hobbes* pretends, why should not that, were it granted, throw out the sucker rather than cause it to be impelled inward; it being no way likely, that in case some air should get out of the cavity of the cylinder, it could so move the outward air, as that the reflex of that impulse should make that free outward air bear more strongly against the outside of the sucker than the inside of the same sucker is preſt against by the included air, whose *impetus* is incomparably less diffused? But not to be thought a more nice and diligent opponent than the matter requires, I shall drive this discourse no further, but rather desire it may be observed in general, that whatever be resolved to be in the cavity of the cylinder when the sucker is depreſt, yet since it is manifest, that it is at least, in great part, devoid of common air; and since the sucker, with the appended weight may, if the instrument leak not, be impelled up, when, in all probability, those forced undulations of the air, that may be supposed to have been made by the sucker, have ceased; the Cartesians, Mr. *Hobbes*, and those others, that will not have recourse to the unintelligible attraction of some rarefied substance within, must ascribe so strange a phænomenon to the pressure of the air without. But I shall no further press this fourth objection, partly because it is added to the other three, only *ex abundanti*, and partly because this chapter is grown so long already.

I know indeed that after the exposition last recited out of Mr. *Hobbes*, he makes the Academian Dialogist confess, that the rest of the phænomena of our engine may also not uneasily be reduced to his principles. But perhaps they, that take notice of the variety of those phænomena we have set down in our treatise, will scarce be of his mind; and those, that have considered what has been discoursed in this chapter against his four principal explications, and what I am about to subjoin in the following part of this treatise, concerning divers other solutions that he gives, will perhaps be inclined to think, that others may be like these, without being therefore necessarily true.

CHAP. V.

In which divers scattered explications, and other passages in Mr. Hobbes's *Dialogue, are examined.*

I Proceed then to the fifth chapter, in which and the next I glean up and examine divers scattered passages, wherein he offers at somewhat, by way of argument, against some things we had delivered in our epistolical treatise: I say, what he offers by way of argument; for as to those passages, that do but either praise himself or disparage his adversaries, I have almost as little leisure as inclination to take notice of them, and do not much apprehend that the virtuosi (especially such as know us both),

both) will think what I write the less rational for being civil; or will let me suffer, in their opinions, for neglecting to trouble them, in a philosophical controversy, with matters, that do but very little belong to it.

To skip then what Mr. *Hobbes* is pleased to say, in the first page of his Dialogue, concerning some disputable discoveries about sensation, which he challenges to himself; and to pass by divers other things in the second or third following pages, which relate to him, or to the society he writes against, rather than to the nature of the air; we should begin with the opinion he thinks fit, in the fifth page, to impute to us, as if we distinguished what is fluid from what is not so, only (for so his ratiocination imports) by the bigness of the parts of which a body consists: but designing, in an Appendix to be subjoined to this discourse, to examine what I find in this Dialogue dispersed touching fluidity, I shall now only say, that he does very much mistake and misrepresent my doctrine of fluidity; wherein I expressly teach, that the principal cause or condition of it is not the size, but the motion of the small parts that compose the fluid body.

To take up then the particulars we are to examine, in the order (as far as conveniently may be) wherein I find them lie in the author's Dialogue, and passing by at present those things, which either we have considered already, or are not to consider in this place; the first particular, that offers itself to be taken notice of, is this passage at the bottom of the twelfth page:

Page 12. B. *In vas apertum infudimus aquam, in aqua fistulam statuimus erectam, longam, exilissimam; observavimus autem aquam è vase subjecto in erectam fistulam ascendisse.*

A. *Nec mirum; nam superficiem aquæ, particulæ aeri interspersæ aquæque contiguæ motu suo verberabant, ita ut aqua non potuit in fistulam non ascendere, & sensibiliter quidem in fistulam valde angustam.*

'*B.* We poured water into an open vessel; we placed in the water a long streight, 'slender tube; and we observed, that the water did ascend from the vessel underneath into the erected tube.

'*A.* No wonder: for the small particles, that are interspersed in the air near the 'water, did, by their motion, beat upon the surface of the water, so that the water 'must of necessity ascend into the pipe, and that sensibly into a pipe, that was so 'exceeding slender.'

To this I say, that it is manifest, by what I write in my epistle, that I did not then take upon me, nor do I undertake in this place, to assign the true reason of the proposed phænomenon. An attempt of this kind has been since addressed to me, which being ingenious, if not also true, may be consulted. In the mean time I cannot but declare, that I am no way satisfied with Mr. *Hobbes*'s exposition: for, to say nothing of the motion he ascribes to the particles dispersed through the air, he leaves the difficulty unsolved, since there being common air as well within the cavity of the slender pipes as without it, he neither shews, nor so much as offers at, a reason, why the pressure of the air within should not resist the pressure of the same kind of air without; as we see it does in greater pipes. And possibly he would have past by this particular, if he had not overlooked the advertisement I gave towards the close of the 35th experiment; that it would concern those, who should undertake to shew the causes of this phænomenon, to bethink themselves also of a reason, why, if the experiment be tried with quicksilver instead of water, the surface of the liquor will, instead of being higher, be lower within the pipe than without it. Whereas if Mr. *Hobbes*'s explication be sufficient, why should not the contrary happen in quicksilver as well as in water?

THE

The next passage I have to consider is in the 13th page, thus set down: *Siquis post impulsionem revulsionemque suctoris aliquoties repetitam, epistomium superni orificii recipientis conetur extrahere, inveniet illud valde gravitare, tanquam si multarum librarum pondus ab eo penderet. Unde contingit hoc?* Page 13, 14.

A. *Ab aëris, qui est in recipiente fortissimo conatu circulari facto à violento ingressu aëris inter superficiem suctoris convexam & cylindri concavam, generato per iteratam illam impulsionem revulsionemque suctoris, quam vos perperam vocatis exsuctionem aëris. Nam propter naturæ plenitudinem, epistomium extrahi non potest, quin aër qui est in recipiente (epistomio contiguus) una extrahendus sit. Qui quidem aër, si quiesceret, facillimè epistomium sequeretur: sed dum velocissimè circuit, satis difficulter sequitur; id est, videtur esse valde gravis.*

B. *Verisimile est: nam ut aër novus in recipiens paulatim admittitur, etiam apparentem illam gravitatem paulatim perdit.*

'If any one after the frequently-repeated impulse and retraction of the sucker, 'endeavour to draw out the stopple of the upper orifice of the receiver, he shall find 'it gravitates very much, as if a weight of many pounds hung upon it. Whence 'comes this?

'*A.* From a strong circular endeavour of the air within the receiver, made by the 'violent ingress of the air between the convex surface of the sucker and the concave 'of the cylinder, procured by the repeated impulse and revulsion of the sucker, 'which you improperly call the exsuction of the air. For by reason of the fulness 'of nature the stopple cannot be drawn out, but the air that is in the receiver (con-'tiguous to the stopple) must be drawn out too: which air, if it were settled and at 'rest, the stopple would easily be drawn out; but while that does most swiftly circu-'late, it comes out very hardly, that is, it seems to be very heavy.

'*B.* Very likely: for as soon as fresh air is by degrees let into the receiver, it like-'wise by degrees loses this seeming gravity.'

But, I do not much fear, that this explication will keep the experiment from continuing to be thought, by ingenious men, a notable confirmation of our hypothesis. For, to pass by something, that, though I am no way satisfied with, cannot well be examined in few words; I answer, first, that if there be such a vehement circular endeavour, as he imagines, of the air in the receiver, by which motion he elsewhere teaches (as we have seen above) that the air rushes out with violence enough to make the atmosphere lift up in our cylinder above an hundred pound weight; I see not, why it should not rather throw out the stopple under consideration, than hinder its extraction. And I see not why, when the external air is readmitted at the stop-cock into the exhausted receiver, and thereupon there does sensibly follow for a little while a whirling about of the included air, the stopple, that just before seemed so much to resist the being drawn out, should cease to make any such resistance. Nor do I see, how the plenitude of nature should, as is here intimated, hinder the extraction of the stopple; for, according to the Plenists, the world and the receiver must be at all times equally full. And if the contiguous air must for Mr. *Hobbes*'s reason necessarily be extracted with the stopple in one case, I see not, why the like should not happen in another. But since Mr. *Hobbes* is pleased to call us experimentarian philosophers, let us shew, that such explications as these of his need not make us ashamed of the name. I say then, that it appears by our experiments, that there is no such *fortissimus conatus circularis* in the exhausted receiver as he pretends; but that there is indeed an endeavour of the ambient atmosphere to press inwards the parts of the glass and cover, that are contiguous to it. For, as I have also noted already, a light bladder suspended in

in the cavity of the receiver betrayed no such motion as Mr. *Hobbes* here supposes. To which I shall now add, that neither were a pair of scales suspended within the same cavity; nor was a long magnetical needle, that rested upon the point of another needle, at all whirled about by this imaginary motion of the air. Besides, if you leisurely loosen the brass stopple, so that it may be very near, but not contiguous, to the sides of the socket, you shall manifestly perceive a strong current of air to flow into the receiver at that passage: and more than once, when instead of that piece of brass we stopt the hole in the cover with our cement, one might observe, sometimes whilst we were pumping, sometimes after we had done pumping, that the outward air by degrees depressed the superficies of the cement exposed to it, and made it concave, and now and then would break through it, thrusting it inward with great violence and noise.

IN the same page our author rectifies, after his way, another of our explications in these words; *Vidimus item aquam demissam in recipiens post suctoris aliquot reciprocationes ita bullire, ac si supposito igne fervesceret.* Page 13.

A. *Id quoque accidit propter velocitatem aëris, ut dictum est, in recipiente circumeuntis; nisi forte aquam illam dum bullit calidam quoque esse deprehendatis. Nam si certi essemus illam calescere, alia causa phænomeni excogitanda esset.*

B. *Imo certi sumus, quod non calescit sensibiliter.*

A. *Quid ergo tali aquæ motui conferre posse putas majorem vel minorem atmosphæræ gravitatem?*

B. *Neque illum motum attribuunt, puto, atmosphæræ.*

'We saw also water, being let down into the receiver, after some returns of the 'motion of the sucker, to bubble so as if it had boiled over a fire.

'*A.* This likewise happens, as we spake, by reason of the swiftness of the cir-'culating air; unless perhaps you find the water hot too whilst it bubbles. For if 'we were sure it was hot, we must find out some other cause of the phænomenon.

'*B.* We are certain it is not sensibly hot.

'*A.* In what therefore can the greater or lesser motion of the atmosphere promote 'such a motion as this?

'*B.* I suppose they do not attribute this motion to the atmosphere.'

BUT, I confess, I see not, how the circular motion of the air within the receiver could in a phial with a long neck produce such effects as in my epistle are recited; especially I see not, how such a wind passing along the surface of the water could raise so many and so strangely big bubbles, which seemed many of them to rise from the lower parts of the water, and swelled notably as they ascended; and how such a wind should carry up the most part of the water through the long neck of the phial, and as it were spout into the receiver.

As for what he says about the gravity of the atmosphere, it is plain enough, that my conjecture ascribes the phænomenon to the taking off, not the gravity of the external air, but of the pressure of the air within the receiver; though I see not why the removal of the weight of the atmosphere, if it could be out of the engine effected, should not have a like operation.

AND (to answer Mr. *Hobbes*'s question as it should have been put) that, which I think the greater or lesser pressure of the air confers to this phænomenon is this; that whereas common experience shews us, that water by being heated is expanded, and has bubbles generated in it; and whereas our former experiments, especially the 28th, have made it appear, that there is wont to be in water and other liquors aërial particles, which tend to expand themselves, and do actually do so, in numerous bubbles,

bubbles, when the pressure of the incumbent air is considerably lessened: in the present phænomenon, that pressure being, by the exhaustion of the receiver, taken off, the aërial particles and agitated vapours, that abound in the hot water, are allowed to expand themselves, as before they could not, and to make such numerous and great bubbles, that thereby a good part of the water is carried out of the phial. So that I somewhat wonder, what makes Mr. *Hobbes* speak, as if there were no sensible heat of the water under consideration, since it is expressly said, that it was put in hot; and if it were put in cold, could by no pumping be brought to the least shew of effervescence. And as for his explication of the phænomenon, the experimentarian philosophers need not the objection lately made against it; for I have already evinced by experiments, that there is in our exhausted receivers no such peculiar motion of the air as he ascribes the phænomenon to; nay, when there is manifestly a whirling about the air in the glass upon the admission of the external air, the production of numerous bubbles in the water presently ceaseth. And therefore I see not, why Mr. *Hobbes* might not have let alone my conjecture (for I proposed it, and look upon it, as no more) unless he could either have disproved it better, or substituted a more probable one than he has in its place.

As for what he adds in these words, *Ab hoc experimento manifestum est, quod recipiens per exuctionem hanc quam vocatis aëris, non sit vacuum. Nam moveri aqua non potuit nisi à movente aliquo moto & contiguo. Itaque phænomenum hoc demonstrationem suppositionis meæ continere videtur non infirmam:* 'It is manifest from this experiment, that the 'receiver is not made empty by this exsuction of air, as you call it: for the water 'could not be moved but by some contiguous mover, that was itself in motion: 'therefore this phænomenon seems to contain no weak demonstration of my hypo-'thesis:' I am not obliged to answer it, but leave that to those, that are profest Vacuists; against whom I must doubt whether his ratiocination will conclude, though the consequence be not manifest to me. For himself allows his terrestrial atoms an innate circular motion, which consequently needs not depend upon some body contiguous and moved; and the Vacuists will say, that the particles of the water being strongly agitated, when it was put into the receiver (whether by fiery corpuscles swarming in it, or otherwise) and the resistance of the incumbent air being taken off, the phænomenon would be produced just as it is, though we should suppose no other body to succeed in the room of the exhausted air. And besides, though some subtile particles of active matter should get in, to agitate the aqueous and aërial corpuscles, yet (they may say) there is no necessity, that such minute particles should be numerous enough to fill up exactly all the little spaces deserted by the air. And even upon this supposition, as it would not follow, that such relinquished spaces were all of them quite empty; so would not the phænomenon at all prove, much less manifestly prove, that they were quite full. And since an actual heat, that is, a brisk and various agitation of its small parts, is requisite to the boiling of the water in this experiment; perhaps others will not think it more absurd, that the removal of the pressure of the air should occasion this expansive motion in the water, than that, which Mr. *Hobbes* must allow, that in air compressed by quicksilver, or some other weight, the removal of that pressure is sufficient to make that air expand itself by the flying abroad of its parts. Page 3.

And whereas Mr. *Hobbes* urges this other argument against the Vacuists, *Præterea, dic mihi, bullientem aquam potuistin' conspicere?* Page 13, 14.

B. *Quidni?*

A. *Nonne visionem fieri concedunt vestri per actionem continuam ab objecto ad oculum? Nonne etiam putant actionem esse motum, & omnem motum esse corporis? Quomodo ergo*

potuit ab objecto, nempe aqua, ad oculos tuos motus per vacuum (id est, per non corpus) derivare?

B. *Non affirmant nostri ita vacuum esse recipiens, ut nullus omnino aër relictus sit.*

A. *Nil refert an totum recipiens vacuum sit, an magna ejus pars; nam utrumvis supponatur, derivatio motus ab objecto ad oculum intercipietur.*

B. *Ita videtur, nec habeo quod respondeam.*

'Besides, tell me, could you see the water bubbling in that manner?

'*B.* What else?

'*A.* Do not your associates grant, that vision is made by a continued action from 'the object unto the eye? Do they not also think action to be motion, and all motion to be of some body? How therefore could the motion be derived from the 'object, the water, unto your eyes through a vacuum, that is somewhat, that is not 'a body?

'*B.* Our friends do not affirm the recipient to be so empty, that no air at all 'is left.

'*A.* No matter, whether the receiver be wholly, or for the greater part empty; 'for which ever you suppose, the derivation of the motion from the object to the eye 'will be intercepted.

'*B.* It may be so; I cannot tell what to answer.'

The Vacuists will perhaps answer him, as I answered *Franciscus Linus* to an almost like objection. And those of them (which make far the greatest number) who plead but for an interspersed vacuum, will perhaps tell him, that they take vision to be made, not by such a propagation of impulse as he does, but by a trajection of effluvia, that issuing out of the sun, and traversing the diaphanous bodies interposed, rebound from the object to the eye. And according to this doctrine they may ask Mr. *Hobbes*, why a motion may not be made through a vacuum or *non corpus?* Nay, how it can naturally be stopt *in vacuo*, where there is nothing to resist it? But controversies of this nature it lies not upon me to prosecute.

IN the 14th page, Mr. *Hobbes* having recited that experiment of ours, of killing animals included in our receiver by the exhaustion of the air in two or three minutes of an hour, subjoins these words; *Credin' tu animalia ista tam cito interempta esse eo quod carerent aëre? Quomodo ergo sub aquam vivunt urinatores, quorum aliqui (assueti à pueritiâ) caruere aëre per horam integram? Inclusa in recipiente animalia occidit motus ille idem vehementissimus, quo distenduntur rumpuntur que inclusæ vesicæ.* 'Do you think 'these animals were therefore so quickly killed, because they wanted air? How then 'do they, who make a trade of diving, live under water, of whom there be some, 'who being accustomed from their childhood, have wanted air a whole hour? No, 'that most vehement motion, by which bladders shut therein are distended and 'broken, kills these animals shut up in the receiver.' But, though he says no more in this place concerning this matter, yet it seems he either much liked his own conjectures, or greatly disliked mine, since in his epistle dedicatory to the learned *Sorberius* he singles out this sole phænomenon to explicate; *Ego contra* (says he there) *neque aërem exugi posse, neque inclusum animal (etsi exuctus esset) tam cito moriturum esse existimo. Actio quidem, quam mors illa sequitur, videri potest vel suctio quædam & propterea exsuctione conclusi aëris interfici animal, respiratione sublatâ) vel etiam compulsio aëris ab omni parte versus centrum sphæræ, cui animal includitur; & sic videri potest mori à tenacitate compressi aëris, quasi aqua suffocatum; nimirum haustum in intima pulmonum aërem solito tenaciorem, inter arteriam & venam pulmonis, cursum sanguinis intercipiendo sistere.* 'I, 'on the contrary, think, that neither the air can be sucked out, nor that the animal 'would so soon die, if it were sucked out. The action indeed, to which this death

Page 14.

'is

'is a consequent, may seem either a certain suction (and so, that the animal is killed 'by the exsuction of the included air, its respiration being taken away) or a compul-'sion of the air from all parts towards the center of the spherical glass, in which the 'animal is inclosed, and so may be seen to die stifled by the tenacity of the com-'pressed air, as it were, with water; the air more tenacious than usual being drawn 'into the inwards of the lungs, and there, between the pulmonary artery and vein, 'stopping the course of the blood.' But, as I proposed by conjecture doubtingly, and profess myself to be in a further inquiry about the use of the air and of respiration; so I must still think, that we want some further or clearer discovery about that matter, notwithstanding what has been delivered concerning it by Mr. *Hobbes*. For his argument against my conjecture is, in the passage that proposes it, answered by himself: for he plainly intimates, that divers, who can live without air (which yet I might question, if he means without any air at all) for a whole hour, are accustomed to it from their childhood. Wherefore, unless the animals, that died in my engine, had been for a long time framed by degrees to live without air, it will not follow, that the want of it could not dispatch them in a short time, as ordinary men may be drowned in a few minutes. And having purposely let down some mice and small birds into a deep glass filled with water, and kept them from emerging by a weight tied to their legs or tails, though some lived longer than others, yet I observed them to be killed fast enough to keep my conjecture from being incredible: especially the last we made trial of, though a large and lusty mouse, appeared to be quite dead within somewhat less than one minute, measured by the vibrations of a pendulum. And we particularly took notice, that, before drowning, divers bubbles, which seemed to be the respired air, came out of their mouths, and ascended through the water.

And as for the explication, that Mr. *Hobbes* would recommend instead of mine, not to urge, that I could wish he had been pleased to shew us, how the tenacious air he imagines to be inspired comes to produce those strange convulsions, and other symptoms mentioned in my epistle; not to urge this, I say, we have already disproved the supposition his opinion is built on, namely, that there is in the exhausted receiver such a *motus vehementissimus*, as he pretends: besides that he shews not how this motion comes to kill the included animals, which I was wont to keep, not near the center of the receiver, where he seems to think this motion most operative, but near the bottom of it, that the included animal might have something firm under his feet. Nor does it at all clear the difficulty, that he would have this motion the same, whereby included bladders are distended and broken. For, besides that it is very hard to conceive, how the tenacity of the air, or its beating from all parts upon the convex surface of an almost quite empty bladder (for in such also the experiment he refers to will succeed) should make it burst outwards; besides this (I say) we have already proved, that the distension, and breaking of bladders in our receiver, proceeds not from any such motion of the neighbouring air as is here presumed, but from a quite differing, if not from a contrary cause.

In the same page our author makes a digression from the engine, and discourses of another experiment, which I have long since often made. But though his explication be liable enough to just exceptions (as I can make good, if it be required) yet because the experiment is none of those I delivered, I shall leave it to be examined by others. And for the same reason I forbear to meddle with that he has in the next page concerning the wind-gun. As to what he has in the same 16th page, in these words; *Placet mihi tua magis hypothesis quam illa de vi aëris elastica: nam video, quod à veritate* Page 16.

illius veritas dependet vel vacui vel pleni; sed à veritate hujus nihil sequitur in neutram partem quæstionis. Aëris, inquit, structura similis est compressæ lanæ. Bene est. Lana fit ex filis. Recte. Sed cujus figuræ? Si parallelopipedi, nulla potest esse compressio partium: si non parallelopipedi, erunt inter fila illa spatia quædam relicta; quæ, si vacua sunt, supponunt vacuum, ad probandum, quod vacuum est possibile; si plena, plenum dicunt quod vacuum putant: 'Your hypothesis pleases me better than that of the spring of 'the air: for from its truth depends the truth of a vacuum or a plenum; but from 'the truth of that, nothing follows on either part of the question. The make of the 'air (says he) is like that of compressed wool. Well; wool is made of hairs or 'threads. Right; but of what figure? If of a parallelopipedon, there can be no 'compression of parts; if not of a parallelopipedon, there will be betwixt the hairs 'certain spaces left, which if they be empty, they suppose some place empty, to 'prove that a vacuum is possible; if full, they say that is full, which they suppose 'to be empty:' To this passage, I say, I cannot but represent, that the question is not, whether from the hypothesis, that ascribes a spring to the air, depends the proof of a vacuum or a plenum, but whether the hypothesis itself be true or no. For, sure there are many things certain in natural philosophy, from whose truth that of a plenum or a vacuum cannot be deduced. And to what he adds concerning the structure of the aërial particles, the Vacuists may tell him, that they make no such argument as he is pleased to make for them; and do not commonly employ the figure of the aërial particles to prove a vacuum, but other arguments, such as Mr. *Hobbes* has not yet well answered: and having by them, as they judge, proved interspersed vacuities, they might without inconvenience suppose in an aërial corpuscle little empty pores, upon whose account it may be capable of compression, in case they should think fit (which I know not that any of them does) to assign it the figure of a parallelopipedon. But this controversy the Vacuists may, if they please, prosecute. In the same page Mr. *Hobbes* begins, and in the next he continues, a long discourse concerning the going out of fire in our receiver upon the exhaustion of the air: the passage is too prolix, and does too little concern the spring of the air to be here totally transcribed, or examined period by period. In sum, he endeavours to do two things: the one is to reduce what happens to kindled coals placed in our engine to what happens in certain mines, wherein when some thick damps ascend, both charcoals and candles are soon extinguished thereby: the other is to shew, that by the reciprocation of the sucker, the air impelled first into the cylinder, and then into the receiver, is put into such a motion, as gives it a certain middle consistence, as he speaks, betwixt the consistence of pure air and that of water. But I shall not need to examine this second part of his discourse, because I deny the first; and being able to disprove the thing itself, namely, the thickness of the air in the exhausted receiver, I need not spend time about what he teaches *de modo*.

To examine then only the first of the abovementioned particulars, I shall begin with observing, that his story of the damp to be met with in mines is more largely set down by Mr. *Hobbes* in that chapter of his Elements of Philosophy, where he treats of gravity; in which place he seems to mention it (to use his own expression) as a story of doubtful credit, which it is not like he would have done, if he had then seen it. Which I mention, not that I deny the story for the main, nor that I would bring Mr. *Hobbes* into a suspicion of relating things untrue as matters of fact, his enemies themselves having not accused him of such a meanness; but because, if he have not since observed the thing himself, there may easily be a mistake in some of the circumstances; as for instance, the number of minutes, wherein the thick air choaks the

the fire: and it is upon that circumstance, that the validity of what he deduces from the observation chiefly depends. But, however the matter fare with these subterraneal damps, we have already proved by several of the experiments of our engine, that in the exhausted receiver there is no such motion of the air as is here supposed. And it may be sufficiently proved, that whatever remains in the receiver is not such a substance as Mr. *Hobbes* would have it; for that, he here tells us, is of a consistence betwixt air and water; and in the above-cited place of his Elements he says, that it is not much lighter than water. But by the Magdeburgick experiment (we have already had occasion to mention two or three times) it is evident, that the receiver, by being exhausted of common air, is so far from growing heavier, much less so much heavier as it must, if it were filled with a substance not much lighter than water, that it lost above an ounce of its former weight. And to this agrees what we see happen in æolipiles, that grow lighter, when the air is expelled. Besides, if the receiver be in our present case filled with a substance, whose consistence is so much nearer that of water than is our common air, as Mr. *Hobbes* would have it; how chance a pendulum should not move very sensibly slower in it, when in water the diadromes are so exceedingly much more slow? And the breaking of an hermetically-sealed bubble in our receiver outwards, when the air was much exhausted, and not before, together with divers other experiments that might be easily applied to this purpose, in our epistle, do sufficiently evince, that it is not a thicker and far heavier air, but a more yielding and lighter, that remains after pumping in the cavity of our receiver.

And thus much as to Mr. *Hobbes*'s discourse upon our experiment. But as for the thing itself, it appears, that when I related it, I thought it might admit a further inquiry: and indeed there may be so many ways of extinguishing fire (as we see that the flame of a candle may be blown out by the wind, or quenched in water, or put out by the compression of a pair of snuffers, or suffocated for want of air to receive its fuliginous steams, or (if that be a different way) stifled by the thick exhalations of deep mines or of new wine) that as it is not in all cases so easy to assign the true cause of the extinction of fire; so it is unsafe to conclude with Mr. *Hobbes*, that because a candle or a live-coal may, a great way under ground, be extinguished by a thick damp, therefore the effect must proceed from the like cause in our receiver, where there is no sign of any damp or unusual thickness of the air, but of the contrary.

But let us follow Mr. *Hobbes* to the next passage, wherein he seems fond enough of playing the censor. For, speaking of our 11th experiment, wherein the coals, that seemed almost dead in our exhausted receiver, being taken out into the air began to shine again, having made his academian dialogist say, *Fuere eorum aliqui, qui remansisse dixerunt in carbonibus illis (quanquam extincti videbantur) particulas quasdam igneas, quæ, admisso aëre, ventilatæ cæteram molem denuo accenderent:* 'There were 'some of them, that said there remained in those coals (though they seemed extin- 'guished) some fiery particles, which, being blowed up by the air, upon its admission 'did rekindle the rest of the mass.' The other (by whom Mr. *Hobbes* is meant) answers him, *Næ illi quæ dicerent non videntur cogitasse, sed sortitos esse.* 'In good 'faith they seem not so much to have considered what they should speak, as to have 'taken it up at all adventures.' This is very severe. But let us see what weighty reason he has to be so: *Credin' tu in carbone ignito partem aliquam non carbonem, sed ignem esse; aut in caudente ferro partem inesse, quod ferrum non sit, sed ignis?* 'Do you 'believe, that in a kindled coal, there is any part, which is not a coal, but fire; or 'in a red-hot iron, there is any part, that is not iron, but fire?' But some will think, that by these words, he does rather propose than prove his opinion: wherefore let us hear

Page 181.
Page 182.

Page 18. hear his ratiocination, which he annexes in the following words: *Ab unica scintilla magnæ urbis incendium nasci potest. Atqui si ignis corpus ab ignito diversum sit, non plures potuere esse partes igneæ in toto incendio, quam in una illa scintillula. Videmus corpora diversorum generum à luce solis tam per refractionem quam reflectionem factam in speculis comburentibus accendi posse: neque tamen quenquam esse credo, qui putet particulas igneas à sole ejectas transire posse per substantiam globuli crystallini. In aëre intermedio ignis nullus est.* 'A great city may be set on fire by one spark: now, if the body of fire be different 'from the thing fired, there can be no more parts of fire in the whole town on fire, 'than that one spark. We see bodies of divers kinds may be set on fire by the light 'of the sun, as well by the refraction as the reflection, that is made in burning-'glasses. And yet I do not believe, that there is any man thinks, that particles of 'fire darted from the sun can pass through the substance of a crystal globe. And in 'the air, between the sun and the globe there is no fire.' But I doubt his adversaries will say, that he is so far from having in this passage well confuted their opinions, that he seems not to have well understood them: for they will tell him, that they teach not, that the fire is materially different from every part of the fewel; but that the igneous corpuscles, whilst they are divided, blended and oppreſt with the others, have not the power to shine or burn, till being by some spark or other body actually burning, or by some other equivalent cause extricated, they flock together in swarms, and then are able to burn and shine, that is, to appear fire: which fire is yet but a part of the fewel; as appears by the phlegm, ashes, and perhaps other incombustible parts of the coal or other fewel. So that the Atomists and divers others will not allow, what Mr. *Hobbes* infers about an *incendium*. And whereas he tells us he believes, that no body thinks *particulas igneas à sole ejectas transire posse per substantiam globuli crystallini*; he seems to me to have very little heeded the Epicurean hypothesis. For not only the learned *Gassendus*, but I know not how many other Atomists (besides other Naturalists) ancient and modern, expressly teach the sun-beams to consist of fiery corpuscles, trajected through the air, and capable of passing through glass; whereby these authors give an account of those *specula ustoria*, that burn by reflexion. These things I represent, not that I intend here to adopt the Atomists opinion of the nature of fire, of which I am not obliged to declare my thoughts here, and have done it elsewhere; but to shew, that Mr. *Hobbes*'s arguments are not a sufficient ground for so heavy a censure. And if a coal be kindled at one end, though Mr. *Hobbes* would have the kindled end a coal, not fire; yet if he please to hold it in his naked hand, he may find that differs enough from the other end to deserve another name. And I, that but related a phænomenon, did not perhaps express myself much less warily, if not more so, than Mr. *Hobbes* himself. For whereas my words are these,
Page 32. *We presently took out the coals, in which it seems there had remained some little parcels of fire, rather covered than totally quenched; for in the open air the coals began to be rekindled in several places:* Mr. *Hobbes* even in his elements of philosophy speaks thus
Chap. 30. sect. 14. upon a somewhat like occasion; *If a grate filled with coals thoroughly kindled, and burning never so brightly, be let down, as soon as ever it is below C. the fire will begin to grow pale, and shortly after (losing its light) be extinguished, no otherwise than if it were quenched in water: but if the grate be drawn up again presently, while the coals are still very hot, the fire will by little and little be kindled again, and shine as before.*

Page 18. As for the reason Mr. *Hobbes* assigns of our experiment in the lately mentioned passage of his dialogue, being grounded upon such a thickness of the air in the receiver as we have already disproved, it needs not to be examined. And lastly, as to
Page 18. what he subjoins in these words, *Quando autem est, quod de homine vere pronunciare possumus*

possumus quod est mortuus, sive (quod idem est) animam expiravit. Cognitum enim est homines nonnullos pro mortuis habitos postridie elatos revixisse.

A. *De puncto temporis, quo anima à corpore separatur, difficile est statuere. Perge igitur ad experimenta alia.*

'When is it, that we may truly say of a man that he is dead, or (which is the 'same) hath expired his soul? For it has been known, that some men, who have 'been taken for dead, being brought out the next day, revived?

'*A.* It is hard to determine the point of time, in which the soul is separated from 'the body. Proceed therefore to other experiments.'

I confess I see not why that needless question might not have been well spared, if he designed to give it no better answer.

CHAP. VI.

Wherein other passages of Mr. Hobbes'*s Dialogue, that concern the author, are examined.*

WHAT our author has in the 19th page concerning a bladder, has been already examined: wherefore I proceed to the next passage in the same page, which is this:

B. Si acus magnete excitus libere pendeat intra recipiens, sequetur tamen ille motum ferri quod circumducitur extra recipiens. Item objecta intus posita ab iis qui extra sunt videbuntur, & soni intus facti audientur, omnia hæc æque post atque ante exuctionem aëris, nisi quod soni sunt aliquanto post quàm ante debiliores.

A. Manifestissima hæc sunt signa recipientis semper pleni, nec posse inde exugi aërem. Quod autem soni inde sentiantur debiliores, signum est consistentiæ aëris. Consistentia autem aëris à motu ejus est per lineas diametraliter oppositas.

'*B.* If a needle excited by a loadstone hang freely within the receiver, it will ne-'vertheless follow the motion of the iron, which is drawn about without the receiver. 'So objects put within will be seen by those that are without, and sounds made within 'will be heard without: all these as well after as before the exsuction of the air, ex-'cept that the sounds are somewhat more weakly heard after than before.

'*A.* These are most manifest signs, that the receiver is always full, and that the 'air cannot thence be sucked out. That the sounds thence are more weak to one's 'hearing is a sign of the consistence of the air; for the consistence of the air is dia-'metrically opposite from its motion.' But I meet with few of the Vacuists, who, even in the Torricellian experiment, think the place relinquished by the quicksilver to be perfectly void, most of them allowing, that though it be not quite full of body, yet it may contain some of the earth's magnetical steams, or of those igneous corpuscles that flow from the sun, or both of them. Now against these, who would from our experiments deduce but only an interspersed vacuum, I see not, that the phænomena mentioned by Mr. *Hobbes* do conclude half so manifestly as he pretends: for, as to the motion of the needle within the receiver, it is known, that they are wont to ascribe magnetical attraction to certain effluvia, that issuing out of the loadstone are subtile enough to pass through the pores of the closest bodies without excepting glass; so that although the receiver were quite emptied before, the needle might be wrought upon by magnetical corpuscles, that need not be supposed to fill the tenth part of the receiver. I know indeed, that Mr. *Hobbes* has another hypothesis of the phænomena of the loadstone; but I know, that divers learned writers have

have absolutely rejected it, and not one such, that I have heard of, has approved it. And as for the other two phænomena here mentioned by Mr. *Hobbes*, the Atomists may answer, that the first (touching objects seen in the receiver) has been shewn already not to overthrow their doctrine; and that the other (concerning the debilitation of sounds) makes against him, not for him; since we have already disproved, that consistence of the air, whereto he ascribes it. And the same arguments, that overthrow that opinion, may make it seem somewhat strange, that he should subjoin our experiment of two like pendula, whose vibrations we found not manifestly to differ within and without our exhausted receiver. For the former should move far slower than the other, according to Mr. *Hobbes*'s conceit, that the receiver, when we say it is exhausted, is filled with a substance of a middle consistence betwixt pure air and water, and not much lighter than water *. But whether the receiver be in such cases adequately full or no, the Vacuists may further consider. For its being granted to be full would not overthrow either of my hypothesis, namely, the weight and spring of the air.

IN the same 19th, and some following pages, Mr. *Hobbes* has a long discourse against my conjecture at the reason I propose in my 31st experiment, why (as I here express it) if the exquisitely polished surfaces of two flat pieces of marble be so congruous to each other, that upon their mutual application there results an immediate contact, they will stick so fast together, that he, that lifts up the uppermost, shall, if the undermost be not exceeding heavy, lift up that too, and sustain it aloft in the
Page 6. free air. The conjecture itself is in the same page thus set down; 'That the lower 'superficies of that (undermost) stone being freely exposed to the air, is pressed upon 'by it; whereas the uppermost surface being contiguous to the superior stone, is 'thereby defended from the pressure of the air, which consequently pressing the 'lower stone against the upper, hinders it from falling, as we have elsewhere more 'fully declared.' Which last words I therefore omit not, because they shew, that I handle this matter in this place but incidentally, and may make use of what I have delivered, where I treat of it more expressly; as I have since done in print in the *History of Fluidity and Firmness*, which Mr. *Hobbes* appears to have seen by those censures of some passages of it, that I shall hereafter examine.

HIS whole discourse concerning my conjecture, and his scheme, would be too prolix to be entirely inserted. But the thing his discourse drives at is to shew, that neither the spring nor weight of the air have any thing to do with this phænomenon: and therefore when he had made his academian relate, that two coherent marbles suspended in our receiver did not fall asunder upon the exhaustion of it, he subjoins
Page 10. that it was, *quia nihil istic erat, quod ageret atmosphæræ pondus*; and annexes, *Experimento hoc excogitari contra opinionem eorum, qui vacuum asserunt, aliud argumentum fortius aut evidentius non potuit. Nam si duorum cohærentium alterutrum secundum eam viam in qua jacent ipsæ contiguæ superficies propulsum esset, facile separarentur, aëre proximo in locum relictum successivè semper influente; sed illa ita divellere, ut simul totum amitterent contactum, impossibile est, mundo pleno. Oporteret enim, aut motum fieri ab uno termino ad alium in instante, aut duo corpora eodem tempore in eodem esse loco: quorum utrumvis dicere est absurdum.* 'Because there was nothing there, that the 'weight of the atmosphere should do; no more strong or evident argument could be 'made against a vacuum than this experiment. For if of two coherent marbles 'either of them should be thrust forward that way, that their surfaces lie contiguous,

* Compare that in the 30. Chap. 14. Sect. of his Physics, with that of his Dialogue, page 15, 16.

'they would easily be severed; the neighbouring air successively flowing into the de-
'serted place. But so to pull them asunder, that at one time they should lose their
'whole contact, is impossible, the world being full. For then either motion must be
'made from one term to another in an instant, or two bodies at the same time must
'be in the same place: to say either of which, is absurd.' But how this should be so cogent and manifest an argument against the Vacuists, I confess I do not well discern. For that, which it proves (if it prove any thing) seems to be, that in case the cohering marbles could be so severed, as to lose at once their whole contact, the world might be concluded not to be full: but I see not how it thence follows, that therefore there can be no vacuum. For my part, I would demand, whether the so strong cohesion of the marbles be necessary or not to the plenitude of the world. If it be, how chance a sufficient weight hung to the lower marble can immediately draw them asunder? And if it be not, why does not Mr. *Hobbes* assign some other cause of their so strong adhesion, if it depend neither upon the spring nor weight of the air? As for the non-separation of the two marbles in our receiver, I have said, in the same 31st experiment, that the cause may probably be the pressure of the air remaining in the receiver not sufficiently exhausted. And this conjecture I have more fully defended in what I have written about it against *Franciscus Linus*; where I shew, that it is no way unlikely, the remaining air should be able to sustain a weight of four or five ounces hanging at the lower marble, since the free air was able to support between 400 and 500 ounces hanging at the same.

But Mr. *Hobbes* tells us, that the cause I assign of the cohesion of our marbles is liable to huge inconveniences; of the greatness of which we may judge by the first of them; *Confitentur* (says he) *tum ipsi tum alii omnes, ponderationem omnem conatum esse per lineas rectas undiquaque ad centrum terræ; & proinde non cylindrum vel columnam fieri, sed per pyramidem, cujus vertex est centrum terræ, basis pars superficiei atmosphæræ.* Page 20.
'They themselves and all others confess, that all ponderation is an endeavour every
'way by right lines into the center of the earth; and so that it is made not by the
'figure of a cylinder or column, but by a pyramid, whose top is the center of the
'earth, and whose basis is part of the surface of the atmosphere.' As if it were much material, whether a body, whose basis is scarce two inches diameter, and whose length amounts to some thousands of miles, be considered as a cylinder or a pyramid. Certainly *Stevinus*, and other learned writers of the hydrostatics, would scarce have made his an objection, since they scruple not to make it a postulatum, that all not very distant perpendiculars be looked upon as parallel, though they allow such perpendiculars would meet in the center of the earth. What he adds, partly in these words, *Conatus ergo punctorum omnium ponderantium propagabitur ad superficiem marmoris superioris, antequam possit propagari ulterius (puta) ad terram*; 'Therefore the endeavour
'of all the points, that ponderate, will be propagated to the surface of the upper
'marble, before it can be propagated further (suppose) to the earth;' and partly, in the following lines, to prove, that the whole endeavour of the pyramid, that leans upon the upper marble, is terminated there, and that there is no endeavour of the atmosphere against the under-superficies of the lowermost marble: this ratiocination seems grounded, partly upon a conceit of his about the nature of gravity, according to which I see not, why any body placed between the sides of that pyramid, or rather cone, whereof the upper-superficies of the higher marble is the basis, should descend upon the account of gravity; and partly from a mistake of my opinion. For I do nowhere, that I know of, speak, as if I thought this sustentation of the lower marble were performed by little globules, or other minute bodies, protruding one Page 20.

 another

another directly towards the center of the earth, and rebounding from a perfectly smooth superficies: nor need I say, that the lower stone is sustained by the pressure of the self-same pillar of the atmosphere, that is incumbent on the upper, since other parts of the atmosphere, some on the one hand, and some on the other, pressing obliquely upon the uneven surface of the earth, may have their pressure upward terminated against the lower surface of the undermost marble. And in the mentioned *History of fluidity and firmness*, speaking (*page* 187.) of the adhesion of flat glasses (and the reason is the same in our flat marbles) I plainly deduce it from the pressure of the fluid air, which, like a liquor, diffusing itself upon the surface of the terrestrial globe, because its descent is there resisted, does, like water and other liquors, press almost equally every way, and strongly endeavour to thrust away any body against which it can bear; so that wherever the pressure is taken off from one part of a body, and not from the opposite, that body will be prest toward that part, whether it be downwards, or sidewards, or upwards, where that formerly equal resistance is removed. And this explication I do in that discourse back with considerations and experiments, which Mr. *Hobbes* is not pleased to take any notice of: wherefore I hold it not amiss to add here two or three other experiments to one of those mentioned there.

FIRST then, an æolipile being by heat freed from air as much as you can, if the little hole at the extremity of the neck, by which the air gets in and out, be presently and carefully stopt with wax, and afterwards suffered to cool, there will not be in the cavity of the æolipile a resistance any thing near equal to the pressure of the outward air. And therefore if you perforate the wax, that air will violently be impelled in at the unstopt orifice, whether the neck be held parallel or perpendicular to the horizon, or in any other situation in respect of the center of the earth. And the like will happen, if the æolipile be unstopt under water.

NEXT, I relate in the mentioned history, that having drawn some of the air out of a large glass with a narrow mouth, and thereby destroyed the equality of force betwixt the weight of the outward air, and the now weakened spring of the inward; I found, that by immediately applying a flat body to the orifice of the vessel, that body was readily lifted up and sustained in the air as long as I pleased, though the weight of it exceeded twenty ounces.

THIRDLY, I lately met with the relation of an experiment, which does abundantly make out the power of the ambient atmosphere to press bodies against each other, when it cannot get between their internal surfaces. For the ingenious author of the Magdeburgic experiment writes to the industrious *Schottus*, that having caused two copper-plates to be made almost in the form of scales a little above half an ell in diameter, and exactly congruous, if laid upon one another; *Has* (says our Jesuit) *si mutuo sibi imponit, & aërem extrahit, adeo ab externi aëris gravitate compressæ atque unitæ tenentur, ut sex viri robusti eas divellere non possint. Quod si tandem adhibito omni conatu divellantur, crepitum edunt sclopeti aut musquetæ explosioni non minorem; quamprimum vero per claviculum seu epistomium apertum vel minimus aditus laxatur aëri, sponte separantur.* 'These scales he puts one upon another, and draws out the air, and then are they kept 'so compreſt and united by the gravity of the external air, that six strong men cannot 'pull them asunder. But if at length, by the use of the utmost endeavour, they are 'plucked in sunder, they make a noise equal to the report of a musquet; but as soon 'as ever, by the stop-cock opened, there is the least entrance given to the air, they 'are severed of their own accord.'

Schott. Mech. Hyd. Pneumat. page 46.

AND,

And, fourthly, if a glass phial (such as will be anon more fully described) have a pipe open at both ends so fitted into it, that no air can get in and out betwixt the neck and it; and if the phial be so far filled with water, that the lower end of the pipe be well immersed therein; if then you suck at the upper end of the pipe, the water will ascend to a good height; which argues its being forced upwards by the oblique pressure of the air in the phial: for it is only in the pipe, and not in the phial, that there is any air in the same perpendicular with the water that is impelled up.

But let us follow Mr. *Hobbes* a little further. Having asked this question, *Sed vis illa elastica, quam in aëre esse dicunt, nihilne ad marmor sustinendum conferre potest?* he answers, *Nihil omnino; non enim conatus in aëre est ullus ad centrum terræ magis quam* Page 20, *ad aliud quodvis punctum universi. Quoniam enim gravia omnia tendunt à circumferentia* 21. *atmosphæræ ad centrum terræ, & inde rursus ad circumferentiam atmosphæræ per easdem lineas reflexas, conatus sursum conatui deorsum æqualis erit, & proinde mutuo se perimentes neutrâ conabuntur viâ.* 'But can the spring, which they say is in the air, confer no-'thing to the holding up the marble? —— nothing at all: for there is no endeavour 'of the air to the center of the earth, more than to any other point in the universe. 'For seeing that heavy things tend from the circumference of the atmosphere unto the 'center of the earth, and thence again to the circumference of the atmosphere by 'the same reflected lines, the endeavour upwards will be equal to the endeavour 'downwards, and so destroying one another, they will endeavour neither way.' But that the spring of the air may perform somewhat in the case proposed, I hope the newly mentioned experiments have evinced. And the reason he annexes to his negative, as also the difficult example he subjoins, of a man lying in the bottom of the sea, seem rather opposed to the weight than the spring of the air. But we have already, both by experiments, and by his own concessions, sufficiently proved, that the air is not devoid of gravity: and that it likewise gravitates upon the terraqueous globe, which in this page he seems to deny, we have proved by divers reasons, and particularly in our 25th experiment, by the vast expansion of air under water, when the pressure of the incumbent air was taken off from the water.

As for the scheme he annexes, I confess I do not well see what he drives at in it; at least if it be intended for a confutation of the conjecture I have been defending: nor am I the only person, that complain of his writing often enough obscurely. And as far as we can judge by the conclusion, couched in these words, *Non potest ergo pars* BC. (which in the explication of the scheme he calls *pars atmosphæræ intra totam posita ubicunque) (propter magnitudinem) quantumvis gravis sit, descendere, neque ergo premere sive gravitare*; 'Therefore the part BC (that is, a part of the atmosphere 'placed any where within the whole) cannot (by reason of its greatness) descend, al-'though it be heavy, and therefore it cannot press or gravitate;' his notions about gravity are somewhat strange, and probably either do not concern us, or will be found repugnant to those experiments, on which our conjecture is grounded.

And as for what he adds, *Si possibile esse negarem, ut diligentiâ & arte humanâ duæ* Page 22. *superficies corporum duorum inter se per omnia puncta ita accurate congruæ fiant, ut ne minimo quidem corpusculo creabili transitus permittatur, non video quomodo illi aut suam hypothesim tueri, aut negationem nostram improbitatis arguere jure possent:* 'If I should 'deny it possible, that by the art of man, two surfaces of two bodies could be made 'so accurately fit, that they should touch in all points, so that there could no creable 'corpuscle pass between them; I do not see, how they could defend their own hypo-'thesis, or disprove our negative assertion:' I confess I do not see how this argumentation

tation proves any thing against the interest, either of the weight, or so much as of the spring of the air in the cohesion of our marbles. For, provided that the corpuscles of the air get not in between the two stones, the pressure of the air may well suffice to keep them together. And, lastly, as for that modest passage (that immediately precedes the words newly recited) wherein he says, *Utraque illa phantasia, tum gravitatis atmosphæræ, tum vis elasticæ sive antitupiæ aëris, somnium erat. Siquidem autem illis concederetur esse aliquam in filiculis aëris antitupiam, quæretque aliquis, unde illa curvata quidem sed quiescentia moverentur rursus ad rectitudinem, deberent illi, si physici haberi volunt, causam ejus aliquam possibilem assignare:* 'Both these fancies, as well that 'of the weight, as of the spring or antitupy of the air, are dreams. But if it be 'granted, that there is a kind of recoiling in those small hairs or slender corpuscles, 'of which the air consists; one may inquire, whence it is, that those crooked bodies, 'settled and at quiet in that posture, came to be moved into a streightness. They 'ought, if they will be esteemed natural philosophers, to assign some possible cause 'of this:' we have already given an account, why we forbore to assign a cause of the motion of restitution; but methinks Mr. *Hobbes* might have, for the speaking so, chosen a fitter place than this, where he gives me so fair an opportunity to tell him again, that he should, if he would be thought a Naturalist, have assigned some cause of the phænomenon, about which he had all this while been disputing; which since even he himself has declined to do, perhaps the phænomenon will be thought somewhat difficult, and my attempt at discovering the cause of it will be at least excused.

Page 21, 22.

But after having so long dwelt upon the consideration of our marbles, it is high time to proceed to what remains.

In the 24th page, Mr. *Hobbes* has that passage I formerly recited touching the glass-fountains, out of which water is thrown up by compressed air. But though I, as well as others, have made use of such fountains, invented by *Vincenzo Vincenti* of *Urbin*, and was unsatisfied with the account he gives of their phænomena; yet in regard some learned men, and particularly Dr. *Ward*, have already examined his explication, and I am not obliged to do so; I shall only take notice of what our author objects, to prove that this phænomenon cannot be solved by the spring of the air, in these words:

Page 24. B. *Cur non potest aqua, quæ cum injiceretur, particulas aëris comprimebat, ab iisdem particulis se explicantibus rursus rejici?*

A. *Quia locum explicatæ majorem non requirunt quam compressæ. Quemadmodum in vase aqua pleno, in qua esset multitudo anguillarum, anguillas sive in se volutas sive explicatas idem semper capit locus. Propellere ergo aquam per vim elasticum, quæ alia non est quam motus corporum se explicantium, non possunt.*

B. *Comparatio illa aëris cum aqua anguillis plena, nostris, credo, non displicebit.*

'*B.* Why cannot the water, which when it was injected did compress the particles 'of air, be again cast out by the same particles explicating themselves?

'*A.* Because when explicated they require no greater place than when compressed: 'as in a vessel full of water, wherein are many eels, the same proportion of place 'receives them, whether they are folded round or at length. Therefore they cannot 'drive up the water by their spring, which is nothing else but the motion of bodies 'explicating themselves.

'*B.* The comparison of air to eels in water, I suppose, will be well received by 'our academians.'

But

But the Elaterists will answer, that neither can his earthy atoms, to whom he ascribes the rejection of the water forced in, truly fill up more parts of space at one time than at another; and therefore the objection might have been spared: but indeed it reaches us not. For we, as Mr. *Hobbes* knows well, are not wont to compare the air to eels, but to wool: and though each hair, that makes up a compressed lock of wool does not really fill more space with wool when extended than when crumpled; yet when there is a congeries of these hairs compressed together, the whole fleece or congeries does by its spring endeavour to thrust away those contiguous bodies, by which it is penned up, as I have more fully explained in my epistle: so that these aërial corpuscles, being pent up by the water forced into the glass, still endeavour to expand themselves by throwing it out.

What our author adds in the same page, as if they were mistaken, that think the experiments of the Plenists tended (especially till of late) to prove, that the generality of them did not always mean by a vacuum a space perfectly devoid of all corporeal substance, but any space here below, that is not filled with a visible body, or at least with air (for these are my words, to which I suppose Mr. *Hobbes* alludes): to this, I say, it is scarce worth while to make answer, the controversy being of such small moment (though I think I could easily enough do it) especially since he rather excuses those, that may have negligently expressed themselves, than disproves what I said. And since I spoke chiefly and by name of the Peripatetic schools, he may well allow, that their expressions concerning this matter were not always so accurate, whilst in this very passage he concludes with these words; *Vides quam ineptum sit ad explicationem effectuum taliam advocare verba metaphorica, ut fugam vacui, horrorem naturæ, &c. quibus olim ad existimationem suam tuendam usæ sunt scholæ:* 'You see, how foolish 'a thing it is to bring for the explication of such effects metaphorical words, as the 'shunning of a vacuum, the abhorrence of nature, *&c.* which heretofore the schools 'used, to defend their reputation.' Nor is what he adds concerning the vacuum to be attributed to *Democritus* and *Epicurus* either clear enough, or of concernment enough to our dispute, to be insisted on by us; especially since I see not to what purpose he brings it in. Page 250.

But there are in this page two particulars, which, though they make little or nothing against what I said of the Plenists, may deserve to be taken notice of.

The second (for I think it expedient to dispatch that first) is couched in these words; *In hydriis perforatis ideo hæret aqua, quia quæ per tantillum foramen exiturit, adeo exigua est, ut non posset ita in longitudinem se diffundere, ut descendendo aditum aëri faciat, per foraminum circumferentias; neque aër ab exeunte aqua pulsus locum alium (in mundo pleno) habere potest præterquam quem aqua deseret:* 'In the gardener's watering-pots 'therefore is the water suspended, because that which issues out at so small a hole is 'so little, that it cannot diffuse itself to such a length, that by its descent it may give 'passage to the air through the circumferences of the holes. Nor can the air driven 'off by the water going out find any other place, besides that which the water 'leaves.' Page 251.

But this experiment I have already examined as it is proposed in his *Elements of Philosophy*; and therefore I shall now only say to the light variation I find of it here, that the reason here assigned, why the water in gardeners pots closed at the top does not descend, is not rightly assigned, since (to omit other objections) by Monsieur *Pascbal*'s experiment it appears, that though in pipes of no great length the water will not run out, yet if the pipe be long enough, though the orifice be no wider, the water will descend without giving passage to the air at the circumference of it.

But

Pag. 25. BUT the other particular here mentioned by Mr. *Hobbes*, who thus proposes it, *Qui per fistulam ore aquam sugit, aërem medium prius sugit, quo distentum aërem externum removet; qui remotus locum (in pleno) habere nisi proximum removendo non potest; & sic continua pulsione aqua tandem pellitur in fistulam, succeditque aëri, qui exugitur:* He that 'sucks water into his mouth by a pipe, first sucks up the air between, whereby he 'removes the distended external air, which being removed (the world being full) it 'can have no place but by removing the next, and so by continual pulsions the water 'is at length driven into the pipe, and doth succeed the air, which is sucked out;' deserves a more particular consideration. For this account of the ascension of liquors by suction is not only here given by Mr. *Hobbes*, but for the main by the learned *Gassendus* himself, and other Atomists, and is generally acquiesced in by the modern philosophers; perhaps the rather, because it seems not to establish or overthrow a vacuum. But though I shall not deny, but that many phænomena of nature may be probably explicated by this propagation and return of motion; yet there are some phænomena here below, which I see not how the Cartesians, or the Atomists, or Mr. *Hobbes* can explicate, without admitting the spring of the air, and which perhaps by the spring of the air may be explicated, without the recurring to such a propagation and return of impulse. Divers instances to this purpose I elsewhere consider, but at present I shall propose only one experiment purposely devised to shew, that both Vacuists and Plenists should admit an elastical power in the air. I took then a glass-vessel consisting of two parts; the one was a phial capable of containing about a pound of water, and the other a pipe open at both ends, the lower of which reached within two inches of the bottom of the phial. This pipe was by the glass-man fastened into the neck of the phial, not by any cement, lest it should be pretended, that the air might undiscernedly get in or out, but with melted glass of a good thickness: into this vessel, by the open pipe, I at length (for it is somewhat difficult) poured water enough to swim a pretty way above the lower extreme of the pipe, and then often inclined the vessel, to give a free intercourse betwixt the air within the phial and that without it, that if the internal air were compressed by the affusion of the water, it might free itself, as it readily did by ascending in bubbles along the inclined pipe, till the outward and inward air were reduced to an equality of pressure. Now if all suction were produced by the pressure of the air, thrust away by the dilated chest of him that sucks, and so thrusting the water or other liquor into the pipe at which he sucks, it seems evident, in our case, that the water would not ascend by suction; since by the contrivance of the vessel, the air thrust away by him, that sucks, cannot at all come to bear or press upon the water. And yet, whether the pipe were inclined or erected, the water did according to my expectation easily enough ascend, upon suction, to the top of the pipe, and ran over into my mouth. I say, easily enough; because that though the spring of the air pent up in the phial were able, upon the decrease of the pressure of the outward air, occasioned by my sucking, to impel the water strongly enough into the pipe; yet, when a pretty quantity of water had been so impelled up, the included air gaining thereby more room to expand itself, its spring was thereby so far weakened, that the water ascended far less easily than in ordinary suction. The other circumstances worth noting in this experiment belong not to this place; and what has been delivered may, I hope, suffice for the purpose it is alleged for. Only one particular I shall here add, by way of confirmation of what I said touching the weakened spring of the air, and it is this; that partly to shew some, who yet embrace the opinion of the schools, that the ascension of the water in the pipe did not proceed from any such tendency in the water itself to ascend

ascend for prevention of a vacuum, and partly for other reasons, that concern not this place, I did carefully take out the water by degrees, till the lower end of the pipe was but very little under the surface of the water; though in the cavity of the pipe, the water, as it usually will be in pipes that are not wide, was a pretty deal higher. Then suffering the vessel to rest, and sucking at the upper end of the pipe, the water (as I foresaw it would be) was impelled up, yet without reaching near the top, till the surface of it was fallen a little below the bottom of the pipe. But then, though I continued sucking, no more water ascended into the pipe, but the air passing through it towards my mouth, did in its passage toss up the water, that was already in the pipe, and turn it into bubbles (of a strong bigness, when the cavity of the pipe would permit it) which broke (not without noise) one after another: and thus the ascending air for a pretty while kept the water in the pipe from falling back to that in the phial. But when I removed my mouth, the spring of the air remaining in the cavity of the phial, being debilitated by the recess of the air I had (as men are wont to speak) sucked out, it was not able to resist the pressure of the outward air, and accordingly the water in the pipe was not only depressed into the phial, but the outward air forced its way in many bubbles, and not without some noise, through the water contiguous to the bottom of the pipe, till the pressure of the included air and that of the atmosphere were reduced to an equality.

But in the same 25th page our author tells us, that the society he writes against, would have the cause of filtration, and that of the passage of water through siphons, to be the same. To which he annexes, this peremptory passage; *Id vero impossibile est. Nam in siphone nisi ambo crura aquâ impleantur, aqua è pelvi non ascendet. Ascensionis causa in pannum est motus ille terrearum atomorum, quæ aquæ contiguæ sunt, motus (inquam) circularis simplex aëri in quo moventur communicatus; quæ atomi aquam ferientes in materiam laneam incutiunt, incussæ autem magis magisque madefaciunt, donec madida tota sit. Cum vero madida tota sit, &c.* 'But that is impossible: for in a siphon, unless 'both legs are filled with water, the water will not ascend out of the bason. The 'cause of its ascent into that cloth is the motion of the earthy atoms, which are near 'the water; I say the simple circular motion communicated to the air, in which they 'move; which atoms striking the water, beat it up into the woolly matter, which 'beating of them against the cloth, makes it more and more moist, till it becomes 'all over wet. And when it is so, *&c.*' Thus far he; but the passage in my epistle, upon which he seems to have grounded his opposition, is but this, (wherewith I begin my 35th experiment) *Some learned mathematicians* (I meant the industrious *Schottus* and some Cartesians) *have of late ingeniously endeavoured to reduce filtres to siphons; but still the cause of the ascension of water and other liquor, both in siphons and filtration, needing (for aught we have yet found) a clearer discovery and explication, we were desirous to try*, &c. So that neither did I ascribe this reduction (of filtres to siphons) to a society, which was not then in being, nor perhaps so much as designed; nor did I adopt it myself; but expressed a desire to have it further examined. But as for the cause of filtration itself, I may take a fitter opportunity to discourse of it: in the mean time, I doubt, whether the reason here assigned by Mr. *Hobbes* will not seem as well precarious, as the *motus circularis simplex* of earthy atoms, whereon it is grounded. Nor does his explication render a reason, why quicksilver will not ascend the 14th part as high in the filtre, though in part immersed into it, as water; nay, will not reach so high, where it is contiguous to the filtre, as where it is not: nor why it should begin to ascend, since, for aught he shews to the contrary, the pressure of the air, even in the sense he takes the air, ought to be the same on that part of the surface

ſurface of the liquor, which is contiguous to the filtre, and on any other part of the ſame ſurface. To which I ſhall only add, that as reſolutely as Mr. *Hobbes* ſays it is impoſſible for the water to aſcend out of the veſſel into a ſiphon, unleſs both the legs be filled with that liquor, he would probably have ſpoken more warily, and diſtinguiſhed betwixt ſiphons, if he had been pleaſed to take notice of what I relate in the forementioned 35th experiment, of a ſmall glaſs-ſiphon I deviſed, whereof when the ſhorter leg was but dipped in water, the liquor did preſently, as it were of itſelf, run down the longer leg. Which experiment, beſides other conſiderations, may induce us to ſuſpect, that the nature of ſiphons, and of filtration, may not yet be ſo thoroughly underſtood, as not to deſerve a further inquiry.

BUT to draw at length towards a concluſion of our troubleſome *Examen*; it remains only, that I take ſome notice of the general corollary, that Mr. *Hobbes* is pleaſed to deduce from his whole diſcourſe, of the experiments exhibited in our engine.

Pag. 23. A. *Fateris ergo* (ſays he) *nihil hactenus à collegis tuis promotam eſſe ſcientiam cauſarum naturalium, niſi quod unus eorum machinam invenerit, qua motus excitari aëris poſſit talis, ut partes ſphæræ ſimul undiquaque tendant ad centrum, & ut hypotheſes* Hobbianæ, *ante quidem ſatis probabiles, hinc reddantur probabiliores.*

B. *Nec fateri pudet; nam, eſt aliquid prodire tenus, ſi non datur ultra.*

A. *Quid tenus? Quorſum autem tantus apparatus & ſumptus machinarum factu difficilium, ut eatenus tantum prodiretis, quantum ante prodierat* Hobbius? *Cur non inde potius incepiſtis, ubi ille deſiit? cur principiis ab eo poſitis non eſtis uſi? Cumque* Ariſtoteles *recte dixiſſet, Ignorato motu ignorari naturam, quomodo tantum in vos ſuſcipere onus auſi eſtis*, &c.

'*A.* You confeſs then, that your collegiates have as yet in nothing advanced the 'knowledge of natural cauſes, but that one of them hath found out an engine, in 'which there may be ſuch a motion of the air excited, that the parts of the ſphere 'may together every way tend unto the center; and that the hypotheſes of Mr. '*Hobbes*, before probable enough, may be thence made more probable.

'*B.* Right; I am not aſhamed to confeſs it; for it is ſomewhat to arrive ſo far, 'if we can make no further progreſs.

'*A.* Why *ſo far?* To what end ſuch preparation and charge for engines difficult 'to be made, to make no further diſcovery than Mr. *Hobbes* had made before you? 'Why did you not rather begin where he ended? Why did you not uſe the prin-'ciples he had laid; and when *Ariſtotle* had rightly ſaid, that *without the knowledge* '*of motion there is no knowledge of nature*; how durſt you take ſuch a taſk upon your-'ſelves?'

As to what he ſays to the diſparagement of the aſſembly, and in his own praiſe, the laws I preſcribed myſelf, at the beginning of this diſcourſe, forbid me to reply to what is more likely, amongſt civil and judicious readers, to prejudice his own reputation, than theirs he is ſo diſpleaſed with. And as for that, which he aſſigns to be the uſe of our engine, I ſhall very willingly leave it to the ingenious to judge, what uſe may be made of it. But as for this mentioned by Mr. *Hobbes*, if he means here, as he elſewhere teaches (pag. 13 and 19) that the motion he ſpeaks of is produced by that of the air impelled betwixt the ſides of the ſucker and the cylinder; perhaps it will be thought I have ſufficiently proved, that it is not *any* of its uſes, ſo far is it from being its chief or only uſe. But I confeſs I ſomewhat wonder Mr. *Hobbes* ſhould quarrel with me (for it is I that in my epiſtle employ the following verſe) for ſaying,

6

Eſt

Est aliquid prodire tenus, si non datur ultra.
'Thus to have made an entrance, though we miss
'Of further progress, some performance is.'

And this not, as some would perhaps suspect, because I do not imitate him, in speaking of myself, as he does of himself, but because he thinks the expression too arrogant. For since he here confesses, that his hypotheses are by this engine rendered more probable, some will perchance think that to be enough to entitle my experiment to some degree of usefulness, unless Mr. *Hobbes*'s doctrine of the air had found more embracers than before these seasonable, though despised experiments, it was observed to have. But, since either of us may be partial in his own case, I am very well content to leave others to judge, both whether my expression have been guilty of arrogance, and how much he has done more than *prodire tenus* in all the past long discourse against me, when they have considered, what new experiment or matter of fact Mr. *Hobbes* has therein added to enrich the history of nature, what new truths he has discovered, or what errors (except one of his own in the last page) he has well confuted. These things, I say, I am very well content to leave to be judged of by all disinterested persons, without being much discouraged by the differing strain, wherein Mr. *Hobbes* thinks fit to speak of his own performances and mine, or invited to imitate him in that way of writing; my endeavours (such as they are) having hitherto been favoured with such a reception among the Virtuosi, that possibly I may have almost as little need as reason to commend them.

But it is somewhat troublesome work to argue long with a man, that's angry with an expression, which perhaps none but he would have found fault with for want of modesty: and therefore as I have left unrecited several provoking and very undeserved expressions he employs in the same page, and even that passage, where, to prove our naturalists and mathematicians professed they would not receive truth coming from him, he alleges only a saying (whether true or no, I examine not) of Dr. *Owen*, who, besides that his profession was divinity, not philosophy or mathematics, neither is nor ever was of our society: as, I say, I have (for the reason newly intimated) declined taking notice of matters of this nature; so I will not now stay to inquire, why he urges us, whom he would have men take for Vacuists, with the authority of *Aristotle*, whom, on other occasions, he is wont to use with as little respect, as if he were a member of our society. Nor shall I now examine, why here, and elsewhere, he sends us to his own writings for the doctrine of motion; as if, to omit antienter authors, such great personages as *Galilæo*, *Mersennus*, *Verulam*, *Des Cartes*, *Gassendus*, *Balianus*, *Johannes Marcus Marci*, *Honoratus Fabri* (not to mention other moderns, nor those of our own assembly, as the eminently-learned Sir *Kenelme Digby*, and the others, whom their modesty forbids me here to name) had not most of them learnedly, and some of them copiously, written of local motion before Mr. *Hobbes*'s books, where he treats of it, came abroad into the world: this, I say, I shall not insist on, because I would hasten to a conclusion. Only one thing I must add, that whereas he accuses us of devising elaborate and sumptuous engines, I do not fear to find so many readers of his mind, that I need make them excuses for what perhaps will obtain their thanks. And whatever Mr. *Hobbes* may think, for my part I freely confess, that I love truth so well, that I do not think, not only my pains and charges, but, even what I rate much higher, my time itself, too much for the discovery of truth; or (that Mr. *Hobbes* may not think me partial) even for the establishment of such truths, as, though discovered by some, are yet as far more generally opposed than embraced.

THERE remain yet some other pages of Mr. *Hobbes*'s dialogue, wherein he speaks of fire, and cold, and ice, and light, and colours, and fluidity, and hardness, and thickness, and ethics, and politics, and the duplication of the cube, and the quadrature of the circle, and several other subjects. But these I forbear to meddle with, not that I approve them all, or the greatest part, but partly, because I am too much tired already, to be fond of engaging in controversies, that I am not tied to meddle with (except what concerns fluidity and firmness, which I shall, God willing, examine by itself) partly, because divers passages relate to persons, not things: partly because I do not much fear, that Mr. *Hobbes* will find every reader so easy and complaisant as he makes his academian, who in many passages of the dialogue speaks not unsuitably to what he does in the last page of it; where he excepts but one particular, (and that is neither the *duplication of the cube, nor the squaring of the circle*) when he tells Mr. *Hobbes, discedo jam multo (ut mihi videor) quam ante certior, & quæ dixisti omnia teneo & probo:* and partly, because Mr. *Hobbes* has some things, as about fire, and certain colours, which I am not forward to reject, though the considerations, that incline me to some such opinions, be perhaps very differing from the grounds, on which he proposes them. And indeed as well my nature as my custom forbids me to scruple to learn, if I can, of persons much less famous than Mr. *Hobbes*. It is far from my humour to write against all that every man says, that (how causelessly soever) writes against me; and I am almost as much indisposed to reject as to embrace, without distinction, whatever it be, that this or that man teaches.

CHAP. VII.

(Being an Appendix to the past Discourse:)

Wherein is examined what Mr. Hobbes *teaches concerning Fluidity and Firmness.*

ALTHOUGH Mr. *Hobbes* do not name me for the maintainer of the opinions which I have proposed in the *History of Fluidity and Firmness*, and which he censures; yet, since that history, after having been mentioned in the book I have hitherto been defending, was published a good while before Mr. *Hobbes*'s dialogue; and since some of the chief particulars he censures or takes notice of, are delivered there, and some of them, perchance, no where else; I think it may concern me to examine what he says either against my opinions, or in favour of his own, touching *Fluidity and Firmness*. And if it be said, that he meant not his objections particularly against me, but rather against some other person, who may have liked such opinions; I shall answer, that I am content to leave to others the defence of their particular opinions, and to have as much of the following discourse, as is concerned in this allegation, looked upon as written only upon this supposition, that my writings are those he designs to oppose. But there being other things in what he discourses about *Fluidity and Firmness*, which, for the reasons freshly intimated, I think fit here to consider, I chuse to gather up together the passages touching these subjects, which I find scattered in his dialogue, that I may have the better opportunity to clear up the matter itself under debate.

BUT, before I go further, I must, at the very entrance of this discourse, take notice, that in the fifth page, where Mr. *Hobbes* begins to dispute against our notion of *Fluidity*, he very much mistakes my opinion, as may appear by these words, which he puts into the mouth of his academian; *Sed plerique nostrum naturam fluidam à non fluida distinguimus magnitudine partium, ex quibus corpus aliquod constat, & quasi compingitur:*

Page 5.

pingitur: itaque non modo aërem, aquam & liquorem omnem, sed etiam cinerem & pulverem, tanquam fluida contemplamur. Et fluida ex non fluidis composita esse posse non negamus; nam divisibilitatem illam infinitam non concoquimus. To which he answers, *Divisio quidem infinita concipi non potest, divisibilitas autem facile. Ego contra, distinctionem non capio intra fluida & non fluida, quam sumitis à magnitudine partium; nam si caperem, ruina illa sive rudera illa, quæ jacent in ecclesia Paulina, mihi dicenda essent fluida: sin propter nimiam lapidum magnitudinem fluida illa esse negaveritis, defini mihi magnitudinem illam, quam habens pars ruentis muri propter eam sit dicenda fluida. Tu vero, qui divisibilitatem infinitam non capis, dic mihi, quæ tibi apparet causa, quare Deo omnipotenti difficilius esse putem creare corpus fluidum, & cujus partes actu diffluant, omni data atomo minus, quam creare oceanum. Itaque desperare me facis omnem conventus vestri fructum, dicendo, quod putant aërem, aquam, & cætera fluida constare ex non fluidis, tanquam si murum, cujus ruentes lapides aliquosque discurrunt, dicerent esse fluidum. Si sic loquendum est, nihil non est fluidum; nam etiam marmor comminui potest in partes omni atomo Epicureana minores.* 'But 'most of us distinguish the nature of fluid from that which is not fluid, by the great-'ness of the parts, of which any body consists and is made up with. Wherefore we 'do not not only look upon air, water, and all liquors, but upon ashes also and dust, 'as fluid bodies. And we deny not, that fluid things may be made of things not 'fluid; for we do not digest the notion of infinite divisibility.

'*A.* Infinite division cannot be conceived, but (infinite) divisibility may easily. 'I, on the contrary, do not understand the distinction of fluids and not fluids, which 'you take from the greatness of the parts: could I digest this, I must say, the ruins 'of shattered rubbish stones, that lie in *Paul*'s, were fluid. But if those ruins cannot 'be called fluid, because the stones are too big, define me the bigness, that the parts 'of a ruined wall must have, that they may be called fluid. But you, that cannot 'understand infinite divisibility, tell me what you think to be the cause, why I 'should think it more hard for almighty God to create a fluid body less than any 'atom proposed, that its parts might actually flow, than to create the ocean? There-'fore you make me despair of any fruit of your meeting, by saying, that they think 'air, water, and other fluids consist of non-fluids; as if a wall, that began to fall 'and be ruinous, were called by them a fluid body. If they may speak so, every 'thing is fluid, for even marble itself may be broken into parts less than any atom 'imagined by *Epicurus*.' Thus he. But in my History, though I make the smallness of the parts, whereof a body consists, one of the requisites to its being fluid; yet at the end of the 13th section I call the various agitation of those particles the principal qualification of all, and in the beginning of the 14th section I call it the chief condition of a fluid body. And therefore he much mistakes, if he thinks, that we always consider ashes and dust as fluids absolutely speaking. But as he * somewhere tells us, that by fire he understands the combustible matter itself, not simply and always, but then only when, *&c.* so neither do I look upon the dust of alabaster (to my words about which, I suppose, he has a respect in the passage under consideration) as a fluid body simply and always; for I clearly teach the contrary, but only when and whilst its parts are not alone reduced to a competent smallness, but are also actually put into such a various agitation as makes the body they compose (even according to Mr. *Hobbes*'s own definition, which is, that 'fluid bodies are those 'whose parts may, by very weak endeavour, be separated from one another') emulate a fluid body, by the very easy cession of its component corpuscles, and by its boiling like a liquor. By this the reader may discern how little, that makes against me, which Mr. *Hobbes* talks, and seems to do it seriously, of the ruins of *Paul*'s; as if, according

* *Hobbes de Corp. cb. 27.*

Mr. Hobbes de Corpore, cap. 26. part 4.

according to my opinion, *Ruina illa sive rudera, quæ jacent in ecclesia Paulina, dicenda essent fluida*; 'the ruins that lie in *Paul*'s church might be called fluid.' For it is most evident, that I require in the parts of a fluid body both minuteness and such a motion along each other, as makes them easily yield to the touch: which qualifications, how well they belong to the ruins of *Paul*'s, is, I think, not very difficult to determine; though in the same passage Mr. *Hobbes* does again make use of the like example, to which he subjoins, *Si sic loquendum est, nihil non est fluidum*; 'if they may so 'speak, there is nothing but is fluid.' Which, how little it follows from my doctrine about *Fluidity*, there needs not a quick-sighted reader to discern. As for the reason he annexes in these words, *Nam etiam marmor comminui potest in partes omni atomo Epicureana minores*; 'for even marble may be beaten into parts less than any Epicurean 'atom:' I would gladly know, by what art Mr. *Hobbes* can divide marble into lesser particles than such as are naturally indivisible (for such *Epicurus* makes his atoms to be:) nor do I see how, in case this could be done, it proves, that there is not any thing that is not fluid. For I say, that the blocks of marble before comminution are not fluid, either according to him or according to me; nay, the greatest comminution imaginable would not, according to my doctrine, make a lump of marble fluid, unless the heap composed of the parts, how minute soever, were actually and variously set a moving amongst themselves. But he would perhaps have spoken more warily, if he had considered the difference there is betwixt saying, that all things are fluid, and saying, as I do, that there are many bodies, that are now solid, which by comminution, and other requisite alterations, may be made parts of a fluid body: as hard ice may be turned into fluid water, and quicksilver precipitated *per se* into a red powder, may be reduced to running mercury. As for what he says of an infinite divisibility of body, it is scarce in this place worth while to examine it.

For I have shewn in the *History of Firmness*, that this divisibility (which I had then considered) does no way overthrow my doctrine of fluidity; nor does Mr. *Hobbes* here answer what I there discourse. Besides that indeed, I do not so well understand what he means and drives at, when he says that *Divisio quidem infinita concipi non potest, divisibilitas autem facile*: 'Division that is infinite cannot be conceived, divisibi-'lity may easily.' For since in this very passage and within a very few lines he has recourse in this matter to God's omnipotence, I see not, why an infinite division cannot be as well conceived as an infinite divisibility, since sure an omnipotent agent is able to do what is possible to be done; and why else should a body be called infinitely divisible? Besides, when Mr. *Hobbes* has recourse to what God can do (whose omnipotence we have both great reason to acknowledge) it imports not to the controversy about fluidity to determine what the almighty Creator can do, but what he actually has done. And lastly, whereas my adversary requires to have the magnitude defined, which a part of a falling wall ought to have to deserve the name of fluid; first, he should have clearly proved, that fluidity belongs to any one single part of matter, how minute soever, and not rather to an aggregate of particles. And next I say those corpuscles, that compose a fluid body may be of several sizes, as those of water, oil, and quicksilver, provided they be little enough to be put into the agitation, requisite to give the aggregate they make up, the qualities, that are wont to denominate bodies fluid; and it is no more requisite for me to define precisely the magnitudes of the parts of a fluid body, than for Mr. *Hobbes* in his definition above-recited to define (which he will not easily do) what precise degree of endeavour must be signified by that very weak endeavour, by which if the parts of a body can be separated from one another, he thinks fit to call them fluid. But though I thought it

it not amiss to make these animadversions upon Mr. *Hobbes*'s ratiocination, yet as to the opinion itself, for whose sake he speaks so severely and so despairingly of our society, if it be considered as I proposed it, he shews me as yet no cause at all to renounce it. For that which I taught is this, that if a solid body be reduced into parts minute enough, those solid corpuscles, whilst they are put into a convenient motion, may become parts of a fluid body. And against this Mr. *Hobbes*'s indignation seems stronger than his argument: for that which he objects being, as we have lately seen, that at this rate all bodies must be fluid; it is evident by what I have already argued, that he infers this absurdity not from my opinion, but his own mistake of it: nor did I content myself with the proofless proposal of my conjecture, but I delivered, in several parts of the often mentioned history, particular experiments to evince what I taught; as that a consistent coagulum of pure spirits of urine and wine may, by bare digestion, be turned into a permanent liquor; and that the fluid body of quicksilver may, without any sensible addition, be turned into a permanent dry powder, and may again in a trice by bare heat be turned into a lasting fluid body. Whereto I added other experiments, which together with these Mr. *Hobbes* would possibly have thought fit to answer, if he had found it easy for him to do so.

After this passage extant in Mr. *Hobbes*'s fifth page, that I have all this while been examining, I remember nothing in his dialogue, that requires to be insisted on about fluidity and firmness, till we come to the 29th page; where, having asked what cause the academians assign of hardness, it is answered, that some of them assign three: to which Mr. *Hobbes* so far agrees as to say, *Quin corpuscula (qualia sunt atomi, quas supponit* Lucretius *atque etiam* Hobbius*) jam ante dura facile possint ab aliqua dictarum causarum compingi, ita ut totum ex illis factum durum fiat, dubitandum non est:* 'But the corpuscles (such as are the atoms supposed by *Lucretius* and also by Mr. *Hobbes*) being hard before, might be easily compacted by any of the mentioned causes; so that it is not to be doubted, but that the whole to be made of those corpuscles will be hard.' But then he would have us assign the cause of that he calls *durum primum*. But after some discourse, wherein he is pleased to approve an objection of mine against some learned men, that ascribe all cohesion of bodies to a certain glue, he answers himself the objection he frames against my doctrine about hardness, and thereby allowed me to proceed to what he further presses in these words; *Si dura ex primis duris fieri dicant, quare non & fluida fieri putant ex primis fluidis? An creari fluida maxima potuere, ut æther, minima non potuere? Qui corpusculum durum aut fluidum primus fecit, potuit, si libuisset, illud fecisse tum majus, tum minus quocunque corpore dato. Quod si fluidum fiat ex non fluidis, ut vos dicitis, & durum ex duris tantum, nonne sequitur ex fluidis primis, neque fluidum fieri neque durum?* 'If hard bodies are made out of parts originally hard, why are not fluid bodies made of parts originally fluid? Could great fluids, as the æther, be created; and could not small ones? He that first made a body hard or fluid, could, if he would, have made it greater or less than any other proposed body. Now if a fluid body be made of parts not fluid (as you speak) and hard bodies only from hard parts; doth it not follow, that nothing neither fluid nor hard is made of original fluids?' But against this passage I have divers things to represent. For, first (not now to mention, that it may be questioned with what propriety one part of matter more than another may be called *primum durum*) he should have told us what he means by his *prima fluida*, and how he proves that there are any such; which since he has not done, it will be at least as hard for a considering man to acquiesce in his question as to answer it. For my part, I know no fluid body upon whose account, as of an ingredient, all others are fluid.

Page 29.

Page 30.

fluid. And I think, it will be hard for Mr. *Hobbes* to shew, that water, quicksilver, and purely-rectified chymical oils (to name now no other liquors) do consist of such *fluida prima* as he teaches, whereto they owe all their fluidity. And it is plain by several experiments delivered in our history, and even by the obvious changes of water and ice into one another, that it is the motion, rest, and the texture of the corpuscles, which compose a body, that make it firm or fluid. As for what Mr. *Hobbes* demands, whether the smallest fluids imaginable could not as well have been created as the æther, it proves nothing against me, the question not being, what might have been made, but what is so. And he should have answered the arguments I * allege to make it improbable that, a fluid body is, as he would persuade us in his book † *De corpore*, always divisible into bodies equally fluid, as quantity into quantities. It is true, he there tells us that, though many others do not, 'he understands by 'fluidity, that which is made such by nature equally in every part of the fluid body, 'in such manner, as water seems fluid, and to divide itself into parts perpetually 'fluid.' But whether others will take this for a clear notion of fluidity, I think may well be doubted; and he should not barely say, but prove (which I think he will find hard to do) that the corpuscles of water divide themselves so as he teaches, since we see, that not only they cannot penetrate glass, but are unable to be driven in at the pores of more open bodies, which other liquors easily pierce into. And, lastly, as to Mr. *Hobbes*'s question, *Quod si fluidum fiat, &c.* it is easy to foresee, what, according to my doctrine, I may answer: for, not to mention that the argumentation is invalid, unless by *fluidum*, he mean *omne fluidum*, I reply, that till he have explained what he means by his *fluida prima*, and proved that there are such, the question needs no answer. Besides that, whatever he, upon mistakes, strives to infer, my doctrine is so far from affirming, that there are many parts of matter, of which neither fluid nor hard bodies can be made; that I teach, as we have lately seen, that there are multitudes of parts, that may, by being reduced to a sufficient smallness, and put into a convenient motion, or by being brought to a mutual contact and rest, be made to constitute either a fluid body or a firm one: as may be exemplified in the formerly-mentioned instances of two subtile liquors, that immediately composed a consistent body; and of quicksilver, which without additaments was made sometimes a powder, and sometimes a liquor. What Mr. *Hobbes* adds in the next page about the difficulty of explaining the diaphaneity of glass or crystal, in case they consisted of corpuscles hard and implicated, or having their pores in any way whatsoever disjoined, I must not now insist on; since besides that such a disquisition would require almost a volume, the true and general cause of transparency in bodies is, in my poor opinion, one of the abstrusest things in natural philosophy; and Mr. *Hobbes*'s explication of it (though none of his worst conceits) has, for aught I can find, fallen short of satisfying the curious, as well as those of other men have done. But to me, that have not taken upon me to write elements of philosophy, it is enough, that I have, by competent experiments, and other proofs, confirmed the truth of my doctrine about the cause of firmness; though I attempt not to explicate the other qualities of the same bodies, whose explication my undertaking does not exact. Wherefore I hope I may now hasten to conclude this Appendix, with spending a few words on the notion of fluidity and firmness Mr. *Hobbes* would substitute instead of mine. For, having now (perhaps but too prolixly) examined what he has been pleased to object against my doctrine, I shall not need spend time to vindicate the experiments and considera-

Page 31.

* History of Firmness, page 227. and elsewhere.

† Chap. 16. sect. 4.

tions

tions whereon I built it; Mr. *Hobbes*, for reasons best known to himself, not having thought fit to take notice of them.

Mr. *Hobbes*'s theory of fluidity and firmness is thus delivered by himself;

B. *Quænam duri & fluidi sunt principia?* Page 30.

A. *Quid aliud nisi fluidi quidem quies, duri autem motus quidam ad illum effectum producendum idoneus? Per quietem intelligo duarum partium inter se quietem, cum se mutuo tangunt quidem, sed non premunt: nam & fluida moveri tota possunt retenta fluiditate, & dura quiescere, ut tamen partes eorum moveantur.*

'*B.* What are the principles of fluidity and firmness?

'*A.* Of fluidity, nothing but rest; of firmness, motion; such as is fit to produce 'that effect. By rest I understand the rest of two parts one with another, when they 'each touch, but neither press one another. For entire bodies of fluids may be in 'motion, their fluidity abiding, and hard bodies be at rest, although their parts be 'in motion.'

I doubt not, but this will to most readers seem a paradox. And as for his ratiocination contained in the two last lines, I shall readily allow him, that *fluida moveri tota possunt retenta fluiditate*; since that I think agrees at least as well with my hypothesis as his: but whereas he adds, that hard bodies may rest, and yet their parts be moved, that may in one case be conceived, and in another not. For indeed the implicated parts of a firm body may be made to tremble, or a little vibrate as it were to and fro, as those of a sounding bell do, or as in a hedge, the branches and twigs may be shaken by the wind, whilst the trees and bushes themselves continue rooted in the ground. But that, in a body, the constituent corpuscles should all, or most of them, be moved quite out of their places, in respect of one another, as was lately shewn to happen in fluids, and yet the body continue hard, is more requisite than easy to be proved.

But Mr. *Hobbes* contents himself to allege, in favour of his strange notion touching fluidity and firmness, three particulars, which, I confess, afford me not the least satisfaction.

The first is drawn from what he formerly taught touching the swift motion of the air in our cylinder; which example (as he calls it) having repeated, he adds, *Atque* Page 30. *hinc manifestum est vehementem esse in aëre ita moto & clauso compressionem, quantam scilicet efficere potest vis illa qua incussus erat; atque etiam à tanta compressione aliquem gradum consistentiæ fieri, quanquam consistentia aquæ minorem. Quod si esset in iisdem particulis aëris omnibus, præter motum illum, quo altera alteram premit, motus ille circularis simplex, isque satis vehemens, impossibile fere esset unam earum à suo circello dimoveri, quin, reliquis particulis resistentibus, totus simul premeretur, id est, totum durum esset: durum enim est totum illud, cujus nulla cedit pars nisi cedente toto. Vides ergo posse fieri duritiem in fluidissimo aëre per motum hunc circularem simplicem particularem, quibus duo motus contrarii ante dederant vertiginem.* 'And hence it is manifest, that there is a great compression 'in the air so moved and shut up; namely, so great as that force, by which it was 'driven in, was able to make; and also, that from so great compression some degree of 'consistence must be made, though less than that of the consistence of water. Now, 'if in all the same particles of air, besides the motion, by which one presses another, 'there was also the simple circular motion, and that vehement enough, it would be 'almost impossible any one of them should be moved from its little circle, but that 'the other particles resisting, the whole would be pressed together, that is, become 'hard; for that is hard of which no part gives place but upon the motion of the 'whole. You see therefore that hardness may be made in a most fluid body by this

'simple circular motion of particles, which was before imparted to them by two 'contrary motions.'

BUT, I confess, I do not see, how the *motus circularis simplex* he talks of should give such a hardness to the fluid air: nor is it manifest to me, how the air, that perfectly fills the cylinder, can be by motion compress, especially so far as to obtain thereby a degree of consistence fit to be mentioned, as he speaks of it, when (without adding the word *much*, or any other equivalent term) he says, that yet it is *less than the consistence of water*. For the cylinder being, according to him, perfectly full of air, I see not how the pumping can make the cavity (to use his own expression elsewhere) *fuller than full*; nor consequently can compress air to a consistence any thing near that of water, without penetration of dimensions. But these things were mentioned only *ex abundanti*, for the violent motion itself of the air in the cylinder (which motion the argument supposes) has been already, in the examen of one of the former passages of his Dialogue, sufficiently manifested to be contrary to experience.

THE second thing Mr. *Hobbes* alleges is his conceit of the generation of flesh within the muscles of the human body. But besides that, he takes for granted two or three things, which many learned anatomists and physicians, even among the moderns, will scarce allow him, and which he does not prove: besides this, I say, (which I may elsewhere have occasion to consider further) the account he gives of the generation of flesh from these suppositions, is far from being evidently enough deduced to vie for clearness with many of those experiments, which I have alleged in favour of the opinion he opposes. And whereas he adds, *Atque talis quidem esse* Page 31. *potest causa efficiens duri primi; duri autem secundi, id est, duri à cohæsione durorum primorum, causa potest esse motus ille idem circularis simplex conjunctus cum contactu eorundem superficiali, vel etiam intricatione:* 'And such indeed may be the cause of the *durum* '*primum*, or first hard body. But of the second, that is, of the cohesion of the two 'of these first hard bodies, the cause may be the very same circular motion, con-'joined with their superficial contact, or perhaps their being one with another intri-'cated:' Not again to say any thing to his *durum primum*, I confess I do not see how the *motus circularis simplex* should need to be superadded to the contact or intrication of the cohering firm corpuscles, to procure a cohesion, to which it is needless, and which, in divers cases, may be rather hindered than furthered by it.

THE third thing, that Mr. *Hobbes* alleges, is not so much a confirmation of his own doctrine, as an objection, as he seems to think, against that of his adversaries. Page 31. For, *Si supponamus* (says he presently after his last-recited words) *cum illis, duritiei causam esse magnitudinem aut crassitiem partium, quam rationem reddere poterimus, cur durior vel firmior sit aqua congelata, quam est eadem aqua ante congelationem?* 'If we 'suppose with them, that the cause of hardness is the greatness or thickness of the 'parts, what reason can we give, why congealed water should be harder or firmer, 'than the same water is before such congelation?' But it may easily be replied, that we make not the bigness or grossness of the parts of a body the only or chief cause of its hardness, but their rest by one another, which the parts of frozen water have; whereas those of unfrozen water have it not, but are in a state of fluidity, and consequently not of firmness. Which may be illustrated by what I † elsewhere relate of pure oil of aniseeds, and a substance I distilled out of benzoin, both which bodies were sometimes fluid and sometimes consistent, as the greater or lesser warmth of the air kept their parts in a due motion, or suffered them to rest.

† In the History of Fluidity and Firmness.

BUT

But in exchange of these few and unconcluding arguments, which are all that Mr. *Hobbes* alleges to countenance his paradox, how many experiments and reasons might we transcribe out of our History of Fluidity and Firmness, in favour of the contrary truth?

And as critical as Mr. *Hobbes* appears in laying down the requisites of a good hypothesis, I must make bold to the two conditions he mentions (page 11.) *ut sit conceptibilis (id est, non absurda)* which, whether it be enough, I now dispute not; *& ut ab ea concessa inferri possit phænomeni necessitas*; 'that it be conceivable, that is, 'not absurd; and that from its being granted, the necessity of the phænomenon 'may be inferred;' to add a third, namely, that it be not inconsistent with any other truth or phænomenon of nature. Which third condition whether divers of Mr. *Hobbes*'s hypothesis (which himself in this place calls *mirandæ*) do not want, we have in part already considered in the treatise, to which this is an Appendix; and (as I newly intimated) I might further shew, as to his notion of fluidity and firmness, if I would here repeat all the experiments mentioned in my history of them, though they be not all that I have made ready to the same purpose against another opportunity: but partly wearinesS, and partly a natural unwillingness to repeat, induces me rather to refer my reader thither. Which when I do, I do not forget, that Mr. *Hobbes* appears offended at me and others for troubling ourselves to make unobvious experiments. But that I may not repeat what, in divers treatises, I represent concerning the usefulness of such experiments, I shall now only oppose to the authority of Mr. *Hobbes* in this Dialogue (wherein he has been pleased to chuse those he calls the *Experimentarian Philosophers* for his adversaries) the authority and reason of the same Mr. *Hobbes* in another Dialogue (published but the year before) where one of his two discoursers having said, *Qui corpora corporibus admovendo, nova & mirabilia ostendunt naturæ opera, mirum in modum incendunt animos hominum amore philosophiæ, & ad causas investigandas non parum instigant, eoque nomine laude digni sunt*: 'They who, putting bodies to bodies, shew the new and admirable works of nature, 'do wonderfully inflame the minds of men with the love of philosophy, and do 'not a little instigate them unto the search of causes, and on that account are worthy 'of commendation:' The other confirms it, by adding, *Ita est*; *nam historiam naturalem (sine qua scientia naturalis frustra quæritur) locupletant*: 'True; for they en-'rich natural history, without which natural science is in vain sought for.' And howsoever, Mr. *Hobbes* needed not have recourse to such experiments, as he would be thought to disapprove (I mean elaborate ones) to discern, that his notions do not over-well agree with the phenomena. For, if there be not a various motion in the small parts of water and such liquors, whence it is, that a lump of common salt being thrown into a pot of water, is there dissolved into minute bodies, whereof many are carried to the very top of the water, and are so exquisitely diffused and mingled with the liquor, that each least drop of it contains numbers of saline corpuscles? And if motion be the cause rather of hardness than fluidity, how comes it to pass, that in frosty weather ice is by heat (which Mr. *Hobbes* will not deny to be motion, or an effect of it) turned from a hard to a fluid body? and that metals, as gold and silver, *&c.* whilst they are cold, or exposed but to a moderate heat, are firm and consistent bodies; and by a violent heat, which does manifestly give their parts a various and vehement agitation (as appears by their sudden dissipating of spittle, grease, and far more stable bodies, cast upon them, into smoak) are put into a fluidity; which, upon their removal from the fire, they quickly exchange for firmness?

Mr. Hobbes in examinat. & emendationes, &c. Dialog. 6.

BUT ſince the want of more to ſay would not, in haſte, put a period to this diſcourſe, I am content to let my haſte break it off; eſpecially ſince after I have thus examined what Mr. *Hobbes* teaches concerning fluidity and firmneſs, either here, or in that ſection of his Elements, where he pretends to define them, I think I need not fear, that a doctrine, which I have perhaps with ſome care endeavoured to eſtabliſh, for the main, upon experiments, ſhould be overthrown by opinions, whoſe grounds are but ſuch as we have already ſeen; and in pleading for which, the author is pleaſed not only to leave almoſt all my arguments untouched, but not ſo much as to offer at explicating, by his principles, any of thoſe numerous and important phænomena of fluidity and firmneſs, delivered in the treatiſe he oppoſes.

AND now leaving Mr. *Hobbes*, to apply myſelf to the reader, I have, to the things hitherto diſcourſed, but this one thing to add concerning them; that as little cauſe as Mr. *Hobbes* has hitherto given me, to diſtruſt what I have written of fluidity and firmneſs; yet I am not now more confident of my conjectures than I was, when, toward the end of the preface to the hiſtory of theſe two qualities, I ſpoke diffidently enough of the theorical part of that treatiſe. And I freely confeſs, that the great difficulty of things, and the little abilities I find myſelf furniſhed with to ſurmount it, do often, in general, beget in me a *great* diſtruſt even of things, whereof my adverſary's objections give me not *any*.

SOME

MOTIVES and INCENTIVES

TO THE

LOVE of GOD,

Pathetically discourſed of, in a LETTER to a FRIEND.

To the COUNTESS of *WARWICK*.

MY DEAR SISTER,

I EXPECT you ſhould ſomewhat wonder, that after having for above eleven years been careful to keep this following letter from the public view, and that too notwithſtanding the ſolicitation (not to ſay importunity) of divers illuſtrious perſons, and even your commands to releaſe it from its confinement; I ſhould now at length give way to its paſſing abroad into the world, and its making you a public and ſolemn addreſs. Wherefore judging myſelf obliged to give you an account of a paper, for which you have been long pleaſed ſo highly and ſo obligingly to concern yourſelf, I muſt, to remove your wonder, inform you, that I am reduced to this publication in my own defence. For, whilſt I was far from dreaming of permitting this epiſtle to paſs out of my cloſet, it happened, that a broken copy of it did (by I know not what misfortune for me) fall into the hands of a neceſſitous perſon, who would needs perſuade himſelf, that by printing it, he might relieve ſome of his preſent wants; and thereupon proffered to ſell the copy for a ſum of money. But my good fortune leading him to a ſtationer, to whom my name was not unknown, he very civilly ſent me forthwith notice of the propoſition that was made him, and after came himſelf to acquaint me, that the copy, about which he had been treated with, being but one of two or three, that were then abroad, ſome or other of them would, queſtionleſs, ſoon find the way to the preſs. This unwelcome accident did little leſs trouble than ſurprize me; for, beſides that it impoſed on me the neceſſity of a publication I had ſo long declined; and beſides that I knew, that * compoſures of a very differing nature being expected from me, the appearing of this, inſtead of them, would make this unwelcome to many, though it had fewer imperfections than it has: beſides theſe things (I ſay) my ſight was then, and is ſtill, ſo impaired by a diſtemper in my eyes; and the hours I could diſpoſe of were ſo pre-ingaged to philoſophical themes

* Some Treatiſes relating to Experimental Philoſophy.

that I could not promiſe myſelf ſo much as to read it it over before its going to the printer. But conſidering after all this, that the copy I had by me was like to prove more full, and leſs faulty, than any of thoſe, that ſome endeavoured to obtrude upon the world, I thought it leſs inconvenient to venture mine own abroad, than to run the hazard of a ſurreptitious edition of a diſcourſe, that could ſo ill bear the appearing with any other blemiſhes, than thoſe it brought with it into the world from my pen. And therefore having put it into the hands of perſons, whoſe eminent abilities the more knowing part of the nation has long and juſtly admired, and begged their impartial opinion and caſtigations of it (giving them entire liberty to alter or expunge whatever they diſliked;) and finding (by haſtily turning over the leaves) that their caſtigations were only enough to let me ſee, that they had heedfully read it over; and were accompanied with encouragements, which care was taken to keep me from looking upon as meer compliments: I let it go to the preſs, without ſo much as having once peruſed it, or heard it read over, ſince the ſtationer firſt gave me the advertiſement I told you of. Which I made the leſs ſcruple to do, becauſe a critic, whoſe judgment and piety I much reverence, ſeriouſly endeavoured to perſuade me, that I ought not in conſcience to decline publiſhing what he was pleaſed to think proper to kindle or cheriſh the flames of divine love in the breaſts of the readers. And my haſte itſelf did afterwards promiſe me theſe advantages, that notwithſtanding my book's not coming forth ſooner, I ſhould not loſe the excuſe of youth I had when it was written; nay, and that the faulty paſſages, which may be met with in it, will perhaps be charged upon thoſe, that ſuffered them to paſs uncorrected, when they had ſo abſolute a power to expunge or reform them. Theſe, and the like motives, having induced me to conſent to the publication of the following letter, I needed not deliberate long, to whom I ſhould addreſs it. For, ſince that accompliſhed *Lindamor*, whom it ſo much concerned, has left the world, there is no perſon in it, to whom this addreſs is any thing near ſo due, as unto you, dear ſiſter. It was at that delicious *Leeze*, where you are now the miſtreſs, that this letter was written; and it was of you that I borrowed thoſe hours I ſpent in writing it. It was to you, that I ſhewed it almoſt ſheet by ſheet, before I reſolved to ſend it away. It is you, that can beſt excuſe the imperfections of it, as knowing not only the more obvious, but the more private avocations and other diſadvantages, among which it was penned. It is you, that have ever ſince ſollicited me to divulge it, and have given me the greateſt encouragements to do ſo, not only by thoſe ſollicitations, which implied your own favourable opinion of it, but by procuring me (by concealing or diſguiſing my name) the unſuſpected opinions of divers competent judges. In a word, this addreſs belongs upon ſo many ſcores to you, that I could not make it any where elſe, without manifeſtly wronging you. I know, dear Siſter, that it is not uſual to dedicate books to ſo near a relation; and that it is uſual, in dedicatory epiſtles, both to depreciate what one has written, and to extol the perſon that one writes to, eſpecially if it be one of your ſex and quality. But you know too, that I never ſwore allegiance to cuſtom, and therefore will not, I ſuppoſe, wonder to ſee me as little ſolicitous to conform to it on this occaſion as on others. In an age, when ſo few perſons have merit enough to keep that from being flattery, which ſhould be but praiſe, I am not at all ambitious of caſting myſelf upon the unhappy neceſſity, of either flattering, or appearing rude; when by better chuſing the objects of my addreſſes, I can, as occaſion requires, give praiſes without untruth, or forbear them without incivility. Nor dare I preſume, that a lye ceaſes to be a fault, by being put into a dedicatory epiſtle; as antiently the hurtful beaſts forgot their pernicious nature, when brought into the ark. Not to mention,

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mention, that books of devotion have generally the ſtrange and unhappy fate of being leſs welcome to them, that moſt, than to them, that leſs need them. As for the diſcourſe I preſent you, though my opinion of it may be gueſſed at, by the privacy, to which I have ſo long confined it; yet becauſe, as in phyſic, to have a good opinion of the medicine, and the doctor, ſo in books of devotion (which are a kind of phyſic for the mind) to have one of the compoſure, or of the writer, both inclines us to look after the preſcriptions, and advantages their operations: for this reaſon I ſhall ſay nothing to diſparage a diſcourſe, which is of a nature to aim more at the being practiſed by the readers, than the being pardoned; and ſhall not at all repine to ſee it find, from the public, as favourable a reception, as it has hitherto met with in cloſets. And now, dear Siſter, though I know you will not, yet I doubt, my other readers will expect, that this letter ſhould, according to the mode of epiſtles dedicatory, be concluded with commendations of you, and compliments to you; it being almoſt as much out of faſhion in ſuch addreſſes to omit giving praiſes, as it is to believe the praiſes given on ſuch occaſion. But, though I can praiſe you without either diſbelieving myſelf, or fearing to be diſbelieved by any that knows you; yet, beſides that our relation, and our friendſhip, would make ſuch a way of writing miſbecoming me on all occaſions; I know your piety and your modeſty would peculiarly diſallow it upon this; where the ſubject I am to entertain you with is of ſuch a nature, as would make a flaunting addreſs but a very unſuitable introduction to it. The nature of my theme, as well as the ſtrictneſs of your virtue, and our friendſhip, forbidding me here to celebrate you, otherwiſe than by letting the world ſee, that I dare, even in a dedicatory addreſs, without fear of diſpleaſing you, forbear to celebrate you. And if it be demanded, why then I refrain not likewiſe from writing to you a dedication? I ſhall anſwer, that I am unwilling to loſe this opportunity of making a public acknowledgment, that as I have the honour to be your brother, and your friend, ſo I have the juſtice to think it very much my happineſs to be ſo: eſpecially, ſince you are pleaſed to aſſure me, that my relation to you has rather been the occaſion than the ground of a friendſhip, that does, whilſt it enjoins me to profeſs myſelf without compliment, oblige me to be, with all the ſincerity that is poſſible,

My dear Siſter,

Your moſt affectionate Brother, and

Moſt faithful, humble Servant,

ROBERT BOYLE.

Advertisements to the Reader;

(As they were written some years since, when some intelligent persons were to have a sight of the following Discourse.)

TO prevent those scruples, which else might arise in the perusal of the ensuing discourse, the reader of it is requested to take notice.

I. THAT though ignorance hath been of late so much in fashion with too many of our young nobility, that some passages of the ensuing papers may seem unfit to have been written to a young gentlemen; yet the person they were addressed to, being both a traveller, a linguist, and a scholar, it was not improper to discourse with him at the rate of supposing him so qualified.

II. THAT the ensuing letter was not written single, being but the last of divers, wherein love in general was confessed, justified, and celebrated; wherein the received way of making love was explicated, defended, and exposed; wherein constancy and inconstancy in love were argued for and against; wherein Platonic love was explicated, celebrated, and wherein the cure of love was proposed and prosecuted; wherein (to dispatch) the controverted points concerning love were discoursed of, in a way suited to the several themes, and to the humours and principles of the supposed writers. But all the former papers (written in compliment to a fair lady) though very free from the guilt of either licentiousness or profaneness, have been by the author judged too little serious, either to appear alone, or to accompany the ensuing discourse, wherein he has expressed his own opinions, as in the former he but deduced those of imaginary persons.

III. THAT the following paper was to have been attended by another, wherein the properties and duties of seraphic love were to have been explicated and insisted on; but the author, before he had written that other, accidentally meeting with a French book, wherein though what he had already discoursed of were not handled, he found, that much of what he had designed to set down was anticipated, and the most pertinent considerations his intended themes suggested, were skilfully displayed already; he feared it might be injurious to his reader, and himself, if he should either turn a transcriber of others notions, or decline the best thoughts belonging to his theme, that the less good ones might pass without suspicion for his own.

L'Amour Divin, de Mr. du Moulin.

IV. THAT the ensuing trifle having been written in haste, and by snatches, amongst distracting avocations, and far from literary accommodations, is freely submitted to the censure of learned and impartial perusers, especially divines; who are by the author requested to believe its mistaken passages, and unwary expressions (if it contain any such) to have been altogether unintentional, and such as they shall never find it to be more than one labour to make him discern and retract; he being resolved, by God's assistance, to continue still obsequious enough to all clearer discoveries of truth, to be able to say, *Errare possum, hæreticus esse nolo.* And as he is on his part so resigned, and so willing to be rectified, so he expects this equity from those, to whose judgments he submits, that, considering he intended not to write a solemn and regular treatise, but a private and familiar epistle, they will pass by such unaccuratenesses, as are wont to be incident to composers of this later nature. He presumed he might sometimes make bold with the vulgar laws of tenses, treating of an eternity,

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that admits them not. He hoped, that the freedom taken in ſome of his expreſſions concerning love, and the wonted objects of it, would be excuſed, by its being exacted by the deſign he had upon *Lindamor*, though he had not (which yet he has) otherwiſe accounted for it*. He intends not to adopt thoſe (too often erroneous) traditions of the vulgar, whence he ſometimes draws compariſons, that he may not deny himſelf the liberty taken by the generality of writers (without excepting the learneder ſort of them) who ſcruple not in popular compoſures to make ſimilies and alluſions grounded on popular traditions and perſuaſions. As for thoſe common places, which the method of our author's diſcourſe allowed him not to decline, he hopes the reader will diſcern by his way of writing, that he loves to uſe them, not as travellers, but as hunters uſe the highways: for the traveller, when he once gets into the road, makes it his buſineſs to keep in it; whereas the hunter, if he meets one lying in his way, is not fond of ſtaying in it, but nimbly croſſes it, or traverſes ſo much of it, as is requiſite to be paſt through in the purſuit of his game. And laſtly, if in ſome few paſſages of the following letter (eſpecially about the beginning of it) the expreſſions are leſs grave and ſolemn than the deſign; the author hopes, that even they will yet appear tolerably ſerious, for the airieſt parts of a letter, written by one young gentleman to another.

SOME

MOTIVES

TO THE

LOVE of GOD.

My dearest Lindamor,

I AM very much delighted to learn, both by the voice of fame and the information of much more credible relators, that *Hermione*'s cold uſage has cured you of the fever her ſcorching eyes had given you: and that when once you found yourſelf reſerved to ſhew, what wonder her eyes were able to perform, you ſeaſonably reſolved to become an inſtance of the power, rather of reaſon, than of love; and accordingly, did yourſelf the right to fruſtrate the vain hopes, your inſulting miſtreſs cheriſhed to manifeſt in you, that her charms were capable to make your flame perſevere, when her change had made it as well cauſeleſs as hopeleſs. I could wiſh indeed for your ſake, that you owed your cure more entirely to your reaſon, and leſs to your reſentment; that the extraction of your freedom may no ways blemiſh it. But ſince unallayed ſatisfactions are joys too heavenly, to fall to many men's ſhares on earth, I

* In a dedication of all the forementioned letters to my Lady *T. J.*

cannot

cannot but conclude, that your recovery, even on these terms, deserves I should congratulate it; for the French say truly, that *les plus courtes folies sont les meilleures.* And liberty being too high a blessing to be divestible of that nature by circumstances; I (that seldom deplore him, who by losing his mistress recovers himself) think, that *Hermione* has but intentionally, not eventually disobliged you; and hath made your flames a better return, by restoring you your own heart, than she could have done by exchanging hers for it. But that which not least endears to me your recovery, is, that I am assured by persons, from whom I dare credit even so welcome news, that my endeavours proved so happy as to be conducive to it, and that the considerations I ventured to present you, did at least so far contribute to your freedom, as to give you the desire and the design of regaining it. For, I hope, I need not tell you, that I seldom use endeavours, whose prosperousness is more welcome to me, than those that aspire to serve *Lindamor.* And though I cannot ever pay you any great services, in relation to my vastly greater desires, yet I can scarce do you little ones, in relation to the delight resulting from the having done you any. Nor has the joy, which this success of my discourses brings me, been sparingly increased by my having ventured them with much more desire, than expectation, of their prospering; and less out of any strong hope they would succeed, than out of an unwillingness to leave the means, I thought least improbable, unessayed; being invited to excite you to greater hopes than I durst allow myself for you, by the example of generals, who, whatsoever distrustful thoughts they harbour in their breasts, suppose, that (before the battle) to make their soldiers fight successfully, it is as well conducive as requisite, by encouraging orations, to make them think they shall do so. For, although I endeavoured indeed to persuade you, that reason being born sovereign of the passions; though her lenity or supineness do sometimes both occasion and permit their usurpations, she is seldom so divested of her native power, but that whensoever she pleaseth to employ what she hath left, she is able to resume what she hath lost. And though I was willing you should believe, that to perfect what your resentment had begun, was a task so easy, that the victory was as much in your power, as the resolution of attempting it; yet, notwithstanding all this, I say, I was once half persuaded, that to undertake the curing of a lover was the next weakness to the being one. And, *Lindamor*, to deal ingenuously with you, your recovery hath circumstances in it, that make me very apprehensive, that you are not yet out of the danger of a relapse, and that you have not half so absolutely abandoned your former amorous constitution of mind, as the former idol of it. I know, that from a person, who, for one that hath never yet been hurt by *Cupid* is accused of using him slightly and severely enough, you will expect endeavours to preserve you from relapses, by such dissuasions from love, as its votaries will scarce vouchsafe so mild a title to, as that of invectives against it. And I shall ingenuously acknowledge, *Lindamor*, that I have been sometimes no very unready satirist on that theme; and with a pen, relishing of the liberty I cherished in my heart, endeavoured to disabuse those servile souls, that being born to reason, so far degraded themselves, as to boast solely an excess of passion; and had such low and narrow thoughts of felicity and misery, as to expect either from a woman's usage. All which I thought I might the freelier do, because having never known the infelicities of love, but in the sufferings of others, I might probably suppose, that my declamations against it would pass for the productions of my reason, not my revenge. But, *Lindamor*, though the extravagances of some men's folly have been sometimes too great, to let me avoid laughing somewhat satirically at it; yet I am really too little an enemy to love, unless excessive or misplaced, by indistinct and disfiguring

disfiguring considerations, to represent to you the noblest passion of the mind, as its most hideous and formidable disease.

To love even with some passionateness the person you would marry, is not only Sect. 1. allowable but expedient, being almost necessary to the duty of fixing your affection, where you have once engaged your faith; for, it hath been observed, that love doth seldom suffer itself to be confined by other matches than those of its own making. And few, but they, that are so wise as they see cause to be almost in love before-hand with those they marry, prove so honest, as afterwards to be in love with none else. Since therefore the marriage of a wise man supposes at least as high a degree of love, as he is capable to cherish without forfeiting that title; I can scarce disallow the being moderately in love, without being injurious to marriage, which is a relation, that though I can with much less reluctancy permit others, than contract myself; yet dare I not absolutely condemn a condition of life, as expedient to no man, without which, even Paradise and innocence were not sufficient to complete the happiness of the first man. Thus you see, *Lindamor*, that I do not promiscuously quarrel with all sorts of love, but endeavour only to possess you with this truth, that as anciently among the Jews there were odoriferous unguents, which it was neither unusual nor unlawful to anoint themselves with, or bestow upon their friends; but there was a certain peculiar composition too of a precious ointment, which God having reserved to be employed in his own service, with that the perfuming of their friends was criminal and sacrilegious: so there are regulated degrees of love, which it is not forbidden to harbour for a friend, a mistress, or a wife; but there is too, a certain peculiar strain, or (if I may so call it) heroic temperament of love, which, wherever it is found, makes it belong, as unalienable, as justly unto God. A virtuous wife may love both her husband's relations and her own, and yet be truly said to love him with her whole heart; because there is a certain unrivalled degree of kindness, and a peculiar sort of love, which constitutes true conjugal affection, that she confines to him, and reserves entirely for him, and would think it criminal to harbour for any other person: so a religious soul may obey the command, of loving God entirely, though she allow her affections other objects; provided they be kept in a due subordination to, and kept from entering into competition with, that love, which ought to be appropriated to him; and which results chiefly from an either altogether, or almost unincreasable elevation, and vastness of affection; from an entire resignment to, and an absolute dependence on, the loved party; from a restless disquiet upon the least sense or doubt of her displeasure; from a greater concern in her interests, than one's own; from an expectation of no less than felicity or wretchedness from her friendship or indignation; or at least, a persuadedness, that nothing can be a greater happiness, than her favour, or deserve the name of happiness without it. For, wherever a passion has these properties, or any of them, conspicuous in it; it cannot, but by being consecrated to God, avoid becoming injurious both to to him and to itself. The very nobleness of it entitles him to it; as in some kingdoms (and particularly here in *England*) though veins of coarser metals may belong to the owner of the soil they grow in, yet all the mines of the more perfect metals (as gold and silver) are by the law made mines royal, and belong to the king, to whom their value appropriates them. By reflecting upon this peculiar notion of love, you may be pleased, *Lindamor*, to interpret such indefinite expressions as you may meet with in the following discourse. And this love I have taken the freedom to style *Seraphic Love* (not out of any affectation of tumid words or titles, but) borrowing the name from (if the Romish divines be good marshals of the heavenly host) those nobler spirits of the celestial

hierarchy, whose name * in the language to which it belongs, expresses them to be of a flaming nature; and whose employment (mentioned in the evangelical prophet's mysterious vision) sufficiently points at the divine object, to which the flames, that warm them, aspire and tend. And methinks, *Lindamor*, that you should find it no faint invitation to embrace Seraphic Love, that you may have the advantage, by making yourself a rival to these glorious spirits, to make them your friends, and the honour to be engaged in a service, where you are sure of such illustrious concurrents. At least if you be of the mind of that generous youth (to whose successful valour the conquered world was both theatre and trophy) who refused to run at the Olympic games, because there were no monarchs to run with him. But I fear, *Lindamor*, I have a little digressed; since I might have told you in fewer words, that it is not my design in this paper to declaim against love in general, or make a solemn harangue of the fickleness of women; and that therefore, as when young gallants (such as you, *Lindamor*) are subject to cast away their love upon unfit objects, their discreet friends (sensible of the truth of the Italian comic's observation, that

Isa. vi. 2, 3.

Onestà contra' amore
E troppo frale schermo
In giovinetto cuore;

In youthful hearts bare vertue's wont to prove
But a weak shield against the darts of love:)

without taking any more than necessary notice of their former fond and straggling passions, reclaim them by either matching them, or at least (in order thereunto) engaging their addresses to persons, whose beauty or prerogatives may both legitimate or confine their affections: so I shall now endeavour to prevent the future gaddings of your love to objects, that cannot deserve so transcendent and disinterested a one, as I have observed yours to have been; by preferring and engaging it to the true object, that passion was born to, the noblest it can aspire to, and the most satisfying it can enjoy. Yes, *Lindamor*, as it has hitherto been my not-unprosperous task to unhood your soul, I shall now make it my business to shew her game to fly at. I see, that love in *Lindamor* is too noble and predominant an affection, to be either easy or fit to be destroyed. It will therefore be my design, not to suppress your flame, but to address it. I wished it withdrawn from *Hermione*, not to annihilate it, but to transfigure it. I would not have a passion, which wanted nothing, but a due object, to be seraphic love, like vulgar men, be swallowed up by death, the common fate; but be ennobled, by a destiny like that of *Enoch* and *Elias*, who, having ceased to converse with mortals, died not, but were translated into heaven.

Sect. 2. WHAT has been said already, *Lindamor*, hath, it seems, sufficed to rectify your love; by disabusing it, and shewing you, how unfitly it was placed on its former objects. Your proficiency in that invites me to proceed with you to a new lesson, and (mindful of that true saying of an eminent father, *Nemo aliquem amat, quem non vult esse meliorem*) to endeavour to exalt your passion by directing and settling it upon an object, the due contemplation of whose lovelinese may cure as perfectly all hurts re-

* The same *Seraphim*, in Hebrew, springs from the root *Saraph*, which signifies to burn or flame: whence, *Numb.* xxi. 6. those pernicious creatures, that our translators english, *fiery serpents*, are styled in the original *Hannechashim Hasseraphim*.

ceived

ceived from any mortal beauty, as anciently the ſight of the myſterious ſerpent on the pole did cure the hurts the fiery ſerpents gave. For, ſince to gaze ſtedfaſtly on an outward beauty, where all your looking will but diſcover the ſame face, is found ſo effectual to kindle or to blow the fire of love (which the Greeks prettily enough expreſs by their ἐκ τοῦ ὁρᾶν γίνεται τὸ ἐρᾶν) how much muſt a due contemplation enamour us of that divine, and, though refulgent, yet ever more and more discoverable object, where attention and wonder ſtill mutually excite and cheriſh each other? whence the zealouſeſt and perfecteſt lovers of God are the glorious angels, of whom our Saviour ſays, that in heaven, *they always behold the face of his father which is in heaven*; and thoſe bleſſed ſaints, whoſe employment, and whoſe happineſs is, in the *Revelation*, expreſſed to be, *to follow the lamb whitherſoever he goes.* And thoſe unblemiſhed perſons are in that place recorded to have kept themſelves undefiled by mortal beauties, to teach us, that the nearer and clearer intuition, that heaven affords them of the glorious object of their paſſion, was not the cauſe of it, but the recompence; and that whilſt they lived exiles here on earth, it was ſuch a ſpeculation, as I am recommending *(Lindamor)* to you, wherewith they lived (to borrow the expreſſion uſed of *Moſes) as ſeeing him who is inviſible*; a ſight, whoſe glory made them look on fading beauties with as undazzled and untroubled eyes, as eagles can be ſuppoſed to caſt on glow-worms, when they have been newly gazing on the ſun.

Num. xxi. 8, 9. Mat. xviii. 10. Rev. xiv. 4. Heb. xi. 27.

To engage your love, *Lindamor*, to this ſublimeſt of objects, I ſhall deſire, that all that I have ſaid to divert your former flames, and all that may ſay to depreciate and diſcredit thoſe degrading and diſquieting amours, that are wont to inveigle mortals, may be looked upon as meant, rather by way of preparative, than of motive, aiming but to diſabuſe and rectify your mind, that with compoſed and unpreposſeſſed thoughts you may judge of the object I propoſe to you. Which to qualify you to be but impartial to, this method ſeemed expedient; being but imployed upon ſuch a ſcore, as that on which the Platoniſts uſed to premiſe certain virtues, which they (ſignificantly enough) called purgative, to diſpoſe and fit the mind for the reception of ſublimer truths. As phyſicians to clear the ſight are wont to purge the head, that the eye, freed from all darkening and diſguiſing tinctures, may the more perfectly diſcern the objects preſented to it. And certainly, the love I would perſuade, is of a nature, that makes nothing more conducive to it than the greateſt uncloudedneſs of the eye, and the perfecteſt illuſtration of the object; which is ſuch, that the clereſt reaſon is the moſt advantageous light it can deſire to be ſeen by. You know the ſtory of that witty wench, who uſed merrily to wiſh her lovers all good qualities, but a good underſtanding; for that, ſaid ſhe, would make them out of love with me. The divine object, to which I would prefer you, *Lindamor*, is ſo far from being obnoxious to a danger of that nature, that I need not diſparage any other to enamour you of this. For, the Creator is too full of excellences to need to be recommended or ſet off by the creature's defects. He was ſovereignly lovely in himſelf, before they had a being; nor has his giving us that paſſion, called love, made him the unfitter object for it. To love God, but becauſe your miſtreſs has perſonal faults, is a greater, than any of thoſe, for which you have declined her, and is ſuch a ſoloeciſm in ſeraphic love, as it were in the Perſian religion (for that has yet embracers in the Eaſt, as thoſe, that have there converſed with them, aſſure me) to make the ſpots, that blemiſh the moon, the motives of their worſhipping the ſun. *Hermione* is a lady, to whoſe noble relations I am too much a ſervant, not to have a reſpect for her; and I cannot without concluding her an extraordinary perſon, remember, how lately ſhe enjoyed the honour of captivating *Lindamor*; and though ſhe has ſince, by injuriouſly

Sect. 3.

declining, justly forfeited the glorious title of his mistress; yet the qualities, that gave it her, made me, among divers others, like her so well, that it is now my quarrel to her, that she is not what she was. And therefore whatever may fall from my pen to her disadvantage, relates to her but as she was, or may again be, an obstacle to your devotedness to seraphic love. To engage you to which (all this notwithstanding) *Lindamor*, I must do you the right to tell you, that God deserves the highest elevation of your love, though *Hermione* had not rejected it. Were she not only as handsome as you fancied her, but as constant as you wished her, her beauty and her friendship too concurring could not have made her other than as disadvantageous as injurious a rival unto God; and, in the very fruition of as obliging returns, as her kindness could have made your passion, she would have deserved but a more moderate degree of it. Angels, to whom women were never false or cruel, love their Maker above all things: and he to whom St. *John* presents his adoration, refused it for himself to direct it unto God. Would not yourself, *Lindamor*, have thought your *Hermione* absolutely handsome, though Moors had not worn the devil's livery; and the thing called deformity had been as much a stranger, as it is unwelcome to her sex? It is true, such is our frailty, that, as the Israelites needed a cruel and oppressive bondage in *Egypt*, to make them resolve on returning to the blessed land of *Canaan*; so oftentimes, the amorous soul needs the harsh usage of a disdainful mistress, to disgust it with its thraldom, and make it aspire to its more genuine and satisfying object. I may therefore allow *Hermione*'s defects to have given you freedom and occasion to consider the prerogatives of *Seraphic Love*; but I must not allow them to be the chief motives of your embracing it. For, it were too injurious to its glorious object, to make that but the refuge of a defeated passion, whose transcendency entitles it to more than the most elevated and entire one. Alas, God needs not these beggarly parallels with any fading objects, to become that of our disabused devotion; and he were lovely without comparison, as well as he is so above it. And though I be not so unjust to deny, that there are ladies (some of which I have the happiness not to be unknown to myself) whose excellencies are capable of ennobling their whole sex: yet their greatest accomplishments compared to his perfections, whose gifts they are, are, in that eclipsing company, as inconspicuous as the faint qualities of more ordinary persons. As when in a clear morning, the rising-sun vouchsafes to visit us, as well those bright stars that did adorn our hemisphere, as those dark shades that did benight it, vanish: consonantly whereunto, give me leave to observe to you, *Lindamor*, that though divers of God's attributes are through his goodness participated by his creatures, yet the scripture makes so vast a disparity betwixt the excellencies, that it ascribes to men, and the same perfections considered as they exist in God, that it seems absolutely to exclude created beings from any title to those attributes; because they possess them but in a way so inferior to that transcendent, peculiar, and divine manner, in which they belong to God. Thus our Saviour says to him, that (taking him but for a man) called him good, *Why calleſt thou me good? there is none good but one*, that is, *God*. Thus St *Paul* calls God (or Christ) the only potentate, μόνος δυνάστης, though the earth be shared by several potentates; and even the devout eunuch in the *Acts*, and the deposed grandees mentioned by the blessed virgin in her canticle, are, in the original, stiled potentates. Thus, there be wise virgins as well as foolish, and though our Saviour tells us, that *the children of this world are in their generations wiser than the children of light*; yet St. *Paul* scruples not to term his Maker, *The only wise God*: and thus he elsewhere paraphrases him, *He that hath only immortality*, though angels and human souls be deathless. In so incommunicable a manner does the superiority

Rev. xxii. 4. and xix. 1.

Mat. xix. 16, 19. 2 Tim. vi. 15. Acts viii. 27. Luke i. 52. Mat. xxv. Luke xvi. 8. 2 Tim. i. 15. 1 Tim. vi. ib.

riority of God's nature make him possess those very excellencies, which the diffusiveness of his goodness makes him pleased to communicate. I am the more zealous, *Lindamor*, to transfigure your love into devotion (which I must desire you to look upon but but as a varied name for *Seraphic Love*) because I have observed your passion to have been extremely impatient of confinement, and to have esteemed whatever may be termed limits, to be prisons. Few therefore can need more, or deserve better, an object for their love, for which too immense a vastness were impossible. And such a one is God, whose sovereign perfections render him so uncapable of being loved too much, that the most aspiring passion can scarce arrive (so much as) to lessen its disproportion to the object. Other passions, like other rivers, are most liked, when they calmly flow within their wonted banks; but of *Seraphic Love*, as of *Nilus*, the very inundations might be desireable, and his overflowings make him the more welcome. For mortal beauties, our passions are like ourselves: if our statute chance to exceed a certain size or standard, it makes us monstrous; but devotion is like a flawless diamond, where the bigness taxes the value, and the unusual bulk both rates and inhances the lustre and the price. To give God all our love is the greatest command both of the law and gospel (in its capacious and teeming womb, both comprizing and cherishing all the other services God requires) and that there is not more exacted of us, is not, that an addition were culpable, but because it is impossible. So noble is the nature of devotion, that it admits of failings but by one of the extremes, which is that of defect. For, mediocrity (whose office it is to restrain us from approaching the utmost limits) which in other passions is an excellence, is here an imperfection. Or, at least, if mediocrity be that, which creates passions virtues, the mediocrity of this love must consist in the boundlessness of it, since that is it, which makes it most a virtue. The man after God's own heart, is not afraid to own, even to his Maker, an ardency of love for him, which must be expressed by the significant metaphor of thirst; and that such a thirst too, as makes the panting hart (by naturalists observed to be a very dry creature) *bray* (as I remember the Hebrew hath it) for those refreshstreams, whose want distresses and reduces her to an almost gasping condition: *My very soul* (saith he) *thirsteth for God.* And we know, that thirst is not only so violent an appetite, that it lessons the wonder of that monarch's bargain, whom history records to have parted with his kingdom for a cup of water; but thirst doth so confine our longings to what it craves, that nothing else can satisfy them. The wealth of both the *Indies* would not excuse the want of a needed cup, supposing their possessor tormented with an appetite, which cannot be quenched but by drink. To which I must add, that the uneasiness of unrelieved thirst is not, like that of other inconveniences, lessened by continuance, but grows by lasting the more unsupportable. The same inspired poet scruples not also to profess so sensible and so active a concern for God's interests, that the *zeal of God's house had eaten him up*; and hugely troubled he is, that others are not affected with the same zeal. *I beheld* (says he) *the transgressors, and was grieved, because they kept not thy word.* Nay, *rivers of waters*, says he, *run down mine eyes, becouse they kept not thy law.* And to manifest how much the tenderness and unreservedness of his love made him think those his friends or enemies, that were so to God; *Mine eyes*, (says he) *shall be upon the faithful of the land, that they may dwell with me: he that walketh perfect in the way, he shall serve me. Do not I hate them, O Lord, that hate thee, and am not I grieved with those, that rise up against thee? I hate them with a perfect hatred, I count them mine enemies.* At this rate did pious *David* love his Maker; but he was so far from thinking this rate excessive, that, transported by the sense of his personal disability to pay that divine object all the love, that

Psa. xlii. 1.

Cervina caro sicca est, &c. *Serrare.* Insti. De Alimentor. facultatibus, *lib.* 4. *part* 1. *cap.* 3. *Vers.* 2.

Psal. cxix. 15.

Ibidem vers. 136.

Psal. ci. 6.

Psal. cxxxix. 21, 22.

that his perfections merited, he is not content to rouse up all his own faculties to praise God (*Bless the Lord, O my soul, and all that is within me bless his Holy Name*) but he invites all the godly to assist him in the payment of so vast a debt; *Love the Lord, all ye saints, for*, &c. And again, *Praise the Lord, all ye nations, praise him all ye people.* And not content neither frequently to do this, as may appear by very many passages of his sacred poems, he extends his invitation to the angels, and all the other hosts of God, and concludes the book of *Psalms* with a, *Let every thing, that hath breath, praise the Lord.* Hallelujah.

Psal. ciii. 1. Psal. cxvii. 1. Psalm cxxxvii. 2.

Sect. 4. Nor does it invalidate what has now been delivered, that some men have, even by devout persons, been blamed for too much devotion; for, it was not an excess of love, but a want of discretion, that was guilty of their faults: the expressions of our love to God ought to be regulated, not by our blind and wild fancies, but by his revealed will (as Christ says, *if you love me, keep my commandments.*) And therefore it is very possible to be too devout, not because any expression of *Seraphic Love* can be made with too much ardency, whilst it is considered abstractedly in itself, and irrelatively to the rest; but because that there being several duties of love, which require an ardency of it, it is injurious to exercise all that in one alone, or a few, that belongs equally to the neglected others. We must not (as too many professors are now wont to do, of whose error you may receive a fuller account in some other papers) dash in pieces the two tables of the law against one another; but must so *love God with all our hearts, as to love our neighbour as ourselves.* You know what our Saviour saith to the Pharisees, that tithed mint and cummin, with a neglect of judgment, mercy, and faith, those weightier matters of the law; *These ought you* (ἔδει) *to have done, and not to leave the other undone.* And indeed this partiality, *Lindamor*, which makes us display so much of the strength and vigour of our spirits in some favourite duties, that we can but languidly and perfunctorily perform those others we are less fond of, begets in devotion a disease, not unlike that new one in children we call the rickets, which some learned physicians do not improbably conceive to raise from the unequal nutrition of the parts. For though none of them receive excessive nourishment, yet some of them receiving as much as is convenient for them, and thereby growing up to their natural bigness, whilst others are less nourished than (were the body healthful) they would be, do grow so little, that the founder parts seem overgrown, and so the disproportion betwixt them and the ricketing ones makes the whole body they compose mishapen and unwieldy. But, *Lindamor*, this proves not, that we can love God too much, but only that we may employ too much of that love, in this or that way of expressing it. Whilst we are (as *Job* speaks) *Inhabitants of these houses of clay*, there are many duties, which do as well challenge an intensity of our affections, as those which relate more immediately to God. As St. *Paul* tells us, that there is difference betwixt married and single persons, the affections of the one being at liberty to devote themselves more undistractedly to God, whereas those of the other are distracted; (as *Adam*'s were betwixt his Maker and his rib). But where a direct and immediate expression of love to God defrauds not any other duty, there it is free from the danger of excess. Though prayers may easily be too long, and fasts grow exorbitant; yet Christ could spend the whole night in prayer, and fast forty days without immoderateness, when the other expressions of his love to his Father, and the other exercises of his mediatory function, were not thereby disturbed, but furthered and promoted. And so *Elijah* might inculpably fast long, when that fasting did not disable him to prosecute his journey to the mount of God; and though just men here on earth must express their love to their master by (that busy distracting,

Mat. xxiii. 23.

Job iv. 19.

1 Cor. vii. 32, 33, 34.

1 Kings xix. 2.

diſtracting, and remoter way of ſervice) trading with his talent truſted to them; yet when their diveſture of mortality diſpenſes them for thoſe laborious and avocating duties to diſtreſſed Chriſtians, and their own ſecular relations, which are here requiſite to be performed; their glorified ſpirits may now, without any immoderate devotion, employ, I ſay not, their time, but their eternity itſelf, in converſing with God, and following the Lamb whitherſoever he goes. And congruouſly I obſerve, that the four myſterious beaſts, allowed to approach neareſt to the throne of God, though their many wings, and more numerous eyes, intimate them of a very active nature, are repreſented to us in the apocalypſe, as addicted but to one employment, ceaſing *neither day nor night from ſaying, Holy, Holy, Holy, Lord God Almighty*; and from *giving glory, and honour, and thanks unto him.* And of *thoſe that have whited their robes in the blood of the Lamb*, this account is in the ſame book given us, *that they are before the throne of God, and ſerve him by day and night in his Temple.* So true is it, that no degree of *Seraphic Love* can be exceſſive; nay, not any expreſſion of it immoderate, unleſs it be made ſo, not by its greatneſs, but by its uſurpation, whereby it either ingroſſes or invades what belongs to its injured and languiſhing aſſociates. Our love unto the creatures is a preſent, but unto God it is a tribute; and though we may eaſily play the prodigals in parting (over-freely) with our gifts, we can ſcarce be ſo in the payment of our debts; for, be the ſums never ſo vaſt we pay away, their being due, in ſpight of their being great, makes the diſburſement too much an act of juſtice, to be one of profuſeneſs. *Seraphic Love* (whoſe paſſionateneſs is its beſt complexion) has then moſt approached its nobleſt meaſure, when it can leaſt be meaſured; nor aught its extent to admit any other limits, than an utter diſability to exceed thoſe that terminate it. For he alone loves God as much as he ought, that, loving him as much as he can, ſtrives to repair the deplored imperfection of that love, with an extreme regret to find his love no greater. Such a ſublimity of love will beſt entitle you to the conſolation accruing from that memorable paſſage of St. *John*, where he ſays, that *God is love, and he that dwelleth in love dwelleth in God, and God in him*; which ſupplies me with a forcible inducement, to invite you to an eager aſpiring to a tranſcendency in devotion, ſince it may render ſelf-denial ſo eaſy, that it will (at laſt) almoſt diveſt that name. For this ſublimer love being, by an intimate conjunction with its object, wholly devoted to it, and throughly refined from all baſe droſs of ſelfiſhneſs and intereſt, nobly begets a moſt ſtrict union of our wills with God's (or rather) a perfect ſubmiſſion of the one to the other. And thus, when it is become your will to obey his, no diſpenſations of providence will immoderately diſquiet you: for, you poſſeſs your wiſhes in general, and in bulk, though poſſibly not always in retail. For, your chiefeſt deſire being to ſee your Maker's will fulfilled, your knowledge of his being the ſovereign and uncontrouled diſpoſer of the events aſſures you, that all accidents, that can befal you, are but exact accompliſhments of his will, and conſequently of yours, ſo far forth as that is included and comprized in his. When you have reſigned, or rather conſigned, your expropriated will (if I may ſo call it) to God, and thereby (as it were) entruſted him to will for you; all his diſpoſals of, and his diſpenſations towards you, are in effect the acts of your own will, with the advantage of their being directed and ſpecified by him: an advantage, that does at once aſſure you both of their rectitude and ſucceſs. God's wiſdom, power, and love to you conſidered, how much more happy muſt you be in your opinions of his chuſing for you, than your immediate own? The patient thinks himſelf obliged to gratify his phyſician for chuſing for him what ſorts of meat he is to feed on; though the doctor be wont to make ſuch a choice for him, as deprives him of the

Rev. iv. 6, 7.

ver. 8.

Rev. vii. 14, 15.

1 Joh. iv. 16.

dishes he best likes, and oftentimes confines him to those he loaths. Alas, how often might God say of our requests, as Christ did of those of the two aspiring disciples, *Ye know not what to ask?* I admire and blush to read in an heathen satyrist so heavenly a lesson, as,

Permittes ipsis expendere numinibus, quid
Conveniat nobis, rebusque sit utile nostris:
Nam pro jucundis utilia quæque dabunt dî.
Charior est illis homo, quam sibi: nos animorum
Impulsu, & cæca pravaque cupidine ducti,
Conjugium petimus partumque uxoris; at illis
Notum, qui pueri, qualisque futura sit uxor.

'Unto the wiser gods the care permit,
'Of what's for us and our affairs most fit.
'They will for pleasant things the best confer;
'To whom man is than to himself more dear,
'We by our blinder passions led astray,
'Do for a wife perhaps or children pray;
'Which they may chance refuse us out of love,
'Knowing what both the wife and boys would prove.'

The consideration of which made a heathen philosopher say, that he was wont only in general terms to beg good things of the gods, leaving it to them to determine what things were good for him. And indeed our own wishes are but too commonly as blind as *Rachel's*, who having so eagerly longed for children, that she impatiently cries, *give me children or else I die*, died in child-bearing; and as destructive to the wishers, as their longings proved to the murmuring *Israelites*, who loathing the wholesome manna (that bread of angels) God had provided for them, are their own bane in the flesh they had so greedily lusted for. Thus, *Lindamor*, that so affrighting virtue of self-denial proves to be little more than a son's letter of attorney to his father, of whose paternal kindness and consummate abilities in the management of affairs his confidence amounts unto a certainty. Nay, till my second thoughts checked the over-forward impetuosity of my first, I was about to add, since God resents an infinite satisfaction in the accomplishment of his own will, your making over your whole will to God will impart to you that felicity, proportioned to the degree of the resignment. And as the eye, whilst by the optic nerve tied unto the head, so changed, can taste delights, which it is dead to, being once severed from it, though (otherwise) it enjoy the best condition of which its inanimate nature can be supposed to be capable; so may your will, by an identity or sameness (in tendency, though not in nature) with your Maker's, as it were engrafted into God's, receive a new and enlarged capacity, which will enable you to contain and relish joys, highly transcending those, which the fullest fruition of your private wishes were able to create. Thus self-denial is a kind of holy association with God, and (by making you his partner) interests you in all his happiness and acquisitions. And consonantly we see, that glorified saints and blessed angels, whose will have the most exquisite and exact conformity to God's, enjoy a happiness most approaching his; whereas the apostate spirits, in a confirmed repugnancy to his will, find the extremity of wretchedness.

Gen. xxv. 3.

Num. xi. 33.

But

But though I dare not own, *Lindamor*, so bold a sally, yet I dare without scruple improve the discourse that preceded it, to make out to you an advantageous difference of *Seraphick Love* from ordinary flames. For, he that makes a present of his heart to any mortal beauty, even by her welcoming it, and lodging it with her own, grows subject to have it wounded in her breast. Those misfortunes reach him, that would otherwise terminate in her: her afflictions torment him, whilst his own reprieve him; and the felicity of two persons grows requisite to make one happy. The letting out our love to mutable objects doth but enlarge our hearts, and make them the wider marks for fortune, and capable of being wounded in more places: for, although love may as well make us participate the joys, as resent the infelicities of the parties loved; yet even the least unhappy persons do in so fickle and so tempestuous a sea, as we all find this world, meet with so many more either cross winds, or stormy gusts, than prosperous gales; and we are so much more sensible of pain than pleasure (an aching corn, though less than a sickness, unfitting us to relish the otherwise perfect health of the whole body) that even friendship itself, though a much calmer affection than love, ought to be declined as injurious to our quiet, did we consider it but as a partnership of fortunes, not an exercise of virtues. But he, whose wiser love settles itself on God, is not only, by the immutable, and even essential happiness of that adorable object, secured from participated infelicities; but finds his personal crosses and distresses sweetened by considering, that what he most loves is most happy, and as able as willing in due time to make him so. And though *Seraphick Love* make us partake but God's felicities, yet his acceptance of it makes him resent our sorrows. *In all their afflictions he was afflicted*, says the prophet of God, and of the Israelites. And so the Son of God (who is so much one with those that love him, that both he as the head, and they as the members, are sometimes, as making up one body, called by one name, Christ) though as high as heaven, above the reach of personal or immediate persecutions, called out to *Saul* for an intention of harming those that loved him, *Saul, Saul, why persecutest thou me?* and to demonstrate the tenderness of this compassion, the prophet says to the returned Israelites, concerning God, *He that toucheth you, toucheth the apple of his eye.* Nor is God's compassion like a mistress's, a grieving only, and an useless pity, whereby the suffering lover is oftentimes less comforted, as it proceeds from her kindness, than afflicted, because it breeds her disquiet; but God's is a compassion, though active, yet serene, and worthy of himself, which without producing the discomposure, produces the effects of the most sensible pity, by engaging him to a timely relief and rescue: as that freshly mentioned expression, *In all their afflictions he was afflicted*, is immediately followed by, *and the angel of his presence saved them; in his love, and in his pity he redeemed them, and he bare them and carried them all the days of old.* Yes, this pity, for its not disturbing God's happiness, inclines him not the less to express a sense of our miseries, and makes us find (to use a scripture phrase, as I would render it) χάριν εἰς ἴυκαιρον βοήθειαν (*grace for an opportune relief,*) I say, with the divine writer, *an opportune* or *seasonable relief*; because it comes not always when it is most desired, but when it is most fit; and when that is, he that hath at once all present, past, and future things in his prospect, is fitest to determine. Christ's words to his disciples, *It is not for you to know the times or the seasons, which the Father hath put in his own power*, are applicable to more cases than that which occasioned them. The Canaanitish woman must put up a refusal, and the reproachful name of a dog (which yet, by the way, was a paraphrase commonly enough used by the Jews of the heathen, and as such was understood by this Ἑλληνίς Gentile not Greek *.) The Israelites (comprising the patriarchs

Sect. 5.

Isai. lxiii. 3.

1 Cor. xii. 12.

Zech. ii. 8.

Heb. iv. 16.

Acts i. 7.

Mat. vii. 27.

* The text refers her extraction to *Syrophœnicia*, the same region with *Canaan*.

 their

their progenitors) were reduced to wait four hundred and thirty years, ere they were
Gal. iii. 17. introduced into the promised land; and during a great part of that long space of time, languished and groaned under the heavy burdens, and other as heavy pressures
Ezekiel xxviii. 24. of the as cruelly as unsuccessfully politick Egyptians. St. *Paul* himself prayed the
2 Cor. xxii. 7. Lord thrice for the removal of that rude σκόλοψ τῇ σαρκὶ, *thorn to the flesh* (whatever
Luke ii. 48. that may mean). Nay, of the blessed virgin-mother herself, her divine Son would not be found till the third day, though she sought him sorrowing. And *Lazarus*, to whom, even during his sickness, he vouchsafed (a title, to which all *Cæsar*'s were
Joh. i. ver. 3. but trifles) the style of friend, which emboldened the pious *Mary* to paraphrase him by a *he whom thou loveſt*, was permitted not only to lie a dying, but to die; his rescue being deferred, till it was thought impossible, and was so indeed to any less power than omnipotence. Which manifests, that as no degree of distress is unrelieveable by his power, so no extremity of it is inconsistent with his compassion, no not with
Mal. iii. 3. his friendship. He, whose spirit inspired the prophets, is in the last of them represented under the notion of a refiner: and it is not the custom of refiners to snatch the beloved metal out of the fire, as soon as it feels the violence of that purifying element; nay, nor as soon as it is melted by it, but they let it long endure the brunt of the active flames, actuated by exciting blasts, till it have stood its due time in the fire, and there obtained its full purity and splendor. And I hope, you will give one, that converses with furnaces, though he be no pretender to the philosopher's stone, leave to implore a chymical metaphor, and observe, that though in afflictions, especially national or publick calamities, God oftentimes seems to make no distinction betwixt the objects of his compassion, and those of his fury, indiscriminately involving them in the same destiny; yet his prescience and intentions make a vast difference, where his inflictions do not seem to make any: as when on the same test, and with the selfsame fire, we urge as well the gold, as the blended lead or antimony; but with foreknowing and designing such a disparity in the events, as to consume the ignobler minerals, or blow them off into dross or fumes, and make the gold more pure and full of lustre.

Sect. 6. It is true, *Lindamor*, and (not to be suspected of partiality towards a love, which so little needs it to be thought fit to be preferred before all other passions) I shall acknowledge it, that the happiness resulting from those many prerogatives I have endeavoured to discover to you in a transcendent degree of *Seraphick Love*, is moderated by the effects of that sublimity, the eager desires it creates of a more complete fruition, of its perfect and divine object. Such aspiring sallies of the longing soul
Cant. ii. 5. made the languishing spouse in the *Canticles* cry out, *Stay me with flagons, comfort me*
Phil. i. 23. *with apples, for I am sick of love.* Such made the ravished apostle desire to return
* Luk. xii. 36. (for so I should rather translate the word, ἀναλῦσαι, there, and so I find it * elsewhere to signify) *and to be with Christ:* and the inspired poet thus expresses his longings to
Psal. xlii. 1, 2. the blessed object of them, *As the hart panteth after the water-brooks, so panteth my soul after thee, O God. My soul thirsteth for God, for the living God: when shall I come and appear before God?* But, *Lindamor*, it was fit, that to elevate our thoughts and wishes to heaven, some peculiar, and elsewhere incommunicable degree of joy should be reserved for us there. And it is a good sign, and such as worldly objects cannot boast, when the incompleatness of our *Seraphick Lover*'s happiness, in his fruitions, proceeds not from their want of satisfactorinesſ, but his want of an entire possession of them. And let me tell you, *Lindamor*, that even this uneasy state of separation is sweetened with as much allay as is consistent with its being a grief. For, the divine evidence and teacher of God's love pronouncing a thirst after perfection to be a title
Mat. v. 6. to it (according to those Scriptures, *Blessed are they that hunger and thirst after righteousness;*

ousness; for they shall be satisfied: and, *Let him that is athirst come, and whosoever will, let him take of the water of life freely)* and the joys of heaven being so vast, that they diffuse their nature to all the grounded hopes men have to obtain them; each new assurance is a new degree of them, and is acceptable to our hope, though uneasy to our desires. And these baitings at compleat felicity should not be more unwelcome for the present disquiet they suppose, than the contrary for the zeal they argue, and the felicity they promise. For this production of the spirit in our hearts may be justly termed as the Spirit himself in scripture is, an earnest; which though, by being such, it confesses itself not to be the entire sum, yet it is not only a part of it, but a pledge: and, *Lindamor*, how supportable is this thus qualified allay of the joys of *Seraphick Love*, in comparison of the disquiets, and the torments, that are wont to attend sensual love? I shall not lose [time] to enumerate how many it is supposed to have sent to their graves; because, though I find those tragical stories rife enough in romances, yet I find them rarities every where but in those fabulous composures: and though I have had the curiosity to visit some of those warmer regions, where the flames of love are thought to burn with more violence, yet (bating the duels and the pox) I remember not to have observed love to have ever been the death of any man; unless, speaking like philosophers, who make reason the essential constituent form of a man, we will affirm, that love, by dethroning reason, though it leave the lover alive, doth kill the man. But, though I am loth to put so bad a compliment upon mankind, as to say, that love is wont to destroy men's lives; yet I think it would be no calumny to say, it much disquiets them. I could ask you, how long many a lover must continue a servant, to purchase the honour of being taken notice of to be so? And I could recruit that question with pretty store of others of the like nature; but that I suppose your memory will save my pen the labour of representing to you the torments of love, which they that feel them, would little less justly than they do frequently style martyrdoms, if the greatness only, without the cause and object of men's sufferings, sufficed to make them martyrs. And though the condition of lovers be in romances so dextrously and delightfully described, that not only sanguine readers are transported, but even I myself have been surprized into inclinations, to admire and envy their felicity; yet when some, I was concerned for, have been really concerned and engaged in some adventures, my envy quickly turned into pity. For, the repulses, the regrets, the jealousies, the fears, the absences, the despairs, and the rest of the afflicting disquiets of lovers; though in well-writ romances they are soon read over by the diverted pursuer, yet they are not so soon weathered out, nor so easily supported by the disconsolate lover; whose infelicities, though they may be perhaps so handsomely deplored, as to delight the reader, yet trust me, *Lindamor*, it is a much happier condition to be free from misfortunes, than to be able to complain eloquently of them. And as I have with delight beheld a storm excellently drawn by some rare artist's pencil; but when I was this spring tost by the rude winds, that blew me out of *Holland*, I found a real storm a very troublesome and uneasy thing: so the condition of a lover, though drawn by a smooth pen, is wont strangely to affect and please us; yet when men are really engaged in it, they find it full of hardships and disquiet. It is a much better condition to be looked on than embraced; and experience gives men of it (sadder and) more unwelcome notions than description did. Nor fancy, *Lindamor*, that the troublesomeness of your sufferings in love proceeded but from their not being acceptable to her, for whom you endured them; for had your mistress crowned them with myrtle, and proved as kind to you as *Hymen* could have made her, yet, I fear, she could have recompensed you but by disabusing you, and could not

Rev. xxi. 17.

2 Cor. v. 5.

have freed you from the need of happineſs, but only from a miſtake concerning it. For, methinks, *Lindamor*, moſt of theſe tranſitory goods, that we are ſo fond of, may not unfitly be reſembled to the ſenſitive plant, which you have admired at *Sion-garden*; for as, though we gaze on it with attention and wonder, yet, when we come to touch it, the coy deluſive plant immediately ſhrinks in its diſplayed leaves, and contracts itſelf into a form and dimenſions, diſadvantageouſly differing from the former; which it again recovers by degrees, when touched no more: ſo theſe objects, that charm us at a diſtance, and whilſt gazed on with the eyes of expectation and deſire; when a more immediate poſſeſſion hath put them into our hands, their former luſtre vaniſhes, and they appear quite differing things from what before they ſeemed; though after deprivation or abſence hath made us forget their emptineſs, and we be reduced to look upon them again at a diſtance, they recover in moſt men's eyes their former beauty, and are as capable as before to inveigle and delude us. I muſt add, *Lindamor*, that when I compare to the ſenſitive plant moſt of theſe tranſitory things, that are flattered with the title of goods, I do not out of that number except moſt miſtreſſes. For, though I am no ſuch an enemy to matrimony, as ſome (for want of underſtanding the raillery, I have ſometimes uſed in ordinary diſcourſe) are pleaſed to think me, and would not refuſe you my advice (though I would not ſo readily give you my example) to turn votary to *Hymen*; yet I have obſerved ſo few happy matches, and ſo many unfortunate ones; and have ſo rarely ſeen men love their wives at the rate they did, whilſt they were their miſtreſſes; that I wonder not, that legiſlators thought it neceſſary to make marriages indiſſoluble, to make them laſting. And I cannot fitlier compare marriage than to a lottery; for in both, he that ventures may ſucceed, and may miſs; and if he draw a prize, he hath a rich return of his venture: but in both lotteries there is a pretty ſtore of blanks for every prize. And for your particular, *Lindamor*, the world is much miſtaken in both your humours, if *Hermione*'s and yours be not ſo unſuitable, that (to make haſte from ſo nice a ſubject) had ſhe juſtified your expectation of her kindneſs, you would have poſſeſſed the perſon without poſſeſſing the happineſs you expected; and might have found yourſelf as ſenſibly diſappointed by her grant, as you were by her change.

But I forget, *Lindamor*, that I reſolved not to inſiſt on parallels; and therefore, inſtead of proſecuting the diſcourſe my pen has ſlipt into, concerning the advantages of *Seraphick*, compared with ordinary love, I ſhall venture to encourage you to the former, by ſhewing you, that your paſt addictedneſs to the latter may prove ſerviceable to you in it. Yes, *Lindamor*, I ſhall not ſcruple to tell you, that your paſſion for *Hermione* may not a little facilitate your devotion, partly by breaking all the chains, excepting one, that faſtened your affection to unſatisfying objects, and reſtrained it from ſoaring to the ſublimeſt; and partly, by exalting your paſſion to a height fit for ſeraphick flames. For love hath this of noble, that it makes us diveſt ourſelves of ſelfiſhneſs, ſlight fortune, quiet, ſafety, honour, life, and all our own concernments, when their coming into competition with the loved party's intereſt, may render their ſacrifice acceptable to her; and make us think goods or ills deſerve thoſe names, but as they come to us from or through her. You could ſcarce have learned a better leſſon, even from a much better maſter; for love having thus accuſtomed you to (what is thought moſt difficult in that virtue) the acts of ſelf-denial, you need almoſt but transfer your flames from an inferior and miſtaken to their true and nobleſt object, and you will have exalted and refined your love into devotion; to the latter of which a ſublime elevation of the firſt is ſuch a diſpoſition, as the having formerly by looſſer airs and (perchance) wanton ſongs, learned to improve and to command one's voice, is,

is, to the skill of singing those devout hymns and heavenly anthems, in which the church militant seems ambitious to emulate the triumphant, and eccho back the solemn praises and hallelujahs of the celestial quire. And, as by hunting, though (possibly) we follow but some poor fugitive hare, or some such trifling game, we gain that vigour, that sufferance, and agility, that fits us for the toils and military hardships that are exacted in the pursuit of glory, and of empire: so though in love (devotion's prenticeship) the courted creature be often considerable enough to make our elections fit to be numbered among such as those that made love be painted blind; yet in the progress and conduct of our passion, we contract such disinterested and resigned habitudes, as, being preferred to serve celestial objects, do excellently qualify us for devotion. And in effect, a fervent love seems little else than devotion misaddressed, where our own very expressions may serve to disabuse us: for, when you give your mistress the style of goddess, and talk of nothing to her, but offering up of hearts, adoring, sacrifices, martyrdoms; does not all this imply, that though it be said to her, it is meant to a divinity: which is so much the true and genuine objects of men's love, that we cannot exalt that passion for any other, without investing it with the notion and attributes of God? as children disclose the inbred kindness they have for those persons, by calling the babes they most doat upon by their dear mother's, or loved nurse's names. And as *Aaron*, and the revolting Jews, by justifying to themselves their adoration of the idols they had set up, by attributing the title of God to what they adored, did tacitly acknowledge adoration to be due only to the Deity; so does a lover, by naming what he worships a divinity, tacitly confess the deity to be the proper object of that highest and peculiar strain of worship.

Ex. xxxii. 4, 5. *In the latter of which it is expressly said that the feast was proclaimed* יהוה *To Jehovah, the peculiar name of the true God.*

And this truth, *Lindamor*, the very fickleness of lovers concurs to testify: for, what men call, and think inconstancy, is nothing but a chace of perfect beauties, which our love fruitlesly follows and seeks in several objects, because he finds it not entire in any one. For creatures have but small and obscure fragments of it, which cannot fix, nor satisfy an appetite, born for, and (though unwillingly) aspiring unto God; who is proclaimed the true and proper object of our love, as well by men's fickleness to women, and the angel's constancy to him. Just as the trembling restlessness of the needle, in any but the north point of the compass, proceeds from and manifests its inclination to the pole; its passion for which both its wavering and its rest bear equal witness to. That unsatisfiedness with transitory fruitions, that men deplore as the unhappiness of their nature, is indeed the privilege of it; as it is the prerogative of men not to care for, or be capable of, being pleased with whistles, hobby-horses, and such fond toys as children doat upon, and make the sole objects of their desires and joys. And by this you may, *Lindamor*, in some degree imagine the unimagineable suavity, that the fixing of one's love on God is able to bless the soul with; since, by so indulgent a father, and competent a judge as God himself, the decreed uncontentingness of all other goods is thought richly repaired by its being but an aptness to prove a rise to our love's settling there. *Sect. 8.*

And hitherto, my dearest *Lindamor*, I have endeavoured to recommend unto you *Seraphick Love*, by mentioning some of its properties, which seem to relate more to the love itself, than to the divine object of it. But I fear you'll think I have too long entertained you with considerations, which, besides that they are not altogether the importantest that belong to this discourse, I have been by haste reduced to pen in the unaccurate order, wherein they offered themselves to my thoughts, not the method, wherein I should have presented them to yours. *Sect. 9.*

And

Sect. 10. And therefore, *Lindamor*, since the noblest and supreme motives to the love of God consist in his own infinite perfections and prerogatives; and since the properties of God's love to us do advantage us much more (and consequently are likelier to endear devotion to us) than those of ours to him; the former not only moving God to kindle in us, but to cherish and foment, and (if our own wilful extinction interpose not) to crown the latter: for both these reasons, I say, *Lindamor*, I doubt not, but you'll think it seasonable for me to proceed, to consider that higher sort of motives to devotion, and to evince, that the several things, which are wont most to engage and heighten our affections, do, in a peculiar and transcendent manner, shine forth and constellate in God. That, you know, which enamoured you of *Hermione*, I need not prove to you, to have been your supposing her full of loveliness and excellencies in herself; and your believing, that the love she vouchsafed you was great, free, constant, or advantageous to you. And that all these properties do not only eminently exist, but illustriously concur in God, and his love, I must now, *Lindamor*, (with strong desires of doing it prosperously) attempt to manifest.

Sect. 11. First then, our highest love is made God's due by the excellency and prerogative of his nature. But, trust me, *Lindamor*, when necessitated (by a method exacted by the nature of this discourse) I find myself engaged to say something by way of celebration of God's perfections; I am very sensible, I can but detract from what I desire to praise, and must inevitably appear unable to speak worthily of a theme, to which even Seraphim themselves cannot do right. And if, as the scripture assures us, those things never fell under the senses, nor entered the thoughts of men, which God
1 Cor. ii. 9. has reserved for those that love him; how ineffable and incomprehensible must those things be, which he has reserved for himself? the infinite superiority of his nature, above all created beings, placing a vast disparity betwixt his greatest communicated vouchsafements, and his boundless, and therefore to creatures incommunicable, perfections.

Wonder not therefore, *Lindamor*, that my weak eyes dare not dwell long upon an object, which they cannot stedfastly gaze on long without being dazzled; and do not marvel, that I scruple not to use seeming hyperboles, in the mention of perfections, which make the highest hyperboles but seeming ones; both God's nature and his word
Neh. ix. 5. declaring him to be *exalted above all blessing and praise*. If it were seasonable, *Lindamor*, to entertain ourselves but with those attributes of God, which are legible or conspicuous in the creation; we might there discern the admirable traces of such immense power, such unsearchable wisdom, and such exuberant goodness, as may justly ravish us to an amazement at them, rather than a bare admiration of them. And I must needs acknowledge, *Lindamor*, that when with bold telescopes I survey the old and newly discovered stars and planets, that adorn the upper region of the world; and when with excellent microscopes I discern, in otherwise invisible objects, the unimitable subtilty of nature's curious workmanship; and when, in a word, by the help of anatomical knives, and the light of chymical furnaces, I study the book of nature, and consult the glosses of *Aristotle*, *Epicurus*, *Paracelsus*, *Harvey*, *Helmont*, and other learned expositors of that instructive volume: I find myself oftentimes reduced
Psal. civ. 24. to exclaim with the *Psalmist*, *How manifold are thy works, O Lord? in wisdom hast thou made them all!* And when I have been losing myself in admiration of what I understand not, but enough to admire, and not to comprehend; I am often obliged to interrupt or break off my inquiries, by applying to the works of God's creation
Rom. xi. 33. the expression used by St. *Paul*, of those of his providence, *O the depth of the riches, both of the wisdom and knowledge of God! how unsearchable are his judgments, and his ways*

ways untraceable! And exclamations of this nature may the attentive consideration of any other of God's attributes deservedly produce. But having elsewhere treated of this subject in a peculiar discourse, I shall now, *Lindamor*, invite you to consider with me, how much you, and those that are conscious to their having virtue enough in themselves, to make them prize it in others, are in love with *Cato*, *Scipio*, and those other heroes, that did ennoble, and almost exceed mankind, upon the bare knowledge of their virtues; although from them we derive no personal advantage, (their death having numerous ages preceded our nativity). Since then we pay so much disinterested love to some few faint and ill-refined virtues, that never did profit us; how much on such a score, and at that rate, should we love him, who so possesses all perfections, that each of his perfections is infinite? Were you and I our own creators, *Lindamor*, and wholly independent upon God, without either need or hope to taste his bounty; his native excellencies, and what he has done for others, should surely ravish us, and enamour us of him. Though his benefits to us did not entitle him to our love, his essence (the source and only motive of those benefits) would give him a right to it; and though we owed him nought for what we are, we yet should owe him love for what he is. He is that glorious sun, from whom (as beams) all created perfections flow, and in whom they all concenter. To omit God's sovereign majesty (which places him so high, that but to own for him so familiar and levelling an affection as love, much more to expect to be re-loved by him, were not the least saucy presumption man could be guilty of, did not his own commands make it a duty) not to insist on this, I say, let us a while consider that proper and peculiar attraction of love, his loveliness; which is such, that, did we but once see it, all creature-competitions (even we being judges) would then be as impossible as they are now unjust. In the fifth Evangelist's prophetic visions, the Seraphim themselves (those glorious ornaments of the celestial hierarchy) are represented as covering their faces in God's presence, either blushing at their comparative deformity, or unable to sustain the unqualified splendor of so divine brightness; whence perhaps it became of old the Jewish fashion (as some frequent expressions in their writers intimate) when they went to pray, to veil their heads and faces (though now I have in their synagogues seen them cover their heads, not their faces, with those white garments they wear at their public devotions). And, *Lindamor*, if *Moses*'s face, by but a few days converse with God, reflected such a light as dazzled mortal eyes; and if his swift posts, the angels, when sent on errands to us here on earth, even when they may be supposed (if I may so speak) to wear their travelling clothes, and stood as much to our frailty in the form, as the region, they appear to us in, do, in spite of that darkening condescension, so much transcend all objects here on earth, that the scripture often mentions, that even those, that aspired to imitate their virtues, were confounded at their presence: and if, in this veiling habit, they appear so glorious, that their thus disadvantaged beauty is made the compliment and hyperbole of that quality; what may we, or rather, what may we not conclude of God himself, of whom the scripture says, *He that planted the ear, shall he not hear? he that formed the eye, shall he not see?* That is, he, that imparts a faculty, or an excellence to the creature, shall not he himself much more eminently possess it? And in effect, the most unblemished created beauties are but faint shadows (or trulier, foils) of his. Those drops of prettiness, scatteringly sprinkled amongst the creatures, were designed to defecate and exalt our conceptions, not to inveigle or detain our passions: for God did never intend them to terminate our love, but only by our eyes to exalt our faith above them, and by the beauties our sight can apprehend to raise us to a confidence, that

Isa. vi. 7.

Psal. xciv. 9.

2 King ii. 11. that there is in their author more than we can either see or comprehend. Like *Elijah*'s fiery chariots, though they be pure and bright, and consist of the noblest and Gen. xxiv. 10-53. gloriousest materials, they are meant by God but to carry us up to him. And as the patriarch's steward was furnished with so sumptuous an equipage to court *Rebecca*, not for himself, but for *Isaac*; so all the lovelinesS imparted to the creature is lent it, but to give us some more enlarged conceptions of that vast confluence and immensity, that exuberates in God. To make the righteest use of fading beauties, you must consider God and them, as you were wont to do your mistress's picture and its crystal cover: where, though that native glass were pure and lovely, and very richly edged, yet to gaze on it was not the chiefest business of your eye; nor did you in it terminate your sight, but greedily look through and beyond it, upon the adored image, that solid veil betrayed. Methinks, *Seraphic* and our common lovers behold exterior beauties with a difference resembling that, wherewith children and astronomers consider *Galileo*'s optic glasses (with one of which telescopes, that I remember I saw at *Florence*, he merrily boasted, that he had *trovato la corte a Giove*;) which the one prizes most for what they appear; the other, for what they discover. For children contenting themselves to wonder at the length, and fall in love with the workmanship and gildings of the tube, do thus but gaze upon them; whereas astronomers look through them, and, scarce taking notice of the unusual ornaments, or the shape, employ them to find out unknown lights in the sky, and to descry in heaven bright stars, unseen before, and other celestial novelties and beauties.

Sect. 12. I DENY not, *Lindamor*, that God has been pleased to adorn some of his creatures with hints and impresses, as well of his lovelinesS as his other excellences; but they do much more faintly and imperfectly resemble him, than the counterfeit sun we sometimes see in a cloud emulates the true one, by whose refracted and reflected beams, some acute modern naturalists suppose it to be produced. For though this derivative sun shine with a not-inconsiderable lustre, comparatively to the rest of the dark cloud it ennobles; yet is it not only as much inferior to the true sun in brightness, as in height, but it enjoys alone a precarious, dependent, and almost momentary being, which often it loses in a very short time, and never attains to preserve a very long one. And therefore, to neglect that supreme resplendency, that shines in God, for those dim representations of it, that we so doat on in the creature, is as preposterous and absurd, as it were for a Persian to offer his sacrifice to a parhelion (as the Greeks call that meteor) instead of adoring the sun. And certainly, *Lindamor*, if 2 Kings vi. 17. our dim sight, like that of the prophet's servant, who saw the mountains near *Dothan* covered with horses and chariots of fire, were privileged to discern otherwise invisible objects; our ravished eyes would behold lovelinesS enough in God, to make us incessantly exclaim in the language of the prophet, *How great is his goodness, and how* Zech. ix. 17. *great is his beauty!* Agreeably whereunto we may observe, that as the loadstone doth attract most powerfully the brightest needles, made of the purest steel, and those that most approach it; so angels, who, of all created beings, enjoy the unclouded'st light, and the most clear knowledge of their Maker, do love him with a constancy so fixed, that in five thousand and some odd centuries of years (effluxed since the creation) they could never see any thing, either in God, or out of him, capable to seduce them to a change; God's lovelinesS (which is such, that the eternal fruition of himself creates even his felicity) is like rare music, which, though it do delight all its partakers, the knowingest artists still do highliest value, and are most ravished with the transports it produceth, being proportioned to the degrees of the skilfulness of its admirers. The apostate spirits indeed deserted their first station, not frighted thence by

by any blemish they descried in God; but probably fell by a saucy affectation of a parity; and (as divines tell us) a sacrilegious aspiring to perfections, which they were criminally unable to behold, without wishing them their own. And for their present separation from God, it is their curse and punishment. And I shall ingenuously confess to you, *Lindamor*, that by the advantageous ideas I entertained of God's perfections, I have been sometimes inclined to think, that the reason why God tells *Moses*, *Thou canst not see my face, for there shall no man see me and live*, might be, that as transcendent objects destroy the sense, so lovely and glorious a sight (whose continuance shall make our happiness in heaven) would let in joys, and would create desires, too mighty for frail mortality to sustain. The ravished soul being shewn such game as that, would hate so eagerly, that she would break those leashes, that tie her to the body (and thereby hinder her flight to that wished union); and the glad heart (too narrow a receptacle for so much joy) to make room for such guests, would stretch unto a rupture. Longing or joy have sometimes singly reached degrees, that have made them fatal; and why then should their union, in such extremes, be thought uncapable of producing the like effect? Nor is it, perchance, more a privilege of the next life, that we shall then see God, than that we shall survive that glorious sight; which is too dazzling an object for mortal eyes, till (to use St. *Paul*'s expression) *our mortal shall have put on immortality*. But I must no longer lose myself in a theme, on which it is so easy to speak much, and so impossible to say enough. If I be not very much mistaken, they are so, who presume to give us satisfactory definitions of God's nature, which we may perhaps more safely define by the impossibility of its being accurately defined. And I must confess, I do the least expect the best description of the divine nature from those, that are most forward to take upon them to explicate it; for our words being but the representations of our notions, and they being necessarily finite, as our being; few men are (methinks) more likely to be mistaken in the nature of what is infinite (and consequently of God's attributes) than those that think descriptions can comprize it. Nor will an assiduity and constancy of our speculations herein relieve us; for too fixed a contemplation of God's essence does but the more confound us. As I remember St. *Paul* tells the *Jews*, that *he could not see for the glory, or that light*, that shone from heaven about him at his conversion. And this the famous *Simonides* experimentally found, who being asked what God was, took a day's time to consider of it; after being called upon for his answer, he desired two days longer to think upon it; and, when they were expired, demanded four days longer; and so continued a great while, each time increasing his demands for respite, till, being prest to give an account of such a dilatory way of proceeding, he ingenuously acknowledged, that the longer he considered God's nature, the less he comprehended it. And indeed, experience teaches us, that they, that gaze stedfastliest on the noon-day's sun, can least of all discern what it is, being rather dazzled than instructed by so confounding an object. Nor, is it only, *Lindamor*, a task of too great difficulty for a mere man, perfectly to explicate that incomprehensible nature of God, to which nothing but his own infinite understanding can be adequate; but, methinks, even to celebrate God's perfections, cannot be soberly attempted, without a very deep sense of a man's own unworthiness and incapacity. For, the same excellencies, that furnish us with praises, do transcend them. And the fruitfulness of the subject may as well deter, as invite our pens; since, as we are sure we shall not want theme, so we are certain we cannot dignify it. And for my part, *Lindamor*, though my subject exacted of me the praises I have endeavoured to ascribe to God, yet I should have thought it rather to decline the laws of method, than be

Exod. xxxiii. 20.

1 Cor. xv. 54.

Acts xxii. 6, 11.

reduced to derogate from what I would extol; if the neceſſity of detracting from God's perfections were not equal to that of mentioning them; and if that neceſſity were not as glorious to God, as greater praiſes than we are able to aſcribe him, could be. But, *Lindamor*, having thus done right to my method, though I could not to my theme, I ſhall only invite you to imitate with me thoſe Perſians, that adored the ſun, thought the light he lent them ſerved but to make the ſource of it admired, and not to pry into his abſtruſe eſſence with it. And though I might ſay much more concerning God's perfections, I muſt henceforth think ſilence the propereſt language I can now employ; for it is ſilence, that beſt expreſſes our wonder: and ſure, wonder is never more ſeaſonable than when God is the object; a proſtrate veneration being the ſafeſt apprehenſion of him, that is incomprehenſible.

Acts. 13. HAVING thus conſidered, *Lindamor*, how fit an object God is of our higheſt love for what he is in himſelf; let us now proceed to derive further proofs of the ſame truth from what he is to us (that your gratitude may contend with your reaſon, which ſhall moſt heighten your devotion) and we ſhall find in the vaſtneſs, freeneſs, diſintereſtedneſs, conſtancy, and advantageouſneſs of his love to us, that more than all the love we can pay him were but a little part of that we owe him.

BUT for the firſt attribute, we have aſſigned his love (the greatneſs of it) it being a general property, diffuſed through all the reſt, and conſpicuous in them, it requires now to be treated of apart.

WE ſhall then proceed to the freeneſs or unmeritedneſs of God's love; to believe which ſtrangely vaſt, we need but conſider, that we ſo little could at firſt deſerve his love, that he loved us even before we had a being; and our felicity in his decrees preceded our exiſtence in this world. God loved you numerous ages before you were; and his goodneſs is ſo entirely its own motive, that even your creation (ſince when, alone you can pretend to merit his love) is the effect of it. This benefit alone were ſufficient to render God the object of our love, though we were that of his averſion. For (as the Perſians adored the ſun even when it ſcorched them) we eſteem ourſelves obliged to love and honour our parents, in ſpight of their being wicked and unkind, though they be but God's inſtruments in our production, and made us what we were born, not arbitrarily, but in virtue of his ordination. But God, to confer on us, in the moſt excellent and endearing manner, the bleſſing promiſed to his an-
Hoſ. xiv. 4. cient people, when he vouchſafed to aſſure them, that *he would love them freely*, was pleaſed to love us, not only when we were not at all, but when we were his enemies;
Rom. v. 10. *If when we were enemies* (ſaith St. *Paul*) *we were reconciled to God by the death of his Son*, &c. Our inexiſtence indeed was a condition, wherein nothing in us was capable of being a motive of God's love: but our enmity proceeded farther, and made us worthy of his deteſtation; as if his love were nothing unleſs it vanquiſhed obſtacles,
Rom. v. 4. as well as wanted motives. This gave the Apoſtle a juſt cauſe to ſay, that *God commendeth his love towards us, in that while we were yet ſinners Chriſt died for us*: that is, when we wanted all motives to invite his love, unleſs our very want of them ſhould
John. iii. 16. paſs for one. And how did God expreſs his love unto us? even by the gift of the Son of his love; *For God ſo loved the world*; (ſays the divine token of his love) *that*
Phil. ii. 6, 7, 8. *he gave his only begotten Son*. And how did that Son love it? *He* (ſays the Apoſtle) *being in the form of God, thought it no robbery to be equal with God; but made himſelf of no reputation, and took upon him the form of a ſervant, and was made in the likeneſs of men. And being found in faſhion as a man, he humbled himſelf and became obedient unto death, even the death of the croſs.* That is, that he would love at no leſs rate than death; and, from the ſupereminent height of glory, ſtooped and abaſed him-

ſelf

ſelf to the ſufferance of the extremeſt of indignities, and ſunk himſelf to the bottom of abjectedneſs, to exalt our condition to the contrary extreme. *He was wounded for our tranſgreſſions, he was bruiſed for our iniquities, the chaſtiſement of our peace was upon him, and with his ſtripes we are healed*, ſays the Prophet. *For ye know the grace of our Lord Jeſus Chriſt, that though he was rich, yet for your ſakes he became poor, that ye through his poverty might be made rich*, ſays the Apoſtle. Men having diſpleaſed God, and conſequently forfeited all right and natural poſſibility to happineſs; even whilſt they compleated the forlornneſs of their condition, by the lethargy of not being ſenſible of it, and were as careleſs to ſeek means of recovery, as they had been unable to deviſe them of themſelves; even then, his reſtleſs love would never be at quiet, till it had ſet his omniſcience on work, to contrive expedients, and find out a way to reconcile his juſtice and his mercy, in reconciling ſinners to himſelf. And this merciful deſign, by the incarnation of his Son, he proſecuted in a way ſo worthy of himſelf, and ſo advantageous to us, that our juſt wonder at it may keep us from having any, to find that, as St. *Peter* informs us, the very angels (prompted, queſtionleſs, by a religious curioſity) ardently deſire to look into thoſe divine myſteries. I find it hotly diſputed amongſt divines (not only betwixt the Socinians and the Orthodox, but betwixt Orthodox and Orthodox) whether or no God could, without violating his juſtice, have deviſed any other courſe for the expiation of ſin, than the paſſion and death of Chriſt. But, without venturing to determine, whether or no God could, to redeem us, have choſen any other way; we may ſafely think, that he has choſen the moſt obliging and moſt endearing way; diſplaying in this divine manner of reſcuing us the ſevereſt juſtice, and the higheſt mercy; the greateſt hatred of ſin, and the greateſt love to Sinners; ſince by thoſe unequalled and unvaluable ſufferings, to which he delivered up, for us, that Son, who is near unto him, that he truly ſaid, *I and the Father are one*, he at once manifeſted, both how much he hated ſin, which he ſo heavily puniſhed in the perſon he moſt loved (though that ſurety but adopted it, to free men from the inſupportable vengeance of it) and how much he loved ſinners, by giving up what he ſo loved, for a ranſom of thoſe that were guilty of what he ſo hated. And therefore our Saviour, though he did ſuch great things to ſatisfy the unbelieving and contumacious Jews of his being their promiſed Meſſiah, would not decline death to convince them; and, though he had not ſeldom done ſo much to make himſelf the object of their faith, would not be invited from the croſs, though the chief prieſts and ſcribes themſelves ſaid at his crucifixion, *Let him now come down from the croſs, and we will believe on him.* And Chriſt, to convince the world of their unableneſs to emerge and recover out of that deep abyſs, wherein the load of ſin (which, in ſcripture, is called *a weight*) had precipitated fallen man, came not into the world, until well nigh 4000 years of ſickneſs had made the diſeaſe deſperate, and the cure almoſt hopeleſs. So inveterate an obſtinacy at once widening the diſtance betwixt God and man, and proclaiming the latter's diſability to find, by his own wiſdom, expedients of reunion. Thus Chriſt healed and diſpoſſeſſed a dumb perſon, who was able to make intreaties but by the diſability of pronouncing them; and might truly ſay to the ſecure world, *I am found of them, that ſought me not.* And when our Saviour was come into the wretched world, of all the numerous miracles recorded in the goſpel, he ſcarce did any for his own private relief. And to ſhew, that as he endured his ſorrows for our ſakes, that *by his ſtripes we might be healed*; ſo were the joys he taſted in relation to us. We read not (which is highly obſervable) in the whole goſpel, that ever he rejoiced but once; and that was, when his returned diſciples informed him, that they had victoriouſly *chaſed devils* and *diſeaſes* out of

Iſai. liii. 5.
2 Cor. viii. 9.
1 Pet. i. 12.
John x. 30.
Matt. xxiii. 7.
Heb. xii. 2.
Mat. ix. 32.
Iſai. lxv. 1.
Iſai. liii. 5.
Luke x.

oppressed mortals, and that by his *authority* men had been *dispossessed* of both the temper and punishment of sin. He conversed among his contemporaries with virtues, as well attesting what he was, as prophecies and miracles could do; and to teach man, how much he valued him above those creatures, that man makes his idols, he often altered and suspended the course of nature for man's instruction, or his relief, and reversed the laws established in the universe, to engage men to obey those of God, by doing miracles so numerous and great, that the Jews unbelief may be almost counted one. Yet were those wonders wrought for a generation, that ascribed them to the devil, and returned them with so unexemplified an ingratitude, that it is not the least of his wonders, that he would vouchsafe to work any of them for such blasphemous wretches; who were indeed, as some of the later Jews have too truly styled themselves, in relation to their fathers, *Chometz ben ya-yin*, vinegar the child of wine, a most degenerate offspring of holy progenitors. He suffered so much for them, that made him do so, that he suffered the addition of misery of being thought to suffer deservedly; *And he was numbered with the transgressors.* And though he lived as much a miracle as any he did, yet did his condition sometimes appear so despicable and forlorn, that men could not know his deity but by his goodness, which was too infinite not to belong incommunicably to God. And though it were once a saying of our Saviour's, *Greater love hath no man than this, that a man lay down his life for his friends*; yet is not what is said of the love here mentioned, to be understood of love indefinitely, or generally considered, but only of the single acts or expressions of a man's love to his friends compared betwixt themselves. And so the alleged passage seems to mean but this, that among the single acts of kindness to a man's friends, there is not any one more highly expressive of a real and sincere love, than to part with one's life for their sakes. This text therefore would not be indefinitely applied to the affection of love itself, as if it could not possibly be greater than is requisite to make a man content or willing to die for his friends; for he, that sacrifices, besides his life, his fortune also, his children, and his reputation, does thereby express more love to them, than he could do by parting with his life only for them. And he that is forward to die for those, that hate him, or, at least, know him not, discloses a more plentiful and exuberant stock of love, than he that does the same kindness but for those that love him. And thus our Saviour would be understood, unless we would say, that he out-practised what he taught; for, he came to lay down his life even for his enemies, and (like the kind balsam tree, whose healing wounds weep sovereign balm to cure those that made them) he refused not to die for those that killed him, and shed his blood for some of those that spilt it. And so little was his injured love to the ungrateful world discouraged or impaired by the savage entertainment he met with in it, that, after he had suffered from wretched men (for whose sakes he left heaven to become capable of suffering) such barbarous indignities, as might have made bare punishments appear mercy, and even cruelty itself seem no more than justice; when, I say, to hope for so much as his pardon were presumption, he was pleased to create confidence of no less than his love, a virtue. Nor think it, *Lindamor*, impertinent to our present theme, that I insist so much on what Christ has done and suffered for us, since both he himself informs us, that *he and his Father are one*; and some of the texts already mentioned have taught us, that it was an effect of God's love also to the world, that *he gave his only begotten Son to redeem it*; and, that *God commendeth his love towards us, in that while we were yet sinners Christ died for us.* Wherefore I shall, without scruple, proceed to observe to you, that so free is Christ's dilection, that the grand condition of our felicity is our belief, that he is disposed to make

Mat. xii. 24.
Mat. iii. 12.
Isai. liii. 12
John xv. 13.
John x. 30. *See also* John xiv. *ver.* 9, 10, 11.
John iii. 16.
Rom. v. 8.

make us happy, on terms, not only so honourable to him, but so advantageous to us, that, I was about to say, that possibly faith itself would scarce be exacted as requisite to our happiness, but that the condition does increase the benefit, by vouchsafing us bold and early anticipations of it: for, *faith* being (as the Apostle terms it) *the substance of things hoped for, and evidence* (or conviction) *of things not seen*, wafts our joys to this side of the grave, bows heaven down to us, till our freed spirits can soar up to heaven; and does us such a service, as the Jewish spies did to their countrymen, by bringing them over to this side *Jordan* into the wilderness, some of the pleasant and delicious fruits of the blessed Land of Promise. I said, *Lindamor*, that faith was the grand condition required in God's free grant of eternal life. Not that I would ascribe any thing to a lazy, speculative, and barren faith, in opposition to that lively and active one, which is called by the Apostle, πίστις δι' ἀγάπης ἐνεργουμένη, *faith operating by love*; since I am informed by St. *James*, that the divorce of faith and works is as destructive to religion, as that of soul and body is to life: but that I was willing to mind you, that though true faith (which cries like *Rachel, give me children or else I die*) be ever the pregnant mother of good works, yet are not those works the cause, but the effects and signs of God's first love to men (however afterward the children may nurse their parents.) As, though the needle's pointing at the poles be, by being an effect, an argument of its having been invigorated by the loadstone, or received influence from some other magnetic body; yet is not that respect unto the north the cause, but the operation of the iron's being drawn by the attractive mineral, *thou art good, and dost good*, says the Psalmist to his Maker. The greatness of his goodness is that, which makes it ours; nor doth he do us good, because that we are good, but because he is liberally so; as the sun shines on dunghills, not out of any invitation his beams find there, but because it is his nature to be diffusive of his light; yet with this difference, that whereas the sun's bounty, by being rather an advantage to us than a favour, deserves our joy, and not our thanks; because his visits are made designlessly, and without any particular intention of address (by such a bare necessity of nature, as that which makes springs flow out into streams, when their beds are too narrow to contain the renewed water that doth incessantly swell the exuberant sources;) God, on the contrary, for being necessarily kind, is not less freely or obligingly so, to you, or me. For, though some kind of communicativeness be essential to his goodness, yet his extension of it without himself, and his vouchsafing it to this or that particular person, are purely arbitrary. To omit his love to the numberless elect angels; the strict relations betwixt the persons of the blessed Trinity, supplying God with internal objects, which employed his kindness before the creation, and himself being able to allow his goodness the extent of infinity for its diffusion. But (having glanced at this only by the by) we may yet further admiringly observe, that whereas men usually give freelieſt where they have not given before, and make it both the motive and excuse of their desistance from giving any more, that they have given already; God's bounty hath a very different method: for he uses to give, because he hath given, and that he may give. Consonantly to which, when the revolting Israelites had broken the contents, whilst *Moses* was bringing them the tables of the Law, and had thereby provoked the incensed giver of it to the thoughts of a sudden extirpation of so ingrateful and rebellious a people; we may observe, that, whereas God, as unwilling to remember his former goodness to them, speaking to *Moses*, calls them, *Thy people which thou broughtest out of the land of Egypt: Moses*, on the other side, to engage God to the new mercy of a pardon, represents to God his former mercy to them, and calls them *God's people*, which *he brought*

Heb. xi. 1.
Numb. xiii. 23, 27.
Gal. v. 6.
Jam. ii. 26.
Gen. xxx.
Psal. cxix. 68.
Exod. xxxi. 7.
Verse 11.

brought forth out of the land of Egypt, with great power, and with a mighty hand. And so conspicuous in the eternal Son was this property of the merciful Father, that when sick *Lazarus*'s sisters implored his rescue for their expiring brother, the motive
John xi. 3. they employed, and which prospered their addresses, was, *Lord, behold* (not, he who loveth thee, but) *he whom thou loveſt is sick.* And as he takes the first inducements of his bounty from himself; so do his former favours both invite and give rates to his succeeding blessings. And there is reason for it; for his pure love being all the merit, by which man can pretend to the effects of his bounty, it is but just, that the degree of his love should proportion those favours, which it is our only title to; and that God's liberality should as well afford measures as motives to itself.

Sect. 14. Nor is God's love less disinterested than free. His grand design upon us is but to make us instruments and partakers of his glory, and to bring us to everlasting happiness, by a way, that does as well elevate and dignify our nature, as the condition reserved for us will. His method of saving us, if but complied with, does here, as
Col. i. 12. the Apostle speaks, *fit us for the inheritance of the Saints in light*; we being made (as
2 Pet. i. 4. St. *Peter* speaks) *partakers of the divine nature, having escaped the corruption that is in the world through lust.* So that those things, wherein the noblest of the philosophers placed their felicity, serve but to qualify and prepare Christians for that higher blessedness, that is reserved by God for those that love him; and cannot but be heightened and endeared by the value, which graces and virtues had given men on earth, for such a noble and rational kind of happiness, as is apportioned to them in heaven. What ends can he have upon us, whose goodness and his blessedness are both infinite? He was inconceivably happy (in his own self-sufficiency) before the creatures had a being; and sure that felicity, that needed not themselves to be supreme, needs nothing that they can do *. Nor was it his indigence, that forced him to make the world, thereby to make new acquisitions; but his goodness, that pressed him to manifest, and to impart his glory, and the goods, which he so overflowingly abounds with. Witness his suspension of the world's creation, which certainly had had an earlier date, were the Deity capable of want, and the creatures of supplying it. St. *Paul*, in his epistle
1 Tim. i. 11. to *Timothy*, styles God Μακάριος, which we translate *the blessed God*, but may, per-
2 Tim. vi. 15. haps, more properly be rendered *the happy God:* and elsewhere, in the same epistle,
Acts xxvii. 24, 25, 26. he truly calls him *the happy*, as well as *only potentate. God* (says the Apostle) *that made the world, and all things therein, seeing that he is Lord of heaven and earth, dwelleth not*, &c. *As though he needed any thing, seeing that he giveth to all life, and breath, and all things.* And, *in him we live, and move, and have our being.* And indeed so coherent in the mind of a meer man, that does but consider and understand the import of his own notions, is the belief of God's happiness to that of his being, that I remember the Epicurean, *Lucretius* himself, even in that impious passage, where he denies divine providence, and in a seeming, but injurious complement, would, under the pretence of easing God of it, deprive him of the government of the world, does yet confess, that the divine nature must necessarily enjoy a supreme and endless tranquillity; adding (to bring this to our present purpose) that it is

——Privata dolore omni, privata periclis,
Ipsa suis pollens opibus, nihil indiga nostri.

* *—— Quid enim immortalibus atque beatis,*
Gratia nostra queat largiri emolumenti,
Ut nostra quidquam causa gerere aggrederentur? Lucret. ex Epic.

Whereby

Whereby he acknowledges,

> ‘ That from all griefs and dangers of them freed
> ‘ Rich in itself, it has of us no need.

Or, if you will have him speak of the gods in the plural, like a heathen poet, that

> ‘ Far above griefs and dangers, those blest powers,
> ‘ Rich in their native goods, need none of ours.

A much nobler poet tells us, that *the earth is the Lord's, and the fulness thereof, the world, and they that dwell therein.* Agreeably whereunto, that *great God, that formed all things* (as in our translation the scripture calls him) says in one of the Psalms, *If I were hungry, I would not tell thee, for the world is mine, and the fulness thereof.* His ubiquity excludes all wishes of remove, by making his essence incapable of exclusion; for, whither should he desire to transport himself, that is every where, and can wish himself in no place, where he is not already? His sufficiency is such, that he can see no goods, but what he gives or hath (or rather both bestows and possesses) his plenty being so unexhausted a spring of goods, that his liberality does less impoverish God, than the sun's light does him, or imparted knowlege impairs the teacher's stock. And therefore, though St. *James* does very justly call God *the father of lights*, who is the bestower of every good and every perfect gift; yet the *friend of God* (as the scripture calls *Abraham*) and that royal priest (whom the writer to the Hebrews teaches us to have been so illustrious a type of him, whom he calls *the high priest of our profession*) do both of them, in the same chapter, style him, *The possessor of heaven and earth.* No, no, God needs not beg from, nor covet in the creatures shallow streams, those goods, of which he not only hath, but is the source. Our greatest services to our Creator must be to discharge ourselves, not to advantage him, nor as thinking to add any thing to a felicity, which were not infinite, could it admit increase. Our highest performances, though they be dues, amount not unto tributes, but are rather like those pepper-corns of rent, which freeholders pay, not with hope or with intent to enrich their landlord, but to acknowledge, that they hold all from him. When we admire the sun, our seeing of his light doth not increase it; it makes it not greater, but only makes it ours; and when we turn away, or shut our eyes, that glorious planet suffers no eclipse, and is not at all darkened or impaired, nor doth he thereby lose his light, but we: the easiness of the application requires, and excuses its omission. *If thou sinnest* (says *Elihu* in *Job*) *what dost thou against him? if thou be righteous, what givest thou him? or what receiveth he of thine hand? thy wickedness may hurt a man, as thou art, and thy righteousness may profit the son of man.* In effect, the wicked's spite against God is but like a madman's running his head against the wall, that leaves the wall unshaken, but dashes his own brains out. God inhabits a felicity (as well as light) inaccessible to all inferiour attempts: his sovereign tranquillity is so sublimely placed, that it is above the reach of all disquieting impressions; and, like the stars, that feel not the diseases their inauspicious influence produces, he doth not resent the torments he inflicts. God's justice is not less essential to him than his mercy; witness that (the numbers of the saints and the reprobate considered) thousands fall sacrifices to the severer attribute, for one that proves capable of the milder. He said, *He would get himself honour upon* Pharaoh *and all his host*, when he designed their ruin in the *Red Sea*: and *Moses* said, *he hath triumphed*

Psal. xxiv. 1. Prov. xxvi. 10. Psal. l. 12. Jam. i. 17. Jam. ii. 23. Heb. vii. *per totum.* Heb. iii. 1. Gen. xiv. 18, 22. Job xxxv. 6, 7, 8. 1 Tim. vi. 16. Exod. xiv. 17, 18. Exod. xv. 21.

triumphed gloriously in effecting it. And in *Ezekiel* he says, *Behold I am against thee,* O Sidon, *and I will be glorified in the midst of thee, and they shall know, that I am the Lord, when I shall have executed my judgments in her, and shall be sanctified in her.* Thus, when a flash of God's indignation (kindled by that strange fire they presumed to offer before him) had blasted the two presumptuous sons of *Aaron*, God is said *to have been sanctified in them that come nigh him, and to have been glorified before all the people*; such eminent and exemplary instances of severity manifesting him to be so holy in his laws, and so concerned for them, that even the ministers of his altars shall not violate them with impunity, but find him (what the writer to the *Hebrews* calls him) *a consuming fire*; who will be glorified before all his people, either by the obedience of those that approach him, or by their destruction. So to evidence, that God can derive satisfaction as well from the exercise of provoked justice, as from that of his provoked mercy, the sacred orator uses this remarkable *antanaclasis: And it shall come to pass, that as the Lord rejoiced over you to do you good, and multiply you; so will the Lord rejoice over you to destroy you, and bring you to naught.* Thus, though it be truly said of God by the prophet *Jeremiah*, that *he doth not afflict willingly, nor grieve the children of men*; (and therefore the determined consumption of the whole land, which our bibles english, *God's work is strange work*, other translators read, *Opus alienum suum*): yet when the sins of incorrigible offenders are grown to that provoking height, that his mercy intercedes no more to avert or suspend the inflictions of his justice, than how much he can satisfy himself in destroying those, that would not be preserved, may be guessed at by that formidable expression in *Ezekiel*; where, having foretold what havock the sword, the famine, and the pestilence should make amongst the intractable and dispersed Israelites, he adds (as a kind of Ἐπινίκιον) *thus shall mine anger be accomplished, and I will cause my fury to rest upon them, and I will be comforted.* The howlings of the damned as well sound forth his praises, as do the hallelujahs of the saints; they both do sing to him an everlasting canticle of praise: only in this great concert of his whole intelligent creation, the designlessly conspiring voices are as differing as the conditions of the respective singers. Hell's darkness doth as well contribute to God's glory, as heaven's eternal splendor; as shadows, judiciously placed, do no less praise the painter, than do the livelier and brighter colours. And as when the earth doth send black, noisome, and sulphureous exhalations up toward the sky, alas, they reach not heaven, nor discompose the spheres; but all the storms and thunders they produce fall on that globe they came from, and there do all their mischief: so, the wicked may wrong God indeed, yet do they really harm but themselves by all their greatest sins, which trouble him chiefly, but because they necessitate him to punish them; for the transgressions, that do most provoke God, do him not the least harm. An impious person may (as *Elihu* lately informed us) hurt a man as himself is, not that supremely blessed Deity; the result of whose infinite perfections is a resembling happiness, which is as inseparable from him as his essence. Our offences may derogate from his accessional glory, not from his essential felicity; or rather, the most desperate sinners, by their greatest crimes, can but change the attribute they should bring honour to, and but oppose the glorifying of his goodness to occasion the glorifying of his justice; since he will be infallibly glorified, soon or late, either by men's actions, or their sufferings; by their practice of duties, or punishment for sin. Thus you see, how little God is beholden to you for your declining hell; nor will the score be very much encreased by your addresses and attempts for heaven. *Can a man* (says *Eliphaz*) *be profitable unto God, as he that is wise may be profitable unto himself? Is it any pleasure to the Almighty, that thou art righteous? or is it gain unto him,*

Ezek. xxviii. 22.

Levit. x. 3.

Heb. xii. 29.

Deut. xxviii. 63.

Lam. iii. 33.

Isa. xxviii. 21.

Ezek. xiii. 5.

Job. xxii. 2, 3.

him, that thou makeſt thy ways perfect? Congruouſly to which ſenſe the Pſalmiſt ſays, *my goodneſs extendeth not to thee.* The fire, that we kindle on God's altars, heats and enlightens us, but warms not heaven at ſo diſtant a remove, nor is wanted in the ſun's reſidence. We have all the redolence of the perfumes and incenſe we burn upon his altars; the ſmoke doth vaniſh ere it can reach the ſky, and whilſt it is undiſperſed, but clouds and but obſcures it. Alas, our beſt performances are as uſeleſs ſervices to God, as the heir's bringing wax to his departing father is to him; which adds not any thing to the rich man's ſtore, and is by him deſired and accepted, only to ſeal away a fortune to his ſon. Though therefore it be true, that God is pleaſed with our performances, yet is that welcome he vouchſafes to give them ſo far from enabling us by them to requite his love, that it increaſes the unrequitedneſs of it; ſince he is delighted with them, as they afford him juſt riſes to reward them. How far from mercenary is then God's bounty! ſince he accepts our acknowledgments of his former bleſſings (chiefly) to make them opportunities of conferring freſh ones; as good old *Iſaac* deſired his ſon's veniſon, that from the reliſhing of that ſavory meat he might take an opportunity to bleſs him. Pſal. xvi. 2. Gen. xxvii. 4.

And the better to diſcover, how diſintereſted God's favours are, let us further conſider, how little they are requitable; for we can give him nothing but his own (nor, heaven knows, all that neither:) and both the will and power to ſerve him are his, upon ſo juſt and many ſcores, that we are unable to retribute, unleſs we do reſtore; and all the duties we can pay our maker are leſs properly requitals than reſtitutions. When *David* and his officers had offered towards the ſtructure of that magnificent temple, which they ſeemed ambitious to make a manſion inferiour to heaven only; the king himſelf gave three thouſand talents of gold, and ſeven of refined ſilver; and the heads of the people five thouſand talents (beſides ten thouſand drachms) of gold, ten thouſand talents of ſilver, eighteen thouſand of braſs, and a hundred thouſand of iron (a treaſure, of which I ſcarce remember to have read the like in any hiſtory) beſides a number of all manner of precious ſtones, capable of impoveriſhing the very *Indies.* They perfumed this noble and unequalled offering with a ſolemn confeſſion, which perhaps, in God's eſteem was much more precious than it: *Thine, O Lord* (ſays the royal prophet in the name of all) *is the greatneſs, and the power, and the glory, and the victory, and the majeſty; for all that is in heaven and in earth is thine. Thine is the kingdom, O Lord, and thou art exalted as head above all. Both riches and honour come of thee, and thou reigneſt over all. And in thine hand is power and might; and in thine hand it is to make great, and to give ſtrength unto all. Now therefore, our God, we thank thee, and praiſe thy glorious name. But who am I? and what is my people, that we ſhould be able to offer ſo willingly after this ſort? for all things come of thee, and of thine own have we given thee. For we are ſtrangers before thee, and ſojourners, as were all our fathers. Our days on earth are as a ſhadow, and there is none abiding. O Lord our God, all this ſtore, that we have prepared to build thee an houſe, for thy holy name, cometh of thine hand, and is all thine own. Who* (ſays the Apoſtle, in a queſtion that imports its own negative) *hath firſt given to him, and it ſhall be recompenſed to him again? for of him, and through him, and to him, are all things.* Nay, even our love itſelf (that poor man's ſurety and exchequer, that doth pay all his debts by ſupplying him with the prerogative to coin his deſires and wiſhes of an arbitrary value) is here unable to diſcharge our debts; our love being too much the effect, to be capable of being the recompenſe of God's. And ſurely, the divine amoriſts had cauſe to ſay, that *herein is love, not that we loved God, but that he loved us.* And, as the ſame Apoſtle elſewhere ſpeaks, *we love him, becauſe he firſt loved us.* 1 Chron. xxix. 11—16. Rom. xi. 35, 36. 1 John iv. 10. 1 John iv. 19.

If, in effect, we look upon the unworthy contest betwixt God's mercies and most men's ingratitude, and but reflect upon the small return of love that the greatest disbursements of his do usually bring home; we cannot but acknowledge (as *David* in the lately mentioned scripture did) that our loving God for his favours, is one of the greatest favours that we love him for. So unrequitable is God's love, and so insolvent are we, that that love vastly improves the benefit, by which alone we might have pretended to some ability of retribution; and so unlimited is this impotence of ours to recompense or repay God's dilection, that it extends to, and fetters our very wishes. For God enjoys an affluence of felicity, so perfect and entire, that even our wishes can aim at nothing for him worthy of him, unless instructed by what he already actually possesses; and the sense of this same very impotence, to some of the greatest proficients in *Seraphic Love*, appears not the least uneasy property of it. It grieves us sensibly, to see ourselves reduced to be only passive, and the receivers in this commerce. We would fain contribute something, and cannot always refrain from devoting our wishes to increase his happiness, to whom we owe all ours. And some holy persons (particularly St. *Austin*) have, by the exuberance of their gratitude and devotion, been transported to make wishes, and use expressions, wherein their affections had a greater share than their reason, and which argued them much better to apprehend, how much God deserved of them, than how little he needed them. But, upon second thoughts, we shall find, that the cause of our grief ought to turn it into our joy, since the desires we would frame, aiming at God's being infinitely happy, are all fulfilled before they are conceived, and that in the most advantageous and noblest way; for could God's happiness admit accession by our accomplished wishes, there were then a possibility of his wanting something to render it compleat. And sure, it is a more supreme felicity to be by nature transcendently above all increase of blessedness, than to receive the greatest, that men can wish.

Sect. 16. To proceed now to the constancy of God's love; we cannot entertain of God any apprehensions, not altogether unworthy of him, and criminally injurious to him, without believing, that to think, that he can be inconstant, is as great a crime as it were a misery to find him so. His love is like his essence, immutably eternal, reaching from everlasting to everlasting; it preceded the nativity of time, and will survive its utmost period and obsequies. *Having loved his own, which were in the world, he loved them unto the end*, says the Evangelist: and when St. *James* had told us, that *every good gift and every perfect gift is from above and cometh down from the father of lights*, he adds (to complete our consolation) *with whom is no variableness, neither shadow of turning. Of his own will begat he us of the word of truth.* And in effect, since God takes the motives of his love to us from himself, not from us, the unchangeableness of his nature seems strongly to infer that of his charity, and our happiness in it. For, *I am the Lord, I change not; therefore ye sons of Jacob are not consumed*, says God by the last of his prophets: and in *Jeremiah* he tells his people, *I have loved thee with an everlasting love.* And what God once said to the generous *Joshuah, I will never leave thee nor forsake thee*, is by the writer to the Hebrews applied to believers in general; for *the gifts and calling of God* (says the same author elsewhere) *are without repentance.* Nor do those crosses, that seem due to his anger, destroy the immutability of his love, since even that anger is an effect of it, proceeding from a fatherly impatience of seeing a spot unwiped off, in the face he loves too well to see a blemish in it; and from his desire to see his child an object fit for a larger measure of his kindness: as when we beat the dust out of a suit we fancy, we strike not out of anger, but only to remove that, which doth sully it, and hinder us

John xxxi. 3.
Jam. i. 17.
Mal. iii. 6.
Josh. i. 5.
Heb. xiii. 5
Rom. xi. 29.

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to take that delight in it, which our fondness would be pleased with a just cause to find. *As many as I love, I rebuke and chasten* (says our Saviour.) And, *I know, O Lord* (says the Psalmist) *that thy judgments are right, and that thou in faithfulness hast afflicted me*; the furnace of affliction being m ant but to refine us from our earthly drossiness, and soften us for the impression of God's own stamp and image. The great and merciful architect of his church (whom not only the philosophers have stiled, but the scripture itself calls τεχνίτης, an artist or artificer) employs not on us the hammer and the chizzel, with an intent to wound or mangle us, but only to square and fashion out hard and stubborn hearts into such *living stones*, as may both grace and strengthen his heavenly structure. Nor is God only thus constant to his love, but to his loveliness. Our female beauties are usually as fickle in their faces as their minds, and more certainly in the former; because, though casualties should spare them, age brings in a necessity of a change, nay, a decay; leaving our doaters upon red and white incessantly perplexed, by the incertainty both of the continuance of their mistress's kindness, and of the lasting of her beauty (both which are necessary to the amorist's joys and quiet): for, sometimes, when the mistress's humour doth not change so much, as to prove guilty of the fault of inconstancy, her face alters enough to make her lovers wish inconstancy no fault; or, that she had committed it, that her fickleness might afford them the excuse of imitation or revenge. But, in devotion, we are equally secure from both these dangers; since God doth not desist from blessing us with his love, nor ceases ever from deserving the height of ours. Nor is he only constant in making us the objects of his love, but also in bending and inclining us to make him the object of our strongest affections; so that he not only persists in continuing to us both the offer and value of his love, but perseveres to give us a receptive disposition to welcome it to us, and reflect it up to him. The want of such a disposition lost *Adam* paradise, and the fallen angels heaven; there being to the object, that must secure our love, such a nature requisite in reference to our affections, as philosophers are pleased to ascribe to the world's center in relation to heavy bodies, which, they teach us, that magnetic point has the double faculty, not only to draw thither, but to keep there. For so untoward and cross-grained are we, in point of our own good, and so unfit to procure, and ready to desert, our own felicity, that neither its excellency is a sufficient motive to carry our addresses to it, nor its possession a competent tie to intercept in us all designs of revolts and divorces: but we must be used as peevish children are, who (on the one side) when their mouths are out of taste, and they refuse to take what is necessary for them, must have it not only offered them, but forced upon them, and be (as it were) made to receive it; and who (on the other side) must be restrained from gadding, when the beauty of the mansions they live in cannot invite their stay, but they would gladly leave the proudest palaces architecture can boast, to run into the street, and dabble in the kennel. All these three properties of divine constancy are not ill shadowed in the operations of the loadstone (a mineral, in which I have made too many experiments, not to be by you allowed to make some comparisons to it.) For, first, it never forsakes its inclinations for the steel; next, being united to it, it retains so constantly its attractive qualities, that it gives not the needle any motive of deserting it; and thirdly, it doth never rightly touch the amorous steel, without leaving an impression, which ever after disposes it to a conversion to that magnetic posture, which best fits it to receive fresh influences. To which let me add this other resemblance betwixt God's work on us, and the loadstone's on the iron, that the kind stone attracts a needle to it, not to advantage itself by that union, but to impart its virtue to what

Rev. iii. 19. Psal. cxix. 75. Heb. xi. 10. 1 Pet. ii. 5.

it draws. Besides, absence and rivals, those frequent ruiners of other lovers happiness, can threaten nothing of formidable to yours. For, absence (which so divorces us from that, which animates us, that lovers do not so improperly stile it death, if death be but the separation of soul and body) by God's ubiquity we are secured from; he is ever present with us, or rather in us. You, that, not long since, so highly valued the opportunities of conversing with your mistress for some few moments, shall here find your privileges improved to a permission; nay, an invitation, of entertaining the object of your love at all times; no hour renders your visits unseasonable, nor no length tedious: he is rather welcomest to God, that comes to him oftnest, and stays with him longest. What favours were vouchsafed to that antient prophetess (who was likewise one of the first evangelists) who for many years *departed not from the temple, but served God with fastings and prayers night and day*, the beginning of St. *Luke*'s gospel may inform me. The midnight hymns of *Paul* and *Silas* did not only not disturb or offend him they praised, but procured the visit of an angel to bring them miraculous and unexpected liberty, as a proof of the acceptableness of their seemingly unseasonable devotions. When *Enoch had walked with God* as many years as the year has days, God was so far from being importuned or tired by that lasting assiduity, that vouchsafing him an unexampled exemption from death, he was pleased by a new and a nearer cut to heaven, to admit him to a yet closer, more immediate, and more undistracted communion with himself. And when *Moses* had spent no less than *forty days and forty nights* in conversing (if I may use so familiar a term) with God in the mount, he brought down thence, instead of a penance for his importunity, so signal and radiant a testimony of God's peculiar favour, that his dazzled countrymen were as much disabled, as invited, to gaze on an object of so much wonder. And then, how proud do we see many lovers of their sufferings, when she but knows of them, for whom they are endured? But in *Seraphic Love*, there is not the least good wish, or privatest suffering, nay, not a whispering sigh, or closer thought, that silently groans or aspires in the amorous soul, but he both sees and hears, that puts his servants *tears into his bottle*, sweetening and recompensing the greatest misfortunes that his love occasions, with such support and joys, as hinder us to feel them, and make them deserve a contrary name. Each amorous soul may say to God with *David*, *Thou knowest my down-sitting, and my up-rising; thou understandest my thoughts afar off; thou compassest my path, and my lying down, and art acquainted with all my ways.* And Christ also himself has so attentive an eye upon the amorous soul, that he is held forth in the Apocalypse, as telling the ruler of the church of *Smyrna*, *I know thy works, and tribulation, and poverty*; and saying to the angel of the church of *Pergamus*, *I know thy works, and where thou dwellest, even where Satan's seat is; and thou holdest fast my name, and hast not denied my faith, even in those days, wherein Antipas was my faithful martyr, who was slain among you, where Satan dwelleth.* So that no endearing circumstance of our love escapes unobserved by him, who has done and suffered so much to engage us to it. God remembers not our endeavours to serve him the less, for our having forgotten them: *When saw we thee any way distressed, and relieved thee?* will be the question of those, to whom heaven itself will be at the last day awarded, as having ministered to their Redeemer. Those, that in degenerate times, such as ours, *Lindamor*, did, like *Lot* in *Sodom*, mourn for their sins, that mourned not for their own, and condoled among themselves the spreading wickedness of the times they lived in; though probably the dangers threatened them by the very sinfulness they deplored made them affect such privacies in their conferences, as freed them from the thoughts of being over-heard; yet the scripture informs us (and it

Luke ii. 37, &c.
Act. vi. 25, 26, &c.
Gen. v. 22, 23, 24.
Exod. xxxiv. 30.
Psal. lvi. 8.
Psal. cxxxix. 2, 3.
Rev. ii. 8, 9.
Verse 12, 13.
Mat. xxv. 37, &c.

it is a comfortable as well as memorable passage) that *the Lord hearkened and heard it, and a book of remembrance was written before him, for them, that feared the Lord, and that thought upon his name: then shall he return, and discern betwixt the righteous and the wicked, between him that serveth God, and him that serveth him not.* Mal. iii. 16, 17. *I know,* says Christ (not only to the angel of *Smyrna*, but to each true sufferer for him) *thy works, and tribulation, and poverty: fear none of these things, that thou shalt suffer; be thou faithful unto death, and I will give thee a crown of life.* Rev. ii. 9, 10. God is often pleased to accept those good thoughts and intentions of his servants, which never arrive at actual performances. Though *David* built not the temple he designed, yet his son, that did it, informs us, that God said unto him; *Forasmuch as it was in thine heart to build an house for my name, thou didst well in that it was in thine heart, &c.* 2 Chron. vi. 8. And it is the epithet our Saviour gives God, *your Father which seeth in secret, &c.* Mat. vi. 6. Nor need we fear our rivals should supplant us, since we can have none in devotion, whose prayer and endeavour it is not, that God would love us more. For his love to you being (as the chiefest merit) the strongest motive and title unto theirs, they cannot but wish him well, whom God doth love so; and cannot wish him better, than by imploring for him fresh additions, both of that love of God, and grateful dispositions to return it. Our Saviour assures us, that *there is joy in the presence of the angels of God over one sinner that repenteth.* Luke xv. 7, 10. And the sole hymn (except a visionary one) I find recorded of the celestial choir, was sung for a blessing to mankind, wherein (for aught I know) their love and sympathy alone concerned them: *For unto us* (men) *the child is born, and unto us the Son is given, who took not upon him the nature of angels, but the seed of Abraham.* Luke ii. 13, 14. Isai. ix. 6. Heb. ii. 16. So noble and so disinterested doth divine love make ours, that there is nothing, besides the object of that love, that we love more than our concurrents in it, perchance out of a gratitude to their assisting us, to pay a debt (of love and praise) for which alas! we find our single selves but too insolvent.

Perhaps I need not mind you, *Lindamor*, that divers passages of the foregoing discourse suppose the truth of their doctrine, who ascribe to God, in relation to every man, an eternal, unchangeable and inconditionate decree of election, or reprobation. Yet concerning the controversies betwixt the Calvinists and the Remonstrants about predestination, and the coherent doctrines, it were improper to give you here my sense. Those, that are truly pious of either party, are perhaps otherwise looked on by God than by any other, as contending, which of God's attributes should be most respected; the one seeming to affirm irrespective decrees, to magnify his goodness; and the other to deny them, but to secure the credit of his justice. And even in honouring the same attribute, his goodness, these adversaries seem rivals, the one party supposing it best celebrated by believing it so irresistible, that to whomsoever it is intended, he cannot but be happy; and the other thinking it most extolled by being believed so universal, that it will make every man happy, if he pleases: the one party electing to honour free-grace, by assigning it (as to men) an unlimited vast extent; as the other does, by ascribing it an infallibly victorious degree. But though my haste, and the nature of my theme make me decline the controversies about predestination; yet since the doctrine, that maintains it, is not only by almost all the rest of mankind, but by the rest of the protestant churches themselves (the Lutherans, and divers learned divines of the church of *England*) not only rejected, but detested as little less than blasphemous (as indeed they, that judge it an error, cannot but be tempted to think it a dangerous one, and of very pernicious consequence, so far forth as its sequels are permitted to have influence on men's practice) I think it not amiss to advertise you, that the doctrine of predestination is not necessary

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to justify the freeness and the greatness of God's love. For so conspicuous and refulgent a truth is that of God's being the author of man's felicity, that the dispute betwixt the Calvinists and Arminians is not so much concerning the thing, as concerning the manner of its being proffered; the former affirming grace to be irresistibly presented; the latter though they deny it to be irrejectable, yet granting, not only that it is altogether free and undeserved, but also, that the proffer is made, both with a power enabling those, to whom it is rendered, to accept it, and with such engaging invitations, that man at his first conversion need contribute nothing to his felicity, but the not wilfully refusing it, and may more properly be said to owe it unto God, than the beggar to owe his alms to his reliever, though he opens his hand to receive it; which he might have declined to do, if he would have wilfully courted his own prejudice. Christ paid *a ransom to redeem us*, it is true, and he is therefore
2 Pet. ii. 1. called, *the Lord that bought us:* but it was God's free goodness, both to provide us that Christ, and to accept of that ransom, neither of which he was obliged to do; and therefore the scripture ascribes it not to the justice, but to the love of God to the
John iii. 16. world, that *he sent his only begotten Son to redeem it:* and St. *Paul* in the same text tells
Rom. iii. 24. us, both that *we are justified*, δωρεὰν τῇ αὐτοῦ χάριτι, *freely by his grace*, and yet that it is, διὰ τῆς ἀπολυτρώσεως, *through the redemption* (purchased for a ransom; the original word Englished redemption relating to the price paid for the redeeming of captives) *that is in Jesus Christ.* It is confessed on all hands, that merit must be disclaimed, and those that seem to expect something from God as a due, acknowledge, that if his promise did not, their actions could not, make it so; and that it is to his mercy they owe the
2 Tim. iv. 7, 8. right they have to confide in his justice. St. *Paul*, who having *fought the good fight, finished his course, and kept the faith*, expected *a crown of righteousness from the Lord*, under
Eph. ii. 8. the notion of the righteous judge; yet, tells us, *that by grace we are saved, through faith, and that not of ourselves, it is the gift of God.* Whose promises, now they are made us, al-
2 Pet. i. 4. low us indeed to expect heaven from his justice; but *the making us those great and precious promises* (as St. *Peter* justly styles them) must be acknowledged the pure effect of *his free and undeserved goodness*; which to believe infinite, we need but consider the disproportion betwixt such a recompense as eternal glory, and the least imperfect performances of ours: which though they needed not pardon, could not at least challenge any reward from him, who, as our Creator, has such a right to exact of us what services he pleases, without proposing us any recompence, that our exactest obedience to all his commands
Luke xvii. 10. would yet leave us to confess ourselves *unprofitable servants, who have done but what it was our duty to do*; and what, if we had not done, we had given God, who had the power, the right and provocation to punish us. And indeed, so conscious are men generally (if not naturally) to their being beholden to God for their felicity, that even those, that mistake or oppose his way of doing them good, will yet be sure to find out some notion or other, under which they may conceive themselves God's debtors for his blessings. That the more sober sort of Romish catholics themselves ascribe not so much to merit (properly so called) nor so little to God's grace, as the more quarrelsome writers of their party have given the more eager disputants of ours occasion to reproach them, were perhaps no difficult task to manifest, if my haste would give me leave. That the Arminians own the freeness and unmeritedness of
* *Especially* God's grace, the Remonstrants * confession and apology are very careful to satisfy the
chap. xvii. Numb. 5, 6. world. And even the Socinians (how prosperously I determine not) are not a little, or unindustriously sollicitous to free their erroneous doctrine of justification from the
Hab. i. 16. objected guilt of its tendency to draw the embracers of it to *sacrifice to their own nets*, and thank themselves for their felicity. Which brings into my mind a passage, that I lately

I lately read in one of the chief modern upholders of that sect, *Schlichtingius*; who is wont, in my opinion, where his subject will bear it, to discourse as fairly and as rationally as almost any writer that I have met with of his persuasion; and who labours to reconcile *Socinus* his doctrine with the freeness of God's grace, by considerations, which, not to injure him, I shall present you with in his own words. *Ad retundendam vero* (says he, disputing against the learned *Meisnerus*) *arrogantiam justificatorum, & ne dicant se meruisse gratiam, non est necesse servum in homine arbitrium inducere; non debet virtus tolli, ut tollatur arrogantia. Sufficit,* 1. *Quod nec velle nec perficere possint, nisi Deus & voluntatem excitet, & vires augeat.* 2. *Quod ea, quæ divinis adjuti viribus faciunt, nullo modo dignitate & pretio divinæ gratiæ respondeat, sed infinito intervallo ab ea absint.* Nay, though the modern and degenerate Jews be, upon the score of being the great patrons of man's free-will, not causelessly esteemed the great oppugners of God's free-grace; yet both from their famous Rabbi, and my learned acquaintance, *Manasseh Ben-Israel*, and from divers others of their most eminent writers, has the truth sometimes extorted confessions, which, though made upon erroneous grounds, were not very far short of orthodox. To which purpose I remember, that a Jewish professor of Hebrew (who assisted me in my studies of that mysterious tongue) being, as the rest of his nation, an eager and peremptory champion of free-will, conceived, that even that liberty, which to us seems least to indebt men to their Creator, did transcendently oblige him unto God. For, one day that we were privately and freely discoursing together of matters of religion, he told me, he thought men owed more to God's goodness, than the very angels do. For, said he, whereas God, without any good work of theirs, but purely out of his goodness, conferred on them that blest condition they enjoy, by giving man a free-will, by the good use of which he may glorify his Maker, when, by abusing it, it is in his power to dishonour him; he allows man that highest satisfaction and privilege, of co-operating to his own felicity. Pag. 97.

And now, *Lindamor*, we are arrived at the last property, which qualifies God the fittest object for our love, which is, the advantageousness of his to us, both in the present and the future life. And first, even in this world we owe God no less than all the goods we possess. We owe him both what we have, and that we are: for we may say truly of God with the Psalmist, *It is he that hath made us, and not we ourselves.* And we were not only in his hands so like *clay in the potter's*, that he might have made us any thing; but we were so purely that negative, from whence we were extracted, that he (if he had pleased) for ever might have left us to our first nothing. His love is the first original and fountain-blessing; all the rest are but as pipes (and instruments) to convey, and serve but to hand it to us. Your wit wins you applause, your industry heaps you up treasures; be it granted. But who gave you that wit? and did both give and prosper that boasted industry? Certainly, God as much gives us all the goods we possess, as he that gives a beggar a thousand pounds, gives him the clothes, and meat, and all the bravery it helps him to. But besides these more obvious presents of God's bounty, we enjoy other effects of his goodness, which, though by the customarineſs of their being possessed, they prove less conspicuous than the other, yet grow no less prized, when the want or loss of them makes us sensible of the true value of them. Had I the leisure, *Lindamor*, to lead your thoughts with me to the galleys, and shew you there those wretched captives, that are chained to the oars they tug at, and though exposed to all the miseries and hardships of a tempestuous sea, have oftentimes cause given them, by their barbarous usage ashore, to fear the ocean less than any port, save death. Could I draw from you the curtains of Sect. 18. Psal. cx. 3. Isai. lxiv. 8.

sick

ſick and dying men, and open to you that ſad ſcene, where ſome pine and languiſh by diſtempers, that deprive them of all joys, advantages, and (what is more conſiderable) uſes of life, before they eaſe them of life itſelf; others breathe rather than live, perpetually tormented either with their diſeaſes or phyſic to protract a wretched life, upon terms that turn it into a trouble; and others ſtruggling with the rude pangs of death, are yet, perchance, leſs tormented by them, than by the ſad proſpect of their former life, and the remembrance of thoſe criminal pleaſures, *which yet it perhaps leſs troubles them that they muſt now forego, than that they once enjoyed them.* Should I, *Lindamor*, bring you into hoſpitals, and ſhew you there the various ſhapes of human miſery, and how many ſouls, narrowly lodged (if I may ſo ſpeak) in ſynecdochical bodies, ſee their earthen cottages moulder away to duſt; thoſe miſerable perſons by the loſs of one limb after another ſurviving but part of themſelves, and living to ſee themſelves dead and buried by piece-meal. Should I, to diſpatch, *Lindamor*, ſhew you all the ſeveral companies of mourners, that almoſt make up mankind, and diſcloſe to you, how copious ſhowers of tears do almoſt every where water (not to ſay overflow) this vale of miſeries; you would, perchance, ſee cauſe to think, that God's privative (if I may ſo ſpeak) may contend with his poſitive favours; and that you owe little leſs for what you are not, than for what you are, to that diſcriminating mercy of his, to which alone you owe your exemption from miſeries, as great as
2 Cor. iv. 7. the bleſſing it confers on you. For, *who maketh thee to differ?* is a queſtion, that may be as well aſked. in reference to our external, as to our ſpiritual condition. Which invites me to mind you, *Lindamor*, that you are yet more engaged to God's love for protecting you from thoſe groſs vices, that diſfigure moſt men's minds, than from thoſe leſs dangerous, though more reſented diſeaſes, that diſtemper their bodies.

1 Pet. i. 18. For, *ambition, luſt, avarice, revenge*, and even that *vain converſation*, which young gentlemen are generally pleaſed to think ſo innocent, are really more formidable and pernicious diſeaſes and calamities, than thoſe, that reduce men to take phyſic, or thruſt them into hoſpitals. To evince the truth of which paradox, I hope I ſhall not need to mind you of judging of the dangerouſneſs of diſeaſes by the nobleneſs of that part affected, ſince I can tell you, that he, that cannot err, ſeems daily to juſtify our aſſertion, by inflicting ſickneſs and the ſharpeſt outward calamities on his own deareſt children, to preſerve them from the contagion of ſin, or cure them of the unfilial habitudes of it. And therefore, ſince, when we ſee a tender mother apply a painful cauſtic to the neck of her favourite-infant, threatened by the apoplexy, we ſcruple not to conclude, that ſhe thinks the trouble of an iſſue, an evil inferior to the convulſion fit; ſo when we ſee our heavenly father ſends infirmities and croſſes, to reſcue thoſe he loves, from the contagion or dominion of ſin; we may ſafely conclude, he thinks affliction a leſs evil than guilt, ſince he is too wiſe and indulgent a phyſician to cure with a remedy worſe than the diſeaſe. In the eighth of *Deuteronomy*, there is a caution given the Iſraelites, leſt proſperity (which is wont to be a kind of *Lethe*, that makes men forget all but their enjoyments) ſhould make any of them ſay
Deut. viii. 17, 18. in his heart, *my power, and the might of my hand hath gotten me this wealth. But* (ſaith my text) *they ſhall remember the Lord their God; for he it is, that giveth them power to get wealth.* It is not the revolting Iſraelites only, of whoſe ignorance of his bounty
Hoſ. xi. 3. God may complain, as he did by the prophet, by whom he ſaid, *I taught Ephraim to go, taking them by their arms, but they knew not that I healed them:* and there are
Hoſ. ii. 8, 9. but too many, of whom he might ſay, as he did by the ſame prophet: *For ſhe did not know, that I gave her corn, and wine, and oil, and multiplied her ſilver and her gold,* which

which they prepared for Baal; therefore will I return and take away my corn in the time thereof, and my wine in the season thereof, and will recover my wool and my flax given to cover her nakedness. And this will make way for the design I had to recommend the advantageousness of God's love, by saying, that as for spiritual goods, he gives us in this life so rich an earnest of expected joys, that even the earnest is a stock large enough to subsist with comfort on, and really out-values and transcends all those momentary pleasures, it requires us to forsake, to keep up a title to eternal ones. But to particularize God's mercies to us in this very life, would certainly take up a considerable part of it. And yet the love God bears us dies not with us, nor doth (as men's affections) either endure a funeral in our tombs, or survive only in a useless grief (or an esteem as bootless). No, God's love is so far from resembling the usual sort of friends, who, when they have accompanied us to the grave, do there leave us; that, like the angels, that *carried Lazarus's soul to Abraham's bosom*, its officiousness begins then most to appear, when our dark eyes are closed, and is then truest to the beloved soul, when she forsakes the body; giving each blessed saint cause to say of God, what *Naomi* did of *Boaz*, that *he hath not left off his kindness to the living, and to the dead.* *Now*, indeed (says our Saviour's favourite) *are we the sons of God, and it doth not yet appear what we shall be; but we know, when he shall appear, we shall be like him.* This blessed expectance must be now my theme, because the narrow limits, which my design hath placed to this discourse (of the advantages accruing from God's love) will leave no more room untaken up by heaven.

Luke xvi. 22. Ruth. ii. 20. 1 John iii. 1.

But, *Lindamor*, before I proceed to set forth to you the greatness of the felicity reserved for us in heaven, it will, I fear, be requisite to mind you of the lawfulness of having an eye to it. For many, not undeservedly, applauded preachers have of late been pleased to teach the people, that to hope for heaven is a mercenary legal, and therefore unfilial affection. Indeed, to hope for heaven as wages for work performed, or by way of merit, in the proper and strict acception of that term, were a presumption, to which none of the divines, we dissent from, can be too much an enemy, nor perhaps more so than I am. But to take in God's blessings among the motives of loving God, is, but to do as he did, who said, *I love the Lord because he hath heard my voice and my supplications*; and to look upon the joys of heaven, to comfort and support us in the hardships and losses to be undergone in our journey thitherward, is to imitate no worse a man than *Moses*, of whom it is said, that *he esteemed the reproach of Christ greater riches than the treasures in Egypt; for he had respect*, or turned his eye (ἀπέβλεπε) *unto the recompence of the reward.* It is indeed, *Lindamor*, a happy frame of mind, to be able to love God purely for himself, without any glance at our own advantages. But though I dare not deny, that it is possible to attain to so high and disinterested a kind of love, yet I think, that that excellency supposed to be vouchsafed to some men, is not by the scripture exacted as a duty from all men. Were all the recompense of piety of a worldly nature, and to be here received, the actions invited to, by the intuition of it, might pass for mercenary. But when heaven is chiefly hoped for, as it will admit us unto the fruition of God himself in Christ, and that the other joys expected there are so far from being of a sensual or a worldly nature, that they are known not to be attainable, till by death, the senses and bodies themselves, and all the merely animal faculties be abolished: for a heaven so considered, I say, to forego readily all the pleasures of the senses, and undergo chearfully all the hardships and dangers, that are wont to attend a holy life, is *Lindamor*, such a kind of mercenariness, as none, but a resigned, noble, and believing soul is likely to be guilty of. If I should say; that fear itself, and even the fear of

Sect. 19. Psa. cxvi. 1. Heb. xi. 26.

hell, may be one justifiable motive of men's actions, though I should propose, what those I am reasoning with would think a paradox; yet I should perhaps hold forth therein no more than the scripture does, *Let us therefore fear*, (says the writer to the Hebrews) *left a promise being left us of entering into his rest, any of you shall seem to come short of it.* And no less eminent an herald of the gospel than St. *Paul*, who successfully maintained the evangelical against the legal spirit, thus professeth of himself; *I keep under my body, and bring it into subjection, left by any means, when I have preached to others, I myself should be a cast-away.* And it was not to slaves or hirelings, that Christ directs this admonition; *I say unto you*, my friends, *be not afraid of them, that kill the body, and after that have no more that they can do: but I will forewarn you whom you shall fear; fear him, which, after he hath killed, hath power to cast into hell; yea, I say unto you* (a gemination, which the present controversy shews not to have been causeless) *fear him.* Where the paraphrase given of God is not barely descriptive, but ratiocinative (to borrow those terms of the schools) in warning us not only whom we should, and should not fear, but why we should fear the one, and not the other. As when St. *Paul* says, *I know whom I have trusted*, he means what manner of person, how *faithful* (as St. *Peter* elsewhere calls God) *and how omnipotent:* whence immediately he adds, *And I am persuaded, that he is able to keep that which I have committed unto him against that day.* More texts of the same import might be added, if the design of those already alleged were other than to facilitate the admission of the more plausible truth we have been making out, and which to us seems very clearly held forth in those and the like scriptures, which are therefore cited out of the new testament, that they might have the greater authority with one sort of our antagonists. *I press toward the mark of the prize of the high calling of God in Christ Jesus. Blessed are they, that do his commandments, that they may have right to the tree of life, and may enter in through the gates into the city; laying up in store for themselves a good foundation against the time to come, that they may lay hold of eternal life. To them, who by patient continuing in well-doing, seek for* (ζητοῦσι) *glory, and honour, and immortality, eternal life.* And of Christ himself, whose love to God is questionless filial and unequalable, it is said, *Looking unto Jesus the author and finisher of our faith, who, for the joy that was set before him, endured the cross, despised the shame, and is set down at the right hand of the throne of God.* Nor see I, why it should be unfilial for a child of God to further the raising of those passions, which his heavenly Father intends to have raised in him, upon the same grounds and motives, that God is pleased to employ to excite them. And since the scripture seems plainly to invite our hopes, by recording St. *Paul*'s having said, *Every man, that striveth for the mastery, is temperate in all things; now they do it to obtain a corruptible crown, but we an incorruptible:* And by representing our Saviour himself, as saying in one place, *Rejoice, and be exceeding glad, for great is your reward in heaven:* In another, *Be thou faithful unto death, and I will give thee a crown of life.* And in divers others speaking to the like purpose; since, I say, the scripture seems thus to allure our hopes, would it not be a kind of accusing it of an aptness to delude and ensnare us, to teach, that it proposeth to us the powerfullest objects to incite our passions, if it be sinful to cherish and harbour the passions naturally belonging to those objects? And certainly, *Lindamor*, since God, who, as our Creator, knows the frame and constitution of man's soul, incomparably better than he himself, is pleased to deal with our hopes and our fears, to engage us to his service; it very ill becomes us, either to quarrel with his methods of working on our spirits, or to reject any help, which he has been pleased to afford a piety, which, for aught ever I could observe, does even in the best men find resistance enough to keep any help, that can

Heb. iv. 1.
1 Cor. ix. 27.
Luk. xii. 4, 5.
1 Pet. iv. 19.
2 Tim. i. 12.
Phil. iii. 14.
Rev. xxii. 14.
1 Tim. vi. 19.
Rom. ii. 17.
Heb. xii. 2.
1 Cor. ix. 25.
Mat. v. 12.
Rev. ii. 10.

can be employed to promote it, from being superstitious. And truly, the animating or discouraging influence, that hope, or the want of it, is wont to have upon our endeavours, makes me very apprehensive, that since the enlivening hopes of heaven are not able to make most men's endeavours other than very languid, the forbidding those supporting hopes would soon weaken and decrease our endeavours into none at all.

BUT, *Lindamor*, though I may perhaps have taken some pains in studying controversial divinity, yet I take so little pleasure in writing of it, that though not only a seasonable duty to truth, but a necessary one to the ensuing part of this discourse, have pressed me to serve in this cause; yet I shall perhaps obtain your pardon sooner than my own, for having thus long suspended the discoursing to you of the advantageousness of God's love to us, as it gives us here a right, and will hereafter give us admission to heaven; heaven, the bright seat of so much happiness, that we shall scarcely count amongst our joys, that heaven is the seat of them. There the excellency of the possessed goods shall as much disappoint our expectations, as in other fruitions the emptiness is wont to do. The Apostle tells us, that *eye hath not seen, nor ear heard, neither have entered into the heart of man, the things which God hath prepared for them that love him.* Such pure refined delights not only stoop not unto sense, but are sublime enough even to transcend imagination. When fancy hath formed and shaped the perfectest ideas, that its abstractions can make, of blessedness; our own more happy experiences of greater must disabuse us, when we come to heaven; which is a soil, whose fruitfulness is so confined to joy, that even our disappointments and mistakes shall there contribute to our happiness; which will so much partake of his immensity, whose gift it is, that you see the Apostle gives it a negative description; and to create in us apprehensions underogatory from what we shall possess, not only removes our thoughts from all we do enjoy, but exalts them above all that we can fancy. At which way of proceeding, that you may the less wonder, *Lindamor*, be pleased to consider, that in heaven our faculties shall not only be gratified with suitable and acceptable objects, but shall be heightened and enlarged, and consequently our capacities of happiness as well increased as filled. A child not yet released out of the homely prison of the womb cannot there possibly frame ideas of those delights, which will be afforded him by the pleasing noises, and the glittering objects, that will present themselves to him after his birth. And the same child, whilst he continues in his non-age, though he may with delight look upon emblems finally drawn and painted, and may take some pleasure in beholding the neat and surprizing characters and flourishes of a Greek and Hebrew bible curiously printed; yet he cannot then imagine the pleasure the same objects will afford him, when age and study shall have ripened and instructed his intellectuals, and made him capable both of understanding and relishing the excellent moralities, couched in those ingenious emblems, and the profound and saving mysteries, wherewith that divine book, the scripture (especially in its original tongues) does, to an intelligent and religious peruser, appear replenished. Such a double advantage, *Lindamor*, among others, the admission into heaven brings those, to whom that blessing is vouchsafed: for besides that set of objects (if I may so speak) so new and so peculiar to heaven, that their ideas could never enter into men's thoughts before their admission there; besides this, I say, our then enlarged capacities will enable us, even in objects which were not altogether unknown to us before, to perceive things formerly undiscerned, and derive thence both new and greater satisfactions and delights. Wonder not, *Lindamor*, that in mentioning the joys of heaven, I use the expressions I find less detractory from a theme, as much

Sect. 20.

1 Cor. ii. 9.

above our praiſes, as the heaven they are enjoyed in is above our heads. For, though ſuch expreſſions may ſeem ſomewhat tumid and aſpiring, and fitter much for one that celebrates, than for one that but aſſerts; yet cannot I ſcruple to uſe ſeeming hyperboles in the mention of felicities, which make the higheſt hyperboles but ſeeming ones. For thoſe joys, *Lindamor*, of heaven, are like its ſtars, which, by reaſon of our remoteneſs from them, appear extremely little, though really in themſelves they are ſo vaſt, that a leſs than the largeſt is by odds greater than the biggeſt object upon earth, nay, than the whole earth itſelf. And therefore as if I were to take you with me to contemplate the planets, I would ſhew you them through ſuch a teleſcope, as by a greatning thoſe bright objects in compariſon of what to an unaſſiſted eye they appear, doth ſomewhat leſſen the diſadvantage of remoteneſs, and ſhews them with leſs detraction from their true magnitude; ſo mentioning to you the felicities of heaven, I think it not unlawful or improper to endeavour, by repreſentations tranſcending what they appear, to give you notions leſs inferiour to what they are.

In heaven, then, we ſhall taſte happineſs enough, to enable us to rectify the definition of it. We may there be inſtructed, how to name and rate all goods, by thoſe that will concentre into the felicity we ſhall poſſeſs, which ſhall be there made up of the confluence, perfection, and perpetuity of all true joys. For heaven will make us happy, not (as philoſophy pretends to do) by the confinement, but by the fruition of our deſires; which ſhall neither fail in the choice of their objects, nor miſs of the enjoyment of them, but be both unerringly juſt, and infallibly accompliſhed; in the former of which properties (as our then rectified reaſon will conſider things) we ſhall think ourſelves happier (yet) than in the latter. We ſhall there reſemble the ſaints we here admire, and ſhall not only ſee, and be like thoſe pious worthies, whoſe virtues eclipſe theirs, which among the heathen deified leſs deſerving heroes; thoſe excellent perſons, that did as well ennoble as inſtruct mankind, giving us cauſe to bluſh, that we are men, and whoſe ſtories have the unparallelled honour
Heb. xii. 23. of being recorded by inſpired pens; thoſe ſpirits, I ſay, *of juſt men made perfect*, as the ſcripture terms them, ſhall be our conſtant and familiar company, into whoſe bleſſed ſociety we ſhall not only be welcome, but increaſe it. In heaven, we ſhall have a bleſſed and familiar converſation with thoſe ſame glorious ſpirits, whoſe nature doth inveſt them with ſuch a luſtre, that all the diſadvantage of their diſguiſes, when they appear to us, doth ſcarce ſuffice to confine our raptures to reſpects below idolatry, and darken them into objects for our wonder, not adoration. There we ſhall ſee (a ſight worthy dying for) that bleſſed Saviour, of whom the ſcripture does ſo much and ſo excellently entertain us, and who having done and ſuffered ſo much for us, does ſo highly deſerve of us, both upon the account of his ineſtimable benefits.
Ἐσκήνωσεν ἐν ἡμῖν. Yes, there ſhall we ſee that holy and divine perſon, who, when he vouchſafed (as his
John i. 14. favourite-diſciple ſpeaks) *to pitch his tent among us*, and dwell with men on earth, to fit them by his merits and example to dwell with him in heaven, did ſo admirably mix an awful majeſty with an humble meekneſs, and the aſſumed infirmities of his human nature with the ſeaſonable coruſcations of his divine; and expreſſed in his whole life ſo perfect and exemplary a virtue, and yet ſo much ſweetneſs and gentleneſs towards thoſe aſpirers to it, that were the moſt ſhort of it; that the Jews them-
Mat. vii. 37. ſelves could ſay of him, that *he had done all things well*; and his very enemies, that were employed to apprehend him as a malefactor, confeſſed even to thoſe, that ſent
John vii. 46. them to do ſo, that *never man ſpake like him*. And his Apoſtles, who had moſt opportunity to pry narrowly into his actions, and were of a condition and breeding very unlike to infuſe into them heroic reſolutions, did in ſpight of the frequent reproofs

 their

their failings extorted from him, and of the hardſhips that attended his ſervice, think even death itſelf in his company, more eligible, than life led out of it; *let us alſo go; that we may die with him* (ſays even the diſtruſtful *Thomas* to his fellow-diſciples.) John xi. 14
But, *Lindamor*, we ſhall there ſee the Son of God, not in that *form of a ſervant*, which he put on that he might ſuffer for us, and exerciſe his prieſtly and prophetic function here below; but in that regal ſtate and condition, which belongs to him by virtue of his kingly office; on whoſe ſcore he is ſtyled in the ſcripture, *King of Kings, and Lord of Lords*, all power or authority being (as himſelf ſpeaks) given him both in heaven and in earth. And how nobly attended may we ſuppoſe this divine monarch to be in his exalted condition in heaven, when in his ſtate of humiliation on earth, whilſt he was in the *wilderneſs among the wild beaſts, the angels* are recorded to have *miniſtred unto him*; and whilſt he lay ſwathed in a homely manger, the multitude of the celeſtial hoſt were heard to ſolemnize his miraculous birth, according to that paſſage of the writer to the Hebrews, *when he bringeth in the firſt begotten into the world* (he ſaith) *and let all the angels of God worſhip him!* And yet ſuch conſiderable and noble creatures thoſe immaterial intelligences, called angels, are, that one of them in one night was able to deſtroy above a hundred and fourſcore thouſand men in the blaſphemous *Senacherib*'s impious camp. And ſo much majeſty and ſuperiority does their nature give them in reference even to the eminenteſt of mortals, that when the undaunted *Joſhua* had boldly challenged one of them, that appeared to him in the likeneſs of a man, and demanded, whom he was for; when he knew him to be an angel (unleſs he ſuppoſed him to be that promiſed Meſſiah, who is elſewhere called *the angel of the covenant*, as it is in the original, and in the ſame text *the Lord*) he alters his addreſs unto him into this ſubmiſſive one, *What ſaith my Lord unto his ſervant?* And even wiſe and holy *Daniel* himſelf, who was the ſecond perſon of the world for power, and the firſt for much nobler attributes, wiſdom, and underſtanding, who ſaw ſo many governors of vaſt provinces, and profeſſors of admired magic, inferiour to himſelf, yet confeſſes to the angel, that appeared to him, that he was confounded and diſanimated at his preſence, and adds, *How can the ſervant of this my Lord talk with this my Lord?* We may ſafely therefore expect, that we ſhall then behold our Saviour, not in thoſe diſguiſes, which disfigured him in their eyes, that only conſidered his ſufferings; but in that triumphant condition, wherewith they are now rewarded. The thorns of his (now no more galling, but adorning) crown will appear upon that radiant head of his more glorious, than thoſe of the flaming buſh, wherein God appeared to *Moſes*. And we ſhall not ſee that deſpicable form, which made the prophet ſay (perſonating the generality of the Jewiſh nation contemporary with the Meſſias)—*he hath no form, nor comelineſs, and when we ſhall ſee him, there is no beauty that we ſhould deſire.* But we ſhall ſee him encompaſſed with ſo much majeſty, and ſhining with ſo much of his genuine ſplendor and beauty, that we ſhall deſervedly eſteem him *Nepble, the Admirable.* And our raviſhed ſouls ſhall, by the more attentive contemplation of him, but find more cauſe to imitate the ſpouſe in *Solomon*'s myſtic epithalamium; who having dwelt upon the beauty of the ſeveral parts, that concurred to the accompliſhing the divine bridegroom, exclaims in an epiphonema, very contrary to the expreſſion lately mentioned in the prophet, he *is altogether deſires*, or by an Hebraiſm, *moſt deſirable*, which our Engliſh renders *he is altogether lovely.* His eyes will there appear as St. *John* repreſents them, of active fire, and will into the willing breaſts of the raviſhed beholders ſhoot flames as pure, as holy, and as deathleſs, as thoſe fathers of the church, who believed not the angels altogether incorporeal, may be ſuppoſed to have conceived the ſeraphims to conſiſt of.

Phil. ii. 7.
Rev. xix. 16.
Mat. xxviii. 18.
Mat. i. 13.
Luke i.
Heb. i. 6.
2 King xix. 35.
Mal. iii. 1.
Joſh. v. 13, 14.
Dan. x. 17.
Exod. iii. 2.
Iſa. liii. 2.
Iſa. ix. 6.
Cant. v. 10.
Rev. i. 14.

of. Certainly, since (as the scripture informs us) *it never entered into the heart of man, what God hath reserved in heaven for those that love him*; that glory can be but imperfectly expressed by the bare epithet of inconceivable, with which God rewards the meritorious sufferings and obedience of that only begotten son of his love, for whose
Dan. xii. 3. sake he is pleased to confer on so many thousands of men unimaginable glories. He, that vouchsafes even to many of his servants a brightness like that of the stars, will
Mal. iv. 2. sure communicate a more radiant lustre to the *sun of righteousness*, that only son of
Heb. i. 2, 3. his, *whom* (to borrow inspired expressions) *he hath made heir of all things, by whom also he made the worlds: who being the brightness of his glory, and the express image of his person, and upholding all things by the word of his power, when he had by himself*
Eph. i. 21. *purged our sins, sate down on the right hand of the majesty on high, far above all principality, and power, and might, and dominion, and every name that is named, not only in this world, but in that which is to come.* Thereby exalting him not only above all earthly princes and potentates, but even above the highest orders of the spirits of the celestial hierarchy.

Sect. 21. But apprehend not, *Lindamor*, that this sublime exaltation of Christ will make him despise the meanest of his saints, or disdain communion with him. For, St.
Phil. ii. 7. *Paul* assures us, that he was in the *form of God*, when he vouchsafed to take upon him the form of a servant, to make us free: and sure, he that condescended so far, and stooped so low, to invite and to bring us to heaven, will not refuse us a gracious reception there. In the days of his flesh he was pleased to own *Lazarus*, even in the dishonours of the grave, and vouchsafed him in that despicable condition the glorious title of his friend; and when he descended the Mount of Olives, all the loud acclamations of the glad multitude, that sang *Hosannahs* to him, and strewed his way with their palms, their vestments, and their praises, could not divert him from deploring,
Luke xix. 37, 38, 41. even with tears, *Jerusalem*'s approaching fate; and expressing in the midst of his triumphs, a concern for the very worst and stubbornest of his enemies. And, lest it should be thought, that he was thus concerned for worthless mortals, only whilst he shared and felt the miseries of their condition; give me leave to observe to you, *Lindamor*, that immediately after his resurrection, whilst the sense of the sudden and unexampled change of his condition was fresh upon him, and the remembrance of the Apostles ingratitude in deserting him, almost as recent, he sticks not to give those very disciples, that forsook him, the glorious title (and that a more familiar one
Matt. xxviii. 10. than we find him to have allowed them on earth) of his brethren; and particularly
Mark xvi. 7. shews himself solicitous for that *Peter*, who, in spight of all those empty boasts, wherewith he not only seemed to defy death, but Christ's prediction too, did not only
Acts vii. forsake his Master, but denied him. He can in heaven be so concerned for his distressed members here on earth, that not only he can take notice of expiring *Stephen*, thorough that cloud of fatal stones that battered down his prison; and can, when
Acts ix. 4. *Saul* was persecuting the vulgar Christians, cry out unto him, *Saul, Saul, why persecutest thou me?* as if he, and those that love him, were but one; but in the messages
In the second and third chapters of the Apocalypse. he sends to the Angels or governors of the seven churches, he sufficiently manifests, that single Christians may be as particularly in his thoughts, as if they had no other object: his greatness will not make his kindness less familiar, but only more obliging;
Rev. iii. 20. he disdains not, even after his ascension, to say, *Behold, I stand at the door and knock, if any man hear my voice, and open the door, I will come in to him, and will sup with him, and he with me.* That king, in the parable, by whom himself is represented, is
Matt. xxv. pleased himself to welcome each individual trusty servant with a peculiar *Euge, bone serve.* And in another parable he scruples not to represent the great condescensions

and

and familiarity, wherewith the Lord shall remunerate the faithful and watchful servant, by telling his disciples, *Blessed are those servants, whom the Lord, when he cometh, shall find so watching. Verily I say unto you, that he shall gird himself, and will make them to sit down to meat, and will come forth to serve them.* Luke xii. 37. At which expression you will the less admire, if you consider, that besides that it is parabolical, and probably hyperbolical, and therefore not to be taken (as the French speak, *au pied de la lettre*) in a strict sense; we find in the evangelical story, that our Saviour disdained not to converse familiarly with publicans and sinners, nor even to wash the feet of his own disciples. And since he condescended to such familiarity to those frail mortals, so many of whom were apt to turn the favours of God into wantonness, we may the less admire at his gracious condescensions to those, the sinlessness of whose condition will keep them from turning his vouchsafements into any thing, but occasions of joy and gratitude. Marvel not, *Lindamor*, as at an impertinency, that I appear so solicitous to possess you with high apprehensions of the supereminent dignity and felicity of our exalted Saviour; and that in the loftiest and most magnificent expressions, the scripture affords me, I have endeavoured to make out to you, that the bright sun of righteousness is now grown uncapable of suffering eclipses, but shines with unclouded and unequalled splendor; and that we shall in heaven (as the writer to the Hebrews speaks) *see Jesus, who was made a little* (or for a short while, βραχυ τι) *lower than the angels, for the suffering of death, crowned with glory, and honour.* Heb. ii. 9. For it will make heaven more heavenly to us, to find him reigning there, who hath suffered so much for us; and for whom, if we ever come thither, we shall have so much suffered. For, since our love will, as the rest of our surviving graces, be perfected and grow compleat in heaven; the ineffable happiness of our dear Redeemer must needs bring an increase to ours, commensurate to the ardency of our love for him, and we shall relish no happiness more than that, which is therefore ours, because he is happy, and supposes in him, what it confers on us. Besides that, since the regal dignity, wherein (as Mediator) he is instated, may make us owe our salvation to his sentence, as well as to his merits; heaven itself will be inestimably endeared to us, by our injoying it upon his account. I need not tell you, what a value lovers are wont to set upon the least favours, that can be bestowed on them by their mistresses; and that oftentimes a worthless ribband, a bracelet of hair, or some such trifle, that nothing can make precious but her representing it, is highlier valued by the transported lover than the richest presents of nature, or of fortune. How great a blessing must it then be, *Lindamor*, to owe joys, that need not endearing circumstances to merit the name of happiness, to a person so beloved, that even heaven itself will bring more felicity to the amorous soul, as it is a proof of his love, than that it is a donative of his bounty: as passionate brides do incomparably more value their wedding-rings which their glad lovers give them, as pledges, than as gold. It is said of the persecuted and disgraced Apostles, that *they rejoiced to have been counted worthy to suffer shame for his name.* Acts v. 41. But if they deservedly rejoiced to be allowed to suffer for him, of how much joy shall they have just cause, that are admitted to reign with him? His having supported the hardships and the toils (for I must not now call them evils) to which our afflicted condition of mortality is exposed, does so much alleviate them, and refresh us under them, that in this sense also it may be truly said, that *the chastisement of our peace was upon him, and by his hurts we are healed.* Isa. liii. 5. But he that relieved us even by his cross, will sure do more for us by his crown, when he shall admit, and even invite, each faithful servant to no less a blessing than *to enter into his master's joy.* Matt. xxv. 21. So rich a source of happiness did Christ design to make himself to us in all his capacities and conditions, that

that in heaven and in earth, it was, and will be, his gracious and constant imployment to share our griefs, or impart to us his joys; and either lessen our miseries by his sufferings, or increase our happiness by his felicity.

EXPECT not, *Lindamor*, that I should solemnly beg your pardon for this seeming excursion, till I come to think it a digression, to insist on the blessedness of Christ in heaven, treating of the happiness, which those that love him will enjoy there. Wherefore, not to spend time in imploring a needless forgiveness, I shall proceed to tell you, *Lindamor*, that in heaven we shall not only see our elder brother Christ, but probably also our kindred, friends, and relations, that living here in his fear, died in his favour. For, since our Saviour tells us, that *the children of the resurrection shall be* ἰσάγγελοι, *equal to*, or like *the Angels*; who yet, in the visions of *Daniel* and St. *John*, appear to be acquainted with each other; since, in the parable of the miserable Epicure, and the happy beggar, the father of the faithful is represented as knowing, not only the person, and present condition, but the past story of *Lazarus*: since the instructer of the Gentiles confidently expects his converted and pious Thessalonians, to be *his crown at the great day, when the having turned many to righteousness*, shall, as the scripture foretels, confer a star-like and immortal brightness: since (which is chiefly considerable) the knowledge of particular actions, and, consequently, persons, seems requisite to the attainment of that great end of God, in the day of judgment, the manifestation of his punitive and remunerative justice: since, I say, these argumets, besides divers others, are afforded us by the scripture, we may safely conclude it probable, that we shall know each other in a place, where, since nothing requisite to happiness can be wanting, we may well suppose (at least, if we can imagine here, what we shall think there) that we shall not want so great a satisfaction, as that of being knowingly happy, in our other-selves, our friends. Nor is this only probable, *Lindamor*, but it is not improbable, that those friends, that know us in heaven, shall welcome us thither. For, since Christ assures us, that the very angels (though they be so far from being related to our persons, that they are foreigners to our very nature) receive accession of joy for a relenting sinner, that by repentance begins to turn towards God; you will think it absurd, that in a place where charity shall not only continue (as St. *Paul* speaks) but grow perfect, our dear friends should rejoice to see us, not only begin to turn towards God, but come home to him. Nor is it unlikely, that our transported souls shall mutually congratulate each other, their having now fully escaped the numerous rocks, and shelves, and quicksands, and threatening storms, and no less dangerous calms, through which they are at length arrived at that peaceful haven, where innocence and delight (which are here so seldom matched) are inseparable companions, both of each other, and each blessed resident. With those friends we here lamented, we shall there rejoice. And it will be but need, that the discovery of each other's virtues should bring us to a mutual knowledge of our persons; for otherwise, we shall be so changed, that we should never know our friends; and should scarce know ourselves, were not an eminent increase of knowledge a part of that happy change. For those departed friends, whom at our last separation we saw disfigured by all the ghastly horrors of death, we shall then see assisting about the majestick throne of Christ, with their once *vile bodies transfigured into the likeness of his glorious body*; mingling their glad acclamations with the hallelujahs of thrones, principalities, and powers, and the most dignified favourites of the celestial court. I need not tell you, *Lindamor*, that we shall be more justly transported at this meeting, than was good old *Jacob* at that of his son *Joseph*, whom having long mourned for dead and lost, he found not only alive, but a great favourite,

Luke xx. 36.
Luke xvi.
1 Thess. ii. 19, 20.
Dan. ii.
Luke xv. 7.
1 Cor. xiii. 8.
Phil. iii. 21.

rite, ready to welcome him to an unknown court. For, whereas the patriarch said to his son, *Now let me die, since I have seen thy face*; the seeing of our friends in heaven will assure us, that we shall for ever live with them there. The re-union of friends being there as privileged from divorce, as that of soul and body, (which scarce will be more strict and satisfactory.) For, here indeed, if our friends do not allay our love or affection by unwelcome actions, or their contagious sufferings, we commonly doat on them to a degree, that, as it were, reduces God to deprive us early of them, and snatch our idols, and his rivals, from us. But there, our, on both sides, compleated graces will not only deserve, but allow a higher strain of friendship. The near contemplation and fruition of the infinitely transcendent perfections of the Creator, keeping all our kindness to the creatures not only subordinate to the love we owe to God, but grounded on it; as excited needles, when they stick fastest to each other, owe their union to their having both been touched by the load-stone; to which they have therefore both of them stronger inclinations, than either to the other. Gen. xlvi. 30.

THERE, probably, we shall satisfactorily understand those deep and obscure mysteries of religion, which the profoundest clerks, that love not to flatter themselves, acknowledge they are unable fully to comprehend; being, after all the toil and industry of their anxious enquiries reduced to sit down with the Apostle's ὦ Βάθος, in admiration of that depth, whose bottom they cannot fathom. There we shall understand those obscure passages of (that divine book, and uncapable of flattery) the scripture, which, notwithstanding all that bold criticks, and learned expositors, have attempted to illustrate it, does still continue, in many places, obscure. There, discerning how exquisitely the several parts of scripture are fitted to the several times, persons and occurrences, wherein their all-foreseeing author intended most to use them; we shall discern not only a reconcileableness, but a friendship, and perfect harmony, betwixt those texts, that here seem most at variance; and shall discover not only the sense of the obscurer passages, but the requisiteness of their having been written so obscurely. That strange and peculiar, as well as otherwise cryptical method and style of scripture, which often costs us so much study to find it rational, we shall there discover to be admirable, and worthy of its omniscient author. There, I hope, we shall have clearly expounded to us those riddles of providence, which have, but too often tempted, even good men, to question God's conduct in the government of the world; whilst the calamities and persecutions of virtue, and innocence, seem approved by him, who accumulates prosperities on their criminal opposers. There we shall be convinced, that all these seeming irregularities, which the heathen thought fit to impute to the giddy whimsies of a blind female deity, are not only consistent with God's justice and goodness, but are productions of it. And though such a belief do here, to intelligent persons, seem perhaps a greater piece of self-denial, than to refrain from wine, or gold, or mistresses; yet in heaven it will appear as reasonable, as here we find it difficult. For, as *Bildad* speaks in *Job*, *We are but of yesterday, and know nothing; because our days upon earth are but a shadow.* And the shortness of our transitory lives not permitting us to continue long enough spectators here, to see above a scene or two at most of that great play, acted by mankind upon the stage of the world; it is no wonder we are apt to harbour sinister thoughts of the contriver of a plot, whose neither beginning nor end we are acquainted with: which yet is little less injurious, than it were to censure the lofty tragedian *Seneca*, or some other matchless artist, having perused but a piece of some tragedy, whereof the latter part never arrived at our view. But, when once God's whole plot (if I may so speak) and conduct in the administration of the world shall come to be disclosed; all Sect. 24. Rom. xi. 33.

 those

those revolutions and occurrences of empires, states, families, and particular persons, which men are here so prone to quarrel with, will there appear so just, so requisite, and so seasonable, that those very things, which here tempted us to deny God, shall there engage us to praise him; and we shall not so properly be satisfied with his providence, as ravished. But especially, we shall be transported with wonder and gratitude, when God shall vouchsafe to discover to his particular servants, the reasons of his dispensations towards them, and make out to them not only the necessity and justice, but even mercifulness of those very afflictions, that were most imputed to his severity, (no stroke from God's paternal hand, either lighting sooner, or falling heavier, or staying longer, than the occasion, that extorted it, exacted) and convince them, that their hopes were never disappointed, but to secure their title to better things than those they hoped for; nor their inferior interests prejudiced, but for the advantage of their supreme ones. Yes, all that unwelcome darkness, that here surrounded our purblind understandings, will vanish at the dawning of that bright, and (as St. *Peter*'s * expression may be interpreted) eternal day; wherein the resolution of all those difficulties, which here exercised (and perhaps distressed) our faith, shall be granted us to reward it. And I must profess to you, *Lindamor*, (as unfashionable as such a profession may seem to a gentleman not yet two and twenty) that I find the study of those excellent themes, God's word, and his providence, so difficult, and yet so pleasing and inviting, that could heaven afford me no greater blessing, than a clear account of the abstruse mysteries of divinity and providence, I should value the having my understanding gratified and enriched with truths of so noble and precious a nature, enough to court heaven at the rate of renouncing for it all those unmanly sensualities and trifling vanities, for which inconsiderate mortals are wont to forfeit the interest their Saviour so dearly bought them in it.

2 Pet. iii. 18. * ἡμέρα αἰώνος.

Sect. 25. Nor shall we only converse with saints and angels, but with that infinitely more glorious Deity, that made them what they are, without at all impoverishing himself. In heaven, we shall enjoy (its maker) God, and *see him as he is*, who (as the scripture telleth us) shall then be *all in all*; comprising all the goods, we value in the creatures, as eminently and fully, as the sun doth the light, that twinkleth in the stars.

2 John iii. 2. 1 Cor. xv. 28.

Anaxagoras. If one, that was none of the least of the philosophers, scrupled not to tell the man, that asketh him, what he was born for, that it was to contemplate the sun; if our best naturalists themselves, though the darkness, that is here cast upon things, and the dimness of our intellectual eyes, (which I remember *Aristotle* fitly compares to those of an owl at noon-day) permit us to discern but very little of that wisdom, power, and goodness, which he has expressed in the creation; are yet often transported and ravished with a just admiration of the perfections he has displayed in his workmanship: if the wise queen, that came so far to visit *Solomon*, was put almost into an extasy, by the sight of his (though wise, yet human) ordering of things: and if the *angels themselves* (as St. *Peter* informs us) *desire to look into the mysterious contrivances of God*, in order to man's redemption: how great a satisfaction, *Lindamor*, may we justly conceive it must needs be, to be admitted to see so much of God, as heaven will discover to us? especially, since so much of our future happiness will consist in that beatific vision, (as schoolmen justly call it) that St. *John* concluded, that *we shall be like God*, because *we shall see him as he is*. And our Saviour himself paraphrases our celestial felicity by this blessed vision, where he says, *Blessed are the pure in heart for they shall see God:* As on the other side, the writer to the Hebrews employs the being denied the sight of that divine object, as a description of extreme wretchedness, in that text, where, having exhorted those, to whom he writes, to *follow*

1 Pet. i. 12. 1 John iii. 2. Mat. v. 8.

follow peace and holineſs, he adds, as the formidableſt menace which he could make use of, to deter them from ſlighting his exhortation, *without which, no man ſhall ſee the Lord.* And by this viſion our Saviour ſeems to deſcribe the happineſs even of angels; where, forbidding the ſcandalizing of any of thoſe little ones that believe in him, he adds, to enforce what he had ſaid, that *their angels do always ſee the face of his Father in heaven.* We ſhall be ſo taken up with the contemplation and fruition of that glorious object, (in whoſe infinity all goods are included and dilated) that ages, numberleſs as the joys that beatific viſion abounds with, will ſcarce afford us leiſure for a diverſion to any other pleaſures, than thoſe itſelf creates. Which are ſo numerous and ſo entire, that we ſhall there deſire nothing that we have not, except more tongues to ſing more praiſes to him, or at leaſt a capacity to pay him greater thanks for what we have. And even thoſe deſires, God's gracious acceptation will make, in being conceived, accompliſhed: for, otherwiſe heaven's reſidents ſcarce know any other want, than that of need to wiſh; the compleat bleſſedneſs of their condition reducing them to a happy uſeleſſneſs of wiſhes, by affording them a full prepoſſeſſion of all the objects of deſire. There time, like fire, having deſtroyed whatever it could prey on, ſhall, at laſt, die itſelf, and ſhall go out into eternity. Whoſe nature is ſuch, that though our joys, after ſome centuries of years, may ſeem to have grown elder, by having been enjoyed ſo many ages, yet will they really ſtill continue new, not only upon the ſcores of their welcomeneſs and freſhneſs, but by their perpetually equal (becauſe infinite) diſtance from a period. There our felicity ſhall always be the ſame, yet ever new. Wearineſs arguing imperfection, either in the object or the appetite; the former of which is impoſſible in God, and the latter ſhall ceaſe in heaven. Where our felicity ſhall be ſo great, that variety itſelf ſhall not be needed as a part of it. And, if heaven do admit variety, it may be ſuppoſed ſuch a one, as ſhall conſiſt in a further knowledge of the firſt object (God) not a forſaking of it; and ſuch as ariſes from the fixed beholding of the changing necks of doves, or ſuch as we may ſee in the diverſified refractions of the ſame ſparkling diamond. In God there is (if I may ſo ſpeak) ſuch a various identity, that the fruition of him both ſatisfies and creates deſires: though that, without ſatiety; and this, without diſquiet. Other delights, like the uſual clothes we wear, quickly grow ſtale, and are ſoon worn out; whereas celeſtial pleaſures participate the prerogative of the Jews garments in the wilderneſs, of not impairing by being uſed long. But as the amorous needle, once joined unto the loadſtone, would never, uncompelled, forſake the inchanting mineral; but, after ages, cling no leſs cloſely to it than the firſt moment of their union: ſo do the ſaints in heaven, with the ſame undiminiſhed freſhneſs, ever poſſeſs their joys, as if each moment were the firſt that they poſſeſſed them in. And, if their happineſs does not improve by laſtinger enjoyment, it is becauſe it was firſt uncapable of increaſe. Or, if our pleaſures do admit acceſſion, they ſhall receive it from our aſſurance, that we ſhall taſte them for ever, and perpetually repeat (or rather continue) the ſame renewed fruitions to an eternity, endeared by nothing more than by the quiet leiſure it will afford us, undiſtractedly to employ it, in celebrating of the author's praiſes; and in a condition, happier in that by it we grow paſt doing, than by paſt ſuffering ill. In heaven (in a word) our inexhauſted joys will be ſo numberleſs, and ſo immenſe, that we ſhall need (as well as have) eternity itſelf to taſte them fully.

Heb. xii. 14.

Mat. xviii. 10.

Deut. xxix. 5.

But, *Lindamor*, it was not my deſign to give you a particular topography of this celeſtial *Canaan*; but only in a few words to let you ſee, that *it is a land flowing with milk and honey.* And though I acknowledge, I have given you but dark deſcriptions (and ſometimes rather poetical than chorographical) of what the apoſtle ſtyles *the inhe-*

Ex. iii. 8.

Col. i. 12. *inheritance of the saints in light:* yet since, to whet our longings for fruitive (or experimental) knowledge, it is reserved among the prerogatives of being in heaven, to know how happy we shall be, when there; and since, what the scripture hath revealed of paradise, seems meant rather to quicken our obedience, than satisfy our curiosity; I may, for those purposes, have perhaps tolerably performed that task (of heavenly topography) by the acknowledgments of my disability to do it worthily. I shall now only add this property of our expected bliss, that the vast multitude of partners does detract nothing from each private share, nor does the publickness of it lessen propriety in it. This ocean of felicity being so shoreless, and so bottomless, that all the saints and angels cannot exhaust it; it being as impossible for an aggregate of finites to comprehend or exhaust one infinite, as it is for the greatest number of mathematick points to amount to, or constitute a body. Our neighbour-regions do all enjoy the benefit of light as well as we; yet we enjoy not less, than if they enjoyed none. Indeed there is this difference between the sun of righteousness and that of heaven; that, whereas the latter, by his presence, eclipses all the planets, (his attendants;) the former, though radiant with a much brighter splendor, will, by his Col. iii. 4. presence, impart to his saints, according to that of the Apostle; *When Christ, who is our life, shall appear, then shall we also appear with him in glory.* So that the elect, in relation to this sun, shall not be like stars, which his shining obscures and makes to disappear; but, like polished silver, or well-glazed arms, or those vaster balls of burnished brass, the tops of churches are sometimes adorned with, which shine not till they be shined upon, and derive their glittering brightness, and all the dazling fire that environs and illustrates them, from their being exposed (unskreened) to the Cant. vi. 3. sun's refulgent beams. *I am my beloved's, and my beloved is mine,* says every saint (with the spouse in the *Canticles*) to his Redeemer. *David* says of them that put Psal. xxxvi. 6. their trust in God, *That he shall abundantly satisfy them with the fatness of his house, and make them drink of the river of his pleasures:* as if he meant to insinuate, that, as when a multitude of persons drink of the same river, none of them is able to exhaust it, and yet each of them may have the full liberty of drinking as much as he can, or as much as he could, though none but himself should be allowed to drink of it; so, whosoever enjoys God, enjoys him wholly, or at least doth enjoy him so entirely, in relation to that man's capacity, that the fruition of whatsoever rests unenjoyed of God is forbidden by the immensity of the object, and not the prepossession of his rivals. The angels, though of a nature differing from ours, and thereby placed above the personal experience of our sufferings and infirmities, do yet so sympathise with us, that (as our Saviour informs us) they rejoice at the repentance of a sinner. Luke xv. 7. And, though the members of the Church militant, and those of the triumphant, live as far asunder as heaven is from earth, and are not more distant as to place, than Eph. iii. 15. differing as to condition; yet St. *Paul* reckons all the saints to be but *one family in heaven and in earth.* If then the disparity of residences, of qualities, and of conditions, cannot now hinder the lovers of God from being so concerned in one another; how much of endearing kindness may we suppose, that they will interchange, when both their love shall be perfected, and all those other graces too that are proper to cherish and increase it? For the same Apostle, who, to assist us to conceive the strictness of the union both betwixt Christ and his saints, and the saints among themselves, tells us, that *he is the head, and they are his body, and members in particular;* 1 Cor. xii. 27. teaches us to make this inference, that (to express his doctrine in his own words) *If one member suffer, all the members suffer with it; and if one member be honoured, all the members rejoice with it.* Yes, *Lindamor*, in that blessed condition, our wills being perfectly conformed unto our Maker's, no saint nor angel can enjoy his love without Ver. 26.

possessing

poſſeſſing a proportionate degree of ours. And then, ſince perfect friendſhip appropriates to each friend the croſſes and proſperities of the other, (as good *Barzillai* could not be highlier obliged by *David*, than by the king's kindneſs to his ſon) each ſeveral beatitude in heaven ſhall (in ſome ſort concern the whole ſociety, and) be ours: as aſtronomers teach us, that the earth receives addition of light by the ſun's beams beſtowed upon the ſtars, and from the moon reflecting upon her. And becauſe our perſonal capacities are too narrow to contain all that joy, we are (by the ſtrange arithmetick of friendſhip) in a manner multiplied into as many happy perſons, as there are ſaints and angels bleſſed in heaven. Our perfect union to our common head, and mutual communion with each other, applying and bringing home every felicity of theirs to us: this friendly reciprocal ſympathy teaching us each glorified ſaint's bleſſedneſs, and him ours, by a bleſſed circulation, which makes us increaſe, by our reſenting them, thoſe joys (of others) whoſe increaſe we reſent.

1 Sam. xix. 37.

But my thoughts are engaged in ſo good a company, *Lindamor*, that they keep me from conſidering how faſt the hours paſs, and have almoſt made me forget, that the time, which my occaſions allowed me for ſcribbling to you, is ſo far ſpent, that not now at laſt to reprieve you from the perſecution of my blunt pen, were to be almoſt as injurious to my own affairs, as to your patience. Hereafter yet I may poſſibly make you ſome amends for this, with riper diſcourſes of the nature and duties, or (if you will) the properties and returns of this love, to which I have hitherto preſented you ſome motives. To (the laſt of) which I might add, that, our love to the creatures is an earneſt, but to God it is a title; the one makes us the objects, but the other makes the object ours: that, ſince there is in love ſo ſtrange a magick, as to transform the lover into the object loved; we ought to be extremely careful of the dedication of a paſſion, which, as it is placed, muſt either dignify our nature, or degrade it: and not to addreſs any lower (or, which is all one, to any other) object, the higheſt intenſity of a love, which cannot ſtoop without our degradation. And theſe I might expatiate on, and recruit them with many other motives, additional to thoſe I have already inſiſted on; but that I may more properly reſerve them to the treatiſe of the properties of that love, whoſe nature ſo partakes that of its object, that there can hardly be produced more powerful motives to it, than the conditions of it. Since then (as I freſhly intimated) I cannot but fear, that your tired patience, as well as my urgent occaſions (though theſe will recall me to-morrow morning to my own weſtern hermitage) doth at preſent ſummon me to leave you: and ſince I cannot do ſo in a happier place than heaven, I ſhall ſuſpend my farewels, only to beg you to believe, that ſo noble a motive of exalting friendſhip, as the ambition of rendering mine a fit return for yours, hath ſo improved my kindneſs, that my affection, without wronging its greatneſs, could not expreſs itſelf by any leſs attempt than this of gaining you the greateſt, and the moſt deſirable of all goods, by elevating (that noble harbinger of your ſoul) your love to heaven: whoſe joys alone are not inferior to thoſe, which the being made inſtrumental to procure them you would really create in,

My dearest *Lindamor*,

Your moſt faithful,

Moſt affectionate, and

Moſt humble Servant,

ROBERT BOYLE.

From *Leſe*, this 6th of *Aug.* 1643.

ADVERTISEMENT.

WHILST the fourth edition of the foregoing treatise was upon finishing, it happened, that a devout and very ingenious friend of the author's having pressed to have the perusal of some occasional meditations, that lay by him, on several subjects, met, among others, with that which follows: and finding the subject to be of affinity with what is rather touched than insisted on, in the 65th page of the past treatise, * touching men's inability to praise God worthily, and having a great deal of partiality for the reflection, would needs have it annexed, as it was, to the newly finished edition: notwithstanding the author's having too just cause to urge, that, besides the imperfections, that are made necessary by the sublimity of the theme, such occasional papers, especially his, are wont to have peculiar unaccuratenesses, as being designed rather to entertain the writer himself than any other reader.

[* page 169. of the volume.]

AN OCCASIONAL REFLECTION UPON A LETTER

Received in *April* 1662;

Containing an account of what passed on the King's Coronation-day, in a little country town.

I NEED not, *Pyrocles*, after what we have been reading, tell you, that the writer of this letter thinks, that both in what he has said of the king, and in what he has done to solemnise his coronation, he has behaved himself rarely well. For I doubt not, but you easily discern by his way of writing, that he is highly satisfied with his performances, and expects that he shall, if not be thanked by the king, at least be mentioned in the news-book. But it will, I fear, be requisite to tell you, that this honest man is not alone of his mind; for being his landlord's bailiff, he is esteemed at that rate by his neighbours, and looked upon as a man very considerable in his parish; and is perhaps thought to have a right to pity most of those, that do not admire what he has now been doing. And yet you and I, who pretend not to be courtiers, can, in his rural encomiums, and in his ill-contrived way of honouring his prince, easily discover so much that might have been mended, and so much that might be laughed at, that if the king, according to his wonted graciousness, vouchsafe this action his smiles, it must not be in consideration of the suitableness of the performances to the occasion, but, partly as they proceed from a hearty, though ill-expressed, loyalty and love, and partly, as they afford him a subject of merriment. And

And not only the nice critics, who have seen those magnificent solemnities, and heard the eloquent panegyrics, wherewith the principal cities and assemblies in the nation have thought they did but part of what they should; and not only those assiduous courtiers, who, by the honour of a nearer access, have opportunities (denied to others) of discovering those particularities, that may best give a high veneration for a great person and a great prince, to those, that are qualified to discern and relish such things: not only these, I say, will have a quite other opinion of the rural praises, and antique ceremonies, that were so well liked a hundred miles from *London*; but this country-man himself, if he were admitted to the court, and bred a while there, would, in time, see so great a distance betwixt what he has done, and what a person better bred might have done, that he could not remember without blushes, what he now looks upon with triumph.

And now I must on this occasion confess to you, *Pyrocles*, that I have (on other rises) several times been revolving in my thoughts, what the angels think of those praises and descriptions of God, that men devise (for I intend not here to speak of those the scripture suggests) and wherein we are most applauded by others, and do oftentimes, perchance, applaud ourselves. For those celestial courtiers (if I may so call them) have several advantages to assist them in the celebration of our common master, which we poor mortals want. For first, they are free from those selfish and inordinate affections, that too often hinder us, either from discerning the excellency of divers of God's attributes and ways, or from duly acknowledging it. They have no sins to keep them from descrying the justness of what he does; they have no ingratitude to oppose the fuller resentments of his goodness; and they are not tempted, not to discern and adore his wisdom, for fear they should appear culpable for repining at his dispensations. And indeed, their longevity allowing them the full prospect, from end to end, of those intricate transactions of providence, of which short-lived mortals do commonly see but a part; they are questionless far more satisfied with the incomparably better contrivances they discern in the management of human affairs, than we are with the conduct or plots of the most skilfully written plays and romances. Besides, those happy spirits, of whom the scripture tells us, that *they stand before God*, and that they *continually see his face*, have by that privilege the blessed opportunities of discovering in the Deity they contemplate and serve, many excellences, which even they could never, but by experience have formed any thoughts of; and they see in one another's solemn adorations and praises a way of honouring the object of them, so much transcending the utmost of what we here aim at, that their homages to their Creator may well be supposed of a far nobler kind than ours. And lastly, when I consider, how much less unworthy thoughts and expressions touching things divine the same person may have, when come to his full maturity of age and parts, and whilst he was but a child in both; and when I consider, how much more advantageous conceptions of the wisdom displayed in the universe, and particularly in the contrivance of a human body, one, that is a true philosopher, and a skilful anatomist, may have, in comparison of a man illiterate, and unacquainted with dissections: when, I say, I consider these things, and compare the dim twilight of human intellects in this life with that clear and radiant light, which the scripture ascribes to angels; I cannot but think, that, having to the privilege of a much nearer access than is allowed us to contemplate God's perfections, the advantage of having incomparably more illuminated intellects to apprehend them with, they must frame otherguise conceptions of the divine attributes, and glorify the possessor at an otherguise rate, than is allowed to those, whose understandings are so dim, and whose residence is so remote from that blessed place, where the perfections they would extol are most displayed.

Luke i. Matth. xviii.

Assisted

Assisted by these and the like advantages, *Pyrocles*, those happy spirits may well frame notions, and employ expressions in honour of their Maker, so far transcending ours, that, though the angel's goodness keeps them, doubtless, from beholding them with contempt; yet, we may well think, they look upon them with such a kind of pity, as that, wherewith great wits and courtiers look upon the mistakes and imperfections of what they did and writ, when they were but school-boys; and as that, wherewith, when we shall be admitted to the society of the angels, we shall look back upon our former selves. No, *Pyrocles*, to praise God is a debt, which, though we should ever be paying, we must always owe; not only because the renewed obligations will last as long as we, but because, though the intire sum were possibly to be paid, we have no coin of the value, that would be requisite to make a payment of that nature. It is true indeed, that some men say much more than others upon a subject, on which none can say enough, and which even *the spirits of just men made perfect* can but imperfectly celebrate. It may be too, that the praises we pay to God procure us some from men, and perhaps even from orators and encomiasts; and though I hope no man can so far flatter himself, as to think he can flatter what he can never do right to; yet the zealousness of our endeavours, and the applause that others entertain them with, may perhaps tempt us to think, that, because in our expressions we have surpassed ourselves, we have almost equalled our theme: as if to make our praises too great for any other subject, were sufficient to make them great enough for God. But alas, how widely must we be mistaken! since our expressions, if we speak sense, can at best but fully represent our conceptions, and those being but the notions of a finite creature, must needs fall extremely short of perfections, which were not what they are, if they were not infinite. No, when we have employed the loftiest hyperboles, and exhausted all the celebrating topics and figures of rhetoric; when we have dressed metaphysical abstractions in poetic raptures; when we have ransacked whatever things are most excellent among the creatures, and having defæcated them, and piled them up together, have made that heap but a rise to take our soaring flight from; when we have summed up, and left beneath our expressions, all that we are here wont to acknowledge above them; nay, when instructed, as well as inflamed, and transported by that *inaccessible light*, that is inhabited by what we adore, we seem raised and elevated above all that is mortal, and above ourselves, and say things, that nothing else could either inspire or merit: even then, I say, those expressions, which any otherwise applied would be hyperboles, do but express our devotion, not the divine object of it, and declare how much we honour him, rather than what he is. And indeed, none but the possessor of an infinite intellect can be able to say, what the possessor of other infinite perfections deserves to have said of him. And whatever zealous skill we praise God with, we do far less honour him than injure him, if we think our aspiringest praises can arrive so far, as, I say not to reach, but so much as to approach their subject.

Heb. x.

1 Tim. vi. 16.

But, let not this inevitable impotence, *Pyrocles*, trouble or discourage us. Those blessed souls, that *follow the lamb whithersoever he goes*, do (as we are taught in the Apocalypse) make it their business, and find it their happiness, to spend a great part of their eternity in extolling him, by whom they are placed in a condition, where they can have no employment, but what is holy and noble. And even here below, the praising of God is a work, wherein we imitate, though we do not equal the angels, and are busied in the same employment, though not with the same skill. Nay, heaven itself exempts not its residents from an impotence, which belongs to creatures, not as they are imperfect ones, but as they are creatures. Even the members of the church triumphant do not triumph over this necessary impotence; their praises may need pardon,

Rev. xiv. 4.

pardon, even in a place where they can sin no more; and they can expect but from God's goodness the acceptance of those praises, that are improved, as well as occasioned, even by their being made partakers of his glory. Nay, in the prophet *Isaiah*'s extatic vision, the Seraphim themselves, that are introduced, as answering one another's glad acclamations to God, are likewise represented, as covering (out of respect their faces with their wings. But, *Pyrocles*, as I was saying, this unavoidable disability to say things worthy of God need not at all trouble us; since we pay our homages to one, whose goodness our expressions can as little equal, as they can his other attributes. He that created us, will not impute it to us, that we act but as creatures; and since he has declared, that *where there is a willing mind, a man is accepted according to what he has, and not according to what he has not*; the impotence I have been speaking of, ought to bring us rather joy than trouble, since the infinite distance betwixt us, without lessening his favourable acceptance of our praises, supposes the boundless perfections of him, whom those praises (through his goodness) help to give us an interest in; and no son would repine at his royal father's greatness, how immense soever, being sure, that greatness would not lessen his kindness. For it is less desirable to be able to describe the power and excellencies of him we have an interest in, than to have an interest in one, whose power and goodness exceeds whatever we can say or fancy of them.

Isa. vi. 2.

2 Cor. viii. 12.

To conclude, *Pyrocles*, since on the one side God is most truly said in the scripture to be so *glorious, that he is exalted above all blessing and praise*, and consequently, though *I could* (to use St. *Paul*'s phrase) *speak with the tongues of men and angels*; yet the highest things I could say of the divine perfections, must needs be therefore far below them, because a creature were able to say them. And, since on the other side, it is of us men, that God vouchsafes to say, *whoso offereth praise, glorifieth me*; and his transcendent excellency is so far from being inconsistent with a resembling graciousness, that such a benignity is one of the most conspicuous parts of it: I will not forbear to pay my praises unto one, whose deserving infinitely more than I can offer keeps him not from accepting as much less than he deserves. But then I must not presume to *fill my mouth with his praises*, without sensibly acknowledging, that there is not any subject, whereon my expressions can more want eloquence, than on this subject; even eloquence itself would want expressions.

Nehem. ix. 5.

1 Cor. xiii. 1.

Psal. xxiii.

CERTAIN

PHYSIOLOGICAL ESSAYS,

AND OTHER

TRACTS;

Written at distant Times, and on several Occasions.

[In the second Edition, some of the Tracts were enlarged by Experiments, and the work increased by the addition of a Discourse about *ABSOLUTE REST* in BODIES.]

An Advertisement to the Reader: Prefixed to the first Edition, put forth, A. D. 1661.

THE publisher desires, that the reader should be informed, that neither were the following treatises written near about the same time (some of them being divers years older than some others) nor yet are they now published in the same order that they were written in. For the first of these discourses (though penned about four years since) was not only written after the second, third, and fourth, but after divers other essays, which the author has yet lying by him among his papers; it being intended for a kind of introduction to all those treatises, which under severel names, but chiefly that of *Physiological Essays*, the author had then composed. But having, during the late confusions, so disposed of his papers to secure them, that he could not himself seasonably recover them; and being engaged by promise to some friends, to let about half a dozen of his small tracts come abroad into the world by such a time, he was fain to send the following treatises to the press as they came, some at one time, some at another, to his hands. And this his occasions did now and then reduce him to do in such haste, that he could not attend the correcting either the printer's lapses or his own, and particularly was was obliged, partly by haste, and partly by a distemper in his eyes, to send away the *History of Firmness* without so much as reading it over. All which it is hoped the equitable reader will consider in his favour, and bear with what may be imputable to such circumstances.

I SHOULD add nothing further, were it not that to save the reader the trouble of guessing, who is meant by that *Pyrophilus*, to whom most of the following treatises are addressed, I think it requisite to inform him, that the person veiled under that name, is that hopeful young gentleman, Mr. *Richard Jones*, only son to the Lord Viscount *Ranelagh* and an excellent Lady, sister to the author.

A

PROËMIAL ESSAY,

WHEREIN, WITH SOME

Considerations touching EXPERIMENTAL ESSAYS in general,

Is interwoven such an Introduction to all those written by the Author, as is necessary to be perused for the better understanding of them.

I KNOW not, *Pyrophilus*, whether what you will meet in the ensuing discourses will prove worthy of your taking notice of; yet I dare tell you, that if all my endeavours to serve you were not duties, I should think I might deserve your thanks for venturing to write them for your sake. For I am sufficiently sensible, both how unlearned I am, and in how learned an age I presume to write: nor has the great number of those escaped my observation, who finding it a much easier task to censure experimental composures than to write such, endeavour to acquire the title of judicious, by condemning all things, that themselves have not written, or thought on. And indeed, *Pyrophilus*, I had, besides these, so many other discouraging considerations in my eye, whilst I was setting down the following essays, that I should scarce have prosecuted a design so full of trouble, and so unlikely by its success to make amends for it, if I had thought it free, for the securing of my own quiet and credit, to suppress observations which might prove serviceable to you, who having sufficiently conversed with books, are now desirous to begin to converse with things themselves. But I must confess, I look upon experimental truths as matters of so great concernment to mankind, that in spite of the just sense I have of my own disabilities, I am deterred from complying with those inclinations and motives that endear silence to me, by considering the fate of him, who though he had less entrusted to him than any of his companions, was yet severely punished for burying his single talent. And though, *Pyrophilus*, I could not without such reluctancy resolve to write, yet I found it much more uneasy to resolve to write so soon. For I could not but consider, that being yet but very young, not only in years, but, what is much worse, in experience, I have yet much more need to learn, than ability to teach. And I considered too, that after a man is grown somewhat acquainted with things themselves, and has taken some general notice of the cognations, differences, and tendencies of their properties, he may every day so much improve his knowledge, that I am apt to think, that if God should be pleased to protract my life a few years longer, I shall at the end of them be able to look upon what I have hitherto written with pity, if not with blushes. And I have often observed, that it is wont to happen in the productions of the mind, as in those of the body. For as those, that apply themselves to procreation too young,

and before they have attained to their full vigour and ſtrength, do generally both hinder their own growth, and become the parents but of weak and ſhort-lived children; ſo they, that too early, and before their judgment and experience be fully ripe, addict themſelves to write books, do commonly both hinder their own proficiency in knowledge, and write but immature, and therefore ſeldom laſting treatiſes. Nor ſhould I, *Pyrophilus*, have ever prevailed with myſelf to preſent you ſo early theſe diſcourſes, ſince, by keeping them longer by me, I might eaſily by ſecond thoughts, and freſh experiences, be enabled to correct and enrich them, did not the frequent and dangerous diſtempers, to which my very ſickly conſtitution has of late rendered me too obnoxious, make me juſtly doubt, whether or no, if I ſhould long forbear to write, death would not ſooner come than the expected maturity of age and judgment. And though I had no ſuch conſideration to move me to make haſte to tender to you the enſuing diſcourſes, yet this would ſuffice to engage me to preſent them you with all their preſent defects; that if I ſhould keep them till I can make them leſs unworthy of you, I muſt keep them till you are grown paſt the need of them.

AND now that I have told you, *Pyrophilus*, both why I have written the enſuing diſcourſes, and why I keep them not by me long enough to preſent them you with fewer imperfections; I ſuppoſe you will expect, that I ſhould next tell you, why I have caſt them into eſſays, rather than into any other form. To ſatisfy you about this particular, *Pyrophilus*, I muſt freely acknowledge to you, that it has long ſeemed to me none of the leaſt impediments of the real advancement of true natural philoſophy, that men have been ſo forward to write ſyſtems of it, and have thought themſelves obliged either to be altogether ſilent, or not to write leſs than an entire body of phyſiology: for, from hence ſeem to have enſued not a few inconveniences.

AND firſt, when men, by having diligently ſtudied either chymiſtry, anatomy, botanics, or ſome other particular part of phyſiology, or perhaps by having only read authors on thoſe ſubjects, have thought themſelves thereby qualified to publiſh compleat ſyſtems of natural philoſophy, they have found themſelves, by the nature of their undertaking, and the laws of method, engaged to write of ſeveral other things than thoſe, wherein they had made themſelves proficients; and thereby have been reduced, either idly to repeat what has been already, though perhaps but impertinently enough, written by others on the ſame ſubjects; or, elſe to ſay any thing on them rather than nothing, leſt they ſhould appear not to have ſaid ſomething to every part of the theme, which they had taken upon themſelves to write of.

IN the next place, the ſpecious and promiſing titles and comprehenſive method of theſe ſyſtems have been often found to perſuade unwary readers, that all the parts of natural philoſophy have been already ſufficiently explicated; and, that conſequently it were needleſs for them to put themſelves to trouble and charges in making further inquiries into nature, ſince others having already ſufficiently made it their buſineſs to inveſtigate and explicate phyſiological truths, our buſineſs needs now be no more than to learn what they have taught, and thankfully to acquieſce in it.

NOR has the ſyſtematical way of writing been prejudicial only to the proficiency of ſome readers, but alſo to the reputation of ſome writers of ſyſtematical books. For it not unfrequently happens, that when a writer, to vent ſome few peculiar notions or diſcoveries of his own, preſumes to write a whole body of philoſophy, what is truly his own, though excellent in its kind, is either loſt in the croud of the things he has borrowed from others, and ſo comes to be over-looked, or at leaſt not ſufficiently taken notice of, by the reader; or elſe the unwelcome, and yet in ſuch compoſures

ſcarce evitable, repetition of many things, that others had, I know not how often, written before, occaſions the laying aſide of the whole book, as a rhapſody of trite and vulgar notions, ſcarce worth the peruſing. And by this means the author often loſes the reputation of his peculiar notions, as well as the reader the benefit of them; and that, which would have made an excellent and ſubſtantial eſſay, paſſes but for a dull and empty book.

BUT the worſt inconvenience of all is yet to be mentioned; and that is, That whilſt this vanity of thinking men obliged to write either ſyſtems or nothing is in requeſt, many excellent notions or experiments are, by ſober and modeſt men, ſuppreſſed; becauſe ſuch perſons being forbidden by their judgment and integrity to teach more than they underſtand, or aſſert more than they can prove, are likewiſe forbidden by cuſtom to publiſh their thoughts and obſervations, unleſs they were numerous enough to ſwell into a ſyſtem. And indeed it may be doubted, whether the ſyſtematical writers have not kept the world from much more uſeful compoſures than they have preſented it with. For there are very few men, if any at all, in the world, that are enriched with a competent ſtock of experiments and and obſervations to make out clearly and ſolidly, I ſay not all the phænomena of nature, but all thoſe, that belong to chymiſtry, anatomy, or any ſuch conſiderable ſubordinate doctrine of phyſiology. And thoſe very men, that are diligent and judicious enough to ſtudy proſperouſly any of thoſe parts of phyſiology, are obliged to ſpend ſo much time in the accurate proſecution of that, and are wont to be thereby made ſo wary, and ſo thoroughly acquainted with the difficulty of phyſiological inveſtigations, that they will leaſt of all men be forward to write ſyſtems.

AND what I ſay, *Pyrophilus*, of the inconveniences, that have hitherto been obſerved to flow from men's forwardneſs to write entire bodies of philoſophy, may be, in its degree, applied to the practice of thoſe, that pretend to give us compleat accounts of chymiſtry, or almoſt (I ſay almoſt) any other conſiderable and comprehenſive part of natural philoſophy: though I deny not, that in ſuch attempts, which are much leſs difficult than the former, ſome men have done mankind conſiderable ſervice, though they have not fully performed what the titles of their writings ſeem to promiſe. Nor am I ſo rigid as to be unwilling, that, from time to time, ſome very knowing writer ſhould publiſh a ſyſtem of phyſiology, or any part of it, according to the beſt authors and obſervations of that time: for ſuch a work may be uſeful, partly, for the inſtructing of youth in ſchools and academies; and partly, that men may have, from time to time, an inventory of what hath been already diſcovered; whereby the needleſs labour of ſeeking after known things may be prevented, and the progreſs of mankind, as to knowledge, might the better appear. But then it is to be wiſhed, that ſuch writings were not publiſhed but by very intelligent perſons, nor till ſome conſiderable improvement have been made in knowledge ſince the laſt work of that nature. Nor would I be thought to diſallow ſuch writings of very learned men, as though they may bear very general titles, yet are not publiſhed by their authors as compleat bodies or ſyſtems of phyſiology, but rather as general principles (almoſt like the hypotheſes of aſtronomers, to aſſiſt men to explicate the already-known phænomena of nature. For of ſuch kind of writings, if their authors be (as for the moſt part they are) ſubtile and inquiſitive men, there may be very good uſe, not ſo much by their gratifying the intellect with the plauſible account of ſome of nature's myſteries; as becauſe on the one ſide their writers, to make good their new opinions, muſt either bring new experiments and obſervations, or elſe muſt conſider thoſe, that are known already, after a new manner, and thereby make us take notice

notice of ſomething in them unheeded before; and on the other ſide, the curioſity of readers, whether they like or diſapprove the hypotheſis propoſed, is wont to be thereby excited to make trial of ſeveral things, which ſeeming to be conſequences of this new doctrine, may, by their proving agreeable or repugnant to experiment, either eſtabliſh or overthrow it.

AND that you may know, *Pyrophilus*, what kind of writings I mean, I ſhall name to you the learned *Gaſſendus* his little *Syntagma* of *Epicurus*'s philoſophy, and that moſt ingenious gentleman Monſieur *Deſcartes* his principles of philoſophy. For though I purpoſely refrained, though not altogether from tranſiently conſulting about a few particulars, yet from ſeriouſly and orderly reading over thoſe excellent (though diſagreeing) books, or ſo much as Sir *Francis Bacon*'s *Novum Organum*, that I might not be prepoſſeſſed with any theory or principles, till I had ſpent ſome time in trying what things themſelves would incline me to think; yet beginning now to allow myſelf to read thoſe excellent books, I find by the little I have read in them already, that if I had read them before I began to write, I might have enriched the enſuing eſſays with divers truths, which they now want, and have explicated divers things much better than I fear I have done. But of ſuch writers the number is yet (and will I fear always be) ſo ſmall, that I ſhall not need to make many exceptions, when I treat of the uſefulneſs of writing books of eſſays, in compariſon of that of writing ſyſtematically: or, at leaſt, *Pyrophilus*, whilſt I preſume not to judge of other men's abilities, I hope it may be lawful for me to confeſs freely to you concerning myſelf, that I am very ſenſible of my being far from having ſuch a ſtock of experiments and obſervations, as I judge requiſite to write ſyſtematically; and I am apt to impute many of the deficiencies to be met with in the theories and reaſonings of ſuch great wits as *Ariſtotle*, *Campanella*, and ſome other celebrated philoſophers, chiefly to this very thing, that they have too haſtily, and either upon a few obſervations, or at leaſt without a competent number of experiments, preſumed to eſtabliſh principles, and deliver axioms. For it very rarely otherwiſe happens, than that theories, that are grounded but upon few and obvious experiments, are ſubject to be contradicted by ſome ſuch inſtances, as more free and diligent inquiries into what of nature is more abſtruſe, or even into the leſs obvious qualities of things, are wont to bring to light. I remember, that being once at *Leyden*, I was brought to the top of a tower, where, in a darkened room (ſuch as is now uſed in many places to bring in the ſpecies of external object) a convex glaſs, applied to the only hole, by which light was permitted to enter, did project upon a large white ſheet of paper, held at a juſt diſtance from it, a lively repreſentation of divers of the chief buildings in the town; all which, upon the admiſſion of more light into the room, by opening the window, did immediately diſappear. And methinks, *Pyrophilus*, that in divers of the philoſophical theories, that have been formerly applauded, ſomething not unlike this may be eaſily obſerved. for though, whilſt they are looked on with ſuch a weak and determinate degree of light, they may appear very artificial and well-proportioned fabrics, yet they appear ſo but in that twilight, as it were, which is requiſite to their conſpicuouſneſs. For if but a full light of new experiments and obſervations be freely let in upon them, the beauty of thoſe (delightful, but fantaſtical) ſtructures does immediately vaniſh.

AND truly, *Pyrophilus*, if men could be perſuaded to mind more the advancement of natural philoſophy than that of their own reputations, it were not, methinks, very uneaſy to make them ſenſible, that one of the conſiderableſt ſervices, that they could do mankind, were to ſet themſelves diligently and induſtriouſly to make experiments and collect obſervations, without being over-forward to eſtabliſh principles and axioms,

believing it uneasy to erect such theories, as are capable to explicate all the phænomena of nature, before they have been able to take notice of the tenth part of those phænomena, that are to be explicated. Not that I at all disallow the use of reasoning upon experiments, or the endeavouring to discern as early as we can the confederations, and differences, and tendencies of things: for such an absolute suspension of the exercise of reasoning were exceeding troublesome, if not impossible. And, as in that rule of arithmetic, which is commonly called *regula falsi*, by proceeding upon a conjecturally-supposed number, as if it were that, which we inquire after, we are wont to come to the knowledge of the true number sought for; so in physiology it is sometimes conducive to the discovery of truth, to permit the understanding to make an hypothesis, in order to the explication of this or that difficulty, that by examining how far the phænomena are, or are not, capable of being solved by that hypothesis, the understanding may, even by its own errors, be instructed. For it has been truly observed by a great philosopher, that truth does more easily emerge out of error than confusion. That then, that I wish for, as to systems, is this, that men, in the first place, would forbear to establish any theory, till they have consulted with (though not a fully competent number of experiments, such as may afford them all the phænomena to be explicated by that theory, yet) a considerable number of experiments, in proportion to the comprehensiveness of the theory to be erected on them. And, in the next place, I would have such kind of superstructures looked upon only as temporary ones; which though they may be preferred before any others, as being the least imperfect, or, if you please, the best in their kind that we yet have, yet are they not entirely to be acquiesced in, as absolutely perfect, or uncapable of improving alterations.

IT were very possible, *Pyrophilus*, to let you see, that all that has been said to recommend to you that form of writing, which (in imitation of the French) we call essays, is but a part of what may be pertinently said to the same purpose. But because this introductory discourse itself is to be but an essay, not a book, I dare not long insist upon the advantages of this sort of discourses. Only because I think, that if I could engage you, *Pyrophilus*, and such other ingenious persons, to cast their physiological observations and reflections into experimental essays, I should thereby do real learning no trifling service, by bringing so useful a way of writing into the request it deserves; upon this consideration, I say, I must beg leave to represent to you this great conveniency of essays, that as in them the reader needs not be clogged with tedious repetitions of what others have said already, so the writer, having for the most part the liberty to leave off when he pleases, is not obliged to take upon him to teach others what himself does not understand, nor to write of any thing but of what he thinks he can write well. And if such essays be but as they should be competently stocked with experiments, it is the reader's own fault, if he be not a learner by them: for indeed when a writer acquaints me only with his own thoughts or conjectures, without inriching his discourses with any real experiment or observation, if he be mistaken in his ratiocination, I am in some danger of erring with him, and at least am like to lose my time, without receiving any valuable compensation for that great loss: but if a writer endeavours, by delivering new and real observations or experiments, to credit his opinions, the case is much otherwise; for let his opinions be never so false, his experiments being true, I am not obliged to believe the former, and am left at liberty to benefit myself by the latter; and though we have erroneously superstructed upon his experiments, yet the foundation being solid, a more wary builder may be very much furthered by it in the erection of more judicious and consistent

ſiſtent fabrics. Such a writer, if I be not wanting to myſelf, will certainly teach me uſeful truths, and if it be not my fault, he can lead me into no errors; and oftentimes the very experiments, that he delivers, beſides that they may be applicable to many other purpoſes unthought of by him, may be either ſufficient, or at leaſt helpful to the very diſcovery of the erroneouſneſs of the opinions they are alleged to countenance: and I may account, that a man, that gives me, together with his conjectures (though erroneous) in matters of phyſiology, ſome noble experiment or obſervation, by which he pretends to verify them, does me no greater injury, than *Galileo* upon his firſt invention of the teleſcope would have done an aſtronomer, if he had told him, that he had diſcovered in heaven thoſe imaginary new ſtars, which a late mathematician has fancied himſelf to have deſcried there, and at the ſame time had made him a preſent of an excellent teleſcope, with expectation thereby the receiver ſhould be made of the giver's opinion; for by the help of his inſtrument the aſtronomer might not only make divers uſeful obſervations in the ſky, and perhaps detect new lights there, but diſcern alſo his miſtake, that gave it him.

AFTER what has been ſaid, *Pyrophilus*, of the uſefulneſs of experimental eſſays, we muſt proceed to ſay ſomething concerning the manner of writing them: but becauſe you will alſo expect to receive ſome account of the enſuing diſcourſes, I ſhall not treat of thoſe two ſubjects a-part, but, in diſcourſing of the following eſſays, ſhall take occaſion to acquaint you with part of my thoughts concerning ſuch kind of compoſures in general, the other conſiderations belonging to the ſame ſubject being already upon ſeveral occaſions diſperſed among, and to be met with in the enſuing diſcourſes themſelves.

AND firſt, as for the ſtyle of our experimental eſſays, I ſuppoſe you will readily find, that I have endeavoured to write rather in a philoſophical than a rhetorical ſtrain, as deſiring, that my expreſſions ſhould be rather clear and ſignificant, than curiouſly adorned: for to a ſubject of the ſerious and important nature of phyſiology that ſaying may unqueſtionably be applied, *Ornari res ipſa negat, contenta doceri.* And certainly in theſe diſcourſes, where our deſign is only to inform readers, not to delight or perſuade them, perſpicuity ought to be eſteemed at leaſt one of the beſt qualifications of a ſtyle; and to affect needleſs rhetorical ornaments in ſetting down an experiment, or explicating ſomething abſtruſe in nature, were little leſs improper, than it were (for him that deſigns not to look directly upon the ſun itſelf) to paint the eyeglaſſes of a teleſcope, whoſe clearneſs is their commendation, and in which even the moſt delightful colours cannot ſo much pleaſe the eye, as they would hinder the ſight. And that it may not be ſuſpected, that thoſe, that would not have it requiſite to imploy a florid ſtyle in treating of philoſophical ſubjects, do but in their own excuſe deny the neceſſity of ſuch rhetorical embelliſhments as they are not able to afford their compoſures, give me leave to ſubjoin, that it was not an unpoliſhed naturaliſt, but that prince of orators, *Cicero* himſelf, who made this ſtudious declaration; *Omne* (ſays he) *quod de re bona dilucidè dicitur, præclarè mihi dici videtur: iſtiuſmodi autem res velle ornatè dicere, pueri.e eſt: planè autem & perſpicuè expedire poſſe, docti & intelligentis viri.* But I muſt not ſuffer myſelf to ſlip unawares into the common place of the unfitneſs of too ſpruce a ſtyle for ſerious and weighty matters; and yet I approve not that dull and inſipid way of writing, which is practiſed by many chymiſts, even when they digreſs from phyſiological ſubjects. For though a philoſopher need not be ſollicitous, that his ſtyle ſhould delight its reader with his floridneſs, yet I think he may very well be allowed to take a care, that it diſguſt not his reader by its flatneſs, eſpecially when he does not ſo much deliver experiments or explicate them, as make reflections

flections or discourses on them: for on such occasions he may be allowed the liberty of recreating his reader and himself, and manifesting, that he declined the ornaments of language, not out of necessity, but discretion, which forbids them to be used, where they may darken as well as adorn the subject they are applied to. Thus (to resume our former comparison) though it were foolish to colour or enamel upon the glasses of telescopes, yet to gild or otherwise embellish the tubes of them, may render them more acceptable to the users, without at all lessening the clearness of the object to be looked at through them.

AND as for exotic words and terms borrowed from other languages, though I expect, that persons not conversant in the philosophical composures written (especially of late) in our language will be apt to suspect me for the willing author of divers new words and expressions; yet as for you, *Pyrophilus*, who peruse other than moral, theological, and historical books in English, and find how much use is made in them of exotic terms, I hope you will find, that I have not at all affected them, but have rather studiously declined the use of those, which custom has not rendered familiar, unless it be to avoid the frequent and unwelcome repetition of the same word (so troublesome to the ear, and so much forbidden by orators) or for some peculiar significancy of some such word, whose energy cannot be well expressed in our language, at least without a tedious circumlocution. And in such cases, *Pyrophilus*, I suppose a writer may be allowed to use exotic terms; especially when custom has not only denizened them, but brought them into request. For, as in the fashions of clothes, though perhaps fools begin them, yet wise men, when they are once generally received, scruple not to follow them, because then obstinately to decline them would be as ridiculously singular as at first it would have been to begin them: so in exotic words, when custom has once made them familiar and esteemed, scrupulously to decline the use of them may be as well a fault, as needlessly to imploy them: for it is not the use, but the affectation of them, that is unworthy a philosopher. And from the latter of those I hope I have kept myself far enough; for I should think myself guilty of a very childish vanity, if the use I made of languages were so to write as to be the less understood. But besides the unintentional deficiencies of my style, I have knowingly and purposely transgressed the laws of oratory in one particular, namely, in making sometimes my periods or parentheses over-long: for when I could not within the compass of a regular period comprise what I thought requisite to be delivered at once, I chose rather to neglect the precepts of rhetoricians, than the mention of those things, which I thought pertinent to my subject, and useful to you, my reader. And for this fault, *Pyrophilus*, since I have made myself guilty of it but for your sake, I think I ought to obtain your pardon at least as easily as my own, since barely to keep you from losing any thing, that I conceived might be serviceable to you, I knowingly expose my style to be censured as disproportionate to itself.

THE next thing, *Pyrophilus*, of which I am to give you an account, is, why I have in the ensuing essays delivered many experiments and observations, which may seem slight and easy, and some of them obvious also, or else perhaps mentioned by others already. To satisfy you about this, I must inform you, that many of the particulars, which we are now considering, were in my first design collected in order to a continuation of the Lord *Verulam's Sylva Sylvarum*, or natural history. And that my intended centuries might resemble his, to which they were to be annexed, it was requisite, that such kind of experiments and observations, as we have been newly speaking of, should make up a considerable part of them. And indeed it were to be wished, that such inquisitive persons, as cannot be at the charge, or have not the op-

portunity, of making new experiments, would busy themselves, as they have opportunity, in industriously collecting, and carefully setting down the phænomena to be met with without the assistance of new experiments, especially such particulars, as seem either to be of moment in order to the hinting or confirmation of some considerable truth, or to the detection of some applauded error, or else to have been, though obvious enough, yet little taken notice of. For I am confident, that very much may be done towards the improvement of philosophy by a due consideration of, and reflexion on, the obvious phænomena of nature, and those things, which are almost in every body's power to know, if he pleases but seriously to heed them; and I make account, that attention alone might quickly furnish us with one half of the history of nature, as well as industry is requisite, by new experiments, to enrich us with the other. And therefore I confess I think myself beholden to him, that first makes me take notice of what I might easily have known, but heeded not before: it not seldom happening, that we are prejudiced by, though we do not complain of that ignorance, from which we might relieve ourselves, if we did but diligently turn our eyes to the observations, wherewith even neighbouring and familiar objects would, if duly consulted, present us. But I digress; and therefore must step back into the way, and tell you, that the reasons, why I first designed the narrative of what I had tried and observed for a continuation of Sir *Francis Bacon*'s Natural History, you will meet with in my preface to that specimen of the intended continuation, which I have given to those of my essays, that treat of promiscuous experiments: and the reason, why I have since declined that succinct way of writing, is for the sake of *Pyrophilus*, that I might have, in a more free and uncircumscribed way of discoursing, a greater liberty to insist on and manifest the reasonableness of such animadversions, as I thought seasonable for a person, who, though a great proficient in the other parts of philosophy, is but a beginner in experimental learning. And the second reason, why I have often made use of seemingly slight experiments, is, because such are more easily and cheaply tried, and they being alleged for the most part to prove some assertion, or credit some admonition, I thought their easiness or obviousness fitter to recommend them, than depreciate them; and I judged it somewhat unkind, or at least indiscreet, to refer you most commonly for proof of what I delivered, to such tedious, such difficult, or such intricate processes, as either you can scarce well make, unless you be already, what I desire my experiments should help to make you, a skilful chymist; or else are as difficult to be well judged, as the truth they should discover is to be discerned. I was also hopeful, that the easiness of divers things inviting you to make trial of them, and keeping you from being disappointed in your expectations, the success of your first attempts would incourage you to make trial also of more nice and difficult experiments. And till you have tried them, do me the right to think, that I deal not unsincerely with you.

THE reasons of my having divers times recorded experiments, which you may have formerly met with, and perchance even in printed books, I have elsewhere deduced in a peculiar discourse on that subject; and therefore shall now only add, that by reason of my being as yet a stranger to the German tongue, wherein the most and best chymical books are said to be written, I may have set down divers chymical experiments and observations, that are extant already in that Hermetical language, (if I may so call it) without having had them from their Dutch publishers, or so much as dreamed of their having been divulged by any man. I have likewise in my preface to the essays, that you will meet with under the title of *Promiscuous Experiments*, given you an account, why I have not refrained from mentioning divers things, which may seem

ſeem very ſlight, becauſe very obvious: and I have long had thoughts to inform you in an intire diſcourſe to be written on purpoſe, why I think, that even the trivial, and therefore ſlighted, truths of phyſiology ought not to be deſpiſed. And for my own part, I ſhall not ſcruple to confeſs to you, that I diſdain not to take notice even of ludicrous experiments, and think, that the plays of boys may ſometimes deſerve to be the ſtudy of philoſophers: for as when we go a hunting, though the flight of the hare and the purſuit of the dogs be to us but ſport and recreation, yet the beaſts themſelves are extreamly earneſt, the one to ſave his threatened life by flight, and the other to overtake his deſired prey; ſo nature acts very ſeriouſly in all the other things, that we make ſports with, and is in very good earneſt, whether we men be ſo or no.

PERHAPS you will wonder, *Pyrophilus*, that in almoſt every one of the following eſſays I ſhould ſpeak ſo doubtingly, and uſe ſo often, *perhaps*, *it ſeems*, *it is not improbable*, and ſuch other expreſſions, as argue a diffidence of the truth of the opinions I incline to, and that I ſhould be ſo ſhy of laying down principles, and ſometimes of ſo much as venturing at explications. But I muſt freely confeſs to you, *Pyrophilus*, that having met with many things, of which I could give myſelf no one probable cauſe, and ſome things, of which ſeveral cauſes may be aſſigned ſo differing, as not to agree in any thing, unleſs in their being all of them probable enough; I have often found ſuch difficulties in ſearching into the cauſe and manner of things, and I am ſo ſenſible of my own diſability to ſurmount thoſe difficulties, that I dare ſpeak confidently and poſitively of very few things, except of matters of fact. And when I venture to deliver any thing, by way of opinion, I ſhould, if it were not for mere ſhame, ſpeak yet more diffidently than I have been wont to do. It is not, that I at all condemn the practice of thoſe inquiſitive wits, that take upon them to explicate to us even the abſtruſeſt phænomena of nature; for I am ſo far from cenſuring them, that I admire them, when their endeavours ſucceed, and applaud them even where they do but fairly attempt. But I think it is fit for a man to know his own abilities and weakneſſes, and not to think himſelf obliged to imitate all that he thinks fit to praiſe. I know alſo, that the way to get reputation is to venture to explicate things, and promote opinions; for by that courſe a writer ſhall be ſure to be applauded by one ſort of men, and be mentioned by many others; whereas by the way of writing, to which I have condemned myſelf, I can hope for little better among the more daring and leſs conſiderate ſort of men, ſhould you ſhew them theſe papers, than to paſs for a drudge of greater induſtry than reaſon, and fit for little more, than to collect experiments for more rational and philoſophical heads to explicate and make uſe of. But I am content, provided experimental learning be really promoted, to contribute even in the leaſt plauſible way to the advancement of it; and had rather not only be an under-builder, but even dig in the quarries for materials towards ſo uſeful a ſtructure, as a ſolid body of natural philoſophy, than not do ſomething towards the erection of it. Nor have my thoughts been altogether idle and wanting to themſelves, in framing notions, and attempting to deviſe hypotheſes, which might avoid the deficiencies obſerved in other men's theories and explications: but I have hitherto, though not always, yet not unfrequently, found, that what pleaſed me for a while, as fairly comporting with the obſervations, on which ſuch notions were grounded, was ſoon after diſgraced by ſome further or new experiment, which at the time of the framing of thoſe notions was unknown to me, or not conſulted with. And indeed I have the leſs envied many (for I ſay not all) of thoſe writers, who have taken upon them to deliver the cauſes of things, and explicate the myſteries of nature, ſince I

have had opportunity to obſerve, how many of their doctrines, after having been for a while applauded, and even admired, have afterwards been confuted by the diſcovery of ſome new phænomenon in nature, which was either unknown to ſuch writers, or not ſufficiently conſidered by them. For I have found it happen, as well to many others (that have publiſhed their opinions) as to me (who have been more private in my gueſſes) in our theories built on either too obvious or too few experiments, what is wont to happen to the falſifiers of coin; for as counterfeit pieces of money will endure ſome of them one proof, as the touch-ſtone, others another, as aqua fortis, ſome a third, as the hammer or the ſcales, but none of them will endure all proofs; ſo the notions I mention (in which ſort I fear too great a part of thoſe hitherto extant may be comprized) may agree very fairly with this or that or the other experiment; but being made too haſtily, and without conſulting a competent number of them, it is to be feared, that there may ſtill after a while be found one or other, (if not many) their inconſtancy with which will betray and diſcredit them.

I HAVE notwithſtanding all this, on ſome occaſions, adventured to deliver my opinion; not that I am very confident of being leſs ſubject to err in thoſe particulars, than in any of the others, wherein I have refrained from interpoſing any conjecture; but becauſe I would manifeſt to you, that I ſcruple not to run the ſame venture with thoſe incomparably better naturaliſts, that have thought it no diſgrace in difficult matters rather to hazard the being ſometimes miſtaken, than not to afford inquiſitive perſons their beſt aſſiſtance towards the diſcovery of truth.

AND becauſe, *Pyrophilus*, in the reaſons and explications I offer of natural effects, I have not for the moſt part an immediate recourſe to the magnitude, figure, and motion of atoms, or of the leaſt particles of bodies, I hold it not unfit to give you here ſome account of this practice; not ſo much for the ſake of thoſe few paſſages in my eſſays, that may be concerned in it, as for that of many learned men, eſpecially phyſicians, whoſe uſeful writings being to be undervalued, and are in danger to be deſpiſed, by an opinion taken up from the miſ-underſtood doctrine of ſome eminent Atomiſts, as if no ſpeculations in natural philoſophy could be rational, wherein any other cauſes of things are aſſigned than atoms and their properties. I conſider then, that generally ſpeaking, to render a reaſon of an effect or phænomenon, is to deduce it from ſomething elſe in nature more known than itſelf; and that conſequently there may be divers kinds of degrees of explication of the ſame thing. For although ſuch explications be the moſt ſatisfactory to the underſtanding, wherein it is ſhewn, how the effect is produced by their more primitive and catholick affections of matter, namely, bulk, ſhape and motion; yet are not theſe explications to be deſpiſed, wherein particular effects are deduced from the more obvious and familiar qualities or ſtates of bodies, ſuch as heat, cold, weight, fluidity, hardneſs, fermentation, *&c.* though theſe themſelves do probably depend upon thoſe three univerſal ones formerly named. For in the ſearch after natural cauſes, every new meaſure of diſcovery does both inſtruct and gratify the underſtanding; though I readily confeſs, that the nearer the diſcovered cauſes are to thoſe, that are higheſt in the ſcale or ſeries of cauſes, the more is the intellect both gratified and inſtructed.

I THINK it therefore very fit and highly uſeful, that ſome ſpeculative wits, well verſed in mathematical principles and mechanical contrivances, ſhould employ themſelves in deducing the chieſeſt modes or qualities of matter, ſuch as are heat, cold, *&c.* and the ſtates or conditions of it (if we think fit to diſtinguiſh theſe from its qualities) as fluid, firm, brittle, flexible, and the like, from the above-mentioned moſt primitive and ſimple affections thereof. And, I think, the commonwealth of learning

ing exceedingly beholden to those heroick wits, that do so much as plausibly perform something in this kind. But I think too, we are not to despise all those accounts of particular effects, which are not immediately deduced from those primitive affections of either atoms, or the insensible particles of matter; but from the familiar, though not so universal, qualities of things, as cold, heat, weight, hardness, and the like. And perhaps it would be none of the least advantages, which would accrue to Naturalists from a satisfactory explication of such qualities by the most primitive and simple ones, that it would much shorten the explication of particular phænomena. For though there be many things in nature, that may be readily enough made out by the size, motion, and figure of the small parts of matter; yet there are many more, that cannot be well explained without a great deal of discourse, and divers successive deductions of one thing from another, if the purposed effect must be deduced from such primary and universal causes; whereas if we be allowed to take the notions of cold, heat, and the like qualities for granted, the explications and proofs may be much more compendiously made. He gives some reason, why stones and iron, and all other heavy bodies, will swim in quicksilver, except gold, which will sink in it; that teaches, that all those other bodies are *in specie* (as they speak) or bulk for bulk, lighter than quicksilver, whereas gold is heavier. He, I say, may be allowed to have rendered a reason of a thing proposed, that thus refers the phænomenon to that known affection of almost all bodies here below, which we call gravity, though he does not deduce the phænomenon from atoms, nor give us the cause of gravity; as indeed scarce any philosopher has yet given us a satisfactory account of it. So if it be demanded, why, if the sides of a blown bladder be somewhat squeezed betwixt one's hands, they will, upon the removal of that which compressed them, fly out again, and restore the blader to its former figure and dimensions; it is not saying nothing to the purpose, to say, that this happens from the spring of those aërial particles, wherewith the bladder is filled, though he, that says this, be not perhaps able to declare, whence proceeds the motion of restitution, either in a particle of compressed air, or any other bent spring.

And as for the reasons of things assigned by physicians, they must be by most of them despised, unless we will allow of such explications, as deduce not things from atoms or their affections, but only either from secondary qualities, or from the more particular properties of mixt bodies. If a physician be asked, why rhubarb does commonly cure loosenesses, he will probably tell you as a reason, that rhubarb is available in such diseases, because it hath both a laxative virtue, whereby it evacuates choler, and such other bad humours as are wont, in such cases, to be the peccant matter; and an astringent quality, whereby it afterwards arrests the flux. But if you further ask him the reason, why rhubarb purges, and why it purges choler more than any other humour; it is ten to one he will not be able to give you a satisfactory answer. And indeed, not only the manner, whereby purgative medicines work, but those other properties, whereby some bodies are diuretick, others sodorifick, others sarcotick, *&c.* are not, I fear, so easy to be intelligibly made out as men imagine, and yet a skilful physician would justly think himself wronged, if the reasons he renders of things in his own profession were denied the name of reasons, because made without recourse to atomical principles. And indeed, there are oftentimes so many subordinate causes between particular effects and the most general causes of things, that there is left a large field, wherein to exercise men's industry and reason, if they will but solidly enough deduce the properties of things from more general and familiar qualities, and also intermediate causes (if I may so call them) from

from one another. And I am the more backward to despise such kind of reasons, because I elsewhere declare, that there are some (for I do not say, many) things, as particularly the origin of local motion, of which, even by the atomical doctrine, no physical cause can well be rendered; since either such things must be ascribed to God, who is indeed the true, but supernatural cause of them; or else it must be said (as it was by the old Epicurians) that they did ever belong to matter: which, considering that the notion of matter may be compleat without them, is not to give a physical efficient cause of things in question, but in effect to confess that they have no such causes. But of this elsewhere more.

In the mean time, that you may not be drawn away to undervalue such writers as I have been pleading for, nor think you ought to refrain from writing what occurs to you, though true and useful, unless you deduce it, or at least can do so, from the Epicurian notions; I shall here briefly represent to you (what perhaps you will not hereafter think a despicable suggestion) that there are two very distinct ends, that men may propound to themselves in studying natural philosophy. For some men care only to know nature, others desire to command her; or, to express it otherwise, some there are, who desire but to please themselves by the discovery of the causes of the known phænomena; and others would be able to produce new ones, and bring nature to be serviceable to their particular ends, whether of health, or riches, or sensual delight. Now as I shall not deny, but that the atomical, the Cartesian, or some such principles, are likely to afford the most of satisfaction to those speculative wits, that aim but at the knowledge of causes; so I think, that the other sort of men may very delightfully and successfully prosecute their ends, by collecting and making variety of experiments and observations; since thereby learning the qualities and properties of those particular bodies they desire to make use of, and observing the power that divers chymical operations, and other ways of handling matter, have of altering such bodies, and varying their effects upon one another, they may, by the help of attention and industry, be able to do many things, some of them very strange, and more of them very useful in human life. When a gunner or a soldier employs gunpowder, it is not necessary, that he should consider, or so much as know, of what, and of how many ingredients (much less of what kind of atoms) it is made, and the proportion and manner, wherein they are mingled; but the notice experience gives him of the power of that admirable concrete, as it is made up and brought to his hands, suffices to enable him to perform things with it, that nothing but their being common and unheeded can keep from being admired. The physician, that has observed the medicinal virtues of treacle, without knowing so much as the names, much less the nature of each of the sixty and odd ingredients, whereof it is compounded, may cure many patients with it. And though it must not be denied, that it is an advantage as well as a satisfaction, to know in general, how the qualities of things are deducible from the primitive affections of the smallest parts of matter; yet whether we know that or no, if we know the qualities of this or that body they compose, and how it is disposed to work upon other bodies, or be brought on by them, we may, without ascending to the top in the series of causes, perform things of great moment; and such, as without the diligent examination of particular bodies, would, I fear, never have been found out *à priori*, even by the most profound contemplators. We see that the artificers, that never dreamed of the Epicurian philosophy, have accommodated mankind with a multitude of useful inventions. And *Paracelsus*, who (besides that he seems none of the most piercing and speculative wits) sure had little recourse to atomical notions, if he ever so much as heard of them, was

able to perform some things, that were truly admirable, besides those he vainly boasted of; as may appear not only by what I elsewhere represent, but by what *Oporinus* himself (as severely as he otherwise writes against his deserted master) confesses he saw of the stupendous cures, which *Paracelsus* wrought with his famous *Laudanum* (whatever he made it of). But we need not go far to find a noble example to our present purpose, since we see, that the bare making of trials with the loadstone, and irons touched by it, though the experimenters were ignorant (as some fear we yet are) of the true and first causes of magnetical phænomena, have produced inventions of greater use to mankind, than were ever made by *Leucippus*, or *Epicurus*, or *Aristotle*, or *Telesius*, or *Campanella*, or perhaps any of the speculative devisers of new hypotheses; whose contemplations aiming for the most part but at the solving, not the increasing or applying, of the phænomena of nature, it is no wonder they have been more ingenious than fruitful, and have hitherto more delighted than otherwise benefited mankind: I say hitherto, because though experience warrants me so to speak now, yet I am not unwilling to think, that hereafter, and perhaps in no long time, when physiological theories shall be better established, and built upon a more competent number of particulars, the deductions, that may be made from them, may free them from all imputation of barrenness. But of these things I otherwhere (though not, as I remember, in any of the following essays) more fully discourse.

AND therefore I shall now resume the subject, that occasioned this long excursion, and add to what I said in excuse of my venturing sometimes to deliver something as my opinion in difficult or controverted cases, that I must declare to you, *Pyrophilus*, that as I desire not my opinions should have more weight with you, than the proofs brought to countenance them will give them; so you must not expect, that I should think myself obliged to adhere to them any longer, than those considerations, that first made me embrace them, shall seem of greater moment, than any that I can meet with in opposition to them. For *Aristotle* spoke like a philosopher, when to justify his dissent from his master *Plato*, he said, among other things, that for the sake of truth, men (especially being philosophers) ought to overthrow even their own tenets (Δόξει δ' ἂν ἴσως βέλτιον εἶναι, καὶ δεῖν ἐπὶ σωτηρίᾳ γε τῆς ἀληθείας, καὶ τὰ οἰκεῖα ἀναιρεῖν, ἄλλως τε καὶ φιλοσόφους ὄντας.) And though for a man to change his opinions, without seeing more reason to forsake them than he had to assent to them, be a censurable levity and inconstancy of mind; yet to adhere to whatever he once took for truth, though by accession of more light he discover it to be erroneous, is but a proud obstinacy, very injurious to truth, and very ill becoming the sense we ought to have of human frailties. And it ought to be esteemed much less disgraceful to quit an error for a truth, than to be guilty of the vanity and perverseness of believing a thing still, because we once believed it. And certainly, till a man is sure he is infallible, it is not fit for him to be unalterable. *Ethic. Nicom.* l. 1. cap. 6.

YOU will easily discern, *Pyrophilus*, that I have purposely, in the ensuing essays, refrained from swelling my discourses with solemn and elaborate confutations of other men's opinions, unless it be in some very few cases, where I judged, that they might prove great impediments to the advancement of experimental learning; and even such opinions I have been wary of meddling with, unless I supposed I could bring experimental objections against them. For it is none of my design to engage myself with, or against, any one sect of Naturalists, but barely to invite you to embrace or refuse opinions, as they are consonant to experiments, or clear reasons deduced thence, or at least analogous thereunto; without thinking it yet seasonable to contend very earnestly for those other opinions, which seem not yet determinable by such experiments

ments or reasons. And indeed, to allude to our former comparison, I would endeavour to destroy those curious but groundless structures, that men have built up of opinions alone, by the same way (and with as little noise) by which such fantastical structures, as those I mentioned to have seen at *Leyden*, may be demolished. To destroy which, it were needless to bring battering engines, since nothing is requisite to that effect but an increase of light. And experience has shown us, that divers very plausible and radicated opinions, such as that of the unhabitableness of the torrid zone, of the solidity of the celestial part of the world, of the blood's being conveyed from the heart by the veins (not the arteries) to the outward parts of the body, are generally grown out of request, upon the appearing of those new discoveries, with which they are inconsistent, and would have been abandoned by the generality of judicious persons, though no man had made it his business, purposely, to write confutations of them: so true is that vulgar saying, that *Rectum est index sui & obliqui.*

BUT when at any time, *Pyrophilus*, I have been induced to oppose others, as I have not denied myself the freedom, that is requisite unto loyalty to truth; so I have endeavoured to use that moderation, and civility, that is due to the persons of deserving men. And therefore you shall find me not only in one essay oppose an author, whom in another I applaud; but sometimes you may find me commending a writer in the very same page, perhaps, where I am endeavouring to disprove his opinions: for I love to speak of persons with civility, though of things with freedom. Nor do I think it reasonable, either that any man's reputation should protect his errors, or that the truth should fare the worse for his sake, that delivers it. And as for the (very much too common) practice of many, who write, as if they thought railing at a man's person, or wrangling about his words, necessary to the confutation of his opinions; besides that I think such a quarrelsome and injurious way of writing does very much misbecome both a philosopher and a Christian, methinks it is as unwise, as it is provoking. For if I civilly endeavour to reason a man out of his opinions, I make myself but one work to do, namely, to convince his understanding; but, if in a bitter or exasperating way I oppose his errors, I increase the difficulties I would surmount, and have as well his affections against me as his judgment: and it is very uneasy to make a proselyte of him, that is not only a dissenter from us, but an enemy to us. And that which makes me the more dislike the bitter way of disputing, which I am reprehending, is, that I have often observed, that though one of the disputants alone be at first in the fault, yet the other is most commonly drawn to share in the guilt, though, to contract it, he must imitate his adversary. For as a mad dog, by biting others, is wont to make those he bites run mad like himself, and do promiscuous mischief; so these so provoking writers are wont to enrage those they offend, and infect them also with their own virulent distemper. But, *Pyrophilus*, when I speak of dealing respectfully with those I dissent from, I would be understood of such, as have well deserved of experimental learning, or at least been candid and sober inquirers after truth. For, as I think, that it would much discourage any prudent person from venturing to communicate what he thinks he knows to the world, to find, that an error proceeding from human weakness, or the darkness and abstrusity of things, seldom escapes being detected without being made matter of disgrace or reproach to the author: so, on the other side, when vain writers, to get themselves a name, have presumed to intrude upon the credulous world such things, under the notion of experimental truths, or even great mysteries, as neither themselves ever took the pains to make trial of, nor received from any credible persons that professed themselves to have tried them; in such cases, I see not how we are obliged to treat writers, that took no pains

pains to keep themſelves from miſtaking or deceiving, nay, that cared not how they abuſe us to win themſelves a name, with the ſame reſpect, that we owe to thoſe, who, though they have miſſed of the truth, believed they had found it, and both intended to deliver it, and took ſome (though not proſperous) pains, that they might convey nothing elſe to us.

I FEAR it will be requiſite, *Pyrophilus*, to tell you, why in ſome of the following eſſays you will meet with many paſſages tranſcribed out of other authors, and in ſome very few or none at all. And therefore to give you firſt a ſhort account of the particular mentioned laſt, I muſt mind you, that it was moſt ſuitable both to my humour and deſign, to deliver only thoſe things, wherewith my own obſervations, or trials, or thoughts, had furniſhed me, without troubling you with the repetition of thoſe things, which had been delivered by others already; thoſe kind of repetitions, unleſs they be made upon ſome ſuch grounds as we ſhall preſently mention, ſeeming to me to be as vainly as ambitiouſly affected by many writers, and being deſervedly troubleſome to judicious readers, who can eaſily diſcern, that they add much more to the bulk of books than of knowledge. But this notwithſtanding, *Pyrophilus*, I thought myſelf obliged on ſome occaſions, for your ſake, to overcome my natural averſeneſs to ſtuff any writings of mine with paſſages tranſcribed from thoſe of others, partly for the reaſons elſewhere inſiſted on, and partly for divers others. As firſt, becauſe ſome particulars are of that ſtrangeneſs, and of that moment, that they need and deſerve to be verified by more than a ſingle atteſtation. Next, becauſe according to the Greek proverb, κοινὰ καινῶς, it is not properly to ſay over the ſame thing again, when the obſervation, experiment, or other paſſage of an author, is either illuſtrated, or otherwiſe improved by the repetition, or elſe applied to ſome purpoſe differing from that, to which the author brought it: that being applicable to many a ſingle experiment or obſervation, which *Seneca* ſomewhere ſays, *Nulla res conſummata eſt dum incipit*; and, *Etiamſi omnia à veteribus inventa ſunt, hoc ſemper novum erit, uſus, & inventorum ab aliis ſcientia & diſpoſitio.* And thirdly, becauſe as the planets and other ſtars have (according to aſtrologers) in their great ſynods or conjunctions, much more powerful, and ſometimes other influences on the air and ſome other ſublunary parts of the world, than are aſcribed to one or two of them out of that aſpect; ſo divers particulars, which whilſt they lay ſingle and ſcattered among the writings of ſeveral authors were inconſiderable, when they come to be laid together in order to the ſame deſign, may oftentimes prove highly uſeful to phyſiology in their conjunction, wherein one of them may ſerve to prove one part or circumſtance of an important truth, and another to explicate another, and ſo all of them may conſpire together to verify that ſaying, *Et quæ non proſunt ſingula, multa juvant.* It may then, I hope, ſuffice to juſtify me on this occaſion, that not appealing to other writers as to judges, but as to witneſſes, nor employing what I have found already publiſhed by them barely as ornaments to imbelliſh my writings, and much leſs as oracles by their authority to demonſtrate my opinions, but as certificates to atteſt matters of fact, I may hope, that their teſtimonies will as well be illuſtrated by mine, as mine by their's, and that all of them may contribute to your better information.

Quæſtion. Natural. lib. 6. cap. 5.
Epiſtola 64.

AND if, *Pyrophilus*, you grant, that upon theſe conſiderations I have not done amiſs to apply to my purpoſe divers of thoſe things, which I found delivered pertinently to them by thoſe writers, which I chanced to caſt mine eyes on, I ſuppoſe you will not think I need to make you an apology, for my having made moſt uſe of the paſſages of thoſe writers, which I ſuppoſe will be moſt difficult to be met with (ſuch as are many books of navigation and other voyages) and eſpecially of French books not

yet tranſlated into Engliſh or Latin. And I think I ſhall leſs need to make an excuſe for my having for the moſt part ſet down the paſſages I recited in the author's own words, that being one of the readieſt ways I know to ſatisfy the reader, and avoid injuring the writer. And indeed, I have met with abundance of quotations, wherein the tranſcriber doth ſo miſtake, or ſo miſrepreſent the cited author's meaning, ſometimes out of inadvertence, but ſometimes too I fear out of indulgence to his own hypotheſis, that if ever I ſhould be tempted to trouble the world with any of my thoughts, I would beſeech my readers, not to look upon any thing as my opinion or aſſertion, that is not delivered in the intire ſeries of my own words; leſt a tranſcriber ſhould make me deliver thoſe things reſolutely and dogmatically, which I deliver but heſitantly and conjecturally; and leſt I ſhould ſeem to ſet down thoſe things poſitively as proceſſes, for whoſe ſucceſs I undertake, which I record but by way of narrative.

FOR my ſo frequently mentioning what I have borrowed from other writers, or received from my friends, I expect to be excuſed by that of *Pliny*, *Benignum eſt (ut arbitror) & plenum ingenui pudoris, confiteri per quos profeceris.* Though I have ſeen divers modern writers, that ſo boldly uſurp the obſervations and experiments of others, that I might juſtly apply to them what the ſame *Pliny* annexes; *Scito enim, conferentem authores me deprehendiſſe à juratiſſimis & proximis veteres tranſcriptos ad verbum neque nominatos, &c.* If other writers ſhould not prove more equitable (for I will not ſay more thankful) than ſuch as theſe, they would quickly diſcourage thoſe, whoſe aims are not very noble and ſincere, from gratifying the public with inventions, whoſe praiſe and thanks would be uſurped by ſuch as will not name them. But perhaps they would be more juſt, if they reflected on what our author adds, *Obnoxii profectò animi & infelicis ingenii eſt, deprehendi in furto malle, quam mutuum reddere, cum præſertim ſors fiet ex uſura.*

AND now I have ſaid this concerning the paſſages I have borrowed from other authors, it will not be improper to add ſomething about thoſe I have declined to borrow. For you may poſſibly marvel, that in divers of the hiſtorical parts of my writings I have omitted ſuch teſtimonies either of *Pliny*, *Solinus*, *Ariſtotle*, *Theophraſtus*, *Ælian*, or, perchance, ſome of the antient phyſicians themſelves (who yet, as more converſant with things, are uſually more credible) as ſeem very pertinent to my diſcourſe, and fit to prove what I deſign. But when I ſhall come to entertain you about natural hiſtory, I doubt not but to ſatisfy you with the reaſons I ſhall offer you of this practice. In the mean time, I ſhall only tell you in ſhort, that though I have a juſt reſpect for thoſe great names I have mentioned; yet the ſenſe I have of the difficulties I have found to make and relate an obſervation accurately and faithfully enough for a naturaliſt to rely on; and the occaſions I have had of looking into divers matters of fact delivered in their writings, with a bold and impartial curioſity; have made me conclude ſo many of thoſe traditions to be either certainly falſe, or not certainly true, that except what they deliver upon their own particular knowledge, or with peculiar circumſtances, that may recommend them to my belief, I am very ſhy of building any thing of moment upon foundations, that I eſteem ſo unſure, and much leſs upon the ſuſpected paſſages, that *Wecker*, *Paracelſus*, *Porta*, &c. abound with. And therefore (though I well enough know, how much I impoveriſh my diſcourſe by this niceneſs) yet I do not think it fair to imploy that as an argument to convince you, that has not that operation upon me myſelf. And I rather take notice of my forbearing to make uſe of the hiſtorical traditions and chymical or magical ſecrets, that I meet in the above-mentioned authors, or any other makers of collections, unleſs the narrative

narrative be (as I was ſaying) expreſsly enough delivered upon the writer's perſonal knowledge, or that of ſome other credible witneſs; not only becauſe I would give you an account, why ſeveral of my writings are unfurniſhed with what moſt readers look on as the richeſt ornaments of other men's; but becauſe if this warineſs could be introduced, it would be the moſt effectual way of perſuading men to write thoſe kind of tracts I would recommend, phyſiological eſſays. For he, that will confine himſelf ſo ſtrictly, will ſcarce be often tempted, on phyſical ſubjects, to write either ſyſtems or volumes.

ANOTHER thing, *Pyrophilus*, I muſt needs advertiſe you of in reference to the enſuing diſcourſes; which is, that beſides thoſe deficiencies in point of ratiocination, which are due to my perſonal diſabilities, I have purpoſely let paſs ſome few (and but very few) inferences, which I diſcerned well enough not to be cogent, becauſe I was willing to acquaint you upon ſome particular occaſions with all the experiments then occurring to me, which I thought might contribute to the illuſtration of the ſubject in hand; though each of them apart did not irrefragably, nor indeed ſo much as ſtrongly infer the concluſion, in order to which they ſeemed to have been mentioned as premiſſes. And this practice I made the leſs ſcruple of, becauſe I was willing to exerciſe thereby your reaſoning faculty, and try how far you would diſcern the tendency of ſeveral things, all of them pertinent enough to the ſubject in hand, but not all of them concluding to the main deſign, in order whereunto they were alleged. And I ſuppoſed, that the things by me mentioned, though not concluſive, being yet experimental, the mention of them, which in a ſtrictly logical way of reaſoning muſt have been forborn, might well make you amends for the exerciſe, to which I intended they ſhould put your reaſon.

THERE remains yet one thing, *Pyrophilus*, of which I ſuppoſe you will expect I ſhould give you an account; and that is, why in the enſuing eſſays I have mentioned divers experiments, which I have not plainly and circumſtantially enough delivered. To ſatisfy you concerning which, I muſt repreſent to you, firſt, that though for your ſake I have oftentimes, contrary to my reaſon and genius, delivered things, to make them more clear, in ſuch a multitude of words, that I now ſeem even to myſelf to have in divers places been guilty of verboſity; yet in ſome other paſſages, treating of things, which uſe had rendered very familiar to me, I may have, to ſhun prolixity, unawares ſlipt into the contrary extreme. Secondly, there are ſome mechanical experiments, wherein I have purpoſely omitted ſome manual circumſtances, becauſe I was unwilling to prejudice ſome ingenious tradeſmen, who make either a livelihood, or at leaſt a gain, by the ſale of the productions of ſuch experiments. And I made the leſs ſcruple to conceal ſuch mechanical circumſtances (if I may ſo call them) becauſe they were not neceſſary to the phiſiological knowledge of the experiments; in naming of which, my intention was to teach you rather philoſophy than trades. Thirdly, I mention ſome things but darkly, either becauſe I received them upon condition of ſecrecy, or becauſe ſome ingenious perſons, that communicated them to me, or others to whom I imparted them, do yet make, and need to make, a pecuniary advantage of them. Fourthly, and ſome things, that, either having been the fruits of my own labours, or obtained in exchange of ſuch, are freely at my own diſpoſal, I have not yet thought fit ſo plainly to reveal, not out of any envious deſign of having them buried with me, but that I may be always provided with ſome rarity to barter with thoſe ſecretiſts, that will not part with one ſecret but in exchange for another, and think nothing worth their deſiring, that is known already to above one or two perſons. And I think it very lawful to reſerve always ſome concealed experiments

riments by me, wherewith to obtain the ſecrets of others, which being thereby gained, the other (as being no longer neceſſary to the former end) may freely be communicated.

AND think not, *Pyrophilus*, that the bare mention of an experiment as having been performed, though the way of making it be concealed, is of no uſe, if the relator of the experiment be a perſon, that may ſafely be credited: for it is ſomething, to be aſſured, that ſuch and ſuch things have been really performed, and conſequently are poſſible to be done, though we be not particularly acquainted with the means of performing them. And he tells you ſomething, that tells you upon his own knowledge, that in ſuch or ſuch bodies, or ways of operating on them, conſiderable things of ſuch or ſuch a nature are to be met with. And for my part, when I go a hawking or ſetting, I think myſelf beholden to him, that aſſures me, that in ſuch a field there is a covey of partridges, though he does no more towards the giving me them. And it is obvious, how much *Europe* is beholden to *Columbus* for the detection of many countries in *America*, which were not diſcovered by him, nor perhaps till long after his death, becauſe he firſt informed us knowingly, that there were unknown regions beyond that vaſt ocean, which ſevers the old world from the new. But I begin to digreſs, and therefore ſhall proceed to tell you, that I am the leſs troubled at my omiſſion of the circumſtantial parts of ſome experiments, becauſe I think it will be much for your advantage to try them over again yourſelf. And as I have taken care by the truth of the experiments I have delivered, to ſecure your ſucceſs, in caſe you try them aright; ſo I cannot be very ſorry, that you ſhould in ſome particulars have a kind of neceſſity laid on you to exerciſe your own induſtry, and thereby increaſe your experience.

BUT beſides all that has been ſaid, *Pyrophilus*, I muſt freely confeſs to you, that there is one thing particularly relating to yourſelf, which has made me refrain from delivering, in the enſuing eſſays, ſome of the chief chymical proceſſes, wherewith they might have been enriched. For not yet knowing with what ſerioufneſs you will addict yourſelf to promote experimental philoſophy, nor what uſe you will make of what has been unveiledly communicated to you, I was ſomewhat unwilling, that ſome things, which had coſt me a great deal of pains, ſhould yet fall into any man's hands, that ſcorns to purchaſe knowledge with ſome pains; and I was deſirous, in caſe you ſhall prove as induſtrious as I hope you will, to have ſomething by me to encourage and cheriſh your induſtry, which may be more ſuitable to your improved knowledge. For I muſt confeſs to you, that in reference to the chymical proceſſes extant in the following diſcourſes, I look upon moſt of them but as trifles, not only in compariſon of thoſe things, which a knowing chymiſt might have delivered on the ſame ſubjects, but even in regard of divers proceſſes (not impertinent to thoſe diſcourſes) wherewith I myſelf (as little as I am a pretender in theſe matters) am not unacquainted: and perhaps I would have given to the following treatiſes the title of trifles, inſtead of that of eſſays, if I had not been afraid of diſcouraging you thereby, and if the chymical part of them had been the chief thing, wherewith I intended to acquaint you in them. But if the reception you give to what we have already written, prove ſuch as may encourage us to proceed, we may perhaps, if God be pleaſed to vouchſafe us life and opportunity, be invited to impart to you thoſe more conſiderable chymical experiments, which either the communication of our friends, or our own labours, have preſented us. For it will be much in the power of the entertainment, which theſe papers ſhall meet with, to make them either the beginning of our labours of this nature, or the end. And in the mean time, I think I may venture to tell you, that,

as inconſiderable as I have confeſſed divers of the chymical proceſſes mentioned in the eſſays to be, yet if ever you take the pains (as I hope you will) to write experimental eſſays, and confine yourſelf to take as little upon truſt as I have done, you will perhaps be ready to believe, that ſometimes a ſhort eſſay of this nature, not to ſay ſome one ſingle experiment, may have coſt me more than a whole treatiſe written on ſuch a ſubject, whereon to be able without diſcredit to write books, it is almoſt ſufficient to have read many. And give me leave to add, that as in ſuch kind of compoſures, oftentimes the enabling himſelf to give a conſiderable advertiſement, or even hint, may coſt the writer more than the making of ſeveral experiments; ſo it may be alſo more beneficial to the reader than the knowledge of them. For we muſt not always meaſure the conſiderableneſs of things by their moſt obvious and immediate uſefulneſs, but by their fitneſs to make or contribute to the diſcovery of things highly uſeful. As, if it be true, what is reported by good authors of the hazel wand, or *virgula divinatoria*, though the hazel tree be much leſs conſiderable in reference to its fruit, or immediate productions, than a peach-tree, an orange-tree, or even an apple-tree; yet may it be made much more valuable than any of them, becauſe, whereas they only preſent us with fruits, this may aſſiſt us to diſcover in latent mines ineſtimable treaſures.

I had almoſt forgot to advertiſe you, *Pyrophilus*, that whereas I have not been ſo ſollicitous as moſt writers are wont to be, to ſwell the enſuing eſſays with the enumeration of the various opinions and arguments of authors about the ſubject I treat of, or to adorn them with acute ſentences, fine expreſſions, or other embelliſhments borrowed from eminent writers; it has not been, becauſe I utterly diſlike the making uſe of thoſe paſſages in claſſic or other authors, that may either give (among the admirers of thoſe writers) ſome authority to our thoughts, or very handſomely and emphatically expreſs them. For I remember, I have known it reprehended by learned men in *Epicurus*, that though he wrote very much himſelf, he would not vouchſafe in his writings to quote thoſe of other men. And that I have not refrained from making uſe, now and then, of thoſe philological ornaments of diſcourſe, when they readily occurred to me, and appeared neither impertinent nor prolix, may, I hope, ſuffice to keep me from being ſuſpected of the vanity of thinking myſelf above other men's aſſiſtance. But the reaſons of my ſo much declining to make uſe of other men's authority, or expreſſions, were chiefly theſe: firſt, That the weakneſs of my eyes has this long time kept me from reading almoſt any books, ſave the Scripture, with ſome critical expoſitions of it, and, here and there, ſome portions of the writings of thoſe that pretend to teach their readers experimental matters: and the unfaithfulneſs of my memory, as to things of no great moment, has made me forget almoſt all the little philological and florid learning I was formerly acquainted with. And really, *Pyrophilus*, as for the books, that treat of natural philoſophy, I am ſo ſenſible of the ſmallneſs of the advantage, which my diſabilities have ſuffered me to make of them, that inſtead of being ambitious to appear a great reader, I could be very well content to be thought to have ſcarce looked upon any other book than that of nature. And in the next place, *Pyrophilus*, though I ignore not, that by this plain and unadorned way of writing, I unkindly deny my eſſays many imbelliſhments, which I might give them, and which perhaps you will think they do abundantly need; yet my frequent diſtempers, journeys, and other avocations, not allowing me ſo much time as I deſired, to entertain you on philoſophical ſubjects, I thought it more requiſite to ſpend thoſe confined hours in acquainting you with my own thoughts, ſuch as they are, than with thoſe of other men; unleſs (as I formerly intimated) I can ſome way or other more

more than barely recite what I recite of theirs. And you will eaſily pardon me the injury, which for your ſake I do my own reputation by this naked way of writing, if you, as well as I, think thoſe the profitableſt writers, or, at leaſt, the kindeſt to their peruſers, who take not ſo much care to appear knowing men themſelves, as to make their readers ſuch.

TWO ESSAYS,

Concerning the

Unſucceſsfulneſs of EXPERIMENTS,

Containing divers Admonitions and Obſervations (chiefly Chymical) touching that SUBJECT.

Advertiſement about the two following Eſſays.

THE author of theſe diſcourſes had enlarged them in this ſecond edition, with divers obſervations and experiments, but that he has made uſe of them already in other papers belonging to his Sceptical or doubting Naturaliſt.

The Firſt ESSAY,

Of the Unſucceſsfulneſs of EXPERIMENTS.

I AM very ſorry, *Pyrophilus*, that to the many (elſewhere enumerated) difficulties which you may meet with, and muſt therefore ſurmount, in the ſerious and effectual proſecution of experimental philoſophy, I muſt add one diſcouragement more, which will perhaps as much ſurprize you as diſhearten you; and it is, that beſides that you will find (as we elſewhere mention) many of the experiments publiſhed by authors, or related to you by the perſons you converſe with, falſe or unſucceſsful (beſides this, I ſay) you will meet with ſeveral obſervations and experiments, which, though communicated for true by candid authors or undiſtruſted eye witneſſes, or perhaps recommended to you by your own experience, may upon further trial, diſappoint

appoint your expectation, either not at all ſucceeding conſtantly, or at leaſt varying much from what you expected.

THIS advertiſement may ſeem of ſo diſcouraging a nature, that I ſhould much ſcruple the giving it you, but that I ſuppoſe the trouble at that unſucceſsfulneſs, which you may meet with in experiments, may be ſomewhat leſſened by your being forewarned of ſuch contingencies: and that if you ſhould have the luck to make an experiment once, without being able to perform the ſame thing again, you might be apt to look upon ſuch diſappointments as the effects of an unfriendlineſs in nature or fortune to your particular attempts, as proceed but from a ſecret contingency incident to ſome experiments, by whomſoever they be tried.

BUT becauſe, *Pyrophilus*, the advertiſement, which I am about to give you, may ſeem, as paradoxical, as diſcouraging: it will be but reaſonable, that I preſent you with ſome inſtances of the requiſiteneſs of it: which I ſhall the more willingly do, becauſe thereby I may not only evince the truth of it, but ſomewhat leſſen the deſpondency it is apt to produce, by letting you ſee, that though ſome of your experiments ſhould not always prove conſtant, you have divers partners in that infelicity, who have not been diſcouraged by it.

To make nice and curious diſtinctions of the ſeveral grounds and occaſions of the unſucceſsfulneſs of experiments, would, perhaps, prove a work of greater difficulty than uſe; and therefore I ſhall content myſelf groſſly to diſtinguiſh the cauſes of that unſucceſsfulneſs, into the particular or miſtaken properties of the materials imployed about them, and ſome ſuch error committed in the handling of theſe materials, as though it hinder the ſucceſs of the experiment, is not eaſy to be diſcerned. Which therefore I mention, that I may diſtinguiſh theſe kind of errors, that I am now to conſider, from thoſe more obvious ones, which proceeding barely from the unſkilfulneſs of the tryers of the experiments, may be eaſily enough diſcerned, and either rectified or avoided by a knowing artiſt, or a perſon well verſed and expert in making thoſe particular experiments, which through that unſkilfulneſs may have miſcarried.

THE materials to be employed about the experiments we are conſidering may alſo admit of ſeveral diſtinctions; as into natural and factitious, ſincere and adulterate, ſimple and compound, *&c.* But we ſhall likewiſe purpoſely forbear the inſiſting on any of theſe, and content ourſelves to caſt what we have to ſay on this part of our theme, into a few and comprehenſive obſervations.

AND in this firſt place we will obſerve, that divers experiments ſucceeded not, becauſe they were at one time tried with genuine materials, and at another time with ſophiſticated ones: and in this caſe it may be all one, as to the event of the experiment, whether the materials, wherewith it was ſucceſsfully tried, were ſophiſticated or not, if thoſe made uſe of in the latter trial were of differing qualities from thoſe employed in the former; becauſe it may very well happen, that ſophiſticated bodies (as we may have occaſion to ſhew hereafter) by the addition of thoſe things, or by that deceitful way of preparation, whereby they have been ſophiſticated, may acquire an aptitude to produce ſuch effects, as, had they not been adulterated, they would not have been fit to do. Now it is ſcarcely imaginable to him, that has not been very converſant with the drugs and ſimples ſold in ſhops, how generally they are adulterated by the fraudulent avarice of the ſellers, eſpecially if they be ſuch, whoſe preciouſneſs may make their ſophiſtication very beneficial to them, that practiſe it. It has been lately much complained of by ſome of the cultivators of clover-graſs, that of a great quantity of the ſeed not any graſs ſprung up; which not being imputable

to

to the soil, nor the sower, proceeds, as some analogical observations make me suppose, from the effeteness (if I may so speak) of the superannuated seed sometimes sold in the shops. And upon this subject I cannot conceal from you what was lately affirmed to me by one of the eminentest and soberest chymists of *Amsterdam*, who was also an Indian merchant, who assured me, that the most of the cinnamon and cloves, that is brought into these western regions, is defrauded in the *Indies* of much of the finest and subtilest aromatical parts, before it be sent into *Europe*. And to give a more familiar instance to our present purpose, you may be pleased to remember, *Pyrophilus*, that in one of the first of these essays, we have made mention to you of great store of living creatures, which we had observed in vinegar; of the truth of which observation we can produce divers learned and severe witnesses, who were not to be convinced of it, till we had fully satisfied them by ocular demonstration: and yet, *Pyrophilus*, there are divers parcels of excellent vinegar, wherein you may in vain seek for these living creatures: and we are now distilling some of that liquor (which if we did not think to be of the strongest and best sort, we should scarce think worth the being distilled for spirit) wherein nevertheless we can neither by candle-light nor by day-light discern any of those little creatures, of which we have often seen swarms in other vinegars. Of such fraudulent tricks as those lately mentioned, I could easily give you divers instances, if I were not afraid of teaching fallacies by discovering them. But some are so notorious, or otherwise of such a nature, as that it may be more useful than dangerous to mention them.

It is commonly known, that sublimate is wont to be sophisticated with arsenick: and how differing the effects of such sublimate may be from those of that, which is faithfully prepared, not only upon metals, but (when mercurius dulcis and other preparations are made of it) upon human bodies, they, and scarce any but they, who are acquainted with the noxious qualities of arsenick, both to metals and men, can readily imagine. And indeed as for chymical preparations, *Helmont* * was not much in the wrong, when he affirmed, there were scarce any, vulgarly sold in shops, to be relied on as faithfully prepared. And for my part, I have so often met with chymical preparations, which I have found unsincere, that I dare scarce trust any, either in the administration of physic, or so much as in the trial of considerable experiments, which either my own furnaces do not afford me, or wherewith I am not supplied by some person, of whose skill and faithfulness I have a good opinion. The other day, having occasion to use some spirit of salt, whereof I was not then provided, I sent for some to a chymist, who making it himself, was the likelier to afford that, which was well made: but though I gave him his own rate for it, at the first rectification even in a retort, a single pound afforded us no less than six ounces of phlegm; and afterwards being further rectified in a high body and gentle heat, the remaining spirit parted with a scarce credible quantity of the like nauseous liquor, and after all these sequestrations of phlegm, was not pure enough, to perform what we expected from from it. Of which complaining to an excellent chymist of my acquaintance, he sent for spirit of salt to a very eminent distiller of it, who gets much by his profession, and passeth for a very honest man: but this spirit, besides its weakness, discovered itself to be sophisticated with either spirit of nitre, or aqua fortis, which betrayed itself by its peculiar and odious smell; whereas spirit of salt skilfully and sincerely drawn is

* *Accipe pulverem* Johannis de Vigo *propria manu paratam, nam alioquin admistus minio est adulteratus, prout qualecunque medicamen chymicum quod venale exstat fraude plenum est.* Helmon. de Febr. c. 14.

Sunt nempe olea essentialia venalia, quæque magno ære penduntur, adulterata omnia atque singula, &c. Idem de Feb. c. 15.

commonly

commonly of a greeniſh colour, bordering upon yellow, and hath uſually a peculiar, and ſometimes (as I can exemplify to you in ſome of mine) a not unpleaſing ſmell. And let me on this occaſion advertiſe you, *Pyrophilus*, that in divers caſes it is not enough to ſeparate the aqueous parts by dephlegmation, as many chymiſts content themſelves to do; but ſome liquors contain alſo an unſuſpected quantity of ſmall corpuſcles of ſomewhat an earthy nature, which being aſſociated with the ſaline ones, do clog or blunt them, and thereby weaken their activity: and therefore ſuch liquors to be well depurated require the being diſtilled off, and that with a ſlow fire, that the dry fæces may be left behind in the bottom of the glaſs. To ſatisfy ſome perſons, that this obſervation is not groundleſs, we have ſometimes taken of the better ſort of ſpirit of ſalt, and having carefully dephlegmed it, removed it into lower glaſſes (that the leſs heat will ſuffice to make the liquor aſcend) and having gently abſtracted the whole ſpirit, there remained in the bottom and the neck of the retort, whence it was diſtilled, ſo great a quantity of a certain dry and ſtiptical ſubſtance, for the moſt part of a yellowiſh colour, that it ſeemed ſtrange to the beholders, that ſo clear a ſpirit ſhould conceal ſo much of it: and we ourſelves ſhould have wondered at it too, had we not remembered, that in what the chymiſts are wont to call the oil or rectified butter of antimony made with ſublimate, the liquor, though diſtilled and very limpid, almoſt like fair water, conſiſts in great part of the very body of the antimony: which we would here manifeſt, but that we elſewhere do it; and therefore chuſe rather in this place to take notice, that the ſpirit of ſalt after this ſecond depuration was ſo changed, that it ſeemed to be a much nobler, and almoſt another liquor than it was before.

BUT to return to our ſophiſticated ſpirit; what differing effects would be produced by true ſpirit of ſalt, and that which is mixt with the ſpirit of nitre, he that knows the great diſparity in the operations of thoſe two liquors, whereof (to mention now no other inſtances) the former will precipitate ſilver, when the latter has diſſolved it, may eaſily inform you. Which inſtances I mention not as the conſiderableſt, which may be produced on this ſubject, but as the freſheſt in my memory.

IN the next place, *Pyrophilus*, I obſerve, that even when the materials imployed about experiments are no way ſophiſticated, but genuine, and ſuch as nature has produced them, or art ought to prepare them; even then, I ſay, there may be a very conſiderable diſparity betwixt concretions of the ſame kind and name, and which paſs without ſuſpicion for bodies of perfectly the ſame nature.

THIS may, to you, *Pyrophilus*, ſeem a great paradox; but perhaps upon examination it will appear a great truth: which becauſe I am, perchance, the firſt, that has ſolemnly aſſerted, I hope I ſhall obtain your pardon, if I inſiſt ſomewhat the longer upon the making it out. For though antimony (and the like is to be underſtood of quickſilver, gold, copper, tin, *&c.*) is wont by almoſt all men without heſitancy to be looked upon as being all of it of the ſame nature as well as denomination; yet he, that will take the liberty to ſuſpect, that they may be deceived in that opinion, and then heedfully obſerve the differing progreſs and event of experiments, may very well diſcern, that there is as well a difference in minerals of the ſame kind, as there is in vegetables and animals of the ſame ſpecies. And as the white roſe, the red roſe, and the damaſk roſe differ much from one another, though all three be roſes; and as the ſour and ſweet orange are very differing betwixt themſelves, and yet both of them from the *China* orange, though all be oranges; and as the hound, the grey-hound, the ſpaniel, the tumbler, the maſtiff, and the water dog, *&c.* are very diverſly qualified, though all of them be dogs: ſo neither are all the parcels of antimony to be

met with in mines or shops of altogether the same qualities, though all of them be antimonial concretes. There is indeed this difference betwixt the variety to be observed in vegetables and animals, and that which is to be found in minerals, that the former is wont to be more obvious to the eye, and betray itself by some difference to be observed, either in the size of creatures of the same kind, or in some peculiar shape or colour, by which it is easy for nature conspicuously to discriminate bodies, that consist of many discernably distinct parts; whereas minerals appearing to the eye either to be perfectly similar, as metals, or at least to consist but of two or three distinct ingredients, as cinnabar, and some other mineral concretions, the diversity to be found betwixt minerals of the same denomination is hardly to be discerned, before experience have discovered it.

And on this subject I consider, that the womb (if I may so speak) of a mineral body is not always like that of an animal, a place by a competent and peculiar involving fence secured from the intrusion of all bodies not of kin to that included in it: but a mineral being generated in the bowels of the earth, its womb is oftentimes accessible and open to other mineral juices or steams, that pass that way, though of never so differing natures from that of the more copious mineral. Insomuch that not only I have had the opportunity to observe (not without some wonder) minerals of differing kinds, as marchasites and metals, marchasites and stones, (I mean stones properly so called) salt and sulphur, and the like, blended in the same small lump of matter; but I have sometimes found, in a great mass of one sort of mineral, small parcels of a mineral of a quite differing kind perfectly inclosed in the substance of the other. But to resume what we were saying before, these intruding bodies (if I may so speak) being coagulated, and perhaps ripened together with the former by length of time, are not easily either separable, or so much as distinguishable at their first digging out of the ground, and much less after their colliquation. For the ignorant or heedless mine man aiming only at the obtaining a quantity of such a metal, or other mineral, as may be vendible under such a determinate name, has neither the design, nor perhaps the skill, to make nice separations of the heterogeneous bodies to be met with in his ore, but melts so much of them as he can promiscuously together; and then sells them, not only to the merchant, but the chymist, for that metal or mineral, whose outward form and properties (as colour, consistence, weight, sound, *&c.*) it has: though that metal, under whose name it passes, be indeed but the predominant ingredient of the lump, wherein divers other minerals may in small quantities lie concealed, and yet upon occasion be discovered by exquisite separations, or discover themselves by unexpected operations, when they meet with bodies fit to act on them, or disposed to receive impressions from them.

I was lately visited by an ingenuous goldsmith of my acquaintance, who complained to me, that being wont to buy parcels of gold brought in small pieces, and as it were sandy corpuscles, from *Guinea*, or some country of that coast, though he found it upon all trials very right gold, yet was it so very pale, that few but expert goldsmiths would meddle with it, as fearing it had been some sophisticated metal; adding, that this exceeding paleness of it sometimes reduced him to melt it with very high coloured gold, or to heighten its tincture with that of copper, to bring it to the colour of ordinary gold.

The probability of this may be proved by what is related by Monsieur *Flacourt*, governor of the French plantation in *Madagascar*, who, in his newly published history of that island, speaking of the metals of it, says, *Il y a bien*, &c. that is, there is certainly gold among the inhabitants of *Madagascar*, which has not been

Histoire de Madagascar, c. 37.

brought hither by foreign ſhips: for it is not poſſible, that ſuch ſhips ſhould have left them ſo much of that metal as they have; and beſides, it is of a differing nature from that of Europe, which they call in this country *Voulamene Voutroüa*. He adds, that this gold, which they call gold of *Malacaſſe*, is pale, and is not worth above ten crowns (or about fifty ſhillings) an ounce; alſo, that the Negroes affirm, that there are many mines of it in the country, where it was formerly digged; that there is three ſorts of it differing in fineneſs from each other, and diſcriminated by the natives by three peculiar names. But that which he adds as moſt conſiderable, is, *that Malacaſſean gold is of ſo very eaſy a fuſion, that it is almoſt as eaſily melted as lead*; whereas we here find the gold we deal with to require conſiderably ſtrong fires, and are wont to caſt in borax to facilitate the fuſion.

HAVING, upon occaſion, had the curioſity not long ſince to viſit ſome mines of lead, and other metals, I find that there is a great difference, and diſcernible even to the eye, betwixt the ſeveral ores; for inſtance, of lead, ſome of which I can ſhew you ſo like ſteel, and ſo unlike common lead-ore, that the workmen upon that account are pleaſed to call it ſteel-ore, which being of more difficult fuſion than ordinary, they are wont to mix it with other ore, which they call firm-ore, to facilitate the melting of it. And I likewiſe took notice of an ore, which for its aptneſs to vitrify, and ſerve the potters to glaze their earthen veſſels, the miners call pottern-ore, and ſell it (at leaſt where I ſaw it digged up) dearer than other ore, from which it differs both viſibly enough, and as the workmen affirm, in divers other (and thoſe leſs obvious) qualities; and yet all theſe ores, after fuſion, do paſs indiſcriminately under the name and notion of lead. In which therefore it is no wonder, that ſeverer inquiries find a great deal of diſparity. I remember I did not long ſince cauſe ſome lead-ore to be tried, which being the moſt promiſing that ever I ſaw, made me ſuppoſe it might contain ſome conſiderable quantity of ſilver: but though it proved ſo rich in lead, as to yield after the rate of ſeventy pound to the hundred, yet one of the moſt expert artiſts in Europe could not extract one grain of ſilver out of it; whereas the lead of very many mines, being ſkilfully examined, will leave behind it, upon the teſt, a proportion of pure ſilver. And though this quantity of ſilver be not conſiderable enough to make ſuch mines as yield it paſs for ſilver mines, (or, as we are wont to call them, mines-royal) becauſe the ſilver will not quit the coſt of extracting it; yet ſuch mines, though they paſs but for lead mines with the metaliſt, may appear to be mixt mines to the naturaliſt, who may meet with divers experiments, wherein the little ſilver that is in them, may make their lead operate differently from that of thoſe ores, which are wholly deſtitute of ſilver.

AND as this diſparity is diſcernible in lead-ores, ſo it may well be ſuppoſed, that the like would be diſcovered in the ores of other metals, if they were but purpoſely and ſkilfully examined. On which ſubject I remember, that a very experienced perſon in theſe affairs, and otherwiſe very candid and ſober, was lately very deſirous I ſhould procure him ſome tin-ore, alleging, that he had met with a ſort of it, which, after a long digeſtion in lixiviate liquors, afforded him a very conſiderable proportion of the richer metals; inſomuch that having a large quantity of that ore, and finding the experiment on it to ſucceed conſtantly, he promiſed to himſelf a vaſt income by it: but when that ſtock of ore was ſpent, the next that he procured, though with great carefulneſs managed as the former, would by no means be brought to afford either ſo conſiderable a benefit, or ſo much as any at all. Which brings into my mind, that having once bought a parcel of block-tin (as the tradeſmen call that, which is of the moſt pure or unmixt, and as yet unwrought) I was deſirous to try,

if I could not make a menſtruum to diſſolve it in ſuch manner as aqua fortis diſſolves ſilver, and aqua regis gold; becauſe chymiſts are wont to complain, that though they have a menſtruum or two that will diſſolve crude tin, yet they want one, that will keep it diſſolved, and will not, which aqua fortis will, let it fall into a calx. Having therefore (by a way that I elſewhere teach) prepared ſuch a liquor as was deſired, I evaporated a ſolution of the forementioned tin, and ſetting it to ſhoot, found ſomewhat, to my wonder, that the cryſtals it afforded were not at all like any kind of vitriol, but broad, flat, and exceeding thin, juſt like thoſe of ſilver. Whereupon for further trial, having examined this ſalt by the tongue, we found not, that it had any ſuch taſte as ſkilfully made calx of tin in ſpirit of vinegar, (wherein it is not every calx of *Jupiter* that is ſoluble) which (the laſt time we tried) ſeemed to us to have, as it were, a chalybeate taſte, but ſuch an exceſſive bitterneſs as may be met with in the cryſtals of ſilver made with aqua fortis. Finding alſo this further reſemblance betwixt the ſalts of theſe two metals, that they did both of them preſently dye upon the nails and ſkin a blackneſs, that could not in a ſhort time be waſhed off; we ſhould have ſuſpected, that the menſtruum had exalted the metal diſſolved in it to a greater cognation to ſilver; but having afterwards proſecuted the ſame trial with the ſame menſtruum, and another parcel of block-tin, (the former being caſually loſt) this metal, though bought very ſoon after the other, and, as I remember, at the ſame place, made us conclude, that the event of our trials proceeded from our having lighted upon a lump of tin, that was of a peculiar nature.

I REMEMBER alſo, that a while ſince a learned and inquiſitive friend of mine found in his land a parcel of ore, part of which he ſhewed me, and ſome of which I can ſhew you, but have not yet made trial of it; which ſeemed to me, among others that looked upon it, to be copper ore, and which did indeed, after fuſion, yield very good copper; but the perſons, to whom he committed the examination of the mine, being very inquiſitive, and extraordinary ſkilful, they did (as one of themſelves immediately after confeſſed to me) find in that ore, beſides the copper, no inconſiderable quantity of ſilver; and in that ſilver (having diſſolved it in aqua fortis) a conſiderable proportion of gold.

BUT to detain you no longer on this ſubject; give me only leave to ſtrenghen the paradox I have propoſed, by the authority of that great and candid chymiſt *Baſilius Valentinus*, who ſpeaking of antimony, after he hath told us, that there are ſeveral kinds of it, and eſpecially two; the one more mercurial, and of a golden property, witneſſed by the ſhining ſtreaks or beams it abounds with; the other more full of ſulphur, but deſtitute of the golden nature that inriches the former; adds, that there is ſuch a different goodneſs betwixt the ſeveral ſorts of antimony, as there is betwixt the ſeveral ſorts of fleſh or fiſh, which, though agreeing in name, and, if you pleaſe, in nature, do exceedingly differ in point of goodneſs. Which brings into my mind the great difference which I have found, even viſible to the eye, betwixt the ſeveral ſorts of antimony; and makes me alſo remember, that the other day being by an excellent chymiſt ſhewed a parcel of antimony as a rarity, upon the ſcore of the various coloured ſulphur, wherewith it was conſpicuouſly inriched, the poſſeſſor of it ſoon after imployed it to make butter of antimony: but though he were very expert in that kind of diſtillation, yet inſtead of the liquor he expected, upon the approach of a gentle fire, he found the neck and body of his retort lined with an antimonial cinnabar, (or at leaſt a red ſubſtance, by him concluded to be ſulphur;) at which being ſurprized, he was pleaſed to withdraw his fire, till he had acquainted me with this accident, and in the yet unbroken retort ſhewed me the cinnabar, which is not wont

wont (as you know) to arise till after the butter of antimony is come over, and the remaining matter be urged with a vehement fire. And it is perhaps to the undiscerned difference of antimonies, that we may sometimes ascribe that contingency, which we have divers times had occasion to take notice of in the making of antimonial cinnabar: for though in our furnaces it hath been very successfully made, yet not only we have afterwards failed of making it, but we have seen much more expert chymists, and who, because of the high value they do (not undeservedly) place upon that medicine, imploy themselves oftener than we in making it, divers times unsuccessfully attempt the preparing it. And it may be perhaps also from some diversity either in antimonies or irons, that eminent chymists have (as we have observed) often failed in their endeavours to make the starry regulus of *Mars* and antimony. Insomuch that divers artists fondly believe and teach (what our experience will not permit us to allow) that there is a certain respect to times and constellations requisite to the producing of this (I confess admirable) body. Upon which subject I must not omit to tell you, that a while since an industrious acquaintance of ours was working on an antimony, which unawares to him was, as we then supposed, of so peculiar a nature, that making a regulus of it alone without iron) the common way, (for his manner of operation I inquired of him) he found, to his wonder, and shewed me his regulus adorned with a more conspicuous star, than I have seen in several stellate reguluses of both antimony and *Mars*. Yet I dare not be too confident that this depended only upon the peculiar nature of that antimony, because since that, my own laboratory has afforded me divers such parcels of regulus without *Mars*, (some of which I have yet by me very fairly stellated) which though made of antimony, that seemed (by its various colours) to be more rich than ordinary in sulphur; yet in regard the antimony did not constantly afford a stary regulus, though by the same person ordered as near as could be after the same manner, it did not so clearly appear to me, whether the different event of the several trials proceeded from the peculiar nature of this or that parcel of antimony, or from some odd and scarce discoverable circumstance in the management of the operation. But in either case, the mention of these uncertain events will properly enough belong to our present discourse.

As in antimony, so (as I intimated above) in divers other minerals a considerable diversity may be observed: and I remember I was lately presented with a piece of a mineral, which to me, as well as to the rest who looked on it, seemed to be an ordinary and worthless marchasite; and yet a Dutch merchant (a skilful mineralist) who was the possessor of it, was very industrious to procure a greater quantity thereof, having in some of it, on which he had made trials, found a rich proportion of pure gold. And the same gentleman, whose copper ore I formerly mentioned, digging for more of that ore, found lately a quantity of red earth, which by knowing mineralists was guessed to be but bolus, and indeed looked very like it; but being melted with *regulus Martis stellatus* by a skilful trier of metals, it divers times richly recompensed the examiner's curiosity, by affording him many grains of fine gold: and though I doubt, whether this gold proceeded from the bolus, or the regulus melted with it, yet however it may serve for an instance to shew, that some mineral bodies, which pass without dispute for minerals of such and such a precise nature, may have lurking in them minerals of a quite other nature, which may manifest themselves in some particular experiments (wherein they meet with proportionate agents or patients) though not in others.

THAT

THAT the talc, which is wont to be employed about coſmeticks, is of very difficult calcination, is ſo known a thing to thoſe that have tried to calcine it, that I have met with good chymiſts, that have looked on all the calces of talcs but as impoſtures. Nor indeed have we calcined Venetian talc without ſome length of time, and much violence of heat. But among many ſorts of talc we have here in *England*, there is none, which a moderate fire will in leſs than an hour reduce into a ſnow-white calx, of which I had lately a parcel by me; and ſome days ſince I met with another ſort of Engliſh talc which I could ſuddenly calcine even with the flame of a candle. And my experienced friend Dr. *K.* aſſures me, that out of a German talc he met with, he did by digeſting it in a ſtrong ſolution of alcalizate ſalts ſeparate pretty ſtore of good gold, and might have made it a very gainful experiment, if all the talc growing in the ſame place had been of the ſame richneſs. The like almoſt has been affirmed to me by a gentleman of eminency, who told me, that from a certain talc he had out of *Norway*, he had once drawn a pretty quantity of very good gold: and it ſeems indeed, that though ſome have been pleaſed to laugh at all attempts of ſequeſtering any thing from any kind of talc; yet ſome parcels of that mineral afford good ſtore of a tincture, which may, for aught I know, be of a golden nature. For I remember I have met with a kind of darkiſh coloured talc (whereof I can yet ſhew you a piece) which when I caſt but into aqua regis, the menſtruum manifeſtly worked upon it, and diſſolved its coloured parts in ſuch plenty, that the filtrated ſolution paſſed without ſuſpicion among divers knowing naturaliſts, to whom I ſhewed it, for a fair ſolution of gold. *Paracelſus* himſelf reckons four kinds of talc, red, white, black, and of that colour, which his interpreter tranſlates luteous: and perhaps each of theſe colours compriſes ſeveral kinds of that mineral. And therefore that mineraliſt did not amiſs, when he added in the ſame diſcourſe, after he had mentioned great variety of marchaſites, ſtones, and other minerals, *Sed & hoc verum eſt, in terra multa adhuc condi, quæ mihi incognita ſunt, ſed eadem nec alii norunt. Certum ſiquidem eſt, progreſſu temporis tot tamque varia à Deo adhuc proditum iri, de quibus nemo noſtrûm nedum unquam ſomniavit.*

Paracel. *de Mineral. Tract. 1.*

Paracel. *Ibidem.*

IT is vulgarly known, that there is a great difference between vitriols, that are reputed to be meerly of the ſame metal. And not to mention thoſe vitriols, that I have either made or ſeen, of leſs uſual colours; nor to take notice of the veins, ſlate, and even looſe earth, impregnated with copperas that I have had: to paſs by all this (I ſay) as for thoſe vitriol ſtones, whereof we in *England* are wont to make our vitriol, I have ſeen, at the chief work where copperas is made, ſo great a variety of them, (divers of which I have yet lying by me) that I could ſcarcely believe the workmen, when they affirmed them to be all copperas ſtones; and cannot but think it both very likely, that ſome of them contain other mineral ſubſtances beſides vitriol, and very poſſible, that the ſaline parts of thoſe ſtones, upon their ſolution by the rain, may work upon theſe other ſubſtances formerly concoagulated with them, and thereby imbue ſome parcels of the vitriol made of them with qualities other than are eſſential to the nature of vitriol, or belong ordinarily to it.

THAT there is alſo a difference betwixt thoſe bodies, that paſs under the general name of common ſalt, cannot but be obvious to any chymiſt, that hath occaſion to make accurate trials on that ſubject. And as for thoſe concretes, that paſs under the name of ſalt-petre, there is probably no ſmall diſparity among them: for beſides the difference which we have obſerved, and which is obvious enough betwixt good Engliſh nitre, and that which is brought us over from *Barbary*, (which before it is much refined abounds very much with an adventitious ſalt, that taſtes much like ſea-

ſalt)

ſalt) beſides this I ſay, thoſe that do uſe both good European and good Eaſt-India ſalt-petre aſſure me, they find much difference betwixt them, and give the preference to the latter. And indeed I have often thought I diſcerned a conſiderable difference in the operations of ſeveral kinds of ſalt-petre even after purification: and probably that ſort of ſalt-petre, which near *London* an ingenious man of my acquaintance does ſometimes (but cannot always) make, chiefly of our ſea-ſalt, hath ſome differing qualities from that, which is drawn the common way out of the earth. And indeed ſalt-petre being but a kind of ſal terræ, generated in very differingly-qualified parcels of earth, may probably receive divers qualities from the particular ſoil, wherein it grows, though theſe qualities lie concealed and unſuſpected under the wonted exterior appearance of nitre. Which conſideration brings into my mind what was lately told me by a very ingenious gentleman, concerning one of the eminenteſt of our *London* phyſicians, who was wont, as this confidant of his aſſured me, as an excellent ſecret, to employ in ſome of his choice remedies that peculiar ſalt-petre, which he had drawn out of the earth digged up in church-yards.

AND ſuch kind of differences would probably in other mineral bodies be taken notice of, if men's prepoſſeſſions did not make them aſcribe the variations they meet with in their experiments, rather to any other cauſe, than the unſuſpected difference of the materials employed about them.

NOR is it only, *Pyrophilus*, among mineral bodies of the ſame name, that ſuch a diverſity is to be found; but, if narrowly looked into, it is very probable, that a greater diſparity may be diſcovered both among vegetables and animals, reputed of the ſame nature, than hath been yet taken notice of. Herbariſts indeed have exerciſed a commendable curioſity in ſub-dividing plants of the ſame denomination, and few naturaliſts ignore, that there are (for inſtance) many ſorts of roſes, and of apples, which differ widely betwixt themſelves, as we ſee the difference betwixt the red roſe and the white, betwixt the crab, the pippin, and the pearmain. But beſides theſe differences, which are obvious enough to be regiſtered by botanick authors, there may be more undiſcerned ones (which yet may be conſiderable ones) betwixt the individuals of the ſame ultimate ſubdiviſion of plants, ariſing partly from the temperature of the air, which makes (for example) ſenna growing in *England* to differ much from that, which is denominated from *Alexandria*; partly from the nature of the ſoil, as is obvious in the change produced in wild ſimples tranſplanted into gardens; and partly from many other cauſes, which we have not now leiſure to inſiſt upon. But we ſee oftentimes, that one roſe much differs from another of the ſame kind, and one pearmain from another pearmain. To which we may add, that the upper cruſt or ſurface of the earth being impregnated with ſubterraneal exhalations of ſeveral ſorts, and tempered with variety of juices, it may very poſſibly be, that ſome particular plant may attract ſuch juice out of a determinate ſpot of ground, as may give it exotick qualities, and make it differ even from the neighbouring plants of the ſame kind: to which purpoſe I remember, that travelling divers years ſince from *Geneva* towards *Italy*, I was in my paſſage through *Switzerland* by a gentleman of thoſe parts (whoſe brother had been formerly my domeſtick) invited to his caſtle, and entertained among other things with a ſort of wine, which was very heady, but otherwiſe ſeemed to be ſack; and having never met with any ſuch liquor during my long ſtay in thoſe parts, I was inquiſitive to know whence it was brought: and being anſwered, that it grew amongſt thoſe mountains, I could not believe it, till they aſſured me, that growing on a little ſpot of ground, whoſe entrails abounded with ſulphur, it had from the ſoil acquired its inebriating property, and thoſe other qualities,

lities, which made it ſo differing from the wine of the reſt of the vineyards of that country. And now I mention wine, give me leave, *Pyrophilus*, to put you in mind of taking notice, what a great change is made in that liquor, when upon the receſs of the ſpirits and more volatile ſulphurous parts, or elſe the new texture they make with the others, it degenerates into vinegar; and yet how little either diminution of quantity or any other alteration doth appear upon this change to the beholder's eye. And though no body is like to loſe an experiment by miſtaking vinegar for wine, becauſe both thoſe liquors and the changes of them are ſo familiar unto us; and becauſe we are wont to taſte each of them before we employ it; yet who knows what changes there may be in other bodies, with whoſe alterations we are unacquainted, though the eye, which is oftentimes the only ſenſe employed about judging of them, diſcern no change in them? as may daily be obſerved in the ſuperannuated ſeeds of plants, which, after their having been kept long beyond their due time, loſe all their germinating power, without loſing any of their obvious qualities. And here let me further obſerve to you, that urine is made much uſe of, not only by dyers, but ſeveral other tradeſmen, in divers operations (ſome of which we may elſewhere have occaſion to treat of) belonging to their profeſſions. Now theſe men being wont indiſcriminately to employ urine, without examining, whether it be rich in ſalt or not, and how long it hath been kept, it may not be impertinent to take notice, that chymiſts, who have occaſion to diſtil it often in great quantities, aſſure me, that they find a notable diſparity betwixt urines, that of healthy and young men abounding much more with volatile ſalt, than that of ſickly or aged perſons; and that of ſuch as drink wine freely being much fuller of ſpirituous and active parts than that of thoſe, whoſe drink is but beer or water. But becauſe the differing ſtrength of urines, though it be very probable, is not ſo eaſily to be ſatisfactorily made out, we ſhall rather inſiſt on this other obſervation confirmed to us by experience, which is, that though tradeſmen are often wont to boil ſuch and ſuch things indifferently in any urine, as if it were all one how new or ſtale it is, they may ſometimes thereby commit conſiderable errors. For recent urine, wherein the ſaline parts are yet entangled among the reſt, will ſuffer itſelf to be boiled above one half or two thirds away, without the avolation of its volatile ſalt and ſpirits. Whereas urine that has been divers weeks kept, is liable to a putrefaction, whereby the cement (if I may ſo ſpeak) of the ingredients that it conſiſts of, periſhing, or ſome change of texture occaſioning their diſjunction, (if not alſo concurring to produce them) the component parts fall aſunder, and the ſaline particles extricating themſelves from the reſt, will even upon a very gentle heat (as trial made on purpoſe has informed us) fly away, and leave a phlegmatick and unactive liquor behind them. In confirmation whereof I muſt acquaint you, *Pyrophilus*, with what lately befel me in reference to the diſtillation of urine: for having cauſed ſome of it to be buried in earthen veſſels in a dunghill, to be there putrified, for five or ſix weeks, I was by divers occaſional journies kept from employing it, till it had lain there between four and five months; and obſerving, when I cauſed it to be taken out, that the covers of the veſſels had not been, by him I employed to put them in, well luted on, and beſides were in ſome places cracked, I ſuſpected, that the heat of the dunghill had not only looſened the ſaline parts of the liquor, but driven them away: and accordingly by diſtilling it in a very gentle heat, and in a very high cucurbit, we obtained, inſtead of an active and ſaline ſpirit, a languid and nauſeous phlegm. And how great odds there may be betwixt ſome experiments made with recent and putrified urine, may be eaſily conceived by him who knows what operation ſalts have in the buſineſs of colours, and is acquainted with their

their efficacy in those other mechanical experiments, wherein urine is wont to be employed. But I fear I have dwelt too long upon this theme, and therefore I shall proceed to the next.

AND in the third place, *Pyrophilus*, I shall observe to you, that there is a great difference to be found among many things prepared by art, that pass under the same general name: which difference may proceed partly from that, which we have already observed to be found in the materials of which such factitious bodies are made, and partly from the way used in preparing them. To these heads many particulars may be reduced. But we shall at present restrain ourselves to the mention of two sorts of prepared bodies, namely, of such as are not purified and exalted enough, and of such as are so too much.

AND to begin with the first of these; it is very certain, that divers chymical experiments delivered by sober authors have been believed false, only because the menstruums or other materials employed in the unsuccessful trials of them were not as highly rectified, or otherwise as exquisitely depurated, as those that were used by the deliverers of those experiments; so that oftentimes the fault of a bad menstruum is injuriously imputed to a good artist. That experienced chymist *Van Helmont*, in his paradoxical treatise of the stone, endeavours (as we have elsewhere mentioned) to explicate the manner of its being generated, by the coagulation immediately ensuing upon the mixture of the two volatile spirits of urine and of wine. This noble experiment has been by many unsuccessfully tried, and has been therefore by them discredited as a chymical fiction: and indeed the first, and I think the second time we attempted to make that coagulum, we found nothing at all of any such thing as we expected upon the confusion of the two fore-mentioned liquors; which though never so much shaken, and afterwards permitted to rest, did never in the least measure coagulate, which made us long suspect the experiment; till at length our favourable thoughts of that expert chymist making us think it possible, that the spirits we employed had not been sufficiently exalted, we dephlegmated some by more frequent, and indeed tedious rectifications (which yet proved but necessary) and then were satisfied by more accurate trials, that *Helmont* had not misinformed us.

So likewise the same author in his treatise *de Peste* much extolling, as a friend to the stomach, the entrails, the nervous parts, and even the head, the tincture or solution of amber made with spirit of wine (which medicine is indeed no ignoble one, when administered to constitutions, that can well bear the heat of it) divers physicians and chymists have attempted the preparing of this tincture with such bad success, that they have given out, that either *Helmont* delivered what was not true, or concealed some considerable circumstance of the process.

WHEREAS having digested sufficiently dephlegmed spirit of wine upon very finely powdered amber (which, if it be the higher-coloured, yields the deeper tincture) in a very gentle heat (for the neglect of which caution even expert artists have often lost their pains and glasses) we have several times had a good yellow tincture of amber, which was discernable in the menstruum both by the smell and taste; and to satisfy some, that suspected the tincture to proceed but from the exaltation of the menstruum itself by digestion, and to manifest, that it was a real solution of the subtiler parts of the amber, we poured some drops of it into beer, or water, into which the spirit of wine suddenly diffusing itself, the dissolved amber was plainly discernable swimming like a thin film upon the surface of the liquor, whence, little by little, it steamed away into the air.

THERE is likewise, as we have tried, to be drawn with spirit of wine from pure salt of tartar a pretty high tincture, and of a taste, which I thought not unworthy

the taking notice of: but having a while since tried to draw this tincture with spirit of wine, which (unknown to me) was much too weak for that purpose, after I had kept the glass a while in digestion, coming to look whether or no the spirit was tinged, I found, that the salt of tartar had drawn to itself and imbibed the aqueous particles of the spirit of wine, and being thereby (for a great part of it) dissolved into a liquor like that, which is commonly called oil of tartar *per deliquium*, the subsiding salt was by the interposition of that saline liquor protected from the action of the spirit of wine; which being by this new way dephlegmed, would not mix with the saline liquor, but swam entirely above it. To which I shall only add in general, that the German chymists are divers of them so accurate in the rectification of their spirit of wine, that in *England*, where we are wont to be less careful about that particular, it is usual enough for those experiments of theirs to be unsuccessfully tried, wherein the alcohol of wine (as they call it) is requisite.

And as spirit of wine, so many other menstruums are made unfit for the perfecting of divers real experiments, barely by their not being sufficiently freed from their weakening aquosity.

Nor is it only, *Pyrophilus*, in menstruums, but in divers other bodies, that the want of an exquisite depuration may produce in experiments variety of events. As for instance, it has been complained of by sober men, that their preparations of silver, though never so carefully made, have been apt to produce violent vomits; whereas we have not observed a well-prepared medicine of duly refined silver to work emetically, even in women and girls, but by seige or urine. But we cannot wonder at the violent operation of medicines made of ordinary silver: for not only that, which is coined, is wont, as the mint-masters themselves have confessed to me, to be allayed with sometimes about a twelfth part, sometimes a smaller or greater proportion of copper, for the greater conveniency of the coin; but even that silver, which is commonly at great rates sold for refined silver, is not wont to be sufficiently freed from its copper. Which I not long since manifested in the presence of one of our richest and eminentest refiners, by dissolving some of his purest silver in his own aqua fortis; for the greenness of the solution quickly betrayed the adherency of *Venus* to the silver. And no wonder, for I have seldom seen our chiefest refiners blow off from their silver upon the test above half its weight of lead, whereas we think not our silver sufficiently refined for some purposes, till it have been freed from five or six times its weight of *Saturn*; and then it has sometimes afforded a solution almost as clear as water, with only now and then a light touch of sky-colour, but nothing near so high as the ceruleous (liquor that is supposed to be a true) tincture of silver, artificially separated from the rest of the body.

Now that ill effects by the mixture of copper may be produced in such medicines, as ought to be of pure silver, he, that is acquainted with the violent emetic qualities of *Venus*, can scarcely doubt. And as in men's bodies, so in other subjects, those experiments may easily deceive the artist's expectation, when he hopes to perform with silver and copper together those things, which suppose and require silver without copper, or any adventitious metal: and as silver, so gold is very often employed for pure, when it is not so: for even the foliated gold, which is commonly sold here in *England*, how fine soever it is reputed, is not altogether free from the pollutions of other metals; for our gold-beaters, though for their own profit sake they are wont to use the finest coined gold they can get (as that which is capable of the greatest extension under the hammer) yet they scruple not to employ coined gold, and that the mint-masters (as themselves inform me) are wont to allay with copper or silver, to make the coin more stiff, and less subject to be wasted by attrition. And as for those many

many goldſmiths and chymiſts, who think their gold moſt requiſitely refined, when they have blown from it on the teſt a due proportion of lead, they may be therein ſometimes miſtaken: for though *Saturn* may carry away with him all the copper, that did imbaſe the gold, yet he does not likewiſe free it from the ſilver (for which purpoſe aqua fortis is therefore wont to be uſed) nay, the ſkilfulleſt refiner, that I ever yet knew, hath ſeveral times affirmed to me, that cupelling fine gold with lead, the gold has after retained and protected from the fire a proportion of ſilver, that lay lurking in the lead, and was afterwards ſeparated from the gold by aqua fortis, but in ſo ſmall a quantity, that the experiment (the coſt and pains conſidered) was not lucriferous. And of this ſort of inſtances, *Pyrophilus*, more might be preſented, if we did not think prolixity might be unwelcome to you.

BUT as many experiments ſucceed not according to expectation, becauſe the menſtruums employed about them were not pure enough; ſo ſome miſcarry, becauſe ſuch menſtruums are but too exactly depurated: for it is not ſo much the purity of liquors in their kind, as their fitneſs for the particular purpoſe, to which they are deſigned, that is in experiments to be principally regarded. For inſtance, we have ſometimes, for recreation ſake, and to affright and amaze ladies, made pieces of white paper and linen appear all on a flame, without either burning, ſinging, or as much as diſcolouring them. This is performed by plunging the paper very thoroughly in weak ſpirit of wine, and then approaching it to the flame of a candle; by which the ſpirituous parts of the liquor will be fired, and burn a pretty while without harming the paper. But if this experiment be tried with exquiſitely rectified ſpirit of wine, it will not ſucceed. Of this phænomenon this plauſible reaſon has been aſſigned, that the flame of the ſpirit of wine is ſo pure and ſubtile, that, like an *ignis lambens*, it will not faſten upon the paper. But experience has informed us, that this conjecture is but a miſtake, for the flame of ſpirit of wine is ſo hot, that I have in lamp-furnaces employed ſpirit of wine inſtead of oil, and with the ſame flame I have not only lighted paper, but candles, and even melted foliated gold. The true reaſon therefore, why that paper is not burned by the flame, that plays about it, ſeems to be, that the aqueous part of the ſpirit of wine being imbibed by the paper, keeps it ſo moiſt, that the flame of the ſulphureous parts of the ſame ſpirit cannot faſten on it. And therefore when the deflagration is over, you ſhall always find the paper moiſt; and ſometimes we have found it ſo moiſt, that the flame of a candle would not readily light it. And on the other ſide, having purpoſely made trials of plunging paper into ſufficiently dephlegmated ſpirit of wine, the paper not having aqueous moiſture to defend it, was very readily kindled and burned by the flaming ſpirit. And one of our beſt ways to try the pureneſs of ſpirit of wine is grounded on this very ſuppoſition: for dipping it in a cotton-wick like that of a candle, and ſetting it on fire, if the flame faſten on the wick, it is a ſign of the goodneſs of the ſpirit; but if it do not, we conclude it to be weak, and not ſufficiently dephlegmed. It hath been likewiſe obſerved, that aqua fortis will work more readily on lead, if it be allayed with water, than if it be purely rectified. I otherwhere alſo mention an aqua fortis I have uſed, which was ſo ſtrong, that it would not well diſſolve ſilver itſelf, unleſs I firſt diluted it with fair water. And within this very week, wherein I write theſe things, I have had an unwelcome proof, that liquors may by too exquiſite a depuration be made unfit for our purpoſes. For having, to gratify ſome ingenious friends, made a certain menſtruum, wherewith we had formerly done ſome things upon gold, which were (not altogether without cauſe) thought ſtrange enough, we took care at this time to ſeparate it from whatever was either of an aqueous or an earthy nature, more exactly than ever we had formerly done. But coming to make uſe of this ſort of menſtruum,

we found to our trouble and loſs, that inſtead of performing its wonted operations upon gold better than before, we could do nothing at all with it: for it will not now by heat itſelf be brought to touch gold, though that metal were wont to be diſſoluble in it even *in frigido*. And to ſatisfy you, that it was the too exquiſite depuration of this liquor, eſpecially from its terreſtrial parts, that thus unfitted it to touch a metal, which is otherwiſe wont to melt as it were naturally in it without ebullition (almoſt like ice in luke-warm water;) we will ſubjoin, that not only we in vain tried to make it ſerviceable by weakening it with fair water; but having, for trial-ſake, taken a little of this numerical parcel of liquor before it was ſo carefully rectified, we found, that it diſſolved crude gold as well as we had reaſon to expect. And it would be conſidered, whether or no in the extraction of the tinctures of ſeveral bodies, chymiſts do not only put themſelves to a needleſs, but to a prejudicial trouble, when they refuſe to employ any other ſpirit of wine, than that which is highly rectified. For, though in many bodies the parts deſired by the artiſts being the ſulphureous ones, the menſtruum is the better for an exquiſite dephlegmation; yet in divers other concretes the uſeful and efficacious parts have in them ſomething of ſaline, which makes them more free to impregnate copiouſly ſuch liquors, as have ſome aqueous mixed with their ſulphureous parts.

But becauſe there is nothing more eaſy than by diluting ſpirit of wine, though never ſo ſtrong, to make it as weak as one pleaſes; and becauſe pure ſpirit of wine is that of all other menſtruums, that chymiſts generally make moſt uſe of, and which coſts them moſt of charge and trouble (inſomuch that here in *London* that, which is perfectly dephlegmed, is valued, in their ſhops that ſell both, at ten times the price of common ſpirit of wine;) I preſume you will not take it ill, that without being obliged to it by the title of this diſcourſe, I take this occaſion to acquaint you with the way I employ to obtain dephlegmed ſpirit of wine; eſpecially ſince the practice of the common way of frequent rectifications is (not to mention other inconveniences) wont to prove either exceeding tedious, or inſufficient. Put then about an inch thick of tartar calcined to whiteneſs (for I find it not neceſſary to reduce it to a ſalt) and very dry, into the bottom of a tall and ſlender glaſs body, and pour on it as much ſpirit of wine, that has been but once rectified, as will, when they have been ſhaked together, ſwim above the tartar a finger's breadth (more or leſs in proportion to the tartar you put in) and then the head and receiver being carefully faſtened on again, in a gentle heat draw off the ſpirit of wine, ſhifting if you pleaſe the receiver, when about half is come over, and if need be, rectifying once more all that you diſtil upon dry calx of tartar as before. Whether or no you may meet with this method in ſome chymical books, I know not: but it ſeems, that either it has not been clearly taught, or has been propoſed by ſuſpected authors, or elſe among other proceſſes, by being found in whoſe company it has been diſcredited. For the moſt antient and experienced diſtillers I have met with, have either contented themſelves to follow the common way of repeated rectifications, though thereby they loſe much time, and much ſpirit of wine; or elſe have had recourſe to peculiar veſſels of ſuch a height, as beſides that they are neither eaſily nor cheaply to be procured, do not, as far as I have hitherto ſeen, excuſe the need of reiterated rectifications. Whereas, when we conſidered, that the fixed ſalt of tartar readily imbibes aqueous bodies, and that yet it will not at all mix with pure ſpirit of wine, it was eaſy to conclude, that the phlegmatic part of the ſpirit of wine would be ſoaked up by the alcalizate ſalt, whereby the inflammable part would be freed from it. And accordingly when we proceeded after the manner above preſcribed, we found, that the liquor, that was produced upon the firſt rectification from the ſalt, being fired in a warm ſilver-ſpoon, did not

leave

leave behind it one drop of phlegm, or so much as the least moisture upon the spoon; nay, and indeed did endure a severer examen, to which for curiosity's sake we thought fit to put it. And when the distillation was carefully made, we found by frequently (for trial-sake) shifting the receiver, that all the spirit that ascended was (to sense) equally pure, since that which came up last of all, even till the calx seemed to begin to grow dry, by beginning to cleave at the top, did burn all away, as well as that which came over first. And having for further trial taken out the calcined tartar, and distilled it with a good fire, it yielded us pretty store of a nauseous and strongly scented liquor, which seemed to be but phlegm, both to the taste, and by its not being at all inflammable, though carefully tried. The same calx of tartar being kept in some earthen vessel upon the fire till it be well dried, which will require a good heat, may be employed more than once in this operation. And it was not needlesly that we prescribed bodies tall and slender; for we found not the experiment to succeed in large and low ones, and much less in retorts, in which the phlegm is wont to rise together with the spirit; yet we found, that provided the distillation were made with a sufficiently mild heat, a glass, though very broad, and but moderately high, would serve the turn so far, as that the first half that ascended (the other being very weak) proved a spirit, that in a silver-spoon would burn perfectly all away. And because white calx of tartar is sometimes not so easy to be procured, we will add, that we have for trial-sake sometimes substituted quick-lime, or salt of pot-ashes (made by a single solution, filtration, and coagulation) with no bad success, especially in case of removing the receiver before the ascension of the last part of the liquor, though even that itself has sometimes from quick-lime come up inflammable enough. And therefore this alcohol of wine we peculiarly call the alcalizate spirit of wine; and the rather, because *spiritus vini tartarizatus*, which perhaps may be thought the propereſt name for it, is employed by eminent chymical writers to signify a different thing. And a practicable way of making such an alcalized and pure spirit of wine we thought not unfit to teach you here once for all, in regard the menstruum is so highly useful, not only for tinctures, extracts, and many other chymical operations, but in the making of divers philosophical experiments, and particularly some of those, which you may meet with in our writings. And an eminently ingenious person (but to me a stranger) chancing to get a sight of this essay, was pleased to give me thanks for this last part of it; because, though he had very often made use of salt of tartar to improve spirit of wine, yet he did it before, not to dephlegm the weaker liquor, but to acuate the strong with the alcali: which though I deny not to be a thing feasible, yet (as I told him) unless it be skilfully attempted, the highly rectified liquor, that is poured on, will rather leave some of its most spiritous parts behind, than carry up so fixt a salt.

The Second ESSAY,

Of Unſucceeding EXPERIMENTS.

WHAT has been already ſaid, *Pyrophilus*, may, I hope, ſuffice to ſhew you, how experiments may miſcarry upon the account of the materials employed in trying them. And therefore we ſhall now paſs on to conſider the contingencies, to which experiments are obnoxious, upon the account of circumſtances, which are either conſtantly unobvious, or at leaſt are ſcarce diſcernible till the trial be paſt. And becauſe theſe circumſtances can hardly be diſcourſed of in an accurate method (which their nature will ſcarce admit of) I ſhall not tie myſelf to any other order in ſetting down the inſtances, which occur to me on this occaſion, than that wherein they offer themſelves to my memory.

AND firſt I muſt acquaint you with what was not long ſince ſeriouſly related to me by doctor *K.* a perſon exceeding far both from the cuſtom, and, in this particular, from the temptation of telling untruths. He then aſſured me, that lending his laboratory in *Holland* to a friend of his during his own abſence, and leaving in that laboratory among other things great ſtore of aqua fortis of ſeveral compoſitions, which he had made, to employ about his famous ſcarlet-dye, this friend of his ſent him word a while after his departure, that by digeſting gold with an aqua fortis, he had ſeparated the tincture or yellow ſulphur from it, and made it volatile (the remaining body growing white) and that with this golden tincture he had, not without gain, turned ſilver (as to part of it) into very perfect gold. Upon which advertiſement the doctor ſpeedily returning to his laboratory, did himſelf with the ſame aqua fortis divers times draw a volatile tincture of gold, which did turn ſilver into true gold: and (that I may add that upon the bye, to gratify your curioſity) when I demanded, whether or no the tincture was capable to tranſmute or graduate as much ſilver, as equalled in weight that gold, from whence the tincture was drawn, he aſſured me, that out of an ounce of gold he drew as much ſulphur or tincture, as ſufficed to turn an ounce and a half of ſilver into that nobleſt metal. Which I am the more diſpoſed to believe, partly becauſe my trials permit me not to doubt of the ſeparableneſs of a yellow ſubſtance or tincture from gold; and partly becauſe I am tempted to think, that ſilver may have in it a ſulphur (to ſpeak in the chymiſts language) which maturation is capable to graduate into a golden one, by having been certified by the obſervations of men very experienced in metalline affairs (and perhaps too by my own) that ſometimes by corroſive liquors (which Sir *Francis Bacon* alſo, if I miſtake not, ſomewhere obſerves) and ſometimes by the operation of common ſulphur (eſpecially well opened and aſſociated with fit ſalts) ſilver has afforded ſome grains of very pure gold. But our doctor found himſelf much miſtaken in the hopes of growing rich by this experiment; for a while after endeavouring to make it again, his hopes were fruſtrated, which he aſcribes to the aqua fortis, and therefore has attempted the ſame work afreſh. But ſince all his trials have been hitherto fruitleſs, it is not improbable, that the diſappointment proceeded from ſome other more abſtruſe cauſe; for we find ſuch adventures to have ſometimes befallen artiſts irreparably. And *Glauber* alone, if you will therein credit him, tells us of ſeveral ways, by which he

he made gold once, and could not do it again. Upon which subject I must not omit those very illustrious testimonies and instances of this nature, that I find recorded by that ornament of his age and quality, the prince of *Mirandola*, in his treatise *de Auro. Lib. 3. cap. 6.* *Novi* (says he) *qui mihi asseruerit semel se ex mobili argento, quod vivum dicitur, stabile verumque argentum confecisse succis & foliis herbarum, idque vendidisse peritis explorandæ metallicæ veritatis; eisdem mox usum se foliis frustra, & quod semel perfecerat, nunquam alias, quanquam id sæpe tentaverit, perficere potuisse.*

Alium novi (says he further) *qui adhuc apud vivos moratur, cui cum aurum & argentum circiter quindecies per artem effectum esset, amisit artem eam, accepitque oraculo socii per quietem habito, id ingratæ mentis vitio contigisse. Ut hinc etiam veritatem apostolici dicti condiscamus, Neque qui plantat, neque qui rigat, est aliquid, sed incrementum dat Deus.* And to both these narratives our learned prince does in the same book add divers others. *Retulit quidam mihi* (subjoins he) *sese aurum ex argento fecisse semel magnâ copiâ; secundo se usum eisdem rebus, fecisse quidem, sed minimâ semper quantitate, sic ut detrimentum lucro majus esse supputaverit. Venisse in mentem, uti detrimentum effugere possit, si non ex argento, sed ex ære melioris conditione metalli, sese consequi experiretur, idque se conjecturis firmis nixum tentavisse: cumque in eo fuisset, ut rem sese adepturum speraret, miris modis evenisse, ut nihil omnino consequeretur.*

Idem (continues the prince) *affirmavit ab amico, qui expertus hoc ipsum fuerat accepisse, qui cum ex cinnabari argentum fecisset optimum, sæpenumero sese postea insistentem operi majore cum diligentia semper eventu rei fuisse frustratum.* And to these relations of this famous prince I could add others of some acquaintances of mine, who having either once or twice made *luna fixa* (as artists call that silver, which wanting but the tincture of gold abides the trial of aqua fortis, *&c.)* or some other lucriferous experiment, have since in vain attempted to do the like again; and yet have their eyes so dazzled by the gold and silver they have (either separated or) made, that they are not to be prevailed with to desist from prosecuting their uncertain hopes.

THAT divers experiments succeed, when tried in small quantities of matter, which hold not in the great, it may save you something to be advertised of; divers projectors, especially chymists, having already very dearly bought the knowledge of that truth, for oftentimes a greater and unwieldly quantity of matter cannot be exposed in all its parts to a just degree of fire, or otherwise so well managed, as a less quantity of matter may be ordered. But this is so manifest a truth to those, that have dealt much in experiments, that whereas many chymists would be vastly rich, if they could still do in great quantities what they have sometimes done in little ones, many have undone themselves by obstinately attempting to make even real experiments more gainful.

I HAVE not been very solicitous to subjoin particulars to the foregoing observations, because that by reason of the contingency of such experiments, as would be the most for my present purpose, you might possibly be tempted to lose toil and charges upon trials, very likely not only to delude your hopes, but perhaps to make you distrust the fidelity of our relations. Yet for illustration-sake of what we have delivered, I am willing to mention some few contingent experiments, that occur to my thoughts.

AND first, it is delivered by the Lord *Verulam* himself, as I remember, and other naturalists, that if a rose-bush be carefully cut as soon as it has done bearing, it will again bear roses in the autumn. Of this many have made unsuccessful trials, and thereupon report the affirmation to be false; and yet I am very apt to think, that the Lord *Verulam* was emboldened by experience to write as he did. To clear up which difficulty, let me tell you, that having been particularly solicitous about the experiment,

I find by the relation both of my own and other experienced gardeners, that this way of procuring autumnal roses will in most rose-bushes most commonly fail, but in some, that are good bearers, it will succeed; and accordingly having this summer made trial of it, I find, that of many bushes, that were cut in *June* in the same row, the greater number by far promise no autumnal roses, but one, that hath manifested itself to be of a vigorous and prolific nature, is at this present indifferently well stored with damask-roses. And there may be also a mistake in the kind of roses; for experienced gardeners inform me, that the musk-rose will, if it be a lusty plant, bear flowers in autumn without the help of cutting. And therefore that may be misascribed to art, which is the bare production of nature. And cinnamon rose-bushes do so much better thrive by cutting than several other sorts, that I remember, this last spring, my gardener having (as he told me) about mid *April* (which was as soon as that kind of rose-bush had done bearing) cut many of them in my garden, I saw about the middle of *June* store of the same bushes plentifully adorned both with buds and with blown flowers.

An uncertainty not unlike that, which we have newly taken notice of in the experiment of producing autumnal roses, has been likewise observed in the attempts, that have been made to make divers sorts of fruit grow upon the same tree. And as for differing sorts of fruits of the same denomination, as apples, pears, *&c.* though some severe naturalists are unwilling to believe, that they can be made to grow upon the same tree; yet we dare not imitate their severity, having lately seen various sorts of pears fed by the same tree, and elsewhere three and twenty sorts of apple-grafts flourishing upon the same old plant, and most of them adorned with fruit. Nay, and though the fruits be not of the same denomination, yet if they be of kin in nature, they may very possibly be brought to grow on the same tree: for we lately gathered ripe apricocks and ripe plums upon one tree, from which we likewise expect some other sorts of stone-fruit. But to make fruits of very differing natures be nourished prosperously by the same stock, is so difficult a thing, that we can at most but reckon it among contingent experiments. For though *Pliny* and *Baptista Porta* relate their having seen each of them an example of the possibility of producing on one tree great variety of differing fruits; and though such a person as the deservedly-famous astronomer Dr. *Ward* assures me, that he has particularly taken notice of pears growing upon an apple-tree; and I elsewhere add a resembling observation of ours; yet certainly this experiment has been for the most part but very improsperously attempted; nor have I yet ever seen it succeed above once, though tried with very much care and industry. And I remember, that this very year, in the same garden where I gathered the apricocks and plums above mentioned, I saw the cions of a pear-tree so skilfully grafted upon an apple-stock, that it flourished very much with blossoms in the spring, and gave me great hopes, that it would bear fruit this newly-past summer, but has deceived my expectation; as divers other plants so grafted in the same garden have for many years deluded the hopes of the skilful master of it, who assures me, that though divers of them did for some years successively afford promising blossoms, yet they all decayed away without bearing any of them any fruit. Which yet may seem somewhat strange, since not only we have this summer gathered pears upon a graft, which a divine, to whom the garden belongs, affirmed to have been grafted upon a quince-tree; and the industrious *Kircher* tells us, that *Experientia docet Persicum moro insitum fructus proferre, &c. de quo nullum est dubium utpote vulgare penè:* 'but experience tells 'us, that as little as a white-thorn and a pear-tree seem of kin, a cion of the latter 'will sometimes prosper will, being grafted upon a stock of the former.'

Artis Mag. Lucis & Umbræ, lib. 1. p. 3 cap. 6.

To contingent experiments, *Pyrophilus*, you may, if you please, refer what is delivered by those learned writers, who affirm, that if a lixivium made of the ashes or fixed salt of a burned plant be frozen, there will appear in the ice the idea of the same plant; for we have divers times purposely made trial of this experiment without the promised success. And I remember, that in the last cold season, proper for such trials, I purposely made a lixivium of fair water and salt of wormwood; and having frozen it with snow and salt after the manner of congelation elsewhere declared, I could not discern in the ice any thing more like to wormwood than to several other plants. And having about the same time, and after the same manner, exposed to congelation a thin phial full of a strong decoction of wormwood (from which an idea of the plant may be more probably expected) those, to whom I shewed it, after it was frozen, could discern as little like wormwood in it as myself. It is true, that in both these phials the ice seemed somewhat oddly figured; but it is true also, not only that we have observed that water, wherein a saline body, as salt petre, or sea-salt, or sugar, *&c.* has been dissolved, has afforded us ice, which seemed to shoot into several figures, but even in ordinary water congealed we have often seen ice figured, as if the water had been no elementary body; which needs not be admired since (to omit other causes, which may concur to the production of this effect) many waters gliding through earths abounding in saline particles of this or that nature, may be easily, in their passage, impregnated with them; whence perhaps it comes to pass, that dyers find some waters very fit, and others very unfit for the dying of scarlet and some other colours. And therefore we cannot but think, that the figures, that are oftentimes to be met with in the frozen lixivium or decoction of a plant, will afford but uncertain proofs, that the idea of each, or so much as of any determinate plant, displays itself constantly in that frozen liquor. And I much fear, that most of those, that tell us, that they have seen such plants in ice, have in that discovery made as well use of their imagination as of their eyes. And it is strange to observe what things some men will fancy, rather than be thought to discern less than other men pretend to see. As I remember Mr. *R.* the justly famous maker of dioptrical glasses, for merriment telling one, that came to look upon a great tube of his of thirty foot long, that he saw through it in a mill six miles off a great spider in the midst of her web; the credulous man, though at first he said he discerned no such thing, at length confessed he saw it very plainly, and wondered he had discovered her no sooner. But yet, *Pyrophilus*, because two or three sober writers do seriously relate some stories of that nature upon their own observation, I am content for their sakes to reckon their experiments rather among the contingent than the absolutely false ones: for it is not impossible, but that among the many figures, which frozen liquors do sometimes put on, there may appear something so like this or that plant, that being looked upon with the favourable eye of a prepossessed beholder, it may seem to exhibit the picture of the calcined vegetable: and we ourselves, not very long since, setting to freeze in snow and salt a fine green solution of good verdegrease which contains much of the saline parts of the grapes coagulated upon the copper by them corroded) obtained an ice of the same colour, wherein appeared divers little figures, which were indeed so like to vines, that we were somewhat surprized at the experiment; and that which increased our wonder was, that another part of the same solution being frozen in another phial by the bare cold of the air, afforded us an ice angularly figured as we have observed the ice of saline liquors oftentimes to be) but not at all like that made by the application of snow and salt. And having, for further trial sake, suffered that ice, wherein the vines appeared to thaw of itself, and having then frozen the liquor a second time in

7

the same phial, and after the same manner as formerly, we could not discern, in the second ice, any thing like that, which we had admired in the first. And in wine and vinegar, as much as those liquors partake of the nature of the vine, we have not, after congelation, observed any peculiar resemblance of it in figure.

THE mention we have been making of ice brings into my memory another experiment, which may perhaps be reckoned likewise among contingent ones, and that is the experiment of burning with ice as with a glass lens; which though some eminent modern writers prescribe to be done, without taking notice of any difficulty in it, yet both we and others, that have industriously enough tried it, have met with such defeating circumstances in it, especially from the uniform texture wont to be met with in most ice, that the making of such burning-glasses may be well enough referred to those experiments, whose constant success is not to be relied on, as we elsewhere more particularly declare.

IN the trade of dying there is scarce any tinging ingredient, that is of so great and general use amongst us as woad or glastum; for though of itself it dye but a blue, yet it is used to prepare the cloath for green and many other of the sadder colours, when the dyers have a mind to make them permanent, and last without fading: but yet in the decocting of woad to make it yield or strike its colour, there are some critical times and other circumstances to be observed; the easy mistake of which oftentimes defeats the dyer's expectation to his very great loss, which sometimes he knows not to what to impute, of which I have heard several of them complain: and therefore divers of our less-expert dyers, to avoid those hazards, leave off the use of woad, though growing plentifully enough here in *England*, and instead of it employ indico, though it cost them dearer, as being brought hither sometimes from *Spain*, sometimes from the *Barbadoes*, and oftentimes even from the *East-Indies*.

OUR *London* refiners, when, to part silver and copper, they dissolve those mixed metals in aqua fortis, are wont afterwards to dilute the glutted menstruum with store of fair water, and then with copper-plates to strike down the dissolved silver. But because by this manner of proceeding much copper is wont, after the separation of the silver, to remain in the menstruum, as may appear by its high tincture, that this thus impregnated liquor may be improved to the best advantage, they are wont to pour it upon what they call whiting which is said to be a white chalk or clay finely powdered, cleansed, and made up into balls) wherewith the tincted parts incorporating themselves, will, in some hours, constitute one sort of verditer fit for the use of painters, and such other artificers as deal in colours, leaving the remaining part of the menstruum an indifferently-clear liquor; whence they afterward, by boiling, reduce a kind of salt-petre fit, with the addition of vitriol (and some fresh nitre) to yield them a new aqua fortis.

AND these things I mention, *Pyrophilus*, that you may know what I mean, when I tell you, that sometimes the refiners cannot make this verditer for a great while together, and yet cannot tell, whence their disability to make it proceeds. Of which contingency I remember I lately heard one of the eminentest and richest of them sadly complain, affirming, that neither he, nor divers others of his profession, were able, not long since, to make verditer for divers months together; and that several others were yet at a loss in reference to that particular: though for his part he had, without knowing the cause of this contingency, found a remedy for it, namely, to warm the menstruum well before it be poured on the whiting; on which, when the liquor was warm, the tincted parts would fasten, though they would not, whilst (according to the custom of refiners) it was poured on cold.

MAKING

MAKING likewise the other day a visit to the chief copperas work we have in *England*, one of the overseers of it, who went along with me to shew me the contrivance of it, assured me, that divers times, by the mistake or neglect of a circumstance in point of time, they had lost, and are yet subject to lose, some thousands of pounds of vitriol at a time, which in spite of their wonted, but not sufficiently attentive and skilful care, would degenerate into an unctuous substance, not to be reduced into good vitriol again; unless by the tedious way of throwing it abroad, and exposing it with the unprepared stones, from which they draw their vitriol, to the rain and sun to be opened anew, and fitted for the yielding of vitriol after the same manner with those crude minerals.

UPON this occasion I must not omit, because much conducing to the scope of our present discourse, a memorable relation, that I have met with in the Indian History of the learned *Josephus Acosta*, who diligently surveyed the famous and almost inestimable mines of *Peru*, and (for one that was not a chymist) has delivered divers considerable and judicious observations about them. That which I am now to mention, is in that chapter, where he treats of the silver of the *Indies*, set down in these words: 'It is strange to see not only the difference betwixt the refining of metal 'by fire, and without it by quick-silver, but also that some of these metals, 'which are refined by the fire, cannot well be molten with any artificial wind, as 'with bellows, but when it is kindled, and blown with the natural air or natural 'wind. The metal of the mines of *Porco* is easily refined with bellows; and that of 'the mines of *Potosi* cannot be molten with bellows, but only by the breath of their 'guayars, which are small furnaces upon the sides of the mountains, built expressly 'where the wind lies, within the which they melt this metal: and though it be hard 'to yield a reason of this difference, yet it is most certain and approved by long 'experience.' *Josephus Acosta, l. 4. cap. 5.*

IF there be any trade that obliges the artificers to be assiduously conversant with the materials they employ, it is that of the glass men; and yet even to them, and in their most ordinary operations, there happen now and then little accidents, which, though they know not well to what to ascribe, are not yet capable of hindering them from doing sometimes what they have done a thousand times. And I remember, that among the last times I have been at a glass-house, an eminently-skilful workman, whom I had purposely engaged to make some vessels for me, that required more than ordinary dexterity, was not able, when I came thither, to make metal (as they call that colliquated mixture of sand and fixt salt, whereof they blow their glasses) tolerably fit to be employed: wherefore he desired me to take the pains to come again another day, and he would try to repair his unluckiness. But the next time I came, though it were upon appointment, his metal proved again unserviceable, and instead of being colourless, when it was cold, looked as if it had been stained with blue and yellow, and was besides brittler than it ought to have been. So that it need be no such wonder, if philosophers and chymists do sometimes miss of the expected event of an experiment but once, or at least but seldom tried, since we see tradesmen themselves cannot do *always*, what, if they were not able to do *ordinarily*, they could not earn their bread.

IT is affirmed by *Helmont* and others, that treat of the *Lapides Cancrorum*, that they grow within the skulls of those craw-fishes, from whence they have their name: but I have known good anatomists complain, that they have sought them in vain in the heads of those fishes, which may well make them distrust the veracity of those, that ascribe them to that sort of animals; yet we have often taken those stony concretions

out of the heads of craw-fishes. But passing lately through *Hungerford*, a town famous for the plenty of such kind of fish, we made diligent enquiry concerning their nature, and were there informed by those that looked to them, that the concretions above-mentioned are to be found in their heads but about that season of the year, wherein they shift their shells, and that at other times of the year, several persons had in vain endeavoured to store themselves with crabs eyes at *Hungerford*. And indeed, having at the last time of my being there (which was about the latter end of *June*) caused divers large ones to be taken out of the water, we found these little stones but in the head of one of them; whereas about a fortnight before, which was near the summer solstice, passing by that place, we found in the wonted parts of the heads several such concretions, as to bigness and shape, but so soft, that we could easily crush and discind them betwixt our fingers. And certainly the mistake of the circumstance of time has much prejudiced the reputation of many truths: and I remember, that *Asellius*, to whose anatomical fortune the world is so much beholden, ingenuously acknowledges, that he had like to have lost the discovery of the milky veins, because having at first suspected those unlooked-for white vessels, which he took notice of in the mesentery of a dog dissected alive, to be some irregular ramifications of nerves, he was much confirmed in his conjecture by the next dog he opened; for having dissected him at an inconvenient distance of time from the dog's repast, the slender vessels he looked for being destitute of the chyle, which is it, that makes them conspicuous, did not appear. So that he had lost the benefit of his first lucky observation, had not his sagacity inclined him to suspect, that if a dog was plentifully fed at a convenient distance of time before his being dissected, the vessel swelled with alimental juices would be the better discernible: whereupon, having feasted another dog some hours before he opened him, he manifestly detected those milky vessels, whose discovery has since set anatomists so usefully on work.

But, *Pyrophilus*, not to exceed the limits of an essay, I must not multiply instances of the contingencies of experiments, but content myself to tell you in general, that in divers cases such circumstances as are very difficult to be observed, or seem to be of no concernment to an experiment, may yet have a great influence on the event of it. If on either of the extremes or poles of a good armed load-stone, you leisurely enough, or divers times, draw the back of a knife, which has not before received any magick influence, you may observe, that if the point of the blade have in this affriction been drawn from the middle of the æquator of the load-stone towards the pole of it, it will attract one of the extremes of an equilibrated magnetick needle; but if you take another knife, that has not been invigorated, and upon the self-same extremity or pole of the load-stone thrust the back of the knife from the pole towards the æquator, or middle of the load stone, you shall find, that the point of the knife has, by this bare difference of position in the blade, whilst it past upon the extreme of the load-stone, acquired so different a magnetick property, or polarity, from that, which was given to the former knife by the same pole of the load-stone, that it will not attract, but rather seem to repel or drive away that end of the magnetick needle, which was drawn by the point of the other knife. And this improbable experiment not only we have made trial of, by passing slender irons upon the extremities of armed load-stones, the breadth of whose steel caps may make the experiment somewhat less strange; but we have likewise tried it by affrictions of such irons upon the pole of a naked terella, and we found it to succeed there likewise: how strange soever it may seem, that the same point or part of the load-stone should imbue iron with contrary properties, barely as they are, during their passing over it, drawn from the æquator of

of the load-ſtone, or thruſt towards it. But whether, or how far this obſervation inſinuates the operation of the load-ſtone to be chiefly performed by ſtreams of ſmall particles, which perpetually iſſuing out of one of its poles, do wheel about and re-enter at the other; we ſhall not now examine, (though this ſeem one of the moſt likely phænomena we have met with, to hint a probable magnetical hypotheſis) contenting ourſelves to have manifeſted, by what plainly appears, how much influence a circumſtance, which none but a magnetick philoſopher would take notice of, may have on an experiment. We have alſo, with pleaſure, obſerved, how artificers in the tempering of ſteel, by holding it but a minute or two longer or leſſer in the flame (or other competent heat) do give it very differing tempers, as to brittleneſs or toughneſs, hardneſs or ſoftneſs: for as when it is taken out of the flame to be extinguiſhed, it looks either red, yellow or blue; ſo they eſteem and find it fit to make knives, engraving tools, or ſprings for watches, *&c.* and yet it paſſes from one colour to another ſo ſwiftly, that none but an artiſt expert in tempering of iron would ſuſpect, that ſo ſmall a difference of time of its ſtay in the flame could produce ſo great a difference in its tempers. On which occaſion, *Pyrophilus*, I call to mind, that making a while ſince ſome trials concerning gravers in the ſhop of a famous artificer, he preſented me, as a great rarity, a graver (which I yet keep) that would make the uſual experiments about tempering of gravers appear falſe to him, that ſhould never try them but upon it; for with all the care, wherewith I tried upon it the known ways of ſoftening gravers, I could not ſoften this: which men eminently ſkilled in theſe matters (together with the perſon that made it) affirmed to have been made of *Damaſco*-ſteel, the ſtrength whereof in cutting iron I have (not without ſome wonder) made trial of. But whether this ſingularity, which we have mentioned in this graver, proceeded from the nature of the ſteel, or from the temper, that it had afterward given it, is not yet agreed upon by thoſe ſkilful men, to whom I have ſhewed it: but one of them, who by making inſtruments for navigators, has had the opportunity of making more than ordinary inquiry into matters of this nature, aſſures me, that he can eaſily ſoften this kind of ſteel, by only taking it off the fire at a certain nick of time, differing from that, which is wont to be obſerved in order to the ſoftening of common gravers. And who knows, but that in many other experiments, ſeemingly deſpicable and unheeded circumſtances may be of great concernment, though by reaſon of want of ſuch particular obſervations, as the frequent dealing with the ſame body has given magnetick philoſophers and artificers occaſion to make, men have not yet taken notice of their importance?

To give you one inſtance to this purpoſe, *Pyrophilus*, let me take notice to you, that divers planters of fruit-trees have with wonder obſerved, that ſome grafts of cherry-trees, for example, have borne fruit the ſame year that they were grafted, (nay, I have obſerved ſome plants to bear fruit the ſame quarter of the year) and others not till the year after their inſition, though neither in the goodneſs of the graft, nor in that of the ſtock, they had obſerved any diſparity, to which the difference abovementioned could be aſcribed; and therefore the bearing or not bearing of the cions of a cherry-tree the firſt year of its inſition is by many gardeners looked upon as a thing meerly contingent. And yet indeed it ſcarce deſerves to be reckoned among ſuch contingent experiments, as we have been hitherto treating of; for I am informed by the trials of more than one of the moſt ſkilful and experienced grafters of theſe parts, that a man ſhall ſeldom fail of having cherries borne by his graft the ſame year, in which the inſition is made, if he take care, that his graft, which muſt be of a good kind, have bloſſom-buds, as they are wont to be called, upon it: whereas

whereas if it were only leaf-buds, as they may be termed, it will not bear fruit till the second season. And this not being taken notice of by vulgar gardeners, makes them, as we have said, impute a needless contingency to the fruitfulness of such kind of grafts. Now to discern such buds as are fit to produce blossoms, from such as will display themselves but in leaves, is no difficult matter, the former sort being more full, and big, and round than the latter, which are wont also to lie more flat and close to the graft. And it was, *Pyrophilus*, such observations as this, that induced us, after the beginning of the former essays, to discriminate from such contingent experiments as those, wherein the cause of the contingency is very abstruse and difficult to be discerned, such other experiments, whose seeming contingency proceeds from more easily discoverable causes; for such, by diligent observation of circumstances, may be reduced to a greater certainty than the others seem capable of. Though I dare not deny, that even divers of those contingent experiments, which to us yet seem to belong to the first sort, by men's future skill and diligence in observation, may be made fit to be reduced to the second sort.

BEFORE I leave this subject, *Pyrophilus*, I dare not omit to say something to you of the *Virgula Divina*, or rather *Divinatoria*, by which many mineralists pretend to discover the latent veins of metals. Some use a forked hazel, whose horns they hold by the ends one in each hand; and others content themselves to chuse a hazel rod (which some will have to be all of the same year's shoot) and this they bind on to another streight stick of any other wood, and walking softly with it over those places, where they suspect the bowels of the earth to be enriched with metals, they say, that if they pass over a metaline vein, the wand will, by bowing towards, discover it. And some dealers in metals I know, who affirm, that by holding the metals successively in that hand, wherein a man holds the rod, he may discover what determinate metal is predominant in the vein: for when he puts into his hand that metal, wherewith the mine chiefly abounds, the wand will manifestly bow more strongly, that when it is held in the hand with any other metal. What to determine concerning the truth of this perplexing experiment, I confess I know not. For *Agricola* himself, after a long debate concerning it, gives us this account of his sense; *Metallicus igitur* (says he) *quia eum virum bonum & gravem esse volumus, virgulâ incantatâ non utetur, quia rerum naturæ peritum & prudentem, furcatam sibi usui non esse, sed, ut supra dixi, habet naturalia venarum signa, quæ observat.* The diligent *Kircherus* informs us in his *Arte Magneticâ*, that having exactly tried the experiment with metals, (for he mentions not his having tried it with mines) he could not find it in any measure succeed; and we ourselves having several times made trial of it in the presence of the confidentest assertors of the truth of it, could not satisfy ourselves, that the wand did really stand either to the metals, when placed under it, or to the metalline veins, when we carried it over mines, whence metalline ore was at that very time digging out. But on the other side, divers good authors, and even our diligent country-man *Gabriel Plat*, though wont to be somewhat too severe to chymists, does ascribe very much to this detecting wand; and divers persons, in other things very far from credulous, have as eye witnesses with great asseverations asserted the truth of the experiment before us: and one gentleman, who lives near the lead-mines in *Somersetshire*, leading me over those parts of the mines, where we knew that metalline veins did run, made me take notice of the stooping of the wand, when he passed over a vein of ore, and protested, that the motion of his hand did not at all contribute to the inclination of the rod, but that sometimes, when he held it very fast, it would bend so strongly as to break in his hand. And to convince me, that he believed himself, he did, upon the promises made

De re metallica, lib. 10. p. 28.

Lib. 3. part 4. cap. 3.

made him by his stooping wand, put himself to the great charge of digging in untried places for mines, (but with what success he has not yet informed me.) Among the miners themselves I found some made use of this wand, and others laughed at it. And this I must take notice of, as peculiar to this experiment, that the most knowing patrons of it confess, that in some men's hands it will not at all succeed, some hidden property in him that uses the wand being able, as they say, to overpower and hinder its inclinatory virtue. To which I must add what a very famous chymist, who affirms himself to have tried many other things with it besides those that are commonly known, very solemnly professed to me upon his own knowledge; namely, that in the hands of those very persons, in whose hands the rod will (as they speak) work, there are certain unlucky hours, governed by such planets and constellations, (which I confess I believed not enough to remember their names) during which it will not work, even in those hands, wherein at other times it manifestly will. But of this experiment I must content myself to say, what I am wont to do, when my opinion is asked of those things, which I dare not peremptorily reject, and yet am not convinced of; namely, that they that have seen them can much more reasonably believe them, than they that have not.

NOR is it only in experiments, *Pyrophilus*, but in observations also, that much of contingency may be: witness the great variety in the number, magnitude, position, figure, *&c.* of the parts taken notice of by anatomical writers in their dissections of that one subject the human body, about which many errors would have been delivered by anatomists, if the frequency of dissections had not enabled them to discern betwixt those things, that are generally and uniformly found in dissected bodies, and those which are but rarely, and (if I may so speak) through some wantonness, or other deviation of nature, to be met with. I remember, that a while since being present at the dissection of a lusty young thief, we had opportunity to observe, among other things, that the interval betwixt two of his ribs was near the back-bone filled up with a thick bony substance, which seemed to be but an expansion of the ribs, and appeared not to have grown there upon occasion of any fracture, or other mischance. About the same time being at a private dissection of a large and young human body with some learned men, an ingenious person, professor of anatomy, there present, chancing to cut a great nerve, spied in the substance of it a little of a very red liquor, which he immediately shewed me, as wondering what it might be: but I concluding it to be blood, presently suspected that it might have proceeded from some small unheeded drop of blood wiped off by the brushy substance of the nerve from the knife wherewith it was cut. Wherefore carefully wiping a dissecting knife, I did in another place cut the nerve asunder, and found another very little drop of pure blood in the substance of it as before. This I did again elsewhere with like success, shewing it to the by-standers, who admired to see a vessel carrying blood (for such they concluded it to be) in the body of a nerve, in regard they remembered not to have ever met with such an accident; though I the less admire it, because I have in an ox's eye or two observed in that coat, which the moderns commonly call the retina, and which seems to be but an expansion of the pith of the optick nerve, little turgent veins manifestly full of blood.

WE further observed in that lately mentioned body, in which we took notice of the irregular conjunction of two ribs, that the lungs, which were very sound, had a supernumerary lobe on one side, which did so little differ from its companions, that we did not, till we had displayed the lungs, take notice of it. And I remember, that a while before, being invited by a company of physicians to a private dissection, and the

the lungs, which otherwise seemed not unsound, appearing in divers places fastened to the ribs, two ingenious anatomists, that were there present, did so little agree in their observations concerning such cases, that the one affirmed, that he had never seen any lungs (which had not been excessively morbid) tied to the thorax; and the other protested, that he had scarce ever opened a diseased body, wherein the lungs did not so adhere. But if it were not improper to mind a young gentleman of venereal observations, I could easily give you an eminent proof of the disagreement of anatomical observators, by insisting on the controversy betwixt the famous writers on that subject, concerning the anatomical notes or tokens of virginity; many eminent authors affirming, that they have seldom failed of finding them in one amongst very many dissected maids; and many other artists, both conspicuous and experienced, preremptorily professing, that they have seldom or never met with the pretended marks in persons even of the most undoubted virginity. And certainly it is very strange, that about a matter, which seems so easily determinable by sense, accurate and sober men should so widely disagree; as that the one should profess he has very rarely, if ever, met with in a human body, what another affirmeth himself to have as seldom, if ever, missed. But because, *Pyrophilus*, this subject is, perhaps, somewhat improper to be insisted on either to, or by, a young man, I shall pass on to tell you, that amongst the accurateſt of our modern writers, I suppose you will readily allow me to reckon Dr. *Harvey* and Dr. *Highmore*; and that though in their excellent treatises of generation they both insist on the production and changes observable in hens eggs, as the patterns, whereunto the generation of other annimals may be referred; yet have we divers times, in the progress of nature in her formation of a chick, observed considerable variations in point of time and other circumstances (though in the main our observations commonly agreed) from what is by them delivered: which diversity may easily proceed from the different constitution of hens, their differing assiduity in sitting on their eggs, the differing qualifications of the eggs themselves, and several other particulars of the like nature. And I remember, that the other day taking notice of this to my learned friend Dr. *Highmore*, he readily acknowledged to me, that he himself had likewise observed divers circumstances in eggs whilst they were hatching, which varied from those set down by him in his book; though he had there accurately expressed the changes he discerned in those eggs, which at the same time afforded him his observations. And indeed there are certain things of such a nature, that scarce any single man's accurateness in making a single observation about them can secure him from appearing unskilful or unfaithful in his observations, unless those, that shall afterwards examine them, chance to be endowed with a somewhat more than ordinary either equity, or sagacity, or both. For instance, he that first affirmed, that a needle animated by a loadstone did constantly convert its extremes to the opposite poles of the earth, could scarce suspect himself of having delivered any thing, which he had not carefully tried. And yet of those pilots, *Gonzales Oviedo* and *Sebastian Cabot*, (who are said to have in *America* first taken notice of the declination of the mariner's needle) he that did first in those far distant parts of the world compare the meridian line afforded by magnetical needles with one mathematically drawn, (which may be readily found by accurate sun dials) and thereby observe the variation of the needle, or its declination from the true meridian line, might easily conclude the observer formerly mentioned to have been faulty, by reason of his finding the needle's variation differing (perhaps by divers degrees) from that delivered by the first observer. And this second man's observation might appear to have been as carelesly made to a hundred other observers, if

if the obſervations of navigators had not made it apparent, that the declination of the needle is far from being the ſame in all places: for though *Cardan* (as *Kircher* and *Fracaſtorius*, as another informs us) be pleaſed to affirm, that the loadſtone declines as many degrees, as the pole-ſtar is diſtant from the pole of the world; yet beſides divers reaſons, common experience ſufficiently manifeſts the inconſiderableneſs (not to ſpeak more harſhly) of that aſſertion. For about the iſlands of the *Azores*, eſpecially that of *Corvo*, over which the firſt meridian is by many ſuppoſed to paſs, the magnetick needle hath been obſerved directly to reſpect the poles, without any ſenſible declination from them; but in other places it is wont to vary ſometimes eaſtward, ſometimes weſtward, more or leſs. Inſomuch that not only our venturous countryman Captain *Thomas James* obſerved it in 63 degrees north-latitude to be no leſs than 27 degrees, 48 minutes; but a learned mathematical writer, that is lately come forth, makes the declination at the *Fretum Davis* to amount to, what is almoſt incredible, 50 degrees. And this deflexion of the needle ſometimes to one ſide of the meridian, ſometime to the other, happens with ſo much ſeeming irregularity, as has made both the diligent *Kircher* himſelf, and divers other magnetick writers, almoſt deſpair of reducing theſe kind of obſervations to any general hypotheſis.

Fournier Hydr. l. 11. c. 11.

In the Table annexed to his voyage.

To which we may add, that perhaps very few even of the exacteſt obſervations of this nature made an age ſince, would now appear accurate to them, that ſhould try them in the ſelf-ſame places wherein, and the ſelf-ſame manner after which they were formerly made. So that the diligenteſt of thoſe obſervers would appear to us to have been negligent, if the ſagacity of ſome of their ſucceeders had not prompted them to ſuſpect, that even in the ſame place the needle's variation may vary. And I remember, that having not long ſince enquired of an Engliſh contriver of mathematical inſtruments for the uſe of ſeamen, what he had obſerved concerning this alteration of the needle's variation, he told me, that by comparing of ancient and modern obſervations made by himſelf and other accurate mathematicians at *London*, he had found the declination conſtantly to decreaſe, and, as he conjectured, about 12 or 13 minutes (though that methinks be much) in a year. And it will be yet more difficult to ſet down any obſervation of this nature, which will appear exact to poſterity, if that ſtrange thing be true (as it may well be) which was related to *Kircher* by a friend of his, who affirms himſelf to have obſerved a notable change of the needle's variation at *Naples*, after a great *incendium* of the neighbouring mountain *Veſuvius*; which alteration he not abſurdly ſuſpects to have proceeded from the very great change made in the neighbouring ſubterraneal parts by that great conflagration. And it ſeems the ſame obſervation has been taken notice of by mathematicians elſewhere. For the learned Jeſuit *Fournier* in his French hydrography tells us in more general terms, that ſince the *incendiums* of *Veſuvius* the declination (of the needle) has notably changed in the kingdom of *Naples*. The ſame author ſomewhere delivers what (if it be true) is remarkable to our preſent purpoſe, in theſe words: 'There are perſons, who have obſerved, that the ſame needle, that declined 5 'degrees upon the ſurface of the earth, being carried down very low into certain 'caves, declined quite otherwiſe.' I added thoſe words, *if it be true*, not to queſtion the veracity of the author, but becauſe it is very poſſible the makers of the obſervation (though learned men) may have been miſtaken in it, without ſuſpecting themſelves in danger of being ſo. For I ſhould ſcarce have imagined, unleſs my own particular obſervation had informed me, in how great a variety of ſtones and other foſſiles the ore of iron may lurk diſguiſed: ſo that it is no way incredible, that knowing chymiſts themſelves, and much more mathematicians and others, not being

Liv. 11. chap. 10.

aware of the obſervation of what I have newly delivered, may preſume, becauſe they ſaw not in the deep caves abovementioned any minerals like the vulgar iron ore, that there is nothing of that metal there, when indeed there may be enough to occaſion that deflexion of the needle; which (eſpecially if it be ſtrongly excited) may be often drawn aſide by iron or other magnetick bodies, at a greater diſtance than thoſe, that have not tried, will be apt to ſuſpect. Which may perhaps be the reaſon, why in the little iſland of *Ilva* (upon the coaſt of *Italy*) where they dig up iron and ſtore of load-ſtones, of which I have ſeen in *Tuſcany* of a prodigious bigneſs, there is indifferent, but neighbouring places, ſuch a ſtrange diſparity of the needle's variation as curious men have recorded.

NOR are magnetical and anatomical obſervations the only ones, which are ſubject to diſagree now and then, without the negligence of thoſe that make them: but I want time, and I fear you would want patience, to conſider at preſent as many of them, as might be eaſily enumerated to you.

I SUPPOSE, *Pyrophilus*, you may have obſerved, how I in the paſt diſcourſe have forborn to inſiſt on medicinal experiments; which I have purpoſely done, becauſe they are ſo many, and almoſt all of them ſubject to ſuch uncertainties, that to inſiſt on them would require much more time, than my occaſions will allow me to ſpend upon this eſſay. And indeed in phyſick it is much more difficult than moſt men can imagine, to make an accurate experiment: for oftentimes the ſame diſeaſe proceeding in ſeveral perſons from quite differing cauſes, will be increaſed in one by the ſame remedy by which it has been cured in another. And not only the conſtitutions of patients may as much alter the effects of remedies, as the cauſes of diſeaſes; but even in the ſame patient, and the ſame diſeaſe, the ſingle circumſtance of time may have almoſt as great an operation upon the ſucceſs of a medicine, as either of the two former particulars; as we may elſewhere have occaſion by ſundry inſtances to manifeſt. But beſides the general uncertainty, to which moſt remedies are ſubject, there are ſome few, that ſeem obnoxious to contingencies of a peculiar nature: ſuch is the ſympathetick powder, of which not only divers phyſicians and other ſober perſons have aſſured me they had ſucceſsfully made trial, but we ourſelves have thought, that we were eye-witneſſes of the operation of it; and yet not only many, that have tried it, have not found it anſwer expectation, but we ourſelves trying ſome of our own preparing on ourſelves, have found it ineffectual, and unable to ſtop ſo much as a bleeding at the noſe; though upon application of it a little before, we had ſeen ſuch a bleeding, though violent, ſuddenly ſtopped in a perſon, who was ſo far from contributing by his imagination to the effect of the powder, that he derided thoſe that he ſaw apply it to ſome of the drops of his blood. Wherefore that the ſympathetick powder and the weapon-ſalve are never of any efficacy at all, I dare not affirm; but that they conſtantly perform what is promiſed of them, I muſt leave others to believe. But making mention of remedies of this nature, though I am willing, *Pyrophilus*, to put a period both to your trouble and my own, yet I muſt not omit to tell you, that whereas the pæony-root has been much commended both by ancient and modern phyſicians of no mean account, as an amulet againſt the falling ſickneſs, and yet has been by many found ineffectual; we have been apt to ſuſpect, that its inefficacy, if it be but infrequent, might poſſibly proceed from its having been unſeaſonably gathered: and when I was laſt in the weſt of *Ireland*, acquainting the eminenteſt of the Galeniſts there with my conjecture, he confirmed me in it, by aſſuring me, that he had often tried the pæony root unſeaſonably gathered without ſucceſs; but having lately gathered it under its proper conſtellation, as they ſpeak, (which is when the decreaſing

decreasing moon passes under *Aries)* and tied the slit root about the necks and arms of his patients, he had freed more than one, whom he named to me, from epileptical fits. Agreeable whereunto I find, that a famous physician of *Grenoble*, Monsieur *des Grands Prez*, in the last of his observations communicated to the famous practical physician *Riverius*, solemnly professes his having divers times freed his patients from the falling-sickness by the single outward application of pæony-roots, collected and applied as is above mentioned. But though he thence infers the usefulness of observing stars in the practice of physic, yet before much weight be laid upon such improbable notions, as most of those of judiciary astrologers, the influence of constellations upon simples, *&c.* ought by severe and competent experiments to be better made out than hitherto it has been.

BUT to say no more of the contingent observations to be taken notice of in trials medical, I could tell you, that I have observed even mathematical writers themselves to deliver such observations as do not regularly hold true. For though it hath been looked upon as their privilege and glory to affirm nothing, but what they can prove by no less than demonstration; and though they used to be more attentive and exact, than most other men in making almost any kind of philosophical observation: yet the certainty and accurateness, which is attributed to what they deliver, must be restrained to what they teach concerning those purely-mathematical disciplines, arithmetic and geometry, where the affections of quantity are abstractedly considered: but we must not expect from mathematicians the same accurateness, when they deliver observations concerning such things, wherein it is not only quantity and figure, but matter, and its other affections, that must be considered. And yet less must this be expected, when they deliver such observations, as, being made by the help of material instruments framed by the hands and tools of men, cannot but in divers cases be subject to some, if not many, imperfections upon their account. Divers of the modern astronomers have so written of the spots and more shining parts or (as they call them) *Faculæ*, that appear upon or about the surface of the sun, as to make their readers presume, that at least some of them are almost always to be seen there. And I am willing to think, that it was their having so often met with such phænomena in the sun, that made them write as they did. And yet when I first applied myself to the contemplation of these late discoveries, though I wanted neither good telescopes, nor a dark room to bring the species of the sun into, yet it was not till after a great while, and a multitude of fruitless observations made at several times, that I could detect any of these solary spots, which having during many months at least appeared so much seldomer than it seems they did before, that I remember a most ingenious professor of astronomy, excellently well furnished with dioptrical glasses, did about that time complain to me, that for I know not how long he had not been able to see the sun spotted. And as for the *Faculæ*, that are written of, as such ordinary phænomena, I must profess to you, *Pyrophilus*, that a multitude of observations made with good telescopes, at several places and times whilst the sun was spotted, has scarce made me see above once any of the looked for brightnesses.

AND as the nature of the material objects, wherewith the mathematician is conversant, may thus deceive the expectations grounded on what he delivers; so may the like happen by reason of the imperfection of the instruments, which he must make use of in the sensible observations, whereon the mixed mathematics (as astronomy, geography, optics, *&c.*) are in great part built. This is but too manifest in the disagreeing supputations, that famous writers, as well modern as antient, have given us of the circuit of the terrestrial globe, of the distance and bigness of the fixed stars

and some of the planets, nay, and of the height of mountains: which disagreement, as it may oftentimes proceed from the differing method and unequal skill of the several observers, so it may in divers cases be imputed to the greater or less exactness and manageableness of the instruments employed by them. And on this occasion I cannot omit that sober confession and advertisement, that I met with in the noble *Tycho*, who having laid out, besides his time and industry, much greater sums of money on instruments than any man we have heard of in latter times, deserves to be listened to on this theme, concerning which he has (among other things) the following passage: *Facilè* (says he) *lapsus aliquis penè insensibilis in instrumentis etiam majoribus conficiendis subrepit, qui inter observandum aliquot scrupulorum primorum jacturam faciat; insuper si ipse situs & tractandi modus non tam absoluta norma perficiatur, ut nihil prorsus desideretur, intolerabilis nec facilè animadvertenda deviatio sese insinuat. Adde quod instrumenta usu & ætate à prima perfectione degenerent. Nihil enim, quod hominum manibus paratur, ab omni mutatione undiquaque existit. Organa enim ejuscemodi, nisi è solido metallo affabre elaborentur, mutationi aëreæ obnoxia sunt; & si id quoque detur, ut è metallica materia constent, nisi ingentia fuerint, divisiones minutissimas graduum non sufficientur exhibent; dumque hoc præstant, sua magnitudine & pondere se ipsa ita aggravant, ut facile tum extra planum debitum aut figuram competentem, dum circumducuntur, declinent, tum etiam sua mole intractabilia redduntur. Quare magis requiritur in instrumentis astronomicis, quæ omni vitio careant construendis, artificium pari judicio conjunctum, quam hactenus à quamplurimis animadversum est. Id quod nos ipse usus longaque docuit experientia, non parvo labore nec mediocribus sumptibus comparata.*

Tycho Brahe, *l.* 2. *de Cometâ An.* 1577. *p.* 153.

HITHERTO our noble author. And as for the observations made at sea, the diligent *Fournier* advertises, that however many sea-captains and others may brag of their mathematical observations made on ship-board, yet he, upon trial of many instruments both at sea and ashore, makes bold to affirm, that no astronomer in the world can be sure to make his observation at sea within ten minutes of the precise truth, no not (says he) upon the sand itself, within one minute of it.

BUT instead of acquainting you with what may be drawn from the writings of our hydrographer, to prove, that his assertion is rather modest than too bold, I shall observe, that the observations even of skilful mathematicians may hold so little, or disagree so much, when they pretend to give us the determinate measures of things, that I remember of three very eminent modern mathematicians, who have taken upon them, by their experiments, to determine the proportion betwixt air and water, the one makes not the weight of water to exceed above 150 times that of air; the other reckons water to be between 13 and 14 hundred times; and the third no less than 10,000 times the heavier. Not to mention a modern and famous writer or two, who have been so mistaken as to think, that the weight of the water in comparison of the air is I know not how much under-reckoned, even by this last (overbold) estimate. And, if I had leisure, I could annex an experiment partly statical, and relating to the weight of the air, which though we made divers times in an hour, yet we missed of the like success twice as often in the same hour, without being able to know beforehand, whether the experiment would succeed within some pounds weight. But of this more perhaps elsewhere.

THE ends, *Pyrophilus*, which we have proposed to ourselves in setting down the things by us delivered in this and the former essay, are principally two.

AND first, we desire, that the instances we have given you of the contingency of experiments, may make you think yourself obliged to try those experiments very carefully, and more than once, upon which you mean to build considerable superstructures.

structures either theorical or practical; and to think it unsafe to rely too much upon single experiments, especially when you have to deal in minerals: for many to their ruin have found, that what they at first looked upon as a happy mineral experiment, has proved in the issue the most unfortunate they ever made. And I remember, that the most experienced mineralist I have hitherto been acquainted with, though his skill has been rather gainful then prejudicial to him, has very seriously told me, that he could quickly grow an extraordinary rich man, if he could but do constantly whatsoever he has done, not only two or three, but many times.

THE other end, *Pyrophilus*, to which I had an eye in writing the past discourses, was, that they may serve for a kind of apology for sober and experimental writers, in case you should not always upon trial find the experiments or observations by them delivered answer your expectations. And indeed it would prove a great discouragement to wary and considerate naturalists from enriching the world with their observations, if they should find, that their faithfulness in setting down what they observed is not able to protect them from blasting imputations of falshood, but that by publishing any thing for the good of others, they must expose their reputation to all the uncertainties, to which any of their experiments may chance to prove obnoxious. It is true indeed, that if a writer be wont to be fabulous or transcriptive, and to deliver things confidently by hear-say, without telling his readers when he does so; if his experiments upon trial succeed not, we may be allowed to impute their unsuccessfulness rather to him, than to ourselves, or to chance, and need not think ourselves obliged to have so much a greater care of his reputation, than he had of his own, as for his sake to try more than once, what he for our sakes never tried so much as once. But if an author, that is wont to deliver things upon his own knowledge, and shews himself careful not to be deceived, and unwilling to deceive his readers, shall deliver any thing, as having tried or seen it, which yet agrees not with our trials of it; I think it but a piece of equity, becoming both a Christian and a philosopher, to think (unless we have some manifest reason to the contrary) that he set down his experiment or observation as he made it, though for some latent reason it does not constantly hold; and that therefore though his experiment be not to be relied upon, yet his sincerity is not to be rejected. Nay, if the author be such an one, as has intentionally and really deserved well of mankind, for my part I can be so grateful to him, as not only to forbear to distrust his veracity, as if he had not done or seen what he says he did or saw, but to forbear to reject his experiments, till I have tried, whether or no by some change of circumstances they may not be brought to succeed. Thus a while since finding in Sir *Francis Bacon*, that he delivers as a somewhat unlikely truth, that spirit of wine will swim upon oil (of almonds) we forthwith made trial of it, but found the oil swim upon the spirit of wine, and this upon several trials before witnesses: but our tenderness of the reputation of so great and so candid a philosopher made us to bethink ourselves, that (though he mentions it not, nor perhaps thought of any such thing, yet) possibly he may have used spirit of wine more pure than ordinary; and thereupon having provided some that was well rectified, we found, that the oil, that was wont to swim upon spirit of wine, not freed from its aqueous parts, did readily sink, and quietly lie in the bottom of that, which was carefully dephlegmed. And so having been informed, that the learned Dr. *Brown* somewhere delivers, that aqua fortis will quickly coagulate common oil, we poured some of those liquors together, and let them stand for a considerable space of time in an open vessel, without finding in the oil the change by him promised (though we have more than once with another liquor presently thickened common oil). Whereupon being

being unwilling, that so faithful and candid a naturalist should appear fit to be distrusted, we did again make the trial with fresh oil and aqua fortis in a long-necked phial left open at the top, which we kept both in a cool place, and after in a digesting furnace; but after some weeks we found no other alteration in the oil, than that it had acquired a high and lovely tincture: notwithstanding which, being still concerned for the reputation of a person, that so well deserves a good one, the like contingencies we have formerly met with in other experiments, made us willing to try, whether or no the unsuccessfulness we have related might not proceed from some peculiar though latent quality, either in the aqua fortis or the oil by us formerly employed: whereupon changing those liquors, and repeating the experiment, we found after some hours the oil coagulated almost into the form of a whitish butter. Nor dare I allow myself to be confident, that I shall not need to be dealt with by you upon some occasions, with the like equity, that I have been careful to express towards others. And since the writing of thus much of this very essay, having desired a a very skilful and candid chymist to do me the favour to provide me some of the purest and strongest spirit of salt, that could be made; he kept some salt in a vehement fire for divers days and nights together, and freed the extracted liquor so carefully and so skilfully both from its phlegm and its terrestrial fæces, that after all I have written in the former essay concerning that menstruum, I must freely confess to you, that I am now satisfied, that a spirit of sea salt may without any unsincerity be so prepared, as to dissolve the body of crude gold, though I could not find, that the solutions I made of that metal were red, but rather of a yellow or golden colour, much like those made with common aqua regis. But neither this artist nor I have been since able to make another spirit of salt capable of dissolving gold, notwithstanding all the industry we have employed about it; which makes me refer this to contingent experiments; unless the prosperous event of our former trial may be ascribed to the quality of the salt, that was distilled, which was brought from the island of *Mayo*, where the scorching sun makes out of the sea-water a salt, that is accounted much stronger and more spirituous than that, which is wont to be made in *France* and other more temperate climates. And let me, *Pyrophilus*, take this opportunity to add, that if I had not very cautiously set down the observation I related in another essay * concerning the little fishes or worms I there teach you to discover in vinegar, I should perhaps need much of your equity, to keep me from being thought to have imposed upon you in what I there delivered. For I have since met with divers parcels of vinegar, wherein the observation could not be made, for one wherein it held; so that I am glad to keep by me some vinegar stocked with those scarce visible animals, to satisfy ingenious men, among whom some have been fain, after their own fruitless trials, to come to me to show them the things delivered in that observation. What I mentioned a little above to have been tried upon sallet-oil, puts me in mind of telling you, that among our experiments concerning the changes of colours, we were about to acquaint you with one, which we had formerly made upon common oil-olive, it seeming to us a not inconsiderable one; since it was a way, that we devised of instantly changing the colour of the oil from a pale yellow to a deep red, with a few drops of a liquor, that was not red, but almost colourless. This experiment, as we were saying, *Pyrophilus*, we were about to set down among others concerning colours: but because we do not willingly rely on a single trial of such things, as we know not to have been ever tried before, we thought it not amiss for greater security to make the experiment

* This is one of those, that make up the book of the usefulness of *Experimental Philosophy*.

the

the second time, but could not then find it to succeed, nor even since upon a new trial (probably by reason of some peculiar quality in that particular parcel of liquor we first made use of) which made us think fit to omit the intended mention of it: but if I had upon my first trial acquainted you with it without any further scruple, you might upon trial have suspected, if not concluded, that I had misinformed you, though I had really delivered nothing but what I had tried. And indeed, *Pyrophilus*, though I have not the vanity to pretend to have deserved so much of you, as such naturalists as Sir *Francis Bacon* have deserved from every ingenious reader of their books; yet perhaps you will do me but right to believe, that though some of the experiments I have delivered may prove contingent, yet I have not delivered them unfaithfully, in reference to what I thought I observed in them, and remembered of them. And though I desire you should so read my writings, as to give no farther assent to my opinions, than the reasons or experiments produced on their behalf require; yet in matters of fact, which I deliver as having tried or seen them, I am very willing you should think, that I may have had the weakness to be mistaken, but not an intention to deceive you.

THERE is yet one thing more, that I shall venture to acquaint you with before I conclude this essay, though you may think it relishes of a paradox, and it is this: that when I am satisfied of the abilities and circumspection of a writer, delivering a matter of fact as upon his own knowledge; I do not presently reject his observation as untrue, much less condemn the person himself as a lyar, whensoever I find, that it seems to be contradicted by a contrary and more undoubted observation, or to contradict a received and plausible either hypothesis or tradition; but rather try, if by fit distinction or limitation I can reconcile them; unless I can imagine something or other, which might probably lead him to mistake. And of this indulgence to an intelligent writer I have this reason to give, that sometimes there happen irregularities contrary to the usual course of things, as is evident in monsters; and sometimes the received hypothesis, though perhaps not to be rejected as to the main, will not hold so universally as men presume; and sometimes too the contradiction betwixt the observations may be but seeming (by reason of the want of some unheeded circumstance necessary to make them inconsistent) and so they may both be true.

WE might dilucidate and confirm what we have newly delivered by several instances, were it not, that this essay is already but too prolix. Wherefore we shall only recommend to your consideration these few particulars.

THAT the Irish spiders (of which, whatever is vulgarly believed to the contrary, myself have in *Ireland* seen divers) are not poisonous, is not doubted by the inhabitants, who have had many ages experience of their harmlessness: and yet I dare not deny what the learned *Scaliger* somewhere affirms, that in (his country, if I mis-remember not) *Gascony* their venom is so pernicious, that they sometimes poison those that tread upon them through the very soles of their shoes. And that even here in *England* (though a country so near to *Ireland*) some spiders (at least) are venomous even without biting, I may elsewhere have occasion to give you an experimental proof.

It is so much taken for granted by divers authors, who pretend likewise to give reasons of it, and by the generality of their readers, that under the same meridian the magnetic needle keeps every where the same variation, without changing it by being carried northwards or southwards, that it is like, if many persons better acquainted with magnetic speculations than trials should read in the relations of the Hollanders, that under the meridian, that passes by the island of *Corvo*, where the needle

needle points directly at the poles, and which is therefore wont to be reckoned the first meridian, they found at two places, the one about 46, the other about 55 degrees of northern latitude, a declination in the former of those elevations of no less than 7 or 8 degrees, and in the latter of a far greater number; and also that they found under the twentieth parallel of southern latitude under the same meridian of the *Azores* 10 or 11 degrees of declination; many, I say, if they should meet with these particulars, probably would suppose the Dutch to have been very bad observers, because these observations do not (as we intimated above) agree with the theory of the needle's declination. And yet if we confer these observations with others of the like nature, made by good navigators and other skilful men along other meridians, we may, I suppose, find cause rather to rectify the general opinion, than reject the Dutch observations for their disagreeing with it; especially if we take into consideration what is affirmed by the Jesuit *Jules Alenis* (whom *Fournier*, amply treating of longitudes, extols for the accurateſt observer of the needle's variation that ever was) sailing into *China* in a great Portugal carraque, and accompanied by the famous pilot *Vincent Rodrique*, who had then made twenty-eight voyages to the *Indies*. For out of one of this father's letters *Fournier* has preserved this memorable passage: 'You must '(says he) take notice of one thing very considerable, namely, that the further you 'go from the æquator in the same meridian, the greater you will find the magnetical 'variation.' There are some eminent modern naturalists, who affirm, that they have assuredly tried by weather-glasses, that cellars and other subterraneal places are colder in winter than in summer: and yet not to oppose to this experiment the common tradition to the contrary, I remember, that the bold and industrious Capt. *James* (formerly mentioned) in the relation of his strange voyage published by his late Majesty's command, has this notable observation, where he relates the excessive coldness of the water they met with in summer in that icy region, where they were forced to winter in the year 1632. 'Moreover our well (says he) out of which we had 'water in *December*, had none in *July*.'

De la Longitude, c. 14.

LASTLY, though in the western parts it have been observed, that generally the inside, or heart, as they call it, of trees, is harder than the outward parts; yet an author, very well versed in such matters, treating of the building of ships, gives it us for a very important advertisement touching that matter, that they have observed at *Marseilles*. and all along the Levantine shores, that that part of the wood, that is next the bark, is stronger than that, which makes the heart of the tree. But to draw at length to a conclusion of this already too tedious essay; the ends above mentioned, *Pyrophilus*, being those, which I proposed to myself in writing the past discourse, you will make an use of it, which I was very far from intending you should, if you suffer it to discourage you from the vigorous prosecution of your inquiries into experimental knowledge. Nor indeed is any thing, that hath been said, fit to persuade you to other than watchfulness in observing experiments, and wariness in relying on them; but not at all to such a despondency of mind, as may make you forbear the prosecution of them: for neither doth the physician renounce his profession, because divers of the patients he strives to cure are not freed from their diseases by his medicines, but by death; nor doth the painful husbandman forsake his cultivating of the ground, though sometimes an unseasonable storm or flood spoils his harvest, and deprives him of the expected fruit of his long toils. For as in physic and husbandry, those, that exercise them, are kept from deserting their professions, by finding, that though they sometimes miss of their ends, yet they oftentimes attain them, and are by their successes requited not only for those endeavours that succeed,

Fourn. Architecture Navale, c. 22.

but for those that were lost; so ought we not by the contingencies incident to experimental attempts, to be deterred from making them, because not only there are many experiments scarce ever obnoxious to casualties, but even among those, whose event is not so certain, you may very probably make an experiment very often, without meeting with any of those unlucky accidents, which have the power to make such experiments miscarry. And sure the prosperous success of many succeeding attempts is well able to make amends for the fruitless pains employed on those few, that succeed not; especially since in experiments it not frequently happens, that even when we find not what we seek, we find something as well worth seeking as what we missed. Of this last mentioned truth we may elsewhere have occasion to discourse more largely; and therefore shall now conclude with barely minding you, that even merchants themselves are not wont to quit their profession, because now and then they lose a vessel at sea, and oft times their ships are by contrary winds and other accidents forced to put in at other ports than those they were bound for. Which example I the rather make use of, because that as the American navigators employed by the European merchants, having been by storms forced from their intended course, have been sometimes thereby driven upon unknown coasts, and have made discovery of new regions much more advantageous to them, than the fairest and constantest winds and weather could have been; so in philosophical trials, those unexpected accidents, that defeat our endeavours, do sometimes cast us upon new discoveries of much greater advantage, than the wonted and expected success of the attempted experiment would have proved to us.

SOME

SPECIMENS

OF

An Attempt to make CHYMICAL EXPERIMENTS useful to illustrate the notions of the CORPUSCULAR PHILOSOPHY.

The PREFACE,

Giving an account of the two following treatises, and proposing the desirableness of a good intelligence betwixt the Corpuscularian Philosophers and the Chymists.

THERE are many learned men, who being acquainted with chymistry but by report, have from the illiterateness, the arrogance and the impostures of two many of those, that pretend skill in it, taken occasion to entertain so ill an opinion, as well of the art, as of those that profess it, that they are apt to repine, when they see any person, capable of succeeding in the study of solid philosophy, addict himself to an art they judge so much below a philosopher, and so unserviceable to him: nay, there are some, that are troubled when they see a man, acquainted with other learning, countenance by his example sooty empirics, and a study, which they scarce think fit for any but such, as are unfit for the rational and useful parts of physiology. I now take notice of these things, because they gave occasion to the two following treatises. For perceiving divers years ago, that some learned men of the temper above described thought it strange (if not amiss also) that one, of whose studies they were pleased to have too favourable an expectation, should spend upon chymical trials (to which I then happened to be invited by the opportunity of some furnaces and leisure) much of those endeavours, which they seemed to think might be far more usefully employed, than upon such an empty and deceitful study: perceiving this, I say, I thought it not amiss to endeavour to manifest, that without seeking after the elixir, that alchymists generally hope and toil for (but which they, that knew me, knew to be not at all in my aim) I did not in the prosecution of chymical trials do any thing either without an end, or unsuitable to the design I had of attempting to promote men's knowledge of the works of nature, as well as their power over them. In order to this, I did not think it enough to shew, that by an insight into chymistry one may be enabled to make some meliorations (I speak not of * transmutations) of mineral and metalline bodies, and many excellent medicines for the health of men, besides divers other preparations of good use in particular trades, and in several oc-

* See the *Essay of the Usefulness of Chymistry* in Sect. II. Part I. *Of the Usefulness of E. P.*

currences

currences of human life; I did not, I say, think it enough to do this, because, that though this might suffice to evince that a rational man might without losing his time employ some of it to understand and promote chymistry; yet this would scarce suffice to manifest it to be useful to philosophy. And therefore there seemed requisite some specimens, which might shew that chymical experiments might be very assistant even to the speculative naturalist in his contemplations and inquiries.

But against my attempting any thing of this nature, three difficulties opposed themselves. The first was the want of leisure, in regard I was already pre-engaged to write of other subjects, and to prosecute some experiments, whose event I was concerned to know. Another impediment was, that for other reasons elsewhere mentioned, and chiefly to keep my judgment as unprepossessed as might be with any of the modern theories of philosophy, till I were provided of experiments to help me to judge of them; I had purposely refrained from acquainting myself thoroughly with the intire system of either the Atomical, or the Cartesian, or any other whether new or revived philosophy; and therefore I could scarce be fit to shew, how chymical experiments might illustrate their doctrines. And thirdly, some of those learned men, for whom I was to write, more favouring the Epicurean, and others (though but a few) being more inclinable to the Cartesian opinions, it seemed very difficult to gratify by the same composures persons of differing persuasions.

But as to the first of these discouragements, since my pre-engagements to other themes were not unknown to those, for whom I was to write, it might reasonably be presumed they would over-look such unaccurateness as should appear imputable to haste: and besides, some such subject might be chosen to write of, as would conveniently admit enlargements and additions, according as my leisure should afterwards serve me to annex them.

On occasion of the second impediment, I remembered, that having divers years before read the lives of the Atomical, among other philosophers, in *Diogenes Laertius*; and having sometimes occasionally heard mention made of divers Epicurean and Cartesian notions, and having hence framed to myself some general, though but imperfect, idea of the way of philosophizing my friends esteemed; I thought I might, without a more particular explicit inquiry into it, say something to illustrate some notions of it, by making choice of such, as, being of the more simple and obvious, did not require skill in the more mysterious points of the hypothesis they belonged to.

And as for the last of the three discouragements abovementioned, I considered, that the Atomical and Cartesian hypotheses, though they differed in some material points from one another, yet in opposition to the Peripatetic and other vulgar doctrines they might be looked upon as one philosophy: for they agree with one another, and differ from the schools in this grand and fundamental point, that not only they take care to explicate things intelligibly; but that whereas those other philosophers give only a general and superficial account of the phænomena of nature from certain substantial forms, which the most ingenious among themselves confess to be incomprehensible, and certain real qualities, which knowing men of other persuasions think to be likewise unintelligible; both the Cartesians and the Atomists explicate the same phænomena by little bodies variously figured and moved. I know, that these two sects of modern naturalists disagree about the notion of body in general, and consequently about the possibility of a true vacuum; as also about the origin of motion, the indefinite divisibleness of matter, and some other points of less importance than these: but in regard that some of them seem to be rather metaphysical than physiological notions, and that some others seem rather to be requisite to the explication of

the first origin of the universe, than of the phænomena of it, in the state wherein we now find it; in regard of these, I say, and some other considerations, and especially for this reason, that both parties agree in deducing all the phænomena of nature from matter and local motion; I esteemed that, notwithstanding these things, wherein the Atomists and the Cartesians differed, they might be thought to agree in the main, and their hypotheses might by a person of a reconciling disposition be looked on as, upon the matter, one philosophy. Which because it explicates things by corpuscles, or minute bodies, may (not very unfitly) be called corpuscular; though I sometimes style it the Phænician philosophy, because some antient writers inform us, that not only before *Epicurus* and *Democritus*, but even before *Leucippus* taught in *Greece*, a Phænician naturalist was wont to give an account of the phænomena of nature by the motion and other affections of the minute particles of matter. Which because they are obvious and very powerful in mechanical engines, I sometimes also term it the mechanical hypothesis or philosophy.

By such considerations then, and by this occasion, I was invited to try, whether, without pretending to determine the abovementioned controverted points, I could, by the help of the corpuscular philosophy, in the sense newly given of that appellation, associated with chymical experiments, explicate some particular subjects more intelligibly, than they are wont to be accounted for, either by the schools or the chymists. And however since the vulgar philosophy is yet so vulgar, that it is still in great request with the generality of scholars; and since the mechanical philosophers have brought so few experiments to verify their assertions: and the chymists are thought to have brought so many on the behalf of theirs, that of those, that have quitted the unsatisfactory philosophy of the schools, the greater number, dazzled as it were by the experiments of Spagyrists, have imbraced their doctrines instead of those they deserted: for these reasons, I say, I hoped I might at least do no unseasonable piece of service to the corpuscular philosophers, by illustrating some of their notions with sensible experiments, and manifesting, that the things by me treated of may be at least plausibly explicated without having recourse to inexplicable forms, real qualities, the four peripatetic elements, or so much as the three chymical principles.

Being once resolved to write some such *specimina* as I formerly judged requisite, I soon bethought myself of the experiment hereafter delivered concerning salt-petre, divers of whose phænomena I did also, as time would permit, cast into one of the essays I was then engaged to write to a friend. And having dispatched that little treatise, it found so favourable a reception among those learned men, into whose hands it came, that I was much encouraged to illustrate some more of the doctrines of the corpuscular philosophy, by some of the experiments, wherewith my furnaces had supplied me; which also, as occasion served, I did, partly by writing some physico-chymical treatises, and partly by making such large notes on the essay concerning salt-petre, as might plentifully contribute to the history of qualities, of which I had sometimes thoughts. And this continued, till in the year before the last the public confusions in this (then unhappy) kingdom reducing me to quit my former design, together with the place, where my furnaces, my books, and my other accommodations were, I fell afterwards upon the making of pneumatical trials, whereof I lately ventured to give the public an account in a book of new experiments physico-mechanical about the air.

Viz. *A. D.* 1659.

I should not trouble the reader with so prolix a preface to such small treatises as those whereto this is prefixed, but for these two reasons: the one, that I hope the

foregoing narrative will make me be the more favourably judged by the philosophers I desire to serve, if sometimes I write less skilfully of their opinions, than perhaps I should have done, had I allowed myself to search into them: and the other, that I am earnestly solicited to publish some other tracts, tending to the same purpose that these do; to which also should I ever be induced, by the reception these may meet with, to trouble the world with them, the same preface, as it is now penned, may serve for an introduction. I had almost forgot to take notice, that whereas at the end of the essay concerning salt-petre I mentioned a then newly published treatise of the laborious *Glauber*'s, which I had not then perused, I found it to contain some observations concerning the history of salt-petre, which, if they be true, are considerable enough; I must again recommend the examination of them to the reader's curiosity, having been hindered by divers avocations from saving him that labour myself. And whereas also some years after I was informed of another little book he had put out since the former, wherein he teaches us a way of purifying salt-petre, to make a conjunction of the spirituous and fixter parts of it, and then to suffer the mixture to evaporate and so crystallize into nitre; this would, I confess, have made me apprehensive of passing for a plagiary with those, that did not know me, but that it was easy for me to clear myself by the testimony of very learned men, who had some years before perused my treatise, and especially of one person (well known by his writings) who was pleased to like it so well, as to desire he might translate it, and had accordingly long before turned it into very elegant Latin. I might perhaps venture to add, that though I could not justify myself by so convincing a proof of my innocence, yet he, that shall take the pains to consider, that I could not borrow of *Glauber* the various phænomena I have particularly set down, and much less the reflections on them, and shall compare in what differing manners, and to what differing purposes, we two propose the making of salt-petre out of its own spirit, and fixed salt (he but prescribing as a bare chymical purification of nitre, what I teach as a philosophical redintegration of it) he, I say, who shall compare these things together, will, perchance, think, that I was as likely to find this last named experiment as another. Which things I say not, as if I scrupled to make use of the industrious *Glauber*'s, or any other man's experiments, especially, when I borrow not with them any of the doctrines I build on them; but because since I neither did nor could take any notice of *Glauber*'s book in mine, I judged it requisite to say something, to prevent my being thought to have unthankfully taken one of the chief passages of my discourse from a book, to which I was utterly a stranger.

The reasons of my thus consenting to publish the following *History of fluidity and firmness*, without the rest of those annotations, which I writ upon the same essay touching salt-petre, are partly, that these are my recentest composures of this nature (having been written but the last year save one) and were set down, when I allowed myself to be less unacquainted with writers addicted to the modern philosophy; partly also, because the considerableness of the subject invited me to make these annotations much more copious, and somewhat less unaccurate, than my notes upon almost any other part of the essay; and partly (and indeed principally) because mention being sometimes made of this history in my freshly published physico-mechanical experiments, both the printer, and some learned gentlemen, who were pleased to think that book not unworthy the translating, have sollicited me to let this treatise be annexed to the several versions they are about of that pneumatical piece, and to the English edition of the three foregoing discourses, which the printer fears would, without the company of these or some others, make but too thin a book.

And

And I thought fit to premise to this history the essay concerning salt-petre, not only because it might appear very improper to publish annotations without the text itself, whereunto they relate; but indeed because I find, that there are still many learned men, of the same disposition with those I have mentioned in the beginning of this preface; whence I am invited to divulge this essay by the same considerations, that at first induced me to write it. Especially since I remember not, that among the new philosophers, I have met with any one experiment, that does so fairly and sensibly accommodate so many of their opinions. And indeed I freely confess, that I shall think myself to have done no useless service to the common-wealth of learning, if I prove so fortunate, as by these, or any other writings of mine to the like purpose, to beget a good understanding betwixt the chymists and the mechanical philosophers, who have hitherto been too little acquainted with one another's learning; there being to this very day a great and almost general misunderstanding betwixt the corpuscular philosophers and the chymists; most of those (on the one hand) looking upon the Spagyrists as a company of mere and irrational operators, whose experiments may indeed be serviceable to apothecaries, and perhaps to physicians, but are useless to a philosopher, that aims at curing no disease but that of ignorance; and most of the Spagyrists (on the other hand) looking upon the Corpuscularians (if I may so call them) as a sort of empty and extravagant speculators, who pretend to explicate the great book of nature, without having so much as looked upon the chiefest and the difficultest part of it; namely, the phænomena, that their art has added to the former edition of this vast and obscure volume. But that some of the principal of the hermetic opinions may be more handsomely accommodated by the notions of the Phœnician hypotheses, than by the common philosophy of elements and substantial forms (which yet their writers so frequently allude to and otherwise employ) may appear from hence, that whereas the schools generally declare the transmutation of one species into another, and particularly that of baser metals into gold, to be against nature, and physically impossible; the corpuscular doctrine rejecting the substantial forms of the schools, and making bodies to differ but in the magnitude, figure, motion or rest, and situation of their component particles, which may be always infinitely varied, seems much more favourable to the chymical doctrine of the possibility of working wonderful changes, and even transmutations in mixt bodies. And on the other side, there are scarce any experiments, that may better accommodate the Phænician principle, than those, that may be borrowed from the laboratories of chymists. For first, chymistry enabling us to depurate bodies, and in some measure to analyze them, and take asunder their heterogeneous parts, in many chymical experiments we may better than in others know what manner of bodies we employ, art having made them more simple or uncompounded, than nature alone is wont to present them us. And next, many chymical operations being performed in close, and yet in transparent vessels, we may better know what concurs to the effects produced, because adventitious bodies (or at least all grosser ones) are kept from intruding upon those, whose operations we have a mind to consider. And lastly, the bodies employed by the chymists being for the most part active ones, the progress of nature in an experiment, and the series of successive alterations, through which the matter passes from first to last, is wont to be made more nimbly, and consequently becomes the more easy to be taken notice of and comprehended. So that all this considered, I hope it may conduce to the advancement of natural philosophy, if, as I said, I be so happy, as, by any endeavours of mine, to possess both chymists and Corpuscularians of the advantages, that may redound to each party by the confederacy I am meditating between

tween them, and excite them both to enquire more into one another's philosophy, by manifesting, that as many chymical experiments may be happily explicated by corpuscularian notions, so many of the corpuscularian notions may be commodiously either illustrated, or confirmed by chymical experiments.

A

PHYSICO-CHYMICAL ESSAY,

Containing an EXPERIMENT, with some Considerations touching the differing parts and redintegration of SALT-PETRE.

SECTION I.

SALT-PETRE, *Pyrophilus*, though in that form, wherein it is sold in shops, it be no very obvious concrete; yet either in its rudiments, or under several disguises, it is not to be found in so great number of compound bodies, vegetable, animal, and even mineral, that it seems to us to be not only one of the most catholick of salts, but so considerable an ingredient of many sublunary concretes, that we may justly suppose it may well deserve our serious enquiries, since the knowledge of it may be very conducive to the discovery of the nature of several other bodies, and to the improvement of divers parts of natural philosophy.

SECT. II.

BUT not having at present much leisure allowed me by several avocations, to make accurate enquiries into the nature of salt-petre in general; and, which is more considerable, being not yet furnished with a competent number of experiments requisite to such a purpose; I must content myself for this time to tender you some assistance, towards the discovery of how differing substances may be obtained from nitre, and compound it again, by presenting you some reflections on an experiment, which my desire to hasten to another subject obliges me to set down nakedly, as I first tried it, by way of narrative.

SECT.

SECT. III.

WE took then common nitre (as we bought it at the druggist's) and having by the usual way of solution, filtration, and coagulation, reduced it into crystals, we put four ounces of this purified nitre into a strong new crucible, * in which (the vessel being first well nealed to prevent cracking, and covered to prevent the falling in of any thing, that might unseasonably kindle the petre) we melted it into a limpid liquor, and whilst it was in fusion, cast into it a small live coal, which presently kindled it, and made it boil and hiss, and flash for a pretty while. After which, we cast in another glowing coal, which made it fulminate afresh; and after that we cast in a third and a fourth, and so continued the operation, till the nitre would neither fulminate nor be kindled any more. After which we continued to keep it in a strong fire for above a quarter of an hour, that if any volatile part should yet remain, it might be forced off.

SECT. IV.

THEN taking out the crucible, and breaking it whilst it was hot, we took out, as carefully as we could, the remaining fixed nitre, before it had imbibed much of the moisture of the air; and dividing it into two equal parts, we dissolved one of those portions in as much fair water as would just suffice for the solution of it, and then dropped on it spirit of salt-petre, till the ebullition occasioned by the mutual action of those contrary liquors did perfectly cease; and forthwith filtrating this mixture, we exposed it in a new open phial to the air in a window; and returning to the other portion of fixed nitre, which we had set apart and not dissolved, we dropped on that likewise of the same spirit, till the hissing and ebullition were altogether ceased, and then we expose this mixture also in an open glass-jar to the air in the same window with the former.

SECT. V.

THE event of these trials was, that the mixture, wherein fair water was employed, did in a few hours fasten to the lower part of the sides of the glass, wherein it was put, some saline particles, which seemed by their form (and partly too by their shoot-

* If it be here demanded, Why the experiment was not made with a greater quantity of salt-petre? we may answer, that the mentioned quantity was most proportinate to the best crucible we then had. And if it be further asked, Whether it were not the better way of obtaining the several substances separable from nitre, to distil it in close vessels without addition of any foreign body? we shall reply only by representing, that the proposed way is not so practicable as one would imagine: for not acquiescing in the common practice of chymists, who are wont to mingle with the salt-petre they distil three or four times its weight of brick, earth, or some other additament, which (especially in so great a proportion) may much alter the nature of the fixed salt remaining behind with it; we have had the curiosity to try more than once, whether we could distil salt-petre *per se* in glass retorts, and found, that though to avoid giving too strong a fire, we once (at least) placed the retort only in a panful of sand; yet when the heat was grown strong enough to melt the salt, it cracked the retort, and did partly run out at the crack: only we obtained some small quantity of a liquor, which by its sourness and operation taught us what we might have expected of the rest of the volatile part of the nitre, in case the vessel would have held till it had passed over into the receiver.

ing

ing about the lower parts of the veſſel) to be ſalt-petre; amongſt whoſe little cryſtals nevertheleſs there appeared to ſwim very little grains (much ſmaller than muſtard-ſeeds) of ſome other kind of ſalt, environed with a downy matter, not unlike that, which is oftentimes to be obſerved in roſe-water, and ſeveral other diſtilled waters, when they begin to decay. The cryſtals were the next day taken out, being by that time grown ſomewhat greater, and more numerous; and diſcloſed themſelves, upon trial, to be indeed nitrous, as well by their manner of burning, as their ſhape. Concerning the latter of which, ſince learned modern writers have miſ-repreſented it, ſome making nitre to be cylindrical, and others of a figure leſs approaching to the true one; I think myſelf obliged in this place to obſerve to you by the way, that having purpoſely conſidered ſome large cryſtals of refined and unanalyzed nitre, the figure being in ſuch beſt diſcerned, they appeared to have each of them ſix flat ſides (not always of equal breadth in reſpect of one another) whereof any two that were oppoſite were commonly parallel. But to return to our augmented cryſtals of nitre; what the other matter, that adhered to them, was, there was ſo very little of it, that we could not well diſcern, though we then ſuſpected it to proceed from the want of a juſt or exact proportion betwixt the volatile and fixed parts of the nitre, that were to be reunited.

SECT. VI.

THE remaining liquor being poured into an open glaſs-jar, and left in the ſame window, continued five or ſix days without manifeſting any conſiderable alteration; but at the end of that time there began to appear in it very fine cryſtalline ſtriæ of petre, which grew more and more numerous during a fortnight longer: at which time, being wearied with attending the ſo ſlow conſumption of the liquor, we poured it from the cryſtals, and ſet it in a digeſting furnace to evaporate more nimbly.

SECT. VII.

THE other mixture, wherein no water was employed, did preſently, for a great part of it, ſubſide in the form of ſalt; over which there ſwam a little liquor, which alſo ſeemed to keep the ſubſiding particles of ſalt from congealing into one coherent maſs, or ſo much as greater lumps: and a part of this drenched ſalt being taken out, and permitted to dry in the air, did not appear very regularly figured, but yet ſeemed here and there to recede very little from the ſhape of ſalt-petre; and being caſt on a quick coal, it burned partly after a manner not peculiar (that we have obſerved) to any diſtinct kind of ſalt; and yet it partly ſeemed to imitate the flaſhing way of deflagration proper to nitre. The remaining part of this ſalt, together with the liquor ſwimming upon it, we kept for about a month in the open air, without diſcerning any obſervable change in the liquor, till towards the latter end of that time, and then we found it partly coagulated into ſmall ſaline maſſes, whoſe figure we were not able to diſcern. And therefore diſſolving the whole mixture in a little fair water, and filtrating it, we found (after evaporation in a digeſting-furnace) about one half of the ſalt ſhot into fine ſmall icicles of the ſhape of cryſtals of petre, but ſomewhat differing from them in taſte upon their firſt being put upon the tongue; but upon a live coal they burned not unlike petre. And the remaining half of this diſſolution, being

somewhat hastily pressed to exhale, let fall its salt in a figure, which we could not reduce either to that of salt-petre, or of any other determinate kind of salt.

For the clear comprehending of this experiment, you may be pleased, *Pyrophilus*, to take notice,

SECT. VIII.

1. THAT a new coal is not to be cast on the nitre, till the detonation occasioned by the former be either quite or almost altogether ended; unless it chance, that the puffing matter do blow the coal too soon out of the crucible, (which often enough happens towards the end of the operation:) which seems to happen chiefly, because the first part of the nitre growing to be predominant, the inflammable and halituous particles cannot break through the matter, now grown more stiff, but by such impetuous eruptions, as make them oftentimes toss back the coals as soon as ever they are cast into the crucible: and in this way of proceeding we have been forced to spend much more time, than the opinion of the already deflagrability (if I may so speak) of salt-petre did beforehand permit us to imagine.

SECT. IX.

2. THAT we discerned by our scales, that the weight of the spirit of nitre requisite to be dropped on, till all the ebullition made betwixt that liquor and the solution of fixed nitre were ceased, did not amount to so great a weight, as the salt-petre lost in its detonation, and yet fell not much short of it.

SECT. X.

3. THAT the fixed nitre this way made differed but very little from vulgar salt of tartar in its lixiviate taste, in its aptness to attract the air, or to relent by the moisture of it, and in its other more obvious qualities; only whereas salt of tartar is wont to be white (which nevertheless being fluxed has been by others, as us, observed to become of a kind of sky-colour) this fixed nitre was of a deep colour betwixt blue and green: which colour, upon the affusion of the spirit of nitre, vanished; whereas otherwise (to observe that to you upon the by) some sort of calcined nitre will so obstinately retain that colour, that I keep by me a bluish green liquor made of fixed-petre, I know not how oftentimes successively resolved *per deliquium* and coagulated again, till it would no longer be reduced to a dry salt, but to an unctuous body easily flowing in heat like wax: my design in which trial it were here somewhat improper to insist on *.

SECT.

* That our friends might not be obliged to wait so long for the redintegration of nitre, but might see the experiment made in as little time as is possible, we devised a more expeditious way of uniting the divorced parts of our salt, and it was only by suffering such fixed nitre, as is mentioned by our author, to run *per deliquium* into a liquor, which being separated from its *faeces* by filtration through cap-paper was very clear and limpid: for, when we had a mind to shew the experiment, we did only upon this liquor drop some spirit of nitre, and that, after such a noise, sparkling and effervescence, as our author speaks of,

SECT. XI.

AND becauſe, *Pyrophilus*, it may be ſuſpected, that the ſalt-petre mentioned to have been produced by the reunion of the volatile and fixed part of that concrete, may have been only ſome aſſociated particles of ſalt-petre, that by lurking undiſcernedly in the fixed nitre had eſcaped the analizing violence of the fire, and by the affuſion of fair water were ſet at liberty to aſſemble together, and thereby diſcloſe themſelves in their true ſhape:

To remove this ſcruple, and to let you ſee that much of the enſuing diſcourſe will not need your ſuppoſing, that the experiment of the redintegration of petre was accurately made, and did accordingly ſucceed; I muſt here annex, that though by divers other chymical experiments, which I have had occaſion to make with ſalt-petre, I ſometimes diſcovered, that now and then ſome undiſcerned particles of the ſalt-petre may poſſibly eſcape our diligence when we make fixed nitre; yet thoſe particles are too few to amount to ſuch cryſtals of petre, as the affuſion of the acid ſpirit upon the lixiviate ſalt are capable of affording. And that we have, to ſatisfy ourſelves farther in this particular, purpoſely ſatiated, according to the former manner, a ſolution of common pot-aſhes, bought of them, that are wont to ſell it in ſhops, (who are not ſo fooliſhly knaviſh, as to adulterate them with ſalt-petre, which is much dearer than pot-aſhes) and filtrating the ſolution from its copious *faeces*, found after evaporation, in the remaining liquor, within about two or three days, and ſometimes much ſooner, pretty ſtore of cryſtalline ſalt in a nitrous figure, which though at firſt it taſted ſomewhat corroſively, (perhaps becauſe the proportion betwixt the nitrous ſpirit and the pot-aſhes was not duly obſerved) yet after it had a while remained upon the tongue, the taſte of it much emulated that of ſalt-petre; and part of it being caſt upon a live coal, did by its blue and halituous flame diſcover itſelf to be of the nature of that ſalt. To which we may add, that we likewiſe tried the experiment with aqua fortis and ſalt of tartar, and thereby produced ſalt-petre, though but in ſmall quantity, and a long time. And thoſe two additional experiments I the rather

of, (all which haſtily vaniſhed) did immediately aſſociate itſelf with a competent proportion of the fixed ſalt ſwimming in the ſolution, and therewith fall down in little icicles of a nitrous ſhape and nature; and when we pleaſed to continue the affuſion of the acid ſpirit, this emergency of ſalt petre would be obſervable from time to time, till either all, or almoſt all the fixed ſalt had united itſelf with the other. And theſe little icicles being dried in lumps, did as well upon the tongue and upon a quick coal, as they had done to the eye, diſcloſe themſelves to be ſo truly nitrous, that our friends were not wont without ſome wonder, as well as much pleaſure, to behold ſalt-petre thus ſuddenly generated in leſs than a minute of an hour. Theſe ſmall icicles being in ſufficient plenty diſſolved in fair water, we did for trial's ſake reduce by congelation to fairer cryſtals.

But though this be the perfecteſt and readieſt way of reproducing nitre, yet becauſe it often requires, eſpecially in dry weather, a long time to reduce fixed nitre *per deliquium* into a liquor, we have ſometimes ſubſtituted the following way: We diſſolved in fair water as much fixed nitre as we could, and filtrating the ſolution through cap paper, we ſatiated it with ſpirit of nitre, after the manner above deſcribed; and then ſetting it to evaporate very ſlowly, and afterwards ſuffering it to cool, we obtained, within ſome hours after the firſt mixture of the liquors, ſtore of fine little cryſtals of petre, which ſhot in the liquor; the remaining part of which being evaporated afforded more of them. And though the evaporation and cryſtallization coſt us divers hours, yet it ſeemed, that the ſalt-petre was produced preſently upon the ceaſing of the conflict betwixt the two liquors. For the mixture, before evaporation, taſted very like a ſolution of common nitre, and the little drops, that upon the effervescence ſkipped out of the glaſs, and fell back upon the ſides of it, did there many of them preſently coagulate into little grains of nitrous ſalt.

mention, because many of the ensuing reflections may be justified by them, although the main experiment made on salt-petre alone should in divers particulars be supposed (for we have used our endeavours that it may not be found) to have been mistaken.

SECT. XII.

The reflections, which may be made on this experiment, are more than I have either the skill or leisure to prosecute; and therefore I shall content myself to present you very succinctly with a few of those, that do the most readily occur to my present thoughts.

And first this experiment seems to afford us an instance, by which we may discern, that motion, figure, and disposition of parts, and such like primary and mechanical affections (if I may so call them) of matter, may suffice to produce those more secondary affections of bodies, which are wont to be called sensible qualities.

SECT. XIII.

And to begin with the tangible qualities, as heat and cold; it is commonly held, that salt-petre is in operation a cold body, if not one of the coldest in the world; and accordingly physicians and chymists are wont to give it in fevers, to allay the inward exæstuations of the blood and humours: and that profound naturalist the Lord *Verulam* highly commends a little of it, and did for many years himself make use of it, to condense the spirits. But whatever it be in inward operation, certainly to the outward sense it appears very cold: and yet the parts of this so cold body (its spirit and alkali, by the latter of which chymists are wont to mean any fixed salt produced by burning) put together, do immediately agitate each other with great vehemency; and did in our experiment produce such an heat, that I could scarcely endure to hold in my hand the phial, wherein much less than an ounce of each was mixed, though but leisurely and almost by drops; as if heat were nothing but a various and nimble motion of the minute particles of bodies. For in our experiment, as long as that confused agitation lasted, so long the heat endured, and with that agitation it increased and abated; and at length, when the motion ceased, the heat also vanished.

SECT. XIV.

Upon the mixture of the two fore-mentioned liquors, there was also produced a very audible sound, not unlike the hissing produced by the quenching of a live coal in water; and this hissing was, as that other is wont to be, accompanied with an effervescence and boiling up of the liquor, with store of bubbles, till it was ready to run over the vessel. This sound seemed to proceed from the nimble and smart percussions of the ambient air, made by the swift and irregular motions of the particles of the liquors: and such a kind of sound, but much louder, was produced by the impetuous eruptions of the halituous flames of the salt-petre upon the casting of a live coal upon it. What interest such a smartness in striking the air hath in the production of sound, may in some measure appear by the motion of a bullet, and that of a switch or other wand, which produce no sound, if they do but slowly pass through

through the air; whereas if the one do ſmartly ſtrike the air, and the other be ſhot out of a gun, the celerity of their percuſſions on the air puts it into an undulating motion, which reaching the ear, produces an audible noiſe, even at a good diſtance from the body, whoſe ſwift paſſage cauſes thoſe nimble vibrations in the air, as we may elſewhere have occaſion to declare. And that in the ſound obſervable in our experiment, the contiguous air receives many ſtrokes from the particles of the liquor, ſeems probable, by the ſudden and eager tumultuation of the parts of the liquors; and by this, that the noiſe increaſed and decayed proportionably to the ebullition of the liquors, and ceaſed altogether as ſoon as the ſaline particles floating in them had by their conflict tired themſelves into quietneſs. And it is to be obſerved, that the noiſe ended long before the heat. To the latter of which ſuch an inteſtine tumult of the parts of many bodies is ſufficient, as is yet uncapable to produce a ſound. As we ſee in amber or good hard wax heated by rubbing; and in many liquors, which retain a conſiderable degree of heat a good while after the expiration of the noiſe they made in boiling.

SECT. XV.

WE mentioned alſo, that our fixed petre was of a bluiſh green colour, which upon the affuſion of the acid ſpirit ſuddenly vaniſhed; that diſpoſition of parts, whereby the light reflected to the eye, was ſo modified as to produce that colour, being now altered. And the like change we have ſometimes obſerved to be producible in fixed nitre, by the bare leaving it a while in the moiſt air. To which I muſt add, that in ſome ſuch kind of experiments I have obſerved the copious fumes, ariſing from the mixture, to make the unfilled part of the glaſs look of a reddiſh colour; which is not more odd than that, which we have lately had opportunity to obſerve in ſoot, which though it be ſo black itſelf, and reſults from the coalition of dark exhalations, yet preſſed with a ſtrong fire, has filled our receivers with fumes white enough to make them look, as if they were repleniſhed with milk. And we have ſometimes alſo taken great pleaſure to behold the variety of colours, which may be now and then diſcerned in the ſublimate, made by gradually ſubliming in an urinal a mixture of equal parts of only white ſal-armoniac and black antimony. But to wander no longer far from our preſent experiment, give me leave to inform you, that a while ſince attempting to make ſalt of tartar, I diſſolved in a little fair water an ingredient of ſalt-petre; by the addition of good aqua fortis, the union of theſe two liquors produced a deep green colour, which not only was diffuſed quite through the mixture, but alſo appeared to reſide peculiarly in certain particles of it. For having for trial-ſake filtred it through cap-paper, there remained in the filtre a powder of a very deep and lovely colour, but in ſo little quantity, that we could not attempt any experiment upon it to make it confeſs its nature. But this circumſtance is not to be omitted, that the ſalt of tartar, that was then employed, was extraordinary pure, having been by a peculiar art (elſewhere to be taught you) brought, without any addition, into fair-figured cryſtals almoſt like lumps of white ſugarcandy. To which I muſt add, that the ſame aqua fortis, with a ſolution of other pure ſalt of tartar, did likewiſe produce a colour much reſembling the former, but much fainter. And it is farther to be taken notice of on this ſubject, partly, that nitre itſelf, although it ſeem to have nothing of kin to redneſs, doth in diſtillation yield blood-red fumes (fondly called by ſome chymiſts the blood of the ſalamander) which fall again into a liquor

liquor that has nothing of red in it; and partly, that the fixed nitre, that did before appear opacous, by a new disposition of its parts conjoined with those of its re-imbibed spirit, becomes again somewhat diaphanous and crystalline, as it was at first.

SECT. XVI.

Upon the mixture of these two liquors there also obtrudes itself upon the sense a very strong and offensive smell, proceeding from the spirit of petre; which perhaps occasioned some chymists to call a menstruum (wherein that nitrous spirit and smell is predominant) the Stygian water. But though the nitrous spirit have a very strong and unwelcome odour of itself, yet is it made much more offensive by being poured on its own fixed salt; for upon their own conflict, the matter, being vehemently agitated, doth more copiously emit such stinking exhalations than before, and sendeth forth fumes manifestly discernible as well to the eye as nostrils. The odour of the fixed nitre is very languid; but that, which it discovers, being dissolved in a little hot water, is altogether differing from the stink of the other, being of kin to that of other alkalizate salts. And yet the salt-petre, from which such differingly-scented bodies spring, and which may again emerge from the coalition of them, has not been observed, as I remember, to have any smell at all.

SECT. XVII.

The tastes of these two bodies are as differing as any of their other qualities: for the spirit is exceedingly acid, and may be called a strong and sour *Acetum Minerale*; whereas the fixt nitre has as strong a taste of salt of tartar as the spirit has of distilled vinegar: and yet these two bodies, whose sapours are so pungent, and so differing, do both spring from and unite into salt-petre, which betrays upon the tongue no heat nor corrosiveness at all, but coldness mixt with a somewhat languid relish retaining to bitterness. And though we must not conceal from you, that in our trial the redintegrated salt-petre had upon its first impression upon the tongue a taste more sharp and perforating (if I may so speak) than ordinary nitre; yet that pungency may not improbably be supposed to have proceeded from some acid particles of the spirit, that were not yet duly incorporated with, but only loosely adherent to, the more perfectly nitrous parts, which afterwards discovered itself upon the tongue. And however, the difference betwixt the taste of this new salt, and those of the acid and alkalizate salts whereof it consisted, and (unquestionably) the taste of these compared with that of the crude petre, which was dissipated into them, were sufficient to warrant this reflection.

SECT. XVIII.

Of the other observables presented us by our experiment, we must, *Pyrophilus*, content ourselves to mention but a few; our haste being such, that it will not permit us either to enumerate them all, or to insist long on any of them.

SECT. XIX.

SECONDLY then, the propoſed experiment ſeems to make it ſomewhat queſtionable, whether or no inflammability doth ſtrictly in all mixt bodies require a diſtinct ſulphureous ingredient; and whether or no in ſome concretes it may not reſult from ſuch a contrivance of parts, as that thereby the particles of the concrete are diſpoſed to be ſet a moving by the adventitious, whether fiery or calorific, corpuſcles of another body, in ſuch numbers, and with ſuch celerity, as may put them into that ſcheme of matter, which we call flame. How violent an heat may be produced upon ſuch an account as this, may in ſome meaſure appear by an experiment, wherein our preſent theme ſalt-petre is the main agent. For if into a phial filled with good ſpirit of nitre you caſt a piece of iron, you may perceive, that the liquor, whoſe parts moved placidly and uniformly before, manifeſted no heat to the touch, meeting with pores and particles in the iron capable of very much altering the motion of its parts (and perhaps alſo that of ſome very ſubtile intercurrent matter) thoſe active parts do preſently begin to penetrate, ſever, and ſcatter abroad the particles of the iron (almoſt as gunpowder doth the pieces of breaking granadoes) with ſuch rapidity, and in ſuch plenty and throngs, that being themſelves alſo put into a very ſwift and irregular motion (whenceſoever it proceeds) there is hereby produced a heat capable (if the quantity of the liquor and metal be great enough) to burn his hand, that holds the veſſel, and perhaps break the veſſel (if it be not very open) all to pieces: whereas by caſting into the ſame ſpirit of nitre little lumps of camphire, whoſe particles were indiſpoſed to occaſion the like diſturbance and agitation in the nitrous ſpirit, we obſerved the agitation made of the particles of the white gum to change it only into a yellowiſh and fluid ſeeming oil.

SECT. XX.

BUT not to wander any further, our own experiment informs us, that ſalt-petre (which not only is inflammable, but burns very fiercely and violently) may be produced by the coalition of two bodies, which are neither of them inflammable; the one being a fixed ſalt, that to become ſuch has already ſuffered the loſs of all that the fire could deprive it of, and the other being a ſpirit abounding with acid particles, which kind of ſalts have been obſerved to be more apt to quench than foment fire.

SECT. XXI.

AND becauſe we may elſewhere, God aſſiſting, treat more particularly of the inflammableneſs of bodies, we will now add but a few lines concerning that of nitre, that this circumſtance of it might not eſcape our obſervation; namely, that upon caſting ſalt-petre on a glowing coal, or upon the caſting of a glowing coal into melted ſalt-petre, the nitre will immediately take fire, and flaſh out into bluiſh and halituous flames; whereas if the ſame nitre be placed in a crucible, though that crucible be by degrees made glowing hot, and do immediately with its concave ſurface in innumerable places touch the particles of nitre, yet the ſtrange ſalt will be thereby melted, but

but not kindled. The reason of which phænomenon I must not now (but may on another occasion) spend time to inquire after.

SECT. XXII.

IT may also, *Pyrophilus*, deserve the inquiry, whence it proceeds, that whereas the body of salt-petre, when committed to distillation, is oftentimes very well dried, and consists of saline parts, which are generally accounted to be of a very dry nature, yet the spirits of petre forced by the fire into the receiver should not, like sal-armoniac, and some other bodies distilled with the like heat and vessels, adhere in the form of sublimate to the receiver, but fall into a liquor, which does not, for aught we have seen or heard of, either totally or in part coagulate again the cold, as we have seen spirit of urine and other volatile liquors (afforded by animal substances) often do; and as we have observed, though rarely, even in the corrosive liquor, that is wont to be called butter of antimony. And the like inquiry may be made concerning the liquidness of the distilled spirits of depreciated salt, calcined vitriol, and divers other bodies, which seem to have been destitute of moisture, when committed to distillation.

SECT. XXIII.

BUT this not being precisely a phænomenon of our experiment, we shall not here prosecute it (though perhaps we elsewhere may) but rather observe to you, *Pyrophilus*, that whereas good spirit of nitre being left in an open vessel, is wont to smoak and waste itself in an exhalation sensible, especially if it be excited by a little heat, not only in the nose but to the eye; this fugitive spirit, when it is once re-united to its own fixed salt, emits no such steam, though kept a good while near a considerable fire: which instance may somewhat assist us to make out, that the most fugitive parts of concretes may in spight of their natural mobility be detained in bodies by their union and texture with the more sluggish parts of them, among which those lighter and more active ingredients may be so entangled, as to be restrained from avolation.

SECT. XXIV.

ANOTHER thing worth considering in our experiment is this, that upon the dropping of the acid spirit into the alkalizate liquor, if you place the open-mouthed glass, wherein the experiment is performed, betwixt the light and your eye, you may plainly discern, that the saline particles of these liquors toss one another (or are tossed by some brisk invisible substance) to the height of divers fingers breadth up into the air, whence most of them fall back into the vessel like a thick shower of little drops of rain. And it were worth inquiring, whence this sparkling of the parts of these mixed liquors arises; and whether the saline corpuscles may be conceived rapidly to move differing ways, and so, thwarting each other in their courses, and rudely jostling at their occursions, some of them are forced to bound or fly upwards (almost like ivory balls meeting each other on a billiard-table). And to assist you in this inquiry, give me leave to inform you, that the particles thus thrown into the air appear to be most of them saline by this observation; that soon after the fall of the fore-mentioned showers,

ſhowers, you ſhall find the ſides of the glaſs, wherein the affuſion of the nitrous ſpirit has been made, all embroidered with little grains of ſalt, left there by thoſe wandering drops, that fell beſides the liquor.

SECT. XXV.

AND let me farther obſerve to you, that there ſeems to be a very nimble agitation in the particles of the ſpirit of nitre, by this, that upon the pouring of aqua fortis (whoſe active part is little elſe than ſpirit of nitre) upon a ſolution of ſalt of tartar in fair water, in which divers ſmall lumps of the ſalt remained yet undiſſolved, we have obſerved the acid ſpirit to ſever the particles of the ſalt with ſuch impetuoſity, that the numberleſs little bubbles produced upon their conflict, and haſtily aſcending in ſwarms from ſome of the little lumps, made them emulate ſo many little, but rapidly riſing, ſprings. And to make it yet appear more probable, that there may be ſuch croſſing motions in the parts of theſe liquors, we obſerved, that after the two contrary ſalts had by their mutual conflict tired each other (or rather had been upon their occurſions faſtened to one another) there would follow no farther ebullition or ſkipping up and down of little drops of the liquors, upon the putting in of more ſpirit of nitre, unleſs there were added likewiſe more of the alkalizate liquor.

SECT. XXVI.

AND, before we paſs on from this reflection, it may not be uſeleſs to take notice of the difference, that there may be betwixt thoſe active parts of a body, which are of differing natures, when they are as it were ſheathed up, or wedged in amongſt others in the texture of a concrete; and the ſame particles when (extricated from theſe impediments) they are ſet at liberty to flock together, and by the exerciſe of their nimble motions diſplay their proper, but formerly clogged activity, or acquire a diſpoſition to be briſkly agitated by ſome fine interfluent matter. For though in the intire body of ſalt-petre the ingredients it conſiſts of, or the differing ſubſtances, into which the fire diſſipates it, do ſo mutually implicate and hinder each other, that the concrete, whilſt ſuch, acts but very languidly; yet when the parts come to be diſlocated, and the halituous and alkalizate particles are enabled or made to diſband from the concrete, and aſſociate themſelves with thoſe of their own nature, we ſee with how great an activity both the acid ſpirit and the fixed ſalt are endowed.

SECT. XXVII.

AND we may yet farther obſerve, that it is not barely an indefinite nimbleneſs of motion, and activity of the particles of ſaline liquors, that enables them to perform each of their particular effects: for to the production of ſome of theſe there ſeems requiſite, beſides perhaps a modification of their motion, a determinate figure of the corpuſcles, anſwerable to that of the pores of the body by them to be diſſolved; as ſpirit of nitre corrodes ſilver, but not gold; which nevertheleſs its particles aſſociated with thoſe of ſal-armoniac, and thereby acquiring a new figure, and perhaps a differing motion, will readily diſſolve: and the liquor of fixed nitre will, for the ſame

reason, dissolve such sulphureous and unctuous bodies as the acid spirit will not corrode: nay, and I have carefully observed, that there may be liquors, that will not dissolve some bodies, unless the motion or activity of their particles be allayed or modified by the mixture of fair water, or such unactive vehicles.

SECT. XXVIII.

Another particular, which in our experiment we may take notice of, is, the unwariness of those vulgar chymists, who presume confidently (and indiscriminately enough) to ascribe to each of the heterogeneous ingredients, or (in their language) principles of a concrete analysis by the fire, the virtues and properties (perhaps too in an exalted degree) of the entire body. But though this be an error of very ill consequence in reference to divers chymical preparations of medicines; yet having elsewhere discoursed purposely of it, we shall here content ourselves to allege against it the instances afforded us by the experiment under consideration: for in that we may observe, that when salt-petre is distilled, the volatile liquor and fixed salt, into which it is reduced by the fire, are endowed with properties exceeding different both from each other, and from those of the undissipated concrete. For the spirit of nitre is (as we formerly have observed) a kind of *Acetum Minerale*, and possesses the common qualities to be met with in acid spirits as such; whereas the fixed nitre is of an alkalizate nature, and participates the qualities belonging generally to lixiviate salts; and salt-petre itself is a peculiar sort of salt, discriminated by distinct properties both from those salts, that are eminently acid, as allum, vitriol, sal-gemmæ, *&c.* and from those, that are properly alkalizate, as salt of tartar and pot-ashes; and accordingly, we may easily observe a vast disparity in the effects and operations of these three bodies. For several, if not all of those mineral ones, which aqua fortis will by corroding dissolve, the solution of fixed nitre will precipitate; and divers, if not all of those sulphureous and unctuous bodies, which the solution of fixed nitre will dissolve, the acid spirit of petre will precipitate. And we have in a trice re-dissolved with the spirit a solution of sublimate precipitated with the other liquor: thus, if into a scarlet tincture made by an infusion of Brasil in fair water, we pour a little spirit of nitre, the shaken liquor will in a moment change its redness for a kind of yellow, which by pouring on it a little of the solution of fixed nitre, may be again graduated into a somewhat sanguine colour, sometimes paler, and sometimes perhaps deeper than the first; whereas a solution of salt-petre itself poured on either of the former tinctures, the red or the yellow, has not been by us discerned to have produced any sensible alteration. And whereas salt-petre itself is partly fixed, and partly volatile, the acid ingredients of it are altogether volatile, the alkalizate fixed. But having elsewhere occasion to speak to this subject, we shall now proceed to tell you, that

SECT. XXIX.

It may pass to another observable presented us by our experiment, that it gives us occasion to inquire, whether the air doth not contribute something to the artificial production of salt-petre, or at least to the figuration of it according to the perfecter shape belonging to that kind of salt: for we formerly observed, that the salt, which was leisurely permitted to shoot of itself in the liquor exposed to the open air, did shoot

shoot into more fair and large crystalline striæ, than those, that were gained out of the remaining part of the same liquor by a more hasty evaporation, though made but in a digesting furnace. And we have also observed, that when once we poured aqua fortis on a strong solution of salt of tartar, till no further effervescence was discernable betwixt them, though the mixture by a somewhat quick heat afforded a salt, that seemed to be very nitrous, yet it would not be brought to shoot in so fair and conspicuously figured crystals of petre, till it had been a good while exposed to the open air: but whether the air itself impregnated with the promiscuous steams of most of the bodies of the terrestrial globe (and perhaps with seminal effluvia from some of them) do really contribute any thing either to the production or figuration of salt-petre in our experiment, I dare not yet determine, for two chief reasons.

SECT. XXX.

WHEREOF the first is, because the figuration seems not improbably ascribable, not so much to the proper efficiency of the air, as to the conveniency, which by quietness, and a competent vehicle to move in, was afforded to the saline particles, to conform themselves (or be conformed by a concourse of agents and circumstances) to that figure, which is most natural to them. For we have observed already, that the fixed nitre, which was not dissolved in water, before the affusion of the acid spirit, did not shoot into the wonted form of crystals of petre, but remained a kind of nitrous powder, the acid and alcalizate saline particles not having a convenient vehicle to expand themselves in; but being necessitated, for want of room, to make an unseasonable and over-hasty coalition, upon which their own weight made them subside in the figures resulting from their casual concourse, and therefore probably differing from those, into which the saline corpuscles would have been disposed, had they been allowed a competency of vehicle and time.

SECT. XXXI.

THE other reason of my hesitancy about the use of the air in our experiment, is, that I inconsiderately forgot to try, whether part of that liquor, which shot into crystals in an open-mouthed-glass exposed to the air, would not have done the like, if it had been left quiet as long as the other was, though in a vessel accurately stopped *: but whatever the air hath to do in this experiment, I dare invite you to believe, that it is so enriched with variety of steams from terrestrial (not here to determine whether it receive not some also from celestial) bodies, that the inquiring into the further uses of it (for I mean not its own uses in respiration, sailing, pneumatical engines, *&c.*) may very well deserve your curiosity. To encourage which, I dare at present only tell you, that though I cannot yet pretend to much experience in this particular, yet we have known such changes (seemingly chymical) made in some saline concretes, by the help chiefly of the volatilizing operations of the open air, as very few, save those, that have attentively considered what *Helmont*, and one or two

* Whether the air have any great interest in the figuration or in the re-production of nitre, the author hath since examined by particular trials; but in vessels and by ways not to be easily described in few words, and therefore the further mention of them is reserved for another discourse.

other artists, have hinted on that subject, or have made trials of that nature themselves, will be apt to imagine.

SECT. XXXII.

And if upon further and exacter trial it appears, that the whole body of the salt-petre, after its having been severed into very differing parts, by distillation, may be adequately re-united into salt-petre equiponderant to its first self; this experiment will afford us a noble and (for aught we have hitherto met with) single instance to make it probable, that that, which is commonly called the form of a concrete, which gives it its being and denomination, and from whence all its qualities are in the vulgar philosophy, by I know not what inexplicable ways, supposed to flow, may be in some bodies but a modification of the matter they consist of; whose parts, by being so and so disposed in relation to each other, constitute such a determinate kind of body, endowed with such and such properties. Whereas if the same parts were otherwise disposed, they would constitute other bodies of very differing natures from that of the concrete, whose parts they formerly were, and which may again result or be produced after its dissipation and seeming destruction, by the re-union of the same component particles, associated according to their former disposition.

SECT. XXXIII.

The redintegration (or re-production) of an analyzed body, if can be accurately and really performed, may give much light to many particulars in philosophy, and would certainly be very welcome both to the embracers of the atomical hypothesis, and generally to those other modern naturalists, who aspire to such explications of nature's phænomena as may at least be understood: all whom I wish, that though men cannot perhaps in all things, yet at least as far as they can, they would accustom themselves to speak and think, as nature does really and sensibly appear to work; and not to acquiesce in notions and explications of things, which, strictly examined, are not intelligible.

Wherefore I am about to attempt a reproduction in vitriol, turpentine, and some other concretes, in which it seems not unlikely to be performable: and perhaps you may see cause to think, that the experiment of salt-petre, even as we have already made and proposed it, though it be not an exact and adequate redintegration, is yet not far from being a real one; the dissipated parts of the concrete truly re-uniting into a body of the same nature with the former, though not altogether of the same bulk.

SECT. XXXIV.

And yet I think it requisite to represent to you, *Pyrophilus*, that salt-petre is a body, whose parts are not organical, and which is not so much as very compounded; and that therefore bodies, that consist of more numerous ingredients, and much more those, whose organical parts require a much more artificial and elaborate disposition or contrivance of their component particles, cannot be safely judged of, by what is possible to be performed on a body of so simple and slight a contexture as is salt-petre: for

for we ſee, that even wine, though no organical body, nor ſo much as the moſt compounded of inanimate concretes, when its ſpirit is, though by the gentleſt diſtillation, drawn from it, will not, by the re-union of its conſtituent liquors, be reduced to its priſtine nature; becauſe the workmanſhip of nature in the diſpoſition of the parts was too elaborate to be imitable, or repairable by the bare and inartificial appoſition of thoſe divided parts to each other: beſides that in the diſſociating action, even of the gentleſt fire, upon a concrete, there does perhaps vaniſh, though undiſcernedly, ſome active and fugitive particles, whoſe preſence was requiſite to contain the concrete under ſuch a determinate form; as we ſee in wine degenerating into vinegar, where the change ſeems to proceed from this, that upon the avolation, or (if I may ſo ſpeak) depreſſion, of ſome ſubtile ſulphureous ſpirits, whoſe receſs or degeneration is not to be perceived by any ſenſible diminution of bulk in the liquor, the remaining parts fall into new leagues or diſpoſitions, and conſtitute an acid liquor ſomewhat fixed and corroſive, and conſequently of qualities very differing from thoſe of the wine, whoſe ſouring produced it: as we more fully declare in our experiments relating to fermentation.

SECT. XXXV.

And certainly there is, as we formerly ſaid, ſo artificial a contrivance of particles requiſite to the conſtitution of the organical parts of living bodies, that it will be ſcarce poſſible for human art or induſtry to imitate, ſo as to equal thoſe exquiſite productions of nature. And therefore I wonder not, that the ſtory of the Phœnix's reſurrection out of her own aſhes ſhould, by the beſt naturaliſts, be thought a meer fiction. And if that relation, mentioned by the inquiſitive *Kircherus* as an eye-witneſs of the re-production (if I may ſo call it) of ſhell-fiſhes near the brink of a lake in the Sicilian promontory *Peloro*, by the watering of their broken bodies with ſalt-water in the ſpring, be ſtrictly true, it ſeems much more improbable, that ſuch changes and viciſſitudes ſhould be bare redintegration of the diſſociated parts of ſuch reſtored bodies; than that (according to what we * elſewhere teach) they ſhould be new productions made by ſome ſeminal particles undiſcernedly lurking in ſome part of the deſtroyed body, and afterwards excited and aſſiſted by a genial and cheriſhing heat ſo to act upon the fit and obſequious matter, wherein it was harboured, as to organize and faſhion that diſpoſed matter according to the exigencies of its own nature. For that in ſome bodies the ſeminal particles may a while ſurvive the ſeeming deſtruction of life, is not altogether without example, as we elſewhere profeſſedly manifeſt. And in *Kircher*'s ſtory it is to be obſerved, that the reſtored animals were but ſhell-fiſh; in whoſe ſlimy and viſcous ſubſtance the ſpirits and prolific parts are probably both more diffuſed and kept from being eaſily diſſipable; to which I know not whether it will be worth while to ſubjoin, that in ſuch fiſhes the mechanical contrivance is but very plain, and, as it were, ſlight and obvious, in compariſon of the exquiſitely elaborated parts of more perfect animals.

Lib. 3. de Arte Magnet. Par. 5. cap. 3.

SECT. XXXVI.

The laſt obſervable, *Pyrophilus*, that we ſhall at preſent take notice of in our experiment, ſhall be this, that it may thereby ſeem probable, that ſome chymical reme-

* In ſome papers about the origin of living creatures ſuppoſed to ſpring of themſelves.

dies

dies may be too rashly rejected by physicians, because oil or spirit of vitriol, aqua regis, or other corrosive liquors, have been employed in their preparation: for it is confidently affirmed by many physicians, and but faintly denied by some chymists, that the corrosive menstruums made use of in the preparation of remedies, can never be so exquisitely washed off from them, but that some of the salts will adhere to the medicines, and perniciously display their corrosive nature in the body of him, that takes them. And it is not to be denied, but that many ignorant and venturous chymists do unskilfully (and therefore dangerously enough) imploy corrosives sometimes without any necessity or real advantage to invite them to it, and sometimes without sufficiently freeing their medicines from the corroding salts, by whose assistance they were prepared. For it is not always the frequency of ablution, though with warm water, that will suffice to carry off the salts from some bodies; and therefore those great artists *Helmont* and *Paracelsus* prescribe some things to be dulcified by the abstraction of the water of whites of eggs (which, though it seemed insipid, hath been found a great disarmer of corrosive salts) and others by the frequent distillation of spirit of wine, which indeed (not to mention the balsamic parts it may leave behind) we have observed to have a faculty of carrying up with it the saline particles of spirit of vinegar adhering to some chymical remedies. But all this notwithstanding, *Pyrophilus*, there may be several bodies (and perhaps more than are commonly taken notice of) which quite alter the nature of the acid salts employed to prepare them, by occasioning those salts to degenerate into another nature, upon the very act of corroding, or else by so associating their own salts with one of those of the dissolving menstruum, that from the coalition of both, there emerges a third body of differing in qualities from either. As in our experiment we find, that the spirit of petre, which is much more sharp and corrosive than the strongest distilled vinegar, and the fixed nitre, which is caustic like salt of tartar, and may, I suppose, well serve for a potential cautery (as surgeons speak) do by their mutual action work themselves into saltpetre, which is far enough from having any eminently fretting quality, and may be safely taken inwardly in a much greater dose than either of its ingredients.

SECT. XXXVII.

How much corrosive salts may dulcify themselves by corroding some bodies, you may easily try, by pouring distilled vinegar, or moderate spirit of vitriol, upon a competent proportion of corals, or crabs eyes, or pearls (or, as I suppose, almost any testaceous body). And for my part, though I am very shy of imploying corrosive liquors in the preparation of medicines; yet I have lately given a preparation of refined silver made with aqua fortis itself, or spirit of nitre, not only innocently, but with such success, that a couple of experienced physicians themselves, that were troubled with a superfluity of serous humours, sent to request it of me for their own use.

SECT. XXXVIII.

It were therefore worth while, in every preparation, where corrosive liquors are wont to be employed, or may seem requisite to be so, to consider the distinct nature of the particular bodies to be wrought upon, or consult experience, whether or no the acid menstruum do communicate to the concrete any particles capable of retaining

their fretting quality after the end of the operation; or whether or no the ſalts do not ſo ſpend and tire themſelves in the act of corroding, that being as it were ſheathed, they become unable to corrode any further; or whether or no the menſtruum do not in the body to be corroded meet with ſome ſuch ſaline particles, as may with it conſtitute a new and inoffenſive ſubſtance, as when ſpirit of vinegar, by corroding calcined lead, is turned with it into a ſalt, not of an acid, but a ſaccharine taſte, ſuch as invited chymiſts to give it the name of ſugar of *Saturn*. In the former of theſe caſes the medicine may be dangerous, unleſs it be after the ſolution or corroſion ended exquiſitely dulcified from all remainder of the corroſive ſalts. But in the two latter caſes the remedies may, in ſpite of the corroſiveneſs of the menſtruums employed about them, be ſafe and innocent enough; for it matters not much, how ſharp and fretting the ſevered ingredients of a remedy were, provided the remedy itſelf reſulting from them be not ſo. And whereas it is objected, that in divers of theſe remedies the corroſive ſalts are not really deſtroyed, but only diſguiſed, becauſe by diſtillation it is poſſible to ſeparate from them the liquors uſed about them as corroſive as ever; it may eaſily be replied, that beſides that, in ſeveral medicines, the matter of fact will not hold in divers others, the objection built on it is much more ſpecious than ſolid: for it very little concerns us to be ſure, that out of the medicines we take or give, the violence of fire cannot ſeparate corroſive ſalts; provided we be duly ſatisfied, that no ſuch ſeparation can be made by the heat or juices of a human body. And therefore, though it have been affirmed, that tartarum vitriolatum would upon the urgent ſollicitation of a ſtrong fire, part with much of (that moſt fretting liquor upon animal ſubſtances) the oil of vitriol, that concurred to its production; yet our beſt and warieſt phyſicians, not only chymiſts but methodiſts, ſcruple not to give it inwardly in ſeveral conſtitutions and diſtempers. And to end this diſcourſe with the experiment, that began it, we clearly ſee, that ſalt-petre is frequently and innoxiouſly given inwardly, though the ſalt, that makes even aqua fortis ſo corroſive, be the principal ingredient of it, and may by diſtillation be driven from it.

SECT. XXXIX.

IT would not have been very uneaſy for me, *Pyrophilus*, to have added to divers particulars of the paſt diſcourſe, experiments and conſiderations tending to countenance or illuſtrate the reflections therein ſet down: but in the firſt place, I wanted leiſure to expatiate; in the ſecond place, I was unwilling to anticipate what I have to ſay to you in other eſſays, eſpecially expecting to have elſewhere occaſion to make mention of ſalt-petre. And beſides all this, I am (to tell you the truth) deſirous to impoſe on you a kind of neceſſity of proſecuting this experiment, further than, when I made it, I had opportunity to do. For as I am apt to think it may prove a noble one, ſo I am ſufficiently ſenſible of my having not yet been able to look into the bottom of it; and that very ſenſe of my own ignorance helped to keep me from lengthening your trouble in this eſſay, leſt by ſolemnly endeavouring to countenance my conjectures, I might be thought dogmatical in a haſty ſcribble, wherein it is much more my deſign to awaken and engage your curioſity. than acquaint you with my opinions. And yet I thought it not amiſs to mention the paſt conſiderations, ſuch as they are, partly becauſe this one inſtance ſeems ſo fairly to accommodate more than one notion of the intelligible philoſophy, which ſeems hitherto not to have ſo much as employed, much leſs produced, any ſtore of experiments; and partly, becauſe

because I would have you take notice, that more observables than one may sometimes be very reasonably sought for in a single experiment. And perhaps too, I was willing by my spending a whole essay upon one experiment, without allowing myself to wander often from it, to invite you to think with me, that experiments ought to be estimated by their value, not their number; and that a single experiment, I say not such as that the last essay treats of, but in general, such as, it may be, may as well deserve an entire treatise, as a great many less considerable ones. As one of those large and orient pearls, that are fit to adorn a monarch's crown, may out-value a very great number of those little (though true) pearls, that are to be bought by the ounce in goldsmiths and apothecaries shops.

SECT. XL.

Having newly met, *Pyrophilus*, with some small treatises freshly published by *Glauberus*, and not having now the leisure to consider, or indeed so much as to peruse, much less the opportunity to make trial of divers particulars, which by turning over the leaves of the book, I find mentioned by him in relation to salt-petre, I must recommend to you the care of examining the particulars he delivers; and trying how far some of them may serve to correct, or to confirm, and how far others may be corrected by what has been in the past discourse set down concerning salt-petre upon experiments; some of whose fruits I can yet shew you, which were made upon the account of the divisibleness of nitre into fixed and volatile parts, long before the publication of *Glauber*'s treatises.

THE

HISTORY

OF

FLUIDITY and FIRMNESS.

Advertisements concerning the following Treatise.

THAT the author in those animadversions upon the *Essay touching Salt-petre*, whereof the ensuing treatise makes a part, might with the more freedom and conveniency add, alter, and even retract as he should see cause, he thought fit to write them, as if they were made on the work of another.

THE author hopes, that the equitable reader, considering that the following particulars touching fluidity and firmness were first written but by way of annotations upon the beginning of the above-mentioned essay, will excuse the unaccurateness of the method, as a fault scarce evitable on that occasion. It is also hoped, that if the reader will remember, that he was told in the preface to the newly-mentioned essay, that most of those, whom to gratify that treatise and the ensuing notes on it were written, were addicted to the Epicurean philosophy; the author's explicating things chiefly according to the atomical principles will not be thought strange, nor be looked upon as a sure argument of his being wedded to the particular opinions, wherein the Atomists differ from other modern Naturalists: especially, since he has on some occasions plainly enough intimated the contrary, by proposing, together with the atomical ways of resolving a thing, another explication more agreeable to the Cartesian, or some other modern hypothesis.

THE following tract was entitled, *A History of Fluidity and Firmness*, because indeed the having set down experiments and other matters of fact relating to the subjects treated of is the main, though not the only thing the author dares pretend to have done in it. And he styles the history, as it now comes abroad, *Begun*; partly, because he would invite abler pens to contribute their observations towards the compleating of what he is sensible he has but begun; and partly because he may hereafter if God permit, do something of that kind himself.

AND lastly, the author, though troubled that he can do it, dares not but advertise the reader, that some pages, partly a little after the beginning, and partly about the middle of the following treatise, having been lost through the negligence or mistake of him, to whose care the sheets, whereon it was written, were committed; he fears he has not been able, otherwise than very lamely and imperfectly, to repair that loss out of his memory.

THE
History of FLUIDITY and FIRMNESS.

The FIRST PART.

Of FLUIDITY.

SECTION I.

WHETHER philosophers might not have done better in making fluidity and firmness rather states than qualities of bodies, we will not now examine. But under which soever of the two notions we look upon them, it is manifest enough, that they are to be reckoned amongst the most general affections of the conventions or associations of several particles of matter into bodies of any certain denomination, there being scarce any distinct portion of matter in the world, that is not either fluid, or else stable or consistent. And therefore, I presume, it may be well worth while to consider, what may be the general causes of these two states, qualities, or affections of matter; and to try, whether by associating chymical experiments to philosophical notions, there may not be given at least a more intelligible and more practical account of both these subjects, than has been hitherto afforded us by the doctrine of the schools, which is wont to appear very unsatisfactory to discerning men; many of whom look upon what is wont to be taught by the Peripatetics, concerning fluidity and firmness, as well as other qualities, to be partly too general to teach us much, and partly too obscure to be understood. And that which at present invites us to this inquiry is, chiefly, that some circumstances of our author's experiment, touching saltpetre, may afford us some useful assistance in our designed search. For though the chief phænomena and circumstances of the experiment may be thought principally to respect fluidity; yet since that and firmness are contrary qualities, and since it is truly, as well as commonly, said, that contraries surveyed together serve to illustrate each other, it may reasonably be hoped, that what the circumstances just now related may give to the nature of fluidity, may facilitate the knowledge of that of compactness: nevertheless, we shall often be obliged to treat of these two qualities together, because the experiments we are to produce do many of them relate to both.

SECT. II.

A BODY then seems to be fluid, chiefly upon this account, that it consists of corpuscles, that touching one another in some parts only of their surfaces (and so being incontiguous in the rest) and separately agitated to and fro, can by reason of the numerous pores or spaces necessarily left betwixt their contiguous parts, easily glide along each other's superficies, and by reason of their motion diffuse themselves, till they meet with some hard or resting body; to whose internal surface, by virtue of that motion,

motion, their ſmallneſs, and either their gravity, or ſomething analogous or equivalent to it, they exquiſitely, as to ſenſe, accommodate themſelves.

SECT. III.

WHAT notion *Epicurus*, and the ancient Atomiſts his followers, had of fluid bodies, may be learned from theſe verſes of his paraphraſt *Lucretius*:

Illa autem debent ex lævibus atque rotundis
Eſſe magis, fluido quæ corpore liquida conſtant.
Nec retinentur enim inter ſe glomeramina quæque,
Et procurſus item in proclive volubilis extat.

And indeed, it is probable enough, that in divers liquors the little ſurfaces of the component particles are ſmooth and ſlippery, and that their being ſo does much facilitate the gliding of the corpuſcles among themſelves; and conſequently, the fluidity of the body they compoſe. Nor is it to be denied, that the ſpherical figure of ſuch corpuſcles may alſo conduce to their eaſy rolling upon one another: but there are divers other figures, which may make the little bodies endowed with them voluble enough to conſtitute a fluid ſubſtance. And the other qualities to be met with in divers liquid ſubſtances, and even in water itſelf, and oil, ſeem to argue their parts to be otherwiſe ſhaped; and thoſe fluid bodies, which are not liquors, as air and fire, ſeem to be compoſed of particles not all or moſt of them round, but of very various, and ſometimes of very irregular figures; and yet that ſuch bodies deſerve to be called fluid ones, will be manifeſt anon. And that they make a much more conſiderable part of the univerſe, than thoſe that are wont to be called liquors, may be argued from hence, that except the earth, the planets, and perhaps too the fixed ſtars, the reſt of the world, as vaſt as it is, ſeems to conſiſt chiefly, if not only, of an ætherial, thin, and fluid ſubſtance, as may appear (to omit other arguments) by what later aſtronomers have obſerved concerning the free and unreſiſted motion of ſuch comets as have by a trajection through the æther, for a long time wandered through the celeſtial or interſtellar part of the univerſe.

SECT. IV.

AND here let us obſerve, that it is not neceſſary to the fluidity of a body, nay, nor to its appearing fluid to the eye itſelf, that the corpuſcles it conſiſts of be crouded as cloſe together as they are wont to be in water, and other bodies, that are commonly looked upon as the only liquors. For though a parcel of matter no bigger than a grain of corn, being rarified into ſmoke, will poſſeſs an incomparably greater ſpace than it did before; and though, if a body be further rarified into flame, its expanſion will be yet much greater: yet both ſmoke and flame may be ſo ordered, as to appear like liquors. We have practiſed divers ways, to make the fumes of bodies acquire a viſibly-level ſuperficies like water; but the eaſieſt, though not perhaps the beſt, is this, (part of which I remember I have ſeen performed as a kind of trick by a very ingenious perſon:) The mouth being filled with the ſmoke of roſemary (that happening to be at hand when I made the experiment) if this ſmoke be plentifully blown into a glaſs pipe of an indifferent ſize, and open at both ends; and if when it is well filled with ſmoke, the lower end be preſently ſtopped, and the glaſs be kept ſtill a

while in an erected posture, the fumes will settle by degrees to a level superficies like water: so that, though you gently incline the pipe any way, the upper surface of the smoke will nevertheless quickly grow parallel to the horizon. And if the glass be further (but slowly) made to stoop, the smoke will seem to run down in a body like water, whilst it continues in the pipe; though when it is come to the lower end of it, instead of dropping down like water, it will commonly rather fly upwards, and disperse itself into the air. And as for flame, I foresee I shall ere long have occasion to mention an experiment, whereby I have sometimes endeavoured to shew, that even two contiguous flames, as expanded bodies as they are, and as open as their texture is, may like visible fluids of a differing kind retain distinct surfaces.

SECT. V.

But instead of examining any further, how many bodies are or may be made visibly to appear fluid ones; let us now resume the consideration of what it is that makes bodies fluid: especially, since having intimated some of the reasons, why we are unwilling to confine ourselves to the Epicurean notion, we hope it will the less be disliked, that we thought fit to make such a description of a fluid substance, as may intimate, that we conceive the conditions of it to be chiefly these three.

The first is the littleness of the bodies that compose it: for in big parcels of matter, besides the greater inequalities or roughnesses, that are usual upon their surfaces, and may hinder the easy sliding of those bodies along one another; and besides that divers other affections of a fluid body cannot well belong to an aggregate of gross lumps of matter; besides these things, I say, the bulk itself is apt to make them so heavy, that they cannot be agitated by the power of those causes (whatever they be) that make the minute parts of fluid bodies move so freely up and down among themselves: whereas it would scarce be believed, how much the smallness of parts may facilitate their being easily put into motion, and kept in it, if we were not able to confirm it by chymical experiments. But we see that lead, quicksilver, and even gold itself, though whilst they are of a sensible bulk, they will readily sink to the bottom of aqua regis, or any other such liquor; yet when the menstruum has corroded them, or fretted them asunder into very minute parts, those minute corpuscles grow then so much more capable of agitation than before, that quitting the bottom of the liquor, they are carried freely every way, and to the top, with the associated parts of the liquor, without falling back again to the bottom, Nay, we see, that ponderous and mineral bodies divided into corpuscles small enough may be made so light and voluble, as to become ingredients even of distilled liquors; as we may learn by what some chymists call the butter, others (simply) the oil, and others the *oleum glaciale* of antimony; which, though it be after rectification a very limpid liquor, yet does in great part consist of the very body of the antimony, as may appear (not to mention its weight) by this, that it is most easy to precipitate out of it with fair water store of ponderous white calx, reducible by art to an antimonial glass. Nay, we make a menstruum, with which we can easily at the first or second distillation bring over gold enough, to make the distilled liquor appear and continue enobled with a golden colour.

And to show yet more particularly, that great bodies are too unwieldy to constitute fluid ones; we may further observe, how as well nature as art, when either of them makes bodies of considerable bulk fluid, is wont, in order thereunto, to make a comminution of them, as we may observe in divers examples.

SECT. VI.

THUS we ſee, that in the ſtomachs of dogs, nature, to reduce bones into thoſe fluid bodies, chyle and blood, does by ſome powerful and appropriated juice, whether belonging to the ſtomach itſelf, or thrown out of the arteries in the paſſage of the circulating blood) diſſolve them into parts ſo minute, that the acuteſt eye would not tempt a man to ſuſpect, that ſuch a liquor had ever been a bone. And that it may not be objected, that this diſſolution is chiefly performed, or at leaſt muſt always be aſſiſted by the liquor, which animals take into their ſtomachs by drinking; I ſhall repreſent not only, that we find by experience, how little common water, the only uſual drink of dogs, wolves, *&c.* is able to diſſolve bones, though they be very long not macerated, but boiled in it; but that (if we may believe natural hiſtorians and credible travellers) there are ſome ſorts of animals, as particularly camels, that may be brought not to drink once in many days, even when they travel in hot climates. And to make you think this the leſs improbable, I ſhall add, that I am familiarly acquainted with an ingenious gentleman, who, as himſelf and an ancient virtuoſo, in whoſe houſe he lives, have informed me, does uſually drink but once in ſeveral days, and then no exceſſive draught neither. And when I aſked him, how long he had actually abſtained not barely from drink, but from thirſting after it? he anſwered, that he had once (ſome few years before) continued about nine days without either taking or needing any drink; and he doubted not, but that he might have continued much longer in that ſtate, if by diſtempering himſelf one night with long and hard ſtudy he had not had ſome light inclination to take a ſmall draught, which ſerved him for about four days longer. And when I aſked him, whether that hot ſummer's day, that preceded the evening, wherein he happened to tell me this, he had not drunk at all? he anſwered negatively. And it adds to the ſtrangeneſs of this peculiarity, that this gentleman is in the flower of his youth, being but about twenty-two years of age, and of a ſanguine and florid complexion. And (to annex that alſo upon the by) I learned by inquiry from him, that he ſweats freely enough, as I remember I ſaw him do; that his diet is the ſame with other men's, without reſtraining him from the free uſe of ſalt meats; and that his urine is in quantity much like that of ordinary men of his age and temperament. But to return to what I was ſaying more generally of the ſtomachical menſtruum of animals; I ſhall add on this occaſion, that to make ſome kind of imitation of it, I prepared, and do elſewhere mention and teach a certain liquor, that I uſe, whereby I have in a ſhort time, and without fire, diſſociated the parts of roaſted or boiled fleſh, bread, fruit, *&c.* and pulled them aſunder into very minute bodies, whereby I have reduced ſometimes one, ſometimes another of them, together with the menſtruum (which needs not much exceed them in bulk) to the conſiſtence of a fluid body.

WE ſee likewiſe, that fuſion makes metals fluid, and in fuſion there is manifeſtly a comminution of the melted body; the heat alone of gold, ſilver, or iron, though increaſed even to ignition, being not able to make thoſe metals become fluid, whilſt they continue in maſſes of any ſenſible bulk. To which I ſhall add anon, that even melted metals may have their fluidity increaſed by a yet further comminution of their parts.

SECT.

SECT. VII.

And to resume here the consideration of that very difficult question, which we have elsewhere mentioned, it seems well worth inquiry, whence it happens, that in the distillation of common salt and other saline bodies, which not only are not fluid, but are hard even to brittleness, there will yet be obtained a perfect and permanent liquor, and from some of them a very considerable proportion of it. In answer to this question it may indeed be said, that in divers dry bodies, such as harts-horn, wood and bones, committed to distillation, the fire does no more than separate the aqueous or other liquid parts from the others, wherewith they were blended in the concrete, and bring them together into the receiver, where they convene into a liquor. But besides that this itself is perhaps more easily said than proved, it does not reach the propounded difficulty: for with what probability can it be affirmed of bodies, that have been already calcined or melted? such as are the red calx of vitriol, and fluxed sea-salt, *&c.* which yet afford liquors, though their aqueous and other looser parts have been already driven away by a strong fire before their being exposed to distillation. I have sometimes then considered, whether it may not seem less improbable to conjecture, that the vehement agitation produced in such bodies by the violence of heat does both divide them into minute corpuscles, and drive over swarms of them into the cold receiver, where losing their former vehemence of agitation, they are reduced into a liquor, chiefly (for I would not exclude concurrent causes) by reason that the fire happened to rend the concrete into parts, by their extreme littleness, or their shape, or both, so easy to be tumbled up and down, that the wonted agitation of the air, propagated by the interposed bodies or medium, or else that the same cause, whatever it be, that gives the air its wonted agitation, is able to give such minute corpuscles enough of it to keep them fluid.

SECT. VIII.

That there is constantly in the air a various motion of the small parts, will be anon declared. That also some bodies will be kept fluid by a much less measure of agitation than is requisite to others, seems probable from hence, that wine will continue a liquor in such a languid warmth of the air, as will not keep the parts of water moving, but permit them to rest in the form of ice. And in cold countries, where wine itself would congeal, (as I have by art made it do here in *England)* it is observed, that though the more aqueous parts will by the loss of their motion be turned into ice, yet the more subtile and spirituous parts remain unfrozen; and so do divers other liquors (especially chymical) of very subtle and voluble parts. And the corpuscles, that chiefly compose that body, which is properly called the air, though it appears by weather-glasses, that cold may very much contribute to condense it, (that is, to occasion the approach of its parts to one another, or, reduce them to a closer order) have not been observed to be frozen by any degree of cold whatsoever; which seems to proceed from hence, that by reason of their extreme littleness, (not excluding their figure) there cannot be so little of agitation about the earth, as not to be sufficient to continue bodies, and consequently to keep them fluid.

Now,

Now, that likewise it is possible, that a saline spirit should consist at least in great part of very minute grains of salt, we elsewhere declare; where it is taught, that a sal-armoniac may be made by spirit of urine and spirit of salt, as the common sal-armoniac is made with crude salt: and there a way is also shewn, how these two salts, (the urinous and the other) as strictly as they are united in the compound, may be readily divorced. And agreeably to this I observe, that as (according to what I elsewhere note) a common aqua fortis may be enabled to dissolve gold, (on which of itself it will not fasten) by the addition of spirit of salt; so I find, that common crude salt barely dissolved in it will give it the like power of working upon gold. Nay, I have tried, that crude nitre, dissolved in good spirit of salt, may make it serve for an aqua regis. And I remember on this occasion, that having enquired of the most noted person in *Holland* for the distilling of corrosive waters, what was the greatest proportion of distilled liquors, that ever he was able to obtain from sea-salt; he (though a man not given so much as to boasting) affirmed to me, that by using instead of the ordinary *Caput mortuum*, as brick-dust, sand, *&c.* (that chymists are wont to mingle with salt before they distil it) a certain whitish clay, he had sometimes brought over almost the whole body of salt into a liquor; insomuch that from a pound of salt he could draw, and that without any extraordinary trouble or degree of fire, fourteen ounces of liquor. And when, because I suspected, that much of this might be water forced from the clay mingled with the salt, I enquired, whether he had ever dephlegmed this liquor; he answered me, that he had purposely done it; and sometimes found no less than about twelve ounces of it to be strong rectified spirit: which brought into my mind that almost incredible passage of *Beguinus*, who somewhere teaching the distillation of another salt, adds to the end of his directions, that if you have wrought well, you shall get from a pound of the matter a pound of spirit. But because from all these liquors, distilled from such kind of salts, it is possible either by rectification, or some more philosophical way, to obtain a portion of phlegm or water, I leave it to further enquiry, whether or no the fluidity of these distilled liquors may not in divers cases be in part furthered by the mixture of some particles of an aqueous nature, (such being fit to make dissolvers and vehicles for salts) which may not absurdly be suspected to have been produced by the action of the fire, upon the concrete committed to distillation; if we allow, with that famous chymist *Helmont*, that by the alkahest all gross bodies may be totally, and that, without it, even oil and salt may, in great part (and that without additaments) be reduced into insipid water.

SECT. IX.

WE shall anon, (when we come to treat of firmness) mention our having made a certain substance so disposed to fluidity, that it may be made to change the stable consistence for a liquid one, by so small an agitation, as only the surplusage of that, which the ambient air is wont to have about the middle even of a winter's day, above what it hath in the first or latter part of it. Nay, we have made even a metalline salt or vitriol capable of this proclivity to liquefaction, of which we have unquestionable witnesses. And therefore, it need not appear incredible, that other heaps or aggregates of corpuscles much lighter than these, though heavier than those of the air, may have all their parts so minute and fitted for motion, that the wonted agitation of the air may not only about noon, but at all other times of the day, keep them in motion, and thereby in the state of fluidity.

SECT.

SECT. X.

And here I must add, that it was not altogether without cause, that I lately took notice of the shapes as well as the sizes of bodies, in reference to their fitness to constitute fluid ones. For though I be not sure, but that in those bodies, as sal-armoniac, antimony, *&c.* which are by the fire sublimed into flowers rather than distilled into liquors, the magnitude of the component corpuscles may not be a hinderance to the fluidity of the body they constitute; yet this seems as probably referable to their figure, unapt for the requisite motion, as to their bulk. And I have sometimes made to this purpose this experiment; that by slowly distilling oil-olive *per se* in a glass-retort, (placed in sand) I found, as I expected, that about the third part of the oil, which was driven over into the receiver, did there coagulate into a whitish body almost like butter. So that although it seemed manifest by the strong smell and very piercing taste of this white substance, that the oil, which afforded it, had its particles, as it were, torn in pieces; and though distillation be wont to obtain liquors even from consistent bodies; yet in our experiment of a concrete, that is naturally fluid, the distilled liquor itself proves not to be so: of which no cause seems more obvious, than that the newly-acquired shape of the dissipated parts of the oily corpuscles makes them unfit for motion; either absolutely speaking, or at least in respect of one another, by making them less pliant than formerly, or giving them a figure more easy to be entangled with the neighbouring corpuscles, or else by making their surfaces less smooth and slippery than before.

SECT. XI.

But to return thither, whence we have digressed, and mention some more familiar examples of the conducivenes of the smallness of a body's disjoined parts to its fluidity, we may take notice, that of bodies, that consist of incoherent parts, and are made up, as it were, by aggregation, those *de cæteris partibus*, in their being poured out, most resemble liquors, that are the smallest; as would appear upon the emptying several sacks, the one of apples, the other of walnuts, the third of filberts, the fourth of corn, the fifth of sand, and the sixth of flour.

Confectioners also, cooks, and others, that make much use of whites of eggs, will easily reduce those clammy and vicious bodies into a thin and fluid substance, to which, for its affinity with water, many give the same name: and yet this difference of fluidity being effected only by long and skilfully beating the mass with a whisk, or even with a spoon, seems to be produced but by pulling asunder the parts (which, perhaps, before were long and somewhat twined) and breaking them into a shorter or lesser, and consequently more voluble ones. And I remember, I have seen a good quantity of that jelly, that is sometimes found on the ground, and by the vulgar called a star-shoot, as if it remained upon the extinction of a falling star, which being brought to an eminent physician of my acquaintance, he lightly digested it in a well-stopt glass for a long time, and by that alone resolved it into a permanent liquor, which he extols as a specifick to be outwardly applied against wens.

SECT.

SECT. XII.

AND here we will subjoin an observation afforded us by the art of casting, which has sometimes yielded us a not unpleasant diversion. It is observed then by goldsmiths well versed in that art, (and has been recommended to me by an artificer eminently skilful in it) as one of the chief remarks belonging to it; that when any such curious work of silver is to be cast, as requires, that the impression of hairs or very slender lines be taken off by the metal, it is not enough, that the silver be barely melted; but it must be kept a considerable while in a strong fusion: for if it be too soon poured out, the figure it will make will be but blunt; whereas if it be kept a competent time in fusion, the matter becoming thereby more liquid as well as hotter, will be thin enough to run into the smallest cavities of the mould, and so receive a figuration even from the delicatest of them. Whence it may probably be deduced, that some bodies already fluid may, by a further comminution of their parts, be made yet more fluid. The like increase of fluidity may be observed in some other fluid bodies, especially unctuous ones, as turpentine, oil, *&c.* when heat begins to break as well as agitate their parts.

I MAY elsewhere have occasion to mention, how, by the operation of the fire, the crystalline salt of urine may be reduced without additaments to a strong and ponderous liquor: though in this, as perhaps also in some of the former instances, it is not unlikely, that (as we may hereafter more particularly declare) there may concur to the produced change of consistence, some alteration in the figure of the corpuscles, whereof the firm body consisted.

AND if that be true, which *Helmont* in several places affirms of his prodigious liquor, alkahest, it is possible to turn plants, animals, stones, minerals, metals, or whatever kind you please of consistent body here below, into a liquor equiponderant to the resolved concrete: which (if granted) seems to argue, that the most solid body, by being divided into parts small enough to be put into motion by the causes, that keep those of water and other liquors in agitation, may become fluid bodies. And this intimation I shall add for the sake of philosophers, that barely by long digestions, (and much more if they be helped by seasonably-repeated distillations) in exactly stopped vessels, and a due degree of heat, there may be made in the parts of many bodies, both vegetable and animal, so great a change from the state of consistence to that of fluidity, as those, that contenting themselves with ordinary courses of chymistry, have not had a peculiar curiosity for trials of this nature, will not be forward to expect.

SECT. XIII.

THE second of the abovementioned three conditions is, that there be store of vacant spaces intercepted betwixt the component particles of the fluid body, or at least about those of them, that are superficial: for without this there will not be room for each of the corpuscles to continue its agitation upon the surfaces of the neighbouring ones; and there would be no cession of any, because there would be no place unpossessed for the impelled corpuscle to be received in. But when I speak of vacant spaces, ordinarily, (if not always) requisite to be intercepted betwixt the particles of fluid

bodies; I intend not to determine, whether or no such spaces should or may be vacuities properly so called; it being commonly sufficient to this second condition of a fluid body, that in the little spaces intercepted between those that either are, or at least are considered as solid parts, there be none but such as will easily yield to them, and cannot considerably resist the freedom of their motions.

WHICH being premised to keep this condition from being mistaken, we may in confirmation of it take notice, how snow, which at its first falling is of a loose and open texture, does easily yield to the impressions of the hand: but when by being strongly compressed and formed into balls, the little icy bodies it consists of are brought into a closer order, and many of them thrust into the little spaces formerly possessed only by the yielding air, they become unable to give way to the motions of our hand as before, but compose a hard and resisting body. We see also, that when water is strongly forced into and kept compressed in a bladder, so that its exterior particles have not about them as before the yielding air to give way to them, when they should, according to their wont, swell about the sides of the bodies that endeavour to press it inwards, it emulates a hard body, and resists such motions as otherwise it would readily yield to; unless a more easy cession be occasioned by the retching of the moistened bladder itself.

AND I chuse to instance in a bladder distended with water, rather than in one full of air, because, though this latter will also emulate a hard body, yet in this case the tension of the bladder would perhaps be ascribed to a kind of spring, which divers experiments have taught us to belong to the air: whence it might be said, that since the inclosed air will suffer itself to be thrust inward a good way, though it will quickly when permitted fly out again; the hardness of a well-blown bladder proceeds not from want of the rooms requisite to the cession of the aërial corpuscles, but to the motion of restitution natural to them; when like an innumerable company of little bows or springs, being bent by the force that compresses the sides of the bladder, they do, as soon as it is taken off, stretch themselves out again (some one way, some another) as far as is permitted them by the imprisoning bladder, which they thus every way keep strongly distended.

BUT this having of vacant spaces, or some yielding matter about the corpuscles of a fluid body, seems requisite to its being so, but as what in a school-term one may call a *Removens prohibens*; I mean, only as it obviates that impediment to their motion, which exquisite fulness may be conceived to give to the various glidings amongst themselves of the parts of a body supposed to be perfectly of the same hardness or softness, or, if you please, altogether equally disposed or indisposed to yield to one another. And although in such bodies, as water, wine, oil, quicksilver, and the like, that are generally agreed upon to be fluid liquors, it will I presume be granted, that this second condition we have been speaking of may take place; yet I will not say, that it were altogether absurd to question, whether there may not be a portion of matter consisting of parts so minute, and so agitated, and consequently so easy to be either crumbled into yet smaller parts, or squeezed into any figure as occasion requires, that they may incessantly change places among themselves, and thereby constitute a most fluid body, without any vacuities, receptacles, or yielding matter about them, unless perhaps it be about the exterior parts of those of them, that from time to time happen to be the superficial corpuscles of this thinnest liquor. But though we have said, that this may be questioned without absurdity, yet it will not so much concern us in this place to examine, whether the affirmative may be rationally

tionally maintained, as to proceed to consider what is farther requisite to that state of matter we are now treating of, especially the qualification yet unmentioned seeming to be the principal of all.

SECT. XIV.

For the third and chief condition of a fluid body is, that the particles it consists of be agitated variously and apart, whether by their own innate and inherent motion, or by some thinner substance, that tumbles them about in its passage through them. For this seems to be the main difference betwixt solid ice and fluid water, that in the one the parts (whether by any newly-acquired texture, or for want of sufficient heat to keep them in motion) being at rest against one another, resist those endeavours of our fingers to displace them, to which in the other, the parts being already in motion, easily give way. For whereas in the ice, every part actually at rest must by the law of nature continue so, till it be put out of it by an external force capable to surmount its resistance to a change of its present state; in water each corpuscle being actually (though but slowly) moved, we need not begin or produce a new motion in it, but only byass or direct that, which it has already, which many familiar instances manifest to be a much easier task. From this agitation of the small parts of liquors it comes to pass, that these little bodies, to continue their motion, do almost incessantly change places, and glide sometimes over, sometime under, and sometimes by the sides of one another. Hence also may be rendered a reason of the softness of fluid bodies, that is, their yielding to the touch: for the particles that compose them being small, incoherent, and variously moved, it can be no difficult matter (as we lately intimated) to thrust them out of those places, which being already in motion they were disposed to quit, especially there being vacant rooms at hand, ready to admit them as soon as they are displaced. And hence it likewise happens, that these little bodies must be very easily moveable any way upon the motion of the mass or liquor, which they compose; and that being very small, and moving so many ways, they cannot but (according to *Aristotle*'s definition of things fluid) be very unfit to bound themselves, but very easy to be bounded by any other firm body; for that hinders them from spreading any further: and yet to continue their various and diffusive motion as much as they can, (especially their gravity, at least here about the earth, equally depressing and thereby levelling as to sense their uppermost superficies) they must necessarily move to and fro, till their progress be stopped by the internal surface of the vessel, which by terminating their progress (or motion toward the same part) does consequently necessitate the liquor those little bodies compose, to accommodate itself exactly (for aught the eye is able to discern to the contrary) to its own figure.

SECT. XV.

This short and general account of fluidity may, we hope, be as well further explicated and illustrated, as confirmed, by the following instances and experiments; and therefore we shall forthwith proceed to them.

And it will be fit to mention in the first place those, that are afforded us by the body our author treats of, salt-petre; they having occasioned our writing about this subject.

SALT-PETRE then may be made fluid two ſeveral ways, either by, or without a liquor.

BY the intervention of a liquor it puts on the form of a fluid body, when being diſſolved in water or aqueous juices, it is not by the eye diſtinguiſhable from the ſolvent body, and appears as fluid as it; which ſeems to proceed from hence, that the agitated particles of the water piercing into the joints or commiſſures of the corpuſcles of the ſalt, do disjoin them, and thereby divide the nitre into parts ſo ſmall, that it is eaſy for thoſe of the water, wherewith they are aſſociated, not only to ſupport them, but move them to and fro: whence it comes to paſs, that theſe particles being ſo ſmall, and ſwimming ſome one way, ſome another in the yielding body of water, make no ſuch reſiſtance againſt the motion either of a man's hand, or other external body, that ſtrives to diſplace them, as they did in their ſaline form.

BUT that with much leſs liquor a nitrous body may be rendered fluid, may appear to him, that ſhall expoſe ſuch fixed nitre, as our author teaches to make, to the moiſt air of a cellar: for there it will run *per deliquium*, (as chymiſts ſpeak) into a liquor, which conſiſts of no more aqueous particles, than are neceſſary to keep the ſaline ones (which ſeem to be much ſmaller than thoſe of unanalyzed nitre) in the agitation requiſite to fluidity.

SECT. XVI.

AND hence we may proceed to conſider, what fluidity ſalt-petre is capable of without the intercurrence of a liquor: and this may be two-fold. For firſt, if it be beaten into an impalpable powder, this powder, when it is poured out, will emulate a liquor, by reaſon that the ſmallneſs and incoherence of the parts do both make them eaſy to be put into motion, and make the pores they intercept ſo ſmall, that they ſeem not at a diſtance to interrupt the unity or continuity of the maſs or body. But this is but an imperfect fluidity, both becauſe the little grains or corpuſcles of ſalt, though eaſily enough moveable, are not always in actual motion; and becauſe they continue yet ſo big, that both they and the ſpaces intercepted betwixt them are, near at hand, perceivable by ſenſe. But if with a ſtrong fire you melt this powdered nitre, then each of the ſaline corpuſcles being ſub-divided into I know not how many others, and theſe inſenſible parts being variouſly agitated by the ſame heat, (both which may appear by their oftentimes piercing the crucible after fuſion, wherein they lay very quietly before it) the whole body will appear a perfect liquor, and be thought ſuch by any beholder, that ſhall judge of it but by the eye: and ſuch alſo is the fluidity of melted metals, in which, when they are brought to fuſion in vaſt quantities, I have ſeen the ſurface waved like that of boiling water, and ſometimes parcels of liquor thrown up a pretty way into the air. And not only fire, and other actually and manifeſtly hot bodies, are able to make ſome hard ones fluid, but it ſeems alſo, that ſome bodies may be brought to fluidity by others, which to the touch appear cold, if they be but fitted to change the texture of the hard body, and put its inflected parts into a convenient motion; as may be ſeen in the chymical experiment of turning the brittle body of camphire into an oil for the time, by letting it lie upon aqua fortis, which perhaps blends and complicates the formerly rigid particles, and puts them into ſuch a motion, that they do as well glide along as ſomewhat twine about each other. And I further tried, (not having found it mentioned by the chymiſts) that camphire may by a dexterous application of heat be brought in cloſe glaſſes,

glasses, both to flow, and to boil almost like oil. It is true, that these liquors taken from the fire quickly lose that name, and grow solid again. But the duration of a thing is not always necessary to denominate it such; for the leaf of a tree, for instance, whilst it flourishes, may be as truly green as an emerald, though the leaf will after a while wither and turn yellow, which the stone will never do: and in cold climates, where lakes, *&c.* at other times navigable, are sometimes frozen so hard, that carts and even great ordnance may safely be drawn over them, ice and water are the one a stable, and the other a liquid body, notwithstanding that the same portion of matter, which at one time is frozen into a hard and solid substance, was a little before a fluid body, and (now and then in a very short time) will be thawed into a liquor again.

SECT. XVII.

I know not whether it be requisite to take notice, that the fluidity, which salt-petre acquires upon fusion by fire, seems very much of kin to that, which is acquired by solution in water. But if fusion be made rather by the ingress and transcursions of the atoms of fire themselves, than by the bare propagation of that motion, with which the agitated particles that compose fire beat upon the out-side of the vessels, that contain the matter to be melted; in such case, I say, both those kinds or manners of fluidity newly ascribed to salt-petre will appear to be caused by the pervasion of a foreign body: only in dissolution the fluid body is a visible and palpable liquor, and consequently more gross, whereas in fusion the fluid substance that permeates it is more thin and subtile, and divides it into much smaller parts, and so adds very little to its bulk.

SECT. XVIII.

But because some scruple may possibly arise about this matter from hence, that the powder of nitre, how fine soever, seems fluid but just whilst it is pouring out, and even then is but very imperfectly so; and that as for fusion, that is wont to reduce the melted body to a new and permanent state, as the formerly-mentioned powder of salt-petre, which before fusion was but a heap of incoherent particles, is by it made a solid and considerably hard body: to prevent, I say, or remove such scruples, we will set down one experiment that we long since met with, as to the main, in the shops of stone-cutters, which, though unregarded by them, will excellently serve to make out what we mention it for. Take then good alabaster, or in defect of this, of that white stone, which is well known to our masons by the name of plaster of *Paris*, beat it very small, and put as many pounds as you please of the finely-searched powder into any flat-bottomed (and first well heated) vessel of brass or iron (bigger or lesser according to the quantity you intend to burn). Increase the fire by degrees, till it grow to be strong, and when the calorific atoms shall have in sufficient numbers pervaded the heap of powder, or, if you please, when the igneous corpuscles have by their numerous and brisk strokes upon the vessel communicated by its means their agitation to the inclosed powder, and when by either of these ways, or both, the fire (which may also resolve some of the more spirituous and exhalable parts, whereof distillation has shewn me, that alabaster is not destitute, into vapours) shall have put the little bodies it consists of into actual motion (which will be quickly done) you shall see it assume the form of a liquor, and boil with numerous great and confused waves just like a seething-pot. And if, whilst it continues in this state, you stir it with

with a ſtick, it will not like a heap of ſand, or, as itſelf would do at another time, reſiſt the motion thereof, but yield thereto like a liquor, and, like it, will ſeem to have ſomething of the nature of a coherent body: for by ſtirring it any thing ſtrongly near one ſide of the veſſel, you may make the waves beat very manifeſtly againſt the oppoſite part of it. And beſides all this, you may obſerve this further reſemblance betwixt this boiling matter and a liquor, that there will fly up out of the pot great ſtore of ſteams like ſmoke, but that they are white, which will ſometimes like ſmoke aſcend, for aught can be diſcerned, to the very top of the chimney, and leave its colour upon the places by which plenty of it hath paſt. Beſides, thoſe, that make this experiment often, as we have taken pleaſure to do, may have the opportunity to obſerve, that when the veſſel has continued ſo long over the fire, that the contained alabaſter relapſes into the form of a heavy moveleſs powder, by keeping it a while longer in the heat, it will for once at leaſt reſume the form of a fluid body, and boil again as before; the ſpirituous ſteams, whoſe avolation promoted the ebullition, being not yet quite ſpent. And laſtly, if when it ſeems moſt a liquor, you take up a little of it, and, as nimbly as you can, caſt it upon a ſheet of white paper, it will not at all wet it, but immediately diſcover itſelf to be a moveleſs incoherent powder, as it was before its being ſet over the fire. Whereby it (I hope) appears, that a heap or aggregate of ſuch little bodies as are neither ſperical nor otherwiſe regularly ſhaped, nor ſmall enough to be below the diſcernment of the eye, may, without either fuſion or being poured out, be made fluid barely by a ſufficiently ſtrong and various agitation (from what cauſe ſoever that proceed) of the particles that make it up, and loſe its fluidity immediately upon the ceaſing of it.

THUS have we ſeen, how very much it conduces to the making of a body fluid, that its ſmall parts be actually moved. And whence this motion proceeds, we ſhall not at preſent venture to determine. For though in the examples newly mentioned, and ſome others, moſt men will be forward to aſcribe the motion produced in the parts of the fluid bodies there mentioned, to the action of the fire, whereunto they were expoſed; yet what it is, that puts the parts of fluid bodies in general into the motion requiſite to make them ſuch, is a queſtion, of which the true reſolution indeed were very deſirable. But the full debate of it will not, I hope, be here expected from me, whilſt I am writing but notes, ſince it would engage me to diſcuſs two or three of the difficulteſt as well as the importanteſt controverſies belonging to natural philoſophy. For firſt, I ſhould be obliged to conſider whether motion, or a propenſity to it, be an inherent quality belonging to atoms in general, and not loſable by them; or whether all motion is communicated by impulſe from one body to another. And ſince thoſe, that of late have taught, that all viſible liquors, as water, oil, quick-ſilver, *&c.* owe their fluidity chiefly to the agitation of ſome thin and reſtleſs matter, which inceſſantly permeates them, do deduce the neceſſity of ſuch an ethereal ſubſtance principally from the impoſſibility, that there can be any vacuum properly ſo called in the univerſe, wherein yet are very many ſpaces unpoſſeſſed by either air or groſſer bodies than it: the examination of this ſubtile matter would draw on the conſideration of the nice controverſies, that perplex philoſophers concerning emptineſs, which it were more difficult for us to examine in few words, than it is neceſſary for us to meddle with them in this place; ſince not writing of the firſt principles of phyſiology, but of fluidity, which is but a ſecondary or derivative quality (if I may ſo call it) it ſeems ſufficient to give a notion of it, that we deduce it not from the unintelligible ſubſtantial form of the fluid bodies, but from thoſe ſimple and general affections of matter, the figure, ſituation, and motion of its ſmall parts.

SECT.

SECT. XIX.

WHEREFORE declining to add any thing in this place to what we have otherwhere discoursed concerning the origin of motion, and the possibility or impossibility of a vacuum; we will proceed to take notice, that there is one thing more, which we may learn from salt-petre touching the nature of fluidity, and that is the distinction betwixt a fluid body and a wetting liquor, which are wont, because they agree in many things, to be confounded, but inconsiderately enough: for though every wetting liquor be fluid, yet every fluid body does not wet. The air, the æther, and even flame itself may be properly called fluid bodies, according to the notion of fluidity hitherto made out, and yet will scarce by any man be called moist liquors; and salt-petre, whilst in fusion, is really a liquor, and so is every melted metal; and yet these wet not the bodies they touch, as do water and other wetting liquors, which are fluid bodies with this peculiar qualification, that they stick to and moisten the dry bodies, which they touch (or at least abound with some parts, which being separated from the rest and reduced to a liquor, will do so.) And, according to this notion, methinks, it may be conceived, that the humidity of a body is but a relative thing, and depends chiefly upon the congruity or incongruence of the component particles of the liquor in reference to the pores of those particular bodies, that it touches: for, sometimes the little eminences and pores of the surface of the dry body, on or against which the liquor flows, are of such magnitudes and figure, that the particles of the liquor find admittance into those pores, and are detained there (by which means they usually soften it) and sometimes the pores and asperities of the dry body's surface are so incommensurate in bigness and figure to the particles of the liquor, that they glide over the surface, without sticking or adhering firmly to any parts of it. This may be exemplified in quicksilver, which cannot be said to be a humid body in respect of our hands or clothes, or of almost all other bodies of the world, upon whose surfaces it will roll without leaving any of its particles lodged in their pores, or fastened to their little eminences, whence it is called by vulgar chymists, the water that wets not the hands: but in reference to divers metals, especially gold and tin, quicksilver may be said to be a humid liquor, for it insinuates itself into their pores, and thereby mollifies their bodies, as other liquors do those, that are moistened by them. And even water, that wets almost all other animal and vegetable, and many mineral bodies, besides that it is commonly enough observed to stand in almost globular drops upon cabbage-leaves, seems not a humid liquor in relation to the feathers of ducks, swans, and other water-fowl; whom nature having designed to fly sometimes in the air, and live sometimes in the water, she providently makes their feathers of such a texture, that they do not, like the feathers of divers other birds, admit the water, which imbibed would make them unfit for the use of flying. And it is observable, that upon the change of texture in a liquor, it may be brought to stick to the surface of a body, to which before it would not adhere; as may appear by this, that though quicksilver alone will not stick to glass, yet if there be mixt with it a due proportion of lead, tin, and tin-glass, though neither of them will adhere to glass, yet their liquid mixture (as we have often tried and elsewhere * taught) readily will, even without the assistance of heat.

* The writing here referred to, is the second tome of *The Usefulness of Experimental Philosophy*, which should have appeared before these specimens.

SECT.

SECT. XX.

If it be objected, that this various agitation of the insensible parts of water and resembling bodies, wherein we make the nature of fluidity chiefly to consist, is but an imaginary thing, and but precariously asserted, since by our own confession they are so small, that the particles themselves, and more, the diversity of their motions are imperceptible by sense, which represents water, for example, to us as one continued body, whose parts are at perfect rest:

If this, I say, be urged against our doctrine, we shall not deny the objection to be plausible, but must not acknowledge it to be unanswerable.

For of the seeming continuity of water and other liquors this may be the reason, that the particles, whereof the liquor consists, being too small to be visible, and being not only voluble, but in actual motion, the pores or vacant spaces intercepted between them must also be too little to be discerned by the eye, and consequently the body must appear an uninterrupted or continued one: not to mention, that, were the parts of the liquor less minute, their shifting of places would hardly be perceived by the eye, each displaced corpuscle being immediately succeeded by another like it. It is true, that a heap of grains of nitre, though upon its effusion out of the vessel it somewhat emulates a fluid body, does yet, when it rests in the vessel, appear to be but an aggregate of many little incoherent bodies heaped up together; because the intervals or holes left between them are great enough to affect the sense: but if the same salt be reduced into an alcohol (as the chymists speak) or impalpable powder, the particles and intercepted spaces being then extremely lessened, the body they make up will much more resemble an intire mass, though it be looked upon from a nearer distance; and so when this powder is by the fire further broken into parts incomparably smaller than those of the powder, and which consequently intercept such extremely little pores, that not only salt-petre, but some metals, and even gold itself (from which it will not be supposed that any thing exhales to lessen it) are by some affirmed (for I have not myself diligently enough observed it, and do yet doubt it) to take up rather less than more room melted than cold, why should we not grant, that these pores may be little enough, not any where to discontinue the body as to sense?

SECT. XXI.

And that the incoherent parts of fluid bodies are also diversly agitated, some this way, and some that way, though the sense cannot discern it, may be proved by their sensible operations *. For without such local motion, how could the particles of water pierce into the recesses of bodies, and occasion those putrefactive alterations that are wont to be imputed to superfluous moisture? and how comes it else to pass, that aqueous liquors so readily diffuse themselves into, and so exquisitely mingle with one another? as we see when red and white wine are in a trice confounded into claret:

* The author now finding, that something concerning the various motion of the parts of fluid bodies, which he has but touched upon, has been, though but briefly and without experiments, yet excellently explained in a mathematical way by Monsieur *des Cartes* in the 56th and 57th articles of the second part of his principles, thinks fit to refer the inquisitive reader thither for fuller satisfaction about that particular.

and

and without this various agitation of the parts of water, how could it be, that lumps of ſugar or ſalt caſt into it ſhould quickly be ſo perfectly diſſolved in it, that the lumps themſelves totally diſappear, and the diſſociated parts are carried about every way by thoſe of the water, even from the bottom to the very top? As is evident particularly in ſea-ſalt, which when the ſuperfluous liquor is ſufficiently exhaled, begins viſibly to coagulate, not at the bottom, but upon the ſurface of the water; and not only ſalt, but even gold itſelf, though the heavieſt of bodies, may have its parts ſo ſcattered by the agitation of thoſe waters, as experience has taught us, and as you may eaſily try by putting a little of the ſolution of gold, made in aqua regis, into fifteen or twenty times as much fair water, which will all thereby be immediately ennobled with a golden colour. That the little bodies, whereof flame conſiſts, are fiercely agitated, appears oftentimes even to the eye, and will ſcarce be denied by him, that conſiders the operations of it, and the vivid beams it darts round about it againſt the neighbouring bodies. And that the particles, that compoſe our common air, are alſo very diverſly agitated, we may be induced to believe by ſundry particulars. As firſt, by thoſe little moats, that from a ſhady place we ſee ſwimming up and down in the ſun-beams, and by the tremulous motion, which that of ſwarms of little bodies in the air ſeems to impart to diſtant objects looked on after ſun-riſe through a good teleſcope (and which by the bare eye in hot weather may be often diſcovered by certain very dilute ſhades, which ſeem to tremble upon the walls of high roofed halls and churches, and other ſpacious buildings.) Next (and more eaſily) by this, that if you take ſalt of tartar, firſt brought to fuſion, and place it in a cellar, or even in an ordinary room, it will in a ſhort time (now and then in a few minutes) begin to relent, and have its ſurface ſoftened by the imbibed moiſture of the air, wherein if it be left long enough, it will totally be diſſolved into clear liquor; which would not be, if the moiſt vapours that help to conſtitute the air, did not move to and fro every way, and were not thereby brought to the ſalt, and enabled to inſinuate themſelves into its pores, and by that means diſſolve it, and reduce it with themſelves into a liquor.

And even in ſummer, when the air is wont to be much dryer than at other ſeaſons of the year, one may quickly diſcover, that there are in the air ſtore of aqueous corpuſcles, moved ſome one way and ſome another, by the experiment of putting into a drinking-glaſs, for want of ice and ſnow, ſome beer or wine actually very cold: for thereby, after a while, the outſide will appear all bedewed with little drops of liquor; which ſeems plainly to be no other than the aqueous ſteams, that ſwimming up and down in great multitudes in the air, are by its agitation towards all parts carried, as every other way, ſo to the ſides of the glaſs, and being there condenſed by the coldneſs of that ſmooth body, turn into viſible and palpable water. And, if I much miſ-remember not, it was one of the circumſtances of the laſt experiment of this kind we have had occaſion to take notice of, that the drops, that faſtened themſelves to the outſide of the glaſs, purpoſely left in part unfilled, reached either not at all, or very little further than the ſurface of the liquor within the glaſs, whoſe coldneſs as it ſeems did not infrigidate thoſe upper parts of the glaſs, to whoſe level the liquor itſelf did not reach. To which I could eaſily add arguments to prove, that the drops we have been ſpeaking of proceeded not from the tranſudation of the liquor within the glaſs, if I thought it worth while to diſprove ſo unlikely a conjecture. But inſtead of that I ſhall only intimate, that from this experiment uſeful hints may be taken both theorical and practical, and particularly, that a reaſon may, perchance, be given of a ſtrange way of catching a ſalt and liquor out of air, barely by glaſs-

veſſels of a peculiar and ſkilful contrivance. Much of what we have lately ſaid will, I preſume, be the leſs wondered at, if we ſubjoin what experience has taught us, that it is not difficult by the help of a convenient furnace and fit veſſels to make that ponderous metal, lead, aſcend to a good height in the open air, in the form of a copious ſmoke: ſuch a ſmoke we diſcerned after a while to be carried ſo many ways by the aërial corpuſcles, that it met with in actual motion, that it was ſoon diſperſed ſo far as to diſappear: which perhaps will be thought ſome confirmation of what we formerly delivered, when we taught, how much the being divided into very minute parts may conduce to the fluidity even of ponderous bodies.

SECT. XXII.

And though quickſilver be, excepting gold, the heavieſt known body in the world, yet when it is reduced into vapour, it ſeems to be carried to and fro like the other terreſtrial particles, that ſwim up and down in our air: for I remember, that an expert gilder not long ſince complained to me, that if when he evaporated quickſilver, he forgot to take off his rings from his hand, though they touched not the quickſilver whilſt it was in a body, the roving fumes would oftentimes faſten upon the gold in ſuch plenty, as would put him to much trouble to get them off from his rings; one of which he ſhewed me, that he had lately thus whitened, and as it were ſilvered over with mercurial fumes, and was then to reſtore to its native yellow.

SECT. XXIII.

But let us return to viſible liquors, and endeavour to prove almoſt *ad oculum*, as they ſpeak, that their inſenſible parts may be every way agitated, though their motion be but ſeldom viſible to us. Take then what quantity you pleaſe of aqua fortis, and diſſolve in it as much as you pleaſe of ordinary coined ſilver (it not being neceſſary for this experiment that it be refined) and pour the coloured ſolution into twelve or fifteen times as much fair water, and then decant or filtrate the mixture, that it may be very clear. If you look upon this liquor, the parts of it will ſeem to be all of them as perfectly at reſt as thoſe of common water; nor will your eye be able to diſtinguiſh any corpuſcles of ſilver ſwimming in the liquor: and yet that there are ſuch metalline corpuſcles agitated to and fro with and by thoſe of the water, will quickly appear, if you immerſe into it a flatted piece of clean copper, for by that time you have held it two or three minutes of an hour (perhaps not ſo long) in the liquor, you ſhall ſee the particles of ſilver, that were roving up and down the liquor, faſten themſelves in ſuch ſwarms to the copper-plate, that they will appear in their native hue, and cover it, as it were, with a looſe caſe of ſilver, which may be eaſily ſhaken off in the form of a metalline powder: and if ſeveral ſuch plates be left all night, or for a competent number of hours, in the bottom of the veſſel, you may the next day find all the particles of ſilver, that were diſperſed through the whole body of the liquor, ſettled upon or about them; the deep bluiſh green tincture you will diſcover in the water proceeding only from ſome little parts of the copper-plates, and of the alloy of the coin, diſſolved by the ſaline particles of the aqua fortis. And I remember, that, to compleat the experiment, I have ſometimes made even theſe fall to the bottom of the veſſel, by leaving a lump or two of ſpelter there for two or three days: for,

for, not only those metalline corpuscles, that were just over or near to the determinate place, where I put the spelter, but also all the rest, into how remote parts soever of the liquor they were diffused, did settle upon the spelter, as appeared both by its increase of bulk, and by their leaving the water clear and colourless; which plainly seems to have proceeded from hence, that the particles of the water were restlessly and every way agitated, and so by frequently gliding along the surface of the spelter they must carry thither of the corpuscles of copper mingled with them, some at one time and some at another, till at length all were brought to it and detained there.

SECT. XXIV.

THAT of the particles of spirit of wine, and such like inflammable liquors drawn from fermented juices, though they seem to the eye to be at rest, a good many do yet move confusedly and very nimbly, I remember I have long since manifested by an easy and ocular proof, which I devised about ten or twelve years ago, when (being yet scarce more than a boy) I first began to consider what fluidity might be. The experiment, as I writ it down, with all the circumstances and observations relating to it, I have not now by me; but having divers times been desired to shew it to learned men (physicians, mathematicians, and others) I cannot have forgotten those phænomena of it, that are the most pertinent to our present subject. Supposing then, that in pure spirit of wine, beside the aqueous parts that glide softly along each other, there are store of volatile and spirituous corpuscles, whose agitation is stronger, I let fall (from a pretty height, that it might be broken into small drops by its fall) into any wide-mouthed glass filled with this liquor (which must not be over dephlegmed, lest the oil sink in it) a little common oil or spirit of turpentine, which I therefore made choice of, because its tenacity, greater than that of the chymical oils of spices, makes it, that it will neither mingle with spirit of wine, nor spread itself, as divers other distilled oils will, upon the surface of it, but keep itself in the form of round drops, whose shape facilitates their motion. The oily drops then swimming at the top of the spirit of wine, will be, by the disorderly rovings of the agile parts of it (which hit against them little globes, as the vinous spirits ascend to exhale) made to move restlessly to and fro in an irregular manner, the drops sometimes bearing up to one another, as if all or most of them were presently to unite into one body, and then suddenly falling off, and continuing to shift places with one another, after a manner pleasant and strange enough to them, that never before saw the experiment: and this dance will continue for half an hour or an hour (or a shorter or much longer time, according to the quantity and strength of the liquor) till the spirituous parts being flown away, the drops being no longer impelled lie at rest upon the dispirited liquor, as they would upon common water. And whereas the nimble motion of the drops might be suspected to proceed from some secret contrariety in nature betwixt the oil of turpentine and spirit of wine; besides, that I could easily shew, that those two liquors have no antipathy, I not only tried the experiment with another inflammable liquor than spirit of wine, but (if I much misremember not) found, as I expected, that little pieces of chopped straw (such both being light and not easily imbibing moisture) being gently let fall upon the spirit of wine, were in a tumultuous manner carried to and fro upon the surface of it; though I am not sure, but that the motion of the oily drops may be in part due to some partial solution made of them by the vinous spirit, which during that action may tumble them to and fro; not to add,

that I have, by ſome trials, been tempted to ſuſpect the air may have ſome intereſt in the motion of the drops. However, I have mentioned the recited experiment, not as if I thought that either it or fugitive ſpirit of wine were fit to teach us the nature of fluid bodies in general, but to ſhew by an ocular example, that there may be a quick and inteſtine motion in ſome parts of a liquor, notwithſtanding that the unaſſiſted eye can diſcern no ſuch matter. I ſhall not here relate, how having cauſed to be hermetically ſealed up ſome of theſe liquors in a glaſs, to try how long the extravagant dance of the drops would laſt, when the more ſpirituous parts of the vinous liquor could not exhale, my veſſel was ſoon broken without any diſcernible violence. Nor ſhall I now take notice of any of the other phænomena of our experiment, partly, becauſe I have elſewhere mentioned moſt of them; and partly, becauſe I think it more pertinent to our preſent theme, to obſerve, that this unſeen agitation of the minute parts will not only hold in light and ſpirituous liquors: for, that the inſenſible parts even of the heavieſt liquors themſelves are alſo in actual motion, though many may think it unfit to be believed, will follow from what has been already delivered concerning the nature of fluid bodies, as ſuch; and may be confirmed by this, that whereas three of the heavieſt liquors, we yet know of, are quickſilver, oil of tartar *per deliquium*, and oil of vitriol; that firſt-named will even in the cold penetrate into the pores of foliated gold, and deſtroy the texture of that cloſeſt of metals: the liquor alſo of ſalt of tartar will in the cold draw tinctures from ſeveral bodies, and we have endeavoured to evince the agitation of the parts of oil of vitriol, not only by ſhewing, how in the cold it would corrode divers metals, but by caſting little pieces of camphire into it, which without the aſſiſtance of the fire were made liquid by it, and appeared like ſo many drops of oil. And he, that yet doubts, whether the parts of theſe two oils (as chymiſts abuſively call them) how ponderous ſoever they be, are fiercely agitated or no, may probably be ſoon ſatisfied by ſhaking an ounce or two of each of them together, and obſerving the heat, hiſſing, ebullition, and ſparkling, that will ſuddenly enſue upon their being blended.

SECT. XXV.

BUT here we muſt take notice, that though it belong to the nature of fluid bodies, that their parts do eaſily ſhift places; yet that is to be underſtood only as to the parts of the ſame fluid bodies, as water, or of ſuch differing fluid bodies as are diſpoſed readily to admit each other's particles, and mingle together, as we ſee in water and wine. For otherwiſe fluid bodies may be of ſuch differing natures, that when two or more of them are put together, they will not mix, but each retain its own diſtinct ſurface; ſo that in regard of one another, the contiguous bodies do in ſome degree emulate each of them the nature of a conſiſtent body: for though it cannot be looked upon as a hard body but a ſoft, becauſe of the eaſy ceſſion of its ſuperficies, yet it does like a compact or conſiſtent body deny to mingle permanently with the contiguous liquor or other fluid ſubſtance. And I ſomewhat wonder, that *Lucretius* and other Atomiſts ſhould (at leaſt for aught I remember) overſee this obſervation, ſince it is obvious enough in oil, which will not mix with water, but float upon its ſurface: not to mention, that quickſilver will not incorporate with any of the familiar liquors known to the antients. I had almoſt forgot, that I promiſed at the beginning of this diſcourſe an inſtance concerning flame, which I will therefore now recite. And it is, that having by an eaſy preparation of copper, by the intervention of a little ſal-armo-

niac (which I have already taught in another treatise *) so opened the body of that metal, as to make it inflammable; I took some small grains of this prepared mineral, and put them under the wick of a strong and actually burning candle, whereby (as I expected) they were with the melted tallow soon carried up to the bottom of the flame, and by it so kindled, that the green (not blue) flame of the cupreous body did (somewhat to the wonder of the spectators) burn for a good while (this combustible matter being marvelously lasting) distinct from the yellow flame of the candle, as if there had been some invisible partition between them. But to return to the unmingleable liquors we were formerly speaking of; the cause, why these retain their distinct surfaces, my present task does not oblige me to inquire into: but this I shall observe in general, that it seems to depend very much upon the texture of the particular liquors, and perhaps too upon the peculiar motions of their minute parts. For I have observed, that though pure spirit of wine and salt of tartar, resolved into a liquor by the moisture of the air, will, when put together, retain distinct surfaces, or presently regain them if you shake the liquors never so strongly together; yet by adding a little fair water to either of them, the texture being thereby altered, it will easily incorporate with the other. And thus although that (as I noted already) common spirit or oil of turpentine will not mingle with spirit of wine, yet having had the curiosity to make a trial with oil of turpentine abstracted skilfully, and with a very gentle fire (for otherwise the experiment may easily miscarry) from melted or at least well decrepitated sea-salt, we found, according to expectation, that though there appeared no visible alteration in the oil, yet we could easily by shaking confound it with pure spirit of wine. Moreover, though lixiviate liquors and oils will not by an ordinary agitation be permanently joined, yet I have tried, that by digesting a good while a solution of salt of tartar with oil of almonds, I could reduce them to a soft saponary substance: which experiment makes somewhat more to my present purpose, than the common practice of soap-boilers, because I did not, as they, boil away the water, wherein the lixiviate salt is dissolved. I might add also, that if you put one part of quicksilver into about four parts of oil of vitriol, you will find (at least if the experiment proceed always after the same manner as it has done with me) that the two liquors will remain distinct whilst you keep them in the cold, but if by degrees of heat you bring the oil of vitriol to boil, it will pierce the surface of the quicksilver, and by partly incorporating with it, reduce it to a substance very differing from what it was. But because these two last experiments may be, with less improbability than the two that preceded them, referred to other causes, I shall no farther insist on them, but take notice of one thing more concerning the difference of fluid bodies. And it is thus.

SECT. XXVI.

I OBSERVED, that some of them do not only not mingle with others, that are contiguous to them, but fashion the surfaces of those others, and reduce them to determinate shapes. This I have taken pleasure to consider in some chymical liquors, which I have purposely put together; for instance, having poured spirit of wine upon oil of tartar *per deliquium* (as chymists call it) I found, that the superficies, wherein they touched each other, was flat, or (as to sense) parallel to the horizon. But if this was done in a very slender or narrow glass, with the mouth unstopped, though the

* An Historical Dialogue about Flame.

lower

lower surface of the spirit of wine which touched the other liquor appeared very level; yet the upper superficies, which was contiguous to the air, was manifestly very concave. And if to these two liquors I did in a broader glass pour oil of almonds, that oil would sink to the bottom of the spirit of wine (that being well rectified) and floating upon the oil of tartar, would separate the two liquors, and both above and below retain a flat or level surface. But if instead of oil of almonds, or another expressed oil, I dropped into pure spirit of wine, swimming upon oil of tartar, some common oil of turpentine; the oil would gather into parcels some of the bigness of hail-shot, some as big as small pistol-bullets, and some of other sizes, which in case they did swim in the spirit of wine, and touched neither of its surfaces, seemed globulous, and continued so (the glasses being stopped) for many hours: but in case they emerged to the upper part of the spirit of wine, as much of them as lay immersed in the spirit (which was by far the greatest part of them) appeared round, and continued so as long as I pleased; the upper parts only of those little globes appearing to have the same surface with the spirit of wine. And I further observed, that some small drops would, as it were, rest constantly upon the superficies of the oil of tartar, touching it but as it were in a point, and continuing to the eye sperical; though the surface of the liquor were purposely now and then somewhat shaken. But that, which I took special notice of, was, that having (upon design) into pure spirit of wine (for upon common spirit oil will swim) let fall some great drops of oil of turpentine, they did at first sink to the bottom of it, and lie upon the surface of the oil of tartar almost like hemispheres, whose convex part was all above the oil of tartar; but after a while they were, as I expected, pressed on all sides and fashioned into round bodies (yet a little more protuberant at the sides than the top) which seemed scarce to touch the surface of the oil of tartar on which they leaned. Divers other observations of this kind were afforded me by some peculiar mixtures, that I made of chymical liquors: but not having the leisure to set them down, much less to inquire into their causes, I should scarce have mentioned what I have already delivered (especially since we found that a light variation of circumstances would often alter the event of such trials, which we have therefore set down barely historically) but that finding that drops of water, quicksilver, and other fluid bodies, seemed to be fashioned into a round figure by that every way almost equal pressure of the ambient air; and having likewise tried, that quicksilver suspended in the air (as it may easily be, if the Torricellian experiment being made in a tube exceeding slender, some air be afterwards dexterously let in to divide the long mercurial cylinder into diverse short ones) has both at the top and bottom, where it is exposed to the action of the air, a very protuberant surface; finding, I say, these effects of fluid bodies upon one another, I thought it not amiss to intimate, how some experiments might be made, that may possibly facilitate the giving an account of the figuration of some of the more considerable fluid bodies, which, as we noted already, make up much the greater part of the universe: especially, since I could easily enough make it probable, that such steams of the terrestrial globe, as may well be supposed to be the chief ingredients of our atmosphere, may like a liquor retain a superficies distinct from that of the ambient and contiguous body. And since we are speaking of the distinct surface of fluids, the occasion invites me to add an experiment, which, though apt to miscarry upon the account of unheeded circumstances, has yet, when all things were rightly ordered, succeeded very well: I will, I say, subjoin it here, because it shews a way of dividing in a trice a liquor transparent, and, as to sense, homogeneous, into two very differing liquors, the one diaphanous,

diaphanous, and the other opacous, which will not mingle. The experiment is this. Diſſolve one ounce of clean common quickſilver in about two ounces of pure aqua fortis, ſo that the ſolution be clear and total; then whilſt it is yet warm, pour into it by degrees, leſt they boil over, half an ounce or one ounce of filings of lead, and if no error, nor ill accident have intervened, the lead will be in a trice precipitated into a white powder, and the mercury reduced into a maſs (if I may ſo ſpeak) of running quickſilver, over which the remaining part of the aqua fortis will ſwim, whereby we may ſee, that liquors being reduced into very minute parts, may mingle very well, the corpuſcles of the one ſupporting in that ſtate thoſe of the other, though in greater bulk; eſpecially, the texture of one being ſomewhat varied, they will retain diſtinct ſurfaces. *N. B.* Note, that when the operation ſucceeds not well, the mercury need not, for all that, be loſt, but may (in great part at leaſt) be recovered by freeing the precipitated matter from the reſt by filtration, and then diligently grinding the white precipitate with water, by which means, the mercury will little by little be gathered into drops. And though this be far from being the true mercury of lead, as I may elſewhere ſhew you; yet ſome inducements, not here to be named, incline me to look upon it, as ſomewhat differing from common mercury, and fitter than it for certain chymical uſes.

SECT. XXVII.

AND here I ſhould paſs on to the conſideration of firmneſs, but that, when a while ago I diſcourſed of the agitation of the corpuſcles that compoſe oil of tartar and oil of vitriol, I forgot to add, that not only in fluid bodies, but in ſome alſo of thoſe that are conſiſtent, there may perhaps be more motion in the inſenſible parts than our ſenſes diſcern, or we are wont to imagine; eſpecially in thoſe bodies, which having been once endowed with life, are, though not fluid, yet either ſoft, or at leaſt not perfectly hard. I have more than once taken pleaſure to look upon an heap of ſwarming bees, for though they make not up a liquid but coherent body, which may be turned upſide down without loſing its coherence, and which being beheld at a diſtance, ſeems to be one entire maſs or body; yet it is evident to him, that looks at them near enough, that the particular bees, that ſwarm, have moſt of them their diſtinct and peculiar motions, and that yet theſe motions of the particular bees deſtroy not the coherency of the heap; becauſe that when one of the more innermoſt bees removes, as ſhe lets go her hold from thoſe, that ſhe reſted on before, and goes away from thoſe that reſted on her, ſo ſhe meets with others, on which ſhe may ſet her feet, and comes under others, that in like manner ſet their feet on her; and ſo by this viciſſitude of mutual ſupports their coherence and their removes are made compatible. And if inſtead of bees, the ſwarm conſiſted of extremely little flies, their particular motions would perhaps be inconſpicuous. And that ſome ſuch thing may happen in the conſiſtent bodies we have been ſpeaking of, ſeems probable from hence, that in wainſcot and other hard wood, we often ſee little heaps of duſt produced in them by putrefaction; and not only in cheeſe we many times ſee multitudes of mites ſtart up, but in apples and other fruits we oftentimes find maggots, though the ſkin be whole (which could not be unleſs the parts of the matter were variouſly tranſpoſed, that is, put into a local motion, and connected after a manner ſuitable to the nature of the inſect to be produced). And by the growth of bones in the bodies of perfecter animals, as well in reſpect of the internal cavity, where the marrow lodges, as of the

external surface, as also by the growth of the shells of oysters and snails (though cold animals) from a size inconsiderable at first, in regard of what is afterwards attained to, and by some other resembling particulars, that constitute even the solid parts of animals, are not, whilst the creature lives (or at least whilst it grows) altogether exempt from some (though slow and insensible) local motion. And I remember, that it has by a very diligent observer been affirmed to me, that he saw several pieces of gum sweat out of an old wainscot of above twenty years standing. Which I the less wonder at, because I have several times seen viscous exsudations disclose themselves like drops of turpentine upon deal-boards, which had been made use of about building. (But of this subject more, perhaps, elsewhere.)

SECT. XXVIII.

After we have hitherto discourses of fluidity, as considered in distinct bodies, we might properly enough say here something of what furtherance or hinderance, in respect of fluidity, one body may receive by being mingled with another. But the consideration of those changes of consistence, which may be produced by mixture, is a subject; that we shall have such frequent occasions to treat of in what we are to deliver about firmness, that we shall now only give this general admonition, that it is not so safe as one would think, to foretel the consistence of a mixture of two or more bodies, from the bare consideration of the consistence of those bodies, whereof it is to be compounded. And that we might at once both manifest this, and insinuate what judgment should be made of what is said by so many chymists and others, who without limitation teach, that the addition of salts to metalline and mineral bodies does much facilitate their fusion, I remember I purposely made and employed this experiment.

We dissolved crude copper in a due quantity of spirit of nitre, and by evaporation reduced the solution to a kind of vitriol of a lovely colour. We also corroded with two parts of spirit of nitre one of good tin, and suffered the mixture to reduce itself (as it easily did) to a substance almost like meal. Of this mixture we put a parcel into a crucible, and suffered it to grow (and for a pretty while to continue) red hot: nay, we put some of it upon a quick coal, and excited the heat by frequently blowing the fire, without finding, that this metalline meal did at all melt, though salt-petre be a fusible salt, and tin itself be of exceeding easy fusion. Whereas, although copper be a metal, which is much harder to melt, not only than tin and lead, but even than silver (as those well know, that mix silver with copper to make a fusible mixture to solder upon copper and brass) yet was this metal, that will endure a long and strong ignition by being joined *per minima* with the same kind of nitrous corpuscles, that had so contrary an effect upon the tin, so strangely disposed to fusion, that the vitriol would melt with as small a heat as that of one's hand. Nay, we have made such a vitriol either with spirit of nitre or with (what differed little from it) a certain aqua fortis, as would even in winter be made to stand melted for divers hours together, by the languid warmth of the sun, though shining on it but through a window, where it also stood but in an unstopped glass. So fit it is, that we consider as well the new texture, that mingled bodies obtain by the association of their particles (whose size and shape, and perhaps motion may be thereby much altered) as the consistence of the particular bodies before their being mixed.

THE

History of FLUIDITY and FIRMNESS.

The SECOND PART.

Of FIRMNESS.

IT is sufficiently known, that the chymists ascribe the firmness and hardness of bodies to salt, and teach, that the saline ingredient of them is the principle of coagulation in them, and the cause of their compactness and solidity. But though this opinion of the chymists be embraced by so many modern philosophers and physicians, that some may think it superfluous to make enquiry after other causes; yet others (to whom the explications of chymists seem not always so much as intelligible) will, upon the very account of the receivedness of the proposed opinion, think it rather worthy to be examined, than to be acquiesced in. However, without making it our business, either to side with, or oppose any sect of naturalists, we will apply ourselves a while to consider the thing itself in prosecution of the design already begun. And having in the foregoing part of our little history taken a general view of fluidity; we will now proceed to try, what light it will afford us to discover the nature of firmness and compactness.

AND since fluidness and stability being contrary qualities, are to be apprehended under contrary notions, we may conceive, that the firmness or stability of a body consists principally in this, that the particles, that compose it, besides that they are most commonly somewhat gross, either do so rest, or are so intangled between themselves, that there is among them a mutual cohesion, whereby they are rendered unapt to flow or diffuse themselves every way, and consequently to be, without violence, bounded and figured by other surfaces than those, which their connection makes themselves constitute.

IN this rude description of firmness we have intimated three principal causes of it, namely the grossness, the quiet contact, and the implication of the component parts.

THE first is grossness of parts, of which we have in effect almost sufficiently discoursed already: for since treating of fluidity, we manifested at large, how conducive smallness of parts was to that quality; it is easy to deduce, that grossness of parts in a body must commonly be a great disposition to its being firm. And bigger corpuscles being *cæteris paribus* more difficult than lesser to be put into motion, when they are once at rest, it is obvious, that a body consisting of such particles is less disposed to become fluid, and consequently more apt to continue firm, than if its component parts were smaller, and thereby more easy to be displaced. But when I speak of the grossness of corpuscles, I pretend not to determine, whether or no body or matter be so perpetually divisible, that there is no assignable portion of matter so minute, that it may not at least mentally (to borrow a school-term) be further divided into still lesser and lesser parts: for allowing this indefinite divisibility of corporeal substance, it is plain, that it may in some sense be averred, that there are no firm bodies, whose

parts are not extremely minute. But I understand by the gross parts I here speak of, such corpuscles, as actually convening to constitute a body, are scarce dissipable or divisible into lesser by the agitation of the ambient air or æther, or by the other causes of the fluidity of bodies.

IT is also to be noted, that when I spake of the fitness of grosser corpuscles to make a firm body, I added, *cæteris paribus*, because it may happen, that the breaking of the small parts of a body into minute particles may make them but the fitter to contribute to the firmness of the body they belong to: for the parts of the divided corpuscles may by their comminution acquire a new, and perhaps a more irregular shape than before, upon whose account they may be more disposed to be entangled among the neighbouring particles, or may be better fitted to get into and fill the pores of some kind of bodies. And in such little particles, not only the minuteness may make them lie closer together, and consequently the better exclude the air; but the greatness of the surface in proportion to the bulk of the matter may perhaps in some cases occasion a fuller contact, and so facilitate the constitution of a very firm body, in case these minute parts (whose intervals, if they intercept any, need not be other than very small) shall be placed and disposed to the best advantage for a full contact of one another. But as I said a while before, from what we have already delivered (concerning the size of parts, when we treated of fluidity) it may easily be understood, how much the magnitude of them may conduce to firmness; and therefore we will presently pass on to the mention of the two other things to be considered, in reference to consistent bodies. Whereof the one is the bare rest of the small and contiguous parts, that make up the firm body; and the other the intricate texture of such parts in the body they make up. And either of these two seems alone sufficient to render a body stable; though nature do perhaps oftentimes make some (though not equal) use of both, to fasten the parts of the same body more firmly together.

OF the former of these causes, I am informed, that the justly famous Monsieur *Des Cartes* has also taken notice, but without adding proof from experiments, or observations. But it seems to have been either overlooked, or (as incongruous to their hypothesis of the innate motion of atoms) rejected by the old atomists, and by *Lucretius*, who takes notice (that I remember) only of the latter: for though they did of old make mention of the sudden divulsion of two flat and solid bodies, yet they employ that observation but to prove a vacuum, (without otherwise taking notice, that I have met with, of those things, that are most material in such observations to our present purpose, and without deducing thence what we shall endeavour to do in order to the explication of the causes of firmness.) Upon what account then soever the atomists have omitted to reckon for a cause of firmness that, which we have newly been speaking of; yet (as we observed above) if two bodies be once at rest against one another, it seems consonant to the catholic laws of nature, that they should continue in that state of rest, till, some force capable to overpower their resistance puts them out of it. And whatever may be said of the unlosable mobility of atoms strictly so taken, yet that divers parts of matter may compose bodies, that need no other cement to unite them, than the juxta-position which we here presuppose, and the resting together of their parts, whereby the air and other fluid bodies, that might dissociate them are excluded, I have been inclined to think by what I have observed in grinding of glasses: for sometimes the convex surface of one body being ground upon the concave surface of another, the two surfaces will happen to be so closely and exactly fitted to one another, (their immediate contact in all their parts, or at least in innumerable of them, hindering

hindering the intercurrence of the air) that a man is not able without breaking one or both of them to pull them directly asunder; but if you will sever them, you must be fain to make one of them to slip along the surface of the other: which makes the glass grinders often complain of the trouble they meet with in separating such bodies. Nay, if you lay two flat glasses ground very true and well polished upon one another, so that their surfaces may almost every where touch each other, (to which it will be requisite to rub them a little one upon another, for the better exclusion of the air) you may by lifting up the uppermost, lift up the lowermost (though perhaps, as we have often tried, two or three times bigger) with it, as if the two plates of glass made but one body. Nay, we have divers times taken up four or five pieces of glass at once, laid and pressed thus one upon another, and might, perchance, have taken up a greater number, if we had had more of them at hand. And trial has also informed us, that if you hold a looking-glass very level with the unfoliated side downward, and rub a little against it a piece of other very flat and very smooth glass, you may easily, by that way only, fasten them to one another; so that the lowermost glass, though large, will hang between the uppermost and the ground, to the wonder of those, that behold it, and can discern nor imagine nothing capable to keep it from falling: and by the same way (as we shall recite anon) we have often made one considerably thick piece of marble take and hold up another, having purposely caused their flat surfaces to be carefully ground and polished, without which the experiment will not succeed. Nor is it requisite, that the glasses be flat to make them adhere very closely together, provided their immediate contact be made according to a large surface: for to what we have already mentioned concerning the cohering of convex and concave bodies, we may add, that having purposely applied a long glass-stopple of an almost conical figure, and well ground, to the mouth of a thick quart bottle, whose neck was made long, and of a figure convenient to receive the stopple, and ground within fit for it, we found, that these two glass bodies touching one another in a multitude of parts, did adhere together so closely, that when the stopple was carefully put in, we could easily, and divers times one after another, lift up the bottle, though there was by our guess above a pound of liquor in it. Unless we suppose, without much probability, that because it is found, that moving them to and fro upon one another, and pressing down the stopple, promotes their sticking, their adhesion may be in part ascribed, either to some elastical motion in the parts of the pressed glass, or to the exquisite adaptation of the almost numberless, though very small, asperities of the one, to the as numerous little cavities of the other; whereby the surfaces do lock in with one another, or are as it were clasped together. For as polished as the surfaces may appear to sense, we must not deny, that there may be such inequalities in them, since being wont to be polished with putty or some such powder, or heap of grating and irregularly shaped corpuscles, they must needs make store of little furrows, and ridges, and other asperities on them. But to insist on these conjectures, were to digress.

YET here we must not decline taking notice, that, at least here below, the sticking together of such bodies, as are of sensible bulk, and whose smooth surfaces immediately touch one another, may possibly not so much proceed from this, that their parts, as we formerly observed, are at rest among themselves, and by their immediate contract do make up as it were but one body; as from the pressure of the atmosphere, proceeding partly from the weight of the ambient air, (mixed with the effluviums of the terrestrial globe) and partly from a kind of spring, by virtue of which the air continually presses upon the bodies contiguous to it, though through accustomance

and negligence, and perhaps some other causes not here to be insisted on, we neither feel it in our own bodies, nor take notice of it in others. Now this pressure of the air every way being supposed, I think the adhering of the smooth bodies we speak of (for we suppose them far greater than the particles of the air) to one another may probably enough be ascribed to this, that when, for instance, the smooth surfaces of two pieces of glass do so exquisitely touch one another, that none of the ambient air is either intercepted or admitted between them, then the undermost glass will suffer no pressure on that side, which touches the uppermost; the parts of the uppermost glass having no sensible spring in them (so that they can only resist, but not repel the other:) but that side of the undermost glass, which is exposed to the air, will be pressed upon thereby; and there being, as we said, no elastical pressure on the other side of the glass to balance this, it is not to be wondered at, that the inferior glass should not fall off from the other, in regard the weight, that would carry it downwards, is much too small to overcome that force of the air, that thrusts it against the uppermost glass: as if one should with his hand thrust a plate of iron broad-wise against the flat cieling of his chamber, the iron would not fall as long as the force of the hand perseveres to press against it. Nor is it material, that in our case the pressure of the atmosphere is supposed to force the lowermost glass upwards; for if we suppose the air to consist of innumerable little springs (as it were) bearing upon and supporting one another, and whereof the lowermost are bent by the weight of all that are incumbent on them, it will be easy to conceive, that near the surface of the earth, (about which the air must diffuse itself by reason of the gravity of its small parts, and the resistance of the earth against their descent) it may press almost equally every way, and by a kind of recoil (though not properly so called) from the terrestrial globe upwards, may strongly press any body, upon which it can bear, against any other, which has no such elastical power to repel from it a body so pressed against it.

THIS difficulty being thus dispatched, we shall proceed by two or three particulars, to confirm our conjecture at the cause, why smooth bodies stick together upon bare juxta-position or contact. And first I observe, that if a piece of flat glass be, as we formerly mentioned, appended to a looking-glass held with the unfoliated side downwards, parallel to the horizon, though the adhering glass will not drop down, yet it will very easily be moved any way along the level surface; and if by inclining the looking-glass any way, you deprive it of its former level, the smaller glass will easily slide downwards upon the surface of the greater. Of which the reason seems to be, partly that the gravity as such of the lower glass does not considerably resist the horizontal motion of it, but only the motion upwards, whereby it must recede from the centre of heavy bodies, as might if need were, be probably deduced from divers instances obvious enough; and partly, or rather chiefly, that to the edges of the glass the ambient air is contiguous as well on the one side as on the other, and so the pressure of the air being equal on every part of the edges, the gravity of the smaller glass is not hindered by the air (which can as fast succeed on one side, as it is displaced on the other) from making it slide down the shelving surface of the greater glass; whereas of the broad and flat sides of the lowermost glass the one is, as we said, pressed by the spring of the air, whilst the other suffers no such pressure from the looking-glass, to which it was applied. And so, if you take a small open-mouthed glass, and plunge it into a vessel full of quicksilver with the mouth upward, that the quicksilver may fill it without leaving any air in it, and if then, whilst it is under the quicksilver, you turn the mouth downwards, and so keeping it upright, lift it up till the mouth be almost come to the top of the mercury you shall perceive, that the glass will

will remain almost full of quicksilver in the vessel: and this will continue so, though you incline the glass this way or that way, provided you keep the mouth of it within the mercury. And this experiment, though more noble when tried with quicksilver, will succeed also when tried, as it may more easily be, with water. Of which the reason seems to be, that the glass hinders the quicksilver in it from the pressure of the incumbent air, whereas the quicksilver in the vessel being exposed to it, must by it necessarily be forced up against the surface of the inverted bottom of the glass, where it meets no elastical power to repel it downwards. For, that it is not nature's abhorrency of a vacuum, that keeps the quicksilver from descending till some air can come to succeed in its room, the famous experiment invented by *Torricellius*, and found true by many others, and ourselves, touching the descent of the quicksilver in any tube of above two foot and a half long, seems clearly to evince. And to confirm what we have said, and shew withal, that it is not so much the contact of bodies according to a large surface, as that contact is considered in itself, as by reason of its being ordinarily requisite to the exclusion of air, that at least here below keeps bodies from falling asunder; I shall relate, that having by a certain artifice out of a large glass (with a narrow mouth) caused a certain quantity of air to be sucked, we found, that by immediately applying a book (which then chanced to lie at hand) to the orifice of the vessel, the book was readily lifted up and sustained in the air as long as we pleased, though the surface of the suspended body could be touched, as is evident, but by the ring, which incircled the orifice of the vessel, and though the weight taken up (besides that it was inconveniently shaped for such a trial, which would probably have succeeded as well with a much greater weight, if we had had one fitly shaped at hand) exceeded twenty ounces *. Of which event the cause seems plainly to be this, that by reason of the exsuction of some air out of the glass, the elastical power of the remaining air was very much debilitated in comparison of the unweakened pressure of the external air, which being able to press the book against the orifice of the vessel with greater strength than the internal air can resist, thereby it comes to pass, that the whole orifice of the vessel, though there be but part of it of solid body, does yet on this occasion perform in some measure the part of an entire surface exactly smooth.

IT may be considered also (to add that upon the bye) whether upon the principle lately explicated may not in some measure depend the solidity of glass. For though its parts seem very little or not at all branched or interwoven one within another, and appear very smooth and slippery, yet since the fire that brought them to fusion, and consequently to be fluid, may well be supposed to have sub-divided and reduced them into small particles, and to have thereby assisted them to exclude the air from betwixt them, it may seem, that it needs not much be wondered at, if the immediate contact of such small and smooth corpuscles suffice to make them hold together; for that their union is strict enough to keep out the air, may appear from this, that those that blow glasses, and those that distil in them, find not the air can traverse the pores even of heated glass: and as for any more subtil matter, we see by the free passage of light and heat, or, to speak more warily, of magnetical effluvia through the glass, without injuring its texture, that such matter, but moderately moved, will not hinder the little solid parts from cleaving together. And on this occasion it might be considered, how much the juxta-position of their corpuscles crouded together by fusion may contribute to the consistence and brittleness of salt-petre, and divers other

* Much more considerable instances of this nature may be met with in the author's *New Physico-Mechanical Experiments*.

bodies,

bodies, which may from an incoherent powder be readily turned into one mass; as also how far the sticking together (for I speak not of the figures composed by them) of the small parts of hanging drops of water, and such other liquors, as are not thought to consist of corpuscles hooked or branched, may be ascribed to the contact of their small parts, and the exclusion of air. These I say, and some other such things might be here considered, but that we are forbidden to examine them particularly, and especially what has been represented touching the solidity of glass, (which we suspect another cause may have a great interest in) by our haste, which calls us to the remaining part of our discourse.

THOUGH then it be hence (to omit other proofs elsewhere mentioned) sufficiently manifest, that the air has a spring, and that a strong one, yet there appears no great necessity of having recourse to it for the giving an account why the two smooth glasses above-mentioned were able to adhere so closely to each other: for a probable reason of the same phænomenon may be rendered by the pressure of the air considered as a weight. And first, we must recall to mind what we (a little above) said of the recoiling (or rebounding of the pressure) of a cylinder of air from the earth, to the suspended piece of glass, proceeding from this, that the fluid air, which is not without some gravity, being hindered by the resisting surface of the terrestrial globe to fall lower, must diffuse itself, and consequently press as well upwards as any other way. Next, we may consider, that when the surfaces of two flat bodies of any notable (and, for example, of equal) breadth do immediately touch each other, and lie both of them level with the horizon, and one of them directly over the other; in this case, I say, since the air cannot move in an instant from the edges to the middle of the two surfaces that lie upon each other, the lowermost cannot be drawn away downwards in a perpendicular line from the uppermost, but that by reason of the stiffness and contact of the two bodies, it must necessarily happen, that at the instant of their separation, should it be effected, the lowermost glass will be pressed upon by the whole (crooked) pillar of air, supposed to reach from the top of the atmosphere, and to have for basis the superficies of the undermost glass. For at that instant, the air having not time to get in between the two glasses, there is nothing between them during that instant to resist the pressure of that air, which bears against the lower superficies of that undermost glass, and consequently such a revulsion of the lower glass cannot be effected, but by a weight or force capable to surmount the power of the weight of the above-mentioned cylinder of the atmosphere; and this, as I said, because that by reason of the sudden separation, the upper surface of the glass has not any air contiguous to it, which, were it there, would (according to the nature of fluid and springy bodies) press as much against the upper surface of the glass, as the pillar of the atmosphere against the lower, and consequently sustain that endeavour of the air against the lower side of the glass, which in our proposed case must be surmounted by the weight or force employed to draw down the lower glass. And hence we may understand (to add that upon the bye,) that it is not necessary, that the contiguous surfaces of the two flat glasses, we have been treating of, be paralleled to the horizon: for if you should hold them perpendicular to it, their divulsion would not cease to be difficult, provided it were attempted to be made by suddenly pulling one of the broad surfaces from the other in a level line, and not by making one of the surfaces slide upon the other; for in the former case, the separation of the contiguous bodies will be hindered by the weight or pressure of the lateral air (if I may so speak) that bears against the broad sides of the glasses contiguous to it. But whereas in these cases we suppose the superficies of the two glasses to be so exactly flat and smooth, that no air at all can come between them; experience has informed us, that it is extremely

extremely difficult, if at all possible, to procure from our ordinary tradesmen either glasses or marbles, so much as approaching such and such an exquisiteness: for we could very hardly get either experienced stone-cutters or persons skilled at grinding of glasses, to make us a pair of round marbles, though of an inch or two only in diameter, that would for so much as two or three minutes hold up one another in the air by contact, though they would easily enough take up each other, if the uppermost were drawn up nimbly, before the air could have leisure to insinuate itself betwixt them.

But this notwithstanding, we endeavoured by the following expedient, not only to manifest, that the power or pressure of the air is in these experiments very great, but also to make some estimate (though but an imperfect one) how great the power is.

Having then provided a pair of marbles of an inch and half in diameter, and as flat and smooth as we could get, and having considered, that as it was the getting in of the air between them, that (for the reason above declared) hindered them from sticking strongly together; so the access afforded to the air was for the most part due to that scarcely evitable roughness or inequality of their surfaces that remained in spite of the polish: considering these things, I say, we supposed, that the intrusion of the air might be for some while prevented by wetting the surfaces to be joined with pure spirit of wine; and that yet this liquor, that seems the freest that we know of from tenacity, would not otherwise than by keeping out the air prove a cement to fasten the stones together. But because the easy separation of such smooth bodies, as adhere but by contact does in great part (as we formerly noted) proceeds from this, that whereas it is very difficult to hold such bodies exactly level for any considerable space of time, and yet the least inclination any way gives the lower body opportunity to slide off; because of this, I say, we resolved in the first place to see what could be done by fastening to the upper marble certain wires and a button, in such manner, as that the lower marble, when it was joined, might freely fall directly down, but not slip much aside, being hindered by the wire. And in pursuit of this we found, that not only the dry marbles could be made to take up and hold up one another, but that once by drawing up the upper marble nimbly, we could take up (but not keep up for any time) together with the lower marble, a scale, and in it a pound weight of 16 ounces Troy.

After this we moistened the surfaces of the marbles with such pure alkalizate spirit of wine, as we elsewhere teach to make, which was so thin and subtile, that not only we burned some of it, before we would employ it about this experiment, in a silver spoon, without leaving so much as any sign of phlegm behind; but it would (in the open air) almost in a moment fly away from the surface of the marble anointed with it, and leave it dry and glossy. The marbles being skilfully wetted, and kept by the above-mentioned wires from slipping aside, we cast into the scale fastened to the lower of them divers weights at several times, and by nimbly pulling up the higher stone, tried many times, how much we could draw up with the lower, and did sometimes take up above an hundred ounces, and once an hundred thirty-two ounces Troy, besides the scale, that contained them, and the marble itself, the diameter of whose smooth surface was by measure but about an inch and two thirds.

But here I must take notice, both in relation to this and the following experiments to be set down concerning smooth marbles, that we never yet found any sort of experiments, wherein such slight variations of circumstances could so much defeat our endeavours; which we therefore mention, that in case such experiments be tried again,

again, it may be thought the less strange, if others be not able to do as much at the first and second, or perhaps the tenth or twentieth trial, as we did after much practice had made us expert in this nice experiment, and suggested to us divers facilitating circumstances, which could not here in few words be particularly set down.

And now, because we perceived, that the spirit of wine was too fugitive and subtile a liquor for our purpose, we supposed, that oil, as it would better fill up the little cavities of the stones, so it would more exactly keep out the air, and less easily vanish into it. And accordingly, having moistened the surfaces with a due proportion of good expressed oil of sweet almonds, and having carefully observed the other requisite circumstances, we took up some drams above four hundred ounces Troy hanging at the lower marble.

And that you may not suspect, that it was by gluing the marbles together, that the oil did enable them to make so much greater resistance against separation than the spirit of wine did; I shall add, that in case the flat surfaces of the joined stones were held not parallel, but perpendicular to the horizon, that so the air might (as we formerly also observed) immediately succeed, as the looser marble should slide off, the weight of some ounces was now and then requisite to draw down the marbles, when they had nothing but spirit of wine between them; whereas they would easily enough slide off from one another, when they were cemented together with oil: perhaps because that the spirit of wine, by reason of the smallness and penetrancy of its parts, and because of its fugitive nature, did not so well fill up the little pores and furrows of the surfaces of the marbles; whereby the little protuberances getting into those little cavities, might more resist the sliding of the marbles upon one another's surfaces, whose texture is better fitted to make their surfaces smooth and slippery.

And to shew, that the resistance of such contiguous marbles to a violent separation is greater in those, which being broader are pressed against or resisted by a proportionably bigger (though not a longer) pillar of the atmosphere; we caused two marbles to be made, whose diameter was three inches, or a very little more: and having after the above-mentioned manner employed spirit of wine to keep out the air from between them, we did after some trials, with the uppermost of them take up the lowermost, and with it four hundred sixty eight, or four hundred and seventy ounces. But making use of oil of almonds instead of spirit of wine, we did with our own hands draw up twice, one time after another, with the undermost stone, a much greater weight, namely, eighty-four pound or 1344 ounces (Troy weight) notwithstanding which weight we manifestly perceived the marble, at which it hung, to stick strongly to the other.

And here again we will take notice, that the interposed oil was so far from being able as a cement, rather than by keeping out the air, to make these marbles stick so close together, that, whereas bodies glued or cemented together are wont to make an almost equal resistance to their being separated, in what posture soever you place them, I made our marbles, even when we tried this last experiment, very freely slide upon one another, by impelling the uppermost to the right hand or to the left, with my finger or my thumb (whereof the reason is intimated above, where we mentioned almost the like case in glasses) and having sometime before taken up a weight, which we conjectured to be not much inferior to that last named, we presently for trial-sake held the marbles with their edges downwards, and found, that those, that in an horizontal position could not be drawn asunder by so great a weight, did in another posture presently fall asunder by their own weight, which made one readily slide off from the other to the ground. Now although we have confessed, that this way of mea-

meafuring the force of the air is not accurate; yet we hope it will not be thought uselefs, fince (not to mention, that by thus breaking the ice, we may make way for the happier inquiries of others) it not only fhows us, that this preffing or fuftaining force of the air, as unheeded as it is wont to be, is very great, but it may alfo affift us to conjecture, how great it is, which though we cannot hereby determine precifely and with certainty, yet we may eftimate it with much lefs uncertainty than otherwife we could.

I know, that the Peripatetics, and the generality of the fchool-philofophers, will confidently afcribe the ticking of the marbles, not to the caufe we have affigned, but to nature's abhorrency and fear of a vacuum. But not to engage ourfelves now in a difquifition, that, when we difcourfed of fluidity, we did (for the reafons there expreffed) decline to meddle with; we will, without difputing whether or no nature either can at all admit, or do abhor a vacuum, content ourfelves to confirm the explication given of this phænomenon by thefe two confiderations. The one, that if nature did fo violently oppofe a vacuum as is pretended, it is not likely, that any force whatfoever that we could employ, would be capable to produce one; whereas in our cafe we find, that a little more weight added to the lower of the marbles is able to furmount their reluctancy to feparation, notwithftanding the fuppofed danger of thereby introducing a vacuum. And my next confideration is, that (according to what we have hitherto delivered) without having recourfe to any fuch difputable principle, a fair account may be given of the propofed phænomenon, by the preffure or weight of the air. And that what we have faid concerning the latter of thefe may be entertained with the lefs difficulty, let us fuppofe, that when the marbles ftick well together, the lowermoft of them, or the appendant weight were faftened to the ground; for in this cafe there appears no reafon to believe that their power to refift feparation would be lefs than it was before. And yet it feems evident, that the uppermoft marble would not be perpendicularly pulled up but by fuch a force as were at leaft (I fay at leaft) able to lift up a weight equal to that of the laft mentioned marble, and of a pillar of air having the ftone for its bafe, and reaching to the top of the atmofphere; fince at the inftance of revulfion, before the air can get in, and fpread itfelf between the ftones, there is not, for aught appears, any fuch body under the upper marble, as can help the hand to fuftain the weight both of that marble and the incumbent cylinder of the atmofphere, which then gravitates upon it, and confequently upon the hand; becaufe there is no air, nor other equivalent body underneath it, to fuftain its part of the weight, as the lower air is wont to do in reference to the heavy bodies that lean on it, and to the weight of the incumbent air. And therefore we need not much marvel, if when only a lefs weight than that of the fore-mentioned pillar of the atmofphere hangs at the lower marble, it fhould be capable of being drawn up by the uppermoft, rather than fuffer a divulfion from it. As we fee, that when two bodies being faftened together, are endeavoured to be drawn afunder by forces or weights not able to feparate them, they will ufually both of them move that way, towards which either of them is the moft ftrongly drawn. On which occafion, I remember what I have fometimes obferved in one of the ways of trying the ftrength of load-ftones; for if the load-ftone be able to take up more than its own weight, you may as well lift up the load-ftone by a knife, as a knife by the load-ftone. And though one accuftomed to judge only by his eyes, would have imagined, that when I held the great weights formerly mentioned fufpended in the air, there was no ftrong endeavour to pull up the upper marble from the lower, becaufe my hand being for a while held fteady, feemed to be at reft; yet he will eafily be invited to fufpect, that in fuch a thought there may be a great miftake, who fhall confider, that neither did the

the weight ſenſibly appear to pull the lower marble downwards, though my hand aſſured me that the weight had not loſt its gravitation. And if I ſhall add, that once, when the weight after having been lifted up into the air, was caſually ſo looſened from the upper marble, as ſuddenly to drop down, my hand, unawares to me, was by the force of that endeavour, it juſt before employed to ſuſtain the fallen weight, carried up with ſuch violence, that I very ſenſibly bruiſed it by the ſtroke it gave againſt the face of a by-ſtander, who chanced out of curioſity to hold his head over the marbles.

AND here it will not be impertinent to bring in an experiment that I once deviſed, not only for other uſes, but to illuſtrate the ſubject we have been hitherto treating of. The trial I lately found regiſtered among my *Adverſaria*, in theſe terms. A braſs valve of about an inch diameter was with cement well faſtened to the ſhorter leg (which was but of very few inches in length) of a long glaſs ſiphon left open at the end of the other leg. This valve being let down to the bottom of a tall glaſs body full of water, ſo that it was (if I much miſremember not) between a foot and half a yard beneath the ſurface of the water, when there was let in as much water into the pipe, as reached in that as high as the ſurface of the external water in the tall cucurbite: then about an ounce weight was put into the oppoſite ſcale of a balance, to the neighbouring ſcale, whereof one end of a ſtring was tied, whoſe other end was faſtened to the ſaid valve, whoſe parts would be thereby drawn aſunder. But when the water was emptied out of the pipe, and the valve was let down to the former depth, there was requiſite about five ounces, that is four ounces more than formerly, to disjoin the parts of the valve, and let the water get in between. And when (the ſiphon being freed from water) the valve was lifted higher and higher, together with the pipe, there needed leſs and leſs weight to make a disjunction; two ounces of additional weight (to the one ounce requiſite to counterpoize the cover of the valve itſelf) on the water ſufficing to lift up the cover, when the valve was held about half way, between its lower ſtation, and the top of the water; a ſingle ounce ſufficing afterwards, and half an ounce of additional weight proving enough to disjoin the parts, when the valve was held but a little beneath the ſurface of the liquor.

THIS relation of an experiment, which I afterwards ſhowed to many virtuoſi, will perhaps ſeem ſomewhat dark to you without a ſcheme; but if you conſider it attentively enough to apprehend it thoroughly, I preſume it will ſhow you, that whether or no there be, upon any other ſcore, a repugnancy to the ſeparation of ſmooth bodies joined by immediate contact; yet certainly there may be a great repugnancy upon the bare account of the gravity of the medium, wherein the divulſion is attempted. For in our caſe the fuga vacui, if there be any, ought to reſiſt the ſeparation of the parts of a valve ſtill kept under water, as much near the top of the water, as at the bottom. And therefore the great difference found in that reſiſtance at thoſe different places may be attributed to the preſſure of the ambient water, that thruſt them together. And though it be true, that air is an exceeding light body in compariſon of water; yet in divers trials I have found the diſproportion in gravity of thoſe two fluids not to exceed that of 1000 to 1. So that conſidering how many miles, not to ſay ſcores of miles, the air may reach upwards, there ſeems no abſurdity at all to ſuppoſe that the bare preſſure of it againſt the marbles formerly mentioned may keep them as coherent as we found them to be *.

BUT

* But ſince this I have been able to make an experiment, that does ſufficiently confirm the former doctrine. For having ſuſpended the two coherent marbles in a capacious glaſs, whence by a certain contrivance,

BUT it is now high time to look back to that part of our discourse, which the consideration of our marbles has so long inticed us from directly prosecuting. Although then it may from the past discourse be conceived, that in bodies of sensible bulk, whose smooth surfaces touch one another, the force of the air does mainly make them cohere; yet it seems, that generally in bodies (whether greater or smaller) it is a sufficient cause of cohesion, that the parts of the body are at rest by one another, though perhaps the entire concretion be removed from place to place. For bodies of sensible bulk being either fluid or consistent, and it being (as above we have taught) the chief requisite of a fluid body, that its small parts be in motion, there seems not any thing necessary to keep a body from being fluid, and consequently to keep it a firm body, but that its contiguous parts be in a state of rest.

I KNOW, that almost all philosophers both antient and modern require something else than the rest of the parts (of which scarce any of them takes the least notice, as of a thing conducive to firmness) to the keeping together the parts of a dry and stable body. But although to engage very far in such a metaphysical and nice speculation were unfit for me (at least at present, when I am but to endeavour to explicate fluidity and firmness in the sensible bodies we converse with;) yet we dare not quite skip it over, lest we be accused of overseeing it. The greater number of contemplators ascribe the effect under consideration to a certain substantial form, to which they assign, among other offices, that of keeping all the parts united into one body. But what this form is, and by what means it unites the parts so strongly in a diamond or a ruby, *&c.* and so loosely in tallow, camphire, or the like slight concretions; and how the substantial form continuing the same in water and ice, the same matter may easily and frequently become by turns a hard and a fluid body; how these, I say, and divers other things are effected by the forms of solid bodies, is to me, I confess, at least as difficult to conceive, as to imagine without it a cause of cohesion in the parts of a dry body.

OTHER learned men there are among the modern naturalists, who have recourse some of them to a spirit, which penetrating and fastening to each other all substances corporeal, unites them into one world; but others fancy rather a certain cement or glue, whereby they conceive the parts of bodies to be made as it were to stick to each other. But as for this last hypothesis, it would be considered, that though glue is made use of to join together bodies of sensible bulk, yet glue itself being a body, that is so too, it must also itself consist of lesser parts sticking to one another; which allows me to demand the cause of a mutual coherence of those parts. And if it be answered, that they likewise stick together by the intervention of some more subtile glue, I shall again represent, that this glue also must consist of corporeal parts; and therefore I shall further demand how these also stick together: and if the like answer be again made me, I shall still renew the like demand, till at length the answerer be reduced to confess, that parts of matter so very small cannot be reasonably supposed to be kept together by a cement. And if the corpuscles, that make up the finest

vance, the air could little by little be drawn out, we found, as we expected, that whilst there remained any considerable quantity of air, in the glass, the lower marble continued to stick to the other, the pressure of the remaining air, though but weak, being yet sufficient for the sustentation of the lower marble, which it was not after the air was further withdrawn. And if when the disjunction was made, the upper marble were by another contrivance let down upon the lower, so as to touch it as before; though whilst the external air was kept out of the glass, the upper part might easily be raised without taking up the lower with it; yet when the outward air was let in, the marbles were pressed together, and became again strongly coherent.

glue imaginable, are not kept together by a cement, we may be allowed to ascribe their adhesion to the immediate contact and rest of the component parts (which is a cause intelligible, and at least probable) till some other sufficient cause be assigned, which I do not take that to be, which is taught by the patrons of the first hypothesis lately mentioned, concerning a spirit diffused through the mass of matter. For not to mention, that the agility of a spirit seems not so proper a qualification for that, which is to fasten bodies together, we may consider, that this substance which is called a spirit, is indeed but a subtile body. And why therefore may not the minute parts of other bodies, if they be conveniently shaped for adhesion, stick to one another, as well as stick to this spirit? And I should here also demand, how the parts of this spirit are kept fast to one another. If any should answer, that this spirit consists of parts, which are inseparable, and yet perhaps of a hooked shape, which fits them to fasten themselves to the bodies they take hold of, and thereby those to one another; this would be to propose such a new notion of the diffused spirit, as I know not whether those, whose opinion I have been examining, did ever dream of, or would be content to adopt: and sure, according to this hypothesis, there must be a wonderful plenty of these little spirits in the grossest bodies; and ice for example, which is thought so destitute of spirits, must be well nigh half made up of them: for these little spirituous parts can fasten no parts of other bodies together but those they touch, since otherwise the parts of other sorts of matter, if but contiguous, might cohere without these, which is against the hypothesis. And since each of these small spirituous corpuscles, if I may so call them, being really a natural body, and by consequence necessarily divisible, at least by thought, into parts, I shall ask the proposers of this new notion of spirit, upon what account this corpuscle can be indivisible; I mean, what it is, if it be not rest and immediate contact, that hinders, but that the parts (or designable parcel of corporeal substance) which are divisible by thought, should be always kept together, and never be actually divided. I am not averse indeed from granting, that they may almost always escape dissolution: but I am apt to suspect, that may be, because that by reason of the extreme smallness, and the rest and strict contact of their parts, they can scarce ever meet with an agent minute and swiftly enough moved, to be able to shatter them, or dissociate the combined parts. For to say, that it is the nature of every such corpuscle to be indivisible, is but to give me cause to demand how that appears; for so important an assertion needs more than a bare affirmation for proof.

And if two of these corpuscles, that are presumed to be indivisible, should, being smooth and of the same figure (as for example cubical) happen to lie upon one another, and a third should likewise chance to be fitly placed upon the uppermost of the two, what should hinder, but that this aggregate may by the violent knock of some other corpuscles be broken in the midst of the whole concretion, and consequently in the middlemost body? For suppose them as adamantine as you please, yet since corpuscles as hard as they are, can be made very violently to knock against them; why may not these grate or break the middlemost corpuscles, or any of the others? as we see that diamonds themselves may be reduced to powder by other diamonds, though not (as artificers versed in the trade inform me) by attrition with any other stone. To prove, that the cohesion of the middlemost of the three lately mentioned small dyes with the other two, the one above it, the other below, is not so strong as that of the parts of that middlemost corpuscle, notwithstanding that the contact between each two adjoining bodies is supposed to be full (for so it must be in such bodies, though not always in others visibly greater, in which some subtiler substance may be supposed

to come in part between them;) to prove this, I say, there must be assigned some better cause of the cohesion of the matter in one part of the proposed body than in the other. And it cannot with probability be pretended, that a corpuscle presumed incapable to be divided, should consist of hooked parts: and if that should be pretended, yet even these hooks also being true bodies, the question would recur concerning them, and be still renewable *in infinitum*. If it should be said, that these minute bodies are indissoluble, because it is their nature to be so; that would not be to render a reason of the thing proposed, but in effect to decline rendering any. And though I know, that in every hypothesis about the principles of things, something is allowed to be assumed, as not being to be explained or proved by any thing more primary than itself; yet I know not whether this excuse be proper in our case, wherein it seems, that the entireness and permanency of any parcel of matter, how minute soever, may be probably enough deduced from the immediate contact, the rest and the extreme littleness of its designable parts. And if for a last refuge it should be said, that the designable parts of these corpuscles are therefore unseparable, because there is no vacuity at all intercepted between them; besides that this is contrary to the supposition formerly made; for such extremely minute dyes as we imagined to be one upon another, having their surfaces, according to our postulatum, flat, smooth, and exquisitely congruous, could, no more than the parts of either of the three corpuscles have any vacuity intercepted between them: besides this, I say, this is both to suppose a vacuum in all divisible bodies, and that too as the cause of their being such, and to decline the former hypothesis touching the use of this spirit, and take sanctuary among the Atomists, to whose opinion about the account, upon which those bodies they call atoms are not dissipated, although some of the considerations we have alleged against the newly-examined opinion may in part be applied; yet divers of their other opinions do so fairly comport with the generality of our experiments in these notes touching *Fluidity and Firmness*, that I am willing to decline clashing with them, by not pursuing now any further a disquisition, which, as I said a while ago, is not necessary to my present design: especially, since the dim and bounded intellect of man seldom prosperously adventures to be dogmatical about things, that approach to infinite, whether in vastness or littleness. Nor indeed would I have that looked upon as a resolute declaration of what I think of so abstruse a subject, which I have rather proposed to avoid saying nothing, where I supposed it expected I should say something.

THE other thing then, which in our description of a firm body we mentioned as capable to make it so, is the texture of the parts (whether homogeneous or not) that constitute it; and though the juxta-position and rest of these parts may possibly alone suffice to make the body stable; yet this texture seems to be the most usual cause of stability, and sometimes also it may super-add a degree of that quality to that, which bodies may have upon the former account only. For, though whilst the parts of the body are actually at rest, it cannot be fluid, yet those parts, if they cohere to one another but by rest only, may *cæteris paribus* be much more easily dissociated and put into motion by any external body actually moved, than they could be, if they were by little hooks and eyes, or other kind of fastenings intangled in one another; it being often necessary in this case violently to break off these fastenings, before the little bodies fastened together by them can be disjoined, and put into such a separate motion, as is requisite to the constituting of a fluid body.

WE formerly made use of that familiar substance, the white of an egg, to illustrate the nature of fluidity: let us now try, whether it will also assist us in our inquiry after the causes of stability. When an egg is made hard by boiling, since whether we suppose this induration to be effected by bare motion or impulse, or else by the insinua-

tion of fiery corpuſcles; ſince I ſay there is nothing, that appears to get in at the ſhell, unleſs perhaps ſome calorific atoms, and, perchance too, ſome little particles of the fluid water it is boiled in, it is not eaſy to diſcover, from whence elſe this change of conſiſtency proceeds, than from a change made in the texture of the parts, whereby they are connected and diſpoſed after a new manner, fit to make them reciprocally hinder the freedom of each other's motions. But if inſtead of hardening the whites of eggs by the heat of the fire, you beat them very well into froth, you may perceive that froth to emulate the nature of a ſtable body: for not only you may raiſe it up to a pretty height, and make it retain a ſharp top almoſt like a pyramid; but I remember I have, for curioſity-ſake, made with a little care a long and proportionably thick body of theſe bubbles hang down from my finger without falling, like an icicle from one of the reeds of a thatched houſe; and yet in this there appears not any alteration to be made in the fluid body, ſave a meer mechanical change of the diſpoſition of its parts: which may be confirmed by water beaten into froth, for there the heaped bubbles will quickly ſubſide and fall back into water of the very ſame conſiſtence it was of before.

Now there may be ſeveral things, whereby a body may be put into ſuch a texture, as is convenient to make it firm or ſtable. And of theſe, before we conſider of them particularly, it will be fit to take notice in general, that for the moſt part it is not from any of them ſingle, but from two or more of them concurring, that the ſtability of concretions proceeds.

THE firſt and chiefeſt of theſe ſeems to be the fitneſs of the ſhapes of the component particles to faſten to each other; as if ſome were figured like the handles of buckets, and others like the hooks, that are wont to be employed to draw them up out of the well, or ſome like buttons, others like loops, ſome like male, others like female ſcrews (as mechanicians ſpeak) or as if many together were ſo variouſly branched, that their parts may be ſo interwoven one within other, as not to be eaſily ſeparable (as we often ſee in a well-made dry hedge, of which if a man go to pull away one bough, he ſhall often be unable to do it without pulling away with it divers others, whoſe ſlender twigs will be intangled with it). An eminent example of the power of the bare texture of many ſmall bodies (even ſuch as each of them apart is not, perhaps, extraordinarily ſhaped for ſuch a purpoſe) to make a ſtable one, is afforded us by ropes and cables; where only by twiſting together and wreathing the ſlender and flexible threads the cable is made up of, they are ſo well, as it were, wedged in between and faſtened to one another, that they conſtitute a body not to be broken by the weight of an iron anchor, nor, perhaps, by the force of a ſhip violently driven on by the fury of the winds and waves. This figuration of the corpuſcles, that make up conſiſtent bodies, ſeems to have been the chief, if not only, cauſe of their conſiſtence, in the judgment of the antient Atomiſts, this being the account, that is given of it by *Lucretius*:

Denique quæ nobis durata ac ſpiſſa videntur,
Hæc magis hamatis inter ſeſe eſſe neceſs' eſt,
Et quaſi ramoſis alte compacta teneri.
In quo jam genere imprimis adamantina ſaxa
Prima acie conſtat, ictus contemnere ſueta,
Et validi ſalices, ac duri robora ferri,
Æraque quæ clauſtris reſtantia vociferantur.

AND

AND indeed, so innumerable may be the correspondent figures, which are fit to fasten bodies to one another, that it is very possible, that two bodies, whereof each apart is fluid, may upon their conjunction immediately intangle their parts in one another, and thereupon acquire such a new texture, that their parts cannot as formerly dissociate themselves at pleasure, and move along one another's surfaces, nor consequently flow after the manner of liquors, but are so connected or intangled, that the motion of one of them will be resisted by many, and so the whole body will become firm or stable. Something like this may be seen in the experiment mentioned by our author, where he teaches, that the distilled liquor of nitre, and that made *per deliquium* out of fixed nitre, will presently, upon their mixture, in part concoagulate into saline, and consequently stable, bodies; but this seeming only a re-union of the saline particles, that did, though invisibly, swim up and down in the aqueous parts of the mingled liquor, which after this separation remains both more copious than the saline parts, and as fluid as before, we will add a noble instance (mentioned to another purpose by *Lully* and *Hartman*) to declare, how much the firmness of bodies depends upon their texture. If you take then the alcohol or highly-rectified spirit of wine, and exquisitely dephlegmed spirit of urine, and mix them in a due proportion (as I remember the last time I made the experiment I took about equal parts by guess, though two of the former to but one of the latter, if this be excellent, be a better proportion) you may in and about a minute of an hour turn these two fluid liquors into a consistent body; and I confess it was not without pleasure, that I have immediately upon the shaking of these two liquors seen them shoot into the likeness of snow, and acquire such a consistence, that I could without spilling the mixture turn the vessel, that contained it, upside down.

BUT I dare not expect to have this experiment believed, even by most of them that shall try it, experience having taught me, that it will not succeed, unless the spirits of urine and of wine be both of them more exactly dephlegmed, than is usual even among chymists. Yet so much more does this coagulation seem to depend upon the salt of urine as of such a texture, than barely as urinous, that we will add that, as the spirit of fermented urine is not (whatsoever some eminent chymists may think or say) so indispensably requisite, but that my curiosity leading me to try, whether other liquors, which I supposed to be of a resembling nature, might not serve the turn, I found, that sufficiently-rectified spirit of hartshorn (to mention that alone here) may be made to supply its place: so I endeavoured to make it probable by this *, that having tried a certain method (though that may seem strange to most chymists) of so ordering urine, that without staying at all to ferment or putrefy it either forty days, or half so many hours, I can make the volatile or saline spirit ascend first in distillation, though I use but some such gentle heat as that of a bath: having, I say, by this means distilled a very strong spirit of unfermented urine, and rectified it too, I found, as I expected, that I could not by any means make it coagulate with spirit of wine, which seemed to proceed from the differing texture of this spirit from that of fermented, or rather putrefied, urine; since I had added nothing to the fresh urine I distilled, but what was extremely fixed, and belonging (as chymists speak) to the mineral kingdom. Whereupon having had the curiosity to inquire of some of my chymical acquaintances, I found, that, they complained, that they had not been able to coagulate spirit of wine with the saline spirit made of meer urine, without any addition at all, when they distilled that urine without a previous putrefaction

* This way the author afterwards published in another book.

(which

(which is not wont to be perfected under six weeks or thereabouts). But to return to our coagulum, we will annex, that this is further remarkable in this experiment, that this white coagulated substance being put into a glass vessel exactly stopt and kept in a gentle heat (which yet itself is not, perhaps, necessary, though expedient) for some weeks or months, will at least for the greatest part by much (for I have not yet totally seen it do so) resume the form of a limpid liquor; as if either all the crooked particles, that connected the small coalitions of the vinous and urinous corpuscles to one another, were, by the motion they were put into by the external heat one after another, broken off, or else the same little concretions (for the menstruum to consist chiefly of them, being able to perform other matters than either of the single liquors whereof it is constituted) either assisted by outward warmth, or enabled thereto by some other cause of mobility, did, after many and various attempts to clear themselves of each other, little by little so unbend or break off the crooked particles, that intangled them, as at length to extricate themselves, and become capable of freely shifting places among themselves, and so of the form of a liquor.

AND here I shall add a couple of experiments, for the sake of their affinity with some parts of the newly-recited experiments about the vino-urinous coagulate.

AND first it seemed to me worth trying, whether some acid salts, being duly ordered, would not concoagulate with spirit of wine, as well as with urinous salts; and having for a while digested together in a convenient proportion pure *saccharum Saturni*, made with spirit of vinegar, and rectified spirit of wine, I found the mixtures so changed in point of consistence, that upon inclining the glass, which contained it, none of it would run down the sides. But this experiment did not afterwards seem to me either easy or consistent, nor is it more than one of the ways, and I doubt none of the best, of attempting what we have proposed.

THE other experiment I promised you, relates to the resolution of the coagulum of spirit of wine (which to be performed by digestion requires a very long time). And I wish I had not some reasons to hinder me from communicating to you the way of making of it at present; it being an experiment that seems somewhat strange in its kind; but that part of it, which is directly pertinent to our present argument, you will, I trust, believe upon my relation, which is, that by the addition of nothing but of a very fixed and very dry body (insomuch that it will not yield any thing by the common way of distillation, even in a naked fire) by the sole addition, I say, of this dry body, the newly-mentioned coagulum, which is also a consistent body, may in a few hours be brought into a permanent liquor (quite distinct from the dry body) which, when experience first recommended this way to me, was of too subtile and penetrating a nature, not to make me expect from it considerable effects, both in chymistry and physic, of which intimation you may be pleased to take notice.

BUT to return to what I was about to subjoin after the mention of our coagulum, as that is an example of firmness produced by texture; I will here, because it is not easy to procure spirits pure enough to make such a concretion as that; I will here, I say, set down another way of speedily hardening one fluid body by another; for if you take the white of an egg, and beat it till it become thin, and then shake well into it about half its quantity (perhaps much less might serve the turn) of right spirit of salt, you shall have in a few minutes the mixture so coagulated, that I remember when we turned the glass, wherein we made it, upside down, not a drop of liquor did run out, though some hours after we obtained a little by breaking the curdled matter. And another experiment much of the nature of this is said to be delivered by Sir *Francis Bacon*, who teaches to coagulate whites of eggs with spirit of wine: and

and indeed, if you obſerve a circumſtance (unmentioned, that I hear of, by him) which is the ſhaking of the two bodies well together, and if your ſpirit of wine be good, the experiment will ſucceed very well; inſomuch that I remember I have made this way a coagulum, from which no liquor would drop down in about a minute of an hour. But whereas this great naturaliſt conceives this hardening of the egg's white to be performed by the heat of the ſpirit of wine, I ſhall willingly confeſs he has aſſigned the cauſe ingeniouſly, but muſt doubt, whether he have done it truly: for there are divers things, that ſeem to argue ſpirit of wine, as inflammable as it is, to abound with a piercing ſalt, and that ſuch ſaline corpuſcles may ſuffice to curdle whites of eggs, our freſhly-mentioned experiment of curdling the white of an egg with ſpirit of ſalt does ſufficiently declare; and not only we have performed the like effect with ſome other acid ſpirits, and particularly that called oil of vitriol, but it may be produced, though more ſlowly, even by a crude ſalt; for by long beating the white of an egg with a lump of allom, you may bring it for the moſt part into white curds. So that if we will allow the coagulation we treat of to be performed by the ſpirit of wine as hot, it ſeems, that that heat muſt be only ſuch, as may be aſcribed to the active particles of ſaline bodies, which yet are commonly accounted rather cold than hot. But becauſe I ſomewhat doubt, how juſtly they are reputed ſo, I will add, that I did purpoſely for trial-ſake, take the ſerum or whey, that is wont to ſwim upon men's blood after it is cold and ſettled, and endeavoured in vain to coagulate it with ſuch ſpirit of wine as would coagulate eggs; and yet this whey will at leaſt as ſoon, as (if not much ſooner than) whites of eggs, coagulate over a gentle heat of embers: which makes it doubtful, whether the effect proceed not from the greater correſpondency in texture of the ſpirit of wine with one of the liquors than with the other, rather than from the heat aſcribed to it, which did not at all coagulate the whey.

BUT although we have mentioned ſome examples to ſhew, that two fluid bodies may be aſſociated into a conſiſtent one; yet we want not an experiment to make it appear, that likewiſe by the change of texture a fluid body may be divided into two conſiſtent ones. This experiment, which we have partly taken notice of before (treating of fluidity) is, that having for trial-ſake by convenient degrees of fire diſtilled over a due proportion of the more volatile parts of ſallet oil, neither the liquor, that came over, nor the ſubſtance, that remained behind in the retort, was fluid, though the oil that yielded them had been ſo.

BUT when I put to the oil before diſtillation a convenient quantity of common ſalt, and one or two other things, that were fit to change the texture of the branched or hookt corpuſcles whereof it conſiſted; I could then obtain an oil of common oil, that both dropt into the receiver in the form of a liquor, and continued a fluid body; which may probably be of good uſe to ſurgeons, varniſhers, and men of ſome other profeſſions.

AND to make it the more likely, that by additaments of ſome ſuch nature as that newly-mentioned, ſome groſſer and cloggy parts are retained, or elſe much ſubtilized and otherwiſe altered; I ſhall add, that proſecuting a hint I happened to meet with in the diſcourſe of a wandering chymiſt, I practiſed a way ſo to defecate the dark and muddy oil of amber drawn *per ſe*, that a pretty proportion of it would come over ſo tranſparent and finely coloured, that the experiment did not a little pleaſe thoſe I ſhewed it to. And if it do not appear upon trial, that this way of preparing oil of amber does by detaining ſome parts, which, though more groſs than the reſt, may yet be no uſeleſs one, impair the remedy; and that it does not, upon ſome other ſcore, infringe the medicinal virtue of the oil, the experiment will not be unuſeful.

For the liquor, that is thus prepared, is not only very diaphanous and well coloured, but so pure and subtile, that it will swim, not only upon water, but upon spirit of wine itself. And it will be no despicable thing, if by extending or further applying this experiment to other indisposed bodies, many empyreumatical oils distilled by strong fires in retorts can be brought to emulate essential oils (as chymists call them) drawn in limbics, as to clearness and lightness.

THE additament I last thought fit to make use of for purifying oil of amber was briefly this. ℞. two pound or somewhat less of good brandy, one pound of good sea-salt, and half a pound of the oil to be subtilized, mix, and distil them together.

UPON the mention I made above of the white coagulum of the spirits of wine and urine, I remember what I have sometimes observed in the essential oil of anniseeds (as chymists speak) distilled with store of water in a limbic and refrigeratory; namely, that in the heat of summer it would remain a perfect liquor like other chymical oils; but during the cold of the winter, though they, notwithstanding that season, continued fluid as before, the oil of anniseeds would coagulate into a body, though not of an uniform texture to the eye like butter (but rather almost like camphire) yet like it white and consistent, not without some kind or degree of brittleness.

AND on this occasion I will here insert an experiment, which should have been set down in that part of the former history of fluidity, where I mention, that the small parts of a body may be sufficiently agitated to constitute a liquor by the air or other agents not sensibly hot themselves. The experiment take thus. Casting by chance my eyes in the winter-time upon a glass of oil of anniseeds, which stood coagulated by the cold of the season, I presently bethought myself of making a liquor (whose process belongs to another treatise) of which, as soon as I had prepared it, I made this trial. I melted with a gentle heat the congealed oil of anniseeds to make it flow, and then covered part of it in another glass with a mixture I had provided: and having let them both rest in the window, I found, that the meer oil being fully refrigerated again, coagulated as before; but that, which was covered with the other liquor, continued fluid both day and night, and in several changes of weather, and does still remain at the bottom of the menstruum a clear oil distinct from it, though I have purposely shaken them together to confound them.

AND because, *Pyrophilus*, I have not discovered to you the menstruum I made use of, I will here present you with a succedaneous experiment made with a common liquor. I took then good clear Venetian turpentine, and having slowly evaporated about a fourth or fifth part of it, till the remaining substance being suffered to cool would afford me a coherent body (or a fine colophony) I caused some of this transparent and very brittle gum (of which I have elsewhere taught you some uses) to be reduced to fine powder: of which I put into pure spirit of wine a greater proportion, than I judged the liquor was capable of dissolving, to the end, that when the spirit had taken up as much of the powder as it could, there might remain at the bottom a pretty quantity of our colophony. On which, though the menstruum (being already glutted) could not act powerfully enough to dissolve it, yet it might give the matter (which it had already so far softened, as to reduce it into a coherent mass) agitation enough to emulate a fluid (though somewhat viscous) body. And accordingly I obtained a sluggish liquor, which continued fluid as long as I pleased to continue the menstruum upon it. The like experiment I tried with clarified rosin, and with fine colophony, though but bought at the shops; and although the trial sometimes succeeded not ill, yet I found not the success constant and uniform, whether because the bodies to be dissolved were not defecated and pure enough, or that I did

not

not hit upon the beſt proportion between the ſolvent and them. But this circumſtance I ſhall not omit, that when the glutinous liquor was ſeparated from the menſtruum, it would by degrees, though but ſlowly, harden in the air. The application of which property, for the preſervation of ſmall and very tender bodies, I ſhall not here more expreſsly hint than by having barely named it. I had forgot to add, that whilſt the ſubſtance continued fluid, I could ſhake it (as I lately told you I could the oil of anniſeeds) with the ſupernatant menſtruum, without making between them any true or laſting union.

WHICH laſt circumſtance brings into my mind another experiment, that I likewiſe forgot to add to that part of the former hiſtory of fluidity, where I take notice, that the particular textures of fluid bodies may be reckoned among the chief cauſes of their being diſpoſed, or indiſpoſed to mingle with one another. For partly to confirm this conjecture, and partly to manifeſt, that it is not univerſally true, which chymiſts are wont to think, that acid ſalts and oils will not incorporate or mingle; I took an arbitrary quantity (and, as I remember equal weight) of common oil of vitriol and common oil of turpentine, as I bought them at the druggiſts: theſe I put together very ſlowly (for that circumſtance ſhould not be omitted) and obtained, according to my deſire, an opacous and very deep-coloured mixture, whoſe almoſt balſam-like conſiſtence was much thicker than either of the liquors that compoſed it. (The like experiment I alſo ſucceſsfully tried with ſome other chymical oils, but found none preferable for this purpoſe to oil of turpentine.) And to make it probable, that the diſpoſition of theſe liquors to mingle thus preſently together depended much on their texture, we made the mixture be warily diſtilled over (for elſe the experiment will ſcarce ſucceed) and thereby obtained (as we elſewhere mention to another purpoſe) a certain groſs ſubſtance, which was that, which ſeemed to mediate the former union betwixt the two liquors. For this ſubſtance being ſeparated, and thereby the texture of one of the liquors (or perhaps both) being changed, the liquors (which came over very clear into the receiver) ſwam upon one another; nor have I ſince been able by ſhaking them together to confound them for any conſiderable time, but they preſently part again, and do to this day remain diſtinct as well as tranſparent. But after having forgot to ſet down theſe things in their proper place, I muſt not forget alſo, that to employ here more words about them were to digreſs.

To this then annex we, that the liquor we elſewhere mention ourſelves to have diſtilled from benzoin, has been, and is ſtill, ſubject to much more frequent viciſſitudes of fluidity and firmneſs: for part of it all the year long continues in the form of a blackiſh oil, and the reſt, according as the ſeaſon of the year or of the day makes the weather cold or hot, frequently changes its texture, ſometimes appearing perfectly the ſame with the newly-mentioned oil, and ſometimes ſhooting into clear and variouſly-ſhaped cryſtals, which faſten themſelves to the bottom and ſides of the veſſel, till a warmer part of the day or of the ſeaſon reſolves them again into a liquor. And theſe two laſt obſervations may alſo ſerve to confirm what we formerly taught, that the fluidity of ſome bodies depended almoſt wholly upon the various agitation of their parts: for in theſe inſtances the parts of the anniſeeds and thoſe of the benzoin, upon the operation or abſence of the languid heat of the ambient air, ſometimes agitating them, and ſometimes ſuffering them to reſt, did conſtitute a fluid or a conſiſtent body. And having thus taken notice of this upon the by, we will add to the other examples mentioned under this ſecond head that, which it afforded us to our preſent purpoſe by ſalt-petre, which being diſſolved in a ſufficient quantity of common water, will ſeem to be loſt in it, and to conſtitute with it one uniform fluid ſubſtance;

but if a competent quantity of that water be boiled (or permitted to exhale) away, and the remaining liquor be suffered to rest a while, especially in a cool place, the saline particles will by re-uniting themselves, and by the exclusion of the aqueous parts, constitute stable and determinately-figured icicles of crystals.

THE consideration of this may suggest to us another way, that seems quite contrary to the former, whereby some bodies may become firm and solid; and that is by the intermingling of a due proportion of water or some other liquor. For, though the small parts of such fluid bodies, being themselves in motion, are apt to give those of others such an agitation as we have formerly taught, that fluidity principally depends on; it seems, that the admission of any liquor must rather conduce to the making of a body fluid than consistent: yet if we consult experience, it will instruct us otherwise; for when I have taken either an equal or a double weight of oil of vitriol and distilled it warily from running mercury, very much the greater part of the liquor would come over, and leave behind it a very white powder considerably fixed. And if we examine that familiar production of chymistry *mercurius dulcis*, (which they now use to make, by subliming together of two parts of crude mercury, with but one of sublimate, which consists chiefly of mercury already) we may find that in that, which is counted the best, the fluid body of quicksilver is so contexed with the salts it carries up in sublimation, that the dry and brittle body they compose may contain far more (perhaps twice more) quicksilver than salt. And other experiments may persuade us, that the mixture of a convenient liquor may cement bodies into one hard concretion, which would scarce be compacted together otherwise. Nor is it against reason, that it should be so; for there may be differing qualifications required to a body, whilst it is constituting, and when it is constituted; and though the motion of the parts, that make it up, oppose the firmness of a formed body, yet it may conduce to the making of a firm body. For when a great many hard corpuscles lie together loose and incoherent, they do, as we formerly noted, emulate a fluid body; whereas by the mixture of a liquor, those loose corpuscles being for a while dissociated and put into motion, they may after many evolutions apply themselves to one another after that manner, that is most requisite to make them touch one another closely, and according to a greater surface. Whereupon it often follows, that the liquor, in which they did formerly swim, is either squeezed out upon their closing, or else so dispersed in small particles, and disposed of among those of the harder corpuscles, that they are unable to agitate them, or prejudice their mutual cohesion.

AND here to dilucidate the subject under consideration by an instance, that seems very pertinent to it, we will make a further use of the experiment formerly mentioned touching the burning of alabaster. For if the powder, after it has done boiling, and has been sufficiently burnt, and kept some hours (the most experienced artificers observing, that it is not so convenient to employ it presently after it is taken off the fire) be well beaten and tempered up with fair water almost to the consistence of thin pap, if the powder have been rightly prepared and skilfully tempered, you shall see that fluid substance in a few minutes of an hour begin to set (as the tradesmen speak) that is, to exchange its fluidity for firmness; so that if it were before cast into a mould, it will perfectly retain the figure of the internal surface thereof.

Now that in our mixture there is for a while such an agitation of the hard parts produced upon the affusion of the water, and afterwards an exclusion of the superfluous water, we may confirm partly by this, that when any considerable quantity of burnt alabaster is tempered up with water, the mixture after a little time grows sensibly hot, and sometimes continues so for a pretty while; and partly also by this, that

that having purposely for trial's sake filled a new and good glass phial, containing about half a pint, or pound, with the mixture we speak of, and when it was top full, stopped it up very close, the liquid mixture within less than half an hour cracked the phial (though standing in a window) in several places, and at those crevices discharged itself of about a spoonful of clear water; the remaining mixture retaining perfectly the figure and dimensions of the phial, and growing as hard as chalk, or somewhat harder, insomuch that we were fain to employ several strokes with a strong iron to divide the mass.

AND let me here add, that some other substances may this way afford much solider bodies than burnt alabaster does; and therefore it may be a thing of good use to inquire out and try what other bodies, easily to be procured, may be thus brought to a new and lasting solidity. For the learned hydrographer, *Fournier*, speaking of those dams or digues (as he calls them in his language) which are sometimes made in the sea to secure shipping, (as I have seen at the port of *Genoa* and elsewhere) after having told us, that the Romans made the fairest harbours in the world by the help of a certain sand to be met with at *Cuma* and *Puteoli* in the kingdom of *Naples*, which sand mingled with a third part of quick-lime acquires in the water a flint-like hardness; subjoins this observation of his own, * *Jay veu, &c.* that is, I have seen (says he) in *Flanders* near *Tournay* a certain sort of ashes of lime made of marble, which was excellent for any kind of work made in the water. For having made a bed of great stones, they cast upon them whole bags full of such ashes instead of mortar, and the water betwixt the stones having tempered up these ashes, petrified them to that degree, that in a short time they became as hard as marble. Thus far he. But to pursue our former discourse.

THAT also, which we intimated of the conduciveness of the various tumblings to and fro of the hard particles to their uniting into one firm concretion, seems confirmable by what we have observed in some saline liquors, especially certain parcels of spirit of hartshorn, which whatever were the constitution of the ambient air, remained fluid some of them for many months, after which the saline corpuscles began to shoot at the bottom of the remaining liquor into exquisitely-figured crystals, which at length grew copious enough. For this spontaneous coagulation of the little saline bodies happening so late, it seemed, that it was preceded by almost innumerable evolutions, which were so many and so various, that at length the little bodies came to obvert to each other those parts of themselves, by which they might be best fastened together and constitute a firm body. Which conjecture seemed the less improbable, because we could not well imagine, that this coagulation proceeded (as that of dissolved alum and other salt is wont to do) from the evaporation of the superfluous liquor; for the glasses, wherein what we have mentioned happened, being carefully stopped, there was no danger of such an avolation; and if any thing could get away, it must have been the subtile, piercing and fugitive spirit, (which indeed, as my nose had informed me, does oftentimes penetrate ordinary stopples, for the flying away of those volatile parts would only have left the remaining liquor more aqueous. And it is well known to those, that deal with such kind of liquors, that the more aqueous they are, the less apt they are to crystallize. And however it will serve our turn, that there was but an insensible diminution of the liquor upon the recess of whatever it was that got through the cork.

* *Hydrograph. du P. G. Fournier*, Lib. II. Cap. 5.

To

To the same purpose I remember also, that having in a crystal phial carefully kept a pretty quantity of well-coloured tincture of amber, made with pure spirit of wine, it remained fluid for a year or two, and during that time presented us with a strange phænomenon, that belongs to other papers †. But having been absent for two or three years from the place where we locked it up, we found, when we came again to look upon it, that though it had formerly remained fluid so long, yet several yellow lumps of amber, almost like beads, with one side flat, had here and there fastened themselves partly to the bottom, and partly to the sides of the glass; the rest of whose internal surface continues yet transparent.

ANOTHER thing, whereby bodies become stable, is the admission of adventitious corpuscles into their pores and recesses. And of the ways, by which these foreign corpuscles may bring the substance they invade to be compact, these four appear the chief.

FIRST then, the adventitious corpuscles we speak of may produce stability in the matter they pervade, by expelling thence those voluble particles, which, whilst they continued in it, did by their shape unfit for cohesion, or by their motion oppose the coalition, or disturb the rest of the other particles whereof the body consisted. But of this having already discoursed, proceed we to what is to follow.

IN the next place then, foreign bodies may contribute to the stability of a substance they get into, by hindering the motion of the little bodies, that constitute it.

AND thirdly, such advenient bodies, especially if they be not of the smallest size, may produce a firmness in the substance, which they get into, by constituting with the particles it consists of corpuscles more unapt for motion, and fitted for mutual cohesion.

THESE two we mention together, because that very often nature employs them together, for the introducing of stability into matter.

To these seems to be reducible the way of turning the fluid body of milk into curds by the mixture of a little runnet; whose saline particles pervading the body of the milk, do not only make a commotion in the parts of it, but fasten the branched particles of it to one another, and with them constitute a body of another texture than was the milk; and the weight of these curdled bodies reducing them by degrees into a closer order, does, whilst it presses them together, squeeze out the thinner and more serous liquor, which the runnet was unable to coagulate, and which being thus sever'd from the grosser parts of the milk, may well be more fluid than milk itself is wont to be. And that there is some coalition of the particles of the runnet with the coagulated ones of the milk, may appear by the complaints, that housewives sometimes make of their dairy-maids, that the cheeses taste too strong of the runnet, when too great a proportion of it has been mingled with the milk. And though we ascribed the curdling of the milk to the saline particles of the runnet, we ignore not, that, not only common runnet, but also divers juices of herbs will curdle milk, as is well known in those parts of *Italy*, where cheese is made without runnet. But we made especial mention of the saline corpuscles of the runnet, because really housewives are wont to salt it, and because saline liquors do manifestly and powerfully operate in the coagulation of milk, which may be curdled by juice of lemons, and I know not how many other acid salts. And to manifest yet further the coagulative power of them, we have sometimes in about a minute of an hour arrested the fluidity of new milk,

† This phænomenon is partly described at large in one of the author's *Physico-Mechanical Experiments*.

and turned it into a curdled substance, only by dexterously mingling with it a few drops of good oil of vitriol. But of the effects of various salts upon milk we elsewhere may, and therefore shall not now, discourse.

BETWEEN this last recited experiment, and the two following ones, it will not be improper to insert the immediately ensuing one, for the affinity, which it seems (in different respects) to have with both.

I REMEMBER then, that I divers years ago prepared a salt, which either was, or at least answered well to the qualities ascribed to that which is now called *Glauberus's sal mirabilis*, which seemed to have in it a coagulative power, in reference to common water. For whereas salt of tartar, common salt, nitre, *&c.* being dissolved in water, do upon evaporation of a sufficient quantity of that water, recover indeed their pristine saline forms, yet they do but coagulate themselves without concoagulating with them, either any water, or at least so much, as chymists have thought worth the taking notice of. Whereas this salt we speak of, being prepared for the purpose, and dissolved in a convenient quantity of water, does upon its re-coagulation so dispose of the aqueous particles, among its own saline ones, that if the experiment be well and carefully made, almost the whole mixture will shoot together into fine crystals, that seem to be of an uniform substance, and are consistent enough to be even brittle, and to endure to be pulverized, sifted, *&c.* though the concretion may have such a proportion of water in it, that (as I remember) when the experiment succeeded well, from three parts of water, and but one of salt, I had about four parts of crystals.

I NEED not tell you, that this salt seems to have a somewhat more than ordinary resemblance of a true coagulum, since it reduces so much water into a stable consistence; yet it does in no contemptible proportion materially concur to the body produced. But I may hereafter (which I must not do now) entertain you about a salt of a differing kind from this; and which put me upon considering, whether there may not be a coagulum more properly so called of common water, which may in a very small proportion operate upon a great quantity of that liquor, as runnet does on milk.

I HAVE not yet examined, whether it will be sufficient to refer merely to the second and third ways lately mentioned of making bodies become stable in the phænomena I am about to speak of, or whether it may be reasonably supposed (and added as a fifth way) that the bodies to be coagulated may (in great part) be brought to be so; by so acting upon the bodies to which they are put, that the agent liquor (if I may so speak) does by its action communicate to the subject it works on, or lose upon some other account some subtile parts whose absence fits the disposed remaining fluid for such a cohesion, as may suffice to make a body be (though very soft, yet) consistent. But however, it will not be amiss to take some notice of effects, which, what e'er the cause be, belong to the *History of Fluidity and Firmness.*

I SOME years since prepared a substance of a whitish colour, which would not only destroy the fluidity of some other liquors, but would give a consistency to a notable proportion of oil of vitriol itself, though the parts of this liquor be presumed, upon the score of its corrosiveness, and its aptness to grow very hot with many other bodies, and make them smoke, to be very vehemently agitated.

AND I remember, that I sometimes shewed the curious a glass phial well stopped, upon the bottom of which lay a little of this newly-mentioned whitish powder, over which there was a considerable proportion of oil of vitriol in a consistent form, without seeming to have any thing to do with the powder, as indeed it had been only poured

poured upon it, and suffered to stand in the cold for some time (which if I mistake not was a day or two) at the end of which the above-mentioned change was wrought on the liquor by the powder, which did not appear to be dissolved thereby. Which phænomenon seemed indeed to argue, that there happened in this experiment (that was not the only one of the kind I then made) something like the coagulation formerly mentioned of quicksilver by the vapour of lead; some subtile parts of the coagulator, if I may so call it, invisibly pervading the liquor, whose fluidity was to be suspended, though it seem not improbable to me, that the effect produced might depend upon both causes, this newly expressed, and the other a little above mentioned; where I guessed, that a change of texture, and thereby of consistence in the menstruum, might be the result of the operation of the menstruum, and the body it acts upon. And because this powder is not so easy to be prepared, I shall add, that you may (though not so well as by the newly-mentioned way) see the coagulation of a menstruum upon a firm body, which it does not seem to dissolve, by the ensuing experiment: Take crystals of salt-petre very well dried, but not powdered; and gently pouring on it in a glass phial some good oil of vitriol till it swim about half an inch, or perhaps more, above the salt; leave the phial closed with a cover of paper in a cool quiet place, where it may not be shaken: and if the trial succeed with you as it did with me, the liquor will, though slowly, so settle itself about the nitre, that though you incline the phial to any side (or perhaps turn it upside down) it will not run out: and I have sometimes taken notice of little saline bodies, and as it were fibres, that seemed to keep the parts of the mixture united together. I made also some other trials to coagulate unflegmatick A. F. upon nitre and some other bodies, the phænomena of which trials did not oblige me to renounce the lately-mentioned conjectures about the causes of such changes of consistency in liquors, as I have been speaking of: for I still think it highly probable, that the best coagulator I have met with acts but as a finer sort of runnet, which in an inconsiderable quantity really disperses material parts of itself through the liquor to be wrought on, though these, when the coagulator is a consistent body, be, perchance, so few or subtile, as not to make any visible diminution of the body it parts with.

A MORE eminent example to our present purpose may be afforded us sometimes, (for I am sure the experiment will not always succeed) by the notable way of coagulating quicksilver, and thereby turning it from a fluid into a firm body, by the vapour of melted lead, in which, when it is taken off the fire (but before it be quite grown hard again) a little cavity must be made with a pebble or a stick, that the quicksilver tied up in a rag may be nimbly put into that hole, and be congealed by the permeating steam of the cooling lead. Which effect may be less hopefully expected by the way wont to be prescribed by authors (most of whom I doubt never made trial of it) than by another that I have practised, and may on another occasion shew you. And that some metalline steam does really invade the quicksilver, seems probable by the wasting of lead by fusion, and by the operations ascribed by chymists to the fume of lead upon gold, about which I may elsewhere tell you what is come to my knowledge.

AND I remember, that not long since, an ingenious physician of my acquaintance keeping some lead long in fusion to reduce it *per se* into a calx, and holding his head often over the melting pot to observe the alterations of the metal, was suddenly purged divers times both upwards and downwards, which both he and I ascribed to the saturnine exhalations. And though I suspected the congelation formerly mentioned might proceed from the egress of some subtile substance, that formerly agitated, but

but after deserted, the mercurial corpuscles; yet that the concretion of the quicksilver might be effected by some benumbing vapour of the lead, seems confirmable by a notable accident, that befel that famous geometrician Doctor *Wallis*, who related it to me as a phænomenon he knew not well what to make of; namely, that he and other learned men at *Oxford* being minded to make the experiment under consideration, they found that, upon the first fusion of the lead, the immersed quicksilver was very well coagulated by it; but when they came to melt it the second time, and put new quicksilver into it, the experiment would not succeed; at which they wondered, finding by trials, that the lead might be so easily deprived of its power of hardening quicksilver. That this observation will always hold true, I am not apt to believe; but that such learned and candid naturalists should either be mistaken in making it, or misrelate it, it were injurious to suspect: wherefore supposing that to have then at least happened, which one of them registered in writing, and more than one of them told me; it seems to countenance what we have delivered, and looks as if, according to our doctrine, there were in lead a coagulative steam or spirit, and yet (at least in that parcel of metal) in so small a proportion, as that it almost totally dislodges or spends itself upon the first opportunity it meets with of passing into quicksilver.

WE have elsewhere, to another purpose, mentioned our having sometimes (for, as we there advertise, it will not always succeed) made an experiment, which seems of kin to the former, and may give much light to the matter under consideration, and it was this; we poured upon aqua fortis common sallet oil, which floated together at the top of it, but after some hours had its texture so changed by the ascending steams or other subtile insinuating particles of the saline liquor, that it was turned into a white consistent (and sometimes a brittle) body like butter, remaining all in one cake on the top of the menstruum. And the like experiment (but in a longer time) we have performed with expressed oil of sweet almonds instead of common oil.

AND to shew further, how much the operation of the same visible agent may be diversified as to the production of fluidity or firmness, according to the differing dispositions of the bodies it acts on; I have sometimes (I remember) taken the same aqua fortis, or spirit of nitre, wherewith I had coagulated expressed oil of olives, and having poured it off from the butter-like substance; I cast into it some good camphire, which, without heat, was thereby reduced into an oil, that retained a distinct superficies from the menstruum, which it swam upon, and would not incorporate with; so that the same numerical menstruum, without the help of any degree of fire, turned a brittle body into a liquor, and the liquor into a brittle body, (for such is the substance, that may be made of common oil, if it be suffered to float long enough upon the coagulating liquor;) which brittle substance (to add that upon the bye) seemed to have received a more durable alteration from the steams of the menstruum than was expected. For not only when melted with fire, it would upon refrigeration recover its consistence without becoming again fluid, as when it was in the form of oil; but I made a trial or two, without success, to reduce it to a liquor, by mixing it with oil of tartar *per deliquium*, which, you know, has a great faculty to find out and mortify acid spirits, such as those of the nitre or aqua fortis, that had (whether as mere acids I now examine not) coagulated our oil.

THE fourth and last way, whereby corpuscles entering from without into a body may give it a stable consistence, is by making such a commotion in the parts of it, as may make them apply themselves one to another according to a greater surface, or

otherwise complicate and dispose them after the manner requisite to make them stick together.

THIS way of making bodies become consistent, is seldom or never employed by nature without the concurrence of some of the other ways already mentioned: but we have distinguished it from the two last recited, because in them we suppose, that some of the adventitious corpuscles are stopped in the body, to whose firmness they conduce, and (though perhaps but in a very considerable proportion) do concur to make it up; whereas here we suppose, that without materially concurring to constitute the body they work upon, they do only agitate and variously move the particles it consists of, perhaps breaking some, bending and twisting others, and in a word so altering the texture, that the parts, that did formerly either move separately, or adhere together but loosely, are now reduced to a closer order, or a more implicated texture, and thereby more firmly connected to one another. That the bare disposition of the parts of a body in reference to each other, without any addition of foreign matter, may do much towards stability, we may see both in some examples formerly mentioned, and in osier wands, which, when lying loosely in an heap together, may each of them very easily be dissociated from the rest; but when they are breeded into a basket, they cohere so strongly, that when you take up any one of them, you shall take up all the rest. To which may be added those many obvious, though perhaps unheeded, instances, wherein by the bare texture of the slender hair or filaments, whereof wool or silk consists, cloth, silk-stockings, and many other durable garments are made by illiterate tradesmen.

WE may also observe the force of bare motion in altering the texture, and thereby the consistence of bodies by the common way of churning, for there the external impulse of the churn makes a great commotion in the parts of the cream, and tumbles and shuffles them perpetually to and fro among themselves; whereupon it happens, that the more branched corpuscles meeting with one another, are intangled, and thereby separated from the rest, and after many occursions all these parts are at length fastened to one another, and excluding those of the butter-milk, which seem not so conveniently shaped for mutual cohesion, do constitute butter: which is wont to be made yet more consistent, or rather more compact, by being beaten or otherwise compressed, as the parts thereby reduced into a closer order squeeze out the fluid butter-milk, that was intercepted among them.

IT will perhaps be thought more strange, that a fluid body, nay a distilled liquor, which is very volatile, and passes for simple and homogeneous, and is at least far less compounded than milk, should by motion, without the mixture of any new matter, be made coherent: and therefore I hope, that it will not only second the example newly alleged, but likewise confirm some main points of our doctrine touching firmness, if we observe, that even the chymical oil of turpentine, which passes for one of the principles or elements of that body, may be in great part, if not wholly, coagulated without addition. And yet (not to anticipate what I may have occasion to deliver elsewhere concerning this experiment) I shall now only relate, that inquiring a while since of a very expert chymist, whether he had not sometimes observed (which I have often done, as I elsewhere declare) oil of turpentine to begin to coagulate, if it were often distilled; he went with me to his laboratory, and there let me see in a receiver some oil of turpentine, which he had often distilled over *per se*, in good part coagulated into a whitish and consistent body: affirming also to me, that he had sometimes, by frequent distillations, without addition, obtained from clear oil of turpentine a far greater proportion of such a stable substance. Whose consistence,

ſiſtence, whether it ſhould be aſcribed to the fire's breaking the oily corpuſcles into parts more fit for mutual coheſion, or whether it proceed from a new texture of the ſame corpuſcles, only chancing by thoſe various evolutions to be diſpoſed after ſuch another manner, as to complicate or otherwiſe connect them, I need not now ſpend time to inquire; ſince it is enough for my preſent purpoſe, that in this example we have one, that declares, how much even motion, without the addition of any ſenſible ſubſtance, may in ſome caſes conduce to firmneſs.

AND here to illuſtrate our doctrine about this quality and fluidity, by ſhewing what the inteſtine motion of the parts, even without the aſſiſtance of adventitious heat, may do, to make a body change its conſiſtence, according to the previous diſpoſition of the matter, and become of firm, fluid; as we lately ſaw oil of turpentine made of fluid, firm: I will add on this occaſion, what I obſerved of oil of wax diſtilled in a retort with an additament of the like nature with that I formerly mentioned, when I ſpoke of the fluid oil drawn from oil of olives. For this oil of wax, though at firſt it came over for the moſt part, if not totally, in the form of a butter; yet by ſtanding on a ſhelf (and that not in a hot place, as a ſtove or laboratory) I obſerved it little by little to reſolve into a tranſparent oil; and purpoſely inquiring of him, that looked after it, whether or no this effect might not be aſcribed to the increaſed warmth of the weather, he aſſured me of the contrary, having taken notice what effects the changes of weather had upon it.

BUT what if we ſhould ſay, that fluidneſs and ſtability depends ſo much upon the texture of the parts, that by the change of that texture the ſame parts may be made to conſtitute either a fluid or a dry body, and that permanently too? Theſe laſt words I add, becauſe of what may be ſaid to this purpoſe concerning the change of water into ice, and ice into water, and of metals into fluid or hard bodies, by fuſion and refrigeration: for in theſe examples the acquired hardneſs of water and fluidity of metals may be preſently loſt upon the bare removal of thoſe bodies into a temperate air; whereas in the inſtance we are to give, the acquired texture is ſo durable, that without an extreme external violence, ſuch as would deſtroy moſt other ſtable bodies, it is not to be deſtroyed. And this inſtance is afforded us by that admirable repoſitory of nature's wonders, quickſilver: for if ſome ounces of this fluid mineral be put into a convenient glaſs veſſel, and that veſſel be firſt exactly ſtopped, and kept for ſix, eight, or ten weeks (or longer, if need be) in a ſand-furnace, whoſe heat may be ſtrong and conſtant, the corpuſcles, that conſtitute the quickſilver, will, after innumerable revolutions, and perhaps bendings, twiſtings, and other changes, be ſo connected to one another, that inſtead of a fluid body, they will appear in the form of a red powder, that chymiſts precipitate *per ſe:* which change is ſo unexampled, that though among the more curious Spagyriſts it be very well known, yet many Naturaliſts cannot eaſily be brought to believe it; whom to convince of the poſſibility of it by a much leſs tedious preparation, I take half a pound or a pound of quickſilver, and with a ſtrong fire diſtil it out of a glaſs retort, and for the moſt part there will remain in the bottom and about the ſides of the veſſel a little red powder, which ſeems to be nothing but part of the fluid body (moſt expoſed to the action of the fire) turned into a dry one in eight or ten hours ſpace.

AFTER what manner the fire produces ſo odd a change in the quickſilver, I do not preſume to know. It is true, that though the parts of liquors do, as we have formerly taught, touch one another but in part of their ſuperficies, yet they all of them ſeem to have ſome degree of viſcoſity, or ſome ſlight and looſe complication or other kind of adheſion of parts, as appears by their being ſo eaſily contexed into thoſe thin

membranes or films we call bubbles, insomuch that not only spirit of wine, that seems the most light, and most fluid of liquors, will afford bubbles, but (what may seem strange) we have divers times purposely observed, that quicksilver itself, as ponderous as it is, especially being suffered to fall in a slender stream into a vessel almost full of the same mineral, will afford bubbles numerous and large enough, although (as those also of the spirit of wine) quickly vanishing.

AND hence it might be imagined, that in the operation we are treating of, some such change is made in the quicksilver, as we formerly observed to be made in the white of an egg, when by a new disposition of its parts, either heat or beating it makes it a kind of stable body, or else it might be pretended, that there is a variety of parts argued to be in quicksilver by the great variety of its effects upon other bodies, and that by the frequent evolutions, which the fire makes of those parts among themselves, they come at length to be so applied to one another, that either they lock into each other, as it were, or slip upon one another's surface in such a manner, as that as much of their surfaces immediately touch one another, as is requisite to make them cohere, as we formerly mentioned of several very smooth pieces of glass mutually adhering without any other cement, than the congruity and immediate contact of their surfaces. But though these conjectures and divers others might be proposed, yet I fear all of them would prove but meer conjectures. Nor were we much assisted to make better by looking upon our mercurial precipitate in one of the best magnifying glasses in the world; for what we there discovered was only, that the red powder had in it many corpuscles of sundry other colours, and that the little grains of powder seemed to be of no determinate shape, but looked like slender fragments of red coral. And having put some small dust of a shining precipitate of gold and mercury into the same augmenting glass, all we could discern was, that the little grains of this precipitate differed from those of that made of mercury alone, in that these (which a chymist would take notice of) were so transparent throughout, that one would verily think he beheld the best sort of those precious stones goldsmiths call granats. But though we pretend not to make out, how the new texture is produced in the quicksilver, yet to make it still more evident, that its change of consistence proceeds from its change of texture, we will add, that having a great curiosity to try whether our powder could not be made fluid again, I procured some precipitate *per se* of a person, who formerly lived with me, and was expert in many mercurial operations, and presented me some of his own making: this being weighed, and put into a convenient glass, was carefully pressed with a naked fire (which should be stronger than that wherewith it was precipitated) and at length it rose by degrees in fumes, which settled in the neck of the glass in many drops of revived running mercury; all which being collected into one, we found, that there wanted but about a sixth or seventh part of what we had put in, and we supposed we should not have wanted that neither, but that the vehemence of the fire had melted the glass, which swallowed up a part of the powder, that made a great shew through it, after what was colliquated had been removed from the fire.

THIS experiment brings into my mind another, that was judged uncommon enough, and it was this: Being not long since master of about half an ounce of a certain mercury, which some ways of examining it, that I had employed, induced me to think mercury of Saturn; I imagined (for some reasons) that it might be made very serviceable to confirm our doctrine touching fluidity and firmness. And accordingly I found upon trial, that I could, barely by shaking it long, reduce it to a black powder: in which form it would continue as long as I please to let it do so.

And when to the bystanders there appeared nothing in it, that gave suspicion of a fluid body, I could in a trice, only by dexterously rubbing it in a small marble mortar, reduce it little by little into running mercury, as it had been before. Which quick passage, from one quality to another, being made, not only without the help of fire, but without adding or taking away any visible substance, proved no ignoble instance, how much motion and rest, and the thence easily resulting texture of the component corpuscles of a portion of matter, may contribute to its fluidity or firmness.

FROM the experiment of precipitating quicksilver *per se*, and from some other things, partly delivered already, and partly to be delivered by and by, we may learn what to think of the opinion of some eminent modern philosophers, who teach, that a fluid body is always divisible into bodies equally fluid, as quantity into quantities, as if the particles of fluid bodies must also be fluid themselves: for by them it seems to appear, that quicksilver, and some other actually fluid bodies consist very much of hard corpuscles, since by the change of their texture they may be deprived of their fluidity and become stable. We see also, that the stiff and solid particles of salts dissolved in common water, and of silver dissolved in aqua fortis, being by those liquors sufficiently dissociated and separately agitated, do with them constitute fluid bodies. And we have elsewhere mentioned to another purpose an experiment, which may not impertinently be repeated here; namely, that by putting together into a glass retort one part of quicksilver and four of common oil of vitriol, and distilling them in a sand furnace with a strong fire, there remained in the bottom of the vessel a ponderous calx or powder, so far from being fluid, that it was but in part dissoluble in water: and that, which seems to prove, that in the very liquid oil of vitriol, though a distilled liquor, the saline corpuscles, that chiefly compose it, do retain their stiffness (generally to be found in undistilled salts) is, that by steeping our calx in fair water, we could separate from it a considerable quantity of particles, which upon the evaporation of the water coagulated into store of saline and brittle bodies. And that these proceeded rather from the menstruum than the metal, we were induced to think, by observing the dry calx, before any water was poured on it: for though the saline part of the mixture did not weigh (perhaps any thing near) so much as the mercurial distinctly did, yet the aggregate or mixture did weigh a great deal more than the quicksilver did when it was put in; and the oil of vitriol, that was abstracted, a great deal less, than it did before it was committed to distillation. Nay, I once or twice observed in a glass, where I kept a quantity of oil of vitriol, that there did spontaneously fasten themselves to the sides little saline crystals, which, when I took out, I found hard and brittle; but when I had, for trial-sake, exposed them to the air, they presently resumed a fluid form, and appeared to be oil of vitriol. In the observation also lately mentioned concerning the spontaneous coagulation of spirit of hartshorn, it seems evident, that bodies, which are all or most of them hard, and appear so when they are commodiously connected to each other, may yet constitute a fluid body when they are reduced to sufficient smallness, and put into a convenient motion. And indeed, if the least particles of fluid bodies were not (many of them at least) indowed with their determinate bigness and shapes, but that such fluid bodies could be always divided into particles fluid also, how comes it to pass, that some liquors cannot pierce into or moisten some bodies, which are easily pervious to other liquors? For if the particles of the excluded liquor were of necessity always divisible into fluid ones, there seems no reason, why they should not be sub-divided into

into so very small ones, as that no pores can be supposed little or oddly figured enough to keep them out.

It is true indeed, that as it is hard to demonstrate, so it is not easy to disprove, that the matter, whereof fluid bodies consists, is capable of being indefinitely divided: and it may be granted too, that by how much the smaller parts a body is divided into, by so much the more easily, *cæteris paribus*, are the parts of that body to be put into motion. But this divisibility of a fluid body into perpetually lesser and lesser parts, belongs not to it properly as it is fluid, but as it is a body; such divisibility, if supposed true, being a primary affection of matter itself, and belonging as well to those portions of it that are hard, as to those that are fluid. And though it were admitted, that such an endless division, as is presumed might be made mentally, (as they speak in the schools) that is, by the thought or operation of the mind, yet it would remain a great question, whether or no nature does actually so far mince and sub-divide bodies; as may appear by what has been freshly noted. And however, it is not only requisite to the constitution of a fluid body, that the parts of it be small enough, but that they be also actually moved. For we observed, not long since, that the dust of alabaster put into motion did (though its corpuscles were not insensible) emulate a fluid body, and immediately ceased to be fluid, when they ceased to be agitated: whereas the particles of water, as minute and apt as they are to constitute a fluid substance, do yet make that hard and brittle body we call ice, when those little particles, upon what account soever, are reduced to be at rest.

By what has been hitherto discoursed, we may also be assisted to judge of the doctrine of the chymists, who teach, that in all bodies, coagulation, stability, hardness, and brittleness depend upon salt: for though what above has been said of curdling of milk by saline liquors, and the hardness and brittleness obvious in salts themselves, may keep us from denying, that the saline principle is very powerful in the coagulation of some bodies, and does produce much firmness or even brittleness in many or most of the concretes wherein it is predominant; yet this hardening power of salt seems not to proceed from any peculiar and inexplicable property it has to coagulate other bodies, or make them compact, but from the shape and motion of its corpuscles, which it seems are more fitted by nature, than those of many other concretes, to insinuate themselves into the pores of other bodies, and fasten their particles to themselves, and to one another, either by wedging their corpuscles together, or by their stiff and slender parts, or their sharp angles, or edges piercing divers of them together; as when many pieces of paper are kept from scattering by a wire, that runs through them, or as when a knife takes up at once divers pieces of bread and meat, by being stuck into them all. But whensoever there is in the constituent parts of the body a sufficient fitness and disposition to adhere firmly to one another, nature may of those parts compose a stable body, whether they abound in salt or no; it not being so much upon chymical principles, or even upon the predominancy or plenty of any one ingredient, as upon the shape and motion of the component parts of bodies, that their fluidity and firmness depend. I will not here urge, that salts are generally reducible by an easy mixture with water into the form of liquors; nor that sea-salt, salt of tartar, and divers other sorts of salts, will of themselves, even in the air, if not very dry, assume the form of fluid bodies; nor yet will I press the shortly to be mentioned example of coral, which is confidently affirmed to be soft, whilst it remains in the salt water, and to grow hard when taken out of it: I will not here, I say, press these and the like arguments, but content myself to have hinted

hinted them, because they are such, as I cannot well in few words make out and vindicate. Wherefore I shall rather demand, what salt can be made appear to pass out of the body of melted lead into that of quicksilver, to perform in it the coagulation above mentioned? What accession of salt is there to be observed, when running mercury is precipitated *per se* into a powder? And how will it be proved, that when in a well-stopped glass the whole body of water is, in frosty nights, turned into firm ice by the cold of the ambient air, that coagulation is performed by salt, it having not yet been made appear by chymists, that either salts or even the distilled spirits of them can penetrate, without a kind of prodigy, the narrow pores of unheated glass? It is usually observed in eggs, that though at their first coming out of the hen's belly, the shells are soft, yet soon after they grow hard and brittle; and yet it appears not, how the saline ingredient is increased to effect this speedy induration: and (to subjoin that by the by) albeit I am not averse from thinking, that the coldness of the outward air, and its imbibing some of the loosest of the moist parts of the soft-egg-shell, may concur to this effect; yet there are many observations of egg-shells, that have been found hard in the womb of the hen. And I well remember I have taken notice, that divers eggs not yet laid, but found at one time in the body of the same hen, were each of them furnished with a compleat and brittle shell. But I think I can draw a much stronger argument against the chymical opinion from the consideration of an egg; for I demand, what plenty of salt can be made appear to pierce the hard shell, and more close-wrought membrane, that both lines it and involves the egg, especially since it is certain, that in *Egypt*, and divers other places, eggs may be hatched by a temperate external heat without the hen. And yet we may here observe, that the same internal substance of the egg, which at first was fluid, the yolk and white, that composed it, being so, is upon the exclusion of the chick turned almost all of it into consistent bodies, some of them tough, as the membranes and gristles of the bird, and some of them harder and almost brittle, as his bones and beak; and all this, as we said, without accession of new salt. At would be hard for chymists to prove, that diamonds and rubies, which are counted the hardest bodies we know (and at particular trials of whose hardness I have sometimes wondered) do abound in salt; at least it will not be unreasonable for us to think so, till chymists have taught us intelligible and practicable ways of separating (at least some) true salt from either of those jewels. And it may be also doubted, whether the blood of animals, when it is freest from serum, do not (though a liquor) as much abound with salt as their skins or their flesh.

AND since it is with chymists that I am now reasoning, I presume I may be allowed to press them with arguments drawn from some of the eminentest writers of their sect. For the generality of chymists, and even those, that are by the rest, and themselves too, called philosophers, not only granting, but asserting and maintaining the transmutation of great quantities of quicksilver and other ignobler metals into silver or gold by means of the white or red elixir, I shall demand of them, whence it happens, that one grain of the powder of projection can turn a whole pound of mercury into true gold or silver, and consequently change a very fluid body into a very firm one; though the proportion of salt employed to coagulate the whole mass of quicksilver would not amount to the six thousandth or seven thousandth part of the liquor; though we should grant, that the powder employed to work this marvellous change were all of it salt, to which yet chymical writers ascribe much more of the sulphurous nature. And to this I shall add, what the famous and acute *Helmont* does to another purpose relate upon the experience of *Raymund Lully* and his own, concerning his:

his prodigious liquor, alkahest; namely, that being abstracted from common quick-silver, it does in a quarter of an hour coagulate it: and yet in this coagulation he points at this as a singular phænomenon, that this liquor, which is as well immortal as exceeding saline, leaves nothing of itself with the mercury, on which it works, and yet so coagulates it, that he prescribes the making it into a subtile powder.

Helmont de Febr. cap. 14.

I REMEMBER also to our present purpose, that a physician of much veracity in what he relates, discoursing with me the other day about an odd preparation, that he saw at the present Duke of *Holstein*'s (that learned prince and great chymist) assured me, that among other things he there took notice of a glass of spirit of urine, which in warm weather remained in the form of a liquor, but in cold weather did totally coagulate into crystalline salt. And being asked by me, if he knew how this urinous body had been prepared? he answered me, that the Duke caused spirit of urine exceeding rich in volatile salt to be distilled very many times; after every distillation re-conjoining all, that came over in a liquid with that, which remained in a saline form, till by very frequent cohobations all the parts of the urinous substance were brought to the union or coalition above-mentioned. What we may propose concerning the various consistence of the saline part of urine upon our own knowledge, we shall for certain reasons reserve for another place.

AND on this occasion we will annex a few particulars, which may tend not only to the making of the chymical hypothesis about the coagulation of bodies doubtful, but to the confirmation of much of the doctrine by us proposed. The first shall be an observation afforded us by the art of making sugar, wherein very great care is taken, that nothing acid (and especially juice of lemons) fall into the chaldrons or other vessels, wherein the juice of the sugar-cane is to coagulate into sugar: for though acidity be generally by the chymists ascribed to salt, yet here the saline bodies are so far from promoting the coagulation of the saccharine syrup, that they would quite hinder it. And because that through the want of sugar-canes in these parts, we are reduced to take this observation upon the credit of others, and because also in itself it seem somewhat strange, we will vouch for it two eminent authors, in whose writings we met with it. The one is the ingenious French publisher of the natural and moral history of those American islands, commonly called by the French *les Isles Antilles*, called by us the Caribbee islands; who describing particularly how his country-men make sugar in those parts, gives this caution towards the latter end, *Sur tout*, &c. that is, above all, great heed must be taken, to let no juice of citrons (or lemons) fall into the caldrons, for that would absolutely hinder the formation of the sugar. The other is the diligent *Gulielmus Piso*, who having given us a particular account, both by words and pictures, of the way of making sugar, tells us that, *si momentum succi limonis vel acidi quid injiciatur, sacchari consistentiam nunquam acquiret, sed in totum perditur*. To which I shall add, that having purposely inquired concerning this observation, it has been confirmed unto me by persons, that pretend more than ordinary knowledge of the art of ordering sugar: which likewise affords us another observation not impertinent to the theme we treat of; for the best authors, that write hereof, inform us, that the juice squeezed out of the sugar-canes is wont first to be boiled and depurated in vast vessels of copper or brass, whence it afterwards is conveyed to be further purified and coagulated into smaller ones; and that whilst it is in the former, they use to pour upon it some very strong lee to facilitate the separation of its feculencies, as in the smaller ones it is usual to pour a little oil or butter upon the boiling juice, to keep the syrup from boiling over. Now that, which they further observe to our purpose, is related almost after the same manner both by our French

Hist. Moral. Cap. 5.

Hist. Nat. & Med. Brasil. l. 4. c. 1.

French author and by *Piso*, and by the latter of them in these words: *observatu dignum* (says he) *si oleum majoribus inderetur abenis in quibus liquor primus, caldo dictus, putrificatur, saccharo conficiendo plane foret ineptus: vicissim si minoribus lixivium sicut majoribus infundatur, æque impossibile saccharum conficere.* So much the fluidity and firmness of bodies depend upon their texture, how much soever chymists would have them depend upon salt. Guliehm. Piso, & Author Gallic. ubi supra.

BUT to this borrowed observation, though borrowed of authors not to be distrusted, we will add two or three experiments of our own, which we hope may the more confirm the doctrine by us proposed touching stability in bodies, because it was our aim in them to bring light by them to the matters we treat of.

FIRST then, we prepared a liquor, elsewhere to be described, which is almost, if not altogether, as saline as *aqua fortis* itself, or any other acid spirit, that is commonly known: and yet when in this liquor we laid fragments of solid harts-horn of several sizes to steep, even in a cold place, the menstruum was so far from hardening them, that it would (without dissolving them as corrosive liquors do metals) gently pierce into them and soften them; so that in about two or three days it would reduce them to a kind of white slime, or mucilaginous substance, at the bottom of the liquor. We took also good salt of tartar, and on it poured good spirit of vinegar, as long as the affusion of it would produce any ebullition: then we distilled off the liquor, which came over almost insipid, the saline parts, that make spirit of vinegar so sharp, being retained by the salt of tartar. Upon the remaining dry mixture we poured fresh spirit of vinegar, as long as any hissing ensued thereupon, and afterwards abstracted the aqueous parts of this parcel of liquor also; and so we proceeded, till having sufficiently impregnated the fixed salt with the saline parts of the distilled vinegar, we obtained, according to our desire, a mixture, which (though it were all made up of salts, and such salts too as being made by the chymical analysis of the bodies whence they were drawn, may according to the chymical doctrine be looked upon as pure and elementary) was yet so near fluidity, that it required not the heat of the fire to turn it presently into a liquor, which shape it assumed with a gentler warmth, than one would expect from a saline body. Lastly, we took common oil of vitriol, and cast into it divers little pieces of camphire, which floating upon it were by degrees and after some hours wholly reduced into a reddish oil, that was to be seen altogether upon the top of the other liquor. Then having formerly tried that oil of vitriol would easily mix with common oil, we tried also, by shaking the saline and camphorate liquors together, to unite them, and easily confounded them into one high coloured liquor, which seemed very uniform, and continued so (at least as to sense) for many hours. Then we added to this mixture three or four times as much fair water, and (as we expected) the camphire immediately recovered a white consistent body, and by degrees settled at the top of the liquor: where we may observe, that the camphire is not made hard but fluid by its mixture with the saline corpuscles of oil of vitriol, and exchanges its fluidity for firmness upon the affusion of saltless water. And thus much it may suffice to have said touching the chymists deriving the stability of bodies from their abounding in salt.

AND as for the hardness and brittleness they ascribe to the same principle, how much they may be increased or diminished in a body, without the accession or decrement of the saline principle or ingredient, may appear by that experiment mentioned by us to several purposes, of tempering a slender piece of steel: for when it has been sufficiently heated, by plunging it red-hot into fair water, which is more likely to dissolve than increase its salt, you may make it not very hard alone, but very brittle;

whereas by only suffering it to cool leisurely in the air, it will be both much less hard and more tough. And if after having quenched it in cold water, you again heat it till it have attained a deep blue, it will become (comparatively) soft and very flexible, and that not from any wasting of the saline ingredient by the fire; for if this softened steel be again heated red hot, and suddenly refrigerated, whether in water or otherwise, as before, it will re-acquire both hardness and brittleness.

Now that by these operations a real change is made in the disposition of the small parts of the steel, we have elsewhere evinced even by a sensible proof. And that by procuring a closer order and more immediate contact of the parts of a body, a man may without increasing the salt increase the hardness of it, is, as we formerly also noted, obvious in snow, which whilst it lies in flakes as it falls upon the ground, composes but a soft and yielding body; but when the same snow is, by being strongly pressed every way betwixt the hands, formed into balls, the little weather icicles or frozen bubbles it consists of, are so approached to one another, and forced into an order, which allows so little waste of room, that the formerly-intercepted spaces being most of them filled up with little bodies, the icicles can no longer yield, as they did before, to the pressure of a man's fingers, but constitute a mass considerably hard, which yet may be made harder being melted into water, and afterwards frozen into ice: for this having been a fluid body (and in such, room is wont to be better husbanded than in others) the bubbles intercepted in it cannot keep it from being of so close a texture, as to be considerably hard.

I know, that not only profest chymists, but other persons, who are deservedly ranked amongst the modern philosophers, do with much confidence entirely ascribe the induration, and especially the lapidescence of bodies, to a certain secret internal principle, by some of them called a form, and by others a petrifying spirit, lurking for the most part in some liquid vehicle. And for my part, having had the opportunity to be in a place, where I could in a dry mould and a very elevated piece of ground cause to be digged out several crystalline bodies, whose smooth sides and angles were as exquisitely figured, as if they had been wrought by a skilful artist at cutting of precious stones, and having also had the opportunity to consider divers other exactly or regularly shaped stones and other minerals, some digged out of the earth by my friends, and some yet growing upon stones newly torn from the rock; I am very forward to grant, that (as I elsewhere intimate) it is a plastic principle implanted by the most wise creator in certain parcels of matter, that does produce in such concretions as well the hard consistence as the determinate figure. We deny not then, that these effects depend most commonly upon an internal principle; but the difficulty consists in conceiving, how that internal principle produces its effects, which these writers not pretending to explicate intelligibly, we thought it not amiss briefly to survey some of the principal ways, by which it seems, that nature makes bodies firm and stable, whereby we may be assisted to judge, whether it be as necessary to have recourse to a plastic principle or a Gorgonic spirit in all other quick and notable indurations of bodies in the cold, as in the hardening such bodies, whose curious and determinate either internal textures or outward shapes (common to several concretions of one kind) argue their having been framed by some one formative power, or by divers seminal principles convened together. But this we will do without affirming, either that she cannot by some other yet unobserved way make consistent bodies, or that of the ways by us discoursed of, she is wont so to confine herself to any one, that she does not frequently make use of two or more of them, to produce the same effect.

And

AND because hardness is a high degree of firmness, I suppose it will not be impertinent to shew by some examples, how small an external operation may, without any appearing adventitious salt, make a soft body hard, and even brittle, when there appears not any other change to be made, than that of the texture or disposition of its component particles.

IT is a tradition amongst naturalists, that coral grows soft at the bottom of the sea, but when it is brought up into the open air, though it retains its bulk and figure, it hardens into a stony concretion, according to that of *Ovid:*

Sic & coralium, quo primum contigit auras
Tempore durescit, mollis fuit herba sub undis.

Ovid. 15. Metamor.

WHETHER or no this tradition is strictly true, we had not opportunity, when we staid at *Marseilles* (whose neighbouring sea is the chiefest in *Europe* where coral is wont to be fished) to give ourselves an ocular satisfaction. But whatever some say to discredit the tradition, nay, how confidently soever *Beguinus* (who seems to have the most strongly argued against it) hath rejected it, it must not be denied to be, sometimes at least, true (and that is enough to serve our present turn). For the learned *Gassendus* in the life of *Piereskius*, relating how that incomparable gentleman had the curiosity to fish for coral near *Toulon* (a noble port not far from *Marseilles)* has among other things this passage: *(viz.)* The plants, which were plucked up and down out were neither red nor handsome till their bark was pulled off; in some parts they were soft; and would give way to the hand, as towards the tops, which being broken and squeezed, they sent forth milk, like that of figs. I remember likewise, that the learned Jesuit *Fournier*, who being also a French hydrographer, and one that writes of *Marseilles* and *Toulon* as places very well known unto him, may be safely credited on this occasion; after he has particularly described the way of fishing corals near *Toulon*, he adds, these plants are neither red nor polished, when they are drawn out of the water, till their rind have been taken off; nay, they are soft, and being broken or else squeezed betwixt the fingers, they throw out a kind of milk resembling that of figs; and when one leaves off pressing them, he may see the small holes or pores, that harboured the milk, that was squeezed out. Thus far he. The credibleness of a good part of these narratives has been confirmed to me by a practiser of physic in the *East-Indies*, who having made some stay at his return on the island of *Mebila* (near that of *Madagascar)* where store of white coral is reported to grow, I enquired of him, whether he had gathered any, and whether he found it soft whilst it was growing? and he answered me, that he had of late years divers times gathered coral upon the sands of that island; and found it, when he gathered it, exceeding white, and (to use his expression) as soft as an onion: adding, that though it would in a very short time grow hard in the air (which he ascribed, how justly I know not, to the external heat of the sun) yet it is very well known to the sea-men, that deal in that ware, that if it be not gathered at a seasonable time of the year, it will not keep long, but either crumble away or otherwise decay: which disagrees not with the experienced *Piso*, who in his natural history of *Brasil*, speaking of some places of the *Brasilian* coast, where divers stony plants, some like little trees, some otherwise framed, may be seen in clear weather growing in the bottom of the sea, tells us, that, *è fundo erutæ mox durissimæ, si insolentur in littore, siccæ niveique coloris fiunt.* As remarkable a change is that I meet with in *Scaliger*, who tells us, as upon his own knowledge, of some, who at the urinary passages voided a slimy matter, which in the air coagulated into a firm

Beguin. Tyrocin. Chym. Lib. 2. Cap. 10.

Lib. 4. Anno Domini 1624.

Hydrogra. de P. Fournier. Lib. 4. Cap. 27.

Lib. 4. Cap. 68.

firm substance. The story being memorable, take it in his own words thus: *Ex bovillis oppidanus nostris adjutus medicamentis eminxit vitrum sane ex illa nobili Paxagoræ pituita, dum mingeretur albuminis mollitie emissum vitri duritie ac splendore, senatoris filius ejecit, puttis modo multos, & maximos: qui aëris contactu postea in gypseam tum speciem tum firmitatem concrevere; hic quoque nunc rectè valet.* Having likewise had the acquaintance of an inquisitive merchant of *Dantzick*, and also of an ingenious chymist, that spent some time in that city and the neighbouring country, along whose coast our European amber is wont to be dragged out of the sea; I inquired of them, whether they had never observed in amber a property like that which is reported of coral: and one of them, as I remember the other also, hath assured me upon his own particular observation, that lumps of amber are sometimes taken soft out of the sea, and grow hard in the air; which is the more credible to me, because I have at a *Polonian* nobleman's seen (besides other intercepted things) a whole spider, and that none of the least, perfectly inclosed in a piece of hard and transparent yellow amber. And elsewhere I have seen ten or twelve (if I misremember not the number) pieces of such amber, which contained, one a fly, another a spider, a third a straw, and each of the rest some such other thing. And it seems not impossible, that the contact of the external air may put the parts of such small bodies into a new motion, whereby some voluble corpuscles, that hinder their reciprocal adhesion, may be excluded, and the particles themselves pressed or otherwise disposed into a closer order. And we find, that some oil-colours, after they are brought to their due temper, may be preserved very long in the same degree of softness, if they and the shells, that contain them, be kept all the while under water, whereas in the air they would quickly change their texture, and become dry and hard.

BUT though in this last mentioned example, and some others, the removal of the body out of the water into the air seem manifestly to contribute to its growing hard, yet it seems not to us so easy to determine, what share the air has in effecting such indurations: for *Gassendus* relates of *Piereskius*, that he being wont in the summer time to wash himself in one of the lesser streams of the river of *Rhone*, he there made the following observation. Once upon a time he felt the ground, which he had wont to find even and soft, to be grown hard with little round balls or bunches, like hard boiled eggs, when the shell is peeled off; at which wondering, he took some of them up, and carried them home, that he might shew them to his master, and demand of him the reason. But the miracle was increased, when a few days after returning to the river, he found those little balls or lumps turned into perfect pebble stones, which he observed likewise to befall those, which he had carried and laid up at home. But how far this story will prove, that such coagulations must be effected by a substantial form of a petrifying liquor, we define not; especially since, not to repeat what we delivered already touching calcined marble out of *Fournier*, we have elsewhere delivered upon our own observation, that two or three spoonfuls of such pap of burnt alabaster, as we have lately been speaking of (and instead of which artificers use another stone called by them plaister of *Paris*, burnt and tempered up with fair water) did in the bottom of a vessel-full of water, into which we poured it, in a short time coagulate into a hard lump, notwithstanding the water, that surrounded it; which, it seems, by the texture of the mass, was kept out of its pores, as it is out of those of the oils of cinnamon and cloves, which, though fluid bodies, and sinking in water, suffer not its particles to insinuate themselves into theirs: and artificers observe, that the coagulating property of burnt alabaster will be very much impaired, if not lost, if the powder be kept too long, especially in the open air, before it be made

Gassend. in vita Piereskii, Lib. 1.

made use of; and when it has been once tempered with water, and suffered to grow hard, they tell me they cannot by any burning or powdering of it again make it near so serviceable for their purpose as before; so much doth the coagulation of these powders mixed with water seemed to depend upon their texture and other mechanical qualities.

I REMEMBER also, that though the bones found in the hearts of deer, and so magnified by physicians, do, in the air, acquire a hard and bony consistence; yet having had the curiosity to consider one of them in the heart of a deer newly killed, I found it there of a cartilaginous softness and flexibility.

AND here I will adventure further to confess, that I have oftentimes doubted, whether or no, not only consistent bodies, but some of the most solid ones in the world, may not have been fluid in the form either of steams or liquors, before their coalition and their concretion either into stones or other mineral bodies. I know there are many, who think, that stones, marchasites, and other such solid and durable bodies, were made together at the creation or other beginning of the universe, and who will not admit, that such concretions can be now generated. But not here to debate that famous controversy, whether stones may be said to grow and to be nourished, in the strict sense of those expressions, I think it not difficult to shew, that such parcels of matter are now to be met with in the form of stones, as did not before appear under that form; but whilst it was divided into minute parts, either was itself some fluid body or other, or at least did as a material part concur to the constituting of one, that was so. Of this, besides what we elsewhere deliver concerning it, we shall anon have occasion to mention some proofs; and therefore we shall now only mention two or three instances. The first whereof shall be, that we saw, among the rarities of a person exceedingly curious of them, a stone flat on the outsides; on one of whose internal surfaces was most lively engraven the figure of a small fish, with all the fins, scales, *&c.* which was affirmed to have been inclosed in the body of that stone, and to have been accidentally discovered, when the stone chancing to receive a rude knock upon its edge, split asunder. I remember also, that a while since a house-keeper of mine in the country informed me, that whilst a little before he caused in my absence one of my walls to be repaired, the mason I was wont to employ casually breaking a stone to make use of it about the building, found in it (to his wonder) a piece of wood, that seemed part of the branch of some tree, and consequently afterwards inclosed with that solid case, wherein he found it. This cavity in the body of the stone, and, as I remember, the stick itself he took out of it, he forthwith brought my house-keeper, to whom I have given directions to send them me. For this example seems to me a more cogent proof of the increase of stones, than some others, that eminent naturalists much rely on, for reasons discoursed of in another place *: where we also make particular mention of that ghur or metalline juice, which though the Latin writers of chymical, and even of metalline, matters have not, that I remember, given us any account of, yet I find a German or two, that were very conversant in the mines themselves, to have in books written in their own language taken a special notice of it. Besides, I have at present something to deliver upon my own observation, which, unless we will suppose (what seems not probable) that there were from the beginning made together with and in the midst of great masses of one kind of mineral little parcels of another of a quite differing sort, seems manifestly enough to argue, that either whole quarries of stone, or heavy and shining minerals, or both,

* In some of the Author's papers about the origin of minerals.

may

may have been fluid bodies. The obſervation, whereon I ground this conjecture, is, not only, that we have met with in lead-ore, and alſo in *Minera Antimonii* particles of a white ſtone or ſpar environed with a metalline body, though I think I have yet by me ſuch lumps of ore; but chiefly, that I have, with my own hands, taken a hard and ponderous ſhining mineral, which I keep for a rarity, like a marchaſite, of the ſhape of a pear, and of about the bigneſs of a walnut, out of the very body of a ſtone, wherein I ſuſpected it to be incloſed, and which environed it on all ſides: and this I took not out of a ſmall and looſe ſtone, but a large ſtone digged out to make ſtatues of. And I remember, that one of thoſe, that wrought upon it, told me, that in faſhioning it into ſtatues they found ſome more minerals in the ſame parcel of ſtone, which were alſo preſented me. To which I ſhall add, that an ingenious ſtatuary having in another place taken much pains to ſaw aſunder a very large ſtone, when he came to the midſt of it, found he could ſaw on no further, and the ſtone being afterwards broken, he perceived, that that, which ſo obſtinately reſiſted his ſaws, was a round marchaſite, which he brought to me, as a lover of ſuch curioſities. But I made him for my further ſatisfaction bring me alſo that part of the ſtone, wherein the marchaſite ſtuck, and by comparing them together diſcerned, that as much of the ſtone, as was contiguous to the marchaſite, had a kind of ruſt about it, and fitted the marchaſite ſo cloſe, as if either the marchaſite had been formerly liquid, and had afterwards been as it were moulded in that receptacle, or the ſtone had been formerly of ſome ſoft or fluid matter, which did exactly accommodate itſelf to the ſhape of the other body; or elſe, as if both the matter of the ſtone and that of the marchaſite had been at once fluid bodies, but had each of them preſerved its own ſurface diſtinct (according to what we formerly noted of different fluids) till one of them (probably the marchaſite) firſt growing hard, the other, as being yet of a more yielding conſiſtence, accommodated itſelf to the harder's figure.

But the moſt eminent inſtances to declare, how much the fluidity and firmneſs of bodies depend upon the contrivance and texture of their parts, are afforded us by thoſe waters, which being permitted to reſt a while, do ſpontaneouſly ceaſe to be fluid, and coagulate into ſtone itſelf. There was lately an ingenious man, who going to viſit ſome leaden mines, wherein he had a ſhare, found in the mountain, in whoſe entrails they were hid, a cave, from whoſe arched roof there dropped down a petreſcent liquor, which oftentimes before it could fall to the ground congealed, and by appoſition of like matter increaſed ſo much, that they hung from the roof like icicles in a froſty-night from the ſides of a houſe; and of theſe he gathered and brought me divers, which are perfect ſtones hard and brittle, and of eight or ten inches long, and proportionably thick. Another ingenious friend of mine being lately in *France*, in the cave ſo famous for petrifying liquor to be there ſeen, obſerving ſome drops of water to congeal into ſtone, whilſt he ſtood by, took them away with him, and ſent them me in a letter. Nay, we ſhall ſcarce deny, that an external agent of almoſt inſenſible bulk may turn animal bodies into ſtony ones, by introducing a new texture into their parts, if we will, with ſome modern writers, believe *Aventinus*, who in his *Bavarian Hiſtory* has recorded, that at a time and place by him ſpecified, above forty countrymen, as alſo ſome milk-maids with their cows killed upon an earthquake, had their bodies by a terrene ſpirit turned into ſtatues, which, he ſays, were ſeen by the chancellor of *Auſtria* and himſelf. And ſome relations of this nature we meet with in other authors, which, if they be allowed of, ſeem much to confirm our doctrine; for in theſe ſtrange petrifications, the hardening of the bodies ſeems to be effected principally, if not only, as in the induration of the fluid ſubſtances of an egg into a chick,

a chick, by altering the disposition of their parts, since the petrifying wind or steam cannot be supposed to have any such considerable (perhaps not any sensible) proportion as to bulk to the body changed by it, as to be thought to effect this change principally as an ingredient.

ADD we to all these things, that *Pamphilio Piacentino* is by another author quoted for writing an unparalleled story, which because written in Italian, I shall English the substance of it, which is this: 'That a woman in *Venice*, after having eaten an 'apple, was taken with hideous tortures, and in the space of twenty four hours 'dying, was turned into exceeding hard stone; and this was judged to be the effect 'of the poisoned apple she had eaten.' Which narrative, if we may believe it as confidently as the famous alledger of it *Pamphilio* appears to do, would seem to argue, that even to the wonderful induration of bodies there is sometimes no other principle requisite, than what may result from the lucky mixture of the parts of several bodies. And lest we should seem to build altogether upon the observations of others, which cannot by us be now brought to strict examination, we will have recourse to a practicable experiment of our own trying, which, though we have elsewhere mentioned, we shall not scruple here to repeat, because we there omitted to speak of that circumstance of it, which is the most pertinent to our present design.

TAKE then two ounces of quicksilver, two ounces and a half of the best verdigrease, about half an ounce or an ounce of common salt, a pint or pound of white-wine vinegar, and as much fair water; mingle the verdigrease, quicksilver, and the salt very well, and put the mixture with a little of the vinegar and water into a new frying-pan (I tried it in a new earthen vessel, but without good success) in which fry it over the fire for divers hours, keeping it continually stirred, and putting in more vinegar and water from time to time, as that already put in consumes away: then take out the mixture, and in several clean waters wash it carefully from the adhering salts; then dry away all the aqueous moisture with a clean linen cloth, and you shall have a bright amalgama almost like quicksilver. Now that which is remarkable and to our present purpose in this experiment, is, that though this dried mixture be a good while after it is perfectly cold not only soft, but so near to fluid, that I have cast it into moulds, and made imbossed images of it (when it has been dexterously made, but scarce otherwise;) I have found, that by laying it a few hours in the air, which seemed less cold than itself, it has acquired such a hardness, that being thrown against the floor it would rebound, and was brittle like over-hardened steel. And yet in this example the induration of the amalgam appears not to proceed from an innate and inward principle, but from the new texture resulting from the coalition of the mingled ingredients, that make up the amalgam, whose parts being variously moved, partly by the fire (and, perhaps too, by the salts) and partly by the native propensity to motion of the mercurial corpuscles, were by little and little, or by degrees, so disposed, that whereas before touching one another but loosely, it was easy to thrust some of them towards the middle of the body, without stirring much of the mass (as to sense) by this change of texture, the particles are brought to touch one another more closely and in greater portions of their surfaces, and to be so complicated, intangled, or otherwise connected among themselves, that you cannot endeavour to thrust one of them out of its place, but that its motion shall be resisted by many others, to whom it is so fastened, that you cannot move one part of the mass without either moving the whole with it, or manifestly breaking it off from the whole, and thereby destroying the continuity and unity of the body.

Now

Now whereas, in setting down this experiment, we spoke as if several ingredients did concur to constitute the soft mass, which afterwards grew so hard, we might very safely do so, since the quicksilver was not so barely changed in texture, as that formerly said to have been coagulated by the meer fume of lead, but concealed in its self a great number of metalline corpuscles besides others, as we made appear by separating from the amalgam, meerly by the force of fire, a pretty quantity of true and perfect copper. That the salts also both were ingredients (though in small proportion) of the mass, and might have some operation upon the other particles, we may render probably by this, that having purposely exposed some of this mass for a pretty while to a moist air, we found, as we looked for, that the formerly invisible particles of salt, that had so insinuated themselves into the amalgam, that all the water, wherein it was washed, did not separate them from it, had so wrought upon the metalline particles, that were most outward, that they had in many parts of the surface of the mass turned themselves with it into a kind of verdigrease, which seemed almost to hide the surface of the concretion. And that in the more inward parts of a much harder body than our yielding amalgam, where cupreous particles abound, saline corpuscles may have a great operation, may appear by certain sorts of minerals to be found in some parts of *England*, and elsewhere, under the form of stones, of which they make vitriol: for these abounding with vitriolate, that is, both saline and metalline particles, will, after they are taken out of the ground, and laid in the open air, by the working of the inward salt, some sooner and some later, swell and burst asunder, which could hardly come to pass, without a great change made in the internal disposition of the parts of such stony concretions. And I remember, that having laid a mineral, of kin to these stones, a while in the air, though but in a chamber, I found its surface powdered with little grains of vitriol, as both their colour and their taste informed me.

Now whether or no we suppose, that the fire did put the parts of the amalgama into any lasting agitation, yet the mass being almost fluid after it was taken from the fire, its parts may, according to our notion of fluidity, be well supposed to have some kind of motion among themselves; and it will not be denied, that the fire might concur with other things, to make that motion convenient to cause the parts to fasten themselves to one another: for that the motion, wherein a soft and almost fluid body is once put, may possibly tend to harden it long after that motion seems to be extinct, may be made probable by what has been affirmed to me by eminent and experienced masons, namely, that the best sort of lime made into mortar will not have attained its utmost compactness till twenty five or thirty years (perhaps not till three or fourscore) after it has been employed in building; and this is given me as one of the reasons, why in the demolishing of antient fabrics, it is sometimes more easy to break the stone than the mortar.

AND lastly, that we also made mention of the texture resulting from the mingled ingredients of our amalgam, we might justify by saying, that having changed the proportion of the quicksilver to the verdigrease, we found, that the amalgam coagulated much more slowly, and, when it was coagulated, was much less hard, than when one used the quantities above specified.

HERE I should put a period for the present to this discourse, but that having in a late writer met with a notable observation of the natural induration of a soft body, I think it worthy to be here annexed; partly, because the French book is not common, no more than the observation; and partly, that by conferring together this natural induration with that artificial one freshly mentioned, it may the better appear, how

how nature and art have sometimes resembling operations in rendering bodies solid. My * author then (by name *Pierre Pelleprat*) being not long since sent with some other Jesuits upon the laudable errand of preaching the gospel to the Indians of the Southern *America*, has among other things this passage in the short relation he makes of the American continent: 'There is (says he) one thing worthy of observation 'near the mouth of this great river, (he speaks of that of the Amazons) which is, 'that men find there a kind of green clay, that is soft as long as it is in the water, 'so that one may print on it all kind of figures, and give it what shape one pleases; 'but when it is exposed to the air, it hardens to that degree, that diamonds are not 'much harder than the stones it affords. — I have (adds he) seen hatchets made of 'this clay, which the savages employed to cut wood with, when they had not the 'use of ours, *&c.*'

AND now at last, I see it is time to put a period to a discourse, that has been unawares lengthened far too much already: but yet I think you will easily pardon me, if I conclude it not abruptly; but with the recital of an experiment, which having had the honour to be seen, as to the main part of it, by an illustrious meeting of curious men, their having been pleased to speak very advantageously of it to others excited a curiosity among them, to know by what artifice effects, that were so uncommon, had been produced. The scope therefore, and the manner of making the experiment, were in short as follows.

BEING desirous to shew, how much fluidity and firmness may depend upon the texture and upon the motion or rest of the insensible parts of bodies, I first make with good spirit of vinegar, a solution of coral so strong, that when it is filtrated and cooled, it will commonly, after some time, begin to have a kind of sediment at the bottom; the clear liquor I gently pour off, when the experiment is to be made, and to this I put a convenient proportion of very well dephlegmed spirit of wine, which, if it be poured on very slowly and warily, may be made for a pretty while to swim upon it in the form of a distinct liquor: but if by a few shakes I mingle them together, they will presently unite into a concretion, of which, when the experiment succeeds very well (as it did when I shewed it to the above mentioned assembly) not one drop will fall to the ground, upon turning up the wide-mouth'd glass it should be made in, and holding it with the mouth directly downwards.

AND this so hastily-produced consistence may be durable enough, if it be carefully looked to: but to dispatch the whole experiment in a short time, I take a little strong spirit of nitre (which perhaps is not needful if good aqua fortis be at hand) and putting about an equal, or other convenient quantity of it to this mixture, I nimbly stir it and the spirit together: whereupon the whole is reduced in a very few minutes to a transparent liquor.

N. B. THOUGH I have divers times made and shewn this experiment, yet there are so many circumstances requisite to make the first part of it succeed very well, (for to make it succeed in some measure is not so difficult) that the event has sometimes deceived me, in spite of the several trials I have made. Wherefore it will not be amiss to intimate,

FIRST, That one of the first times, if not the first, I made such an odd concretion, was, with the solution not made with spirit of vinegar, but with spirit of verdigrease: (which I commonly distil without additament) though afterwards I was in-

* *Relation des Missions des P. P. J. J.* Part. II. Cap. 1.

vited to prefer ſtrong ſpirit of vinegar, which was the liquor, wherewith the recited experiment was exhibited.

SECONDLY, That it often happens, that if the ſolution of coral (which is not the only body wherewith I have made ſuch trials with indifferent good ſucceſs) be not ſufficiently ſtrong and impregnated with ſaline parts, or the ſpirit of wine be not ſufficiently rectified, the ſhaking of the two liquors will not change the conſiſtence of the whole mixture, but leaves ſome part of it fluid; or elſe the concretion will not begin preſently to be made, but require to be waited for a while.

THIRDLY, That I once at leaſt (if not oftener) obſerved, that when by mingling the two liquors and ſhaking them in a narrow-mouthed glaſs, whoſe orifice was ſtopped, they would not con-coagulate (as it was confidently expected they ſhould) yet by trying the experiment in a wide-mouthed glaſs, to which the air had free acceſs, it ſucceeded to my content.

FOURTHLY, That in the reduction of the concretion to a fluid body, it is not proper to employ in ſtirring it a knife or any other metalline body except it be of gold; but rather ſome ſtick of glaſs, or at leaſt ſome clean ſtick of wood, leſt the menſtruum ſhould corrode it, and thereby ſpoil, or at leaſt blemiſh the experiment.

FIFTHLY, that the proportion betwixt the coralline ſolution and the ſpirit of wine depends ſo much upon the ſtrength of the former liquor, and the dephlegmedneſs of the latter, that it is ſcarce poſſible to determine generally and exactly, what quantity of each ought to be taken; and therefore a trial or two made with a little of the particular ſolution you intend to employ (for ſome ſolutions require more, others leſs ſpirit of wine to concoagulate adequately with them) will better direct you, what proportion of ſpirit will ſuit that particular parcel of liquor, than any general rule I can propoſe.

I KNOW I might here, and perhaps it may be expected, that I ſhould take an occaſion to treat alſo of hardneſs, ſoftneſs, brittleneſs, toughneſs, ſtiffneſs, and thoſe other qualities, that are of kin to fluidity and firmneſs; but though I confeſs, I once had thoughts of writing a kind of hiſtory of more qualities than thoſe, yet remembering that wiſe counſel given us by one of the ancients, *Noſcenda eſt menſura ſui*, I am now very well content, after having already tired myſelf and, I fear, you, to recommend ſo uſeful but difficult a work to perſons more able and more at leiſure, than I find myſelf, to go through with ſo great an undertaking; contenting myſelf at preſent, to have attempted in what has been delivered concerning a couple of qualities of ſuch extent, that every ſenſibly big body in the univerſe ſeems indowed with one or other of them (I mean concerning fluidity and firmneſs) the explicating of qualities ſomewhat more intelligibly than is wont to be done in the Peripatetick ſchools, and to have opened a way (which I hope many will tread) of applying chymical obſervations and experiments to the deduction of thoſe effects of qualities from ſuch general and obvious affections of matter; as bigneſs, motion, and figure, which even the hermetical writers have hitherto contented themſelves to refer to ſalt, ſulphur, mercury, and the like: the chymical notion of which three principles, though of very good uſe in ſome other (eſpecially of the more practical) parts of phyſiology, ſeems not as yet to have brought any great light to ſuch matters as we have been treating of having been hitherto directed not ſo much to the indagation of cauſes, as to the production of effects.

OF ABSOLUTE REST IN BODIES.

An Advertisement.

SINCE it hath not been thought amiss, that something should be intimated to the reader about the occasion of the ensuing tract, I shall acquaint him, with it as briefly as I can, by telling him, that it was this. Some very ingenious gentlemen happening to meet as visitants at the author's lodgings, fell accidentally into a discourse about the absolute rest supposed to be in many bodies, that seemed to have its rise from a mistake of the true meaning of a passage or two in the *History of Fluidity and Firmness*, (that was then reprinting.) But the conference chancing to have a period put to it, whilst several things pertinent to the author's purpose remained yet unsaid; the curiousness of the subject invited him to draw up (hastily enough) the sum of what he had said, and might further have said, if opportunity had served, about the point in debate, for the further satisfaction of an inquisitive virtuoso, that was present at it. And this was the rise of the following discourse, which being written on an occasion administered by the *History of Fluidity and Firmness*, whereof a new edition was ready to come abroad; it was thought not improper, that this tract should attend it, as a kind of Appendix, without the first and last part of a letter, whereof the body only is necessary to the design of it.

An ESSAY of the inteſtine Motions of the Particles of Quieſcent Solids:

Where the abſolute Reſt of Bodies is called in queſtion.

SECTION I.

TO remove the doubt or ſcruple, that began to be diſcourſed of juſt before we laſt parted, I ſhall need to do little more than enlarge the particulars, which (you know) I had then time but briefly to make mention of. For the ſtate of the queſtion was (as you may remember) this, *Whether there be among bodies any abſolute reſt?* On which occaſion I anſwered, that reſt being a word, that to me ſeemed ſomewhat ambiguous, I thought it was requiſite to clear the ſenſe of the queſtion, before I offered at anſwering it.

FOR the word reſt, when we ſpeak of diſtinct maſſes of matter, looked upon as quieſcent, does in the vulgar acception of the term ſignify, ſuch a ſtate of a viſible and entire body, or (rather) of the corpuſcles it conſiſts of, that they are actually unmoved as to ſight; the eye (and perhaps the touch) being not able to diſcern any local motion in them.

CONSONANTLY to this firſt member of the diſtinction of the word *reſt*, I briefly intimated to the company, that in this ſenſe of a corpuſcle's being at reſt, I thought it manifeſt, that there is ſuch a thing *in rerum natura*; ſince without granting ſuch a reſt in the component particles of ſome kind of bodies, as diamonds, iron, porphyry, *&c.* it will be (I conceive) very hard to explain, how there can be ſuch ſolid maſſes (as thoſe minerals are) made up of ſmall and ſeparable particles. Which being ſaid, I added, that I ſaw no reaſon, why ſuch a kind of firmneſs, where the inward motion of the inſenſible particles is almoſt infinitely ſlow, may not ſuffice to give an account of as great a firmneſs as we uſe really to find among conſiſtent bodies.

BUT whereas I had intimated to the company, by the lately begun diſtinction, that beſides this popular ſenſe of the word reſt, there was a ſecond more rigid and philoſophical notion, or kind of reſt, which for diſtinction-ſake may be called abſolute or perfect reſt, which imports a continuance of a body in the ſame place *preciſely*, and includes an abſolute negation of all local motion, though never ſo ſlow or imperceptible; I told them, that in this rigid ſenſe of the word reſt, I durſt not affirm, that there are any bodies at reſt in the univerſe (at leaſt for any long time) but willingly allowed it to be made a problem, whether there be any or no; adding, that perhaps I inclined to the negative part of the queſtion.

HAVING thus hiſtorically ſummed up what paſſed betwixt us about the ſtate of the controverſy, I need not tell you, that the doubt I expreſſed was thought to reliſh too much of a paradox; and therefore ſince the company's quick ſeparation allowed me then no opportunity of enlarging, and ſince I promiſed no better arguments, than might

might be expected in a point, that I propoſed but as problematical; I ſhall now endeavour to ſhew you, that the ſide of the problem I was judged inclined to, is (at leaſt) not ſo improbable as ſome thought it.

To prove negatives directly, being wont (as you well know) to be no eaſy taſk, and eſpecially in ſuch caſes as this; you will not, I preſume, expect, that I ſhould attempt the proving of my conjecture otherwiſe than by ſhewing poſitively, that ſome of thoſe bodies, which we think to have their parts moſt at reſt, are not exempted from having inteſtine motions in them; ſince it will be conſequent to ſuch a proof, that it muſt be probable, that in other bodies, whoſe ſolidity is confeſſed to be inferior, the component particles are not in a ſtate of perfect reſt.

SECT. II.

If it were neceſſary and expedient, I ſhould begin my arguments by ſaying ſomething againſt abſolute reſt, in favour of the contrary opinion, by arguing *à priori*, as they ſpeak, from the conſtitution of the world, whether we conſider it according to the Epicurean or the Carteſian hypotheſis of the origin of things. For the Epicureans ſuppoſing this world to be produced by the caſual concourſe of atoms, and aſcribing to every particular atom an innate and unloſeable mobility, or, rather, an actual motion, or a reſtleſs endeavour after it; it is conſonant to think, that though in concretions they ſo entangle one another, that they cannot in a ſhort time, or a viſible manner clear themſelves from one another, yet they do inceſſantly ſtrive to diſentangle themſelves and get away: by which means there are always in the atoms even of ſolid bodies actual endeavours of each of the diſtinct atoms to extricate itſelf from the reſt (which endeavours uſually at laſt ſucceed, whence comes the decay and deſtruction of bodies) and in the mean while theſe perpetual and contrary endeavours produce inteſtine commotions in the internal parts of the body, wherein theſe atoms were impriſoned.

On the other ſide, according to the Carteſian doctrine, the *materia ſubtilis*, that conſtantly paſſes like a ſtream through the pores even of the ſolideſt bodies, may well be ſuppoſed in its paſſage to be continually ſhaking, or otherwiſe agitating the inſenſible particles, that make up the body, that ſeems to be at reſt, without diſcovering their motion to the eye: as when in ſummer-time (to explain myſelf by a compariſon) a gentle breath of wind paſſes through a grove removed a pretty way off from the ſpectator, though his eye diſcern no change in the grove he looks on, yet the wind, as it blows through the trees, will ſhake ſome of the branches as well as the flexible twigs; and not only blow the leaves into various poſtures, but blow ſome of them quite away.

I might eaſily enlarge on this ſubject, but having elſewhere done it on another occaſion, I think it may be now more proper to ſatisfy ſome of the company, who are yet entangled with the ſame prejudice with many other very learned men, who look upon it as a precarious and chimerical fancy of the Atomiſts, to imagine, that in ſolid, and, as to ſenſe, quieſcent bodies, there ſhould be any inteſtine motion of the component particles, neither the motions nor the corpuſcles themſelves being to be ſeen, and both of them being therefore as well incredible as inviſible.

A solemn debate of the whole queſtion about the minuteneſs of atoms belongs not to this place, where it may ſuffice to anſwer the objection.

SECT.

SECT. III.

And firſt, as I have elſewhere hinted, it may appear by divers of the phænomena above cited (in the *Hiſtory of Fluidity*) that when water and ſeveral other liquors ſeem to be continued maſſes of matter, and to be as much at reſt, as the very glaſſes, that contain them; their conſtituent corpuſcles are in an actual and various, though ſlow and unperceived, motion.

Next, that there may be likewiſe ſuch a motion in the minute parts of ſilver and iron themſelves, may be eaſily argued, by heating thoſe metals, till they come to be almoſt red hot: for then, though the eye can diſcern no motion of the corpuſcles thoſe metals conſiſt of, yet their being able to burn thoſe, that hold them in their naked hands, ſhews, that their briſk motions may be diſcovered by the help of the touch; and if you ſpit upon them, the liquor will boil, as if it were over the fire. And leſt it ſhould be objected, that ſo anomalous and violent an agent as the fire is neceſſary to theſe trials, I ſhall add, that, provided the minute parts be ſufficiently agitated, it matters not, whether the motion be produced by fire or no: for by the nimbly hammering of iron or ſilver, you may put the minute parts into ſuch a motion, as will make the metal very hot to the touch, and being communicated to ſpittle or water, will excite bubbles, and ſcatter the diſſipated parts of that liquor into the air, in the form of ſmoke or vapours: nay, I elſewhere ſhew, how I have eaſily excited a very ſenſible, though not a viſible agitation, and heat in the internal parts of a metal, barely by my naked hands, without any external inſtrument whatſoever.

And whereas it may be objected, that though the motion already generated is unſeen, yet we may diſcern a change of the component corpuſcles of a body, which are in the act of altering its texture, and introducing a new alteration or quality in the body to be wrought on, or deſtroying ſome pre-exiſtent quality: I briefly anſwer (for I would not here repeat what I have elſewhere ſaid of this point) by this clear experiment, that though your eye can diſcern no change in the outward and viſible, much leſs in the more latent and internal corpuſcles of iron; a vigorous loadſtone, by paſſing along its axis from one pole of the ſtone to the other, and back again, yet the texture of the iron is by that action of the loadſtone ſo changed, that it acquires, and then loſes thoſe admirable qualities we call the attractive and directive virtue or faculty peculiar to magnetical bodies.

And to ſhew you, that the inviſible motions even of metalline bodies may be quick and briſk enough, and may be ſenſible, though not viſible: we ſhall need to conſider but the ſtate of a good bell ſo long after the clapper has ſtruck it, that no ſhaking or other motion is to be ſeen in the body of the bell itſelf, and yet it cauſes in the air an odd kind of ringing, or, if I may ſo call it, undulating ſound or motion, which will ſometimes laſt a conſiderable while; and if the bell be fitted for ſharp notes, it will not be without a ſhrillneſs: for if ſounds proceed, as is elſewhere made probable, from the nimble percuſſions of the air put into a quick and waving motion by ſonorous bodies, this acuteneſs of ſound will ſhew, that whilſt to the eye the bell ſeems to be at reſt, yet the minute parts of it continue in a very briſk motion, without which they could not ſtrike the air ſtrongly, and faſt enough, to make it produce ſo ſhrill a noiſe in the ear.

But, I confeſs to you, that my thoughts preſent me a difficulty, which, though unmentioned at our meeting, may afford an objection, perhaps more difficult than any of (not to ſay all) the foregoing; namely, that it is ſcarce imaginable, how ſuch ſolid and

and hard bodies ſhould have their internal parts wrought upon by ſuch ſlight agents as the air, and perhaps ſome yet minuter matter, that is diſperſed in it; and how it is poſſible, that where there is an actual motion, it ſhould be ſo ſlow, that a corpuſcle of iron, for inſtance, ſeated in the internal part of a magnetic needle, ſhould ſpend ſo long time, as our conjecture requires, in travelling ſo little a ſpace as from thence to the next ſuperficies of the needle. But to this double objection, though ſome inſtances, which you will meet with in the following part of this paper, may be properly applied to ſolve it; yet not to make your curioſity wait, I will here ſpeak a word or two to each of the members of the objection.

SECT. IV.

And, to the firſt, I ſay, that theſe inteſtine motions of the corpuſcles of hard bodies need not be ſolely, nor perhaps principally aſcribed to thoſe obvious external agents, to which we are wont to refer them, ſince theſe may but excite or aſſiſt the more principal or internal cauſes of the motions we ſpeak of, as you may gather from what was but lately mentioned of the connate and unlooſeable mobility of the atoms, according to *Epicurus*, and the permeation of the moſt ſolid bodies by the Carteſian *materia ſubtilis*; and we may ſee by the ſudden effects of the loadſtone, in endowing ſteel with magnetic qualities, and depriving it of them again (both which ſuppoſe the intervention of a change of texture, and this a production of local motion in the metal) that very minute and inſenſible corpuſcles of matter are not uncapable of effecting durable changes in the ſolideſt bodies.

And as for the other member of the objection, I confeſs it is not eaſy for us, who are wont (perhaps too much) to follow our eyes for guides in judging of things corporeal, and to deny exiſtence to moſt things, whereto nature has denied a viſible bulk: it is not eaſy, I ſay, for us to imagine ſo great a ſlowneſs, as it is very poſſible for nature to make uſe of in her operations, though our not being able to diſcern the motion of a ſhadow of a dial-plate, or that of the index upon a clock or watch, ought to make us ſenſible of the incompetency of our eyes to diſcern ſome motions of natural bodies, which reaſon tells us ought to be incomparably ſlower than theſe. But not now to diſpute about the exiſtence and attributes of infinite ſlowneſs; or at leaſt a ſlowneſs in the next poſſible degree to infinite; I conſider, that it has not, that I know of, been demonſtrated, nor attempted to be ſo; that the motion of the corpuſcles, for example, of the needle above-mentioned, muſt be made in a direct line from the place, where it was firſt ſuppoſed to be, to the ſuperficies of the needle; for it ſeems more rational, and to agree better with the phænomena, to ſuppoſe, that the way of this corpuſcle in the body it would quit is extremely crooked and intricate (almoſt like that of a ſquib in the air, or on the ground) for it being on the one hand urged on by the cauſes, whatever they be, that make it ſtrive to fly away, and on the other hand hindered by the corpuſcles, whereto it is connected, and by the occurſions of other corpuſcles, whoſe motions may be oppoſite to, or diſagreeing with thoſe of our deſigned corpuſcle; it may probably, before it can extricate itſelf, be reduced to encounter and wreſtle, as it were, with many other corpuſcles, and be by them ſometimes thruſt or impelled to the right hand and to the left, and ſometimes alſo repelled inwards, even after it is come to the ſuperficial part of the needle, whence it may not preſently have the liberty to fly away, but may be drawn back by ſome other corpuſcle, wherewith it is yet connected, and which happening to be itſelf thruſt inward, may be drawn

drawn after it, and so entangle again our almost disbanded corpuscle: besides that the gravity of the component particles of a body is sometimes such, that it is easier for the agent, that puts them in motion, to continue them in that slow motion among themselves, than drive them up into so light a medium as the air; as experience shews in those bodies, that are called fixed, as gold and glass, though in actual fusion.

But I forget, that I promised you to decline speculations, and therefore I shall only name to you a couple of instances, which will serve to confirm both what I was lately saying, and what I am now in proving.

SECT. V.

The first of these I shall take from what is usually granted as matter of fact; namely, that if a spring, though made of so hard a body as steel, be forcibly bent, and kept but a moderate while in that posture, as soon as the force, that kept it bent, is removed, it will again return to its former figure; but if it be kept too long in that forced position, it will by degrees lose that, which they call the motion of restitution, and retain its new crooked figure, though the force, that bent it, be removed; which shews both the power of some of the more familiar agents in nature, and (which is that, the shewing whereof I here chiefly aim at) that where there is a continued endeavour of the parts of a body, to put themselves into another state, yet the motion, or rather the progress may be much more slow, than men seem as yet to have taken notice of, since it was a great while before the texture of the corpuscles of the steel were so altered, as to make them lose their former springiness.

But I will second this with a more illustrious experiment, which will at once confirm what I have just now said, and shew, that the air or the invisible corpuscles harboured in it may have no inconsiderable power to act upon, and effect changes in the solidest bodies.

To this purpose I shall only observe to you, that though if a bar of iron having one of its ends held perpendicularly, and at a fit distance, to the lily or north-point of the mariner's compass (I mean that, which points towards the north) it will, as I elsewhere mention, drive it away towards the east or west: and if this same lower end of the bar of iron be put into a contrary posture, it will presently lose its temporary magnetism, as I elsewhere declare. Yet, if this bar be very long kept upright in a window or other convenient place, then, as some late magnetical writers will tell you, it will have acquired a constant and durable magnetic power. Which is a phænomenon, that makes exceedingly for our present purpose, since it hence appears, both that the air, together with the magnetical effluvia of the earth, that it receives in its pores, is able without outward force to work durable changes in so solid a body as iron, and that the motions of the internal parts (for these are requisite to the change of the metal's texture) are performed with a wonderful slowness, since the bar must be very long exposed to the air, perhaps before it acquires any durable magnetism at all, but at least before it acquires so vigorous and fixed a magnetism, as by this means it may attain to.

But, because most of the instances to be proposed in the following part of this essay may serve for confirmations of what we have been discoursing, I shall proceed to them; yet not till I have advertised you, that I purposely decline to mention divers phænomena, that may even by learned men be thought fit examples on this occasion (such as the nutrition, growth, and wasting of animals and vegetables) because such

such bodies receiving constant supplies of corpuscles, of several, and often unknown, natures, there may be difficulties suggested about them, not easy to be cleared without longer discourses, than I can allow this essay.

SECT. VI.

The first instance then, that I shall mention about vegetable substances, shall be taken from *lignum vitæ*, or *guaiacum* (for many artificers give them the same name, and use them promiscuously for the same purposes) of which, though it seem to be the solidest wood we know (insomuch, that I as well as some others have ordinarily used it to pound solid bodies in) yet the skilfullest tradesmen I have met with, have upon my inquiry informed me, that if it be wrought, before it be well seasoned by length of time, it will shew itself very frangible; which an eminent turner told me he had often found to his loss: for having turned divers fine pieces of work of *lignum vitæ*, before it was duly seasoned, he found almost all of them by the heat of the sun (which the uses of many of them required they should sometimes be exposed to) crack, and cleave asunder, into I know not how many parts; whereby those fine pieces of workmanship were quite spoiled. And I remember, that having inquired of an old experienced tradesman, of whom I bought an excellent mortar of *lignum vitæ*, how long he had kept the wood in seasoning before I had the mortar, he answered me (if I much mis-remember not) twenty years, under which time it is not fully seasoned for some purposes; of which opinion of his having occasionally spoken to the lately-mentioned turner, this experienced workman much confirmed me in it; as he likewise did in an observation I not long since made about the slow and unperceived motion, that may be, not only in the more loose and fugitive aqueous parts of *lignum vitæ*, but in far more unlikely ones. For he told me, that he had often found, in turning that wood, cavities of several sizes in the very inward and solid part of the wood (which every way encompassed them) and in those cavities considerable quantities of a certain gum, much cried up by some for an anti-venereal medicine.

The use I would make of these examples is this, that since so solid a body as a trunk of *lignum vitæ* is, when the tree is newly felled, may require so long a time as twenty years, or upwards, to be seasoned, *i. e.* brought to its full compactness and toughness: and since the account, upon which time seasons wood, seems to be this, that by degrees the looser aqueous, and more fugitive parts exhale into the air, whereby the remaining solid ones are brought into a closer order, and have leisure to be so placed among themselves, as is most convenient to make their texture firm and durable; it will follow, that even in the internal parts of this solid wood there must be, not only in the looser and lighter corpuscles, that extricate themselves, and exale away, a true local motion, though much too slow to fall immediately under the discernment of our senses. And, if the lately mentioned gum be either totally, or so much as in part generated, as to sense, after the felling of the tree, as some analogous instances, that I have elsewhere taken notice of, make it probable; then the example will further be considerable to our present purpose, by shewing, that a substance so gross, and so little volatile in comparison of the aqueous parts, as is the brittle gum I speak of, may permeate, to a great thickness, a very solid and inanimate substance; which cannot be done without an intestine, though insensible motion among the parts of the wood, and probably a marvellously slow motion of those of the gum.

SECT. VII.

But it will poſſibly ſeem more ſtrange, that very thin pieces of wood, and thoſe ſawed off from a tree of a much looſer texture, ſhould be much longer in ſeaſoning, than that ſolid and ponderous wood we have been ſpeaking of. And indeed this diſcovery is not to be made, as in *lignum vitæ*, by the brittleneſs, or other obvious qualities in the wood, but by a ſubtiler way: and accordingly having purpoſely conſulted with the makers of muſical inſtruments, and with ſome antient muſicians, I was much confirmed by them in my opinion: and I remember, the laſt maker of viols, lutes, *&c.* of whom I inquired of what age he thought ſuch inſtruments, eſpecially lutes, ought to be, to attain their full and beſt ſeaſoning for ſonorouſneſs; he replied, that in ſome of them twenty years would be requiſite, in others forty, according to the nature and thickneſs of the wood, and other circumſtances. But an antient muſician, that was preſent at what was ſaid, informed me, that there were ſome famous lutes, one or two of which he named to me, that attained not their full ſeaſoning and beſt reſonance, till they were about fourſcore years old. And thus much for inanimate vegetable ſubſtances.

SECT. VIII.

As for the calcined ſtones made up into lime, and ſorts of raw ſtones, I have already obſerved from the credible relations of maſons and others, that the walls in ſome buildings attain not their hardneſs and ſolidity, till they are forty years old, or perhaps much antienter; and ſince in gradually proceeding to this degree of ſolidity, theſe walls reſemble the ſeaſoning of *lignum vitæ* formerly explicated, the motion of the internal parts may be argued from the change of texture as well in theſe as in that.

And, if I would rob other tracts (to which they more properly belong) I could here eaſily add ſome ſuch inſtances of the hardening and ſoftening of ſtones by time, as would much confirm what I have now been delivering; but I ſhall rather chuſe to confine myſelf here to the two examples following, not taken notice of in quarries or by maſons.

The firſt is, that there are marchaſites, conſiſting as well of a ſtony as of a metalline ſubſtance, which, though harder than many other ſorts of ſtones, and even than marble, have yet ſo great a motion in their internal parts, that if they be expoſed to the air, not only they will have a vitriolate efflorescence, if I may ſo ſpeak, on their ſuperficies (as I have obſerved in divers other marchaſites) but they will in tract of time burſt the ſtone in pieces; of which ſort I had ſome time ſince, and I hope I have yet a bulky marchaſite, that I procured from a virtuoſo, that lives juſt by a vitriol-work, whither theſe among other vitriol-ſtones are brought, and where this ſtone being choſen for its largeneſs, was taken up, and carefully kept by that ingenious perſon, till it burſt of itſelf, and till I ſent for it. And to ſatisfy myſelf a little further, that the internal parts of marchaſites may be as well diſpoſed to be vitriolated as the external; I remember I broke a hard marchaſite, that I had from another place, and laying it ſome ſhort time in a chamber-window, I found the new ſuperficies made by the fracture about the middle of the ſtone to have acquired an efflorescence of a vitriolate nature.

The

The other instance, which is very odd, and much talked of, is this: an ingenious gentleman of my acquaintance, casually meeting me one day, told me, that he had a turquois-stone, which, if he were not mistaken, had a wonderful property; for there being in it several spots of colours differing from the rest of the gem, these spots seemed, though very slowly, to move from one part of the stone to the other. And this he thought himself to have taken notice of for very many months (perhaps a couple of years.) This relation seemed so strange, that the relator was not at all surprized, when to ascertain myself of the truth of it, I desired to have the ring, this stone was set in, for a while in my own keeping; to which he readily assenting, besides that I took very heedful notice of the situation of the spots, I employed a very ingenious youth, that then lived with me, and was skilled in drawing, to make the picture of the stone with the spots as they were then placed, and afterwards to have a watchful eye upon it, and from time to time (as once perhaps in two or three weeks) to draw a new picture of them; by comparing of several of which pictures, it was unanimously concluded, that the spots did shift places in the turquois, as if the matter they consisted of made its way through the substance of the stone; as we lately noted, that the gum of *lignum vitæ* seemed to do through the substance of the wood. And as far as we observed, the motion of these spots was exceeding slow and irregular, though perhaps it might have been reduced to a somewhat less uncertainty, were it not, that by an unwelcome accident we were deprived of the opportunity of continuing our observations long enough. And this brings into my mind, that the turquois being a stone, of which I had met strange stories in good authors, I once asked several questions about it of a noted jeweller; and inquiring, among other particulars, whether he had not observed some changes, that seemed spontaneous in the substance or colour of the stone? he replied, that in some few turquoises he had observed two differing blues in differing parts of the same stone, and that one of these colours would by slow and unperceived degrees invade, and at length overspread that part of the stone, which the other colour possessed before. I shall here add, that the same gentleman, that lent me the spotted turquois, shewed me afterward an agate haft of a knife, where was a certain cloud, which he told me an ingenious person had for some years observed to remove to and fro in the stone, and had a while since, to convince the relator, lent him the agate, of whose phænomena he promised me an account, when he shall have had the stone in his custody for a competent time; till the expiration of which, it may suffice to have said of this agate what I have now related.

SECT. IX.

But because that diamonds and glass are generally looked upon, especially by chymists, as bodies of the closest and firmest texture, that nature and art afford, if we could shew an intestine motion even in the parts of these, fitter instances for our purpose could not in reason be desired; I shall venture to say something of each of them, though what I have to say about diamonds, is proposed rather to ground a suspicion of what may be, than a demonstration, that it must be.

In the first place then, to remove that prejudice, that may be entertained from the incomparable hardness of diamonds (which I confess experience has made me admire) as if bodies so hard and solid could not have their parts put into motion but by some extraordinary, not to say, prodigious force; I shall only repeat here, what I have elsewhere shewn, that diamonds are bodies, that easily enough become actually electrical,

trical, and that ſome diamonds (of which ſort I have a ſmall one by me) will by rubbing upon a cloth be brought to ſhine in the dark; the quiſt of both theſe tranſient qualities requiring a change of texture even in the internal parts; and the friction that produces that change, doing it immediately by putting the parts of the ſtone into local motion, it may be thence argued, that a very moderate force may ſuffice to beget an internal motion in the inward particles of diamonds themſelves.

And I am not ſure, but that more hidden agents may make impreſſions upon theſe hardeſt bodies. For in a ring, that I am wont to wear on my little finger, which has no diamond, ſave one more than that ſhining one I lately mentioned, I have, I know not how often, ſeemed to myſelf to obſerve a manifeſtly greater clearneſs and ſparklingneſs at ſome times than at others, though I could not refer it to the clearneſs or dulneſs of the weather, the moiſture or dryneſs of the air, the ſuperficial clearneſs or foulneſs of the ſtone, or any other manifeſt cauſe I could think of. And in this I was the more inclined to think I might not be miſtaken, becauſe, beſides that the notice I took of it, was frequent, I have by me a rough diamond juſt as it came from the rock, in whoſe electrical faculty I have taken notice of changes as to the degree of ſtrength, wherewith it attracted, and that within the compaſs of a very little time, though I could not find any cauſe, whereto I could refer ſo ſurpriſing a phænomenon. And I muſt not here omit, that chancing one day to ſhew the newly mentioned diamond ring to a very ingenious lady, that uſed to wear in rings and bracelets ſtore of thoſe gems, and telling her what changes I had taken notice of in the diamonds; ſhe, who had obſerved more about gems than any lady I had yet met with, appeared but little ſurprized at what I told her, and affirmed to me, that ſhe had divers times obſerved the like alterations in ſome diamonds of hers, which ſometimes would look more ſparklingly than they were wont, and ſometimes far more dull than ordinary. And when I objected, that poſſibly that dulneſs might be imputed to the weather, or ſome caſual foulneſs of the ſurface of the ſtone, ſhe replied, that ſhe had been aware of thoſe circumſtances, rubbing the ſtones clean, and otherwiſe taking care to ſecure an obſervation, which ſhe had made too often to have deceived herſelf in it. If I remember aright, I have elſewhere mentioned how I ſaw a conſiderable, but cloudy, *Hungarian* diamond, which the owner would have preſented me, made clearer by lying for ſome time in a cold liquor; wherein he affirmed, that upon his keeping it longer the ſtone would loſe more and more of its cloudineſs: and what I myſelf ſaw ſufficed me to argue, that changes may be produced even in the inward parts of ſuch diamonds by agents, that act without any appearing violence.

SECT. X.

And if it be true, that diamonds, as I elſewhere obſerve about many other ſtones, may be generated from time to time in the bowels of the earth, it may not perhaps be abſurd to imagine, that even true diamonds, that ſeem perfect, and are fit for rings, may long continue to have an inſenſible motion through the whole ſtone, whereby the corpuſcles it conſiſts of are diſpoſed into a more convenient texture for the conſtituting of an extremely hard body. For though it be taken for granted, that adamantine bodies, becauſe they are generally exceeding hard, are equally ſo, yet that ſuppoſition is not favoured by experience. And I remember, that to ſatisfy myſelf further about ſuch matters, I repaired to an antient artificer eminent in his trade, which was the cutting and ſetting of diamonds; and that having demanded of him, whether he found, that all diamonds were of equal hardneſs; he anſwered me, that having

having dealt in these gems near twenty years in *Amsterdam*, and divers years in *England*, he perceived, that there are of later years brought over worse and worse sorts of diamonds; and that he finds several of the recent diamonds so soft and brittle in comparison of those he was antiently wont to set (and which he with other jewellers called diamonds of the old rock) that he is often afraid, and unwilling to meddle with them, when they are brought to him, lest he should spoil them in the cutting, or polishing. But this I only repeat historically, till further observation shall discover, whether these are diamonds not yet fully ripe, and capable of growing harder by further maturation; or whether they be of a peculiar sort of diamonds, whose nature it is to be always softer than those of the old rock.

SECT. XI.

This brings into my mind a confirmation of the unequal hardness of diamonds, whatever be the cause of it, which I met with in a little book, lately published in his own language by a Frenchman, who giving his reader an account of the eastern diamond-mines from the relation, as he affirms, of a late eye-witness, speaks thus of the third and last mine called *Gazerpoli*: *They are very clear, and of a good water, but they cannot be ground by mutual attrition* (if I understand the term he uses) *but with stones of the same mine; for if one should employ for that purpose the stones of another mine, those of* Gazerpoli *would be broken in pieces. They do also easily break upon the wheel, and those, that are not versed in the knowledge of stones, may easily be deceived* (in them.) Of which our author adds the example of a Portuguese, who refusing 1200 crowns for one of them at *Legborn*, when he went to have it cut at *Venice*, it broke upon the wheel into fifteen or twenty pieces. *Egrenan*, pag. m. 17, 18.

Another example, that seems to make more for our present purpose, is afforded by the same author, speaking of the second mine, which breeds the greatest stones called gane or colonor; for he says, that *sur la plus part*, *i. e.* upon the most of these stones after they are cut, there appears always, as it were, a kind of greasiness or unctuosity, which invites you ever and anon to have recourse to your handkerchief to wipe it off, which one would guess to proceed from some insensible effluvium, that exhaling out of the stone, comes to be checked and condensed by the air on the superficies of it, as it happens to sweat on the skins of animals: the truth of which conjecture I would examine by very nice scales, if I could procure such diamonds.

SECT. XII.

And because rubies, though inferior in hardness to diamonds, are yet harder than most other gems, and much more than marble and the like coarser stones, I will not omit, on this occasion, what was more than once affirmed to me by an observing lady, whom, if she were not too nearly related to me, I could scarce mention without an elogy. For casually casting my eye upon a fair ruby she wore upon her finger, and desiring to consider it more attentively, she pulled off the rich ring it was set in, and reaching it me, told me it was worth my curiosity to consider it. For besides that it was so fine a stone, that it was thought worth being left her as a legacy by a great lady (her dear friend) that was famous, as I knew, for the variety of the rich jewels she was mistress of; this ruby would not unfrequently vary the degrees of its lustre, she knew not why. For sometimes it seemed to be ennobled by a more vivid fire than ordinary, and at other times it would be manifestly more dull and cloudy than

it is wont to be: and this not imputable, as ſhe expreſsly aſſured me upon repeated obſervations, to the cloudineſs of the weather, or any ſuperficial foulneſs of the ſtone. And, that I might be convinced as well as ſhe herſelf was, ſhe deſired me to rub it very clean, and then take notice of the preſent luſtre of it, of which ere long ſhe preſumed ſhe could ſhew me a manifeſt alteration (for I was then come to viſit her, and paſs ſome weeks with her in her houſe) but my occaſions calling me away within a few days after, I had not time to wait for the event of her promiſe.

How far what has been ſaid concerning diamonds may be allowed to be argumentative towards the ſcope of this diſcourſe, I ſhall willingly leave to the diſcovery of time, and further obſervation; the mention I have made of the foregoing particulars having been invited partly by the nobleneſs of the ſubject, which made me willing to add theſe relations to what I have elſewhere written about them; and partly becauſe thus much at leaſt ſeems probably deducible from what I noted about the exciting of diamonds by rubbing, both to attract, and to ſhine, that notwithſtanding their incomparable hardneſs, an inteſtine motion of their minute parts may be without any conſiderable violence quickly produced.

SECT. XIII.

And now it will be time to conſider the other body I promiſed to take notice of, namely, glaſs. For this being thought ſo compact and firm a body, that it is indeſtructable by art or nature, and being alſo of ſo cloſe a texture, that the ſubtleſt chymical ſpirits (that are yet known) cannot pervade it; and laſtly, having given ſuch proofs of the fixedneſs of its parts, as to have long indured the violence even of a glaſs-houſe-fire, we can ſcarce imagine a body more unlikely to have any motion amongſt its component particles: and yet, that they may not be always at perfect reſt among themſelves, I have been induced to think by the following, and the like obſervations.

First, having inquired of a famous and experienced maker of teleſcopes, as well as of thoſe, that uſe ſuch inſtruments, whether he did not obſerve, that the Venice-glaſſes he employed would ſometimes crack of themſelves, whilſt they were yet in plates, and ſometimes do the like after they were ground into convexes, and poliſhed up; he anſwered affirmatively. And though it ſeemed improbable, that glaſſes brought ſo far off as from *Venice*, and many of them kept a good while here in *England* before there be occaſion to grind them, and perhaps longer after their having been ground before they crack, ſhould after all this time retain an internal motion among their component particles; yet I have been induced to conjecture, that ſome ſaline corpuſcles, more numerous than the nature of the glaſs required, may by degrees, though ſlow and unperceived, ſo tend towards the ſuperficies of the glaſs, as either to get out of the pores of it, or crack, or burſt the glaſs in endeavouring to force their paſſage outward. For having purpoſely inquired of the abovenamed artificer, and ſome other obſerving men, that deal in optical glaſſes, whether it had not been taken notice of, that there would ſometimes be, eſpecially in winter and very moiſt weather, a kind of efflorescence of a ſaltiſh taſte manifeſtly diſcernable upon the ſurfaces of their glaſſes; I was anſwered in the affirmative, eſpecially by the above-mentioned artificer, who having more occaſion and opportunity to take notice of ſuch things, told me, that he had by taſting found theſe exſudations ſenſibly ſaltiſh.

SECT.

SECT. XIV.

And I was the more apt to entertain the lately propoſed conjecture, becauſe of a thick glaſs cup, that I have yet by me, in the making of which, to render it the more diaphanous, I ſuppoſed an over-great proportion of ſalt had been employed. For this cup, though for a while it continued clear and intire, yet before the enſuing winter was ended, though it did not ſo crack as to fall to pieces, but ſtill retains its former ſhape, yet it was flawed with ſuch a multitude of little cracks, that at a diſtance it looks like a white, not like a cryſtalline cup.

SECT. XV.

I remember alſo, that I have ſometimes, though not often, had veſſels and other bodies of glaſs of a conſiderable thickneſs, which have of their own accord broken ſuddenly aſunder, with noiſe enough to make me take notice of it. And that this did not always happen for want of the glaſs's being gradually or ſlowly cooled, or, in the workmen's language, nealed; I was perſuaded, not only by the ſpontaneous cracking, not without a loud noiſe of a thick and empty glaſs veſſel, that had for I know not how many months been kept locked up in my ſtudy; but by the like accidents, which I after found had happened unto others. For inquiring of ſome, that made great ſtore of glaſs veſſels, as well as of others that ſold them; I learned from both, that they had ſometimes by their loſſes been made to take notice, that glaſſes, that had been long made, and kept unemployed, would break of themſelves, when there was no viſible outward agent near enough to be ſuſpected of the having broken them. And ſince this very page began to be written, I had a fair cryſtal phial, not too well ſtopped, which cracked at the thick bottom, in a glaſs-cabinet (that was fixed to a wall) where I kept that with other choice phials under lock and key; no other of the included glaſſes (full nor empty) nor yet of the external glaſſes appearing any way cracked or injured. Nay, even great and ſtrong looking-glaſſes are not quite exempted from theſe accidents. For I remember, that having purpoſely inquired of an honeſt man, that furniſhed the greateſt part of *London* with large looking glaſſes, whether he did not ſometimes find them crack, and that with noiſe; he ſhewed me divers large plates of excellent glaſs, and aſſured me, that ſometimes after they had been a good while in his ſhop, ſome of them would of themſelves, not only crack with a loud noiſe, but now and then alſo (though rarely) fly aſunder with that violence, as to break ſome of the neighbouring plates, though thick and ſtrong.

SECT. XVI.

And having alſo a mind to inquire further, whether this diſpoſition to break, in ſome ſorts of glaſs, might not continue much longer than I had opportunity to obſerve, I addreſſed myſelf to an ingenious maſter of a glaſs-houſe, and demanded of him, how long he had taken notice of glaſs to continue ſound and whole, and yet afterwards to break of itſelf. He replied, that he had once a great parcel of glaſſes packed up, which not having the occaſion he expected to vend and make uſe of, lay by him for a great while; and yet when afterwards he had unpacked them, and ranged them, in a ſhort time a great many of them, amounting to about a fourth or third part of the whole number, cracked of themſelves; and when I aſked, how long the parcels

parcels had lain by, before they were opened, he replied, that it was, as he remembered, between four and five years.

SECT. XVII.

These inſtances (to which I could add divers others) I have therefore mentioned, becauſe either of the two hypotheſes, in congruity whereunto they ſeem likelieſt to be intelligibly explicable, will favour the doctrine hitherto patronized. For according to the atomical theory, it may be conceived, that there is a conſtant inteſtine motion of the ſmall parts of the glaſs upon the ſcore of their conſtituent atoms, which endeavour or tend to extricate themſelves and get away, which at laſt they do, by breaking the glaſs in ſome brittle, or other fit place; where (after a multitude of encounters and evolutions) a competent number of them may happen to be got together, and find their motion (outwards) withſtood: whence may enſue ſo unequal an agitation there of the formerly coherent parts of the glaſs, as to make the more agitated ones part from them, that are leſs ſo; and conſequently crack the glaſs. To which agrees what I have often obſerved in chymical and mechanical trials made with glaſs-veſſels, that if there be any grain of ſand or gravel, or any little lump of the alkalizate matter glaſs is made of, conſpicuouſly incloſed in the ſubſtance of the veſſel, it will both be much the more apt to break, and, if it do, will almoſt always begin to crack at that place (whence uſually as from a center ſeveral cracks go ſeveral ways;) the part of the glaſs, where the blemiſh is, being commonly of a differing texture from the reſt (as is often manifeſt to the very eye) and being by that incongruous texture diſpoſed to be put into a motion differing from, and perhaps very diſproportionate to that of the neighbouring parts.

SECT. XVIII.

I must not here ſtay to examine, whether or no this motion of the internal parts may not (in divers caſes) be made more efficacious by the penetration of ſome ſubtile and moiſt matter into the glaſs's pores (eſpecially the more ſuperficial ones of ſome glaſs of a looſer ſort) and ſo by degrees vitiate the texture of the body, and promoting the agitation and ſwelling of the ſaline corpuſcles, enable them to burſt the glaſs, after ſome ſuch manner, as the marchaſites I lately mentioned, came to have a vitriolate efflorescence, and even to be burſt by the operation of the air; this, I ſay, I muſt not now ſtay to examine, becauſe I would haſten to propoſe the ſecond hypotheſis, and tell you, that (elſe) we may, congruouſly to what we elſewhere diſcourſe, imagine, that in tract of time, there is produced in ſome parts of a glaſs a texture, that makes it reſiſt more than it did formerly the free paſſage of the æther, or ſome other ſubtile matter, that was accuſtomed (perhaps ſtream-like) to permeate it before; which tranſient matter now finding its paſſage obſtructed (and, perchance, almoſt quite hindered) by the vitiating of the pores of the glaſs, or ſome other (inconvenient) change of texture in it, and endeavouring to continue its wonted motion through it, does ſo ſtretch the pores, or otherwiſe offer violence to the texture of the body, that it cauſes a divulſion in the parts, which, according as it is more or leſs forcibly or ſuddenly made, does either barely crack the glaſs, or make it fly aſunder. To the precedent doctrine theſe two things agree not ill: the firſt, that glaſs is a body eaſily made electrical by rubbing, which makes it probable, that its particles may eaſily be put into motion. And the ſecond, that ſuch a divulſion may be made in glaſs by but an inequal

inequal motion between the neighbouring parts; as may appear by the chymical practice of cracking glasses, which they often think fit to do, only by applying a red-hot iron to the place, till it be sufficiently heated, and whilst it is very hot, moistening it with cold water (or even spittle) which by cooling the part, that it touched, and consequently checking the agitation of the corpuscles it meets there, whilst the contiguous parts retain their former vehement agitation, occasions a discontinuity or divulsion in the glass, some of whose parts are in so swift, and others are in so slow a motion.

SECT. XIX.

And on this occasion I shall add chiefly, because I would not pretermit so considerable a phænomenon, that even when glass seems to have lost the degree of heat, that one would think necessary to have its shape or bigness sensibly altered, there may remain yet so much agitation in the minute parts, as, when they are modified by the air, by the cold, or by some other invisible agent, to make them alter the bulk or shape of the whole vessel they compose; and that (which one would not expect) by enlarging the vessel, at least if we allow not in the case any change of figure: for it has often been observed in those glass-houses, where they work white glasses, as they call those, that are pure and clear, that when they have blown glasses into a mold, to give them more exactly the desired shape and size, these glasses, when they are cold, cannot well be put again into the same mold they were blown in, and require, that the cells of garde-phials, that are to receive them, be made a little larger: which observation an eminent artificer of my acquaintance, that gets considerably by fitting fine glasses to cases, has much confirmed to me by his complaints of the inconvenience, easy to be incurred, by the not knowing, or not remembering so unlikely an effect of the cooling of glass. But I must not prosecute the consideration of these, and the like phænomena, nor examine, which of the preceding explications is preferable, having mentioned them, as I was saying, chiefly to shew, that, which of them soever we pitch upon, it will argue an intestine motion in the corpuscles of the glass, which motion we shall think may be very slow, if we consider how long a time it is on some occasions, in producing its effects, before it brings them to be discernable to our senses.

SECT. XX.

Having thus made it probable, that among the parts of such solid bodies, as I have hitherto instanced in, there may not be such a perfect rest, as is generally believed; it will, I suppose, be expected, that I should now draw this consequence from what has been said, that there is no such thing as absolute rest in nature. But since at my first mentioning this paradox to you, I proposed it but as problematical, and since I consider, that we are not yet sure, but that though many of the parts of solid bodies may not be always moveless, yet some others of them may sometimes, for a while at least, be at perfect rest: I shall conclude, as I began, and without resolutely denying, that there can be any such thing *in rerum naturâ*, as absolute rest, I shall content myself to say, that it is not either absurd to doubt, whether there be or no; nor improbable to think, that there is not, since we have not found it in those very bodies, where, with the greatest likelihood, it might have been expected.

THE

SCEPTICAL CHYMIST:

OR

Chymico-Physical Doubts and Paradoxes,

TOUCHING THE

EXPERIMENTS,

Whereby vulgar Spagyrists are wont to endeavour to evince their Salt, Sulphur and Mercury, to be the true Principles of Things.

To which, in this Edition, are subjoined divers Experiments and Notes about the Producibleness of Chymical Principles.

A PREFACE INTRODUCTORY to the following Treatise.

TO give the reader an account, why the following treatise is suffered to pass abroad so maimed and imperfect, I must inform him, that it is now long since, that to gratify an ingenious gentleman, I set down some of the reasons, that kept me from fully acquiescing either in the peripatetical, or in the chymical doctrine of the material principles of mixed bodies. This discourse, some years after, falling into the hands of some learned men, had the good luck to be so favourably received, and advantageously spoken of by them, that having had more than ordinary invitations given me to make it public, I thought fit to review it, that I might retrench some things, that seemed not so fit to be shewn to every reader, and substitute some of those other things, that occurred to me of the trials and observations I had since made. What became of my papers, I elsewhere mention in a preface, where I complain of it;

it; but ſince I writ that, I found many ſheets, that belonged to the ſubjects I am now about to diſcourſe of. Wherefore, ſeeing that I had then in my hands as much of the firſt Dialogue, as was requiſite to ſtate the caſe, and ſerve for an Introduction as well to the Conference betwixt *Carneades* and *Eleutherius*, as to ſome other Dialogues, which for certain reaſons are not herewith publiſhed, I reſolved to ſupply, as well as I could, the contents of a paper belonging to the ſecond of the following diſcourſes, which I could not poſſibly retrieve, though it were the chief of them all. And having once more tried the opinion of friends, but not the ſame, about this imperfect work, I found it ſuch, that I was content, in compliance with their deſires, that not only it ſhould be publiſhed, but that it ſhould be publiſhed as ſoon as conveniently might be. I had, indeed, all along the Dialogues ſpoken of myſelf, as of a third perſon; for they containing diſcourſes, which were among the firſt treatiſes, that I ventured long ago to write of matters philoſophical, I had reaſon to deſire, with the painter, to *latere pone tabulam*, and hear what men would ſay of them, before I owned myſelf to be their author. But, beſides, that now I find, it is not unknown to many, who it is, that writ them, I am made to believe, that it is not inexpedient, they ſhould be known to come from a perſon altogether a ſtranger to chymical affairs. And I made the leſs ſcruple to let them come abroad uncompleated, partly, becauſe my affairs and pre-ingagements to publiſh divers other treatiſes, allowed me ſmall hopes of being able in a great while to compleat thoſe Dialogues. And partly becauſe I am not unapt to think, that they may come abroad ſeaſonably enough, though not for the author's reputation, yet for other purpoſes. For I obſerve, that of late chymiſtry begins, as indeed it deſerves, to be cultivated by learned men, who before deſpiſed it; and to be pretended to by many, who never cultivated it, that they may be thought not to be ignorant of it: whence it is come to paſs, that divers chymical notions about matters philoſophical are taken for granted and employed, and ſo adopted by very eminent writers both naturaliſts and phyſicians. Now this, I fear, may prove ſomewhat prejudicial to the advancement of ſolid philoſophy: for though I am a great lover of chymical experiments, and though I have no mean eſteem of divers chymical remedies, yet I diſtinguiſh theſe from their notions about the cauſes of things and their manner of generation. And, for aught I can hitherto diſcern, there are a thouſand phænomena in nature, beſides a multitude of accidents relating to the human body, which will ſcarcely be clearly and ſatisfactorily made out by them, that confine themſelves to deduce things from ſalt, ſulphur and mercury, and the other notions peculiar to the chymiſts, without taking much more notice than they are wont to do, of the motions and figures of the ſmall parts of matter, and the other more catholic and fruitful affections of bodies. Wherefore it will not, perhaps, be now unſeaſonable to let our *Carneades* warn men, not to ſubſcribe to the grand doctrine of the chymiſts, touching their three hypoſtatical principles, till they have a little examined it, and conſidered, how they can clear it from his objections, divers of which, it is like, they may never have thought on; ſince a chymiſt ſcarce would, and none but a chymiſt could propoſe them. I hope alſo, it will not be unacceptable to ſeveral ingenious perſons, who are unwilling to determine of any important controverſy, without a previous conſideration of what may be ſaid on both ſides, and yet have greater deſires to underſtand chymical matters, than opportunities of learning them, to find here together, beſides ſeveral experiments of my own, purpoſely made to illuſtrate the doctrine of the elements, divers others, ſcarce to be met with, otherwiſe than ſcattered among many chymical books; and to find theſe aſſociated experiments ſo delivered, as that an ordinary reader, if he be but acquainted with the uſual chymical terms, may eaſily enough underſtand them; and

even a wary one may safely rely on them. These things I add, because a person, any thing versed in the writings of chymists cannot but discern by their obscure, ambiguous, and almost aenigmatical way of expressing what they pretend to teach, that they have no mind to be understood at all, but by the sons of art (as they call them) nor to be understood even by these, without difficulty and hazardous trials. Insomuch that some of them scarce ever speak so candidly, as when they make use of that known chymical sentence; *ubi palam locuti sumus, ibi nihil diximus.* And, as the obscurity of what some writers deliver makes it very difficult to be understood; so the unfaithfulness of too many others makes it unfit to be relied on. For though unwillingly, yet I must for the truth's sake, and the reader's, warn him not to be forward to believe chymical experiments, when they are set down only by way of prescriptions, and not of relations; that is, unless he, that delivers them, mentions his doing it upon his own particular knowledge, or upon the relation of some credible person, avowing it upon his own experience. For I am troubled, I must complain, that even eminent writers, both physicians and philosophers, whom I can easily name, if it be required, have of late suffered themselves to be so far imposed upon, as to publish and build upon chymical experiments, which questionless they never tried; for if they had, they would, as well as I, have found them not to be true. And indeed, it were to be wished, that now, that those begin to quote chymical experiments, that are not themselves acquainted with chymical operations, men would leave off that indefinite way of vouching, the chymists say this, or the chymists affirm that; and would rather, for each experiment they allege, name the author or authors, upon whose credit they relate it: for by this means they would secure themselves from the suspicion of falshood (to which the other practice exposes them) and they would leave the reader to judge of what is fit for him to believe of what is delivered, whilst they employ not their own great names to countenance doubtful relations; and they will also do justice to the inventors or publishers of the true experiments, as well as upon the obtruders of false ones. Whereas, by that general way of quoting the chymists, the candid writer is defrauded of the particular praise, and the impostor escapes the personal disgrace, that is due to him.

The remaining part of this Preface must be imployed in saying something for *Carneades*, and something for myself.

And first, *Carneades* hopes, that he will be thought to have disputed civilly and modestly enough for one, that was to play the antagonist and the sceptic: and if he any where seems to slight his adversaries tenets and arguments, he is willing to have it looked upon as what he was induced to, not so much by his opinion of them, as the examples of *Themistius* and *Philoponus*, and the custom of such kind of disputes.

Next, in case that some of his arguments shall not be thought of the most cogent sort, that may be, he hopes it will be considered, that it ought not to be expected, that they should be so. For his part being chiefly but to propose doubts and scruples, he does enough, if he shews, that his adversaries arguments are not strongly concluding, though his own be not so neither. And if there should appear any disagreement betwixt the things he delivers in divers passages, he hopes it will be considered, that it is not necessary, that all the things a sceptic proposes should be consonant; since it being his work to suggest doubts against the opinion he questions, it is allowable for him to propose two or more several hypotheses about the same thing; and to say, that it may be accounted for this way, or that way, or the other way, though these ways be perhaps inconsistent among themselves. Because it is enough for him, if either of the proposed hypotheses be but as probable as that he calls.

calls in question. And if he propose many, that are each of them probable, he does the more ratify his doubts, by making it appear the more difficult to be sure, that that way, which they all differ from, is the true. And our *Carneades*, by holding the negative, has this advantage, that if among all the instances he brings to invalidate the vulgar doctrine of those he disputes with, any one be irrefragable, that alone is sufficient to overthrow a doctrine, which universally asserts what he opposes. For it cannot be true that all bodies whatsoever, that are reckoned among the perfectly mixt ones, are compounded of such a determinate number of such or such ingredients, in case any one such body can be produced, that is not so compounded; and he hopes too, that accuratenes will be the less expected from him, because his undertaking obliges him to maintain such opinions in chymistry, and that chiefly by chymical arguments, as are contrary to the very principles of the chymists, from whose writings it is not therefore like he should receive any intentional assistance, except from some passages of the bold and ingenious *Helmont*, with whom he yet disagrees in many things (which reduce him to explicate divers chymical phænomena, according to other notions;) and of whose ratiocinations, not only some seem very extravagant, but even the rest are not wont to be as considerable as his experiments. And though it be true indeed, that some Aristotelians have occasionally written against the chymical doctrine he oppugns, yet since they have done it according to their principles, and since our *Carneades* must as well oppose their hypothesis as that of the Spagyrist, he was fain to fight his adversaries with his own weapons, those of the Peripateric being improper, if not hurtful, for a person of his tenets; besides that those Aristotelians (at least those he met with) that have written against the chymists, seem to have had so little experimental knowledge in chymical matters, that by their frequent mistakes and unskilful way of oppugning, they have too often exposed themselves to the derision of their adversaries, for writing so confidently against what they appear so little to understand.

And lastly, *Carneades* hopes, he shall do the ingenious this piece of service, that by having thus drawn the chymists doctrine out of their dark and smoky laboratories, and both brought it into the open light, and shewn the weakness of their proofs, that have hitherto been wont to be brought for it, either judicious men shall henceforth be allowed calmly and after due information to disbelieve it, or those abler chymists, that are zealous for the reputation of it, will be obliged to speak plainer than hitherto has been done, and maintain it by better experiments and arguments than those *Carneades* hath examined: so that he hopes the curious will one way or other derive either satisfaction or instruction from his endeavours. And as he is ready to make good the profession he makes in the close of his discourse, of being ready to be better informed, so he expects either to be indeed informed, or to be let alone. For though, if any truly knowing chymists shall think fit in a civil and rational way to shew him any truth, touching the matter in dispute, that he yet discerns not, *Carneades* will not refuse either to admit, or to own a conviction; yet if any impertinent person shall, either to get himself a name, or for what other end soever, wilfully or carelessly mistake the state of the controversy, or the sense of his arguments, or shall rail instead of arguing, as hath been done of late, in print, by divers chymists *; or lastly, shall write against them in a canting way, I mean, shall express himself in ambiguous or obscure terms, or argue from experiments not intelligibly enough delivered, *Carneades* professes, that he values his time so much, as not to think the answering such trifles worth the loss of it.

* *G.* and *F.* and *H.* and others, in their books against one another.

And now having ſaid thus much for *Carneades*, I hope the reader will give me leave to ſay ſomething for myſelf.

And firſt, if ſome moroſe readers ſhall find fault with my having made the interlocutors upon occaſion compliment with one another, and that I have almoſt all along written theſe Dialogues, in a ſtyle more faſhionable than that of meer ſcholars is wont to be, I hope I ſhall be excuſed by them, that ſhall conſider, that to keep a due decorum in the diſcourſes, it was fit, that in a book written by a gentleman, and wherein only gentlemen are introduced as ſpeakers, the language ſhould be more ſmooth, and the expreſſions more civil, than is uſual in the more ſcholaſtic way of writing. And indeed, I am not ſorry to have this opportunity of giving an example, how to manage even diſputes with civility; whence, perhaps, ſome readers will be aſſiſted to diſcern a difference betwixt bluntneſs of ſpeech and ſtrength of reaſon, and find, that a man may be a champion for truth, without being an enemy to civility; and may confute an opinion without railing at them that hold it; to whom, he that deſires to convince and not to provoke them, muſt make ſome amends by his civility to their perſons for his ſeverity to their miſtakes; and muſt ſay as little elſe as he can, to diſpleaſe them, when he ſays, that they are in an error.

But, perhaps, other readers will be leſs apt to find fault with the civility of my diſputants, than the chymiſts will be, upon the reading of ſome paſſages of the following Dialogue, to accuſe *Carneades* of aſperity. But, if I have made my Sceptic ſometimes ſpeak ſlightingly of the opinions he oppoſes, I hope it will not be found, that I have done any more, than became the part he was to act, of an opponent: eſpecially, if what I have made him ſay, be compared with what the prince of the Roman orators himſelf makes both great perſons and friends ſay of one another's opinions, in his excellent Dialogues, *de naturâ Deorum:* and I ſhall ſcarce be ſuſpected of partiality in the caſe, by them, that take notice, that there is full as much (if not far more) liberty of ſlighting their adverſaries tenets, to be met with in the diſcourſes of thoſe, with whom *Carneades* diſputes. Nor need I make the interlocutors ſpeak otherwiſe than freely in a Dialogue, wherein it was ſufficiently intimated, that I meant not to declare my own opinion of the arguments propoſed, much leſs of the whole controverſy itſelf, otherwiſe than as it may by an attentive reader be gueſſed at by ſome paſſages of *Carneades*; (I ſay, ſome paſſages, becauſe I make not all, that he ſays, eſpecially in the heat of diſputation, mine) partly in this diſcourſe, and partly in ſome other * Dialogues betwixt the ſame ſpeakers (though they treat not immediately of the elements) which have long lain by me, and expect the entertainment, that theſe preſent diſcourſes will meet with. And indeed they will much miſtake me, that ſhall conclude from what I now publiſh, that I am at defiance with chymiſtry, or would make my readers ſo. I hope the *ſpecimina* I have lately publiſhed of an attempt to ſhew the uſefulneſs of chymical experiments to contemplative philoſophers, will give thoſe, that read them, other thoughts of me, and I had a deſign (but wanted opportunity) to publiſh with theſe papers an eſſay I have lying by me, the greater part of which is apologetical for one ſort of chymiſts. And at leaſt, as for thoſe, that know me, I hope the pains I have taken in the fire will both convince them, that I am far from being an enemy to the chymiſts art (though I am no friend to many, that diſgrace it by profeſſing it) and perſuade them to believe me, when I declare, that I diſtinguiſh betwixt thoſe

* The Dialogues here meant are thoſe about heat, fire, flame, &c. (ſeen by two Secretaries of the Royal Society) that the author ſomewhere complains to have been miſſing, with other things of his, preſently after the haſty removal of his goods by night in the great fire of *London*.

chymists, that are either cheats, or but laborants, and the true *adepti*; by whom, could I enjoy their conversation, I would both willingly and thankfully be instructed; especially concerning the nature and generation of metals; and possibly, those, that know how little I have remitted of my former addictedness to make chymical experiments, will easily believe, that one of the chief designs of this sceptical discourse was, not so much to discredit chymistry, as to give an occasion and a kind of necessity to the more knowing artists, to lay aside a little of their over-great reservedness, and either explicate or prove the chymical theory better than ordinary chymists have done, or, by enriching us with some of their nobler secrets, to evince, that their art is able to make amends even for the deficiencies of their theory. And thus much I shall make bold to add, that we shall much undervalue chymistry, if we imagine, that it cannot teach us things far more useful, not only to physic, but to philosophy, than those, that are hitherto known to vulgar chymists. And yet, as for inferior Spagyrists themselves, they have, by their labours, deserved so well of the common wealth of learning, that methinks it is pity they should ever miss the truth, which they have so industriously sought. And though I be no admirer of the theorical part of their art, yet my conjectures will much deceive me, if the practical part be not hereafter much more cultivated, than hitherto it has been, and do not both employ philosophy and philosophers, and hope to make men such. Nor would I, that have been diverted by other studies as well as affairs, be thought to pretend being a profound Spagyrist, by finding so many faults in the doctrine, wherein the generality of chymists scruple not to acquiesce: for besides that it is most commonly far easier to frame objections against any proposed hypothesis, than to propose an hypothesis not liable to objections (besides this, I say) it is no such great matter, if whereas beginners in chymistry are commonly at once imbued with the theory and operations of their professions, I, who had the good fortune to learn the operations from illiterate persons, upon whose credit I was not tempted to take up any opinion about them, should consider things with less prejudice, and consequently with other eyes than the generality of learners; and should be more disposed to accommodate the phænomena, that occurred to me, to other notions than to those of the Spagyrists. And having at first entertained a suspicion, that the vulgar principles were less general and comprehensive, or less considerately deduced from chymical operations, than was believed, it was not uneasy for me both to take notice of divers phænomena, over-looked by prepossest persons, that seemed not to suit so well with the hermetical doctrine; and to devise some experiments likely to furnish me with objections against it, not known to many, that having practised chymistry longer, perchance, than I have yet lived, may have far more experience, than I, of particular processes.

To conclude, whether the notions I have proposed, and the experiments I have communicated, be considerable, or not, I willingly leave others to judge; and this only I shall say for myself, that I have endeavoured to deliver matters of fact so faithfully, that I may as well assist the less skilful readers to examine the chymical hypothesis, as provoke the spagyrical philosophers to illustrate it: which if they do, and that either the chymical opinion, or the peripatetic, or any other theory of the elements differing from that I am most inclined to, shall be intelligibly explicated, and duly proved to me; what I have hitherto discoursed will not hinder it from making a proselyte of a person, that loves fluctuation of judgment little enough to be willing to be eased of it by any thing but error.

Advertiſement.

THE reader is deſired to take notice, that as the date of the licence witneſſes, this book ſhould have been printed long ago, and there has been a miſtake in the bottom of the title-page, where the year 1680, has been put inſtead of the year 1679, in which it was really printed off, though not publickly expoſed to ſale, till the beginning of this month of *January* 1678-79.

PHYSIOLOGICAL CONSIDERATIONS

TOUCHING

The Experiments wont to be employed to evince, either the four Peripatetick Elements, or the three chymical Principles of mixt Bodies.

Part of the firſt Dialogue.

I PERCEIVE, that divers of my friends have thought it very ſtrange to hear me ſpeak ſo irreſolvedly, as I have been wont to do, concerning thoſe things, which ſome take to be the elements, and others to be the principles of all mixed bodies. But I bluſh not to acknowledge, that I much leſs ſcruple to confeſs that I doubt, when I do ſo, than to profeſs, that I know what I do not: and I ſhould have much ſtronger expectations than I dare yet entertain, to ſee philoſophy ſolidly eſtabliſhed, if men would more carefully diſtinguiſh thoſe things, that they know, from thoſe, that they ignore or do but think, and then explicate clearly the things they conceive they underſtand, acknowledge ingenuouſly what it is they ignore, and profeſs ſo candidly their doubts, that the induſtry of intelligent perſons might be ſet on work to make further enquiries, and the eaſineſs of leſs diſcerning men might not be impoſed on. But becauſe a more particular account will probably be expected of my unſatisfiedneſs, not only with the peripatetick, but with the chymical doctrine of the primitive ingredients of bodies; it may poſſibly ſerve to ſatisfy others of the excuſeableneſs of my diſſatisfaction, to peruſe the enſuing relation of what paſſed a while ſince at a meeting of perſons of ſeveral opinions, in a place that need not here be named; where the ſubject, whereof we have been ſpeaking, was amply and variouſly diſcourſed of.

IT

IT was on one of the fairest days of this summer, that the inquisitive *Eleutherius* came to invite me to make a visit with him to his friend *Carneades*. I readily consented to this motion, telling him, that if he would but permit me to go first and make an excuse at a place not far off, where I had at that hour appointed to meet, but not about a business either of moment, or that could not well admit of a delay, I would presently wait on him, because of my knowing *Carneades* to be so conversant with nature and with furnaces, and so unconfined to vulgar opinions, that he would probably, by some ingenious paradox or other, give our minds at least a pleasing exercise, and perhaps enrich them with some solid instruction. *Eleutherius* then first going with me to the place where my apology was to be made, I accompanied him to the lodging of *Carneades*, where when we were come, we were told by the servants, that he was retired with a couple of friends (whose names they also told us) to one of the arbours in his garden, to enjoy under its cool shades a delightful protection from the yet troublesome heat of the sun.

ELEUTHERIUS being perfectly acquainted with that garden, immediately led me to the arbour, and relying on the intimate familiarity, that had been long cherished betwixt him and *Carneades*; in spight of my reluctancy to what might look like an intrusion upon his privacy, drawing me by the hand, he abruptly entered the arbour, where we found *Carneades*, *Philoponus*, and *Themistius*, sitting close about a little round table, on which, besides paper, pen, and ink, there lay two or three open books. *Carneades* appeared not at all troubled at this surprise, but rising from the table, received his friend with open looks and arms, and welcoming me also with his wonted freedom and civility, invited us to rest ourselves by him; which, as soon as we had exchanged with his two friends (who were ours also) the civilities accustomed on such occasions, we did. And he, presently after we had seated ourselves, shutting the books that lay open, and turning to us with a smiling countenance, seemed ready to begin some such unconcerning discourse, as is wont to pass, or rather waste the time in promiscuous companies.

BUT *Eleutherius* guessing at what he meant to do, prevented him by telling him, I perceive, *Carneades*, by the books, that you have been now shutting, and much more by the posture, wherein I found persons so qualified to discourse of serious matters, and so accustomed to do it, that you three were, before our coming, engaged in some philosophical conference; which I hope you will either prosecute, and allow us to be partakers of, in recompence of the freedom we have used in presuming to surprise you, or else give us leave to repair the injury we should otherwise do you, by leaving you to the freedom we have interrupted, and punishing ourselves for our boldness, by depriving ourselves of the happiness of your company. With these last words he and I rose up, as if we meant to be gone; but *Carneades* suddenly laying hold on his arm, and stopping him by it, smilingly told him, we are not so forward to lose good company as you seem to imagine; especially since you are pleased to be present at what we shall say, about such a subject as that you found us considering. For that being the number of the elements, principles, or material ingredients of bodies, is an enquiry, whose truth is of that importance, and of that difficulty, that it may as well deserve, as require, to be searched into by such skilful indagators of nature, as yourselves. And therefore we sent to invite the bold and acute *Leucippus* to lend us some light by his atomical paradox, upon which we expected such pregnant hints, that it was not without a great deal of trouble, that we had lately word brought us, that he was not to be found; and we had likewise begged the assistance of your presence and thoughts, had not the

messenger we employed to *Leucippus* informed us, that as he was going, he saw you both pass by towards another part of the town; and this frustrated expectation of *Leucippus* his company, who told me but last night, that he would be ready to give me a meeting where I pleased to-day, having very long suspended our conference about the freshly-mentioned subject, it was so newly begun when you came in, that we shall scarce need to repeat any thing, to acquaint you with what had passed betwixt us before your arrival; so that I cannot but look upon it as a fortunate accident, that you should come so seasonably, to be not hearers alone, but, we hope, interlocutors at our conference. For we shall not only allow of your presence at it, but desire your assistance, in it; which I add, both for other reasons, and because though these learned gentlemen (says he, turning to his two friends) need not fear to discourse before any auditory, provided it be intelligent enough to understand them, yet for my part (continues he with a new smile) I shall not dare to vent my unpremeditated thoughts before two such critics, unless, by promising to take your turns of speaking, you will allow me mine of quarrelling with what has been said. He and his friends added divers things to convince us, that they were both desirous, that we should hear them, and resolved against our doing so, unless we allowed them sometimes to hear us. *Eleutherius*, after having a while fruitlessly endeavoured to obtain leave to be silent, promised he would not be so always, provided, that he were permitted, according to the freedom of his genius and principles, to side with one of them in the managing of one argument, and, if he saw cause, with his antagonist, in the prosecution of another, without being confined to stick to any one party or opinion, which was after some debate accorded him. But I, conscious to my own disabilities, told them resolutely, that I was as much more willing, as more fit, to be a hearer than a speaker, among such knowing persons, and on so abstruse a subject. And that therefore I beseeched them, without necessitating me to proclaim my weaknesses, to allow me to lessen them, by being a silent auditor of their discourses: to suffer me to be at which, I could present them no motive, save that their instructions would make them in me a more intelligent admirer. I added, that I desired not to be idle, whilst they were employed, but would, if they pleased, by writing down in short hand what should be delivered, preserve discourses, that I knew would merit to be lasting. At first, *Carneades* and his two friends utterly rejected this motion; and all that my resoluteness to make use of my ears, not tongue, at their debates, could do, was to make them acquiesce in the proposition of *Eleutherius*, who thinking himself concerned, because he brought me thither, to afford me some faint assistance, was content, that I should register their arguments, that I might be the better able, after the conclusion of their conference, to give them my sense upon the subject of it (the number of elements or principles) which he promised I should do at the end of the present debates, if time would permit, or else at our next meeting. And this being by him undertaken in my name, though without my consent, the company would by no means receive my protestation against it, but casting, all at once, their eyes on *Carneades*, they did by that, and their unanimous silence, invite him to begin; which (after a short pause, during which he turned himself to *Eleutherius* and me) he did in this manner.

NOTWITHSTANDING the subtile reasonings I have met with in the books of the Peripateticks, and the pretty experiments, that have been shewed me in the laboratories of chymists, I am of so diffident, or dull a nature, as to think, that if neither of them can bring more cogent arguments to evince the truth of their assertion than are wont to be brought; a man may rationally enough retain some doubts concerning

concerning the very number of those material ingredients of mixt bodies, which some would have us call elements, and others principles. Indeed, when I considered, that the tenets concerning the elements are as considerable amongst the doctrines of natural philosophy, as the elements themselves are among the bodies of the universe, I expected to find those opinions solidly established, upon which so many others are superstructed. But when I took the pains impartially to examine the bodies themselves, that are said to result from the blended elements, and to torture them into a confession of their constituent principles, I was quickly induced to think, that the number of the elements has been contended about by philosophers with more earnestness than success. This unsatisfiedness of mine has been much wondered at by these two gentlemen (at which words he pointed at *Themistius* and *Philoponus*) who, though they differ almost as much betwixt themselves about the question we are to consider, as I do from either of them, yet they both agree very well in this, that there is a determinate number of such ingredients as I was just now speaking of, and that what that number is, I say not, may be (for what may not such as they persuade?) but is wont to be clearly enough demonstrated both by reason and experience. This has occasioned our present conference. For our discourse this afternoon having fallen from one subject to another, and at length settled on this, they proffered to demonstrate to me, each of them the truth of his opinion, out of both the topics, that I have freshly named. But on the former (that of reason strictly so taken) we declined insisting at the present, lest we should not have time enough before supper to go through the reasons and experiments too. The latter of which we unanimously thought the most requisite to be seriously examined. I must desire you then to take notice gentlemen (continued *Carneades*) that my present business doth not oblige me so to declare my own opinion on the subject in question, as to assert or deny the truth either of the peripatetic, or the chymical doctrine, concerning the number of the elements; but only to shew you, that neither of these doctrines hath been satisfactorily proved by the arguments commonly alledged on its behalf. So that if I really discern (as, perhaps, I think I do) that there may be a more rational account than ordinary given of one of these opinions, I am left free to declare myself of it, notwithstanding my present engagement; it being obvious to all your observation, that a solid truth may be generally maintained by no other, than incompetent arguments. And to this declaration I hope it will be needless to add, that my task obliges me not to answer the arguments, that may be drawn either for *Themistius* or *Philoponus*'s opinion from the topic of reason, as exposed to experiments; since it is these only, that I am to examine, and not all these neither, but such of them alone, as either of them shall think fit to insist on, and as have hitherto been wont to be brought either to prove, that it is the four peripatetick elements, or that it is the three chymical principles, that all compounded bodies consist of. These things (adds *Carneades*) I thought myself obliged to premise, partly lest you should do these gentlemen (pointing at *Themistius* and *Philoponus*, and smiling on them) the injury of measuring their parts by the arguments they are ready to propose, the laws of our conference confining them to make use of those, that the vulgar of philosophers (for even of them there is a vulgar) has drawn up to their hands; and partly, that you should not condemn me of presumption for disputing against persons over whom I can hope for no advantage, that I must not derive from the nature, or rules of our controversy, wherein I have but a negative to defend, and wherein too I am like on several occasions to have the assistance of one of my disagreeing adversaries against the other.

PHILOPONUS and *Themistius* soon returned this compliment with civilities of the like nature, in which *Eleutherius* perceiving them engaged, to prevent the further loss of that time, of which they were not like to have very much to spare, he minded them, that their present business was not to exchange compliments, but arguments: and then addressing his speech to *Carneades*, I esteemed it no small happiness (says he) that I am come here so luckily this evening. For I have been long disquieted with doubts concerning this very subject, which you are now ready to debate. And since a question of this importance is to be now discussed by persons, that maintain such variety of opinions concerning it, and are both so able to inquire after truth, and so ready to imbrace it by whomsoever and on what occasion soever it is presented them; I cannot but promise myself, that I shall, before we part, either lose my doubts or the hopes of ever finding them resolved. *Eleutherius* paused not here; but, to prevent their answer, added almost in the same breath. And I am not a little pleased to find, that you are resolved, on this occasion, to insist rather on experiments than syllogisms. For I, and no doubt you, have long observed, that those dialectical subtleties, that the schoolmen too often employ about physiological mysteries, are wont much more to declare the wit of him, that uses them, than increase the knowledge or remove the doubts of sober lovers of truth. And such captious subtleties do indeed often puzzle, and sometimes silence men, but rarely satisfy them; being like the tricks of jugglers, whereby men doubt not but that they are cheated, though oftentimes they cannot declare by what slights they are imposed on. And therefore I think you have done very wisely to make it your business to consider the phænomena relating to the present question, which have been afforded by experiments, especially since it might seem injurious to our senses, by whose mediation we acquire so much of the knowledge we have of things corporal, to have recourse to far-fetched and abstracted ratiocinations, to know what are the sensible ingredients of those sensible things, that we daily see and handle, and are supposed to have the liberty to untwist (if I may so speak) into the primitive bodies they consist of. He annexed, that he wished therefore they would no longer delay his expected satisfaction, if they had not, as he feared they had, forgotten something preparatory to their debate; and that was to lay down, what should be all along understood by the word principle or element. *Carneades* thanked him for his admonition, but told him, that they had not been unmindful of so requisite a thing. But that being gentlemen, and very far from the litigious humour of loving to wrangle about words, or terms, or notions as empty, they had, before his coming in, readily agreed promiscuously to use, when they pleased, elements and principles, as terms equivalent; and to understand both by the one and the other those primitive and simple bodies, of which the mixt ones are said to be composed, and into which they are ultimately resolved. And upon the same account (he added) we agreed to discourse of the opinions to be debated, as we have found them maintained by the generality of the assertors of the four elements of the one party, and of those, that receive the three principles on the other, without tying ourselves to inquire scrupulously, what notion either *Aristotle* or *Paracelsus*, or this or that interpreter, or follower of either of those great persons, framed of elements or principles; our design being to examine, not what these or those writers thought or taught, but what we find to be the obvious and most general opinion of those, who are willing to be accounted favourers of the peripatetic or chymical doctrine, concerning this subject.

I SEE not (says *Eleutherius*) why you might not immediately begin to argue, if you were but agreed which of your two friendly adversaries shall be first heard. And it being

being quickly resolved on, that *Themistius* should first propose the proofs for his opinion, because it was the ancienter, and the more general, he made not the company expect long before he thus addressed himself to *Eleutherius*, as to the person least interested in the dispute.

If you have taken sufficient notice of the late confession, which was made by *Carneades*, and which (though his civility dressed it up in complemental expressions) was exacted of him by his justice, I suppose you will be easily made sensible, that I engage in this controversy with great and peculiar disadvantages, besides those, which his parts and my personal disabilities would bring to any other cause, to be maintained by me against him. For he justly apprehending the force of truth, though speaking by no better a tongue than mine, has made it the chief condition of our duel, that I should lay aside the best weapons I have, and those I can best handle; whereas if I were allowed the freedom, in pleading for the four elements, to employ the arguments suggested to me by reason to demonstrate them, I should almost as little doubt of making you a proselyte to those unsevered teachers, Truth and *Aristotle*, as I do of your candour and your judgment. And I hope you will however consider, that that great favourite and interpreter of nature, *Aristotle*, who was (as his *Organum* witnesses) the greatest master of logic, that ever lived, disclaimed the course taken by other petty philosophers (antient and modern) who not attending the coherence and consequences of their opinions, are more sollicitous to make each particular opinion plausible independently upon the rest, than to frame them all so, as not only to be consistent together, but to support each other. For that great man, in his vast and comprehensive intellect, so framed each of his notions, that being curiously adapted into one system, they need not each of them any other defence than that, which their mutual coherence gives them; as it is in an arch, where each single stone, which, if severed from the rest, would be perhaps defenceless, is sufficiently secured by the solidity and entireness of the whole fabric, of which it is a part. How justly this may be applied to the present case, I could easily shew you, if I were permitted to declare to you, how harmonious *Aristotle*'s doctrine of the elements is with his other principles of philosophy; and how rationally he has deduced their number from that of the combinations of the four first qualities from the kinds of simple motion belonging to simple bodies, and from I know not how many other principles and phænomena of nature, which so conspire with his doctrine of the elements, that they mutually strengthen and support each other. But since it is forbidden me to insist on reflections of this kind, I must proceed to tell you, that though the assertors of the four elements, value reason so highly, and are furnished with arguments enough drawn from thence, to be satisfied, that there must be four elements, though no man had ever yet made any sensible trial to discover their number; yet they are not destitute of experience to satisfy others, that are wont to be more swayed by their senses, than their reason. And I shall proceed to consider the testimony of experience, when I shall have first advertised you, that if men were as perfectly rational, as it is to be wished they were, this sensible way of probation would be as needless, as it is wont to be imperfect. For it is much more high and philosophical to discover things *à priori*, than *à posteriori*. And therefore the Peripatetics have not been very sollicitous to gather experiments to prove their doctrines, contenting themselves with a few only, to satisfy those, that are not capable of a nobler conviction. And indeed they employ experiments rather to illustrate than to demonstrate their doctrines, as astronomers use spheres of pasteboard, to descend to the capacities of such, as must be taught by their senses, for want of being arrived to a clear apprehension of purely mathematical

notions and truths. I ſpeak thus *Eleutherius* (adds *Themiſtius*) only to do right to reaſon, and not out of diffidence of the experimental proof I am to alledge. For though I ſhall name but one, yet it is ſuch a one, as will make all others appear as needleſs, as itſelf will be found ſatisfactory. For if you but conſider a piece of green wood burning in a chimney, you will readily diſcern in the diſbanded parts of it the four elements, of which we teach it and other mixt bodies to be compoſed. The fire diſcovers itſelf in the flame by its own light; the ſmoke, by aſcending to the top of the chimney, and there readily vaniſhing into air, like a river loſing itſelf in the ſea, ſufficiently manifeſts to what element it belongs, and gladly returns. The water in its own form, boiling and hiſſing at the ends of the burning wood, betrays itſelf to more than one of our ſenſes; and the aſhes by their weight, their fierineſs, and their dryneſs, put it paſt doubt, that they belong to the elements of earth. If I ſpoke (continues *Themiſtius*) to leſs knowing perſons, I would perhaps make ſome excuſe for building upon ſuch an obvious and eaſy analyſis; but it would be, I fear, injurious, not to think ſuch an apology needleſs to you, who are too judicious either to think it neceſſary, that experiments to prove obvious truths ſhould be far-fetched, or to wonder, that among ſo many mixt bodies, that are compounded of the four elements, ſome of them ſhould, upon a ſlight analyſis, manifeſtly exhibit the ingredients they conſiſt of. Eſpecially ſince it is very agreeable to the goodneſs of nature, to diſcloſe, even in ſome of the moſt obvious experiments, that men make, a truth ſo important and ſo requiſite to be taken notice of by them. Beſides, that our analyſis, by how much the more obvious we make it, by ſo much the more ſuitable it will be to the nature of that doctrine, which it is alledged to prove; which being as clear and intelligible to the underſtanding as obvious to the ſenſe, it is no marvel the learned part of mankind ſhould ſo long and ſo generally imbrace it. For this doctrine is very different from the whimſies of chymiſts and other modern innovators; of whoſe hypotheſes we may obſerve, as naturaliſts do of leſs perfect animals, that as they are haſtily formed, ſo they are commonly ſhort lived. For ſo theſe, as they are often framed in one week, are, perhaps, thought fit to be laughed at the next; and being built, perchance, but upon two or three experiments, are deſtroyed by a third or fourth, whereas the doctrine of the four elements was framed by *Ariſtotle*, after he had leiſurely conſidered thoſe theories of former philoſophers, which are now with great applauſe revived, as diſcovered by theſe later ages; and had ſo judiciouſly detected and ſupplied the errors and defect of former hypotheſes concerning the elements, that his doctrine of them has been ever ſince deſervedly embraced by the lettered part of mankind: all the philoſophers, that preceded him, having, in their ſeveral ages, contributed to the completeneſs of this doctrine, as thoſe of ſucceeding times have acquieſced in it. Nor has an hypotheſis, ſo deliberately and maturely eſtabliſhed, been called in queſtion, till in the laſt century *Paracelſus* and ſome few other ſooty empirics, rather than (as they are fain to call themſelves) philoſophers, having their eyes darkened, and their brains troubled with the ſmoke of their own furnaces, began to rail at the Peripatetick doctrine, which they were too illiterate to underſtand, and to tell the credulous world, that they could ſee but three ingredients in mixed bodies; which, to gain themſelves the repute of inventors, they endeavoured to diſguiſe, by calling them, inſtead of earth, and fire, and vapour, ſalt, ſulphur, and mercury; to which they gave the canting title of hypoſtatical principles. But when they came to deſcribe them, they ſhewed how little they underſtood what they meant by them, by diſagreeing as much from one another, as from the truth they agreed in oppoſing: for they deliver their

hypotheſes

hypotheſes as darkly as their proceſſes; and it is almoſt as impoſſible for any ſober man to find their meaning, as it is for them to find their elixir. And, indeed, nothing has ſpread their philoſophy, but their great brags and undertakings; notwithſtanding all which (ſays *Themiſtius* ſmiling) I ſcarce know any thing they have performed worth wondering at, ſave that they have been able to draw *Philoponus* to their party, and to engage him to the defence of an unintelligible hypotheſis, who knows ſo well as he does, that principles ought to be like diamonds, as well very clear, as perfectly ſolid.

THEMISTIUS having after theſe laſt words declared by his ſilence, that he had finiſhed his diſcourſe, *Carneades* addreſſing himſelf, as his adverſary had done, to *Eleutherius*, returned this anſwer to it: I hoped for a demonſtration, but I perceive *Themiſtius* hopes to put me off with an harangue, wherein he cannot have given me a greater opinion of his parts, than he has given me diſtruſt for his hypotheſis, ſince for it even a man of ſuch learning can bring no better arguments. The rhetorical part of his diſcourſe, though it make not the leaſt part of it, I ſhall ſay nothing to, deſigning to examine only the argumentative part, and leaving it to *Philoponus* to anſwer thoſe paſſages, wherein either *Paracelſus* or chymiſts are concerned. I ſhall obſerve to you, that in what he has ſaid beſides, he makes it his buſineſs to do theſe two things. The one to propoſe and make out an experiment to demonſtrate the common opinion about the four elements; and the other, to inſinuate divers things, which he thinks may repair the weakneſs of his argument, from experience, and upon other accounts bring ſome credit to the otherwiſe defenceleſs doctrine he maintains.

To begin then with his experiment of the burning wood, it ſeems to me to be obnoxious to not a few conſiderable exceptions.

AND firſt, if I would now deal rigidly with my adverſary, I might here make a great queſtion of the very way of probation, which he and others employ, without the leaſt ſcruple, to evince, that the bodies commonly called mixt are made up of earth, air, water, and fire, which they are pleaſed alſo to call elements; namely, that upon the ſuppoſed analyſis made by the fire of the former ſort of concretes, there are wont to emerge bodies reſembling thoſe, which they take for the elements. For, not to anticipate here, what I foreſee I ſhall have occaſion to inſiſt on, when I come to diſcourſe with *Philoponus* concerning the right, that fire has to paſs for the proper and univerſal inſtrument of analyzing mixed bodies; not to anticipate that, I ſay, if I were diſpoſed to wrangle, I might alledge, that by *Themiſtius* his experiment it would appear rather, that thoſe he calls elements, are made of thoſe he calls mixed bodies, than mixed bodies of the elements. For, in *Themiſtius*'s analyzed wood, and in other bodies diſſipated and altered by the fire, it appears, and he confeſſes, that, which he takes for elementary fire and water, are made out of the concrete; but it appears not, that the concrete was made up of fire and water. Nor has either he, or any man, for aught I know, of his perſuaſion, yet proved, that nothing can be obtained from a body by the fire, that was not pre-exiſtent in it.

AT this unexpected objection, not only *Themiſtius*, but the reſt of the company appeared not a little ſurprized; but after a while, *Philoponus* conceiving his opinion, as well as that of *Ariſtotle*, concerned in that objection: you cannot ſure (ſays he, to *Carneades*) propoſe this difficulty, not to call it cavil, otherwiſe than as an exerciſe of wit, and not as laying any weight upon it. For how can that be ſeparated from a thing, that was not exiſtent in it? When, for inſtance, a refiner mingles gold and lead, and expoſing this mixture upon a cupel to the violence of the fire, thereby ſeparates it into pure and refulgent gold and lead (which driven off together with the

droſs

dross of the gold, is thence called *lithargyrum auri)* can any man doubt, that sees these two so differing substances separated from the mass, that they were existent in it, before it was committed to the fire?

I SHOULD (replies *Carneades)* allow your argument to prove something, if, as men see the refiners commonly take beforehand both lead and gold to make the mass you speak of, so we did see nature pull down a parcel of the element of fire, that is fancied to be placed I know not how many thousand leagues off, contiguous to the orb of the moon, and to blend it with a quantity of each of the three elements, to compose every mixed body, upon whose resolution the fire presents us with fire, and earth, and the rest. And let me add, *Philoponus*, that to make your reasoning cogent, it must be first proved, that the fire does only take the elementary ingredients asunder, without otherwise altering them. For else it is obvious, that bodies may afford substances, which were not pre-existent in them; as flesh too long kept produces maggots; and old cheese, mites; which I suppose you will not affirm to be ingredients of those bodies. Now that fire does not always barely separate the elementary parts, but, sometimes at least, alter also the ingredients of bodies, if I did not expect ere long a better occasion to prove it, I might make probable out of your very instance, wherein there is nothing elementary separated by the great violence of the refiner's fire: the gold and lead, which are the two ingredients separated upon the analysis being confessedly yet perfectly mixed bodies, and the litharge being lead indeed, but such lead, as is differing in consistence and other qualities from what it was before. To which I must add, that I have sometimes seen, and so questionless have you, much oftener, some parcels of glass adhering to the test or cupel, and this glass, though emergent, as well as the gold or litharge, upon your analysis, you will not, I hope, allow to have been a third ingredient of the mass, out of which the fire produced it.

BOTH *Philoponus* and *Themistius* were about to reply, when *Eleutherius* apprehending, that the prosecution of this dispute would take up time, which might be better employed, thought fit to prevent them, by saying to *Carneades*, You made at least half a promise, when you first proposed this objection, that you would not (now at least) insist on it; nor indeed does it seem to be of absolute necessity to your cause, that you should. For though you should grant, that there are elements, it would not follow, that there must be precisely four; and therefore I hope you will proceed to acquaint us with your other and more considerable objections against *Themistius*'s opinion, especially since there is so great a disproportion in bulk betwixt the earth, water and air, on the one part, and those little parcels of resembling substances, that the fire separates from concretes, on the other part, that I can scarce think, that you are serious, when to lose no advantage against your adversary, you seem to deny it to be rational, to conclude these great simple bodies to be the elements, and not the products of compounded ones.

WHAT you alledge (replies *Carneades)* of the vastness of the earth and water, has long since made me willing to allow them to be the greatest and chief masses of matter to be met with here below: but I think I could shew you, if you would give me leave, that this will prove only, that the elements, as you call them, are the chief bodies, that make up the neighbouring part of the world, but not that they are such ingredients as every mixt body must consist of. But since you challenge me of something of a promise, though it be not an entire one, yet I shall willingly perform it. And indeed I intended not, when I first mentioned this objection, to insist on it at present against *Themistius* (as I plainly intimated in my way of proposing it) being only desirous to let you see, that though I discerned my advantages, yet I was willing to

to forego some of them, rather than appear a rigid adversary of a cause so weak, that it may with safety be favourably dealt with. But I must here profess, and desire you to take notice of it, that though I pass on to another argument, it is not because I think this first invalid. For you will find, in the progress of our dispute, that I had some reason to question the very way of probation imployed both by Peripatetics and Chymists, to evince the being and number of the elements. For that there are such, and that they are wont to be separated by the analysis made by fire, is indeed taken for granted by both parties, but has not (for aught I know) been so much as plausibly attempted to be proved by either. Hoping then, that when we come to that part of our debate, wherein considerations relating to this matter are to be treated of, you will remember what I have now said, and that I do rather for a while suppose, than absolutely grant the truth of what I have questioned, I will proceed to another objection.

AND hereupon *Eleutherius* having promised him not to be unmindful, when time should serve, of what he had declared;

I CONSIDER then (says *Carneades*) in the next place, that there are divers bodies, out of which *Themistius* will not prove in haste, that there can be so many elements as four extracted by the fire. And I should, perchance, trouble him, if I should ask him, what Peripatetic can shew us (I say not, all the four elements, for that would be too rigid a question, but) any one of them extracted out of gold by any degree of fire whatsoever. Nor is gold the only body in nature, that would puzzle an Aristotelian (that is no more) to analyze by the fire into elementary bodies; since, for aught I have yet observed, both silver and calcined Venetian talc, and some other concretes, not necessary here to be named, are so fixt, that to reduce any of them into four heterogeneous substances has hitherto proved a task much too hard, not only for the disciples of *Aristotle*, but those of *Vulcan*, at least, whilst the latter have employed only fire to make the analysis.

THE next argument (continues *Carneades*) that I shall urge against *Themistius*'s opinion shall be this; that as there are divers bodies, whose analysis by fire cannot reduce them into so many heterogeneous substances or ingredients as four, so there are others, which may be reduced into more, as the blood (and divers other parts) of men and other animals; which yield when analyzed five distinct substances, phlegm, spirit, oil, salt and earth, as experience has shewn us in distilling man's blood, hartshorn, and divers other bodies, that, belonging to the animal-kingdom, abound with a not uneasily sequestrable salt.

THE

SCEPTICAL CHYMIST:

PART I.

I AM (ſays *Carneades*) ſo unwilling to deny *Eleutherius* any thing, that though, before the reſt of my company, I am reſolved to make good the part I have undertaken of a ſceptic; yet I ſhall readily, ſince you will have it ſo, lay aſide for a while the perſon of an adverſary to the Peripatetics and Chymiſts; and before I acquaint you with my objections againſt their opinions, acknowledge to you, what may be (whether truly or not) tolerably enough added, in favour of a certain number of principles of mixt bodies, to that grand and known argument from the analyſis of compound bodies, which I may poſſibly hereafter be able to confute.

AND that you may the more eaſily examine, and the better judge of what I have to ſay, I ſhall caſt it into a pretty number of diſtinct propoſitions, to which I ſhall not premiſe any thing; becauſe I take it for granted, that you need not be advertiſed, that much of what I am to deliver, whether for or againſt a determinate number of ingredients of mixt bodies, may be indifferently applied to the four Peripatetic elements, and the three chymical principles, though divers of my objections will more peculiarly belong to theſe laſt named; becauſe the chymical hypotheſis ſeeming to be much more countenanced by experience than the other, it will be expedient to inſiſt chiefly upon the diſproving of that; eſpecially ſince moſt of the arguments, that are employed againſt it, may, by a little variation, be made to conclude, at leaſt as ſtrongly againſt the leſs plauſible Ariſtotelian doctrine.

To proceed then to my propoſitions, I ſhall begin with this, that

Prop. 1. *It ſeems not abſurd to conceive, that at the firſt production of mixt bodies, the univerſal matter, whereof they among other parts of the univerſe conſiſted, was actually divided, into little particles, of ſeveral ſizes and ſhapes, variouſly moved.*

THIS (ſays *Carneades*) I ſuppoſe you will eaſily enough allow. For beſides that, which happens in the generation, corruption, nutrition, and waſting of bodies, that, which we diſcover partly by our microſcopes of the extream littleneſs of even the ſcarce ſenſible parts of concretes, and partly by the chymical reſolutions of mixt bodies, and by divers other operations of ſpagyrical fires upon them, ſeems ſufficiently to manifeſt their conſiſting of parts very minute, and of differing figures. And that there does alſo intervene a various local motion of ſuch ſmall bodies, will ſcarce be denied; whether we chuſe to grant the origin of concretions aſſigned by *Epicurus*, or that related by *Moſes*. For the firſt, as you well know, ſuppoſes not only all mixt bodies, but all others to be produced by the various and caſual occurſions of atoms, moving themſelves to and fro by an internal principle in the immenſe or rather infinite vacuum.

vacuum. And as for the inspired historian, he, informing us, that the great and wise author of things did not immediately create plants, beasts, birds, *&c.* but produced them out of those portions of the pre-existent, though created, matter, that he calls water and earth, allows us to conceive, that the constituent particles, whereof these new concretes were to consist, were variously moved, in order to their being connected into the bodies they were, by their various coalitions and textures, to compose.

BUT (continues *Carneades*) presuming, that the first proposition needs not be longer insisted on, I will pass on to the second, and tell you that

Neither is it impossible that of these minute particles divers of the smallest and neighbouring ones were here and there associated into minute masses or clusters, and did by their coalitions constitute great store of such little primary concretions or masses, as were not easily dissipable into such particles, as composed them. Prop. II.

To what may be deduced, in favour of this assertion from the nature of the thing itself, I will add something out of experience; which, though I have not known it used to such a purpose, seems to me more fairly to make out, that there may be elementary bodies, than the more questionable experiments of Peripatetics and Chymists prove, that there are such. I consider then, that gold will mix and be colliquated not only with silver, copper, tin and lead, but with antimony, *Regulus Martis*, and many other minerals, with which it will compose bodies very differing both from gold, and the other ingredients of the resulting concretes. And the same gold will also by common aqua regis, and (I speak it knowingly) by divers other menstruums be reduced into a seeming liquor, insomuch, that the corpuscles of gold will, with those of the menstruum, pass through cap-paper, and with them also coagulate into a crystalline salt. And I have further tried, that with a small quantity of a certain saline substance I prepared, I can easily enough sublime gold into the form of red crystals, of a considerable length; and many other ways may gold be disguised, and help to constitute bodies of very differing natures both from it and from one another, and nevertheless be afterward reduced to the self-same numerical, yellow, fixt, ponderous and malleable gold it was before its commixture. Nor is it only the fixedest of metals, but the most fugitive, that I may employ in favour of our proposition: for quicksilver will, with divers metals, compose an amalgam; with divers menstruums, it seems to be turned into a liquor; with aqua fortis, it will be brought into either a red or white powder, or precipitate; with oil of vitriol, into a pale yellow one; with sulphur, it will compose a blood-red and volatile cinabar; with some saline bodies, it will ascend in form of a salt, which will be dissoluble in water; with regulus of antimony and silver, I have seen it sublimed into a kind of crystals; with another mixture, I reduced it into a malleable body, into a hard and brittle substance by another: and some there are, who affirm, that by proper additaments they can reduce quicksilver into oil, nay into glass, to mention no more. And yet out of all these exotic compounds, we may recover the very same running mercury, that was the main ingredient of them, and was so disguised in them. Now the reason (proceeds *Carneades*) that I have represented these things concerning gold and quicksilver, is, that it may not appear absurd to conceive, that such little primary masses or clusters, as our proposition mentions, may remain undissipated, notwithstanding their entering into the composition of various concretions, since the corpuscles of gold and mer-

cury, though they be not primary concretions of the moſt minute particles of matter, but confeſſedly mixed bodies, are able to concur plentifully to the compoſition of ſeveral very differing bodies, without loſing their own nature or texture, or having their coheſion violated by the divorce of their aſſociated parts or ingredients.

GIVE me leave to add (ſays *Eleutherius*) on this occaſion, to what you now obſerved, that as confidently as ſome chymiſts, and other modern innovators in philoſophy are wont to object againſt the Peripatetics, that from the mixture of their four elements there could ariſe but an inconſiderable variety of compound bodies; yet, if the Ariſtotelians were but half as well verſed in the works of nature, as they are in the writings of their maſter, the propoſed objection would not ſo calmly triumph, as for want of experiments they are fain to ſuffer it to do. For if we aſſign to the corpuſcles, whereof each element conſiſts, a peculiar ſize and ſhape, it may eaſily enough be manifeſted, that ſuch differingly-figured corpuſcles may be mingled in ſuch various proportions, and may be connected ſo many ſeveral ways, that an almoſt incredible number of variouſly qualified concretes may be compoſed of them. Eſpecially ſince the corpuſcles of one element may barely, by being aſſociated among themſelves, make up little maſſes of differing ſize and figure from their conſtituent parts: and ſince alſo, to the ſtrict union of ſuch minute bodies there ſeems oftentimes nothing requiſite, beſides the bare contact of a great part of their ſurfaces. And how great a variety of phænomena the ſame matter, without the addition of any other, and only ſeveral ways diſpoſed or contexed, is able to exhibit, may partly appear by the multitude of differing engines, which by the contrivances of ſkilful mechanicians, and the dexterity of expert workmen, may be made of iron alone. But, in our preſent caſe, being allowed to deduce compound bodies from four very differently qualified ſorts of matter, he, who ſhall but conſider, what you freſhly took notice of, concerning the new concretes reſulting from the mixture of incorporated minerals, will ſcarce doubt, but that the four elements, managed by nature's ſkill, may afford a multitude of differing compounds.

I AM thus far of your mind (ſays *Carneades*) that the Ariſtotelians might with probability deduce a much greater number of compound bodies from the mixture of their four elements, than according to their preſent hypotheſis they can, if inſtead of vainly attempting to deduce the variety and proprieties of all mixed bodies from the combinations and temperaments of the four elements, as they are (among them) endowed with the four firſt qualities, they had endeavoured to do it by the bulk and figure of the ſmalleſt parts of thoſe ſuppoſed elements. For from theſe more catholic and fruitful accidents of the elementary matter may ſpring a great variety of textures, upon whoſe account a multitude of compound bodies may very much differ from one another. And what I now obſerve touching the four Peripatetic elements, may be alſo applied, *mutatis mutandis* (as they ſpeak) to the chymical principles. But (to take notice of that by the by) both the one and the other muſt, I fear, call into their aſſiſtance ſomething, that is not elementary, to excite or regulate the motion of the parts of the matter, and diſpoſe them after the manner requiſite to the conſtitution of particular concretes. For that otherwiſe they are like to give us but a very imperfect account of the origin of very many mixed bodies, it would, I think, be no hard matter to perſuade you, if it would not ſpend time, and were no digreſſion, to examine, what they are wont to alledge of the origin of the textures and qualities of mixed bodies, from a certain ſubſtantial form, whoſe origination they leave more obſcure than what it is aſſumed to explicate.

BUT to proceed to a new propoſition.

I ſhall not peremptorily deny, that from moſt of ſuch mixed bodies, as partake either of animal or vegetable nature, there may, by the help of the fire, be actually obtained a determinate number (whether three, four, or five, or fewer or more) of ſubſtances, worthy of differing denominations. Prop. III.

OF the experiments, that induce me to make this conceſſion, I am like to have occaſion enough to mention ſeveral in the proſecution of my diſcourſe. And therefore, that I may not hereafter be obliged to trouble you and myſelf with needleſs repetitions, I ſhall now only deſire you to take notice of ſuch experiments, when they ſhall be mentioned, and in your thoughts refer them hither.

To theſe three conceſſions, I have but this fourth to add, that

It may likewiſe be granted, that thoſe diſtinct ſubſtances, which concretes generally either afford, or are made up of, may, without very much inconvenience, be called the elements or principles of them. Prop. IV.

WHEN I ſaid, without very much inconvenience, I had in my thoughts that ſober admonition of *Galen*, *cum de re conſtat, de verbis non eſt litigandum*. And therefore alſo I ſcruple not to ſay, elements or principles, partly becauſe the chymiſts are wont to call the ingredients of mixed bodies, principles, as the Ariſtotelians name them, elements; I would here exclude neither: and, partly becauſe it ſeems doubtful, whether the ſame ingredients may not be called principles; as not being compounded of any more primary bodies; and elements, in regard that all mixed bodies are compounded of them. But I thought it requiſite to limit my conceſſion, by premiſing the words, *very much*, to the word *inconvenience*; becauſe, that though the inconvenience of calling the diſtinct ſubſtances, mentioned in the propoſition, elements or principles, be not very great, yet that it is impropriety of ſpeech, and conſequently in a matter of this moment not to be altogether overlooked, you will perhaps think, as well as I, by that time you ſhall have heard the following part of my diſcourſe, by which you will beſt diſcern what conſtruction to put upon the former propoſitions, and how far they may be looked upon, as things that I concede as true, and how far as things I only repreſent as ſpecious enough to be fit to be conſidered.

AND now *Eleutherius* (continues *Carneades*) I muſt reſume the perſon of a ſceptic, and, as ſuch, propoſe ſome part of what may be either diſliked, or at leaſt doubted of, in the common hypotheſis of the chymiſts; which if I examine with a little the more freedom, I hope I need not deſire you (a perſon to whom I have the happineſs of being ſo well known) to look upon it as ſomething more ſuitable to the employment, whereto the company has, for this meeting, doomed me, than either to my humour or my cuſtom.

Now though I might preſent you many things againſt the vulgar chymical opinion of the three principles, and the experiments wont to be alledged as demonſtrations of it; yet thoſe I ſhall at preſent offer you, may be conveniently enough comprehended in four capital conſiderations: touching all which I ſhall only premiſe this in general, that ſince it is not my preſent taſk ſo much to aſſert an hypotheſis of my own, as to give an account wherefore I ſuſpect the truth of that of the chymiſts, it ought not to be expected, that all my objections ſhould be of the moſt cogent ſort, ſince it is reaſon

reason enough to doubt of a proposed opinion, that there appears no cogent reason for it.

To come then to the objections themselves; I consider in the first place, that notwithstanding what common chymists have proved or taught, it may reasonably enough be doubted, how far, and in what sense, fire ought to be esteemed the genuine and universal instrument of analyzing mixed bodies.

THIS doubt, you may remember, was formerly mentioned, but so transiently discoursed of, that it will now be fit to insist upon it; and manifest, that it was not so inconsiderately proposed as our adversaries then imagined.

BUT, before I enter any further into this disquisition, I cannot but here take notice, that it were to be wished, our chymists had clearly informed us, what kind of division of bodies by fire must determine the number of the elements: for it is nothing near so easy, as many seem to think, to determine distinctly the effects of heat, as I could easily manifest, if I had leisure to shew you, how much the operations of fire may be diversified by circumstances. But not wholly to pass by a matter of this importance, I will first take notice to you, that guaiacum (for instance) burnt with an open fire in a chimney, is sequestered into ashes and soot, whereas the same wood, distilled in a retort does yield far other heterogeneities (to use the Helmontian expression) and is resolved into oil, spirit, vinegar, water and charcoal; the last of which, to be reduced into ashes, requires the being farther calcined than it can be in a close vessel. Besides, having kindled amber, and held a clean silver spoon, or some other concave and smooth vessel, over the smoke of its flame, I observed the soot, into which that fume condensed, to be very differing from any thing, that I had observed to proceed from the steam of amber purposely (for that is not usual) distilled *per se* in close vessels. Thus having, for trial's sake, kindled camphire, and catched the smoke, that copiously ascended out of the flame, it condensed into a black and unctuous soot, which would not have been guessed by the smell or other properties to have proceeded from camphire: whereas having (as I shall otherwhere more fully declare) exposed a quantity of that fugitive concrete to a gentle heat in a close glass-vessel, it sublimed up without seeming to have lost any thing of its whiteness, or its nature; both which it retained, though afterwards I so increased the fire, as to bring it to fusion. And, besides camphire, there are divers other bodies (that I elsewhere name) in which the heat in close vessels is not wont to make any separation of heterogeneities, but only a comminution of parts, those that rise first being homogeneal with the others, though subdivided into smaller particles: whence sublimations have been styled, *The Pestles of the Chymists.* But not here to mention what I elsewhere take notice of, concerning common brimstone once or twice sublimed, that, exposed to a moderate fire in subliming-pots, it rises all into dry, and almost tasteless, flowers: whereas being exposed to a naked fire, it affords store of a saline and fretting liquor; not to mention this, I say, I will further observe to you, that as it is considerable in the analysis of mixt bodies, whether the fire act on them, when they are exposed to the open air, or shut up in close vessels, so is the degree of fire, by which the analysis is attempted, of no small moment. For a mild balneum will sever unfermented blood (for instance) but into phlegm and *caput mortuum*, the latter whereof (which I have sometimes had) hard, brittle, and of divers colours (transparent almost like tortoise-shell) pressed by a good fire in a retort yields a spirit, an oil or two, and a volatile salt, besides another *caput mortuum*. It may be also pertinent to our present design, to take notice of what happens in the making and distilling of soap; for by one degree

degree of fire, the ſalt, the water, and the oil or greaſe, whereof that factitious concrete is made up, being boiled up together, are eaſily brought to mingle and incorporate into one maſs; but by another and further degree of heat, the ſame maſs may be again divided into an oleaginous and aqueous, a ſaline and an earthy part. And ſo we may obſerve, that impure ſilver and lead being expoſed together to a moderate fire, will thereby be colliquated into one maſs, and mingle *per minima*, as they ſpeak; whereas a much vehementer fire will drive or carry off the baſer metals (I mean the lead, and the copper or other alloy) from the ſilver, though not, for aught appears, ſeparate them from one another. Beſides, when a vegetable abounding in fixed ſalt is analyzed by a naked fire, as one degree of heat will reduce it into aſhes, (as the chymiſts themſelves teach us) ſo, by only a further degree of fire, thoſe aſhes may be vitrified and turned into glaſs. I will not ſtay to examine, how far a mere chymiſt might on this occaſion demand, if it be lawful for an Ariſtotelian to make aſhes (which he miſtakes for mere earth) paſs for an element, becauſe by one degree of fire it may be produced, why a chymiſt may not upon the like principle argue, that glaſs is one of the elements of many bodies, becauſe that alſo may be obtained from them, barely by the fire? I will not, I ſay, loſe time to examine this, but obſerve, that by a method of applying the fire, ſuch ſimilar bodies may be obtained from a concrete, as chymiſts have not been able to ſeparate; either by barely burning it in an open fire, or by barely diſtilling it in cloſe veſſels. For to me it ſeems very conſiderable, and I wonder, that men have taken ſo little notice of it, that I have not, by any of the common ways of diſtillation in cloſe veſſels, ſeen any ſeparation made of ſuch a volatile ſalt as is afforded us by wood, when that is firſt by an open fire divided into aſhes and ſoot, and that ſoot is afterwards placed in a ſtrong retort, and compelled by an urgent fire to part with its ſpirit, oil and ſalt. For though I dare not peremptorily deny, that in the liquors of guaiacum and other woods diſtilled in retorts after the common manner, there may be ſaline parts, which by reaſon of the analogy may pretend to the name of ſome kind of volatile ſalts; yet queſtionleſs there is a great diſparity betwixt ſuch ſalts, and that which we have ſometimes obtained upon the firſt diſtillation of ſoot (though for the moſt part it has not been ſeparated from the firſt or ſecond rectification, and ſometimes not till the third). For we could never yet ſee ſeparated from woods analyzed only the vulgar way in cloſe veſſels, any volatile ſalt in a dry and ſaline form, as that of ſoot, which we have often had very cryſtalline, and geometrically figured. And then, whereas the ſaline parts of the ſpirits of guaiacum, *&c.* appear upon diſtillation ſluggiſh enough, the ſalt of ſoot ſeems to be one of the moſt volatile bodies in all nature; and if it be well made will readily aſcend with the mild heat of a furnace, warmed only by the ſingle wick of a lamp, to the top of the higheſt glaſs veſſels that are commonly made uſe of for diſtillation. And beſides all this, the taſte and ſmell of the ſalt of ſoot are exceeding differing from thoſe of the ſpirits of guaiacum, *&c.* and the former not only ſmells and taſtes much leſs like a vegetable ſalt, than like that of hartſhorn, and other animal concretes; but in divers other properties ſeems more of kin to the family of animals, than to that of vegetable ſalts, as I may elſewhere (God permitting) have an occaſion more particularly to declare. I might likewiſe by ſome other examples manifeſt, that the chymiſts, to have dealt clearly, ought to have more explicitly and particularly declared by what degree of fire, and in what manner of application of it, they would have us judge a diviſion made by the fire to be a true analyſis into their principles, and the productions of it to deſerve the name of elementary bodies. But it is time, that I proceed to mention the particular reaſons, that incline me to

doubt, whether the fire be the true and universal analyzer of mixt bodies; of which reasons, what has been already objected may pass for one.

In the next place, I observe, that there are some mixt bodies, from which it has not been yet made appear, that any degree of fire can separate either salt, or sulphur, or mercury, much less all the three. The most obvious instance of this truth is gold, which is a body so fixed, and wherein the elementary ingredients (if it have any) are so firmly united to each other, that we find not in the operations, wherein gold is exposed to the fire, how violent soever, that it does discernably so much as lose of its fixedness or weight, so far is it from being dissipated into those principles, whereof one at least is acknowledged to be fugitive enough; and so justly did the spagyrical poet somewhere exclaim,

Cuncta adeo miris illic compagibus hærent.

Gasto Claveus Apolog. Argur. & Oppera.

And I must not omit on this occasion to mention to you, *Eleutherius*, the memorable experiment, that I remember I met with in *Gasto Claveus*, who, though a lawyer by profession, seems to have had no small curiosity and experience in chymical affairs. He relates then, that having put into one small earthen vessel, an ounce of the most pure gold, and into another the like weight of pure silver, he placed them both in that part of a glass-house furnace, wherein the workmen keep their metal (as our English artificers call their liquid glass) continually melted; and, that having there kept both the gold and the silver in constant fusion for two months together, he afterwards took them out of the furnace and the vessels, and weighing both of them again, found, that the silver had not lost above a 12th part of its weight, but the gold had not of his lost any thing at all. And though our author endeavours to give us of this a scholastic reason, which I suppose you would be as little satisfied with, as I was when I read it; yet for the matter of fact, which will serve our present turn, he assures us, that though it be strange, yet experience itself taught it him to be most true.

And though there be not perhaps any other body to be found so perfectly fixed as gold, yet there are divers others so fixed or composed, at least of so strictly united parts, that I have not yet observed the fire to separate from them any one of the chymists principles. I need not tell you, what complaints the more candid and judicious of the chymists themselves are wont to make of those boasters, that confidently pretend, that they have extracted the salt or sulphur of quicksilver, when they have disguised it by additaments, wherewith it resembles the concretes, whose names are given it; whereas by a skilful and rigid examen, it may be easily enough stripped of its disguises, and made to appear again in the pristine form of running mercury. The pretended salts and sulphurs being so far from being elementary parts extracted out of the body of mercury, that they are rather (to borrow a term of the grammarians) de-compound bodies, made up of the whole metal and the menstruum, or other additaments employed to disguise it. And as for silver, I never could see any degree of fire make it part with any of its three principles. And though the experiment lately mentioned from *Claveus* may beget a suspicion, that silver may be dissipated by fire, provided it be extremely violent and very lasting; yet it will not necessarily follow, that because the fire was able at length to make the silver lose a little of its weight, it was therefore able to dissipate it into its principles. For first I might allege, that I have observed little grains of silver to lie hid in the small cavities (perhaps glassed over by a vitrifying heat) in crucibles, wherein silver has been long kept

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in fusion, whence some goldsmiths of my acquaintance make a benefit by grinding such crucibles to powder, to recover out of them the latent particles of silver. And hence I might argue, that perhaps *Claveus* was mistaken, and imagined, that silver to have been driven away by the fire, that indeed lay in minute parts hid in his crucible, in whose pores so small a quantity, as he missed of so ponderous a body, might very well lie concealed.

BUT secondly, admitting, that some parts of the silver were driven away by the violence of the fire, what proof is there, that it was either the salt, the sulphur, or the mercury of the metal, and not rather a part of it homogeneous to what remained? for besides that the silver, that was left, seemed not sensibly altered, which probably would have appeared, had so much of any one of its principles been separated from it; we find in other mineral bodies of a less permanent nature than silver, that the fire may divide them into such minute parts, as to be able to carry them away with itself, without at all destroying their nature. Thus we see, that in the refining of silver, the lead, that is mixed with it (to carry away the copper or other ignoble mineral that embasses the silver) will, if it be let alone, in time evaporate away upon the test; but if (as is most usual amongst those, that refine great quantities of metals together) the lead be blown off from the silver by bellows, that, which would else have gone away in the form of unheeded steams, will, in great part, be collected not far from the silver, in the form of a darkish powder or calx; which, because it is blown off from silver, they call litharge of silver. And thus *Agricola* in divers places informs us, when copper, or the ore of it is colliquated by the violence of the fire with Cadmia, the sparks, that in great multitudes do fly upwards, do some of them stick to the vaulted roofs of the furnaces, in the form of little and (for the most part) white bubbles; which therefore the Greeks, and, in imitation of them, our drugsters call pompholyx: and others more heavy partly adhere to the sides of the furnace, and partly (especially if the covers be not kept upon the pots) fall to the ground, and by reason of their ashy colour, as well as weight, were called by the same Greeks σποδὸς, which, I need not tell you, in their language signifies ashes. I might add, that I have not found, that from Venetian talc (I say Venetian, because I have found other kinds of that mineral more open) from the lapis ossifragus (which the shops call ostiocolla) from *Muscovia* glass, from pure and fusible sand (to mention now no other concretes) those of my acquaintance, that have tried, have been able by the fire to separate any one of the hypostatical principles: which you will the less scruple to believe, if you consider, that glass may be made by the bare colliquation of the salt and earth remaining in the ashes of a burnt plant, and that yet common glass, once made, does so far resist the violence of the fire, that most chymists think it a body more undestroyable than gold itself. For if the artificer can so firmly unite such comparative gross particles as those of earth and salt, that make up common ashes, into a body indissoluble by fire, why may not nature associate in divers bodies the more minute elementary corpuscles, she has at hand, too firmly to let them be separable by the fire? And on this occasion, *Eleutherius*, give me leave to mention to you two or three slight experiments, which will, I hope, be found more pertinent to our present discourse, than at first perhaps they will appear. The first is, that, having (for trial's sake) put a quantity of that fugitive concrete, camphire, into a glass vessel, and placed it in a gentle heat, I found it (not leaving behind, according to my estimate, not so much as one grain) to sublime to the top of the vessel into flowers; which in whiteness, smell, *&c.* seemed not to differ from the camphire itself. Another experiment is that of *Helmont*, who in several places affirms, that a coal

Agricola de Natura Fossil. Lib. 9. cap. 11, and 12.

kept in a glass exactly closed will never be calcined to ashes, though kept never so close in a strong fire. To countenance which, I shall tell you this trial of my own, that having sometimes distilled some woods, as particularly box, whilst our *caput mortuum* remained in the retort, it continued black like charcoal, though the retort were earthen, and kept red-hot in a vehement fire: but as soon as ever it was brought out of the candent vessel into the open air, the burning coals did hastily degenerate or fall asunder, without the assistance of any new calcination, into pure white ashes. And to these two I shall add but this obvious and known observation, that common sulphur (if it be pure and freed from its vinegar) being leisurely sublimed in close vessels, rises into dry flowers, which may be presently melted into a body of the same nature with that which afforded them. Though, if brimstone be burnt in the open air, it gives, you know, a penetrating fume, which being caught in a glass-bell, condenses into that acid liquor called oil of sulphur *per campanam*. The use I would make of these experiments, collated with what I lately told you out of *Agricola*, is this, that even among the bodies, that are not fixt, there are divers of such a texture, that it will be hard to make it appear, how the fire, as chymists are wont to imploy it, can resolve them into elementary substances. For some bodies being of such a texture, that the fire can drive them into the cooler and less hot part of the vessels, wherein they are included, and, if need be, remove them from place to place, to fly the greatest heat, more easily than it can divorce their elements (especially without the assistance of the air) we see that our chymists cannot analyze them in close vessels, and of other compound bodies the open fire can as little separate the elements. For what can a naked fire do to analyze a mixt body, if its component principles be so minute, and so strictly united, that the corpuscles of it need less heat to carry them up, than is requisite to divide them into their principles? So that of some bodies the fire cannot in close vessels make any analysis at all; and others will in the open air fly away in the forms of flowers or liquors, before the heat can prove able to divide them into their principles. And this may hold, whether the various similar parts of a concrete be combined by nature or by art; for in factitious sal armoniac we find the common and the urinous salts so well mingled, that both in the open fire, and in subliming vessels they rise together, as one salt, which seems in such vessels irresoluble by fire alone. For I can shew you sal armoniac, which after the ninth sublimation does still retain its compounded nature. And indeed I scarce know any one mineral, from which by fire alone chymists are wont to sever any substance simple enough to deserve the name of an element or principle. For though out of native cinnabar they distil quicksilver, and though from many of those stones, that the antients called *Pyrites*, they sublime brimstone, yet both that quicksilver and this sulphur, being very often the same with the common minerals, that are sold in the shops under those names, are themselves too much compounded bodies to pass for the elements of such. And thus much, *Eleutherius*, for the second argument, that belongs to my first consideration; the others I shall the less insist on, because I have dwelt so long upon this.

PROCEED we then, in the next place, to consider that there are divers separations to be made by other means, which either cannot at all, or else cannot so well be made by the fire alone. When gold and silver are melted into one mass, it would lay a great obligation upon refiners and goldsmiths, to teach them the art of separating them by the fire, without the trouble and charge they are fain to be at to sever them. Whereas they may be very easily parted by the affusion of spirit of nitre or aqua fortis; which the French therefore call *eau de depart*. So likewise the metalline part of vitriol will not be so easily and conveniently separated from the saline part, even by

by a violent fire, as by the affusion of certain alkalizate salts, in a liquid form, upon the solution of vitriol made in common water. For thereby the acid salt of the vitriol leaving the copper, it had corroded, to join with the added salts, the metalline part will be precipitated to the bottom almost like mud. And that I may not give instances only in decompound bodies, I will add a not useless one of another kind. Not only chymists have not been able (for aught is vulgarly known) by fire alone to separate true sulphur from antimony; but though you may find in their books many plausible processes of extracting it, yet he, that shall make as many fruitless trials as I have done to obtain it, by most of them will, I suppose, be easily persuaded, that the productions of such processes are antimonial sulphurs rather in name than nature. But though antimony sublimed by itself is reduced but to a volatile powder, or antimonial flowers, of a compounded nature like the mineral, that affords them; yet I remember, that some years ago I sublimed out of antimony a sulphur, and that in greater plenty than ever I saw obtained from that mineral, by a method, which I shall therefore acquaint you with, because chymists seem not to have taken notice, of what importance such experiments may be in the indagation of the nature, and especially of the number of the elements. Having then purposely for trial sake digested eight ounces of good and well powdered antimony with twelve ounces of oil of vitriol, in a well stopped glass-vessel, for about six or seven weeks; and having caused the mass (grown hard and brittle) to be distilled in a retort placed in sand, with a strong fire; we found the antimony to be so opened, or altered by the menstruum, wherewith it had been digested, that whereas crude antimony forced up by the fire, arises only in flowers, our antimony thus handled afforded us partly in the receiver, and partly in the neck and at the top of the retort, about an ounce of sulphur, yellow and brittle like common brimstone, and of so sulphureous a smell, that upon the unluting the vessels it infected the room with a scarce supportable stink. And this sulphur, besides the colour and smell, had the perfect inflammability of common brimstone, and would immediately kindle (at the flame of a candle) and burn blue like it. And though it seemed, that the long digestion, wherein our antimony and menstruum were detained, did conduce to the better unlocking of the mineral, yet if you have not the leisure to make so long a digestion, you may, by incorporating with powdered antimony a convenient quantity of oil of vitriol, and committing them immediately to distillation, obtain a little sulphur like unto the common one, and more combustible than, perhaps, you will at first take notice of. For I have observed, that though (after its being first kindled) the flame would sometimes go out too soon of itself, if the same lump of sulphur were held again to the flame of a candle, it would be rekindled and burn a pretty while, not only after the second, but after the third or fourth accension. You, to whom I think I shewed my way of discovering something of sulphureous in oil of vitriol, may perchance suspect, *Eleutherius*, either that this substance was some venereal sulphur, that lay hid in that liquor, and was by this operation only reduced into a manifest body; or else, that it was a compound of the unctuous parts of the antimony, and the saline ones of the vitriol, in regard, that (as *Gunther* informs us) divers learned men would have sulphur to be nothing but a mixture, made in the bowels of the earth, of vitriolate spirits and a certain combustible substance: but the quantity of sulphur we obtained by digestion was much too great to have been latent in the oil of vitriol. And that vitriolate spirits are not necessary to the construction of such a sulphur as ours, I could easily manifest, if I would acquaint you with the several ways, by which I have obtained, though not in such plenty, a sulphur of antimony, coloured and combustible like common brimstone.

Lib. 1. Observat. cap. 6.

And though I am not now minded to discover them, yet I shall tell you, that to satisfy some ingenious men, that distilled vitriolate spirits are not necessary to the obtaining of such a sulphur as we have been considering, I did by the bare distillation of only spirit of nitre, from its weight of crude antimony, separate, in a short time, a yellow and very inflammable sulphur; which, for aught I know, deserves as much the name of an element, as any thing, that chymists are wont to separate from any mineral by the fire. I could perhaps tell you of other operations upon antimony, whereby that may be extracted from it, which cannot be forced out of it by the fire; but I shall reserve them for a fitter opportunity, and only annex at present this slight but not impertinent experiment: that whereas I lately observed to you, that the urinous and common salts, whereof sal armoniac consists, remained unsevered by the fire in many successive sublimations, they may be easily separated, and partly without any fire at all, by pouring upon the concrete finely powdered, a solution of salt of tartar, or of the salt of wood-ashes: for upon your diligently mixing of these, you will find your nose invaded with a very strong smell of urine, and perhaps too your eyes forced to water, by the same subtile and piercing body, that produces the stink; both these effects proceeding from hence, that by the alcalizate salt, the sea salt, that entered the composition of the sal armoniac, is mortified and made more fixt, and thereby a divorce is made between it and the volatile urinous salt; which being at once set at liberty, and put into motion, begins presently to fly away, and to offend the nostrils and eyes it meets with by the way. And if the operation of these salts be in convenient glasses promoted by warmth, though but by that of a bath, the ascending steams may easily be caught and reduced into a penetrant spirit, abounding with a salt, which I have sometimes found to be separable in a crystalline form. I might add to these instances, that whereas sublimate, consisting, as you know, of salts and quicksilver combined and carried up together by heat, may be sublimed, I know not how often, by a like degree of fire, without suffering any divorce of the component bodies, the mercury may be easily severed from the adhering salts, if the sublimate be distilled from salt of tartar, quick lime, or such alkalizate bodies. But I will rather observe to you, *Eleutherius*, what divers ingenious men have thought somewhat strange; that by such an additament, that seems but only to promote the separation, there may be easily obtained from a concrete, that by the fire alone is easily divisible into all the elements, that vegetables are supposed to consist of, such a similar substance, as differs in many respects from them all, and consequently has by many of the most intelligent chymists been denied to be contained in the mixt body. For I know a way, and have practised it, whereby common tartar, without the addition of any thing, that is not perfectly a mineral, except salt-petre, may by one distillation in an earthern retort be made to afford good store of real salt readily dissoluble in water; which I found to be neither acid, nor of the smell of tartar, and to be almost as volatile as spirit of wine itself, and to be indeed of so differing a nature from all, that is wont to be separated by fire from tartar; that divers learned men, with whom I discoursed of it, could hardly be brought to believe, that so fugitive a salt could be afforded by tartar, till I assured it them upon my own knowledge. And if I did not think you apt to suspect me to be rather too backward than too forward to credit or affirm unlikely things, I could convince you, by what I have yet lying by me, of that anomalous salt.

THE fourth thing, that I shall allege to countenance my first consideration, is, that the fire, even when it divides a body into substances of divers consistencies, does not most commonly analyze it into hypostatical principles, but only disposes its parts into new

new textures, and thereby produces concretes of a new indeed, but yet of a compound nature. This argument it will be requisite for me to prosecute so fully hereafter, that I hope you will then confess, that it is not for want of good proofs, that I desire leave to suspend my proofs, till the series of my discourse shall make it more proper and seasonable to propose them.

IT may be further alleged on the behalf of my first consideration, that some such distinct substances may be obtained from some concretes without fire, as deserve no less the name of elementary, than many, that chymists extort by the violence of the fire.

WE see, that the inflammable spirit, or, as the chymists esteem it, the sulphur of wine, may not only be separated from it by the gentle heat of a bath, but may be distilled either by the help of the sun-beams, or even of a dunghill, being indeed of so fugitive a nature, that it is not easy to keep it from flying away, even without the application of external heat. I have likewise observed, that a vessel full of urine being placed in a dunghill, the putrefaction is wont, after some weeks, so to open the body, that the parts disbanding the saline spirit, will within no very long time, if the vessel be not stopped, fly away of itself; insomuch, that from such urine I have been able to distil little or nothing else than a nauseous phlegm, instead of the active and piercing salt and spirit, that it would have afforded, when first exposed to the fire, if the vessel had been carefully stopped.

AND this leads me to consider, in the fifth place, that it will be very hard to prove, that there can no other body or way be given, which will, as well as the fire, divide concretes into several homogeneous substances, which may consequently be called their elements or principles, as well as those separated or produced by the fire. For since we have lately seen, that nature can successfully employ other instruments than the fire to separate distinct substances from mixed bodies, how know we, but that nature has made, or art may make, some such substance, as may be a fit instrument to analyze mixt bodies, or that some such method may be found by human industry or luck, by whose means compound bodies may be resolved into other substances, than such as they are wont to be divided into by the fire? And why the products of such an analysis may not as justly be called the component principles of the bodies, that afford them, it will not be easy to shew; especially since I shall hereafter make it evident, that the substances, which chymists are wont to call the salts, and sulphurs, and mercuries of bodies, are not so pure and elementary as they presume, and as their hypothesis requires. And this may therefore be the more freely pressed upon the chymists, because neither the Paracelsians, nor the Helmontians, can reject it without apparent injury to their respective masters. For *Helmont* does more than once inform his readers, that both *Paracelsus* and himself were possessors of the famous liquor, alkahest, which for its great power in resolving bodies irresoluble by vulgar fires, he somewhere seems to call *Ignis Gehennæ*. To this liquor he ascribes (and that in great part upon his own experience) such wonders, that if we suppose them all true, I am so much the more a friend to knowledge than to wealth, that I should think the alkahest a nobler and more desirable secret, than the philosophers stone itself. Of this universal dissolvent he relates, that having digested with it for a competent time a piece of oaken charcoal, it was thereby reduced into a couple of new and distinct liquors, discriminated from each other by their colour and situation, and that the whole body of the coal was reduced into those liquors, both of them separable from his immortal menstruum, which remained as fit for such operations as before. And he moreover tells us in divers places of his writings, that by his powerful, and unwearied

wearied agent, he could diſſolve metals, marchaſites, ſtones, vegetable and animal bodies of what kind ſoever, and even glaſs itſelf (firſt reduced to powder) and in a word, all kind of mixt bodies in the world, into their ſeveral ſimilar ſubſtances without any reſidence or *caput mortuum*. And laſtly, we may gather this further from his informations, that the homogeneous ſubſtances obtainable from compound bodies by his piercing liquor, were oftentimes different enough both as to number and as to nature, from thoſe into which the ſame bodies are wont to be divided by common fire. Of which I ſhall need in this place to mention no other proof, than that whereas we know, that in our common analyſis of a mixt body, there remains a terreſtrial and very fixed ſubſtance, oftentimes aſſociated with a ſalt as fixed; our author tells us, that by his way he could diſtil over all concretes without any *caput mortuum*, and conſequently could make thoſe parts of the concrete volatiſe, which in the vulgar analyſis would have been fixt. So that if our chymiſts will not reject the ſolemn and repeated teſtimony of a perſon, who cannot but be acknowledged for one of the greateſt ſpagyriſts, that they can boaſt of, they muſt not deny, that there is to be found in nature another agent able to analyze compound bodies leſs violently, and both more genuinely and more univerſally than the fire. And for my own part, though I cannot but ſay on this occaſion what (you know) our friend Mr. *Boyle* is wont to ſay, when he is aſked his opinion of any ſtrange experiment, *That he, that hath ſeen it, hath more reaſon to believe it, than he that hath not*; yet I have found *Helmont* ſo faithful a writer, even in divers of his improbable experiments (I always except that extravagant treatiſe *De Magnetica Vulnerum Curatione*, which ſome of his friends affirm to have been firſt publiſhed by his enemies) that I think it ſomewhat harſh to give him the lye, eſpecially to what he delivers upon his own proper trial. And I have heard from very credible eye-witneſſes ſome things, and ſeen ſome others myſelf, which argue ſo ſtrongly, that a circulated ſalt, or a menſtruum (ſuch as it may be) may, by being abſtracted from compound bodies, whether mineral, animal, or vegetable, leave them more unlocked than a wary naturaliſt would eaſily believe; that I dare not confidently meaſure the power of nature and art by that of the menſtruums, and other inſtruments that eminent chymiſts themſelves are as yet wont to employ about the analyzing of bodies; nor deny, that a menſtruum may at leaſt from this or that particular concrete obtain ſome apparently ſimilar ſubſtance, differing from any obtainable from the ſame body by any degree or manner of application of the fire. And I am the more backward to deny peremptorily, that there may be ſuch openers of compound bodies, becauſe among the experiments, that make me ſpeak thus warily, there wanted not ſome, in which it appeared not, that one of the ſubſtances, not ſeparable by common fires and menſtruums, could retain any thing of the ſalt, by which the ſeparation was made.

AND here, *Eleutherius* (ſays *Carneades*) I ſhould conclude as much of my diſcourſe as belongs to the firſt conſideration I propoſed, but that I foreſee, that what I have delivered will appear liable to two ſuch ſpecious objections, that I cannot ſafely proceed any further, till I have examined them.

AND firſt, one ſort of oppoſers will be forward to tell me, that they do not pretend by fire alone to ſeparate out of all compound bodies their hypoſtatical principles; it being ſufficient, that the fire divides them into ſuch, though afterwards they employ other bodies to collect the ſimilar parts of the compound; as it is known, that though they make uſe of water, to collect the ſaline parts of aſhes from the terreſtrial, wherewith they are blended, yet it is the fire only, that incinerates bodies, and reduced the fixed part of them into the ſalt and earth, whereof aſhes are made up.

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This objection is not, I confess, inconsiderable, and I might in great part allow of it, without granting it to make against me, if I would content myself to answer, that it is not against those, that make it, that I have been disputing, but against those vulgar chymists, who themselves believe, and would fain make others do so, that the fire is not only an universal, but an adequate and sufficient instrument to analyze mixt bodies with. For as to their practice of extracting the fixed salt out of ashes by the affusion of water, it is obvious to allege, that the water does only assemble together the salt the fire had before divided from the earth: as a sieve does not further break the corn, but only bring together into two distinct heaps the flower and the bran, whose corpuscles before lay promiscuously blended together in the meal. This I say I might allege, and thereby exempt myself from the need of taking any farther notice of the proposed objection. But not to lose the rise it may afford me of illustrating the matter under consideration, I am content briefly to consider it, as far forth as my present disquisition may be concerned in it.

NOT to repeat then what has been already answered, I say further, that though I am so civil an adversary, that I will allow the chymists, after the fire has done all its work, the use of fair water to make their extractions with, in such cases wherein the water does not co-operate with the fire to make the analysis; yet since I grant this but upon supposition, that the water does only wash off the saline particles, which the fire alone has before extricated in the analyzed body, it will not be reasonable, that this concession should extend to other liquors, that may add to what they dissolve, nor so much as to other cases than those newly mentioned: which limitation I desire you would be pleased to bear in mind, till I shall anon have occasion to make use of it. And this being thus premised, I shall proceed to observe,

FIRST, that many of the instances I proposed in the preceding discourse are such, that the objection we are considering will not at all reach them. For fire can no more with the assistance of water, than without it, separate any of the three principles, either from gold, silver, mercury, or some other of the concretes named above.

HENCE we may infer, that fire is not an universal analyzer of all mixt bodies, since of metals and minerals, wherein chymists have most exercised themselves, there appear scarce any, which they are able to analyze by fire, nay, from which they can unquestionably separate so much as any one of their hypostatical principles; which may well appear no small disparagement, as well to their hypothesis, as to their pretensions.

IT will also remain true, notwithstanding the objection, that there may be other ways, than the wonted analysis by fire, to separate from a compound body substances as homogeneous as those, that chymists scruple not to reckon among their *tria prima*, (as some of them, for brevity sake, call their three principles.)

AND it appears, that by convenient additaments such substances may be separated by the help of the fire, as could not be so by the fire alone: witness the sulphur of antimony.

AND lastly, I must represent, that since it appears too, that the fire is but one of the instruments, that must be employed in the resolution of bodies, we may reasonably challenge the liberty of doing two things. For whenever any menstruum or other additament is employed, together with the fire, to obtain a sulphur or a salt from a body, we may well take the freedom to examine, whether or no that menstruum do barely help to separate the principle obtained by it, or whether there intervene not a coalition of the parts of the body wrought upon with those of the menstruum, whereby

whereby the produced concrete may be judged to result from the union of both. And it will be farther allowable for us to consider, how far any substance, separated by the help of such additaments, ought to pass for one of the *tria prima*; since by one way of handling the same mixt body, it may, according to the nature of the additaments, and the method of working upon it, be made to afford differing substances from it by other additaments, and another method, nay and (as may appear by what I formerly told you about tartar) differing from any of the substances, into which a concrete is divisible by the fire without additaments, though, perhaps, those additaments do not, as ingredients, enter the composition of the obtained body, but only diversify the operation of the fire upon the concrete; and though that concrete by the fire alone may be divided into a number of differing substances, as great as any of the chymists, that I have met with, teach us that of the elements to be. And having said thus much (says *Carneades*) to the objection likely to be proposed by some chymists, I am now to examine that, which I foresee will be confidently pressed by divers Peripatetics, who, to prove fire to be the true analyzer of bodies, will plead, That it is the very definition of heat given by *Aristotle*, and generally received, *congregare homogenea, & segregare heterogenea*, 'to assemble things of a resembling, and 'disjoin those of a differing nature.' To this I answer, that this effect is far from being so essential to heat, as it is generally imagined; for it rather seems, that the true and genuine property of heat is, to set a moving, and thereby to dissociate the parts of bodies, and subdivide them into minute particles, without regard to their being homogeneous or heterogeneous; as is apparent in the boiling of water, the distillation of quicksilver, or the exposing of bodies to the action of the fire, whose parts either are not (at least in that degree of heat appear not) dissimilar, where, all that the fire can do, is to divide the body into very minute parts, which are of the same nature with one another, and with their totum, as their reduction by condensation evinces. And even when the fire seems most so *congregare homogenea, & segregare heterogenea*, it produces that effect but by accident; for the fire does but dissolve the cement, or rather shatter the frame, or structure, that kept the heterogeneous parts of bodies together, under the common form; upon which dissolution the component particles of the mixt, being freed and set at liberty, do naturally, and oftentimes without any operation of the fire, associate themselves each with its like, or rather do take those places, which their several degrees of gravity and levity, fixedness or volatility (either natural, or adventitious from the impression of the fire) assign them. Thus in the distillation (for instance) of man's blood, the fire does first begin to dissolve the nexus or cement of the body; and then the water, being the most volatile, and easy to be extracted, is either by the igneous atoms, or the agitation they are put into by the fire, first carried up, till forsaken by what carried it up, its weight sinks it down, into the receiver: but all this while the other principles of the concrete remain unsevered, and require a stronger degree of heat to make a separation of its more fixt elements; and therefore the fire must be increased, which carries over the volatile salt and the spirit, they being, though believed to be differing principles, and though really of different consistency, yet of an almost equal volatility. After them, as less fugitive, comes over the oil, and leaves behind the earth and the alcali, which being of an equal fixedness, the fire severs them not, for all the definition of the schools. And if into a red-hot earthen or iron retort you cast the matter to be distilled, you may observe, as I have often done, that the predominant fire will carry up all the volatile elements confusedly in one fume, which will afterwards take their places in the receiver, either according to the degree of their gravity, or according to

to the exigency of their respective textures; the salt adhering, for the most part, to the sides and top, and the phlegm fastening itself there too in great drops, the oil and spirit placing themselves under, or above one another, according as their ponderousness makes them swim or sink. For it is observable, that though oil or liquid sulphur be one of the elements separated by this fiery analysis, yet the heat, which accidentally unites the particles of the other volatile principles, has not always the same operation on this, there being divers bodies, which yield two oils, whereof the one sinks to the bottom of that spirit, on which the other swims; as I can shew you in some oils of the same deer's blood, which are yet by me: nay, I can shew you two oils carefully made of the same parcel of human blood, which not only differ extremely in colour, but swim upon one another without mixture, and, if by agitation confounded, will of themselves divorce again.

AND that the fire doth oftentimes divide bodies, upon the account, that some of their parts are more fixt, and some more volatile, how far soever either of these two may be from a pure elementary nature, is obvious enough, if men would but heed it in the burning of wood, which the fire dissipates into smoke and ashes: for not only the latter of these is confessedly made up of two such differing bodies as earth and salt; but the former being condensed into that soot, which adheres to our chimneys, discovers itself to contain both salt and oil, and spirit and earth (and some portion of phlegm too) which being, all almost, equally volatile to that degree of fire, which forces them up, (the more volatile parts helping, perhaps, as well as the urgency of the fire, to carry up the more fixt ones, as I have often tried in dulcified colcothar, sublimed by sal armoniac blended with it) are carried up together, but may afterwards be separated by other degrees of fire, whose orderly gradation allows the disparity of their volatileness to discover itself. Besides, if differing bodies united into one mass be both sufficiently fixt, the fire finding no parts volatile enough to be expelled or carried up, makes no separation at all; as may appear by a mixture of colliquated silver and gold, whose component metals may be easily sever'd by aqua fortis or aqua regis (according to the predominancy of the silver and gold) but in the fire alone, though vehement, the metals remain unsever'd, the fire only dividing the body into smaller particles (whose littleness may be argued from their fluidity) in which either the little nimble atoms of fire, or its brisk and numberless strokes upon the vessels, hinder rest and continuity, without any sequestration of elementary principles. Moreover, the fire sometimes does not separate, so much as unite, bodies of a differing nature; provided they be of an almost resembling fixedness, and have in the figure of their parts an aptness to coalition, as we see in the making of many plaisters, ointments, *&c.* And in such metalline mixtures as that made by melting together two parts of clean brass with one of pure copper, of which some ingenious tradesmen cast such curious patterns (for gold and silver works) as I have sometimes taken great pleasure to look upon. Sometimes the bodies mingled by the fire are differing enough as to fixidity and volatility, and yet are so combined by the first operation of the fire, that itself does scarce afterwards separate them, but only pulverize them; whereof an instance is afforded us by the common preparation of mercurius dulcis, where the saline particles of the vitriol, sea-salt, and sometimes nitre, employed to make the sublimate, do so unite themselves with the mercurial particles made use of, first to make sublimate, and then to dulcify it, that the saline and metalline parts arise together in many successive sublimations, as if they all made but one body. And sometimes too, the fire does not only not sever the differing elements of a body, but combine them so firmly, that nature herself does very seldom, if

ever, make unions less dissoluble. For the fire meeting with some bodies exceedingly and almost equally fixt, instead of making a separation, makes an union so strict, that itself, alone, is unable to dissolve it; as we see, when an alcalizate salt and the terrestrial residue of the ashes are incorporated with pure sand, and by vitrification made one permanent body, (I mean the coarse or greenish sort of glass) that mocks the greatest violence of the fire, which though able to marry the ingredients of it, yet is not able to divorce them. I can shew you some pieces of glass, which I saw flow down from an earthen crucible purposely exposed for a good while, with silver in it, to a very vehement fire. And some that deal much in the fusion of metals inform me, that the melting of a great part of a crucible into glass is no great wonder in their furnaces. I remember, I have observed too in the melting of great quantities of iron out of the ore, by the help of store of charcoal (for they affirm that sea-coal will not yield a flame strong enough) that by the prodigious vehemence of the fire, excited by vast bellows (made to play by great wheels turned about by water) part of the materials exposed to it, was, instead of being analized, colliquated, and turned into a dark, solid, and very ponderous glass, and that in such quantity, that in some places I have seen the very high-ways, near such iron-works, mended with heaps of such lumps of glass, instead of stones and gravel. And I have also observed, that some kind of fire-stone itself, having been employed in furnaces wherein it was exposed to very strong and lasting fires, has had all its fixed parts so wrought on by the fire, as to be perfectly vitrified, which I have tried by forcing from it pretty large pieces of perfect and transparent glass. And lest you might think *Eleutherius*, that the questioned definition of heat may be demonstrated, by the definition, which is wont to be given and acquiesced in, of its contrary quality, cold; whose property is taught to be *tam homogenea, quam heterogenea congregare*, give me leave to represent to you, that neither is this definition unquestionable; for not to mention the exceptions, which a logician, as such, may take at it, I consider, that the union of heterogeneous bodies, which is supposed to be the genuine production of cold, is not performed by every degree of cold. For we see, for instance, that in the urine of healthy men, when the liquor has been suffered a while to stand, the cold makes a separation of the thinner part from the grosser, which subsides to the bottom, and grows opacous there; whereas, if the urinal be warm, these parts readily mingle again, and the whole liquor becomes transparent as before. And when, by glaciation, wood, straw, dust, water, *&c.* are supposed to be united into one lump of ice, the cold does not cause any real union or adonation (if I may so speak) of these bodies, but only hardening the aqueous parts of the liquor into ice, the other bodies, being accidentally present in that liquor, are frozen up in it, but not really united. And accordingly, if we expose a heap of money, consisting of gold, silver and copper coins, or any other bodies of differing natures, which are destitute of aqueous moisture, capable of congelation, to never so intense a cold, we find not, that these differing bodies are at all thereby so much as compacted, much less united together; and even in liquors themselves we find phænomena, which induce us to question the definition, which we are examining. If *Paracelsus* his authority were to be looked upon as a sufficient proof in matters of this nature, I might here insist on that process of his, whereby he teaches, that the essence of wine may be sever'd from the phlegm and ignoble part by the assistance of congelation. And because much weight has been laid upon this process, not only by Paracelsians, but other writers, some of whom seem not to have perused it themselves, I shall give you the intire passage in the author's own words, as I lately found them in the sixth book of his *Archidoxis*,

Archidoxis, an extract whereof I have yet about me; and it founds thus: *De vino sciendum est, fæcem phlegmaque ejus esse mineram, & vini substantiam esse corpus, in quo conservatur essentia, prout auri in auro latet essentia. Juxta quod practicam nobis ad memoriam ponimus, ut non obliviscamur, ad hunc modum: Recipe Vinum vetustissimum & optimum, quod habere poteris, calore saporeque ad placitum; hoc in vas vitreum infundas, ut tertiam ejus partem impleat, & sigillo Hermetis occlusum in equino ventre mensibus quatuor, & in continuato calore teneatur, qui non deficiat. Quo peracto, hyeme, cum frigus & gelu maxime sæviunt, his permensem exponatur, ut congeletur. Ad hunc modum frigus vini spiritum una cum ejus substantia protrudit in vini centrum, ac separat a phlegmate: congelatum abjice; quod vero congelatum non est, id spiritum cum substantia esse judicato. Hunc in pelicanum positum in arenæ digestione non adeo calida per aliquod tempus manere finito; postmodum eximito vini magisterium, de quo locuti sumus.*

BUT I dare not, *Eleutherius*, lay much weight upon this process, because I have have found, that, if it were true, it would be but seldom practicable in this country upon the best wine: for though this present winter hath been extraordinary cold, yet in very keen frosts, accompanied with lasting snows, I have not been able in any measure to freeze a thin phial full of sack; and even with snow and salt I could freeze little more than the surface of it; and I suppose, *Eleutherius*, that it is not every degree of cold that is capable of congealing liquors, which is able to make such an analysis (if I may so call it) of them by separating their aqueous and spirituous parts: for I have sometimes, though not often, frozen severally red-wine, urine and milk, but could not observe the expected separation. And the Dutchmen, that were forced to winter in that icy region near the arctick circle, called *Nova Zembla*, although they relate, as we shall see below, that there was a separation of parts made in their frozen beer about the middle of *November*, yet of the freezing of their sack in *December* following they give but this account: *Yea and our sack, which is so hot, was frozen very hard, so that when we were every man to have his part, we were forced to melt it in the fire; which we shared every second day, about half a pint for a man, wherewith we were forced to sustain ourselves.* In which words they imply not, that their sack was divided by the frost into differing substances, after such manner as their beer had been. All which notwithstanding, *Eleutherius*, suppose that it may be made to appear, that even cold sometimes may *congregare homogenea, & heterogenea segregare*; and to manifest this, I may tell you, that I did once, purposely, cause to be decocted in fair water a plant abounding with sulphureous and spirituous parts; and having exposed the decoction to a keen North-wind in a very frosty night, I observed, that the more aqueous parts of it were turned by the next morning into ice, towards the innermost part of which, the more agile and spirituous parts, as I then conjectured, having retreated, to shun, as much as might be, their invironing enemy, they had there preserved themselves unfrozen in the form of a high coloured liquor; the aqueous and spirituous parts having been so slightly (blended rather than) united in the decoction, that they were easily separable by such a degree of cold, as would not have been able to have divorced the parts of urine or wine, which by fermentation or digestion are wont, as trial has informed me, to be more intimately associated each with other. But I have already intimated, *Eleutherius*, that I shall not insist on this experiment, not only because, having made it but once, I may possibly have been mistaken in it; but also (and that principally) because of that much more full and eminent experiment of the separative virtue of extreme cold, that was made against their wills, by the formentioned Dutchmen that wintered in *Nova Zembla*; the relation of whose voyage being a very scarce book, it will not

be amiſs to give you that memorable part of it, which concerns our preſent theme, as I cauſed the paſſage to be extracted out of the Engliſhed voyage itſelf.

'GERARD DE VEER, *John Cornelyſon* and others, ſent out of *Amſterdam*, *Anno* 'Dom. 1596, being forced by unſeaſonable weather to winter in *Nova Zembla*, near 'Ice haven; on the thirteenth of *October*, three of us (ſays the relation) went aboard 'the ſhip, and laded a ſled with beer; but when we had laden it, thinking to go to 'our houſe with it, ſuddenly there aroſe ſuch a wind, and ſo great a ſtorm and cold, 'that we were forced to go into the ſhip again, becauſe we were not able to ſtay 'without; and we could not get the beer into the ſhip again, but were forced to let 'it ſtand without upon the ſled. The fourteenth, as we came out of the ſhip, we 'found the barrel of beer ſtanding upon the ſled, but it was faſt frozen at the heads; 'yet by reaſon of the great cold, the beer, that purged out, froze as hard upon the 'ſide of the barrel, as if it had been glued thereon: and in that ſort we drew it to 'our houſe, and ſet the barrel an end, and drank it up; but firſt we were forced 'to melt the beer, for there was ſcarce any unfrozen beer in the barrel: but in that 'thick yeaſt, that was unfrozen, lay the ſtrength of the beer, ſo that it was too 'ſtrong to drink alone, and, that which was frozen taſted like water; and being 'melted, we mixed one with the other, and ſo drank it; but it had neither ſtrength 'nor taſte.'

AND on this occaſion I remember, that having, the laſt very ſharp winter, purpoſely tried to freeze, among other liquors, ſome beer moderately ſtrong, in glaſs veſſels, with ſnow and ſalt, I obſerved, that there came out of the neck a certain thick ſubſtance, which, it ſeems, was much better able than the reſt of the liquor (that I found turned into ice) to reſiſt a froſt; and which, by its colour and conſiſtence, ſeemed manifeſtly enough to be yeaſt; whereat, I confeſs, I ſomewhat marvelled, becauſe I did not either diſcern by the taſte, or find by enquiry, that the beer was at all too new, to be very fit to be drank. I might confirm the Dutchmen's relation, by what happened a while ſince to a near friend of mine, who complained to me, that having brewed ſome beer or ale for his own drinking in *Holland* (where he then dwelt) the keenneſs of the late bitter winter froze the drink, ſo as to reduce it into ice, and a ſmall proportion of a very ſtrong and ſpirituous liquor. But I muſt not entertain you any longer concerning cold, not only becauſe you may think I have but loſt my way into a theme, which does not directly belong to my preſent undertaking; but becauſe I have already enlarged myſelf too much, upon the firſt conſideration I propoſed, though it appears ſo much a paradox, that it ſeemed to require, that I ſhould ſay much to keep it from being thought a meer extravagance: yet ſince I undertook but to make the common aſſumption of our Chymiſts and Ariſtotelians appear queſtionable, I hope I have ſo performed that taſk, that I may now proceed to my following conſiderations, and inſiſt leſs on them than I have done on the firſt.

THE

THE SCEPTICAL CHYMIST:

PART II.

THE ſecond conſideration I deſire to have notice taken of, is this; that it is not ſo ſure, as both Chymiſts and Ariſtotelians are wont to think it, that every ſeemingly ſimilar or diſtinct ſubſtance, that is ſeparated from a body by the help of the fire, was pre exiſtent in it, as a principle or element of it.

THAT I may not make this paradox a greater than needs muſt, I will firſt briefly explain what the propoſition means, before I proceed to argue for it.

AND, I ſuppoſe, you will eaſily believe, that I do not mean, that any thing is ſeparable from a body by fire, that was not materially pre-exiſtent in it: for it far exceeds the power of meerly natural agents, and conſequently of the fire, to produce anew ſo much as one atom of matter, which they can but modify and alter, not create; which is ſo obvious a truth, that almoſt all ſects of philoſophers have denied the power of producing matter to ſecond cauſes, and the Epicureans and ſome others have done the like, in reference to their gods themſelves.

NOR does the propoſition peremptorily deny, but that ſome things obtained by the fire from a mixed body may have been more than barely materially pre-exiſtent in it; ſince there are concretes, which before they be expoſed to the fire, afford us ſeveral documents of their abounding, ſome with ſalt, and others with ſulphur. For it will ſerve the preſent turn, if it appear, that divers things obtained from a mixed body expoſed to the fire were not its ingredients before; for, if this be made to appear, it will be rational enough to ſuſpect, that chymiſts may deceive themſelves, and others, in concluding reſolutely and univerſally, thoſe ſubſtances to be the elementary ingredients of bodies barely ſeparated by the fire, of which it yet may be doubted, whether there be ſuch or no; at leaſt till ſome other argument, than that drawn from the analyſis, be brought to reſolve the doubt.

THAT then, which I mean by the propoſition I am explaining, is, that it may without abſurdity be doubted, whether or no the differing ſubſtances obtainable from a concrete diſſipated by the fire were ſo exiſtent in it, in that form (at leaſt, as to their minute parts) wherein we find them, when the analyſis is over, that the fire did only disjoin and extricate the corpuſcles of one principle from thoſe of the other, wherewith before they were blended.

HAVING thus explained my propoſition, I ſhall endeavour to do two things, to prove it; the firſt of which is to ſhew, that ſuch ſubſtances, as chymiſts call principles, may be produced *de novo* (as they ſpeak.) And the other is to make it probable, that by the fire we may actually obtain from ſome mixed bodies ſuch ſubſtances, as were not, in the newly-expounded ſenſe, pre-exiſtent in them.

To begin then with the firſt of theſe, I conſider, that if it be as true, as it is probable, that compounded bodies differ from one another but in the various textures

tures resulting from the bigness, shape, motion, and contrivance of their small parts, it will not be irrational to conceive, that one and the same parcel of the universal matter may, by various alterations and contextures, be brought to deserve the name, sometimes of sulphureous, and sometimes of a terrene, or aqueous body. And this I could more largely explicate, but that our friend Mr. *Boyle* has promised us something about qualities, wherein the theme I now willingly resign him, will, I question not, be studiously inquired into. Wherefore, what I shall now advance in favour of what I have lately delivered, shall be deduced from experiments made divers years since. The first of which would have been much more considerable, but that by some intervening accidents I was necessitated to lose the best time of the year, for a trial of the nature of that I designed; it being about the middle of *May*, before I was able to begin an experiment, which should have then been two months old; but such as it was, it will not, perhaps, be impertinent to give you this narrative of it. At the time newly mentioned, I caused my gardener (being by urgent occasions hindered from being present myself) to dig out a convenient quantity of good earth, and dry it well in an oven, to weigh it, to put in an earthen pot almost level with the surface of the ground, and to set in it a selected seed, he had before received from me, for that purpose, of squash, which is an Indian kind of pompion, that grows apace; this seed I ordered him to water only with rain or spring water. I did not (when my occasions permitted me to visit it) without delight behold how fast it grew, though unseasonably sown; but the hastening winter hindered it from attaining any thing near its due and wonted magnitude, (for I found the same autumn, in my garden, some of those plants, by measure, as big about as my middle) and made me order the having it taken up; which about the middle of *October* was carefully done by the same gardener, who a while after sent me this account of it: *I have weighed the pompion with the stalk and leaves, all which weighed three pound wanting a quarter; then I took the earth, baked it as formerly, and found it just as much as I did at first, which made me think, I had not dried it sufficiently. Then I put it into the oven twice more, after the bread was drawn, and weighed it the second time, but found it shrink little or nothing.*

BUT to deal candidly with you, *Eleutherius*, I must not conceal from you the event of another experiment of this kind made this present summer, wherein the earth seems to have been much more wasted; as may appear by the following account, lately sent me by the same gardener, in these words: *To give you an account of your cucumbers, I have gained two indifferent fair ones, the weight of them is ten pound and a half, the branches with the roots weighed four pounds wanting two ounces; and when I had weighed them, I took the earth, and baked it in several small earthen dishes in an oven; and when I had so done, I found the earth wanted a pound and a half of what it was formerly; yet I was not satisfied, doubting the earth was not dry: I put it into an oven the second time, (after the bread was drawn) and after I had taken it out and weighed it, I found it to be the same weight. So I suppose there was no moisture left in the earth. Neither do I think, that the pound and half, that was wanting, was drawn away by the cucumber, but a great part of it, in the ordering, was in dust (and the like) wasted: (the cucumbers are kept by themselves, lest you should send for them.)* But yet in this trial, *Eleutherius*, it appears, that though some of the earth, or rather the dissoluble salt harboured in it, were wasted, the main body of the plant consisted of transmuted water. And I might add, that a year after I caused the formerly mentioned experiment, touching large pompions, to be reiterated, with so good success, that if my memory does not much mis-inform me, it did not only much surpass any, that I made

I made before, but seemed strangely to conclude what I am pleading for; though (by reason, I have unhappily lost the particular account my gardener writ me up of the circumstances) I dare not insist upon them. The like experiment may be as conveniently tried with the seeds of any plant, whose growth is hasty, and its size bulky. If tobacco will in these cold climates grow well in earth undunged, it would not be amiss to make a trial with it; for it is an annual plant, that arises where it prospers, sometimes as high as a tall man, and I have had leaves of it in my garden, near a foot and a half broad. But the next time I try this experiment, it shall be with several seeds of the same sort, in the same pot of earth, that so the event may be the more conspicuous. But, because every body has not conveniency of time and place for this experiment neither, I made, in my chamber, some shorter and more expeditious trials. I took a top of spearment, about an inch long, and put it into a good phial full of spring water, so as the upper part of the mint was above the neck of the glass, and the lower part immersed in the water; within a few days this mint began to shoot forth roots into the water, and to display its leaves, and aspire upwards; and in a short time it had numerous roots and leaves, and these very strong and fragrant of the odour of the mint; but the heat of my chamber, as I suppose, killed the plant, when it was grown to have a pretty thick stalk, which with the various and ramified roots, which it shot into the water as if it had been earth, presented in its transparent flower-pot a spectacle not unpleasant to behold. The like I tried with sweet-marjoram; and I found the experiment succeeded also, though somewhat more slowly, with balm and peniroyal, to name now no other plants. And one of these vegetables, cherished only by water, having obtained a competent growth, I did, for trial's sake, cause to be distilled in a small retort, and thereby obtained some phlegm, a little empyreumatical spirit, a small quantity of adust oil, and a *caput mortuum*; which appearing to be a coal, I concluded it to consist of salt and earth: but the quantity of it was so small, that I forbore to calcine it. The water I used to nourish this plant was not shifted nor renewed; and I chose spring-water rather than rain-water, because the latter is more discernably a kind of *ωαιστιφυία*, which, though it be granted to be freed from grosser mixtures, seems yet to contain in it, besides the steams of several bodies wandering in the air, which may be supposed to impregnate it, a certain spirituous substance, which may be extracted out of it, and is by some mistaken for the spirit of the world corporified, upon what grounds, and with what probability, I may elsewhere, perchance, but must not now, discourse to you.

BUT perhaps I might have saved a great part of my labour. For I find, that *Helmont* (an author more considerable for his experiments, than many learned men are pleased to think him) having had an opportunity to prosecute an experiment much of the same nature with those I have been now speaking of, for five years together, obtained at the end of that time so notable a quantity of transmuted water, that I should scarce think it fit to have his experiment, and mine mentioned together, were it not, that the length of time requisite to this may deter the curiosity of some, and exceed the leisure of others; and partly, that so paradoxical a truth, as that, which these experiments seem to hold forth, needs to be confirmed by more witnesses than one, especially since the extravagancies and untruths, to be met with in *Helmont*'s treatise of the magnetic cure of wounds, have made his testimonies suspected in his other writings; though as to some of the unlikely matters of fact he delivers in them, I might safely undertake to be his compurgator. But that experiment of his, which I was mentioning to you, he says, was this: he took 200 pound of earth dried in an oven, and having put it into an earthen vessel, and moistened it with rain-water, he

he planted in it the trunk of a willow-tree of five pound weight; this he watered, as need required, with rain or diſtilled water; and to keep the neighbouring earth from getting into the veſſel, he employed a plate of iron tinned over and perforated with many holes. Five years being effluxed, he took out the tree, and weighed it, and (with computing the leaves that fell during four autumns) he found it to weigh 169 pound, and about three ounces. And having again dryed the earth it grew in, he found it to want of its former weight of 200 pound, about a couple only of ounces; ſo that 164 pound of the roots, wood, and bark, which conſtituted the tree, ſeem to have ſprung from the water. And though it appears not, that *Helmont* had the curioſity to make any analyſis of this plant, yet what I lately told you I did to one of the vegetables I nouriſhed with water only, will I ſuppoſe keep you from doubting, that if he had diſtilled this tree, it would have afforded him the like diſtinct ſubſtances as another vegetable of the ſame kind. I need not ſubjoin, that I had it alſo in my thoughts to try, how experiments, to the ſame purpoſe with thoſe I related to you, would ſucceed in other bodies than vegetables, becauſe importunate avocations having hitherto hindered me from putting my deſign in practice, I can yet ſpeak but conjecturally of the ſucceſs: but the beſt is, that the experiments already made and mentioned to you need not the aſſiſtance of new ones, to verify as much, as my preſent taſk makes it concern me to prove by experiments of this nature.

ONE would ſuſpect (ſays *Eleutherius* after his long ſilence) by what you have been diſcourſing, that you are not far from *Helmont*'s opinion about the origination of compound bodies, and perhaps too diſlike not the arguments, which he employs to prove it.

WHAT Helmontian opinion, and what arguments do you mean? (aſks *Carneades.*)

WHAT you have been newly diſcourſing (replies *Eleutherius*) tells us, that you cannot but know, that this bold and acute Spagyriſt ſcruples not to aſſert, that all mixt bodies ſpring from one element; and that vegetables, animals, marchaſites, ſtones, metals, *&c.* are materially but ſimple water diſguiſed into theſe various forms, by the plaſtick or formative vertue of their ſeeds. And as for his reaſons, you may find divers of them ſcattered up and down his writings; the conſiderableſt of which ſeem to be theſe three; the ultimate reduction of mixt bodies into inſipid water, the viciſſitude of the ſuppoſed elements, and the production of perfectly mixt bodies out of ſimple water. And firſt he affirms, that the *ſal circulatus Paracelſi*, or his liquor alkaheſt, does adequately reſolve plants, animals, and minerals into one liquor or more, according to their ſeveral internal diſparities of parts, (without *caput mortuum*, or the deſtruction of their ſeminal vertues;) and that the alkaheſt being abſtracted from theſe liquors in the ſame weight and virtue wherewith it diſſolved them, the liquors may by frequent cohobations from chalk or ſome other idoneous matter, be totally deprived of their ſeminal endowments, and return at laſt to their firſt matter, inſipid water: ſome other ways he propoſes here and there to diveſt ſome particular bodies of their borrowed ſhapes, and make them remigrate to their firſt ſimplicity. The ſecond topick, whence *Helmont* draws his arguments, to prove water to be the material cauſe of mixt bodies, I told you was this, that the other ſuppoſed elements may be tranſmuted into one another. But the experiments by him here and there produced on this occaſion are ſo uneaſy to be made and to be judged of, that I ſhall not inſiſt on them; not to mention, that if they were granted to be true, his inference from them is ſomewhat diſputable. And therefore I ſhall paſs on to tell you, that as, in his firſt argument, our paradoxical author endeavours to prove water the ſole

ſole element of mixt bodies, by their ultimate reſolution, when by his alkaheſt, or ſome other conquering agent, the ſeeds have been deſtroyed, which diſguiſed them; or when by time thoſe ſeeds are wearied, or exantlated, or unable to act their parts upon the ſtage of the univerſe any longer; ſo in his third argument he endeavours to evince the ſame concluſion, by the conſtitution of bodies, which he aſſerts to be nothing but water ſubdued by ſeveral virtues. Of this he gives here and there in his writings ſeveral inſtances, as to plants and animals; but divers of them being difficult either to be tried or to be underſtood, and others of them being not altogether unobnoxious to exceptions, I think you have ſingled out the principal and leſs queſtionable experiment, when you lately mentioned that of the willow-tree. And having thus, continues *Eleutherius*, to anſwer your queſtion, given you a ſummary account of what, I am confident, you know better than I do, I ſhall be very glad to receive your ſenſe of it, if the giving it me will not too much divert you from the proſecution of your diſcourſe.

THAT *if* (replies *Carneades*) was not needleſsly annexed: for thoroughly to examine ſuch an hypotheſis and ſuch arguments, would require ſo many conſiderations, and conſequently ſo much time, that I ſhould not now have the leiſure to perfect ſuch a digreſſion, and much leſs to finiſh my principal diſcourſe. Yet thus much I ſhall tell you at preſent, that you need not fear my rejecting this opinion for its novelty; ſince, however the Helmontians may, in compliment to their maſter, pretend it to be a new diſcovery, yet though the arguments be for the moſt part his, the opinion itſelf is very antient: for *Diogenes Laertius* and divers other authors ſpeak of *Thales*, as the firſt among the *Grecians*, that made diſquiſitions upon nature. And of this *Thales*, I remember, *Tully* informs us, that he taught all things were at firſt made of water. And it ſeems by *Plutarch* and *Juſtin Martyr*, that the opinion was ancienter than he: for they tell us, that he uſed to defend his tenet by the teſtimony of *Homer*. And a Greek author, the ſcholiaſt of *Apollonius*, upon theſe words, *De natura Deorum.*

> Ἐξ ἰλύος ἐβλάστησε χθὼν αὕτη.
> *The earth of ſlime was made,* Argonaut. 4.

affirms (out of *Zeno*) that the chaos, whereof all things were made, was, according to *Heſiod*, water; which, ſettling firſt, became ſlime, and then condenſed into ſolid earth. And the ſame opinion about the generation of ſlime ſeems to have been entertained by *Orpheus*, out of whom one of the ancients cites this teſtimony, Athenagoras.

> Ἐκ τοῦ ὕδατος ἰλὺς κατέστη.
> *Of water ſlime was made.*

It ſeems alſo by what is delivered in * *Strabo* out of another author concerning the Indians, that they likewiſe held, that all things had differing beginnings, but that, of which the world was made, was water. And the like opinion has been by ſome of the ancients aſcribed to the Phenicians, from whom *Thales* himſelf is conceived to have borrowed it; as probably the Greeks did much of theology, and, as I am apt to think, of their philoſophy too; ſince the deviſing of the atomical hypotheſis commonly aſcribed to *Leucippus* and his diſciple *Democritus* is by learned men attri-

* *Univerſarum rerum primordia diverſa eſſe, faciendi autem mundi initium aquam.* Strabo Geograph. Lib. 15. *circa medium.*

buted to one *Moschus* a Phenician. And possibly the opinion is yet ancienter than so; for it is known, that the Phenicians borrowed most of their learning from the Hebrews. And among those, that acknowledge the books of *Moses*, many have been inclined to think water to have been the primitive and universal matter, by perusing the beginning of *Genesis*, where the waters seem to be mentioned as the material cause, not only of sublunary compound bodies, but of all those, that make up the universe; whose component parts did orderly, as it were, emerge out of that vast abyss, by the operation of the spirit of God, who is said to have been moving himself, as hatching females do, (as the original מרחפת, *Merahephet*, is said to import, and it seems to signify, in one of the two other places, wherein alone I have met with it in the Hebrew bible) upon the face of the waters; which being, as may be supposed, divinely impregnated with the seeds of all things, were by that productive incubation qualified to produce them. But you, I presume, expect, that I should discourse of this matter like a naturalist, not a philologer. Wherefore I shall add, to countenance *Helmont*'s opinion, that whereas he gives not, that I remember, any instance of any mineral body, nor scarce of any animal, generated of water, a French chymist, Monsieur *de Rochas*, has presented his readers an experiment, which if it were punctually such as he has delivered it, is very notable. He then discoursing of the generation of things according to certain chymical and metaphorical notions (which I confess are not to me intelligible) sets down, among divers speculations not pertinent to our subject, the following narrative, which I shall repeat to you the sense of in English, with as little variation from the literal sense of the French words, as my memory will enable me. *Having* (says he) *discerned such great wonders by the natural operation of water, I would know what may be done with it by art imitating nature. Wherefore I took water, which I well knew not to be compounded, nor to be mixed with any other thing than that spirit of life* (whereof he had spoken before) *and with a heat artificial, continual and proportionate, I prepared and disposed it by the above mentioned graduations of coagulation, congelation, and fixation, until it was turned into earth, which earth produced animals, vegetables and minerals. I tell not what animals, vegetables and minerals, for that is reserved for another occasion: but the animals did move of themselves, eat,* &c.—*and by the true anatomy I made of them, I found, that they were composed of much sulphur, little mercury, and less salt.—The minerals began to grow and increase by converting into their own nature one part of the earth thereunto disposed; they were solid and heavy. And by this truly demonstrative science, namely chymistry, I found, that they were composed of much salt, little sulphur, and less mercury.*

Deuter. xxii. 11. Jerem. xxiii. 9.

BUT (says *Carneades*) I have some suspicions concerning this strange relation, which make me unwilling to declare an opinion of it, unless I were satisfied concerning divers material circumstances, that our author has left unmentioned; though, as for the generation of living creatures, both vegetable and sensitive, it needs not seem incredible, since we find, that our common water (which indeed is often impregnated with variety of seminal principles and rudiments) being long kept in a quiet place, will putrify and stink, and then perhaps too, produce moss and little worms, or other insects, according to the nature of the seeds that were lurking in it. I must likewise desire you to take notice, that as *Helmont* gives us no instance of the production of minerals out of water, so the main argument, that he employs to prove, that they and other bodies may be resolved into water, is drawn from the operations of his alkahest, and consequently cannot be satisfactorily examined by you and me.

YET certainly (says *Eleutherius*) you cannot but have somewhat wondered as well as I, to observe, how great a share of water goes to the making up of divers bodies, whose

whose disguises promise nothing near so much. The distillation of eels, though it yielded me some oil, and spirit, and volatile salt, besides the *caput mortuum*, yet were all these so disproportionate to the phlegm, that came from them, (and in which at first they boiled as in a pot of water) that they seemed to have been nothing but coagulated phlegm; which does likewise strangely abound in vipers, though they are esteemed very hot in operation, and will in a convenient air survive some days the loss of their heads and hearts, so vigorous is their vivacity. Man's blood itself, as spirituous and as elaborate a liquor as it is reputed, does so abound in phlegm, that, the other day, distilling some of it on purpose to try the experiment (as I had formerly done in deer's blood) out of about seven ounces and a half of pure blood we drew near six ounces of phlegm, before any of the more operative principles began to arise and invite us to change the receiver. And to satisfy myself, that some of these animal phlegms were void enough of spirit to deserve that name, I would not content myself to taste them only, but fruitlessly poured on them acid liquors, to try if they contained any volatile salt or spirit, which (had there been any there) would probably have discovered itself by making an ebullition with the affused liquor. And now I mention corrosive spirits, I am minded to inform you, that though they seem to be nothing else but fluid salts, yet they abound in water, as you may observe, if either you entangle, and so fix their saline part, by making them corrode some idoneous body, or else if you mortify it with a contrary salt; as I have very manifestly observed in the making a medicine somewhat like *Helmont*'s Balsamus Samech, with distilled vinegar instead of spirit of wine, wherewith he prepares it. For you would scarce believe (what I have lately observed) that of that acid spirit, the salt of tartar, from which it is distilled, will, by mortifying and retaining the acid salt, turn into worthless phlegm near twenty times its weight; before it be so fully impregnated as to rob no more distilled vinegar of its salt. And though spirit of wine exquisitely rectified, seem of all liquors to be the most free from water, it being so igneous, that it will flame all away without leaving the least drop behind it, yet even this fiery liquor is by *Helmont* not improbably affirmed, in case what he relates be true, to be materially water, under a sulphurous disguise: for according to him, in the making that excellent medicine, *Paracelsus* his Balsamus Samech, (which is nothing but sal tartari dulcified, by distilling from it spirit of wine till the salt be sufficiently glutted with its sulphur, and till it suffer the liquor to be drawn off, as strong as it was poured on) when the salt of tartar from which it is distilled hath retained, or deprived it of the sulphurous parts of the spirit of wine, the rest, which is incomparably the greater part of the liquor, will remigrate into phlegm. I added that clause [*In case what he relates be true*] because I have not as yet sufficiently tried it myself. But not only something of experiment keeps me from thinking it, as many chymists do, absurd, (though I have, as well as they, in vain tried it with ordinary salt of tartar) but besides that *Helmont* often relates it, and draws consequences from it; a person noted for his soberness and skill in spagyrical preparations, having been asked by me, whether the experiment might not be made to succeed, if the salt and spirit were prepared, according to a way suitable to my principles, he affirmed to me, that he had that way, I proposed, made *Helmont*'s experiment succeed very well, without adding any thing to the salt and spirit. But our way is neither short nor easy.

I HAVE indeed (says *Carneades*) sometimes wondered to see, how much phlegm may be obtained from bodies by the fire. But concerning that phlegm I may anon have occasion to note something, which I therefore shall not now anticipate. But to return to the opinion of *Thales*, and of *Helmont*, I consider, that supposing the alkahest could

reduce all bodies into water, yet whether that water, because insipid, must be elementary, may not groundlessly be doubted: for I remember the candid and eloquent *Petrus Laurembergius*, in his notes upon *Sala*'s aphorisms, affirms, that he saw an insipid menstruum, that was a powerful dissolvent, and (if my memory does not much mis-inform me) could dissolve gold. And the water, which may be drawn from quicksilver without addition, though it be almost tasteless, you will, I believe, think of a differing nature from simple water, especially if you digest in it appropriated minerals. To which I shall add but this, that this consideration may be further extended. For I see no necessity to conceive, that the water mentioned in the beginning of *Genesis*, as the universal matter, was simple and elementary water; since though we should suppose it to have been an agitated congeries or heap consisting of a great variety of seminal principles and rudiments, and of other corpuscles fit to be subdued and fashioned by them, it might yet be a body fluid like water, in case the corpuscles, it was made up of, were by their Creator made small enough, and put into such an actual motion, as might make them glide along one another. And as we now say, the sea consists of water (notwithstanding the saline, terrestrial, and other bodies mingled with it) such a liquor may well enough be called water, because that was the greatest of the known bodies, whereunto it was like; though, that a body may be fluid enough to appear a liquor, and yet contain corpuscles of a very differing nature, you will easily believe, if you but expose a good quantity of vitriol in a strong vessel to a competent fire. For although it contains both aqueous, earthy, saline, sulphureous, and metalline corpuscles, yet the whole mass will at first be fluid like water, and boil like a seething-pot.

I MIGHT easily (continues *Carneades*) enlarge myself on such considerations, if I were now obliged to give you my judgment of the Thalesian and Helmontian hypothesis. But, whether or no we conclude, that all things were at first generated of water, I may deduce from what I have tried concerning the growth of vegetables, nourished with water, all that I now proposed to myself or need at present to prove; namely, that salt, spirit, earth, and even oil (though that be thought of all bodies the most opposite to water) may be produced out of water; and consequently, that a Chymical principle, as well as a Peripatetic element, may (in some cases) be generated anew, or obtained from such a parcel of matter, as was not endowed with the form of such a principle or element before.

AND having thus, *Eleutherius*, evinced, that it is possible, that such substances as those, that chymists are wont to call their *tria prima*, may be generated anew; I must next endeavour to make it probable, that the operation of the fire does actually (sometimes) not only divide compounded bodies into small parts, but compound those parts after a new manner: whence consequently, for aught we know, there may emerge as well saline and sulphureous substances, as bodies of other textures. And, perhaps, it will assist us in our inquiry after the effects of the operations of the fire upon other bodies, to consider a little, what it does to those mixtures, which being productions of the art of man, we best know the composition of. You may then be pleased to take notice, that though soap is made up by the soap-boilers of oil or grease, and salt, and water diligently incorporated together; yet if you expose the mass they constitute to a gradual fire in a retort, you shall then indeed make a separation, but not of the same substances, that were united into soap, but of others of a distant and yet not an elementary nature, and especially of an oil very sharp and fœtid, and of a very differing quality from that, which was imployed to make the soap. So, if you mingle in a due proportion sal armoniac with quick-lime, and distil them

them by degrees of fire, you shall not divide the sal armoniac from the quick-lime, though the one be a volatile, and the other a fixed substance, but that, which will ascend, will be a spirit much more fugitive, penetrant, and stinking, than sal armoniac; and there will remain with the quick-lime all, or very near all the sea-salt, that concurred to make up the sal armoniac; concerning which sea-salt I shall, to satisfy you how well it was united to the lime, inform you, that I have, by making the fire at length very vehement, caused both the ingredients to melt in the retort itself into one mass, and such masses are apt to relent in the moist air. If it be here objected, that these instances are taken from factitious concretes, which are more compounded than those, which nature produces; I shall reply, that besides that I have mentioned them as much to illustrate what I proposed, as to prove it; it will be difficult to evince, that nature herself does not make decompounded bodies; I mean, mingle together such mixt bodies, as are already compounded of elementary, or rather of more simple ones. For vitriol (for instance) though I have sometimes taken it out of mineral earths, where nature had without any assistance of art prepared it to my hand, is really, though chymists are pleased to reckon it among salts, a decompounded body, consisting (as I shall have occasion to declare anon) of a terrestrial substance, of a metal, and also of at least one saline body, of a peculiar, and not elementary nature. And we see also in animals, that their blood may be composed of divers very differing mixt bodies, since we find it observed, that divers sea-fowl taste rank of the fish, on which they ordinarily feed; and *Hippocrates* himself observes, that a child may be purged by the milk of the nurse, if she have taken elaterium; which argues, that the purging corpuscles of the medicament concur to make up the milk of the nurse; and that white liquor is generally by physicians supposed to be but blanched and altered blood. And I remember I have observed, not far from the *Alps*, that at a certain time of the year the butter of that country was very offensive to strangers, by reason of the rank taste of a certain herb, whereon the cows were then wont plentifully to feed. But (proceeds *Carneades*) to give you instances of another kind, to shew that things may be obtained by the fire from a mixt body, that were not pre-existent in it, let me remind you, that from many vegetables there may without any addition be obtained glass; a body, which I presume you will not say was pre-existent in it, but produced by the fire. To which I shall add but this one example more, namely, that by a certain artificial way of handling quicksilver, you may without addition separate from it at least a fifth, or fourth part of clear liquor; which, with an ordinary Peripatetic, would pass for water, and which a vulgar chymist would not scruple to call phlegm, and which, for aught I have yet seen or heard, is not reducible into mercury again, and consequently is more than a disguise of it. Now, besides that divers chymists will not allow mercury to have any, or at least any considerable quantity of either of the ignoble ingredients, earth and water; besides this, I say, the great ponderousness of quicksilver makes it very unlikely, that it can have so much water in it, as may be thus obtained from it, since mercury weighs twelve or fourteen times as much as water of the same bulk. Nay, for a further confirmation of this argument, I will add this strange relation, that two friends of mine, the one a physician, and the other a mathematician, and both of them persons of unsuspected credit, have solemnly assured me, that after many trials, they made to reduce mercury into water, in order to a philosophical work, upon gold (which yet, by the way, I now proved unsuccessful) they did once, by divers cohobations reduce a pound of quicksilver into almost a pound of water, and this, without the addition of any other substance, but only by pressing the mercury by a skilfully managed

naged fire, in purposely contrived vessels. But of these experiments, our friend, (says *Carneades*, pointing at the Register of this dialogue) will, perhaps, give you a more particular account, than it is necessary for me to do; since what I have now said, may sufficiently evince, that the fire may sometimes as well alter bodies as divide them, and by it we may obtain from a mixed body, what was not pre-existent in it. And how are we sure, that in no other body, what we call phlegm is barely separated, not produced by the action of the fire? since so many other mixed bodies are of a much less constant, and more alterable nature, than mercury (by many tricks it is wont to put upon chymists, and, by the experiments I told you of, about an hour since) appears to be. But, because I shall ere long have occasion to resume into consideration the power of the fire to produce new concretes, I shall no longer insist on this argument at present: only I must mind you, that if you will not disbelieve *Helmont*'s relations, you must confess, that the *tria prima* are neither ingenerable, nor incorruptible substances; since by his alkahest some of them may be produced of bodies, that were before of another denomination; and by the same powerful menstruum all of them may be reduced into insipid water.

HERE *Carneades* was about to pass on to his third consideration, when *Eleutherius* being desirous to hear what he could say, to clear his second general consideration from being repugnant to what he seemed to think the true theory of mistion, prevented him, by telling him, I somewhat wonder, *Carneades*, that you, who are in so many points unsatisfied with the Peripatetic opinion touching the elements and mixed bodies, should also seem averse to that notion touching the manner of mistion, wherein the chymists (though perhaps without knowing, that they do so) agree with most of the antient philosophers, that preceded *Aristotle*; and that for reasons so considerable, that divers modern naturalists and physicians, in other things unfavourable enough to the Spagyrists, do in this case side with them against the common opinion of the schools. If you should ask me (continues *Eleutherius*) what reasons I mean? I should partly by the writings of *Sennertus* and other learned men, and partly by my own thoughts, be supplied with more, than it were at present proper for me to insist largely on. And therefore, I shall mention only, and that briefly, three or four. Of these, I shall take the first, from the state of the controversy itself, and the genuine notion of mistion, which, though much intricated by the schoolmen, I take in short to be this: *Aristotle*, at least, as many of his interpreters expound him, and as indeed he teaches in some places, where he professedly dissents from the antients, declares mistion to be such a mutual penetration, and perfect union of the mingled elements, that there is no portion of the mixed body, how minute soever, which does not contain all, and every of the four elements, or in which, if you please, all the elements are not. And I remember, that he reprehends the mistion taught by the antients, as too slight or gross, for this reason, that bodies mixed according to their hypothesis, though they appear to human eyes, would not appear such to the acute eyes of a lynx, whose perfecter sight would discern the elements, if they were no otherwise mingled, than as his predecessors would have it, to be but blended, not united; whereas the antients, though they did not all agree about what kind of bodies were mixed, yet they did almost unanimously hold, that in a compounded body, though the *Miscibilia*, whether elements, principles, or whatever they pleased to call them, were associated in such small parts, and with so much exactness, that there was no sensible part of the mass, but seemed to be of the same nature with the rest, and with the whole; yet as to the atoms, or other insensible parcels of matter, whereof each of the *Miscibilia* consisted, they retained each of them its own nature, being but by

by apposition or juxta-position united with the rest into one body. So that although by virtue of this composition the mixed body did, perhaps, obtain divers new qualities; yet still the ingredients, that compounded it, retaining their own nature, were by the destruction of the compositum separable from each other, the minute parts disengaged from those of a differing nature, and associated with those of their own sort, returning to be again, fire, earth, or water, as they were before they chanced to be ingredients of that compositum. This may be explained (continues *Eleutherius*) by a piece of cloth made of white and black threads interwoven, wherein though the whole piece appear neither white nor black, but of a resulting colour, that is grey, yet each of the white and black threads, that compose it, remains what it was before, as would appear, if the threads were pulled asunder, and sorted each colour by itself. This (pursues *Eleutherius*) being, as I understand it, the state of the controversy, and the Aristotelians, after their master, commonly defining, that mistion is *miscibilium alteratorum unio*, that seems to comport much better with the opinion of the Chymists, than with that of their adversaries; since, according to that, as the newly-mentioned example declares, there is but a juxta-position of separable corpuscles, retaining each its own nature; whereas, according to the Aristotelians, when what they are pleased to call a mixed body, results from the concourse of the elements, the *Miscibilia* cannot so properly be said to be altered, as destroyed, since there is no part in the mixed body, how small soever, that can be called either fire, or air, or water, or earth.

Nor indeed, can I well understand, how bodies can be mingled other ways, than as I have declared, or, at least, how they can be mingled, as our Peripatetics would have it. For whereas *Aristotle* tells us, that, if a drop of wine be put into ten thousand measures of water, the wine being overpowered by so vast a quantity of water, will be turned into it, he speaks, to my apprehension, very improbably. For, though one should add to that quantity of water as many drops of wine, as would a thousand times exceed it all, yet, by his rule, the whole liquor should not be a *crama*, a mixture of wine and water, wherein the wine would be predominant, but water only; since the wine being added but by a drop at a time, would still fall into nothing but water, and consequently would be turned into it. And, if this would hold in metals too, it were a rare secret for goldsmiths, and refiners; for by melting a mass of gold, or silver, and by but casting into it lead or antimony, grain after grain, they might at pleasure, within a reasonable compass of time, turn what quantity they desire, of the ignoble into the noble metals. And, indeed, since a pint of wine, and a pint of water, amount to about a quart of liquor, it seems manifest to sense, that these bodies do not totally penetrate one another, as one would have it; but, that each retains its own dimensions; and consequently, that they are, by being mingled, only divided into minute bodies, that do but touch one another with their surfaces, as do the grains of wheat, rye, barley, *&c.* in a heap of several sorts of corn. And unless we say, that as when one measure of wheat, for instance, is blended with a hundred measures of barley, there happens only a juxta-position and superficial contact betwixt the grains of wheat, and as many or thereabouts of the grains of barley; so, when a drop of wine is mingled with a great deal of water, there is but an apposition of so many vinous corpuscles to a correspondent number of aqueous ones: unless I say, this be said, I see not how that absurdity will be avoided, whereunto the Stoical notion of mistion (namely by γυσχυσις, or confusion) was liable, according to which the least body may be co-extended with the greatest: since in a mixt body, wherein, before the elements were mingled, there was, for instance, but one pound of water

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to ten thousand of earth, yet according to them there must not be the least part of that compound, that consisted not as well of earth, as water. But I insist, perhaps, too long (says *Eleutherius*) upon the proofs afforded me by the nature of mistion: wherefore I will but name two or three other arguments; whereof the first shall be, that, according to *Aristotle* himself, the motion of a mixt body follows the nature of the predominant element, as those, wherein the earth prevails, tend towards the centre of heavy bodies. And since many things make it evident, that in divers mixt bodies the elementary qualities are as well active, though not altogether so much so, as in the elements themselves; it seems not reasonable to deny the actual existence of the elements in those bodies, wherein they operate.

To which I shall add this convincing argument, that experience manifests, and *Aristotle* confesses it, that the *Miscibilia* may be again separated from a mixt body, as is obvious in the chymical resolutions of plants and animals, which could not be, unless they did actually retain their forms in it. For since, according to *Aristotle*, and I think according to truth, there is but one common mass of all things, which he has been pleased to call *materia prima*; and since it is not therefore the matter, but the form, that constitutes and discriminates things, to say, that the elements remain not in a mixt body, according to their forms, but according to their matter, is not to say, that they remain there at all; since although those portions of matter were earth and water, *&c.* before they concurred, yet the resulting body being once constituted, may as well be said to be simple as any of the elements; the matter being confessedly of the same nature in all bodies, and the elementary forms being, according to this hypothesis, perished and abolished.

AND lastly, and if we will consult chymical experiments, we shall find the advantages of the chymical doctrine above the Peripatetic little less than palpable. For in that operation, that refiners call quartation, which they employ to purify gold, although three parts of silver be so exquisitely mingled by fusion with a fourth part of gold (whence the operation is denominated) that the resulting mass acquires several new qualities, by virtue of the composition, and that there is scarce any sensible part of it, that is not composed of both the metals; yet if you cast this mixture into aqua fortis, the silver will be dissolved in the menstruum, and the gold like a dark or black powder will fall to the bottom of it, and either body may be again reduced into such a metal as it was before; which shews, that it retained its nature, notwithstanding its being mixt *per minima* with the other. We likewise see, that though one part of pure silver be mingled with eight or ten parts, or more, of lead; yet the fire will upon the cupel easily and perfectly separate them again. And that, which I would have you peculiarly consider on this occasion, is, that not only in chymical anatomies there is a separation made of the elementary ingredients, but that some mixt bodies afford a very much greater quantity of this or that element or principle, than of another; as we see, that turpentine and amber yield much more oil and sulphur, than they do water; whereas wine, which is confessed to be a perfectly mixt body, yields but a little inflammable spirit, or sulphur, and not much more earth; but affords a vast proportion of phlegm or water. Which could not be, if, as the Peripatetics suppose, every, even of the minutest particles, were of the same nature with the whole, and consequently did contain both earth and water, and air, and fire. Wherefore as to what *Aristotle* principally, and almost only objects, that unless his opinion be admitted, there would be no true and perfect mistion, but only aggregates or heaps of contiguous corpuscles, which, though the eye of a man cannot discern, yet the eye of a lynx might perceive not to be of the same nature with one another and with their

their totum, as the nature of mistion requires, if he do not beg the question, and make mistion to consist in what other naturalists deny to be requisite to it, yet he, at least, objects that as a great inconvenience, which I cannot take for such, till he have brought as considerable arguments as I have proposed to prove the contrary, to evince, that nature makes other mistions than such as I have allowed, wherein the *Miscibilia* are reduced into minute parts, and united as far as sense can discern: which if you will not grant to be sufficient for a true mistion, he must have the same quarrel with nature herself, as with his adversaries.

WHEREFORE (continues *Eleutherius*) I cannot but somewhat marvel, that *Carneades* should oppose the doctrine of the chymists in a particular, wherein they do as well agree with his old mistress, Nature, as dissent from his old adversary, *Aristotle*.

I MUST not (replies *Carneades*) engage myself at present to examine thoroughly the controversies concerning mistion: and if there were no third thing, but that I were reduced to embrace absolutely and unreservedly either the opinion of *Aristotle*, or that of the philosophers, that went before him, I should look upon the latter, which the chymists have adopted, as the more defensible opinion. But because differing in the opinions about the elements from both parties, I think I can take a middle course, and discourse to you of mistion, after a way, that does neither perfectly agree, nor perfectly disagree with either: as I will not peremptorily define, whether there be not cases, wherein some phænomena of mistion seem to favour the opinion, that the chymists patrons borrowed of the antients, I shall only endeavour to shew you, that there are some cases, which may keep the doubt, which makes up my second general consideration, from being unreasonable.

I SHALL then freely acknowledge to you (says *Carneades*) that I am not over-well satisfied with the doctrine, that is ascribed to *Aristotle*, concerning mistion; especially, since it teaches, that the four elements may again be separated from the mixt body; whereas, if they continued not in it, it would not be so much a separation as a production. And I think the antient philosophers, that preceded *Aristotle*, and chymists, who have since received the same opinion, do speak of this matter more intelligibly, if not more probably, than the Peripatetics: but though they speak congruously enough to their believing, that there are a certain number of primogenial bodies, by whose concourse all those we call mixt are generated, and which, in the destruction of mixt bodies, do barely part company, and reduce from one another, just such as they were when they came together; yet I, who meet with very few opinions, that I can entirely acquiesce in, must confess to you, that I am inclined to differ, not only from the Aristotelians, but from the old philosophers and the chymists, about the nature of mistion. And if you will give me leave, I shall briefly propose to you my present notion of it, provided you will look upon it, not so much as an assertion as an hypothesis; in talking of which, I do not now pretend to propose and debate the whole doctrine of mistion, but to shew, that it is not improbable, that sometimes mingled substances may be so strictly united, that it doth not by the usual operations of the fire, by which chymists are wont to suppose themselves to have made the analysis of mixt bodies, sufficiently appear, that in such bodies the *Miscibilia*, that concurred to make them up, do each of them retain its own peculiar nature; and by the Spagyrists fires may be more easily extricated and recovered, than altered; either by a change of texture in the parts of the same ingredient, or by an association with some parts of another ingredient, more strict than was that of the parts of this or that *Miscibile* among themselves. At these words, *Eleutherius* having pressed him to do what he proposed, and promised to do what he desired

I CONSIDER then (resumes *Carneades*) that, not to mention those improper kinds of mistion, wherein homogeneous bodies are joined, as when water is mingled with water, or two vessels full of the same kind of wine with one another, the mistion I am now to discourse of seems, generally speaking, to be but an union *per minima* of any two or more bodies of differing denominations; as when ashes and sand are colliquated into glass, or antimony and iron into regulus martis, or wine and water are mingled, and sugar is dissolved in the mixture. Now in this general notion of mistion it does not appear clearly comprehended, that the *Miscibilia* or ingredients do in their small parts so retain their nature, and remain distinct in the compound, that they may thence by the fire be again taken asunder: for though I deny not, that in some mistions of certain permanent bodies this recovery of the same ingredients may be made, yet I am not convinced, that it will hold in all, or even in most, or that it is necessarily deducible from chymical experiments, and the true notion of mistion. To explain this a little, I assume, that bodies may be mingled, and that very durably, that are not elementary, nor have been resolved into elements or principles, that they may be mingled; as is evident in the regulus of colliquated antimony, and iron newly mentioned; and in gold coin, which lasts so many ages; wherein generally the gold is alloyed by the mixture of a quantity, greater or lesser (in our mints they use about a 12th part) of either silver, or copper, or both. Next, I consider, that there being but one universal matter of things, as it is known that the Aristotelians themselves acknowledge, who call it *materia prima* (about which nevertheless I like not all their opinions) the portions of this matter seem to differ from one another but in certain qualities or accidents, fewer or more; upon whose account the corporeal substance they belong to receives its denomination, and is referred to this or that particular sort of bodies: so that if it come to lose, or be deprived of those qualities, though it ceases not to be a body, yet it ceases from being that kind of body as a plant, or animal, or red, green, sweet, sour, or the like. I consider, that it very often happens, that the small parts of bodies cohere together but by immediate contact and rest, and that, however, there are few bodies, whose minute parts stick so close together, to what cause soever their combination be ascribed, but that it is possible to meet with some other body, whose small parts may get between them, and so disjoin them; or may be fitted to cohere more strongly with some of them, than those some do with the rest; or at least may be combined so closely with them, as that neither the fire, nor the other usual instruments of chymical anatomies will separate them. These things being premised, I will not peremptorily deny, but that there may be some clusters of particles, wherein the particles are so minute, and the coherence so strict, or both, that when bodies of differing denominations, and consisting of such durable clusters, happen to be mingled, though the compound body made up of them may be very differing from either of the ingredients, yet each of the little masses or clusters may so retain its own nature, as to be again separable, such as it was before. As when gold and silver being melted together in a due proportion (for in every proportion, the refiners will tell you, that the experiment will not succeed) aqua fortis will dissolve the silver, and leave the gold untouched; by which means, as you lately noted, both the metals may be recovered from the mixed mass. But (continues *Carneades*) there are other clusters, wherein the particles stick not so close together, but that they may meet with corpuscles of another denomination, which are disposed to be more closely united with some of them, than they were among themselves. And in such case, two thus combining corpuscles losing that shape, or size, or motion, or other accident, upon whose account they were endowed

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with ſuch a determinate quality or nature, each of them really ceaſes to be a corpuſcle of the ſame denomination it was before; and from the coalition of theſe there may emerge a new body, as really one, as either of the corpuſcles was before they were mingled, or, if you pleaſe, confounded: ſince this concretion is really endowed with its own diſtinct qualities, and can no more by the fire, or any other known way of analyſis, be divided again into the corpuſcles, that at firſt concurred to make it, than either of them could, by the ſame means, be ſubdivided into other particles. But (ſays *Eleutherius*) to make this more intelligible by particular examples; if you diſſolve copper in aqua fortis, or ſpirit of nitre (for I remember not which I uſed, nor do I think it much material) you may by cryſtallizing the ſolution obtain a goodly vitriol; which, though by virtue of the compoſition it have manifeſtly divers qualities, not to be met with in either of the ingredients, yet it ſeems, that the nitrous ſpirits, or at leaſt many of them, may in this compounded maſs retain their former nature: for having for trial-ſake diſtilled this vitriol ſpirit, there came over ſtore of red fumes, which by that colour, by their peculiar ſtink, and by their ſourneſs, manifeſted themſelves to be nitrous ſpirits, and that the remaining calx continued copper, I ſuppoſe you will eaſily believe. But if you diſſolve minium, which is but lead powdered by the fire, in good ſpirit of vinegar, and cryſtallize the ſolution, you ſhall not only have a ſaccharine ſalt exceedingly differing from both its ingredients; but the union of ſome parts of the menſtruum with ſome of thoſe of the metal is ſo ſtrict, that the ſpirit of vinegar ſeems to be, as ſuch, deſtroyed; ſince the ſaline corpuſcles have quite loſt that acidity, upon whoſe account the liquor was called ſpirit of vinegar; nor can any ſuch acid parts, as were put to the minium, be ſeparated by any known way from the *ſaccharum Saturni* reſulting from them both. For not only there is no ſourneſs at all, but an admirable ſweetneſs to be taſted in the concretion; and not only I found not that ſpirit of wine, which otherwiſe will immediately hiſs, when mingled with ſtrong ſpirit of vinegar, would hiſs being poured upon *ſaccharum Saturni*, wherein yet the acid ſalt of vinegar, did it ſurvive, may ſeem to be concentrated; but upon the diſtillation of *ſaccharum Saturni* by itſelf, I found indeed a liquor very penetrant, but not at all acid, and differing as well in ſmell and other qualities, as in taſte, from the ſpirit of vinegar; which likewiſe ſeemed to have left ſome of its parts very firmly united to the *caput mortuum*, which though of a leaden nature was in ſmell, colour, *&c.* differing from minium. Which brings into my mind, that though two powders, the one blue, and the other yellow, may appear a green mixture, without either of them loſing its own colour, as a good microſcope has ſometimes informed me; yet having mingled minium and ſal armoniac in a requiſite proportion, and expoſed them in a glaſs veſſel to the fire, the whole maſs became white, and the red corpuſcles were deſtroyed: for though the calcined lead was ſeparable from the ſalt, yet you will eaſily believe it did not part from it in the form of a red powder, ſuch as was the minium, when it was put to the ſal armoniac. I leave it alſo to be conſidered, whether in blood, and divers other bodies, it be probable, that each of the corpuſcles, that concur to make a compound body, doth, though ſome of them in ſome caſes may, retain its own nature in it, ſo that chymiſts may extricate each ſort of them from all the others, wherewith it concurred to make a body of one denomination.

I KNOW there may be a diſtinction betwixt matter immanent, when the material parts remain, and retain their own nature in the things materiated, as ſome of the ſchoolmen ſpeak (in which ſenſe, wood, ſtones and lime are the matter of a houſe;) and tranſient, which in the materiated thing is ſo altered, as to receive a new form,

without being capable of re-admitting again the old. In which sense the friends of this distinction say, that chyle is the matter of blood, and blood that of a human body, of all whose parts it is presumed to be the aliment. I know also, that it may be said, that of material principles, some are common to all mixt bodies, as *Aristotle*'s four elements, or the chymists *tria prima*; others peculiar, which belong to this or that sort of bodies; as butter and a kind of whey may be said to be the proper principles of cream: and I deny not, but that these distinctions may in some cases be of use; but partly by what I have said already, and partly by what I am to say, you may easily enough guess, in what sense I admit them, and discern, that in such a sense they will either illustrate some of my opinions, or at least will not overthrow any of them.

To prosecute then what I was saying before, I will add to this purpose, that since the major part of chymists credit what those they call philosophers affirm of their stone, I may represent to them, that though when common gold and lead are mingled together, the lead may be severed almost unaltered from the gold; yet if instead of gold a tantillum of the red elixir be mingled with a saturn, their union will be so indissoluble in the perfect gold, that will be produced by it, that there is no known, nor perhaps no possible way of separating the diffused elixir from the fixed lead, but they both constitute a most permanent body, wherein the saturn seems to have quite lost its properties, that made it be called lead, and to have been rather transmuted by the elixir, than barely associated to it. So that it seems not always necessary, that the bodies, that are put together *per minima*, should each retain its own nature; so as when the mass itself is dissipated by the fire, to be more disposed to re-appear in its pristine form, than in any new one, which by a stricter association of its parts with those of some of the other ingredients of the compositum, than with one another, it may have acquired.

And if it be objected, that unless the hypothesis I oppose be admitted, in such cases as I have proposed, there would not be an union, but a destruction of mingled bodies, which seems all one as to say, that of such bodies there is no mistion at all; I answer, that though the substances, that are mingled, remain, only their accidents are destroyed, and though we may with tolerable congruity call them *Miscibilia*, because they are distinct bodies before they are put together, however afterwards they are so confounded, that I should rather call them concretions, or resulting bodies, than mixed ones; and though, perhaps, some other and better account may be proposed, upon which the name of mistion may remain; yet, if what I have said be thought reason, I shall not wrangle about words, though I think it fitter to alter a term of art, than reject a new truth, because it suits not with it. If it be also objected, that this notion of mine, concerning mistion, though it may be allowed, when bodies already compounded are put to be mingled, yet it is not applicable to those mistions, that are immediately made of the elements, or principles themselves; I answer in the first place, that I here consider the nature of mistion somewhat more generally, than the chymists; who yet cannot deny, that there are oftentimes mixtures, and those very durable ones, made of bodies, that are not elementary. And in the next place, that though it may be probably pretended, that in those mixtures that are made immediately of the bodies, that are called principles or elements, the mingled ingredients may better retain their own nature in the compounded mass, and be more easily separated from thence; yet, besides that it may be doubted, whether there be any such primary bodies, I see not why the reason I alleged, of the destructibility of the ingredients of bodies in general, may not sometimes be applicable to salt,

ſalt, ſulphur, or mercury; till it be ſhewn upon what account we are to believe them privileged. And however (if you pleaſe but to recall to mind, to what purpoſe I told you at firſt, I meant to ſpeak of miſtion at this time) you will perhaps allow, that what I have hitherto diſcourſed about it, may not only give ſome light to the nature of it in general (eſpecially when I ſhall have an opportunity to declare to you my thoughts on that ſubject more fully) but may on ſome occaſions alſo be ſerviceable to me in the enſuing part of this diſcourſe.

BUT to look back now to that part of our diſcourſe, whence this excurſion concerning miſtion has ſo long diverted us, though we there deduced, from the differing ſubſtances obtained from a plant nouriſhed only with water, and from ſome other things, that it was not neceſſary, that nature ſhould always compound a body at firſt of all ſuch differing bodies, as the fire could afterwards make it afford; yet this is not all, that may be collected from thoſe experiments. For from them there ſeems alſo deducible ſomething, that ſubverts another foundation of the chymical doctrine. For ſince that (as we have ſeen) out of fair water alone, not only ſpirit, but oil, and ſalt, and earth may be produced; it will follow, that ſalt and ſulphur are not primogenial bodies, and principles, ſince they are every day made out of plain water by the texture, which the ſeed or ſeminal principle of plants put it into. And this would not perhaps ſeem ſo ſtrange, if, though pride or negligence, we were not wont to overlook the obvious and familiar workings of nature; for if we conſider what ſlight qualities they are, that ſerve to denominate one of the *tria prima*, we ſhall find, that nature does frequently enough work as great alterations in divers parcels of matter; for to be readily diſſoluble in water, is enough to make the body, that is ſo, paſs for a ſalt. And yet I ſee not, why from a new ſhuffling and diſpoſition of the component particles of a body, it ſhould be much harder for nature to compoſe a body diſſoluble in water, of a portion of water, that was not ſo before, than of the liquid ſubſtance of an egg, which will eaſily mix with water, to produce by the bare warmth of a hatching hen, membranes, feathers, tendons, and other parts, that are not diſſoluble in water as that liquid ſubſtance was: nor is the hardneſs and brittleneſs of ſalt more difficult for nature to introduce into ſuch a yielding body as water, than it is for her to make the bones of a chick out of the tender ſubſtance of the liquors of an egg. But inſtead of proſecuting this conſideration, as I eaſily might, I will proceed as ſoon as I have taken notice of an objection, that lies in my way. For I eaſily foreſee it will be alleged, that the above mentioned examples are all taken from plants, and animals, in whom the matter is faſhioned by the plaſtic power of the ſeed, or ſomething analogous thereunto. Whereas the fire does not act like any of the ſeminal principles, but deſtroys them all, when they come within its reach. But to this I ſhall need at preſent to make but this eaſy anſwer, that whether it be a ſeminal principle, or any other, which faſhions that matter after thoſe various manners I have mentioned to you, yet it is evident, that either by the plaſtic principle alone, or that and heat together, or by ſome other cauſe capable to contex the matter, it is yet poſſible, that the matter may be anew contrived into ſuch bodies. And it is only for the poſſibility of this, that I am now contending.

THE SCEPTICAL CHYMIST:

PART III.

WHAT I have hitherto discourſed, *Eleutherius*, (ſays his friend to him) has, I preſume, ſhewn you, that a conſidering man may very well queſtion the truth of thoſe very ſuppoſitions, which Chymiſts as well as Peripateticks, without proving, take for granted; and upon which depends the validity of the inferences they draw from their experiments. Wherefore having diſpatched that, which though a chymiſt perhaps will not, yet I do, look upon as the moſt important, as well as difficult, part of my taſk, it will now be ſeaſonable for me to proceed to the conſideration of the experiments themſelves, wherein they are wont ſo much to triumph and glory. And theſe will the rather deſerve a ſerious examination, becauſe thoſe, that alledge them, are wont to do it with ſo much confidence and oſtentation, that they have hitherto impoſed upon almoſt all perſons, without excepting philoſophers and phyſicians themſelves, who have read their books, or heard them talk. For ſome learned men have been content rather to believe what they ſo boldly affirm, than be at the trouble and charge, to try whether or no it be true. Others again, who have curioſity enough to examine the truth of what is averred, want ſkill and opportunity to do what they deſire. And the generality even of learned men, ſeeing the chymiſts (not contenting themſelves with the ſchools to amuſe the world with empty words) actually perform divers ſtrange things, and, among thoſe, reſolve compound bodies into ſeveral ſubſtances, not known by former philoſophers to be contained in them; men I ſay, ſeeing theſe things, and hearing with what confidence chymiſts aver the ſubſtances obtained from compound bodies by the fire to be the true elements, or, (as they ſpeak) hypoſtatical principles of them, are forward to think it but juſt, as well as modeſt, that according to the logicians rule, the ſkilful artiſts ſhould be credited in their own art; eſpecially when thoſe things, whoſe nature they ſo confidently take upon them to teach others, are not only productions of their own ſkill, but ſuch as others know not elſe what to make of.

BUT though (continues *Carneades*) the chymiſts have been able, upon ſome or other of the mentioned accounts, not only to delight but amaze, and almoſt to bewitch even learned men; yet ſuch as you and I, who are not unpractiſed in the trade, muſt not ſuffer ourſelves to be impoſed upon by hard names, or bold aſſertions; nor to be dazled by that light which ſhould but aſſiſt us to diſcern things the more clearly. It is one thing to be able to help nature to produce things, and another thing to underſtand well the nature of the things produced. As we ſee, that many perſons that can beget children, are for all that as ignorant of the number and nature of the parts, eſpecially the internal ones, that conſtitute a child's body, as they that never were parents. Nor do I doubt, but you will excuſe me, if, as I thank the chymiſts for the things their analyſis ſhews me, ſo I take the liberty to conſider, how many, and what

what they are, without being aſtoniſhed at them; as if, whoſoever hath ſkill enough to ſhew men ſome new thing of his own making, had the right to make them believe whatſoever he pleaſes to tell them concerning it.

WHEREFORE, I will now proceed to my third general conſideration; which is, that it does not appear, that three is preciſely and univerſally the number of the diſtinct ſubſtances or elements, whereunto mixt bodies are reſoluble by the fire: I mean, that it is not proved by chymiſts, that all the compound bodies, which are granted to be perfectly mixt, are upon their chymical analyſis diviſible each of them into juſt three diſtinct ſubſtances, neither more nor leſs, which are wont to be looked upon as elementary, or may as well be reputed ſo as thoſe, that are ſo reputed. Which laſt clauſe I ſubjoin, to prevent your objecting, that ſome of the ſubſtances I may have occaſion to mention by and by, are not perfectly homogeneous, nor conſequently worthy of the name of principles. For that which I am now to conſider is, into how many differing ſubſtances, that may plauſibly paſs for the elementary ingredients of a mixt body, it may be analyzed by the fire; but whether each of theſe be uncompounded I reſerve to examine, when I ſhall come to the next general conſideration; where I hope to evince, that the ſubſtances, which the chymiſts not only allow, but aſſert to be the component principles of the body reſolved into them, are not wont to be uncompounded.

Now there are two kind of arguments (purſues *Carneades*) which may be brought to make my third propoſition ſeem probable; one ſort of them being of a more ſpeculative nature, and the other drawn from experience. To begin then with the firſt of theſe.

BUT as *Carneades* was going to do as he had ſaid, *Eleutherius* interrupted him, by ſaying with a ſomewhat ſmiling countenance;

IF you have no mind I ſhould think, that the proverb, *that good wits have bad memories*, is rational and applicable to you, you muſt not forget now you are upon the ſpeculative conſiderations, that may relate to the number of the elements, that yourſelf did not long ſince deliver and concede ſome propoſitions in favour of the chymical doctrine, which I may without diſparagement to you think it uneaſy, even for *Carneades* to anſwer.

I HAVE not, replies he, forgot the conceſſions you mean; but I hope too, that you have not forgot neither with what cautions they were made, when I had not yet aſſumed the perſon I am now ſuſtaining. But however, I ſhall, to content you, ſo diſcourſe of my third general conſideration, as to let you ſee, that I am not unmindful of the things you would have me remember.

To talk then again according to ſuch principles as I then made uſe of, I ſhall repreſent, that if it be granted rational to ſuppoſe, as I then did, that the elements conſiſted at firſt of certain ſmall and primary coalitions of the minute particles of matter into corpuſcles very numerous, and very like each other, it will not be abſurd to conceive, that ſuch primary cluſters may be of far more ſorts than three or five; and conſequently, that we need not ſuppoſe, that in each of the compound bodies we are treating of, there ſhould be found juſt three ſorts of ſuch primitive coalitions, as we are ſpeaking of.

AND if, according to this notion, we allow a conſiderable number of differing elements, I may add, that it ſeems very poſſible, that to the conſtitution of one ſort of mixt bodies two kinds of elementary ones may ſuffice (as I lately exemplified to you, in that moſt durable concrete, glaſs.) another ſort of mixts may be compoſed of three elements, another of four, another of five, and another perhaps of many more.

more. So that, according to this notion, there can be no determinate number assigned, as that of the elements of all sorts of compound bodies whatsoever; it being very probable, that some concretes consist of fewer, some of more elements. Nay, it does not seem impossible, according to these principles, but that there may be two sorts of mixts, whereof the one may not have any of all the same elements as the other consists of; as we oftentimes see two words, whereof one has not any one of the letters to be met with in the other; or as we often meet with divers electuaries, in which no ingredient (except sugar) is common to any two of them. I will not here debate, whether there may not be a multitude of these corpuscles, which by reason of their being primary and simple, might be called elementary, if several sorts of them should convene to compose any body, which are as yet free, and neither as yet contexed and entangled with primary corpuscles of other kinds, but remain liable to be subdued and fashioned by seminal principles, or the like powerful and transmuting agent; by whom they may be so connected among themselves, or with the parts of the bodies, as to make the compound bodies, whose ingredients they are, resoluble into more, or other elements than those that chymists have hitherto taken notice of.

To all which I may add, that since it appears, by what I observed to you of the permanency of gold and silver, that even corpuscles, that are not of an elementary, but compounded nature, may be of so durable a texture, as to remain indissoluble in the ordinary analysis that Chymists make of bodies by the fire; it is not impossible, but that, though there were but three elements, yet there may be a greater number of bodies, which the wonted ways of anatomy will not discover to be no elementary bodies.

BUT, (says *Carneades*) having thus far, in compliance to you, talked conjecturally of the number of the elements, it is now time to consider, not of how many elements it is possible that nature may compound mixed bodies, but (at least, as far as the ordinary experiments of chymists will inform us) of how many she doth make them up.

I SAY then, that it does not by these sufficiently appear to me, that there is any one determinate number of elements to be uniformly met with in all the several sorts of bodies allowed to be perfectly mixed.

AND for the more distinct proof of this proposition, I shall in the first place represent, that there are divers bodies, which I could never see by fire divided into so many as three elementary substances. I would fain (as I said lately to *Philoponus*) see that fixed and noble metal we call gold, separated into salt, sulphur and mercury: and if any man will submit to a competent forfeiture in case of failing, I shall willingly, in case of prosperous success, pay for both the materials and the charges of such an experiment. It is not, that after what I have tried myself I dare peremptorily deny, that there may out of gold be extracted a certain substance, which I cannot hinder chymists from calling its tincture or sulphur; and which leaves the remaining body deprived of its wonted colour. Nor am I sure, that there cannot be drawn out of the same metal a real quick and running mercury. But for the salt of gold, I never could either see it, or be satisfied that there was ever such a thing separated, *in rerum naturâ*, by the relation of any credible eye-witness. And for the several processes, that promise that effect, the materials, that must be wrought upon, are somewhat too precious and costly to be wasted upon so groundless adventures, of which not only the success is doubtful, but the very possibility is not yet demonstrated. Yet that, which most deters me from such trials, is not their chargeableness,

but their unſatisfactorineſs, though they ſhould ſucceed. For the extraction of this golden ſalt being in chymiſts proceſſes preſcribed to be effected by corroſive menſtruums, or the intervention of other ſaline bodies, it will remain doubtful to a wary perſon, whether the emergent ſalt be that of the gold itſelf, or of the ſaline bodies or ſpirits employed to prepare it; for that ſuch diſguiſes of metals do often impoſe upon artiſts, I am ſure *Eleutherius* is not ſo much a ſtranger to chymiſtry as to ignore. I would likewiſe willingly ſee the three principles ſeparated from the pure ſort of virgin-ſand, from oſteocolla, from refined ſilver, from quickſilver freed from its adventitious ſulphur, from Venetian talc, which by long detention in an extreme reverberium, I could but divide into ſmaller particles, not the conſtituent principles; nay, which, when I cauſed it to be kept I know not how long, in a glaſs-houſe fire, came out in the figure its lumps had, when put in, though altered to an almoſt amethyſtine colour; and from divers other bodies, which it were now unneceſſary to enumerate. For though I dare not abſolutely affirm it to be impoſſible to analyze theſe bodies into their *tria prima*; yet becauſe neither of my own experiments, nor any competent teſtimony hath hitherto either taught me, how ſuch an analyſis may be made, or ſatisfied me, that it hath been ſo; I muſt take the liberty to refrain from believing it, till the chymiſts prove it, or give us intelligible and practicable proceſſes to perform what they pretend. For whilſt they affect that enigmatical obſcurity, with which they are wont to puzzle the readers of their divulged proceſſes concerning the analytical preparation of gold or mercury, they leave wary perſons much unſatisfied, whether or no the differing ſubſtances, they promiſe to produce, be truly the hypoſtatical principles, or only ſome intermixtures of the divided bodies with thoſe employed to work upon them, as is evident in the ſeeming cryſtals of ſilver, and thoſe of mercury; which, though by ſome inconſiderately ſuppoſed to be the ſalts of thoſe metals, are plainly but mixtures of the metalline bodies with the ſaline parts of aqua fortis, or other corroſive liquors; as is evident by their being reducible into ſilver or quickſilver, as they were before.

I CANNOT but confeſs (ſaith *Eleutherius*) that though chymiſts may, upon probable grounds, affirm themſelves able to obtain their *tria prima* from animals and vegetables, yet I have often wondered, that they ſhould ſo confidently pretend alſo, to reſolve all metalline and other mineral bodies into ſalt, ſulphur, and mercury. For it is a ſaying almoſt proverbial among thoſe chymiſts themſelves, that are accounted philoſophers, and our famous countryman *Roger Bacon* has particularly adopted it; that *facilius eſt aurum facere, quam deſtruere.* And I fear, with you, that gold is not the only mineral, from which chymiſts are wont fruitleſly to attempt the ſeparating of their three principles. I know indeed (continues *Eleutherius*) that the learned *Sennertus*, even in that book, where he takes not upon him to play the advocate for the chymiſts, but the umpire betwixt them and the Peripateticks, expreſſes himſelf roundly, thus; *Salem omnibus ineſſe (mixtis ſcilicet) & ex iis fieri poſſe omnibus in reſolutionibus chymicis verſatis notiſſimum eſt.* And in the next page, *quod de ſale dixi,* ſays he, *idem de ſulphure dici poteſt.* But, by his favour, I muſt ſee very good proofs, before I believe ſuch general aſſertions, how boldly ſoever made; and he, that would convince me of their truth, muſt firſt teach me ſome true and practicable way of ſeparating ſalt and ſulphur from gold, ſilver, and thoſe many different ſorts of ſtones, that a violent fire does not bring to lime, but to fuſion: and not only I, for my own part, never ſaw any of thoſe newly named bodies ſo reſolved; but *Helmont*, who was much better verſed in the chymical anatomizing of bodies then either *Sennertus* or I, has ſomewhere this reſolute paſſage; *Scio* (ſays he) *ex arena, ſilicibus & ſaxis, non calcariis,*

Sennert. lib. de conſ. & deſſenſ. pag. 147.

Helmon. pag. 419.

calcariis, nunquam sulphur aut mercurium trahi posse: nay, *Quercetanus* himself, though the grand stickler for the *tria prima*, has this confession of the irresolubleness of diamonds; *Adamas* (saith he) *omnium factus lapidum solidissimus ac durissimus ex arctissima videlicet trium principiorum unione ac cohærentia, quæ nulla arte separationis in solutionem principiorum suorum spiritualium disjungi potest.* And indeed, pursues *Eleutherius*, I was not only glad, but somewhat surprized to find you inclined to admit, that there may be a sulphur and a running mercury drawn from gold; for unless you do (as your expression seemed to intimate) take the word sulphur in a very loose sense, I must doubt, whether our chymists can separate a sulphur from gold: for when I saw you make the experiment, that I suppose invited you to speak as you did, I did not judge the golden tincture to be the true principle of sulphur extracted from the body, but an aggregate of some such highly coloured parts of the gold, as a chymist would have called a sulphur incombustible, which in plain English seems to be little better than to call it a sulphur and no sulphur. And as for metalline mercuries, I had not wondered at it, though you had expressed much more severity in speaking of them; for I remember, that having once met an old and famous artist, who had long been (and still is) chymist to a great monarch, the repute he had of a very honest man invited me to desire him to tell me ingeniously, whether or no, among his many labours, he had ever really extracted a true and running mercury out of metals; to which question he freely replied, that he had never separated a true mercury from any metal; nor had ever seen it really done by any man else. And though gold is, of all metals, that, whose mercury chymists have most endeavoured to extract, and which they do the most brag they have extracted; yet the experienced *Angelus Sala*, in his spagyrical account of the seven terrestrial planets (that is, the seven metals) affords us this memorable testimony, to our present purpose: *Quanquam* (says he) *&c. experientia tamen (quam stultorum magistram vocamus) certe comprobavit, mercurium auri adeo fixum, maturum, & arctè cum reliquis ejusdem corporis substantiis conjungi, ut nullo modo retrogredi possit.* To which he subjoins, that he himself had seen much labour spent upon that design, but could never see any such mercury produced thereby. And I easily believe what he annexes; *that he had often seen detected many tricks and impostures of cheating alchymists.* For, the most part of those, that are fond of such charletans, being unskilful or credulous, or both, it is very easy for such as have some skill, much craft, more boldness, and no conscience, to impose upon them: and therefore, though many professed alchymists, and divers persons of quality have told me, that they have made or seen the mercury of gold, or of this or that other metal; yet I have been still apt to fear, that either these persons have had a design to deceive others, or have had not skill and circumspection enough to keep themselves from being deceived.

Quercet. apud Billich. in Theissalo. redivivo, pag. 99.

You recall to my mind (says *Carneades*) a certain experiment I once devised, innocently to deceive some persons, and let them and others see, how little is to be built upon the affirmation of those, that are either unskilful or unwary, when they tell us they have seen alchymists make the mercury of this or that metal: and to make this the more evident, I made my experiment much more slight, short and simple, than the chymists usual processes to extract metalline mercuries; which operations being commonly more elaborate and intricate, and requiring a much more longer time, give the alchymists a greater opportunity to cozen, and consequently are more obnoxious to the spectators suspicion. And that, wherein I endeavoured to make my experiment look the more like a true analysis, was, that I not only pretended, as well as others, to extract a mercury from the metal I wrought upon, but likewise to separate

rate a large proportion of manifeſt and inflammable ſulphur. I take then of the filings of copper about a dram or two; of common ſublimate, powdered, the like weight; and ſal armoniac near about as much as of ſublimate: theſe three being well mingled together, I put into a ſmall phial with a long neck, or, which I find better, into a glaſs-urinal, which (having firſt ſtopped it with cotton) to avoid the noxious fumes, I approach by degrees to a competent fire of well kindled coals, or (which looks better, but more endangers the glaſs) to the flame of a candle; and after a while the bottom of the glaſs being held juſt upon the kindled coals, or in the flame, you may in about a quarter of an hour, or, perchance, in half that time, perceive in the bottom of the glaſs ſome running mercury; and if then you take away the glaſs, and break it, you ſhall find a parcel of quickſilver, perhaps altogether, and perhaps part of it in the pores of the ſolid maſs. You ſhall find too, that the remaining lump being held to the flame of the candle, will readily burn with a greeniſh flame, and after a little while (perchance preſently) will in the air acquire a greeniſh blue, which being the colour that is aſcribed to copper, when its body is unlocked, it is eaſy to perſuade men, that this is the true ſulphur of *Venus*, eſpecially ſince not only the ſalts may be ſuppoſed partly to be flown away, and partly to be ſublimed to the upper part of the glaſs, (whoſe inſide will commonly appear whitened by them) but the metal ſeems to be quite deſtroyed, the copper no longer appearing in a metalline form, but almoſt in that of a reſinous lump: whereas, indeed, the caſe is only this, that the ſaline parts of the ſublimate, together with the ſal armoniac, being excited and actuated by the vehement heat, fall upon the copper, (which is a metal they can more eaſily corrode than ſilver) whereby the ſmall parts of the mercury being freed from the ſalts, that kept them aſunder, and being by the heat tumbled up and down after many occurſions, they convene into a conſpicuous maſs of liquor; and as for the ſalts, ſome of the more volatile of them ſubliming to the upper part of the glaſs, the others corrode the copper, and uniting themſelves with it, do ſtrangely alter and diſguiſe its metallick form, and compoſe with it a new kind of concrete, inflammable like ſulphur; concerning which, I ſhall not now ſay any thing, ſince I can refer you to the diligent obſervations, which I remember Mr. *Boyle* has made concerning this odd kind of verdigreaſe. But, continues *Carneades* ſmiling, you know I was not cut out for a mountebank, and therefore I will haſten to reſume the perſon of a ſceptick, and take up my diſcourſe, where you diverted me from proſecuting it.

IN the next place then I conſider, that, as there are ſome bodies, which yield not ſo many as the three principles; ſo there are many others, that in their reſolution exhibit more principles than three; and that therefore the ternary number is not that of the univerſal and adequate principles of bodies. If you allow of the diſcourſe I lately made you, touching the primary aſſociations of the ſmall particles of matter, you will ſcarce think it improbable, that of ſuch elementary corpuſcles there may be more ſorts than either three, or four, or five. And if you will grant, what will ſcarce be denied, that corpuſcles of a compounded nature may, in all the wonted examples of chymiſts, paſs for elementary, I ſee not why you ſhould think it impoſſible, that aqua fortis, or aqua regis, will make a ſeparation of colliquated ſilver and gold, though the fire cannot: ſo there may be ſome agent found out ſo ſubtile and ſo powerful, at leaſt in reſpect of thoſe particular compounded corpuſcles, as to be able to reſolve them into thoſe more ſimple ones, whereof they conſiſt, and conſequently increaſe the number of the diſtinct ſubſtances, whereunto the mixed body has been hitherto reſoluble And if that be true, which I recited to you a while ago out of *Helmont*, concerning the operations of the alkaheſt, which divides bodies into other

distinct substances, both as to number and nature, than the fire does; it will not a little countenance my conjecture. But confining ourselves to such ways of analyzing mixed bodies, as are already not unknown to chymists, it may without absurdity be questioned, whether besides those grosser elements of bodies, which they call salt, sulphur and mercury, there may not be ingredients of a more subtile nature, which being extremely little, and not being in themselves visible, may escape unheeded at the junctures of the distillatory vessels, though never so carefully luted. For let me observe to you one thing, which, though not taken notice of by chymists, may be a notion of good use in divers cases to a naturalist, that we may well suspect, that there may be several sorts of bodies, which are not immediate objects of any one of our senses; since we see, that not only those little corpuscles, that issue out of the load-stone, and perform the wonders, for which it is justly admired; but the effluviums of amber, jet, and other electrical concretes, though by their effects upon the particular bodies disposed to receive their action, they seem to fall under the cognizance of our sight, yet do they not as electrical immediately affect any of our senses, as do the bodies, whether minute or greater, that we see, feel, taste, *&c.* But (continues *Carneades)* because you may expect I should, as the chymists do, consider only the sensible ingredients of mixt bodies, let us now see what experience will, even as to these, suggest to us.

It seems then questionable enough, whether from grapes variously ordered there may not be drawn more distinct substances by the help of the fire, than from most other mixt bodies. For the grapes themselves being dried into raisins and distilled, will (besides alkali, phlegm, and earth) yield a considerable quantity of an empyreumatical oil, and a spirit of a very different nature from that of wine. Also the unfermented juice of grapes affords other distilled liquors than wine doth. The juice of grapes, after fermentation, will yield a *spiritus ardens*; which, if competently rectified, will all burn away without leaving any thing remaining. The same fermented juice degenerating into vinegar yields an acid and corroding spirit. The same juice tunned up arms itself with tartar; out of which may be separated, as out of other bodies, phlegm, spirit, oil, salt and earth: not to mention what substances may be drawn from the vine itself, probably differing from those, which are separated from tartar, which is a body by itself, that has few resemblers in the world. And I will further consider, that what force soever you will allow this instance, to evince, that there are some bodies, that yield more elements than others, it can scarce be denied, but that the major part of bodies, that are divisible into elements, yield more than three. For, besides those, which the chymists are pleased to name hypostatical, most bodies contain two others, phlegm and earth; which concurring as well as the rest to the constitution of mixts, and being as generally, if not more, found in their analysis, I see no sufficient cause, why they should be excluded from the number of elements. Nor will it suffice to object, as the Paracelsians are wont to do, that the *tria prima* are the most useful elements, and the earth and water but worthless and unactive; for elements being called so, in relation to the constituting of mixt bodies, it should be upon the account of its ingrediency, not of its use, that any thing should be affirmed or denied to be an element: and as for the pretended usefulness of earth and water, it would be considered, that usefulness, or the want of it, denotes only a respect or relation to us; and therefore the presence, or absence of it, alters not the intrinsick nature of the thing. The hurtful teeth of vipers are, for aught I know, useless to us, and yet are not to be denied to be parts of their bodies; and it were hard to shew of what greater use to us, than phlegm and

and earth, are those undiscerned stars, which our new telescopes discover to us, in many blanched places of the sky: and yet we cannot but acknowledge them constituent, and considerably great parts of the universe. Besides that, whether or no the phlegm and earth be immediately useful, but necessary to constitute the body whence, they are separated; and consequently, if the mixt body be not useless to us, those constituent parts, without which it could not have been that mixed body, may be said not to be unuseful to us: and though the earth and water be not so conspicuously operative (after separation) as the other three more active principles, yet in this case it will not be amiss to remember the lucky fable of *Menenius Agrippa*, of the dangerous sedition of the hands and legs, and other more busy parts of the body, against the seemingly unactive stomach. And to this case also we may not unfitly apply that reasoning of an Apostle, to another purpose; *If the ear shall say, because I am not the eye, I am not of the body; is it therefore not of the body? If the whole body were eye, where were the hearing? If the whole were for hearing, where the smelling?* In a word, since earth and water appear, as clearly and as generally as the other principles, upon the resolution of bodies, to be the ingredients, whereof they are made up; and since they are useful (if not immediately to us or rather to physicians) to the bodies they constitute, and so, though in somewhat a remoter way, are serviceable to us; to exclude them out of the number of elements, is not to imitate nature.

AND on this occasion I cannot but take notice, that whereas the great argument, which the chymists are wont to employ to vilify earth and water, and make them be looked upon as useless and unworthy to be reckoned among the principles of mixed bodies, is, that they are not endowed with specifick properties, but only with elementary qualities; of which they use to speak very slightingly, as of qualities contemptible and unactive; I see no sufficient reason for this practice of the chymists: for it is confessed, that heat is an elementary quality, and yet that an almost innumerable company of considerable things are performed by heat, is manifest to them, that duly consider the various phænomena, wherein it intervenes as a principal actor; and none ought less to ignore or distrust this truth than a chymist; since almost all the operations and productions of his art are performed chiefly by the means of heat. And as for cold itself, upon whose account they so despise the earth and water, if they please to read in the voyages of our English and Dutch navigators in *Nova Zembla* and other northern regions, what stupendous things may be effected by cold, they would not perhaps think it so despicable. And not to repeat what I lately recited to you out of *Paracelsus* himself, who by the help of an intense cold teaches to separate the quintessence of wine; I will only now observe to you, that the conservation of the texture of many bodies, both animate and inanimate, does so much depend upon the convenient motion both of their own fluid and looser parts, and of the ambient bodies, whether air, water, *&c.* that not only in human bodies we see, that the immoderate or unseasonable coldness of the air (especially when it finds such bodies over-heated) does very frequently discompose the œconomy of them, and occasion variety of diseases; but in the solid and durable body of iron itself, in which one would not expect, that sudden cold should produce any notable change, it may have so great an operation, that if you take a wire, or other slender piece of steel, and having brought it in the fire to a white heat, you suffer it afterwards to cool leisurely in the air, it will, when it is cold, be much of the same hardness it was of before. Whereas, if as soon as you remove it from the fire, you plunge it into cold water, it will upon the sudden refrigeration acquire a very much greater hardness than it had before; nay, and will become manifestly brittle. And that you may not impute this

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to any peculiar quality in the water, or other liquor, or unctuous matter, wherein such heated steel is wont to be quenched, that it may be tempered; I know a very skilful tradesman, that divers times hardens steel by suddenly cooling it in a body, that is neither a liquor, nor so much as moist. A trial of that nature I remember I have seen made. And, however, by the operation, that water has upon steel quenched in it, whether upon the account of its coldness and moisture, or upon that of any other of its qualities, it appears, that water is not always so inefficacious and contemptible a body, as our chymists would have it pass for. And, what I have said of the efficacy of cold and heat, might perhaps be easily enough carried further by other considerations and experiments; were it not, that having been mentioned only upon the by, I must not insist on it, but proceed to another subject.

BUT (pursues *Carneades*) though I think it evident, that earth and phlegm are to be reckoned among the elements of most animal and vegetable bodies, yet it is not upon that account alone, that I think divers bodies resoluble into more substances than three. For there are two experiments, that I have sometimes made, to shew, that at least some mixts are divisible into more distinct substances than five. The one of these experiments, though it will be more seasonable for me to mention it fully anon, yet in the mean time, I shall tell you thus much of it; that out of two distilled liquors, which pass for elements of the bodies, whence they are drawn, I can, without addition, make a true yellow and inflammable sulphur, notwithstanding that the two liquors remain afterwards distinct. Of the other experiment, which, perhaps, will not be altogether unworthy your notice, I must now give you this particular account: I had long observed, that by the distillation of divers woods, both in ordinary, and some unusual sorts of vessels, the copious spirit, that came over, had, besides a strong taste to be met with in the empyreumatical spirits of many other bodies, an acidity almost like that of vinegar: wherefore I suspected, that though the sourish liquor distilled, for instance, from box-wood, be looked upon by chymists as barely the spirit of it, and therefore as one single element or principle; yet it does really consist of two differing substances, and may be divisible into them; and consequently, that such woods and other mixts, as abound with such a vinegar, may be said to consist of one element or principle, more than the chymists as yet are aware of. Wherefore bethinking myself, how the separation of these two spirits might be made, I quickly found, that there were several ways of compassing it. But that of them, which I shall at present mention, was this: having distilled a quantity of box-wood *per se*, and slowly rectified the sourish spirit, the better to free it both from oil and phlegm, I cast into this rectified liquor a convenient quantity of powdered coral, expecting that the acid part of the liquor would corrode the coral, and being associated with it, would be so retained by it, that the other part of the liquor, which was not of an acid nature, nor fit to fasten upon the corals, would be permitted to ascend alone. Nor was I deceived in my expectation; for having gently abstracted the liquor from the corals, there came over a spirit of a strong smell, and of a taste very piercing, but without any sourness; and which was in divers qualities manifestly different, not only from a spirit of vinegar, but from some spirit of the same wood, that I purposely kept by me without depriving it of its acid ingredient. And to satisfy you, that these two substances were of a very differing nature, I might inform you of several trials, that I made, but must not name some of them, because I cannot do so without making some unseasonable discoveries. Yet this I shall tell you at present, that the sour spirit of box not only would, as I just now related, dissolve corals, which the other would not fasten on, but being poured upon salt of tartar, would immediately boil

and hiſs, whereas the other would lie quietly upon it. The acid ſpirit poured upon minium made a ſugar of lead, which I did not find the other to do: ſome drops of this penetrant ſpirit being mingled with ſome drops of the blue ſyrup of violets, ſeemed rather to dilute, than otherwiſe alter the colour; whereas the acid ſpirit turned the ſyrup of a reddiſh colour, and would probably have made it of as pure a red, as acid ſalts are wont to do, had not its operation been hindered by the mixture of the other ſpirit. A few drops of the compound ſpirit being ſhaken into a pretty quantity of the infuſion of lignum nephriticum, preſently deſtroyed all the bluiſh colour, whereas the other ſpirit would not take it away. To all which it might be added, that having for trial's ſake poured fair water upon the corals, that remained in the bottom of the glaſs, wherein I had rectified the double ſpirit (if I may ſo call it) that was firſt drawn from the box, I found, according to my expectation, that the acid ſpirit had really diſſolved the corals, and had coagulated with them. For by the affuſion of fair water, I obtained a ſolution, which (to note that ſingularity upon the by) was red, whence the water being evaporated, there remained a ſoluble ſubſtance much like the ordinary ſalt of coral, as chymiſts are pleaſed to call that magiſtery of corals, which they make by diſſolving them in common ſpirit of vinegar, and abſtracting the menſtruum *ad ſiccitatem*. I know not whether I ſhould ſubjoin on this occaſion, that the ſimple ſpirit of box, if chymiſts will have it therefore ſaline becauſe it has a ſtrong taſte, will furniſh us with a new kind of ſaline bodies, differing from thoſe hitherto taken notice of. For whereas of the three chief ſorts of ſalts, the acid, the alcalizate, and the ſulphureous, there is none, that ſeems to be friends with both the other two, as I may, ere it be long, have occaſion to ſhew; I did not find, but that the ſimple ſpirit of box did agree very well (at leaſt as far as I had occaſion to try it) both with the acid and the other ſalts. For though it would lie very quiet with ſalt of tartar, ſpirit of urine, or other bodies, whoſe ſalts were either of an alcalizate or fugitive nature; yet did not the mingling of oil of vitriol itſelf produce any hiſſing or effervescence, which you know is wont to enſue upon the affuſion of that highly acid liquor upon either of the bodies newly mentioned.

I THINK myſelf, (ſays *Eleutherius*) beholden to you for this experiment; not only becauſe I foreſee you will make it helpful to you in the enquiry you are now upon, but becauſe it teaches us a method, whereby we may prepare a numerous ſort of new ſpirits, which though more ſimple than any that are thought elementary, are manifeſtly endowed with peculiar and powerful qualities, ſome of which may probably be of conſiderable uſe in phyſick, as well alone as aſſociated with other things; as one may hopefully gueſs by the redneſs of that ſolution your ſour ſpirit made of corals, and by ſome other circumſtances of your narrative. And ſuppoſe (purſues *Eleutherius*) that you are not ſo confined, for the ſeparation of the acid parts of theſe compound ſpirits from the other, to employ corals; but that you may as well make uſe of any alcalizate ſalt, or of pearls, or crabs eyes, or any other body, upon which common ſpirit of vinegar will eaſily work, and, to ſpeak in an *Helmontial* phraſe, exantlate itſelf.

I HAVE not yet tried (ſays *Carneades*) of what uſe the mentioned liquors may be in phyſic, either as medicines or as menſtruums: but I could mention now (and may another time) divers of the trials, that I made to ſatisfy myſelf of the difference of theſe two liquors. But that, as I allow your thinking what you newly told me about corals, I preſume you will allow me, from what I have ſaid already, to deduce this corollary; that there are divers compound bodies, which may be reſolved into four ſuch differing ſubſtances, as may as well merit the name of principles, as thoſe to which

which the chymists freely give it. For since they scruple not to reckon that, which I call the compound spirit of box, for the spirit, or, as others would have it, the mercury of that wood; I see not, why the acid liquor, and the other, should not each of them, especially that last named, be looked upon as more worthy to be called an elementary principle; since it must needs be of a more simple nature than the liquor, which was found to be divisible into that, and the acid spirit. And this further use (continues *Carneades*) may be made of our experiment to my present purpose, that it may give us a rise to suspect, that since a liquor reputed, by the chymists to be, without dispute, homogeneous, is, by so slight a way, divisible into two distinct and more simple ingredients, some more skilful or happier experimenter than I, may find a way either further to divide one of these spirits, or to solve some or other, if not all, of those other ingredients of mixt bodies, that have hitherto passed among chymists for their elements or principles.

THE

SCEPTICAL CHYMIST:

PART IV.

AND thus much (says *Carneades*) may suffice to be said of the number of the distinct substances separable from mixt bodies by the fire: wherefore I now proceed to consider the nature of them, and shew you, that though they seem homogeneous bodies, yet have they not the purity and simplicity that is requisite to elements; and I should immediately proceed to the proof of my assertion, but that the confidence, wherewith chymists are wont to call each of the substances we speak of by the name of sulphur or mercury, or the other of the hypostatical principles, and the intolerable ambiguity they allow themselves in their writings and expressions, makes it necessary for me, in order to the keeping you either from mistaking me, or thinking I mistake the controversy, to take notice to you, and complain of the unreasonable liberty they give themselves of playing with names at pleasure. And indeed if I were obliged in this dispute, to have such regard to the phraseology of each particular chymist, as not to write any thing, which this or that author may not pretend, not to contradict this or that sense, which he may give us as occasion serves to his ambiguous expressions, I should scarce know how to dispute, nor which way to turn myself. For I find that even eminent writers (such as *Raymund Lully*, *Paracelsus*, and others) do so abuse the terms they employ, that as they will now and then give divers things one name; so they will oftentimes give one thing many names; and some of them (perhaps) such, as do much more properly signify some distinct body of another kind; nay, even in technical words or terms of art, they refrain not from this confounding liberty; but will, as I have observed, call the same substance, sometimes the sulphur,

and ſometimes the mercury of a body. And now I ſpeak of mercury, I cannot but take notice, that the deſcriptions they give us of that principle or ingredient of mixt bodies, are ſo intricate, that even thoſe, that have endeavoured to poliſh and illuſtrate the notions of the chymiſts, are fain to confeſs, that they know not what to make of it either by ingenuous acknowledgments, or deſcriptions, that are not intelligible.

I MUST confeſs (ſays *Eleutherius*) I have in the reading of *Paracelſus*, and other chymical authors, been troubled to find, that ſuch hard words and equivocal expreſſions, as you juſtly complain of, do, even when they treat of principles, ſeem to be ſtudiouſly affected by thoſe writers; whether to make themſelves to be admired by their readers, and their art appear more venerable and myſterious, or (as they would have us think) to conceal from them a knowledge themſelves judge ineſtimable.

BUT whatever (ſays *Carneades*) theſe men may promiſe themſelves from a canting way of delivering the principles of nature, they will find the major part of knowing men ſo vain, as when they underſtand not what they read, to conclude, that it is rather the writer's fault than their own. And thoſe, that are ſo ambitious to be admired by the vulgar, that rather than go without the admiration of the ignorant they will expoſe themſelves to the contempt of the learned, thoſe ſhall, by my conſent, freely enjoy their option. As for the myſtical writers ſcrupling to communicate their knowledge, they might leſs to their own diſparagement, and to the trouble of their readers, have concealed it by writing no books, than by writing bad ones. If *Themiſtius* were here, he would not ſtick to ſay, that chymiſts write thus darkly, not becauſe they think their notions too precious to be explained, but becauſe they fear, that if they were explained, men would diſcern, that they are far from being precious. And indeed I fear, that the chief reaſon, why chymiſts have written ſo obſcurely of their three principles, may be, that not having clear and diſtinct notions of them themſelves, they cannot write otherwiſe than confuſedly of what they but confuſedly apprehend: not to ſay, that divers of them, being conſcious to the invalidity of their doctrine, might well enough diſcern, that they could ſcarce keep themſelves from being confuted, but by keeping themſelves from being clearly underſtood. But though much may be ſaid to excuſe the chymiſts when they write darkly, and ænigmatically, about the preparation of their elixir, and ſome few other grand arcana, the divulging of which they may, upon grounds plauſible enough, eſteem unfit; yet when they pretend to teach the general principles of natural philoſophers, this equivocal way of writing is not to be endured. For in ſuch ſpeculative enquiries, where the naked knowledge of the truth is the thing principally aimed at, what does he teach me worth thanks, that does not, if he can, make his notion intelligible to me, but by myſtical terms, and ambiguous phraſes darkens what he ſhould clear up; and makes me add the trouble of gueſſing at the ſenſe of what he equivocally expreſſes, to that of examining the truth of what he ſeems to deliver? And if the matter of the philoſopher's ſtone, and the manner of preparing it, be ſuch myſteries, as they would have the world believe them, they may write intelligibly and clearly of the principles of mixt bodies in general, without diſcovering what they call the great work. But for my part (continues *Carneades*) what my indignation at this un-philoſophical way of teaching principles has now extorted from me, is meant chiefly to excuſe myſelf, if I ſhall hereafter oppoſe any particular opinion or aſſertion, that ſome follower of *Paracelſus* or any eminent artiſt may pretend not to be his maſter's. For, as I told you long ſince, I am not obliged to examine private men's writings (which were a labour as endleſs as unprofitable) being only engaged to examine thoſe opinions about the *tria prima*, which I find thoſe chymiſts I have met with to agree in moſt: and I doubt not, but my arguments againſt

their doctrine will be in great part easily enough applicable even to those private opinions, which they do not so directly and expressly oppose. And indeed, that, which I am now entering upon, being the consideration of the things themselves, whereinto Spagyrists resolve mixt bodies by the fire, if I can shew, that these are not of an elementary nature, it will be no great matter what names these or those chymists have been pleased to give them. And I question not, that to a wise man, and consequently to *Eleutherius*, it will be less considerable to know, what men have thought of things, than what they should have thought.

IN the fourth and last place, then, I consider, that as generally as chymists are wont to appeal to experience, and as confidently as they use to instance the several substances separated by the fire from a mixt body, as a sufficient proof of their being its component elements; yet those differing substances are many of them far enough from elementary simplicity, and may be yet looked upon as mixt bodies, most of them also retaining, somewhat at least, if not very much, of the nature of those concretes whence they were forced. I am glad (says *Eleutherius*) to see the vanity or envy of the canting chymists thus discovered and chastised; and I could wish, that learned men would conspire together to make these deluding writers sensible, that they must no longer hope with impunity to abuse the world. For whilst such men are quietly permitted to publish books with promising titles, and therein to assert what they please, and contradict others, and even themselves as they please, with as little danger of being confuted as of being understood, they are encouraged to get themselves a name, at the cost of the readers, by finding, that intelligent men are wont, for the reason newly mentioned, to let their books and them alone: and the ignorant and credulous (of which the number is still much greater than that of the other) are forward to admire most what they least understand. But if judicious men, skilled in chymical affairs, shall once agree to write clearly and plainly of them, and thereby keep men from being stunned, as it were, or imposed upon by dark or empty words; it is to be hoped, that these men finding, that they can no longer write impertinently and absurdly, without being laughed at for doing so, will be reduced either to write nothing, or books, that may teach us something, and not rob men, as formerly, of invaluable time; and so ceasing to trouble the world with riddles or impertinencies, we shall either by their books receive an advantage, or by their silence escape an inconvenience.

BUT after all this is said (continues *Eleutherius*) it may be represented in favour of the chymists, that, in one regard the liberty they take in using names, if it be excusable at any time, may be more so when they speak of the substances, whereinto their analysis resolves mixt bodies: since as parents have the right to name their own children, it has ever been allowed to the authors of new inventions, to impose names upon them. And therefore the subjects we speak of being so the productions of the chymists art, as not to be otherwise, but by it, obtainable; it seems but equitable to give the artists leave to name them as they please: considering also, that none are so fit and likely to teach us what those bodies are, as they to whom we owed them.

I TOLD you already (says *Carneades*) that there is great difference betwixt the being able to make experiments, and the being able to give a philosophical account of them. And I will not now add, that many a mine-digger may meet, whilst he follows his work, with a gem or a mineral which he knows not what to make of, till he shews it a jeweller or a mineralist to be informed what it is. But that which I would rather have here observed, is, that the chymists I am now in debate with have given up the liberty you challenged for them, of using names at pleasure, and confined themselves by

by their descriptions, though but such as they are, of their principles; so that although they might freely have called any thing their analysis presents them with, either sulphur, or mercury, or gas, or blas, or what they pleased; yet when they have told me, that sulphur (for instance) is a primogeneal and simple body, inflammable, odorous, *&c.* they must give me leave to disbelieve them, if they tell me, that a body, that is either compounded or uninflammable, is such a sulphur; and to think they play with words, when they teach, that gold and some other minerals abound with an incombustible sulphur, which is as proper an expression, as a sunshine night, or a fluid ice.

BUT before I descend to the mention of particulars belonging to my fourth consideration, I think it convenient to premise a few generals; some of which I shall the less need to insist on at present, because I have touched on them already.

AND first I must invite you to take notice of a certain passage in *Helmont* *; which though I have not found much heeded by his readers, he himself mentions as a notable thing, and I take to be a very considerable one: for whereas the distilled oil-olive, though drawn *per se*, is (as I have tried) of a very sharp and fretting quality, and of an odious taste, he tells us, that simple oil being only digested with *Paracelsus's sal circulatum*, is reduced into dissimilar parts, and yields a sweet oil, very differing from the oil distilled from sallet oil; as also, that by the same way there may be separated from wine a very sweet and gentle spirit, partaking of a far other and nobler quality than that which is immediately drawn by distillation, and called dephlegmed aqua vitæ, from whose acrimony this other spirit is exceedingly remote; although the *sal circulatum* that makes these anatomies be separated from the analyzed bodies, in the same weight and with the same qualities it had before. Which affirmation of *Helmont*, if we admit to be true, we must acknowledge, that there may be a very great disparity betwixt bodies of the same denomination (as several oils, or several spirits) separable from compound bodies: for besides the differences I shall anon take notice of, betwixt those distilled oils that are commonly known to chymists, it appears by this, that by means of the *sal circulatum*, there may be quite another sort of oils obtained from the same body: and who knows, but that there may be yet other agents bound in nature, by whose help there may, whether by transmutation or otherwise, be obtained from the bodies vulgarly called mixt, oils or other substances, differing from those of the same denomination, known either to vulgar chymists, or even to *Helmont* himself? But for fear you should tell me, that this is but a conjecture grounded upon another man's relation, whose truth we have not the means to experiment, I will not insist upon it; but leaving you to consider of it at leisure, I shall proceed to what is next.

SECONDLY, then, if that be true, which was the opinion of *Leucippus*, *Democritus*, and other prime anatomists of old, and is in our days revived by no mean philosophers; namely, that our culinary fire, such as chymists use, consists of swarms of little bodies swiftly moving, which by their smallness and motion are able to permeate

* *Illud notabile, in vino esse spiritum quendam mitiorem ulterioris & nobilioris qualitatis participem, quam qui immediatè per distillationem elicitur, diciturque aqua vitæ dephlegmata, quod facilius in simplici olivarum oleo ad oculum spectatur. Quippe distillatum oleum absque laterum aut tegularum additamento, quodque oleum philosophorum dicitur, multum differt ab ejus oleitate, quæ elicitur prius reducto oleo simplici in partes dissimilares sola digestione & salis circulati Paracelsici appositione; siquidem sal circulatum idem in pondere & quantitatibus pristinis ab oleo segregatur, postquam oleum olivarum in sui heterogeneitates est dispositum. Dulce enim tunc oleum olivarum ex oleo, prout & suavissimus vini spiritus à vino hoc pacto separantur, longeque ab aquæ vitæ acrimonia distinctus.* Helmont. Aura vitalis, pag. 725.

the ſolideſt and compacteſt bodies, and even glaſs itſelf; if this (I ſay) be true, ſince we ſee, that in flints and other concretes, the fiery part is incorporated with the groſſer, it will not be irrational to conjecture, that multitudes of theſe fiery corpuſcles, getting in at the pores of the glaſs, may aſſociate themſelves with the parts of the mixt body whereon they work, and with them conſtitute new kinds of compound bodies, according as the ſhape, ſize, and other affections of the parts of the diſſipated body happen to diſpoſe them, in reference to ſuch combinations; of which alſo there may be the greater number, if it be likewiſe granted, that the corpuſcles of the fire, though all exceeding minute, and very ſwiftly moved, are not all of the ſame bigneſs, nor figure: and if I had not weightier conſiderations to diſcourſe to you of, I could name to you, to countenance what I have newly ſaid, ſome particular experiments, by which I have been induced to think, that the particles of an open fire working upon ſome bodies may really aſſociate themſelves therewith, and add to the quantity. But becauſe I am not ſure, that when the fire works upon bodies included in glaſſes, it does it by a real trajection of the fiery corpuſcles themſelves, through the ſubſtance of the glaſs, I will proceed to what is next to be mentioned.

I COULD (ſays *Eleutherius*) help you to ſome proofs, whereby I think it may be made very probable, that when the fire acts immediately upon a body, ſome of its corpuſcles may ſtick to thoſe of the burnt body, as they ſeem to do in quick-lime, but in greater numbers and more permanently. But for fear of retarding your progreſs, I ſhall deſire you to defer this inquiry till another time, and proceed as you intended.

You may then, in the next place (ſays *Carneades*) obſerve with me, that not only there are ſome bodies, as gold and ſilver, which do not by the uſual examens, made by fire, diſcover themſelves to be mixt; but if (as you may remember I formerly told you) it be a de-compound body, that is diſſipable into ſeveral ſubſtances, by being expoſed to the fire it may be reſolved into ſuch, as are neither elementary, nor ſuch as it was upon its laſt mixture compounded of; but into new kinds of mixts. Of this I have already given you ſome examples in ſoap, ſugar of lead, and vitriol. Now if we ſhall conſider, that there are ſome bodies, as well natural (as that I laſt named) as factitious, manifeſtly de-compounded; that in the bowels of the earth nature may, as we ſee ſhe ſometimes does, make ſtrange mixtures; that animals are nouriſhed with other animals and plants; and that theſe themſelves have almoſt all of them their nutriment and growth, either from a certain nitrous juice harboured in the pores of the earth, or from the excrements of animals, or from the putrified bodies, either of living creatures or vegetables, or from other ſubſtances of a compounded nature; if, I ſay, we conſider this, it may ſeem probable, that there may be among the works of nature (not to mention thoſe of art) a greater number of de-compound bodies, than men take notice of; and indeed, as I have formerly alſo obſerved, it does not at all appear, that all mixtures muſt be of elementary bodies; but it ſeems far more probable, that there are divers ſorts of compound bodies, even in regard of all or ſome of their ingredients, conſidered antecedently to their mixture. For though ſome ſeem to be made up by the immediate coalitions of the elements, or principles themſelves, and therefore may be called *prima miſta*, or *miſta primaria*; yet it ſeems, that many other bodies are mingled (if I may ſo ſpeak) at the ſecond hand, their immediate ingredients being not elementary, but theſe primary mixt newly ſpoken of; and from divers of thoſe ſecondary ſorts of mixt may reſult, by a further compoſition, a third ſort, and ſo onwards. Nor is it improbable, that ſome bodies are made up of mixt bodies, not all of the ſame order, but of ſeveral; as (for inſtance)

a concrete may consist of ingredients, whereof the one may have been a primary, the other a secondary mixt body; (as I have in native cinnaber, by my way of resolving it, found both that coarser part, that seems more properly to be ore, and a combustible sulphur, and a running mercury:) or perhaps without any ingredient of this latter sort, it may be composed of mixt bodies, some of them of the first, and some of the third kind. And this may perhaps be somewhat illustrated by reflecting upon what happens in some chymical preparations of those medicines which they call their bezoardicums. For first, they take antimony and iron, which may be looked upon as *prima mista*; of these they compound a starry regulus, and to this they add, according to their intention, either gold or silver, which makes with it a new and further composition. To this they add sublimate, which is itself a de-compound body (consisting of common quicksilver, and divers salts united by sublimation into a crystalline substance) and from this sublimate, and the other metalline mixtures, they draw a liquor, which may be allowed to be of a yet more compounded nature. If it be true, as chymists affirm it, that by this art some of the gold or silver mingled with the regulus may be carried over the helm with it by the sublimate; as indeed a skilful and candid person complained to me a while since, that an experienced friend of his and mine, having by such a way brought over a great deal of gold, in hope to do something further with it, which might be gainful to him, has not only missed of his aim, but is unable to recover his volatilized gold out of the antimonial butter, wherewith it is strictly united.

Now (continues *Carneades*) if a compound body consist of ingredients, that are not merely elementary; it is not hard to conceive, that the substances, into which the fire dissolves it, though seemingly homogeneous enough, may be of a compounded nature, those parts of each body, that are most of kin, associating themselves into a compound of a new kind. As when (for example sake) I have caused vitriol and sal armoniac, and salt-petre to be mingled and distilled together, the liquor, that came over, manifested itself not to be either spirit of nitre, or of sal armoniac, or of vitriol. For none of these would dissolve crude gold, which yet my liquor was able readily to do; and thereby manifested itself to be a new compound, consisting at least of spirit of nitre, and sal armoniac (for the latter dissolved in the former, will work on gold) which nevertheless are not by any known way separable, and consequently would not pass for a mixt body, if we ourselves did not, to obtain it, put and distil together divers concretes, whose distinct operations were known beforehand. And to add on this occasion the experiment I lately promised you, because it is applicable to our present purpose, I shall acquaint you, that suspecting the common oil of vitriol not to be altogether such a simple liquor, as chymists presume it, I mingled it with an equal or a double quantity (for I tried the experiment more than once) of common oil of turpentine, such as together with the other liquor I bought at the drugsters. And having carefully (for the experiment is nice, and somewhat dangerous) distilled the mixture in a small glass retort, I obtained, according to my desire, (besides the two liquors I had put in) a pretty quantity of a certain substance, which sticking all about the neck of the retort, discovered itself to be sulphur, not only by a very strong sulphureous smell, and by the colour of brimstone; but also by this, that being put upon a coal, it was immediately kindled, and burned like common sulphur. And of this substance I have yet by me some little parcels, which you may command and examine when you please. So that from this experiment I may deduce either one, or both of these propositions, that a real sulphur may be made by the conjunction of two such substances as chymists take for elementary, and which did not either

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of them apart appear to have any ſuch body in it; or, that oil of vitriol, though a diſtilled liquor, and taken for part of the ſaline principle of the concrete, that yields it, may yet be ſo compounded a body, as to contain, beſide its ſaline part, a ſulphur like common brimſtone, which would hardly be itſelf a ſimple or un-compounded body.

I MIGHT (purſues *Carneades*) remind you, that I formerly repreſented it as poſſible, that as there may be more elements than five, or ſix; ſo the elements of one body may be different from thoſe of another: whence it would follow, that from the reſolution of de-compound bodies there may reſult mixts of an altogether new kind, by the coalition of elements, that never perhaps convened before. I might, I ſay, mind you of this, and add divers things to this ſecond conſideration; but for fear of wanting time, I willingly pretermit them, to paſs on to the third, which is this, that the fire does not always barely reſolve or take aſunder, but may alſo after a new manner mingle and compound together the parts (whether elementary or not) of the body diſſipated by it.

THIS is ſo evident (ſays *Carneades*) in ſome obvious examples, that I cannot but wonder at their ſupineneſs, that have not taken notice of it. For when wood being burnt in a chimney is diſſipated by the fire into ſmoke and aſhes, that ſmoke compoſes ſoot, which is ſo far from being any one of the principles of the wood, that (as I noted above) you may by a further analyſis ſeparate five or ſix diſtinct ſubſtances from it. And as for the remaining aſhes, the chymiſts themſelves teach us, that by a further degree of fire they may be indiſſolubly united into glaſs. It is true, that the analyſis, which the chymiſts principally build upon, is made, not in the open air, but in cloſe veſſels; but however, the examples lately produced may invite you ſhrewdly to ſuſpect, that heat may as well compound as diſſipate the parts of mixt bodies: and not to tell you, that I have known a vitrification made even in cloſe veſſels, I muſt remind you, that the flowers of antimony, and thoſe of ſulphur, are very mixt bodies, though they aſcend in cloſe veſſels; and that it was in ſtopt glaſſes, that I brought up the whole body of camphire. And whereas it may be objected, that all theſe examples are of bodies forced up in a dry, not a fluid form, as are the liquors wont to be obtained by diſtillation; I anſwer, that beſides it is poſſible, that a body may be changed from conſiſtent to fluid, or from fluid to conſiſtent, without being otherwiſe much altered, as may appear by the eaſineſs, wherewith in winter, without any addition or ſeparation of viſible ingredients, the ſame ſubſtance may be quickly hardened into brittle ice, and thawed again into fluid water; beſides this, I ſay, it ſhould be conſidered, that common quickſilver itſelf, which the eminenteſt chymiſts confeſs to be a mixt body, may be driven over the helm in its priſtine form of quickſilver, and conſequently, in that of a liquor. And certainly it is poſſible, that very compounded bodies may concur to conſtitute liquors; ſince, not to mention, that I have found it poſſible, by the help of a certain menſtruum, to diſtil gold itſelf through a retort, even with a moderate fire; let us but conſider what happens in butter of antimony. For if that be carefully rectified, it may be reduced into a very clear liquor; and yet if you caſt a quantity of fair water upon it, there will quickly precipitate a ponderous and vomitive calx, which made before a conſiderable part of the liquor, and yet is indeed (though ſome eminent chymiſts would have it mercurial) an antimonial body, carried over and kept diſſolved by the ſalts of the ſublimate, and conſequently a compounded one; as you may find, if you will have the curioſity to examine this white powder by a ſkilful reduction. And that you may not think, that bodies, as compounded as flowers of brimſtone, cannot be

brought to concur to conſtitute diſtilled liquors; and alſo, that you may not imagine, with divers learned men, that pretend no ſmall ſkill in chymiſtry, that at leaſt no mixt body can be brought over the helm, but by corroſive ſalts; I am ready to ſhew you, when you pleaſe, among other ways of bringing over flowers of brimſtone (perhaps I might add, even mineral ſulphurs) ſome, wherein I employ none but oleaginous bodies to make volatile liquors, in which not only the colour, but (which is a much ſurer mark) the ſmell and ſome operations manifeſt, that there is brought over a ſulphur, that makes part of the liquor.

ONE thing more there is, *Eleutherius* (ſays *Carneades*) which is ſo pertinent to my preſent purpoſe, that, though I have touched upon it before, I cannot but on this occaſion take notice of it. And it is this; that the qualities or accidents, upon whoſe account chymiſts are wont to call a portion of matter by the name of mercury, or ſome other of their principles, are not ſuch, but that it is poſſible as great (and therefore why not the like?) may be produced by ſuch changes of texture, and other alterations, as the fire may make in the ſmall parts of a body. I have already proved, when I diſcourſed of the ſecond general conſideration, by what happens to plants nouriſhed only with fair water, and eggs hatched into chickens, that by changing the diſpoſition of the component parts of a body, nature is able to effect as great changes in a parcel of matter reputed ſimilar, as thoſe requiſite to denominate one of the *tria prima*. And, though *Helmont* do ſomewhere wittily call the fire the deſtructor and the artificial death of things; and, although another eminent chymiſt and phyſician be pleaſed to build upon this, that fire can never generate any thing but fire; yet you will, I doubt not, be of another mind, if you conſider, how many new ſorts of mixed bodies chymiſts themſelves have produced by means of the fire; and particularly, if you conſider, how that noble and permanent body, glaſs, is not only manifeſtly produced by the violent action of the fire, but has never, for aught we know, been produced any other way. And, indeed, it ſeems but an inconſiderate aſſertion of ſome Helmontians, that every ſort of body of a peculiar denomination muſt be produced by ſome ſeminal power; as I think I could evince, if I thought it ſo neceſſary, as it is for me to haſten to what I have further to diſcourſe. Nor need it much move us, that there are ſome, who look upon whatſoever the fire is employed to produce, not as upon natural, but artificial bodies. For there is not always ſuch a difference, as many imagine, betwixt the one and the other; nor is it ſo eaſy as they think, clearly to aſſign that, which properly, conſtantly, and ſufficiently, diſcriminates them. But not to engage myſelf in ſo nice a diſquiſition, it may now ſuffice to obſerve, that a thing is commonly termed artificial, when a parcel of matter is by the artificer's hand, or tools, or both, brought to ſuch a ſhape or form, as he deſigned before-hand in his mind: whereas in many of the chymical productions the effect would be produced, whether the artificer intended it or no; and is oftentimes very much other than he intended or looked for: and the inſtruments employed are not tools artificially faſhioned and ſhaped, like thoſe of tradeſmen, for this or that particular work; but, for the moſt part, agents of nature's own providing, and whoſe chief powers of operation they receive from their own nature or texture, not the artificer. And, indeed, the fire is as well a natural agent as ſeed: and the chymiſt that employs it, does but apply natural agents and patients, who being thus brought together, and acting according to their reſpective natures, perform the work themſelves; as apples, plums, or other fruit, are natural productions, though the gardener bring and faſten together the cions and the ſtock, and both water, and do perhaps divers other ways contribute to its bearing fruit. But, to proceed to what

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I was going to say; you may observe with me, *Eleutherius*, that, as I told you once before, qualities slight enough may serve to denominate a chymical principle. For, when they anatomize a compound body by the fire, if they get a substance inflammable, and that will not mingle with water, that they presently call sulphur; what is sapid and dissoluble in water, that must pass for salt; whatsoever is fixed and indissoluble in water, that they name earth. And I was going to add, that whatsoever volatile substance they know not what to make of, not to say, whatsoever they please, that they call mercury. But that these qualities may either be produced, otherwise than by such as they call seminal agents, or may belong to bodies of a compounded nature, may be shewn, among other instances, in glass made of ashes, where the exceeding strong-tasted alcalizate salt joining with the earth becomes insipid, and with it constitutes a body; which, though also dry, fixed and indissoluble in water, is yet manifestly a mixed body; and made so by the fire itself.

AND I remember to our present purpose, that *Helmont*, amongst other medicines that he commends, has a short process, wherein, though the directions for practice are but obscurely intimated, yet I have some reason not to disbelieve the process, without affirming or denying any thing about the virtues of the remedy to be made by it. *Quando* (says he) *oleum cinnamomi, &c. suo sali alkali miscetur absque omni aqua, trium mensium artificiosa occultaque circulatione, totum in salem volatilem commutatum est, vere essentiam sui simplicis in nobis exprimit, & usque in prima nostri constitutiva sese ingerit.* A not unlike process he delivers in another place; from whence, if we suppose him to say true, I may argue, that since by the fire there may be produced a substance, that is as well saline and volatile, as the salt of hartshorn, blood, *&c.* which pass for elementary; and since that this volatile salt is really compounded of a chymical oil and a fixed salt, the one made volatile by the other, and both associated by the fire, it may well be suspected, that other substances, emerging upon the dissipation of bodies by the fire, may be new sorts of mixts, and consist of substances of differing natures: and particularly, I have sometimes suspected, that since the volatile salts of blood, hartshorn, *&c.* are fugitive, and endowed with an exceeding strong smell, either that chymists do erroneously ascribe all odours to sulphurs, or that such salts consist of some oily parts well incorporated with the saline ones. And the like conjecture I have also made concerning spirit of vinegar, which though the chymists think one of the principles of that body, and though being an acid spirit it seems to be much less of kin, than volatile salts, to sulphurs; yet, not to mention its piercing smell, which I know not with what congruity the chymists will deduce from salt, I wonder they have not taken notice of what their own *Tyrocinium Chymicum* teaches us concerning the distillation of *saccharum Saturni*; out of which, *Beguinus* assures us, that he distilled, besides a very fine spirit, no less than two oils, the one blood-red and ponderous, but the other swimming upon the top of the spirit, and of a yellow colour; of which, he says, that he kept then some by him, to verify what he delivers. And though I remember not, that I have had two distinct oils from sugar of lead, yet that it will, though distilled without addition, yield some oil, disagrees not with my experience. I know the chymists will be apt to pretend, that these oils are but the volatilized sulphur of lead; and will perhaps argue it from what *Beguinus* relates, that when the distillation is ended, you'll find a *caput mortuum* extremely black, and (as he speaks) *nullius momenti*, as if the body, or at least the chief part of the metal itself, were by the distillation carried over the helm. But since you know, as well as I, that *saccharum Saturni* is a kind of magistery, made only by calcining of lead *per se*, dissolving it in distilled vinegar, and crystallizing the solution; if I had leisure to tell

Helmont, p. 411.

Tyroc. Chym. L. 1. C. 4.

tell you, how differing a thing I did, upon examination, find the *caput mortuum*, so slighted by *Beguinus*, to be, from what he represents it, I believe you would think the conjecture proposed less probable than one or other of these three; either that this oil did formerly concur to constitute the spirit of vinegar, and so that what passes for a chymical principle, may yet be further resoluble into distinct substances; or that some parts of the spirit, together with some parts of the lead, may constitute a chymical oil, which therefore, though it pass for homogeneous, may be a very compounded body; or at least, that by the action of the distilled vinegar and the saturnine calx one upon another, part of the liquor may be so altered, as to be transmuted from an acid spirit into an oil. And though the truth of either of the two former conjectures would make the example I have reflected on, more pertinent to my present argument; yet you'll easily discern, the third and last conjecture cannot be unserviceable to confirm some other passages of my discourse.

To return then to what I was saying just before I mentioned *Helmont*'s experiment; I shall subjoin, that chymists must confess also, that in the perfectly dephlegmed spirit of wine, or other fermented liquors, that, which they call the sulphur of the concrete, loses, by the fermentation, the property of oil, (which the chymists likewise take to be the true sulphur of the mixt) of being unmingleable with the water. And if you will credit * *Helmont*, a pound of the purest spirit of wine may barely, by the help of pure salt of tartar, (which is but the fixed salt of wine) be resolved or transmuted into scarce half an ounce of salt, and as much elementary water, as amounts to the remaining part of the mentioned weight. And it may (as I think I formerly also noted) be doubted, whether that fixed and alcalizate salt, which is so unanimously agreed on, to be the saline principle of incinerated bodies, be not, as it is alcalizate, a production of the fire? For though the taste of tartar, for example, seem to argue, that it contains a salt before it be burned; yet that salt being very acid, is of a quite differing taste from the lixiviate salt of calcined tartar. And though it be not truly objected against the chymists, that they obtain all salts they make, by reducing the body they work on into ashes with violent fires, (since hartshorn, amber, blood, and divers other mixts, yield a copious salt; before they be burned to ashes) yet this volatile salt differs much, as we shall see anon, from the fixed alcalizate salt I speak of; which, for aught I remember, is not producible by any known way, without incineration. It is not unknown to chymists, that quicksilver may be precipitated, without addition, into a dry powder, that remains so in water. And some eminent Spagyrists, and even *Raimund Lully* himself, teach, that meerly by the fire, quicksilver may in convenient vessels be reduced (at least in great part) into a thin liquor like water, and mingleable with it. So that by the bare action of the fire, it is possible, that the parts of a mixed body should be so disposed after new and differing manners, that it may be sometimes of one consistence, sometimes of another; and may in one state be disposed to be mingled with water, and in another not. I could also shew you, that bodies, from which apart chymists cannot obtain any thing, that is combustible, may, by being associated together, and by the help of the fire, afford an inflammable substance. And, that on the other side, it is possible for a body to be inflammable, from which it would very much puzzle any ordinary chymist, and perhaps any other, to separate an inflammable principle or ingredient, Wherefore, since the principles of chymists may receive

* *Ostendi dixi, quomodo lib. una aquæ vitæ combibita in sale tartari siccato, vix fiat semuncia salis, cæterum totum corpus fiat aqua elementalis.* Helmont. in Aura vitali.

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their denominations from qualities, which it often exceeds not the power of art, nor always that of the fire to produce; and since such qualities may be found in bodies, that differ so much in other qualities from one another, that they need not be allowed to agree in that pure and simple nature, which principles, to be so indeed, must have; it may justly be suspected, that many productions of the fire, that are shewed us by chymists, as the principles of the concrete, that afforded them, may be but a new kind of mixts. And to annex, on this occasion, to these arguments taken from the nature of the thing, one of those, which logicians call *ad hominem*, I shall desire you to take notice, that though *Paracelsus* himself, and some, that are so mistaken, as to think he could not be so, have ventured to teach, that not only the bodies here below, but the elements themselves, and all the other parts of the universe, are composed of salt, sulphur and mercury; yet the learned *Sennertus*, and all the more wary chymists, have rejected that conceit, and do many of them confess, that the *tria prima* are each of them made up of the four elements; and others of them make earth and water concur with salt, sulphur and mercury, to the constitution of mixt bodies. So that one sort of these Spagyrists, notwithstanding the specious titles they give to the productions of the fire, do in effect grant what I contend for. And, of the other sort I may well demand, to what kind of bodies the phlegm and dead earth, to be met with in chymical resolutions, are to be referred? For either they must say, with *Paracelsus*, but against their own concessions, as well as against experience, that these are also composed of the *tria prima*, whereof they cannot separate any one from either of them; or else they must confess, that two of the vastest bodies here below, earth and water, are neither of them composed of the *tria prima*; and that consequently those three are not the universal, and adequate ingredients, neither of all sublunary bodies, nor even of all mixed bodies.

I KNOW, that the chief of these chymists represent, that though the distinct substances into which they divide mixt bodies by the fire, are not pure and homogeneous; yet since the four elements, into which the Aristotelians pretend to resolve the like bodies by the same agent, are not simple neither, as themselves acknowledge, it is as allowable for the chymists to call the one *principles*, as for the Peripateticks to call the other *elements*; since in both cases the imposition of the name is grounded only upon the predominancy of that element, whose name is ascribed to it. Nor shall I deny, that this argument of the chymists is no ill one against the Aristotelians. But what answer can it prove to me, who, you know, am disputing as well against the Aristotelian elements, as the chymical principles, and must not look upon any body as a true principle or element, but as yet compounded, which is not perfectly homogeneous, but is further resoluble into any number of distinct substances, how small soever? And as for the chymists calling a body salt, or sulphur, or mercury, upon pretence, that the principle of the same name is predominant in it, that itself is an acknowledgment of what I contend for; namely, that these productions of the fire are yet compounded bodies. And yet whilst this is granted, it is affirmed, but not proved, that the reputed salt, or sulphur, or mercury, consists mainly of one body, that deserves the name of a principle of the same denomination. For how do chymists make it appear, that there are any such primitive and simple bodies in those we are speaking of; since it is upon the matter confessed by the answer lately made, that these are not such? And if they pretend by reason to evince what they affirm, what becomes of their confident boasts, that the chymist (whom they therefore, after *Beguinus*, call a *philosophus* or *opifex sensatus*) can convince our eyes, by manifestly shewing in any mixt body those simple substances he teaches them to be composed of?

And indeed, for the chymists to have recourse in this case to other proofs than experiments, as it is to wave the grand argument, that has all this while been given out for a demonstrative one; so it releases me from the obligation to prosecute a dispute, wherein I am not engaged to examine any but experimental proofs. I know it may plausibly enough be represented, in favour of the chymists, that it being evident, that much the greater part of any thing they call salt, or sulphur, or mercury, is really such; it would be very rigid to deny those substances the names ascribed them, only because of some slight mixture of another body, since not only the Peripateticks call particular parcels of matter elementary, though they acknowledge, that elements are not to be any where found pure, at least here below, and since especially there is a manifest analogy and resemblance betwixt the bodies obtainable by chymical anatomies, and the principles whose names are given them; I have, I say, considered that these things may be represented: but as for what is drawn from the custom of the Peripateticks, I have already told you, that though it may be employed against them, yet it is not available against me, who allow nothing to be an element, that is not perfectly homogeneous. And whereas it is alledged, that the predominant principle ought to give a name to the substance whorein it abounds; I answer, that that might much more reasonably be said, if either we or the chymists had seen nature take pure salt, pure sulphur, and pure mercury, and compound of them every sort of mixt bodies. But, since it is to experience that they appeal, we must not take it for granted, that the distilled oil (for instance) of a plant is mainly composed of the pure principle called sulphur, till they have given us an ocular proof, that there is in that sort of plants such an homogeneous sulphur. For as for the specious argument, which is drawn from the resemblance betwixt the productions of the fire, and the respective, either Aristotelian elements, or chymical prinicples, by whose names they are called; it will appear more plausible than cogent, if you will but recall to mind the state of the controversy; which is not, whether or no there be obtained from mixt bodies certain substances, that agree in outward appearance, or in some qualities, with quicksilver or brimstone, or some such obvious or copious body; but whether or no all bodies, confessed to be perfectly mixt, were composed of, and are resoluble into a determinate number of primary unmixt bodies. For, if you keep the state of the question in your eye, you will easily discern, that there is much of what should be demonstrated, left unproved by those chymical experiments we are examining. But (not to repeat what I have already discovered more at large) I shall now take notice, that it will not presently follow, that because a production of the fire has some affinity with some of the greater masses of matter here below, that therefore they are both of the same nature, and deserve the same name; for the chymists are not content, that flame be looked upon as a parcel of the element of fire, though it be hot, dry, and active, because it wants some other qualities belonging to the nature of elementary fire. Nor will they let the Peripateticks call ashes, or quicklime, earth, notwithstanding the many likenesses between them; because they are not tasteless, as elementary earth ought to be: but if you should ask me, what then it is, that all the chymical anatomies of bodies do prove, if they prove not, that they consist of the three principles into which the fire resolves them; I answer, that their dissections may be granted to prove, that some mixt bodies (for in many it will not hold) are by the fire, when they are included in close vessels, (for that condition also is often requisite) dissoluble into several substances differing in some qualities, but principally in consistence. So that out of most of them may be obtained a

fixt substance, partly saline, and partly insipid, an unctuous liquor, and another liquor, or more, that without being unctuous have a manifest taste. Now, if chymists will agree to call the dry and sapid substance, salt; the unctuous liquor sulphur; and the other, mercury; I shall not much quarrel with them for so doing: but if they will tell me, that salt, sulphur, and mercury are simple and primary bodies, whereof each mixt body was actually compounded, and which was really in it antecedently to the operation of the fire, they must give me leave to doubt, whether (whatever their other arguments may do) their experiments prove all this. And if they will also tell me, that the substances their anatomies are wont to afford them, are pure and similar, as principles ought to be, they must give me leave to believe my own senses, and their own confessions, before their bare assertions. And that you may not *(Eleutherius)* think I deal so rigidly with them, because I scruple to take these productions of the fire for such, as the chymists would have them pass for, upon the account of their having some affinity with them; consider a little with me, that in regard an element or principle ought to be perfectly similar and homogeneous, there is no just cause, why I should rather give the body proposed the name of this or that element or principle, because it has a resemblance to it in some obvious quality, rather than deny it that name upon the account of divers other qualities, wherein the proposed bodies are unlike; and if you do but consider, what slight and easily producible qualities they are, that suffice, as I have already more than once observed, to denominate a chymical principle or an element, you will not, I hope, think my wariness to be destitute either of example, or else of reason. For we see, that the chymists will not allow the Aristotelians, that the salt in ashes ought to be called earth, though the saline and terrestrial part symbolize in weight, in dryness, in fixedness and fusibility, only because the one is sapid and dissoluble in water, and the other not: besides, we see that sapidness and volatility are wont to denominate the chymists mercury or spirit; and yet how many bodies, think you, may agree in those qualities, which may yet be of very differing natures, and disagree in qualities either more numerous, or more considerable, or both? For not only spirit of nitre, aqua fortis, spirit of salt, spirit of oil of vitriol, spirit of alum, spirit of vinegar, and all saline liquors distilled from animal bodies, but all the acetous spirits of woods freed from their vinegar; all these, I say, and many others must belong to the chymists mercury, though it appear not, why some of them should more be comprehended under one denomination than the chymists sulphur, or oil, should likewise be; for their distilled oils are also fluid, volatile, and tasteable, as well as their mercury: nor is it necessary, that their sulphur should be unctuous or dissoluble in water, since they generally refer spirit of wine to sulphurs, although that spirit be not unctuous, and will freely mingle with water. So that bare inflammability must constitute the essence of the chymists sulphur; as uninflammableness, joined with any taste, is enough to intitle a distilled liquor to be their mercury. Now since I can further observe to you, that spirit of nitre and spirit of hartshorn being poured together will boil and hiss, and toss up one another into the air, which the chymists make signs of great antipathy in the natures of bodies, (as indeed these spirits differ much both in taste, smell, and operations) since I elsewhere tell you of my having made two sorts of oil out of the same man's blood, that would not mingle with one another; and since I might tell you divers examples I have met with, of the contrariety of bodies, which according to the chymists must be huddled up together under one denomination; I leave you to judge, whether such a multitude of substances, as may agree in these

ſlight qualities, and yet diſagree in others more conſiderable, are more worthy to be called by the name of a principle (which ought to be pure and homogeneous) than to have appellations given them, that may make them differ, in name too, from the bodies from which they ſo widely differ in nature. And hence alſo, by the by, you may perceive, that it is not unreaſonable to diſtruſt the chymiſts way of argumentation, when being unable to ſhew us, that ſuch a liquor is (for example) purely ſaline, they prove, that at leaſt ſalt is much the predominant principle, becauſe that the propoſed ſubſtance is ſtrongly taſted, and all taſte proceeds from ſalt; whereas thoſe ſpirits, ſuch as ſpirit of tartar, ſpirit of hartſhorn, and the like, which are reckoned to be the mercuries of the bodies, that afford them, have manifeſtly a ſtrong and piercing taſte, and ſo has (according to what I formerly noted) the ſpirit of box *&c.* even after the acid liquor that concurred to compoſe it has been ſeparated from it. And indeed, if ſapidneſs belong not to the ſpirit or mercurial principle of vegetables and animals, I ſcarce know how it will be diſcriminated from their phlegm; ſince by the abſence of inflammability it muſt be diſtinguiſhed from their ſulphur: which affords me another example, to prove how unaccurate the chymical doctrine is in our preſent caſe; ſince not only the ſpirit of vegetables and animals, but their oils are very ſtrongly taſted, as he, that ſhall but wet his tongue with chymical oil of cinnamon or of cloves, or even turpentine, may quickly find, to his ſmart. And not only I never tried any chymical oils, whoſe taſte was not very manifeſt and ſtrong; but a ſkilful and inquiſitive perſon, who made it his buſineſs by elaborate operations to depurate chymical oils, and reduce them to an elementary ſimplicity, informs us, that he never was able to make them at all taſteleſs: whence I might infer, that the proof chymiſts confidently give us of a body's being ſaline, is ſo far from demonſtrating the predominancy, that it does not clearly evince ſo much as the preſence of the ſaline principle in it. But I will not (purſues *Carneades)* remind you, that the volatile ſalt of hartſhorn, amber, blood *&c.* are exceeding ſtrongly ſcented, notwithſtanding that moſt chymiſts deduce odours from ſulphur, and from them argue the predominancy of that principle in the odorous body; becauſe I muſt not ſo much as add any new examples of the incompetency of this ſort of chymical arguments; ſince having already detained you but too long in thoſe generals, that appertain to my fourth conſideration, it is time, that I proceed to the particulars themſelves, to which I thought fit they ſhould be previous.

THESE generals (continues *Carneades)* being thus premiſed, we might the better ſurvey the unlikeneſs, that an attentive and unpreposſeſſed obſerver may take notice of in each ſort of bodies, which the chymiſts are wont to call the ſalts of ſulphurs or mercuries of the concretes that yield them, as if they had all a ſimplicity, and identity of nature: whereas ſalts, if they were all elementary, would as little differ as do the drops of pure and ſimple water. It is known, that both chymiſts and phyſicians aſcribe to the fixt ſalts of calcined bodies the virtues of their concretes; and conſequently very differing operations. So we find the alkali of wormwood much commended in diſtempers of the ſtomach; that of eye-bright for thoſe that have a weak ſight; and that of guaiacum (of which a great quantity yields but a very little ſalt) is not only much commended in venereal diſeaſes, but is believed to have a peculiar purgative virtue, which yet I have not had occaſion to try. And though, I confeſs, I have long thought, that theſe alkalizate ſalts are, for the moſt part, very near of kin, and retain very little of the properties of the concretes whence they were ſeparated; yet being minded to obſerve watchfully, whether I could meet with any exceptions

exceptions to this general obervation, I obſerved at the glaſs-houſe, that ſometimes the metal (as the workmen call it) or maſs of colliquated ingredients, which by blowing they faſhion into veſſels of divers ſhapes, did ſometimes prove of a very differing colour, and a ſomewhat differing texture, from what was uſual. And having inquired, whether the cauſe of ſuch accidents might not be derived from the peculiar nature of the fixt ſalt employed to bring the ſand to fuſion, I found, that the knowingeſt workmen imputed theſe miſ-adventures to the aſhes of ſome certain kind of wood, as having obſerved the ignobler kind of glaſs I lately mentioned to be frequently produced, when they had employed ſuch ſorts of aſhes, which therefore they ſcruple to make uſe of, if they took notice of them beforehand. I remember alſo, that an induſtrious man of my acquaintance having bought a vaſt quantity of tobacco-ſtalks to make a fixt ſalt with, I had the curioſity to go ſee, whether that exotick plant, which ſo much abounds in volatile ſalt, would afford a peculiar kind of alkali; and I was pleaſed to find, that in the lixivium of it it was not neceſſary, as is uſual, to evaporate all the liquor, that there might be obtained a ſaline calx, conſiſting like lime quenched in the air of a heap of little corpuſcles of unregarded ſhapes; but the fixt ſalt ſhot into figured cryſtal, almoſt as nitre or ſal armoniac and other uncalcined ſalts are wont to do. And I further remember, that I have obſerved, in the fixt ſalt of urine, brought by depuration to be very white, a taſte not ſo unlike to that of common ſalt, and very differing from the wonted cauſtick lixiviate taſte of the other ſalts made by incineration. But becauſe the inſtances I have alledged of the difference of alkalizate ſalt are but few, and therefore I am ſtill inclined to think, that moſt chymiſts and many phyſicians do, inconſiderately enough, and without warrant from experience, aſcribe the virtues of the concretes, expoſed to calcination, to the ſalts obtained by it; I ſhall rather, to ſhew the diſparity of ſalts, mention in the firſt place the apparent difference betwixt the vegetable fixt ſalts and the animal volatile ones: as (for example) betwixt ſalt of tartar, and ſalt of hartſhorn; whereof the former is ſo fixt, that it will indure the brunt of a violent fire, and ſtand in fuſion like a metal; whereas the other (beſides, that it has a differing taſte and a very differing ſmell) is ſo far far from being fixt, that it will fly away in a gentle heat as eaſily as ſpirit of wine itſelf. And to this I ſhall add, in the next place, that even among the volatile ſalts themſelves, there is a conſiderable difference, as appears by the diſtinct properties of (for inſtance) ſalt of amber, ſalt of urine, ſalt of man's ſkull, (ſo much extolled againſt the falling ſickneſs) and divers others, which cannot eſcape an ordinary obſerver. And this diverſity of volatile ſalts I have obſerved to be ſometimes diſcernable even to the eye, in their figures. For the ſalt of hartſhorn I have obſerved to adhere to the receiver, in the form almoſt of a parallelopipedon; and of the volatile ſalt of human blood (long digeſted before diſtillation, with ſpirit of wine) I can ſhew you ſtore of grains of that figure, which geometricians call a rhombus; though I dare not undertake, that the figures of theſe or other ſaline cryſtals (if I may ſo call them) will be always the ſame, whatever degree of fire have been employed to force them up, or how haſtily ſoever they been made to convene in the ſpirits or liquors, in the lower part of which, I have uſually obſerved them, after a while to ſhoot. And although, as I lately told you, I ſeldom found any difference, as to medical virtues, in the fixt ſalts of divers vegetables; and accordingly I have ſuſpected that moſt of theſe volatile ſalts, having ſo great a reſemblance in ſmell, in taſte, and fugitiveneſs, differ but little, if at all, in their medicinal properties: as indeed I have found them generally to agree in divers of

of them (as in their being ſomewhat diaphoretick and very deopilative) yet I remember * *Helmont* ſomewhere informs us, that there is this difference betwixt the ſaline ſpirit of urine and that of man's blood, that the former will not cure the epilepſy, but the latter will. Of the efficacy alſo of the ſalt of common amber againſt the ſame diſeaſe in children, (for in grown perſons it is not a ſpecifick) I may elſewhere have an occaſion to entertain you. And when I conſider, that to the obtaining of theſe volatile ſalts (eſpecially that of urine)there is not requiſite ſuch a deſtructive violence of the fire, as there is to get thoſe ſalts that muſt be made by incineration, I am the more invited to conclude, that they may differ from one another, and conſequently recede from an elementary ſimplicity. And if I could here ſhew you, what Mr. *Boyle* has obſerved, touching the various chymical diſtinctions of ſalts; you would quickly diſcern, not only that chymiſts do give themſelves a ſtrange liberty to call concretes ſalts, that are, according to their own rules, to be looked upon as very compounded bodies; but that among thoſe very ſalts, that ſeem elementary, becauſe produced upon the anatomy of the bodies, that yield them, there is not only a viſible diſparity, but, to ſpeak in the common language, a manifeſt antipathy or contrariety: as is evident in the ebullition and hiſſing, that is wont to enſue, when the acid ſpirit of vitriol, for inſtance, is poured upon pot-aſhes, or ſalt of tartar. And I ſhall beg leave of this gentleman, (ſays *Carneades)* caſting his eyes on me, to let me obſerve to you, out of ſome of his papers, particularly thoſe, wherein he treats of ſome preparations of urine, that not only one and the ſame body may have two ſalts of a contrary nature, as he exemplifies in the ſpirit and alcali of nitre; but that from the ſame body there may, without addition, be obtained three differing and viſible ſalts. For he relates, that he obſerved in urine, not only a volatile and cryſtalline ſalt, and a fixt ſalt, but likewiſe a kind of ſal armoniac, or ſuch a ſalt as would ſublime in the form of a ſalt, and therefore was not fixt, and yet was far from being ſo fugitive as the volatile ſalt; from which it ſeemed alſo otherwiſe to differ. I have indeed ſuſpected, that this may be a ſal armoniac properly enough ſo called, as compounded of the volatile ſalt of urine, and the fixt of the ſame liquor, which, as I noted, is not unlike ſea-ſalt; but that itſelf argues a manifeſt difference betwixt the ſalts, ſince ſuch a volatile ſalt is not wont to unite thus with an ordinary alkali, but to fly away from it in the heat. And on this occaſion I remember, that to give ſome of my friends an ocular proof of the difference betwixt the fixt and volatile ſalt of (the ſame concrete) wood, I deviſed the following experiment: I took common Venetian ſublimate, and diſſolved as much of it as I well could in fair water; then I took wood-aſhes, and pouring on them warm water, diſſolved their ſalt; and filtrating the water, as ſoon as I found the lixivium ſufficiently ſharp upon the tongue, I reſerved it for uſe. Then on one part of the former ſolution of ſublimate dropping a little of this diſſolved fixt ſalt of wood, the liquors preſently turned of an orange colour; but upon the other part of the clear ſolution of ſublimate putting ſome of the volatile ſalt of wood (which abounds in the ſpirit of ſoot) the liquor immediately turned white, almoſt like milk, and after a while let fall a white ſediment, as the other liquor did a yellow one. To all this that I have ſaid concerning the difference of ſalts, I might add what I formerly told you, concerning the ſimple ſpirit of box, and ſuch like woods, which differ much from the other

* *Error vero per diſtillationem nobis monſtrat etiam ſpiritum ſalinum plane volatilem odore nequicquam ut nec guſtu deſtinguibilem a ſpiritu urinæ; in eo tamen eſſentialiter diverſum, quod ſpiritus talis cruoris curat epilepſiam, non autem ſpiritus ſalis lotii.* Helmont. Aura Vitalis

ſalts

ſalts hitherto mentioned, and yet would belong to the ſaline principle, if chymiſts did truly teach, that all taſtes proceed from it. And I might alſo annex, what I noted to you out of * *Helmont* concerning bodies, which, though they conſiſt in great part of chymical oils, do yet appear but volatile ſalts: but to inſiſt on theſe things, were to repeat; and therefore I ſhall proceed.

THIS diſparity is alſo highly eminent in the ſeparated ſulphurs or chymical oils of things. For they contain ſo much of the ſcent, and taſte, and vertues, of the bodies, whence they were drawn, that they ſeem to be but the material craſis (if I may ſo ſpeak) of their concretes. Thus the oils of cinnamon, cloves, nutmegs and other ſpices, ſeem to be but the united aromatic parts, that did enoble thoſe bodies. And it is a known thing, that oil of cinnamon, and oil of cloves, (which I have likewiſe obſerved in the oils of ſeveral woods) will ſink to the bottom of water; whereas thoſe of nutmegs and divers other vegetables will ſwim upon it. The oil (abuſively called ſpirit) of roſes ſwims at the top of the water in the form of a white butter, which I remember not to have obſerved in any other oil drawn in any limbeck; yet there is a way (not here to be declared) by which I have ſeen it come over in the form of other aromatic oils, to the delight and wonder of thoſe that beheld it. In oil of aniſeeds, which I drew both with, and without fermentation, I obſerved the whole body of the oil in a cool place to thicken into the conſiſtence and appearance of white butter, which, with the leaſt heat, reſumed its former liquidneſs. In the oil of olive drawn over in a retort, I have likewiſe more than once ſeen a ſpontaneous coagulation in the receiver: and I have of it by me thus congealed; which is of ſuch a ſtrangely penetrating ſcent, as if it would perforate the noſes that approach it. The like pungent odour I alſo obſerved in the diſtilled liquor of common ſoap, which forced over from minium, lately afforded an oil of a moſt admirable penetrancy: and he muſt be a great ſtranger, both to the writings and preparations of chymiſts, that ſees not in the oils they diſtill from vegetables and animals, a conſiderable and obvious difference. Nay, I ſhall venture to add, *Eleutherius*, (what perhaps you will think of kin to a paradox, that divers times out of the ſame animal or vegetable, there may be extracted oils of natures obviouſly differing. To which purpoſe I ſhall not inſiſt on the ſwimming and ſinking oils, which I have ſometimes obſerved to float on, and ſubſide under the ſpirit of guaiacum, and that of divers other vegetables diſtilled with a ſtrong and laſting fire; nor ſhall I inſiſt on the obſervation elſewhere mentioned, of the divers and unmingleable oils afforded us by human blood, long fermented and digeſted with ſpirit of wine; becauſe theſe kinds of oils may ſeem chiefly to differ in conſiſtence and weight, being all of them high coloured and aduſt. But the experiment, which I deviſed to make out this difference of the oils of the ſame vegetable, *ad oculum*, (as they ſpeak) was this that follows: I took a pound of aniſeeds, and having groſly beaten them, cauſed them to be put into a very large glaſs retort almoſt filled with fair water; and placing this retort in a ſand-furnace, I cauſed a very gentle heat to be adminiſtered during the firſt day, and a great part of the ſecond, till the water was for the moſt part drawn off, and had brought over with it at leaſt moſt of the volatile and aromatic oil of the ſeeds. And then increaſing the fire, and changing the receiver, I obtained beſides an empyreumatical ſpirit, and a quantity of aduſt oil; whereof a little floated upon the ſpirit, and the reſt was more heavy,

* *Aliquando oleum cinnamomi, &c. ſuo ſali alkali miſcetur abſque omni aqua, trium menſium artificioſa occultaque circulatione, totum in ſalem volatilem commutatum eſt.* Helmont. Tria Prima Chymicorum, &c, pag. 412.

and

and not easily separable from it. And whereas these oils were very dark, and smelled (as the chymists speak) so strongly of the fire, that their odour did not betray from what vegetables they had been forced; the other aromatic oil was enriched with the genuine smell and taste of the concrete; and spontaneously coagulating itself into white butter, did manifest itself to be the true oil of aniseeds: which concrete I therefore chose to employ about this experiment, that the difference of these oils might be more conspicuous than it would have been, had I, instead of it, distilled another vegetable.

I HAD almost forgot to take notice, that there is another sort of bodies, which though not obtained from concretes by distillation, many chymists are wont to call their sulphur; not only because such substances are, for the most part, high coloured (whence they are also, and that more properly, called tinctures) as dissolved sulphurs are wont to be; but especially because they are, for the most part, abstracted and separated from the rest of the mass by spirit of wine: which liquor those men supposing to be sulphureous, they conclude, that what it works upon, and abstracts, must be a sulphur also. And upon this account they presume, that they can sequester the sulphur even of minerals and metals; for which it is known, that they cannot by fire alone separate it. To all this I shall answer, that if these sequestered substances were indeed the sulphurs of the bodies, whence they are drawn, there would as well be a great disparity betwixt chymical sulphurs obtained by spirit of wine, as I have already shewn there is betwixt those obtained by distillation in the form of oils: which will be evident from hence, that not to urge, that themselves ascribe distinct virtues to mineral tincture of gold against such and such diseases; the tincture of antimony, or of its glass, against others; and the tincture of emerald against others; it is plain, that in tinctures drawn from vegetables, if the superfluous spirit of wine be distilled off, it leaves at the bottom that thicker substance, which chymists use to call the extract of the vegetable. And that these extracts are endowed with very different qualities, according to the nature of the particular bodies, that afforded them (though I fear seldom with so much of the specific virtues, as is wont to be imagined) is freely confessed both by physicians and chymists. But, *Eleutherius* (says *Carneades*) we may here take notice, that the chymists do as well in this case, as in many others, allow themselves a licence to abuse words: for not again to argue from the differing properties of tinctures, that they are not exactly pure and elementary sulphurs; they would easily appear not to be so much as sulphurs, although we should allow chymical oils to deserve that name. For however in some mineral tinctures, the natural fixedness of the extracted body does not always suffer it to be easily further resoluble into differing substances; yet in very many extracts drawn from vegetables, it may very easily be manifested, that the spirit of wine has not sequestered the sulphureous ingredient from the saline and mercureal ones, but has dissolved (for I take it to be a solution) the finer parts of the concrete (without making any nice distinction of their being perfectly sulphureous or not) and united itself with them into a kind of magistery, which consequently must contain ingredients or parts of several sorts. For we see, that the stones, that are rich in vitriol, being often drenched with rain-water, the liquor will then extract a fine and transparent substance coagulable into vitriol; and yet though this vitriol be readily dissoluble in water, it is not a true elementary salt, but, as you know, a body resoluble into very differing parts, whereof one (as I shall have occasion to tell you anon) is yet of a metalline, and consequently not of an elementary nature. You may consider also, that common sulphur is readily dis-

ſoluble in oil of turpentine, though notwithſtanding its name it abounds as well, if not as much, in ſalt as in true ſulphur; witneſs the great quantity of ſaline liquor it affords, being ſet to flame away under a glaſs bell. Nay, I have, which perhaps you will think ſtrange, with the ſame oil of turpentine alone eaſily enough diſſolved crude antimony finely powdered into a blood red balſam, wherewith perhaps conſiderable things may be performed in ſurgery. And if it were now requiſite, I could tell you of ſome other bodies (ſuch as perhaps you would not ſuſpect) that I have been able to work upon with certain chymical oils. But inſtead of digreſſing further, I ſhall make this uſe of the example I have named. That it is not unlikely, but that ſpirit of wine, which by its pungent taſte, and by ſome other qualities, that argue it better (eſpecially its reducibleneſs, according to *Helmont*, into alcali, and water) ſeems to be as well of a ſaline as of a ſulphureous nature, may well be ſuppoſed capable of diſſolving ſubſtances, that are not merely elementary ſulphurs, though perhaps they may abound with parts, that are of kin thereunto. For I find, that ſpirit of wine will diſſolve gum lacca, benzoin, and the reſinous parts of jallap, and even of guaiacum; whence we may well ſuſpect, that it may from ſpices, herbs, and other leſs compacted vegetables, extract ſubſtances, that are not perfect ſulphurs, but mixt bodies. And to put it paſt diſpute, there is many a vulgar extract drawn with ſpirit of wine, which committed to diſtillation will afford ſuch differing ſubſtances, as will loudly proclaim it to have been a very compounded body. So that we may juſtly ſuſpect, that even in mineral tinctures it will not always follow, that becauſe a red ſubſtance is drawn from the concrete by ſpirit of wine, that ſubſtance is its true and elementary ſulphur. And though ſome of theſe extracts may perhaps be inflammable; yet, beſides that others are not, and beſides that their being reduced to ſuch minuteneſs of parts may much facilitate their taking fire; beſides this, I ſay, we ſee, that common ſulphur, common oil, gum lac, and many unctuous and reſinous bodies, will flame well enough, though they be of very compounded natures: nay, travellers of unſuſpected credit aſſure us, as a known thing, that in ſome northern countries, where fir-trees and pines abound, the poorer ſort of inhabitants uſe long ſplinters of thoſe reſinous woods to burn inſtead of candles. And as for the redneſs wont to be met with in ſuch ſolutions, I could eaſily ſhew, that it is not neceſſary it ſhould proceed from the ſulphur of the concrete, diſſolved by the ſpirit of wine; if I had leiſure to manifeſt, how much chymiſts are wont to delude themſelves and others, by the ignorance of thoſe other cauſes, upon whoſe account ſpirit of wine and other menſtruums may acquire a red or ſome other high colour. But to return to our chymical oils, ſuppoſing that they were exactly pure; yet I hope they would be, as the beſt ſpirit of wine is, but the more inflammable and deflagrable. And therefore, ſince an oil can be by the fire alone immediately turned into flame, which is ſomething of a very differing nature from it; I ſhall demand, how this oil can be a primogeneal and incorruptible body, as moſt chymiſts would have their principles; ſince it is further reſoluble into flame, which, whether or no it be a portion of the element of fire, as an Ariſtotelian would conclude, is certainly ſomething of a very differing nature from a chymical oil, ſince it burns, and ſhines, and mounts ſwiftly upwards; none of which a chymical oil does, whilſt it continues ſuch. And if it ſhould be objected, that the diſſipated parts of this flaming oil may be caught and collected again into oil or ſulphur; I ſhall demand, what chymiſt appears to have ever done it: and without examining, whether it may not hence be as well ſaid, that ſulphur is but compacted fire, as that fire is but diffuſed ſulphur, I ſhall leave you to conſider, whether

whether it may not hence be argued, that neither fire nor sulphur are primitive and indestructible bodies. And I shall further observe, that at least it will hence appear, that a portion of matter may, without being compounded with new ingredients, by having the texture and motion of its small parts changed, be easily, by the means of the fire, endowed with new qualities, more differing from them it had before, than are those, which suffice to discriminate the chymists principles from one another.

WE are next to consider, whether in the anatomy of mixt bodies, that, which chymists call the mercurial part of them, be uncompounded, or no. But to tell you true, though chymists do unanimously affirm, that their resolutions discover a principle, which they call mercury, yet I find them to give of it descriptions so differing, and so ænigmatical, that I, who am not ashamed to confess, that I cannot understand what is not sense, must acknowledge to you, that I know not what to make of them. *Paracelsus* himself, and therefore, as you will easily believe, many of his followers, does somewhere call that mercury, which ascends upon the burning of wood, as the Peripatetics are wont to take the same smoke for air; and so seems to define mercury by volatility, or (if I may coin such a word) effumability. But since, in this example, both volatile salt and sulphur make part of the smoke, which does indeed consist also both of phlegmatic and terrene corpuscles, this notion is not to be admitted; and I find, that the more sober chymists themselves disavow it. Yet to shew you, how little of clearness we are to expect in the accounts even of later Spagyrists, be pleased to take notice, that *Beguinus*, even in his *Tyrocinium Chymicum*, written for the instruction of novices, when he comes to tell us what are meant by the *tria prima*, which for their being principles ought to be defined the more accurately and plainly, gives us this description of mercury; *Mercurius* (says he) *est liquor ille acidus, permeabilis, penetrabilis, æthereus, ac purissimus, à quo omnis nutricatio, sensus, motus, vires, colores, senectutisque præproperæ retardatio.* Which words are not so much a definition of it, as an encomium: and yet *Quercetanus*, in his description of the same principle, adds to these divers other epithets. But both of them, to skip very many other faults, that may be found with their metaphorical descriptions, speak incongruously to the chymists own principles. For if mercury be an acid liquor, either hermetical philosophy must err in ascribing all tastes to salt, or else mercury must not be a principle, but compounded of a saline ingredient and somewhat else. *Libavius*, though he finds great fault with the obscurity of what the chymists write concerning their mercurial principle, does yet but give us such a negative description of it, as *Sennertus*, how favourable soever to the *tria prima*, is not satisfied with. And this *Sennertus* himself, though the learnedest champion for the hypostatical principles, does almost as frequently, as justly, complain of the unsatisfactorinesse of what the chymists teach concerning their mercury; and yet he himself (but with his wonted modesty) substitutes instead of the description of *Libavius*, another, which many readers, especially if they be not Peripatetics, will not know what to make of. For scarce telling us any more, than that in all bodies, that, which is found besides salt and sulphur, and the elements, or, as they call them, phlegm and dead earth, is that spirit, which, in *Aristotle*'s language, may be called ἐσία ἀνάλογος τῶν ἄνω στοιχείῳ· he says that, which I confess is not at all satisfactory to me, who do not love to seem to acquiesce in any man's mystical doctrines, that I may be thought to understand them.

Chym. Tyrocin. lib. 1. cap. 1.

IF (says *Eleutherius*) I durst presume, that the same thing would be thought clear by me, and those, that are fond of such cloudy expressions, as you justly tax the chymists for, I should venture to offer to consideration, whether or no, since the mercurial principle, that arises from distillation, is unanimously asserted to be distinct from

the ſalt and ſulphur of the ſame concrete, that may not be called the mercury of a body, which though it aſcend in diſtillation, as do the phlegm and ſulphur, is neither inſipid like the former, nor inflammable like the latter. And therefore I would ſubſtitute to the too much abuſed name of mercury, the more clear and familiar appellation of ſpirit, which is alſo now very much made uſe of, even by the chymiſts themſelves of our times, though they have not given us ſo diſtinct an explication, as were fit, of what may be called the ſpirit of a mixt body.

I SHOULD not perhaps (ſays *Carneades*) much quarrel with your notion of mercury. But as for the chymiſts, what they can mean, with congruity to their own principles, by the mercury of animals and vegetables, it will not be ſo eaſy to find out; for they aſcribe taſtes only to the ſaline principle, and conſequently would be much put to it to ſhew what liquor it is, in the reſolution of bodies, that not being inſipid, for that they call phlegm, neither is inflammable as oil or ſulphur, nor has any taſte; which, according to them, muſt proceed from a mixture, at leaſt, of ſalt. And if we ſhould take ſpirit in the ſenſe of the word received among modern chymiſts and phyſicians, for any diſtilled liquor, that is neither phlegm nor oil, the appellation would yet appear ambiguous enough. For plainly, that which firſt aſcends in the diſtillation of wine and fermented liquors, is generally as well by chymiſts as others reputed a ſpirit. And yet pure ſpirit of wine being wholly inflammable, ought, according to them, to be reckoned to the ſulphureous, not the mercureal principle. And among the other liquors, that go under the name of ſpirits, there are divers, which ſeem to belong to the family of ſalts; ſuch as are the ſpirit of nitre, vitriol, ſea-ſalt and others, and even the ſpirit of hartſhorn, being as I have tried, in great part, if not totally, reducible into ſalt and phlegm, may be ſuſpected to be but a volatile ſalt, diſguiſed by the phlegm mingled with it into the form of a liquor; however, if this be a ſpirit, it manifeſtly differs very much from that of vinegar, the taſte of the one being acid, and the other ſalt; and their mixture, in caſe they be very pure, ſometimes occaſioning an effervescence like that of thoſe liquors the chymiſts account moſt contrary to one another. And even among thoſe liquors, that ſeem to have a better title, than thoſe hitherto mentioned, to the name of ſpirits, there appears a ſenſible diverſity; for ſpirit of oak, for inſtance, differs from that of tartar, and this from that of box, or of guaiacum. And in ſhort, even theſe ſpirits, as well as other diſtilled liquors, manifeſt a great diſparity betwixt themſelves, either in their actions on our ſenſes, or in their other operations.

AND (continues *Carneades*) beſides this diſparity, that is to be met with among thoſe liquors, that the moderns call ſpirits, and take for ſimilar bodies, what I have formerly told you concerning the ſpirit of box-wood, may let you ſee, that ſome of thoſe liquors not only have qualities very differing from others, but may be further reſolved into ſubſtances differing from one another.

AND ſince many modern chymiſts and other naturaliſts are pleaſed to take the mercurial ſpirit of bodies for the ſame principle, under differing names, I muſt invite you to obſerve, with me, the great difference, that is conſpicuous betwixt all the vegetable and animal ſpirits I have mentioned, and running mercury. I ſpeak not of that, which is commonly ſold in ſhops, that many of themſelves will confeſs to be a mixt body; but of that, which is ſeparated from metals, which by ſome chymiſts, that ſeem more philoſophers than the reſt, and eſpecially by the above mentioned *Claveus*, is (for diſtinction ſake) called *mercurius corporum*. Now this metalline liquor being one of thoſe three principles, of which mineral bodies are, by Spagyriſts, affirmed to be compoſed, and to be reſoluble into them, the many notorious differences betwixt them

them and the mercuries, as they call them, of vegetables and animals, will allow me to infer, either that minerals and the other two sorts of mixt bodies consist not of the same elements, or that those principles, whereunto minerals are immediately resolved, which chymists with great ostentation shew us as the true principles of them, are but secondary principles, or mixts of a peculiar sort, which must be themselves reduced to a very differing form, to be of the same kind with vegetable and animal liquors.

BUT this is not all; for though I formerly told you, how little credit there is to be given to the chymical processes, commonly to be met with, of extracting the mercuries of metals, yet I will now add, that supposing, that the more judicious of them do not untruly affirm, that they have really drawn true and running mercury from several metals (which I wish they had clearly taught us how to do also) yet it may be still doubted, whether such extracted mercuries do not as well differ from common quicksilver, and from one another, as from the mercuries of vegetables and animals. *Claveus* *, in his apology, speaking of some experiments, whereby metalline mercuries may be fixt into the nobler metals, adds, that he spake of the mercuries drawn from metals; because common quicksilver, by reason of its excessive coldness and moisture, is unfit for that particular kind of operation; for which, though a few lines before he prescribes in general the mercuries of metalline bodies, yet he chiefly commends that drawn by art from silver. And elsewhere, in the same book, he tells us, that he himself tried, that by bare coction the quicksilver of tin or pewter *(argentum vivum ex stanno prolicitum)* may, by an efficient cause (as he speaks) be turned into pure gold. And the experienced *Alexander van Suchten* somewhere tells us, that by a way he intimates may be made a mercury of copper, not of the silver colour of other mercuries, but green; to which I shall add, that an eminent person, whose name his travels and learned writings have made famous, lately assured me, that he had more than once seen the mercury of lead (which whatever authors promise, you will find it very difficult to make, at least in any considerable quantity) fixt into perfect gold. And being by me demanded, whether or no any other mercury would not as well have been changed by the same operations, he assured me of the negative.

AND since I am fallen upon the mention of the mercuries of metals, you will, perhaps, expect *(Eleutherius)* that I should say something of their two other principles; but I must freely confess to you, that what disparity there may be between the salts and sulphurs of metals or other minerals, I am not myself experienced enough in the separations and examens of them, to venture to determine: (for, as for the salts of metals, I formerly represented it as a thing much to be questioned, whether they have any at all.) And for the processes of separation I find in authors, if they were (what many of them are not) successfully practicable, as I noted above, yet they are to be performed by the assistance of other bodies, so hardly, if upon any terms at all, separable from them, that it is very difficult to give the separated principles all their due, and no more. But the sulphur of antimony, which is vehemently vomitive, and the strongly scented anodyne sulphur of vitriol, inclines me to think, that not only mineral sulphurs differ from vegetable ones, but also from one another, retaining much of the nature of their concretes. The salts of metals, and of some sort of minerals, you will easily guess (by the doubts I formerly expressed, whether me-

* *Dixi autem de argento vivo à metallis prolicito, quod vulgare ob nimiam frigiditatem et humiditatem nimium concretioni est contumax, nec ab auro solum alteratio exercere potest.* Gasp. Clave. in Apol.

tals

tals have any salt at all) that I have not been so happy as yet to see, perhaps not for want of curiosity. But if *Paracelsus* did always write so consentaneously to himself, that his opinion were confidently to be collected from every place of his writings, where he seems to express it, I might safely take upon me to tell you, that he both countenances in general, what I have delivered in my fourth main consideration, and in particular warrants me to suspect, that there may be a difference in metalline and mineral salts, as well as we find it in those of other bodies. For, *Sulphur* (says he) *aliud in auro, aliud in argento, aliud in ferro, aliud in plumbo, stanno, &c. sic aliud in saphro, aliud in smaragdo, aliud in rubino, chrysolitho, amethysto, magnete, &c. item aliud in lapidibus, silice, salibus, fontibus, &c. Nec vero tot sulphura tantum, sed & totidem salia; sal aliud in metallis, aliud in gemmis, aliud in lapidibus, aliud in salibus, aliud in vitriolo, aliud in alumine: similis etiam mercurii est ratio. Alius in metallis, alius in gemmis, &c. Ita ut unicuique speciei suus peculiaris mercurius sit. Et tamen res saltem tres sunt; una essentia est sulphur; una est sal; una est mercurius. Addo quod & specialius adhuc singula dividantur; aurum enim non unum, sed multiplex, ut & non unum pyrum, pomum, sed idem multiplex, totidem etiam sulphura auri, salia auri, mercurii auri; idem competit etiam metallis & gemmis; ut quod saphyri præstantiores, læviores, &c. tot etiam saphyrica sulphura, saphyrica salia, saphyrici mercurii, &c. Idem verum etiam est de turconibus & gemmis aliis universis.* From which passage, *Eleutherius*, I suppose you will think I might without rashness conclude, either that my opinion is favoured by that of *Paracelsus*, or that *Paracelsus* his opinion was not always the same. But because in divers other places of his writings he seems to talk at a differing rate of the three principles and the four elements, I shall content myself to infer from the alleged passage, that if his doctrine be not consistent with that part of mine, which it is brought to countenance, it is very difficult to know what his opinion concerning salt, sulphur, and mercury, was; and that consequently we had reason, about the beginning of our conferences, to decline taking upon us, either to examine or oppose it.

Paracelf. de Mineral. Tract 1. pag. 141.

I know not whether I should on this occasion add, that those very bodies, the chymists call phlegm and earth, do yet recede from an elementary simplicity. That common earth and water frequently do so, notwithstanding the received contrary opinion, is not denied by the more wary of the modern Peripatetics themselves: and certainly most earths are much less simple bodies than is commonly imagined even by chymists, who do not so considerately, to prescribe and employ earths promiscuously in those distillations, that require the mixture of some *caput mortuum*, to hinder the flowing together of the matter, and to retain its grosser parts. For I have found some earths to yield, by distillation, a liquor very far from being inodorous or insipid; and it is a known observation, that most kinds of fat earth kept covered from the rain, and hindered from spending themselves in the production of vegetables, will in time become impregnated with salt-petre.

But I must remember, that the water and earths I ought here to speak of, are such as are separated from mixt bodies by the fire; and therefore to restrain my discourse to such, I shall tell you, that we see the phlegm of vitriol (for instance) is a very effectual remedy against burns; and I know a very famous and experienced physician, whose unsuspected secret (himself confessed to me) it is, for the discussing of hard and obstinate tumours. The phlegm of vinegar, though drawn exceeding leisurely in a digesting furnace, I have purposely made trial of; and sometimes found it able to draw, though slowly, a saccarine sweetness out of lead; and as I remember, by long digestion, I dissolved corals in it. The phlegm of the sugar of Saturn is said to have very peculiar properties. Divers eminent chymists teach, that it will dissolve pearls,

pearls, which being precipitated by the ſpirit of the ſame concrete, are thereby (as they ſay) rendered volatile; which has been confirmed to me, upon his own obſervation, by a perſon of great veracity. The phlegm of wine, and indeed divers other liquors, that are indiſcriminately condemned to be caſt away as phlegm, are endowed with qualities, that make them differ both from meer water, and from each other. And whereas the chymiſts are pleaſed to call the *caput mortuum* of what they have diſtilled (after they have, by affuſion of water, drawn away its ſalt) *terra damnata*, or earth, it may be doubted, whether or no thoſe earths are all of them perfectly alike; and it is ſcarce to be doubted, but that there are ſome of them, which remain yet unreduced to an elementary nature. The aſhes of wood, deprived of all the ſalt, and bone-aſhes, or calcined hartſhorn, which refiners chooſe to make teſts of, as freeſt from ſalt, ſeem unlike: and he that ſhall compare either of theſe inſipid aſhes to lime, and much more to the calx of talc (though by the affuſion of water they be exquiſitely dulcified) will perhaps ſee cauſe to think them things of a ſomewhat different nature. And it is evident in colcothar, that the exacteſt calcination, followed by an exquiſite dulcification, does not always reduce the remaining body into elementary earth; for after the ſalt or vitriol (if the calcination have been too faint) is drawn out of the colcothar, the reſidue is not earth, but a mixt body, rich in medical virtues (as experience has informed me) and which *Angelus Sala* affirms to be partly reducible into malleable copper: which I judge very probable; for though, when I was making experiments upon colcothar, I was deſtitute of a furnace capable of giving a heat intenſe enough to bring ſuch a calx to fuſion; yet having conjectured, that if colcothar abounded with that metal, aqua fortis would find it out there, I put ſome dulcified colcothar into that menſtruum, and found the liquor, according to my expectation, preſently coloured as highly, as if it had been an ordinary ſolution of copper.

THE

SCEPTICAL CHYMIST:

PART V.

HERE *Carneades* making a pauſe, I muſt not deny (ſays his friend to him) that I think you have ſufficiently proved, that theſe diſtinct ſubſtances, which chymiſts are wont to obtain from mixt bodies, by their vulgar diſtillation, are not pure and ſimple enough to deſerve, in rigour of ſpeaking, the name of elements, or principles. But I ſuppoſe you have heard, that there are ſome modern Spagyriſts, who give out, that they can, by further and more ſkilful purifications, ſo reduce the ſeparated ingredients of mixt bodies to an elementary ſimplicity, that the oils (for inſtance) extracted from all mixts ſhall as perfectly reſemble one another, as the drops of water do.

If you remember (replies *Carneades*) that at the beginning of our conference with *Philoponus*, I declared to him before the rest of the company, that I would not engage myself at present to do any more, than examine the usual proofs alledged by chymists, for the vulgar doctrine of their three hypostatical principles; you will easily perceive, that I am not obliged to make answer to what you newly proposed; and that it rather grants, than disproves what I have been contending for: since by pretending to make so great a change in the reputed principles, that distillation affords the common Spagyrists, it is plainly enough pre-supposed, that before such artificial depurations be made, the substances to be made more simple were not yet simple enough to be looked upon as elementary: wherefore in case the artists you speak of could perform what they give out they can, yet I should not need to be ashamed of having questioned the vulgar opinion touching the *tria prima*. And as to the thing itself, I shall freely acknowledge to you, that I love not to be forward in determining things to be impossible, till I know and have considered the means, by which they are proposed to be effected. And therefore I shall not peremptorily deny, either the possibility of what these artists promise, or my assent to any just inference, however destructive to my conjectures, that may be drawn from their performances. But give me leave to tell you withal, that because such promises are wont (as experience has more than once informed me) to be much more easily made, than made good by chymists, I must with-hold my belief from their assertions, till their experiments exact it; and must not be so easy as to expect before-hand an unlikely thing, upon stronger inducements than are yet given me: besides that I have not yet found by what I have heard of these artists, that though they pretend to bring the several substances, into which the fire has divided the concrete, to an exquisite simplicity, they pretend also to be able by the fire to divide all concretes, minerals, and others, into the same number of distinct substances. And in the mean time I must think it improbable, that they can either truly separate as many differing bodies from gold (for instance) or ostiocolla, as we can do from wine, or vitriol; or that the mercury (for example) of gold or Saturn would be perfectly of the same nature with that of hartshorn; and that the sulphur of antimony would be but numerically different from the distilled butter or oil of roses.

But suppose (says *Eleutherius*) that you should meet with chymists, who would allow you to take in earth and water into the number of the principles of mixt bodies; and being also content to change the ambiguous name of mercury for that more intelligible one of spirit, should consequently make the principles or compound bodies to be five; would you not think it something hard to reject so plausible an opinion, only because the five substances, into which the fire divides mixt bodies, are not exactly pure, and homogeneous? For my part (continues *Eleutherius*) I cannot but think it somewhat strange, in case this opinion be not true, that it should fall out so luckily, that so great a variety of bodies should be analyzed by the fire into just five distinct substances; which so little differing from the bodies, that bear those names, may so plausibly be called oil, spirit, salt, water, and earth.

The opinion you now propose (answers *Carneades*) being another than that I was engaged to examine, it is not requisite for me to debate at this present; nor should I have leisure to do it throughly. Wherefore, I shall only tell you in general, that though I think this opinion in some respects more defensible than that of the vulgar chymists; yet you may easily enough learn from the past discourse, what may be thought of it: since many of the objections made against the vulgar doctrine of the chymists seem, without much alteration, employable against this hypothesis also. For,

besides that this doctrine does, as well as the other, take it for granted (what is not easy to be proved) that the fire is the true and adequate analyzer of bodies, and that all the distinct substances obtainable from a mixt body by the fire, were so pre-existent in it, that they were but extricated from each other by the analysis; besides that this opinion too ascribes to the productions of the fire an elementary simplicity, which I have shewn not to belong to them; and besides that this doctrine is liable to some of the other difficulties, wherewith that of the *tria prima* is incumbered: besides all this, I say, this quinary number of elements (if you pardon the expression) ought at least to have been restrained to the generality of animal and vegetable bodies, since not only among these there are some bodies (as I formerly argued) which, for aught yet has been made to appear, do consist, either of fewer or more similar substances, than precisely five. But in the mineral kingdom, there is scarce one concrete, that has been evinced to be adequately divisible into such five principles or elements, and neither more nor less, as this opinion would have every mixt body to consist of.

AND this very thing (continues *Carneades*) may serve to take away or lessen your wonder, that just so many bodies as five should be found upon the resolution of concretes. For since we find not, that the fire can make any such analysis (into five elements) of metals and other mineral bodies, whose texture is more strong and permanent, it remains, that the five substances under consideration be obtained from vegetable and animal bodies, which (probably by reason of their looser contexture) are capable of being distilled. And as to such bodies, it is natural enough, that, whether we suppose, that there are, or are not, precisely five elements, there should ordinarily occur in the dissipated parts, a five-fold diversity of scheme (if I may so speak.) For, if the parts do not remain all fixed, as in gold, calcined talc, *&c.* nor all ascend, as in the sublimation of brimstone, camphire, *&c.* but after their dissipation do associate themselves into new schemes of matter; it is very likely, that they will, by the fire, be divided into fixed and volatile; (I mean, in reference to that degree of heat, by which they are distilled) and those volatile parts will, for the most part, ascend either in a dry form, which chymists are pleased to call, if they be tasteless, flowers; if sapid, volatile salt; or in a liquid form. And this liquor must be either inflammable, and so pass for oil; or not inflammable, and yet subtile and pungent, which may be called spirit; or else strengthless or insipid, which may be named phlegm, or water. And as for the sixth part, or *caput mortuum*, it will most commonly consist of corpuscles, partly soluble in water, or sapid (especially if the saline parts were not so volatile, as to fly away before) which make up its fixt salt; and partly insoluble and insipid, which therefore seems to challenge the name of earth. But although upon this ground one might easily enough have foretold, that the differing substances obtained from a perfectly mixt body by the fire would for the most part be reducible to the five newly-mentioned states of matter; yet it will not presently follow, that these five distinct substances were simple and primogeneal bodies, so pre-existent in the concrete, that the fire does but take them asunder. Besides that it does not appear, that all mixt bodies (witness, gold, silver, mercury, *&c.*) nay, nor perhaps all vegetables, which may appear by what we said above of camphire, benzoin, *&c.* are resoluble by fire into just such differing schemes of matter. Nor will the experiments, formerly alleged, permit us to look upon these separated substances as elementary or uncompounded. Neither will it be a sufficient argument of their being bodies, that deserve the names, which chymists are pleased to give them, that they have an analogy in point of consistence, or either volatility or fixed-

 ness,

nefs, or elfe fome other obvious quality, with the fuppofed principles, whofe names are afcribed to them. For, as I told you above, notwithftanding this refemblance in fome one quality, there may be fuch a difparity in others, as may be more fit to give them differing appellations, than the refemblance is to give them one and the fame. And indeed it feems but fomewhat a grofs way of judging of the nature of bodies, to conclude without fcruple, that thofe muft be of the fame nature, that agree in fome fuch general quality, as fluidity, drynefs, volatility, and the like: fince each of thofe qualities, or ftates of matter, may comprehend a great variety of bodies, otherwife of a very differing nature; as we may fee in the calxes of gold, of vitriol, and of Venetian talc, compared with common afhes; which yet are very dry, and fixed by the vehemence of the fire, as well as they. And as we may likewife gather from what I have formerly obferved, touching the fpirit of box-wood, which though a volatile, fapid, and not inflammable liquor, as well as the fpirits of hartfhorn, of blood and others, (and therefore has been hitherto called the fpirit, and efteemed for one of the principles of the wood that affords it) may yet, as I told you, be fubdivided into two liquors, differing from one another, and one of them at leaft, from the generality of other chymical fpirits.

BUT you may yourfelf, if you pleafe, (purfues *Carneades)* accommodate to the hypothefis you propofed, what other particulars you fhall think applicable to it in the foregoing difcourfe. For I think it unfeafonable for me to meddle now any further with a controverfy, which, fince it does not now belong to me, leaves me at liberty to take my own time to declare myfelf about it.

ELEUTHERIUS perceiving, that *Carneades* was fomewhat unwilling to fpend any more time upon the debate of this opinion, and having perhaps fome thoughts of taking hence a rife to make him difcourfe it more fully another time, thought not fit as then to make any further mention to him of the propofed opinion; but told him,

I PRESUME I need not mind you, *Carneades*, that both the patrons of the ternary number of principles, and thofe that would have five elements, endeavour to back their experiments with a fpecious reafon or two; and efpecially fome of thofe embracers of the opinion laft named (whom I have converfed with, and found them learned men) affign this reafon of the neceffity of five diftinct elements; that otherwife mixt bodies could not be fo compounded and tempered, as to obtain a due confiftence and competent duration. For falt (fay they) is the bafis of folidity, and permanency in compound bodies, without which the other four elements might indeed be variouffy and loofely blended together, but would remain incompacted; but that falt might be diffolved into minute parts, and conveyed to the other fubftances to be compacted by it, and with it, there is a neceffity of water. And that the mixture may not be too hard and brittle, a fulphureous or oily principle muft intervene, to make the mafs more tenacious: to this a mercurial fpirit muft be furperadded; which by its activity may for a while permeate, and, as it were, leaven the whole mafs, and thereby promote the more exquifite mixture and incorporation of the ingredients. To all which, laftly, a portion of earth muft be added, which by its drynefs and porofity may foak up part of that water, wherein the falt was diffolved, and eminently concur with the other ingredients to give the whole body the requifite confiftence.

I PERCEIVE (fays *Carneades* fmiling) that if it be true, as it was lately noted from the proverb, *That good wits have bad memories*, you have that title, as well as a better, to a place among the good wits. For you have already more than once forgot, that

that I declared to you, that I would at this conference examine only the experiments of my adverſaries, not their ſpeculative reaſons. Yet it is not (ſubjoins *Carneades*) for fear of meddling with the argument you have propoſed, that I decline the examining it at preſent. For if when we are more at leiſure, you ſhall have a mind, that we may ſolemnly conſider of it together; I am confident we ſhall ſcarce find it inſoluble. And in the mean time we may obſerve, that ſuch a way of arguing may, it ſeems, be ſpeciouſly accommodated to differing hypotheſes. For I find, that *Beguinus*, and other aſſertors of the *tria prima*, pretend to make out by ſuch a way the requiſiteneſs of their ſalt, ſulphur and mercury, to conſtitute mixt bodies, without taking notice of any neceſſity of an addition of water and earth.

AND indeed neither ſort of chymiſts ſeem to have duly conſidered, how great variety there is in the textures and conſiſtences of compound bodies; and how little the conſiſtence and duration of many of them ſeem to accommodate and be explicable by the propoſed notion. And not to mention thoſe almoſt incorruptible ſubſtances obtainable by the fire, which I have proved to be ſomewhat compounded, and which the chymiſts will readily grant not to be perfectly mixt bodies; (not to mention theſe, I ſay) if you will but recall to mind ſome of thoſe experiments, whereby I ſhewed you, that out of common water not only mixt bodies (but even living ones) of very differing conſiſtences, and reſoluble by fire into as many principles as other bodies acknowledged to be perfectly mixt, may be produced; if you do this, I ſay, you will not, I ſuppoſe, be averſe from believing, that nature, by a convenient diſpoſition of the minute parts of a portion of matter, may contrive bodies durable enough, and of this, or that, or the other conſiſtence, without being obliged to make uſe of all, much leſs of any determinate quantity of each of the five elements, or of the three principles to compound ſuch bodies of. And I have (purſues *Carneades*) ſomething wondered, chymiſts ſhould not conſider, that there is ſcarce any body in nature ſo permanent and indiſſoluble as glaſs; which yet themſelves teach us may be made of bare aſhes, brought to fuſion by the meer violence of the fire; ſo that, ſince aſhes are granted to conſiſt but of pure ſalt and ſimple earth, ſequeſtered from all the other principles or elements, they muſt acknowledge, that even art itſelf can of two elements only, or if you pleaſe, one principle and one element, compound a body more durable than almoſt any in the world. Which being undeniable, how will they prove, that nature cannot compound mixt bodies, and even durable ones, under all the five elements or material principles?

BUT to inſiſt any longer on this occaſional diſquiſition, touching their opinion, that would eſtabliſh five elements, were to remember as little as you did before, that the debate of this matter is no part of my firſt undertaking; and conſequently, that I have already ſpent time enough in what I look back upon but as a digreſſion, or at beſt an excurſion.

AND thus, *Eleutherius*, (ſays *Carneades*) having at length gone through the four conſiderations I propoſed to diſcourſe unto you, I hold it not unfit, for fear my having inſiſted ſo long on each of them may have made you forget their ſeries, briefly to repeat them by telling you, that

SINCE, in the firſt place, it may juſtly be doubted, whether or no the fire be, as chymiſts ſuppoſe it, the genuine and univerſal reſolver of mixt bodies;

SINCE we may doubt, in the next place, whether or no all the diſtinct ſubſtances, that may be obtained from a mixt body by the fire, were pre-exiſtent there, in the forms, in which they were ſeparated from it;

SINCE alſo, though you ſhould grant the ſubſtances ſeparable from mixt bodies by the fire, to have been their component ingredients, yet the number of ſuch ſubſtances does not appear the ſame in all mixt bodies; ſome of them being reſoluble into more differing ſubſtances than three, and others not being reſoluble into ſo many as three;

AND ſince, laſtly, thoſe very ſubſtances, that are thus ſeparated, are not, for the moſt part, pure and elementary bodies, but new kinds of mixts;

SINCE, I ſay, theſe things are ſo, I hope you will allow me to infer, that the vulgar experiments (I might perchance have added, the arguments too) wont to be alledged by chymiſts to prove, that their three hypoſtatical principles do adequately compoſe all mixt bodies, are not ſo demonſtrative, as to induce a wary perſon to acquieſce in their doctrine, which, till they explain and prove it better, will, by its perplexing darkneſs, be more apt to puzzle than ſatisfy conſidering men, and will to them appear incumbered with no ſmall difficulties.

AND from what has been hitherto deduced (continues *Carneades*) we may learn, what to judge of the common practice of thoſe chymiſts, who, becauſe they have found, that divers compound bodies (for it will not hold in all) can be reſolved into, or rather can be brought to afford two or three differing ſubſtances more than the ſoot and aſhes, whereunto the naked fire commonly divides them in our chimneys, cry up their own ſect for the invention of a new philoſophy; ſome of them, as *Helmont*, *&c.* ſtyling themſelves philoſophers by the fire; and the moſt part not only aſcribing, but, as far as in them lies, engroſſing to thoſe of their ſect the title of Philoſophers.

BUT alas, how narrow is this philoſophy, that reaches but to ſome of thoſe compound bodies, which we find but upon, or in the cruſt or outſide of our terreſtrial globe, which is itſelf but a point in compariſon of the vaſt extended univerſe, of whoſe other and greater parts the doctrine of the *tria prima* does not give us an account! For what does it teach us, either of the nature of the ſun, which aſtronomers affirm to be eight ſcore and odd times bigger than the whole earth? or of that of thoſe numerous fixt ſtars, which, for aught we know, would very few, if any of them, appear inferior in bulk and brightneſs to the ſun, if they were as near us as he? What does the knowing that ſalt, ſulphur and mercury, are the principles of mixt bodies, inform us of the nature of that vaſt, fluid, and ætherial ſubſtance, that ſeems to make up the interſtellar, and conſequently much the greateſt part of the world? For as for the opinion commonly aſcribed to *Paracelſus*, as if he would have not only the four Peripatetick elements, but even the celeſtial parts of the univerſe, to conſiſt of his three principles, ſince the modern chymiſts themſelves have not thought ſo groundleſs a conceit worth their owning, I ſhall not think it worth my confuting.

BUT I ſhould, perchance, forgive the hypotheſis I have been all this while examining, if, though it reaches but to a very little part of the world, it did at leaſt give us a ſatisfactory account of thoſe things, to which it is ſaid to reach. But I find not, that it gives us any other than a very imperfect information even about mixt bodies themſelves: for how will the knowledge of the *tria prima* diſcover to us the reaſon, why the load-ſtone draws a needle, and diſpoſes it to reſpect the poles, and yet ſeldom preciſely points at them? How will this hypotheſis teach us, how a chick is formed in the egg, or how the ſeminal principles of mint, pompions, and other vegetables, that I mentioned to you above, can faſhion water into various plants, each

each of them endowed with its peculiar and determinate ſhape, and with divers ſpecifick and diſcriminating qualities? How does this hypotheſis ſhew us, how much ſalt, how much ſulphur, and how much mercury muſt be taken to make a chick or a pompion? And if we know that, what principle is it, that manages theſe ingredients, and contrives (for inſtance) ſuch liquors, as the white and yolk of an egg into ſuch a variety of textures, as is requiſite to faſhion the bones, veins, arteries, nerves, tendons, feathers, blood, and other parts of a chick? and not only to faſhion each limb, but to connect them all together, after that manner, that is moſt congruous to the perfection of the animal, which is to conſiſt of them? For to ſay, that ſome more fine and ſubtile part of either, or all the hypoſtatical principles is the director in all this buſineſs, and the architect of all this elaborate ſtructure, is to give one occaſion to demand again, what proportion and way of mixture of the *tria prima* afforded this architectonick ſpirit, and what agent made ſo ſkilful and happy a mixture? And the anſwer to this queſtion, if the chymiſts will keep themſelves within their three principles, will be liable to the ſame inconvenience, that the anſwer to the former was. And if it were not to intrench upon the theme of a friend of ours here preſent, I could eaſily proſecute the imperfections of the vulgar chymiſts philoſophy, and ſhew you, that by going about to explicate by their three principles, I ſay not, all the abſtruſe properties of mixt bodies, but even ſuch obvious and more familiar phænomena as fluidity and firmneſs, the colours and figures of ſtones, minerals, and other compound bodies, the nutrition of either plants or animals, the gravity of gold or quickſilver compared with wine or ſpirit of wine; by attempting, I ſay, to render a reaſon of theſe (to omit a thouſand others as difficult to account for) from any proportion of the three ſimple ingredients, chymiſts will be much more likely to diſcredit themſelves and their hypotheſis, than ſatisfy an intelligent inquirer after truth.

BUT (interpoſes *Eleutherius*) this objection ſeems no more than may be made againſt the four Peripatetick elements; and indeed almoſt againſt any other hypotheſis, that pretends by any determinate number of material ingredients to render a reaſon of the phænomena of nature. And as for the uſe of the chymical doctrine of the three principles, I ſuppoſe you need not be told by me, that the great champion of it, the learned *Sennertus*, aſſigns this noble uſe of the *tria prima*, that from them, as the neareſt and moſt proper principles, may be deduced and demonſtrated the properties, which are in mixt bodies, and which cannot be proximately (as they ſpeak) deduced from the elements. And this, ſays he, is chiefly apparent, when we inquire into the properties and faculties of medicines. And I know (continues *Eleutherius*) that the perſon you have aſſumed, of an opponent of the hermetick doctrine, will not ſo far prevail againſt your native and wonted equity, as to keep you from acknowledging, that philoſophy is much beholden to the notions and diſcoveries of chymiſts.

Senn. de Conf. & Differ. p. 165.

IF the chymiſts you ſpeak of (replies *Carneades*) had been ſo modeſt, or ſo diſcreet, as to propoſe their opinion of the *tria prima*, but as a notion uſeful among others, to increaſe human knowledge, they had deſerved more of our thanks, and leſs of our oppoſition: but ſince the thing, that they pretend, is not ſo much to contribute a notion toward the improvement of philoſophy, as to make this notion (attended by a few leſs conſiderable ones) paſs for a new philoſophy itſelf; nay, ſince they boaſt ſo much of this fancy of theirs, that the famous *Quercetanus* ſcruples not to write, that if his moſt certain doctrine of the three principles were ſufficiently learned, examined, and cultivated, it would eaſily diſpel all the darkneſs, that benights our

minds, and bring in a clear light, that would remove all difficulties; this school affording theorems and axioms irrefragable, and to be admitted without dispute by impartial judges, and so useful withal, as to exempt us from the necessity of having recourse, for want of the knowledge of causes, to that sanctuary of the ignorant, occult qualities: since I say, this domestick notion of the chymists is so much overvalued by them, I cannot think it unfit, they should be made sensible of their mistake; and be admonished to take in more fruitful and comprehensive principles, if they mean to give us an account of the phænomena of nature; and not confine themselves, and (as far as they can) others, to such narrow principles, as I fear will scarce enable them to give an account (I mean an intelligible one) of the tenth part (I say not) of all the phænomena of nature; but even of all such, as by the Leucippian, or some of the other sorts of principles, may be plausibly enough explicated. And though I be not unwilling to grant, that the incompetency I impute to the chymical hypothesis is but the same, which may be objected against that of the four elements, and divers other doctrines that have been maintained by learned men; yet since it is the chymical hypothesis only, which I am now examining, I see not why, if what I impute to it be a real inconvenience, either it should cease to be so, or I should scruple to object it, because other theories are liable thereunto, as well as the hermetical. For I know not, why a truth should be thought less a truth for the being fit to overthrow variety of errors.

I AM obliged to you (continues *Carneades*, a little smiling) for the favourable opinion you are pleased to express of my equity, if there be no design in it. But I need not be tempted by an artifice, or invited by a compliment, to acknowledge the great service, that the labours of chymists have done the lovers of useful learning; nor even, on this occasion, shall their arrogance hinder my gratitude. But since we are as well examining the truth of their doctrine, as the merit of their industry, I must, in order to the investigation of the first, continue a reply, to talk at the rate of the part I have assumed; and tell you, that when I acknowledge the usefulness of the labours of Spagyrists to natural philosophy, I do it upon the score of their experiments, not upon that of their speculations; for it seems to me, that their writings, as their furnaces, afford as well smoke as light; and do little less obscure some subjects, than they illustrate others. And though I am unwilling to deny, that it is difficult for a man to be an accomplished naturalist, that is a stranger to chymistry; yet I look upon the common operations and practices of chymists, almost as I do on the letters of the alphabet, without whose knowledge it is very hard for a man to become a philosopher; and yet that knowledge is very far from being sufficient to make him one.

BUT (says *Carneades*, resuming a more serious look) to consider a little more particularly what you alledge in favour of the chymical doctrine of the *tria prima*, though I shall readily acknowledge it not to be unuseful, and that the devisers and embracers of it have done the commonwealth of learning some service, by helping to destroy that excessive esteem, or rather veneration, wherewith the doctrine of the four elements was almost as generally, as undeservedly entertained; yet what has been alledged concerning the usefulness of the *tria prima*, seems to me liable to no contemptible difficulties.

AND first, as for the very way of probation, which the more learned and more sober champions of the chymical cause employed to evince the chymical principles in mixt bodies, it seems to me to be far enough from being convincing. This grand and leading argument, your *Sennertus* himself, who lays great weight upon it, and tells

tells us, that the moſt learned philoſophers employ this way of reaſoning to prove the moſt important things, propoſes thus: *Ubicunque* (ſays he) *pluribus eædem affectiones & qualitates inſunt, per commune quoddam principium inſint neceſſe eſt, ſicut omnia ſunt gravia propter terram, calida propter ignem. At colores, odores, ſapores, eſſe* φλογιστὸν, *& ſimilia alia, mineralibus, metallis, gemmis, lapidibus, plantis, animalibus inſunt. Ergo per commune aliquod principium & ſubjectum inſunt. At tale principium non ſunt elementa. Nullam enim habent ad tales qualitates producendas potentiam. Ergo alia principia, unde fluant, inquirenda ſunt.*

IN the recital of this argument, (ſays *Carneades*) I therefore thought fit to retain the language, wherein the author propoſes it, that I might alſo retain the propriety of ſome Latin terms, to which I do not readily remember any that fully anſwer in Engliſh. But as for the argumentation itſelf, it is built upon a precarious ſuppoſition, that ſeems to me neither demonſtrable nor true; for how does it appear, that where the ſame quality is to be met with in many bodies, it muſt belong to them upon the account of ſome one body whereof they all partake? (for that the major of our author's argument is to be underſtood of the material ingredients of bodies, appears by the inſtances of earth and fire he annexes to explain it.) For to begin with that very example, which he is pleaſed to alledge for himſelf; how can he prove, that the gravity of all bodies proceeds from what they participate of the element of earth, ſince we ſee, that not only common water, but the more pure diſtilled rain-water is heavy; and quickſilver is much heavier than earth itſelf, though none of my adverſaries has yet proved, that it contains any of that element. And I the rather make uſe of this example of quickſilver, becauſe I ſee not how the aſſertors of the elements will give any better account of it than the chymiſts. For if it be demanded, how it comes to be fluid, they will anſwer, that it participates much of the nature of water. And indeed, according to them, water may be the predominant element in it, ſince we ſee, that ſeveral bodies, which by diſtillation afford liquors, that weigh more than their *caput mortuum*, do not yet conſiſt of liquor enough to be fluid. Yet if it be demanded, how quickſilver comes to be ſo heavy, then it is replied, that it is by reaſon of the earth, that abounds in it; but ſince, according to them, it muſt conſiſt alſo of air, and partly of fire, which they affirm to be light elements, how comes it, that it ſhould be ſo much heavier than earth of the ſame bulk, though to fill up the poroſities and other cavities it be made up into a maſs or paſte with water, which itſelf they allow to be a heavy element. But to return to our Spagyriſts, we ſee, that chymical oils and fixt ſalts, though never ſo exquiſitely purified and freed from terreſtrial parts, do yet remain ponderous enough. And experience has informed me, that a pound (for inſtance) of ſome of the heavieſt woods, as guaiacum, that will ſink in water, being burnt to aſhes will yield a much leſs weight of them (whereof I found but a ſmall part to be alkalizate) than much lighter vegetables: as alſo that the black charcoal of it will not ſink as did the wood, but ſwim; which argues, that the differing gravity of bodies proceeds chiefly from the particular texture, as is manifeſt in gold, the cloſeſt and compacteſt of bodies, which is many times heavier than we can poſſibly make any parcel of earth of the ſame bulk. I will not examine, what may be argued touching the gravity, or quality analogous thereunto, of even celeſtial bodies, from the motion of the ſpots about the ſun, and from the appearing equality of the ſuppoſed ſeas in the moon; nor conſider, how little thoſe phænomena would agree with what *Sennertus* preſumes concerning gravity. But further to invalidate his ſuppoſition, I ſhall demand, upon what chymical principle fluidity depends? And yet fluidity is, two or three perhaps excepted, the moſt diffuſed

diffused quality of the universe, and far more general than almost any other of those, that are to be met with in any of the chymical principles, or Aristotelian elements; since not only the air, but that vast expansion we call heaven, in comparison of which, our terrestrial globe (supposing it were all solid) is but a point, and perhaps too, the sun and the fixt stars, are fluid bodies. I demand also, from which of the chymical principles, motion flows; which yet is an affection of matter much more general than any, that can be deduced from any of the three chymical principles? I might ask the like question concerning light, which is not only to be found in the kindled sulphur of mixt bodies, but (not to mention those sorts of rotten woods, and rotten fish, that shine in the dark) in the tails of living glow-worms, and in the vast bodies of the sun and stars. I would gladly also know, in which of the three principles, the quality, we call sound, resides as in its proper subject; since either oil falling upon oil, or spirit upon spirit, or salt upon salt, in a great quantity, and from a considerable height, will make a noise, or, if you please, create a sound, and (that the objection may reach the Aristotelians) so will also water upon water, and earth upon earth. And I could name other qualities to be met with in divers bodies, of which I suppose my adversaries will not in haste assign any subject, upon whose account it must needs be, that the quality belongs to all the other several bodies.

AND, before I proceed any further, I must here invite you to compare the supposition we are examining, with some other of the chymical tenets. For first they do in effect teach, that more than one quality may belong to, and be deduced from, one principle. For, they ascribe to salt, tastes, and the power of coagulation; to sulphur, as well odours as inflammableness; and some of them ascribe to mercury, colours; as all of them do effumability, as they speak. And on the other side, it is evident, that volatility belongs in common to all the three principles, and to water too. For it is manifest, that chymical oils are volatile; that also divers salts emerging upon the analysis of many concretes, are very volatile, is plain from the fugitiveness of salt, of hartshorn, flesh, *&c.* ascending in the distillation of those bodies. How easily water may be made to ascend in vapours, there is scarce any body, that has not observed. And as for what they call the mercurial principle of bodies, that is so apt to be raised in the form of steam, that *Paracelsus* and others define it by that aptness to fly up; so that (to draw that inference by the way) it seems not, that chymists have been accurate in their doctrine of qualities, and their respective principles, since they both derive several qualities from the same principle, and must ascribe the same quality to almost all their principles and other bodies besides. And thus much for the first thing taken for granted, without sufficient proof, by your *Sennertus:* and to add that upon the by (continues *Carneades)* we may hence learn, what to judge of the way of argumentation, which that fierce champion of the Aristotelians against the chymists, *Anthonius Guntherus Billichius* employs, where he pretends to prove against *Beguinus*, that not only the four elements do immediately concur to constitute every mixt body, and are both present in it, and obtainable from it, upon its dissolution; but that in the *tria prima* themselves, whereunto chymists are wont to resolve mixt bodies, each of them clearly discovers itself to consist of four elements. The ratiociation itself (pursues *Carneades)* being somewhat unusual, I did the other day transcribe it, and (says he, pulling a paper out of his pocket) it is this: *Ordinamur, cum Beguino, à ligno viridi, quod si concremetur, videbis in sudore aquam, in fumo aërem, in flamma & prunis ignem, terram in cineribus: quod si Beguino placuerit ex eo colligere humidum aquosum, cohibere humidum oleaginosum, extrahere ex cineribus salem;*

In Thesab radicum, cap. 10. pag. 73, 74

ego

ego ipsi in unoquoque horum seorsim quatuor elementa ad oculum demonstrabo, eodem artificio, quo in ligno viridi ea demonstravi. Humorem aquosum admovebo igni. Ipse aquam ebullire videbit, in vapore aërem conspiciet, ignem sentiet in æstu, plus minus terræ in sedimento apparebit. Humor porro oleaginosus aquam humiditate & fluiditate per se, accensus vero ignem flamma prodit, fumo aërem, fuligine, nidore, & amurca terram. Salem denique ipse Beguinus siccum vocat & terrestrem, qui tamen nec fusus aquam, nec caustica vi ignem celare potest; ignis vero violentia in halitus versus nec ab aëre se alienum esse demonstrat; idem de lacte, de ovis, de semine lini, de garyophillis, de nitro, de sale marino, denique de antimonio, quod fuit de ligno viridi judicium eadem de illorum partibus, quas Beguinus adducit, sententia, quæ de viridis ligni humore aquoso, quæ de liquore ejusdem oleoso, quæ de sale fuit.

THIS bold discourse (resumes *Carneades*, putting up again his paper) I think it were not very difficult to confute, if his arguments were as considerable, as our time will probably prove short for the remaining and more necessary part of my discourse: wherefore referring you for an answer to what was said concerning the dissipated parts of a burnt piece of green wood, to what I told *Themistius* on the like occasion, I might easily shew you, how slightly and superficially our *Guntherus* talks of the dividing the flame of green wood into his four elements; when he makes that vapour to be air, which being caught in glasses and condensed, presently discovers itself to have been but an aggregate of innumerable very minute drops of liquor: and when he would prove the phlegm's being composed of fire, by that heat, which is adventitious to the liquors, and ceases upon the absence of what produced it (whether that be an agitation proceeding from the motion of the external fire, or the presence of a multitude of igneous atoms pervading the pores of the vessel, and nimbly permeating the whole body of the water) I might, I say, urge these and divers other weaknesses of his discourse. But I will rather take notice of what is more pertinent to the occasion of this digression; namely, that taking it for granted, that fluidity (with which he unwarily seems to confound humidity) must proceed from the element of water, he makes a chymical oil to consist of that elementary liquor; and yet in the very next words proves, that it consists also of fire, by its inflammability; not remembering, that exquisitely pure spirit of wine is both more fluid than water itself, and yet will flame all away, without leaving the least aqueous moisture behind it, and without such an amurca and soot as he would deduce the presence of earth from. So that the same liquor may, according to his doctrine, be concluded by its great fluidity to be almost all water; and, by its burning all away, to be all disguised fire. And by the like way of probation our author would shew, that the fixt salt of wood is compounded of the four elements. For (says he) being turned by the violence of the fire into steams, it shews itself to be of kin to air; whereas I doubt, whether he ever saw a true fixt salt (which to become so, must have already endured the violence of an incinerating fire) brought by the fire alone to ascend in the form of exhalations; but I do not doubt, that if he did, and had caught those exhalations in convenient vessels, he would have found them as well as the steams of common salt, *&c.* of a saline, and not an aëreal nature. And whereas our author takes it also for granted, that the fusibility of salt must be deduced from water, it is indeed so much the effect of heat variously agitating the minute parts of a body, without regard to water, that gold (which by its being the heaviest and fixedest of bodies, should be the most earthy) will be brought to fusion by a strong fire, which sure is more likely to drive away, than increase its aqueous ingredient, if it have any. And on the other side, for want of

of a ſufficient agitation of its minute parts, ice is not fluid, but ſolid; though he preſumes alſo, that the mordicant quality of bodies muſt proceed from a fiery ingredient: whereas, not to urge, that the light and inflammable parts, which are the moſt likely to belong to the element of fire, muſt probably be driven away by that time the violence of the fire has reduced the body to aſhes; not to urge this, I ſay, nor that oil of vitriol, which quenches fire, burns the tongue and fleſh of thoſe, that unwarily taſte or apply it, as a cauſtic doth, it is precarious to prove the preſence of fire in fixt ſalts from their cauſtic power, unleſs it were firſt ſhewn, that all the qualities aſcribed to ſalts muſt be deduced from thoſe of the elements; which, had I time, I could eaſily manifeſt to be no eaſy taſk. And not to mention, that our author makes a body, as homogeneous, as any he can produce for elementary, belong both to water and fire, though it be neither fluid nor inſipid, like water; nor light and volatile, like fire; he ſeems to omit in this anatomy the element of earth, ſave that he intimates, that the ſalt may paſs for that: but ſince a few lines before, he takes aſhes for earth, I ſee not how he will avoid an inconſiſtency either betwixt the parts of his diſcourſe, or betwixt ſome of them and his doctrine. For ſince there is a manifeſt difference betwixt the ſaline and the inſipid parts of aſhes, I ſee not how ſubſtances, that diſagree in ſuch notable qualities, can be both ſaid to be portions of an element, whoſe nature requires that it be homogeneous, eſpecially in this caſe where an analyſis by the fire is ſuppoſed to have ſeparated it from the admixture of other elements, which are confeſſed by moſt Ariſtotelians to be generally found in common earth, and to render it impure. And ſure, if when we have conſidered, for how little a diſparity's ſake the Peripatetics make theſe ſymbolizing bodies, air and fire, to be two diſtinct elements, we ſhall alſo conſider, that the ſaline part of aſhes is very ſtrongly taſted, and eaſily ſoluble in water; whereas the other part of the ſame aſhes is inſipid and indiſſoluble in the ſame liquor: not to add, that the one ſubſtance is opacous, and the other ſomewhat diaphanous, nor that they differ in divers other particulars; if we conſider thoſe things, I ſay, we ſhall hardly think, that both theſe ſubſtances are elementary earth. And as to what is ſometimes objected, that their ſaline taſte is only an effect of incineration and aduſtion, it has been elſewhere fully replied to, when propoſed by *Themiſtius*, and where it has been proved againſt him, that however inſipid earth may perhaps, by additaments, be turned into ſalt, yet it is not like it ſhould be ſo by the fire alone; for we ſee, that when we refine gold and ſilver, the violenteſt fires we can employ on them give them not the leaſt reliſh of ſaltneſs. And I think *Philoponus* has rightly obſerved, that the aſhes of ſome concretes contain very little ſalt, if any at all: for refiners ſuppoſe, that bone-aſhes are free from it, and therefore make uſe of them for teſts and cupels; which ought to be deſtitute of ſalt, leſt the violence of the fire ſhould bring them to vitrification. And having purpoſely and heedfully taſted a cupel made of only bone-aſhes and fair water, which I had cauſed to be expoſed to a very violent fire, actuated by the blaſt of a large pair of double bellows, I could not perceive, that the force of the fire had imparted to it the leaſt ſaltneſs, or ſo much as made it leſs inſipid.

BUT (ſays *Carneades*) ſince neither you nor I love repetitions, I ſhall not now make any of what elſe was urged againſt *Themiſtius*, but rather invite you to take notice with me, that when our author, though a learned man, and one that pretends ſkill enough in chymiſtry to reform the whole art, comes to make good his confident undertaking, to give us an ocular demonſtration of the immediate preſence of the four elements in the reſolution of green wood, he is fain to ſay things, that agree very little with one another. For about the beginning of that paſſage of his, lately recited

cited to you, he makes the ſweat, as he call it, of the green wood, to be water; the ſmoke, air; the ſhining matter, fire; and the aſhes, earth: whereas a few lines after, he will, in each of theſe, nay (as I juſt now noted) in one diſtinct part of the aſhes, ſhew the four elements. So that either the former analyſis muſt be incompetent to prove that number of elements, ſince by it, the burnt concrete is not reduced into elementary bodies, but into ſuch as are yet each of them compounded of the four elements; or elſe theſe qualities, from which he endeavours to deduce the preſence of all the elements in the fixt ſalt, and each of the other ſeparated ſubſtances, will be but a precarious way of probation; eſpecially if you conſider, that the extracted alkali of wood, being, for aught appears, at leaſt as ſimilar a body, as any that the Peripatetics can ſhew us, if its differing qualities muſt argue the preſence of diſtinct elements, it will ſcarce be poſſible for them, by any way they know of employing the fire upon any body, to ſhew, that any body is a portion of a true element. And this recalls to my mind, that I am now but in an occaſional excurſion, which aiming only to ſhew, that the Peripatetics, as well as the chymiſts, take, in our preſent controverſy, ſomething for granted, which they ought to prove, I ſhall return to my exceptions, where I ended the firſt of them, and further tell you, that neither is that the only precarious thing, that I take notice of in *Sennertus* his argumentation: for when he infers, that becauſe the qualities he mentions, as colours, ſmells, and the like, belong not to the elements, they therefore muſt to the chymical principles, he takes that for granted, which will not in haſte be proved; as I might here manifeſt, but that I may by and by have a fitter opportunity to take notice of it. And thus much at preſent may ſuffice to have diſcourſed againſt the ſuppoſition, that almoſt every quality muſt have ſome δεκτικὸν πρῶτον, as they ſpeak, ſome native receptacle, wherein, as in its proper ſubject of inheſion, it peculiarly reſides; and on whoſe account that quality belongs to the other bodies, wherein, it is to be met with. Now this fundamental ſuppoſition being once deſtroyed, whatſoever is built upon it, muſt fall to ruin of itſelf.

BUT I conſider further, that chymiſts are (for aught I have found) far from being able to explicate by any of the *tria prima*, thoſe qualities, which they pretend to belong primarily unto it, and in mixt bodies to deduce from it. It is true indeed, that ſuch qualities are not explicable by the four elements; but it will not therefore follow, that they are ſo by the three Hermetical principles: and this is it, that ſeems to have deceived the chymiſts, and is indeed a very common miſtake amongſt moſt diſputants, who argue, as if there could be but two opinions concerning the difficulty about which they content; and conſequently they infer, that if their adverſaries opinion be erroneous, their's muſt needs be the truth; whereas many queſtions, and eſpecially in matters phyſiological, may admit of ſo many differing hypotheſes, that it will be very inconſiderate and fallacious to conclude (except where the opinions are preciſely contradictory) the truth of one from the falſity of another. And in our particular caſe it is no way neceſſary, that the properties of mixt bodies muſt be explicable either by the Hermetical, or the Ariſtotelian hypotheſis; there being divers other and more plauſible ways of explaining them, and eſpecially that, which deduces qualities from the motion, figure, and contrivance of the ſmall parts of bodies; as I think might be ſhewn, if the attempt were as ſeaſonable, as I fear it would be tedious.

I WILL allow then, that the chymiſts do not cauſeleſsly accuſe the doctrine of the four elements of incompetency to explain the properties of compound bodies. And for this rejection of a vulgar error, they ought not to be denied what praiſe men may

deserve for exploding a doctrine, whose imperfections are so conspicuous, that men needed but not to shut their eyes, to discover them. But I am mistaken, if our hermetical philosophers themselves need not, as well as the Peripatetics, have recourse to more fruitful and comprehensive principles than the *tria prima*, to make out the properties of the bodies they converse with. Not to accumulate examples to this purpose, because I hope for a fitter opportunity to prosecute this subject, let us at present only point at colour, that you may guess by what they say of so obvious and familiar a quality, how little instruction we are to expect from the *tria prima* in those more abstruse ones, which they, with the Aristotelians, style occult. For about colours, neither do they at all agree among themselves, nor have I met with any one, of which of the three persuasions soever, that does intelligibly explicate them. The vulgar chymists are wont to ascribe colours to mercury; *Paracelsus* in divers places attributes them to salt; and *Sennertus*, having recited their differing opinions, dissents from both; and refers colours rather unto sulphur. But how colours do, nay, how they may, arise from either of these principles, I think you will scarce say, that any has yet intelligibly explicated. And if Mr. *Boyle* will allow me to shew you the experiments, which he has collected about colours, you will, I doubt not, confess, that bodies exhibit colours, not upon the account of the predominancy of this or that principle in them, but upon that of their texture, and expecially the disposition of their superficial parts; whereby the light rebounding thence to the eye is so modified, as by differing impressions variously to affect the organs of sight. I might here take notice of the pleasing variety of colours exhibited by the triangular glass (at it is wont to be called) and demand, what addition or decrement of either salt, sulphur, or mercury, befalls the body of the glass by being prismatically figured; and yet it is known, that without that shape it would not afford those colours as it does. But because it may be objected, that these are not real, but apparent colours; that I may not lose time in examining the distinction, I will alledge against the chymists a couple of examples of real and permanent colours drawn from metalline bodies; and represent, that without the addition of any extraneous body, , quicksilver may by the fire alone, and that in glass-vessels, be deprived of its silver-like colour, and be turned into a red body; and from this red body without addition likewise may be obtained a mercury bright and specular as it was before. So that I have here a lasting colour generated and destroyed (as I have seen) at pleasure, without adding or taking away either mercury, salt, or sulphur. And if you take a clean and slender piece of hardened steel, and apply to it the flame of a candle at some little distance short of the point, you shall not have held the steel long in the flame, but you shall perceive divers colours, as yellow, red and blue, to appear upon the surface of the metal, and, as it were, run along in chase of one another towards the point; so that the same body, and that in one and the same part, may not only have a new colour produced in it, but exhibit successively divers colours within a minute of an hour, or thereabouts; and any of these colours may, by removing the steel from the fire, become permanent, and last many years. And this production and variety of colours cannot reasonably be supposed to proceed from the accession of any of the three principles, to which of them soever chymists will be pleased to ascribe colours; especially considering, that if you but suddenly refrigerate that iron, first made red-hot, it will be hardened and colourless again; and not only by the flame of a candle, but by any other equivalent heat conveniently applied, the like colours will again be made to appear and succeed one another, as at the first. But I must not any further prosecute an occasional discourse, though that were not so difficult for me to do, as I fear

De C. of. & Differ. cap. 11. p. 186.

fear it would be for the chymists to give a better account of the other qualities, by their principles, than they have done of colours. And your *Sennertus* himself (though an author I much value) would I fear have been exceedingly puzzled to resolve, by the *tria prima*, half that catalogue of problems, which he challenges the vulgar Peripatetics to explicate by their four elements. And supposing it were true, that salt or sulphur were the principle, to which this or that quality may be peculiarly referred, yet though he, that teaches us this, teaches us something concerning that quality, yet he teaches us but something. For indeed he does not teach us that, which can in any tolerable measure satisfy an inquisitive searcher after truth. For what is it to me to know, that such a quality resides in such a principle or element, whilst I remain altogether ignorant of the cause of that quality, and the manner of its production and operation? How little do I know more than any ordinary man of gravity, if I know but that the heaviness of mixt bodies proceeds from that of the earth they are composed of, if I know not the reason, why the earth is heavy? and how little does the chymist teach the philosopher of the nature of purgation, if he only tells him, that the purgative virtue of medicines resides in their salt? For, besides that this must not be conceded without limitation, since the purging parts of many vegetables extracted by the water, wherein they are infused, are at most but such compounded salts (I mean mingled with oil, and spirit, and earth, as tartar and divers other subjects of the vegetable kingdom afford;) and since too that quicksilver precipitated either with gold, or without addition, into a powder, is wont to be strongly enough cathartical, though the chymists have not yet proved, that either gold or mercury have any salt at all, much less any that is purgative; besides this, I say, how little is it to me, to know, that it is the salt of the rhubarb (for instance) that purges, if I find, that it does not purge as salt; since scarce any elementary salt is in small quantity cathartical; and if I know not how purgation in general is effected in a human body? In a word, as it is one thing to know a man's lodging, and another, to be acquainted with him; so it may be one thing to know the subject, wherein a quality principally resides, and another thing to have a right notion and knowledge of the quality itself. Now that, which I take to be the reason of this chymical deficiency, is the same, upon whose account I think the Aristotelian and divers other theories incompetent to explicate the origin of qualities. For I am apt to think, that men will never be able to explain the phænomena of nature, while they endeavour to deduce them only from the presence and proportion of such or such material ingredients, and consider such ingredients or elements as bodies in a state of rest; whereas indeed the greatest part of the affections of matter, and consequently of the phænomena of nature, seems to depend upon the motion and the contrivance of the small parts of bodies. For it is by motion, that one part of matter acts upon another; and it is, for the most part, the texture of the body, upon which the moving parts strike, that modifies the motion or impression, and concurs with it to the production of those effects, which make up the chief part of the naturalist's theme.

Senn. de Cons. & Differ. p. 165, 166.

BUT (says *Eleutherius*) methinks for all this, you have left some part of what I alleged, in behalf of the three principles, unanswered. For all that you have said will not keep this from being a useful discovery, that since in the salt of one concrete, in the sulphur of another, and the mercury of a third, the medicinal virtue of it resides; that principle ought to be separated from the rest, and there the desired faculty must be sought for.

I NEVER

I NEVER denied (replies *Carneades*) that the notion of the *tria prima* may be of ſome uſe; but (continues he, laughing) by what you now allege for it, it will but appear, that it is uſeful to apothecaries, rather than to philoſophers; the being able to make things operative being ſufficient to thoſe, whereas the knowledge of cauſes is the thing looked after by theſe. And let me tell you, *Eleutherius*, even this itſelf will need to be entertained with ſome caution.

FOR firſt, it will not preſently follow, that if the purgative or other virtue of a ſimple may be eaſily extracted by water or ſpirit of wine, it reſides in the ſalt or ſulphur of the concrete; ſince unleſs the body hath before been reſolved by the fire, or ſome other powerful agent, it will, for the moſt part, afford in the liquors I have named, rather the finer compounded parts of itſelf, than the elementary ones. As I noted before, that water will diſſolve not only pure ſalts, but cryſtals of tartar, gum arabic, myrrh and other compound bodies. As alſo ſpirit of wine will diſſolve not only the pure ſulphur of concretes, but likewiſe the whole ſubſtance of divers reſinous bodies, as benzoin, the gummous parts of jalap, gum lacca, and other bodies, that are counted perfectly mixt. And we ſee, that the extracts made either with water or ſpirit of wine are not of a ſimple and elementary nature, but maſſes conſiſting of the looſer corpuſcles, and finer parts of the concretes whence they are drawn; ſince by diſtillation they may be divided into more elementary ſubſtances.

NEXT, we may conſider, that even when there intervenes a chymical reſolution by the fire, it is ſeldom in the ſaline or ſulphureous principle, as ſuch, that the deſired faculty of the concrete reſides; but as that titular ſalt or ſulphur is yet a mixt body, though the ſaline or ſulphureous nature be predominent in it. For, if in chymical reſolutions, the ſeparated ſubſtances were pure and ſimple bodies, and of a perfect elementary nature; no one would be indued with more ſpecific virtues, than another; and their qualities would differ as little as do thoſe of water. And let me add this upon the by, that even eminent chymiſts have ſuffered themſelves to be reprehended by me for their over-great diligence in purifying ſome of the things they obtain by fire from mixt bodies. For though ſuch compleatly purified ingredients of bodies might perhaps be more ſatisfactory to our underſtanding, yet others are often more uſeful to our lives; the efficacy of ſuch chymical productions depending moſt upon what they retain of the bodies, whence they are ſeparated, or gain by the new aſſociations of the diſſipated among themſelves; whereas, if they were meerly elementary, their uſes would be comparatively very ſmall; and the virtues of ſulphurs, ſalts, or other ſuch ſubſtances of one denomination, would be the very ſame.

AND by the way, *Eleutherius*, I am inclined upon this ground to think, that the artificial reſolution of compound bodies by fire does not ſo much enrich mankind, as it divides them into their ſuppoſed principles; as upon the ſcore of its making new compounds by new combinations of the diſſipated parts of the reſolved body. For by this means the number of mixt bodies is conſiderably increaſed; and many of thoſe new productions are endowed with uſeful qualities; divers of which they owe not to the body, from which they were obtained, but to their newly-acquired texture.

BUT thirdly, that, which is principally to be noted, is this, that as there are divers concretes, whoſe faculties reſide in ſome one or other of thoſe differing ſubſtances, that chymiſts call their ſulphurs, ſalts, and mercuries; and conſequently may be beſt obtained, by analyzing the concrete, whereby the deſired principles may be had ſevered or freed from the reſt; ſo there are others, wherein the nobleſt properties lodge not in the ſalt, or ſulphur, or mercury, but depend immediately upon the form, or (if

(if you will) result from the determinate structure of the whole concrete; and consequently they, that go about to extract the virtues of such bodies, by exposing them to the violence of the fire, do exceedingly mistake, and take the way to destroy what they would obtain.

I REMEMBER that *Helmont* himself somewhere confesses, that as the fire betters some things, and improves their virtues, so it spoils others, and makes them degenerate. And elsewhere he judiciously affirms, that there may be sometimes greater virtue in a simple, such as nature has made it, than in any thing, that can by the fire be separated from it. And lest you should doubt, whether he means by the virtues of things those, that are medical; he has in one place this ingenuous confession; *Credo* (says he) *simplicia in sua simplicitate esse sufficientia pro sanatione omnium morborum.* Nay, *Barthius*, even in a comment upon *Beguinus*, scruples not to make this acknowledgment; *Valde absurdum est* (says he) *ex omnibus rebus extracta facere, salia, quintas essentias; præsertim ex substantiis per se plane vel subtilibus vel homogeneis, quales sunt uniones, corallia, moschus, ambra, &c.* Consonantly whereunto he also tells us (and vouches the famous *Platerus*, for having candidly given the same advertisement to his auditors) that some things have greater virtues, and better suited to our human nature, when unprepared, than when they have past the chymists fire: as we see, says my author, in pepper; of which some grains swallowed perform more towards the relief of a distempered stomach, than a great quantity of the oil of the same spice.

Helmont. *Pharm. & Dispen. Nov.* pag. 498. *Vide* Jet. *ad* Begu. Lib. 1. Cap. 17.

IT has been (pursues *Carneades*) by our friend here present observed concerning salt-petre, that none of the substances, into which the fire is wont to divide it, retains either the taste, the cooling virtue, or some other of the properties of the concrete; and that each of those substances acquires new qualities not to be found in salt-petre itself. The shining property of the tails of glow-worms does survive but so short a time the little animal made conspicuous by it, that inquisitive men have not scrupled publickly to deride *Baptista Porta* and others; who, deluded perhaps with some chymical surmises, have ventured to prescribe the distillation of a water from the tails of glow-worms, as a sure way to obtain a liquor shining in the dark. To which I shall now add no other example than that afforded us by amber; which, whilst it remains an intire body, is endowed with an electrical faculty of drawing to itself feathers, straws, and such like bodies; which I never could observe either in its salt, its spirit, its oil, or in the body I remember I once made by the re-union of its divided elements; none of these having such a texture as the intire concrete. And however chymists boldly deduce such and such properties from this or that proportion of their component principles; yet in concretes, that abound with this or that ingredient, it is not always so much by virtue of its presence, nor its plenty, that the concrete is qualified to perform such and such effects; as upon the account of the particular texture of that and the other ingredients, associated after a determinate manner into one concrete; though possibly such a proportion of that ingredient may be more convenient than another, for the constituting of such a body. Thus in a clock, the hand is moved upon the dial, the bell is struck, and the other actions belonging to the engine are performed, not because the wheels are of brass or iron, or part of one metal and part of another, or because the weights are of lead; but by virtue of the size, shape, bigness, and coaptation of the several parts; which would perform the same things, though the wheels were of silver, or lead, or wood, and the weights of stone or clay, provided the fabric or contrivance of the engine were the same: though

it be not to be denied, that brass and steel are more convenient materials to make clock-wheels of, than lead, or wood. And to let you see, *Eleutherius*, that it is sometimes at least, upon the texture of the small parts of a body, and not always upon the presence, or recess, or increase, or decrement of any one of its principles, that it may lose some such qualities, and acquire some such others, as are thought very strongly inherent to the bodies they reside in; I will add to what may, from my past discourse, be referred to this purpose, this notable example, from my own experience; that lead may, without any additament, and only by various applications of the fire, lose its colour; and acquire sometimes a grey, sometimes a yellowish, sometimes a red, sometimes an amethystine colour; and after having past through these, and perhaps divers others, again recover its leaden colour, and be made a bright body. That also this lead, which is so flexible a metal, may be made as brittle as glass, and presently be brought to be again flexible and malleable as before. And besides that the same lead, which I find by microscopes to be one of the most opacous bodies in the world, may be reduced to a fine transparent glass; whence yet it may return to an opacous nature again; and all this, as I said, without the addition of any extraneous body, and meerly by the manner and method of exposing it to the fire.

BUT (says *Carneades*) after having already put you to so prolix a trouble, it is time for me to relieve you with a promise of putting speedily a period to it; and to make good that promise, I shall, from all that I have hitherto discoursed with you, deduce but this one proposition by way of corollary: *That it may as yet be doubted, whether or no there be any determinate number of elements; or, if you please, whether or no all compound bodies do consist of the same number of elementary ingredients or material principles.*

THIS being but an inference from the foregoing discourse, it will not be requisite to insist at large on the proofs of it; but only to point at the chief of them, and refer you for particulars to what has been already delivered.

IN the first place then, from what has been so largely discoursed, it may appear, that the experiments wont to be brought, whether by the common Peripatetics, or by the vulgar chymists, to demonstrate, that all mixt bodies are made up precisely either of the four elements, or the three hypostatical principles, do not evince what they are alleged to prove. And as for the other common arguments, pretended to be drawn from reason in favour of the Aristotelian hypothesis (for the chymists are wont to rely almost altogether upon experiments;) they are commonly grounded upon such unreasonable or precarious suppositions, that it is altogether as easy and as just for any man to reject them, as for those, that take them for granted, to assert them, being indeed all of them as indemonstrable as the conclusion to be inferred from them; and some of them so manifestly weak and proofless, that he must be a very courteous adversary, that can be willing to grant them; and as unskilful a one, that can be compelled to do so.

IN the next place, it may be considered, if what those patriarchs of the Spagyrists, *Paracelsus* and *Helmont*, do on divers occasions positively deliver, be true; namely, that the alkahest does resolve all mixt bodies into other principles than the fire, it must be decided, which of the two resolutions (that made by the alkahest, or that made by the fire) shall determine the number of the elements, before we can be certain how many there are.

AND in the mean time, we may take notice in the last place, that as the distinct substances, whereinto the alkahest divides bodies, are affirmed to be differing in nature

ture from those, whereunto they are wont to be reduced by fire, and * to be obtained from some bodies more in number than from some others; since he tells us, he could totally reduce all sorts of stones into salt only, whereas of a coal he had two distinct liquors: so, although we should acquiesce in that resolution which is made by fire, we find not, that all mixt bodies are thereby divided into the same number of elements and principles; some concretes affording more of them than others do; nay, and sometimes this or that body affording a greater number of differing substances by one way of management, than the same yields by another. And they, that out of gold, or mercury, or *Muscovy*-glass, will draw me as many distinct substances, as I can separate from vitriol, or from the juice of grapes variously ordered, may teach me that, which I shall very thankfully learn. Nor does it appear more congruous to that variety, that so much conduceth to the perfection of the universe, that all elemented bodies be compounded of the same number of elements, than it would be for a language, that all its words should consist of the same number of letters.

THE

SCEPTICAL CHYMIST:

OR,

A Paradoxical Appendix to the foregoing Treatise.

PART VI.

HERE *Carneades* having dispatched what he thought requisite to oppose against what the chymists are wont to allege for proof of their three principles, paused a while, and looked about him to discover, whether it were time for him and his friend to rejoin the rest of the company. But *Eleutherius* perceiving nothing yet to forbid them to prosecute their discourse a little further, said to his friend (who had likewise taken notice of the same thing) I half expected, *Carneades*, that after you had so freely declared your doubting, whether there be any determinate number of elements, you would have proceeded to question, whether there be any elements at all. And I confess it will be a trouble to me, if you defeat me of my expectation; especially since you see the leisure we have allowed us may probably suffice to examine that paradox; because you have so largely deduced already many things pertinent to it, that you need but intimate how you would have them applied, and what you would infer from them.

* *Novi saxum & lapides omnes in merum salem suo saxo aut lapidi & æquipondérantem reducere absque omni prorsus sulphure aut mercurio.* Helmont, pag. 490.

CARNEADES having in vain represented, that their leisure could be but very short, that he had already prated very long, that he was unprepared to maintain so great and so invidious a paradox, was at length prevailed with to tell his friend; since, *Eleutherius*, you will have me discourse *extempore* of the paradox you mention, I am content (though more perhaps to express my obedience, than my opinion) to tell you, that (supposing the truth of *Helmont*'s and *Paracelsus*'s alkahestical experiments, if I may so call them) though it may seem extravagant, yet it is not absurd to doubt, whether, for aught has been proved, there be a necessity to admit any elements, or hypostatical principles, at all.

AND, as formerly, so now, to avoid the needless trouble of disputing severally with the Aristotelians and the chymists, I will address myself to oppose them I have last named, because their doctrine about the elements is more applauded by the moderns, as pretending highly to be grounded upon experience. And, to deal not only fairly, but favourably with them, I will allow them to take in earth and water to their other principles. Which I consent to the rather, that my discourse may the better reach the tenets of the Peripatetics; who cannot plead for any so probably, as for those two elements; that of fire above the air being generally by judicious men exploded as an imaginary thing; and the air not concurring to compose mixt bodies as one of their elements, but only lodging in their pores, or rather replenishing, by reason of its weight and fluidity, all those cavities of bodies here below, whether compounded or not, that are big enough to admit it, and are not filled up with any grosser substance.

AND, to prevent mistakes, I must advertise you, that I now mean by elements, as those chymists, that speak plainest, do by their principles, certain primitive and simple, or perfectly unmingled bodies; which not being made of any other bodies, or of one another, are the ingredients, of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved: now whether there be any one such body to be constantly met with in all, and each, of those, that are said to be elemented bodies, is the thing I now question.

BY this state of the controversy you will, I suppose, guess, that I need not be so absurd, as to deny, that there are such bodies as earth and water, and quicksilver and sulphur: but I look upon earth and water, as component parts of the universe, or rather of the terrestrial globe, not of all mixt bodies. And though I will not peremptorily deny, that there may sometimes either a running mercury, or a combustible substance be obtained from a mineral, or even a metal; yet I need not concede either of them to be an element in the sense above declared; as I shall have occasion to shew you by and by.

To give you then a brief account of the grounds I intend to proceed upon, I must tell you, that in matters of philosophy, this seems to me a sufficient reason to doubt of a known and important proposition, that the truth of it is not yet by any competent proof made to appear. And congruously hereunto, if I shew, that the grounds, upon which men are persuaded, that there are elements, are unable to satisfy a considering man, I suppose my doubts will appear rational.

Now the considerations, that induce men to think, that there are elements, may be conveniently enough referred to two heads: namely, the one, that it is necessary, that nature make use of elements to constitute the bodies that are reputed mixt. And the other, that the resolution of such bodies manifests, that nature had compounded them of elementary ones.

IN

IN reference to the former of these considerations, there are two or three things, that I have to represent. And I will begin with reminding you of the experiments I not long since related to you concerning the growth of pompions, mint, and other vegetables out of fair water. For by those experiments it seems evident, that water may be transmuted into all the other elements; from whence it may be inferred, both, that it is not every thing chymists will call salt, sulphur, or spirit, that needs always be a primordiate and ingenerable body; and that nature may contex a plant (though that be a perfectly mixt concrete) without having all the elements previously presented to her to compound it of. And, if you will allow the relation I mentioned out of Monsieur *de Rochas* to be true; then may not only plants, but animals and minerals too, be produced out of water. And however there is little doubt to be made, but that the plants my trials afforded me, as they were like in so many other respects to the rest of the plants of the same denomination; so they would, in case I had reduced them to putrefaction, have likewise produced worms or other insects, as well as the resembling vegetables are wont to do: so that water may, by various seminal principles, be successively transmuted into both plants and animals. And if we consider, that not only men, but even sucking children are, but too often, tormented with solid stones; and that divers sorts of beasts themselves (whatever *Helmont* against experience thinks to the contrary) may be troubled with great and heavy stones in their kidneys and bladders, though they feed but upon grass and other vegetables, that are perhaps but disguised water; it will not seem improbable, that even some concretes of a mineral nature may likewise be formed of water.

WE may further take notice, that as a plant may be nourished, and consequently may consist of common water; so may both plants and animals (perhaps even from their seminal rudiments) consist of compound bodies, without having any thing merely elementary brought them by nature to be compounded by them: this is evident in divers men, who whilst they were infants were fed only with milk, afterwards live together upon flesh, fish, wine, and other perfectly mixt bodies. It may be seen also in sheep, who, on some of our English downs or plains, grow very fat by feeding upon the grass, without scarce drinking at all; and yet more manifestly in the maggots, that breed and grow up to their full bigness within the pulps of apples, pears, or the like fruit. We see also, that dungs, that abound with a mixt salt, give a much more speedy increment to corn and other vegetables, than water alone would do: and it hath been assured me by a man experienced in such matters, that sometimes, when, to bring up roots very early, the mould they were planted in was made over-rich, the very substance of the plant has tasted of the dung. And let us also consider a graft of one kind of fruit upon the upper bough of a tree of another kind; as (for instance) the scion of a pear upon a white-thorn; for there the ascending liquor is already altered, either by the root, or in its ascent by the bark, or both ways, and becomes a new mixt body: as may appear by the differing qualities to be met with in the saps of several trees; as particularly, the medicinal virtue of birch-water, which I have sometimes drunk upon *Helmont*'s great and not undeserved commendation. Now the graft, being fastened to the stock, must necessarily nourish itself, and produce its fruit, only out of this compound juice prepared for it by the stock, being unable to come at any other aliment. And if we consider, how much of the vegetable he feeds upon may (as we noted above) remain in an animal; we may easily suppose, that the blood of that animal, who feeds upon this, though it be a well-constituted liquor, and have all the differing corpuscles, that make it up, kept

in order by one presiding form, may be a strangely decompounded body, many of its parts being themselves decompounded. So little is it necessary, that even in the mixtures, which nature herself makes in animal and vegetable bodies, she should have pure elements at hand to make her compositions of.

HAVING said thus much touching the constitution of plants and animals, I might perhaps be able to say as much touching that of minerals, and even metals, if it were as easy for us to make experiment in order to the production of these, as of those. But the growth or increment of minerals being usually a work of excessively long time, and for the most part performed in the bowels of the earth, where we cannot see it, I must instead of experiments make use, on this occasion, of observations.

THAT stones were not all made at once, but that some of them are now-a days generated, may (though it be denied by some) be fully proved by several examples, of which I shall now scarce alledge any other, than that famous place in *France* known by the name of *Les Caves Goutieres*, where the water falling from the upper parts of the cave to the ground, does presently there condense into little stones, of such figures, as the drops, falling either severally or upon one another, and coagulating presently into stone, chance to exhibit. Of these stones some ingenious friends of ours, that went a while since to visit that place, did me the favour to present me with some, that they brought thence. And I remember, that both that sober relator of his voyages, *Van Linschoten*, and another good author, inform us, that in the diamond mines (as they call them) in the *East-Indies*, when having digged the earth, though to no great depth, they find diamonds, and take them quite away; yet in a very few years they find in the same place new diamonds produced there since. From both which relations, especially the first, it seems probable, that nature does not always stay for divers elementary bodies, when she is to produce stones. And as for metals themselves, authors of good note assure us, that even they were not in the beginning produced at once altogether, but have been observed to grow; so that what was not a mineral or metal before, became one afterwards. Of this it were easy to alledge many testimonies of professed chymists. But that they may have the greater authority, I shall rather present you with a few borrowed from more unsuspected writers: *Sulphuris mineram* (as the inquisitive *P. Fallopius* notes) *quæ nutrix est caloris subterranei fabri seu archæi fontium & mineralium, infra terram citissime renasci testantur historiæ metallicæ. Sunt enim loca, è quibus si hoc anno sulphur effojum fuerit, intermissa, fossione per quadriennium, redeunt fossores, & omnia sulphure, ut antea, rursus inveniunt plena. Pliny* relates, *in Italiæ insula Ilva, gigni ferri metallum.* Strabo *multo expressius; effossum ibi metallum semper regenerari. Nam si effossio spatio centum annorum intermittebatur, & iterum illuc revertebantur, fossores reperisse maximam copiam ferri regeneratum.* Which history not only is countenanced by *Fallopius*, from the income, which the iron of that island yielded the duke of *Florence* in his time; but is mentioned more expressly to our purpose by the learned *Cesalpinus*: *Vena* (says he) *ferri copiosissima est in Italia; ob eam nobilitata Ilva Tyrrheni meris insula incredibili copia etiam nostris temporibus eam gignens: Nam terra, quæ eruitur, dum vena effoditur tota, procedente tempore in venam convertitur.* Which last clause is therefore very notable, because from thence we may deduce, that earth, by a metalline plastick principle latent in it, may be in process of time changed into a metal. And even * *Agricola*

* *In Lygiis, ad Segam oppidum, in pratis eruitur ferrum, fossis ad altitudinem bipedaneam actis. Id decennio renatum, denuo foditur, non aliter ac Ilvæ ferrum.*

himself,

himself, though the chymists complain of him as their adversary, acknowledges thus much and more; by telling us, that at a town called *Saga* in *Germany*, they dig up iron in the fields, by sinking ditches two foot deep; and adding, that within the space of ten years the ditches are digged again for iron since produced, as the same metal is wont to be obtained in *Ilva*. Also concerning lead, not to mention what even *Galen* notes, that it will increase both in bulk and weight, if it be long kept in vaults or cellars, where the air is gross and thick, as he collects from the swelling of those pieces of lead, that were employed to fasten together the parts of old statues. Not to mention this, I say, *Boccacius Certaldus*, as I find him quoted by a diligent writer, has this passage touching the growth of lead: *Fessularum mons* (says he) *in Hetruria, Florentiæ civitati imminens, lapides plumbarios habet; qui si excidantur, brevi temporis spatio, novis incrementis instaurantur; ut* (annexes my author) *tradit Boccacius Certaldus, qui id compertissimum esse scribit. Nihil hoc novi est; sed de eodem Plinius, lib.* 34. *Hist. Natur. cap.* 17. *dudum prodidit, inquiens, mirum in his solis plumbi metallis, quod derelicta fertilius reviviscunt. In plumbariis secundo lapide ab Amberga dictis ad asylum recrementa congesta in cumulos, exposita solibus pluviisque paucis annis, reddunt suum metallum cum fœnore.* I might add to these (continues *Carneades*) many things, that I have met with concerning the generation of gold and silver. But for fear of wanting time, I shall mention but two or three narratives. The first you may find recorded by *Gerhardus*, the physick professor, in these words: *In valle* (says he) *Joachimica argentum gramiuis modo & more è lapidibus mineræ velut è radice excrevisse digiti longitudine, testis est Dr. Schreterus, qui ejusmodi venas aspectu jucundas & admirabiles domi suæ aliis sæpe monstravit & donavit. Item aqua cærulea inventa est Annebergæ, ubi argentum erat adhuc in primo ente, quæ coagulata redacta est in calcem fixi & boni argenti.*

THE other two relations I have not met with in Latin authors, and yet they are both very memorable in themselves, and pertinent to our present purpose.

THE first I met with in the commentary of *Johannes Valebius* upon the *Kleine Baur*, in which that industrious chymist relates, with many circumstances, that at a mine-town (if I may so English the German *Bergstot*) eight miles or leagues distant from *Strasburg* called *Mariakirch*, a workman came to the overseer, and desired employment; but he telling him, that there was not any of the best sort at present for him, added, that till he could be preferred to some such, he might in the mean time, to avoid idleness, work in a grove or mine pit thereabouts, which at that time was little esteemed. This workman, after some weeks labour, had by a crack appearing in the stone upon a stroke given near the wall, an invitation given him to work his way through; which as soon as he had done, his eyes were saluted by a mighty stone or lump, which stood in the middle of the cleft (that had a hollow place behind it) upright, and in shew like an armed man; but consisted of pure fine silver having no vein or ore by it, or any other additament, but stood there free, having only under foot something like a burnt matter; and yet this one lump held in weight above 1000 marks, which, according to the Dutch account, makes 500 pound weight of fine silver. From which and other circumstances my author gathers, that by the warmth of the place, the noble metalline spirits, (sulphureous and mercurial) were carried from the neighbouring galleries or vaults, through other smaller cracks and clefts into that cavity, that there collected as in a close chamber or cellar; whereinto when they were gotten, they did in process of time settle into the fore-mentioned precious mass of metal.

THE

THE other German relation is of that great traveller and laborious chymist *Joannes* (not *Georgius*) *Agricola*; who, in his notes upon what *Poppius* has written of antimony, relates, that when he was among the Hungarian mines in the deep groves, he observed, that there would often arise in them a warm steam, (not of that malignant sort, which the Germans call *Shwadt*, which, says he, is a mere poison, and often suffocates the diggers) which fastened itself to the walls; and that coming again to review it after a couple of days, he discerned, that it was all very fast, and glistering: whereupon, having collected it and distilled it *per retortam*, he obtained from it a fine spirit; adding, that the mine-men informed him, that this steam or damp (as the Englishmen also call it, retaining the Dutch term) would at last have become a metal, as gold or silver.

I REFER (says *Carneades*) to another occasion, the use that may be made of these narratives towards the explicating the nature of metals; and that of fixedness, malleableness, and some other qualities conspicuous in them. And in the mean time, this I may at present deduce from these observations; that it is not very probable, that whensoever a mineral, or even a metal, is to be generated in the bowels of the earth, nature needs to have at hand both salt and sulphur, and mercury to compound it of: for, not to urge, that the two last relations seem less to favour the chymists than *Aristotle*, who would have metals generated of certain halitus or steams, the fore-mentioned observations together make it seem more likely, that the mineral earths, or those metalline steams (wherewith probably such earths are plentifully imbued) do contain in them some seminal rudiment, or something equivalent thereunto; by whose plastick power the rest of the matter, though perhaps terrestrial and heavy, is, in tract of time, fashioned into this or that metalline ore; almost (as I formerly noted) as that fair water was by the seminal principle of mint, pompions, and other vegetables, contrived into bodies answerable to such seeds. And that such alterations of terrestrial matter are not impossible, seems evident from that notable practice of the boilers of salt-petre, who unanimously observe, as well here in *England* as in other countries, that if an earth pregnant with nitre be deprived, by the affusion of water, of all its true and dissoluble salt, yet the earth will, after some years, yield them salt petre again: for which reason, some of the eminent and skilfullest of them keep it in heaps as a perpetual mine of salt-petre. Whence it may appear, that the seminal principle of nitre, latent in the earth, does, by degrees, transform the neighbouring matter into a nitrous body: for though I deny, that some volatile nitre may, by such earths, be attracted (as they speak) out of the air, yet that the innermost parts of such great heaps, that lie so remote from the air, should borrow from it all the nitre they abound with, is not probable, for other reasons besides the remoteness of the air, though I have not the leisure to mention them.

AND I remember, that a person of great credit, and well acquainted with the ways of making vitriol, affirmed to me, that he had observed, that a kind of mineral, which abounds in that salt, being kept within doors, and not exposed (as is usual) to the free air and rains, did of itself in no very long time turn into vitriol, not only in the outward or superficial, but even in the internal and most central parts.

AND I also remember, that I met with a certain kind of marchasite, that lay together in great quantities under ground, which did, even in my chamber, in so few hours begin of itself to turn into vitriol, that we need not distrust the newly recited narrative. But to return to what I was saying of nitre; as nature made this salt-petre out of the once almost inodorous earth it was bred in, and did not find a very

very ſtinking and corroſive acid liquor, and a ſharp alcalizate ſalt to compound it of, though theſe be the bodies, into which the fire diſſolves it; ſo it were not neceſſary, that nature ſhould make up all metals and other minerals of pre-exiſtent ſalt, and ſulphur, and mercury, though ſuch bodies might by fire be obtained from it. Which one conſideration duly weighed is very conſiderable in the preſent controverſy: and to this agree well the relations of our two German chymiſts; for beſides that it cannot be convincingly proved, it is not ſo much as likely, that ſo languid and moderate a heat, as that within the mines, ſhould carry up to ſo great a height, though in the form of fumes, ſalt, ſulphur, and mercury; ſince we find in our diſtillations, that it requires a conſiderable degree of fire to raiſe, ſo much as to the height of one foot, not only ſalt, but even mercury itſelf in cloſe veſſels. And if it be objected, that it ſeems by the ſtink, that is ſometimes obſerved, when lightning falls down here below, that ſulphureous ſteams may aſcend very high without any extraordinary degree of heat; it may be anſwered, among other things, that the ſulphur of ſilver is by chymiſts ſaid to be a fixt ſulphur, though not altogether ſo well digeſted as that of gold.

BUT (proceeds *Carneades*) if it had not been to afford you ſome hints concerning the origin of metals, I need not have deduced any thing from theſe obſervations; it not being neceſſary to the validity of my argument, that my deductions from them ſhould be irrefragable, becauſe my adverſaries the Ariſtotelians and vulgar chymiſts do not, I preſume, know any better than I, *à priori*, of what ingredients Nature compounds metals and minerals. For their argument to prove, that thoſe bodies are made up of ſuch principles, is drawn *à poſteriori*; I mean from this, that upon the analyſis of mineral bodies they are reſolved into thoſe differing ſubſtances. That we may therefore examine this argument, let us proceed to conſider, what can be alledged in behalf of the elements from the reſolutions of bodies by the fire; which you remember was the ſecond topick, whence I told you the arguments of my adverſaries were deſumed.

AND that I may firſt diſpatch what I have to ſay concerning minerals, I will begin the remaining part of my diſcourſe with conſidering how the fire divides them.

AND firſt, I have partly noted above, that though chymiſts pretend from ſome to draw ſalt, from others running mercury, and from others a ſulphur; yet they have not hitherto taught us, by any way in uſe among them, to ſeparate any one principle, whether ſalt, ſulphur, or mercury, from all ſorts of minerals without exception. And thence I may be allowed to conclude, that there is not any of the elements, that is an ingredient of all bodies, ſince there are ſome, of which it is not ſo.

IN the next place, ſuppoſing, that either ſulphur or mercury were obtainable from all ſorts of minerals; yet ſtill this ſulphur or mercury would be but a compounded, not an elementary body, as I told you already on another occaſion. And certainly he, that takes notice of the wonderful operations of quickſilver, whether it be common, or drawn from mineral bodies, can ſcarce be ſo inconſiderate as to think it of the very ſame nature with that immature and fugitive ſubſtance, which in vegetables and animals chymiſts have been pleaſed to call their mercury. So that when mercury is got by the help of the fire out of a metal or other mineral body, if we will not ſuppoſe, that it was not pre-exiſtent in it, but produced by the action of the fire upon the concrete, we may at leaſt ſuppoſe this quickſilver to have been a perfect body of its own kind (though perhaps leſs heterogeneous than more ſecondary mixts) which happened to be mingled *per minima*, and coagulated with the other ſubſtances, whereof the metal or mineral conſiſted. As may be exemplified, partly by native vermilion,

wherein the quickſilver and ſulphur being exquiſitely blended both with one another, and that other coarſe mineral ſtuff (whatever it be) that harbours them, make up a red body differing enough from both; and yet from which part of quickſilver, and of the ſulphur, may be eaſily enough obtained; partly by thoſe mines, wherein nature has ſo curiouſly incorporated ſilver with lead, that it is extremely difficult, and yet poſſible to ſeparate the former out of the latter; and partly too by native vitriol, wherein the metalline corpuſcles are by ſkill and induſtry ſeparable from the ſaline ones, though they be ſo con-coagulated with them, that the whole concrete is reckoned a nong ſalts.

AND here I further obſerve, that I never could ſee any earth or water, properly ſo called, ſeparated from either gold or ſilver, (to name now no other metalline bodies;) and therefore to retort the argument upon my adverſaries, I may conclude, that ſince there are ſome bodies, in which, for aught appears, there is neither earth nor water; I may be allowed to conclude, that neither of thoſe two is an univerſal ingredient of all thoſe bodies, that are counted perfectly mixt, which I deſire you would remember againſt anon.

IT may indeed be objected, that the reaſon, why from gold or ſilver we cannot ſeparate any moiſture, is, becauſe that when it is melted out of the ore, the vehement fire requiſite to its fuſion forced away all the aqueous and fugitive moiſture; and the like fire may do from the materials of glaſs. To which I ſhall anſwer, that I remember I read not long ſince in the learned *Joſephus Acoſta* *, who relates it upon his own obſervation, that in *America* (where he long lived) there is a kind of ſilver, which the Indians call *papas*, and ſometimes (ſays he) they find pieces very fine and pure like to ſmall round roots, the which is rare in that metal, but uſual in gold; concerning which metal he tells us, that beſides this they find ſome, which they call gold in grains, which he tells us are ſmall morſels of gold, that they find whole without mixture of any other metal, which hath no need of melting or refining in the fire.

I REMEMBER, that a very ſkilful and credible perſon affirmed to me, that being in the Hungarian mines he had the good fortune to ſee a mineral, that was there digged up, wherein pieces of gold of the length, and alſo almoſt of the bigneſs of a human finger, grew in the ore, as if they had been parts and branches of trees.

AND I have myſelf ſeen a lump of whitiſh mineral, that was brought as a rarity to a great and knowing prince, wherein there grew here and there in the ſtone, which looked like a kind of ſpar, divers little lumps of fine gold, (for ſuch I was aſſured that trial had manifeſted it to be) ſome of them ſeeming to be about the bigneſs of peas.

BUT that is nothing to what our *Acoſta* † ſubjoins, which is indeed very memorable; namely, that of the morſels of native and pure gold, which we lately heard him mentioning, he had now and then ſeen ſome weighed many pounds: to which I ſhall add, that I myſelf have ſeen a lump of ore not long ſince digged up, in whoſe ſtony part there grew, almoſt like trees, divers parcels, though not of gold, yet of (what perhaps mineraliſts will more wonder at) another metal, which ſeemed to be very pure or unmixt with any heterogeneous ſubſtances, and were ſome of them as big as my finger, if not bigger. But upon obſervations of this kind, though perhaps I could, yet I muſt not at preſent, dwell any longer.

* *Acoſta*'s Natural and Moral Hiſtory of the *Indies*, *L.* 3. *c.* 5. *p.* 212.
† See *Acoſta* in the fore-cited place, and the paſſage of *Pliny* quoted by him.

To

To proceed therefore now (ſays *Carneades*) to the conſideration of the analyſis of vegetables, although my trials give me no cauſe to doubt, but that out of moſt of them five differing ſubſtances may be obtained by the fire, yet I think it will not be ſo eaſily demonſtrated, that theſe deſerve to be called elements in the notion above explained.

AND before I deſcend to particulars, I ſhall repeat and premiſe this general conſideration, that theſe differing ſubſtances, that are called elements or principles, differ not from each other as metals, plants, and animals, or as ſuch creatures as are immediately produced each by its peculiar ſeed, and conſtitutes a diſtinct propagable ſort of creatures in the univerſe; but theſe are only various ſchemes of matter or ſubſtances, that differ from each other, but in conſiſtence (as running mercury and the ſame metal congealed by the vapour of lead) and ſome very few other accidents, as taſte, or ſmell, or inflammability, or the want of them. So that by a change of texture, not impoſſible to be wrought by the fire and other agents, that have the faculty, not only to diſſociate the ſmall parts of bodies, but afterwards to connect them after a new manner, the ſame parcel of matter may acquire or loſe ſuch accidents, as may ſuffice to denominate it ſalt, or ſulphur, or earth. If I were fully to clear to you my apprehenſions concerning this matter, I ſhould perhaps be obliged to acquaint you with divers of the conjectures (for I muſt yet call them no more) I have had concerning the principles of things purely corporeal: for though, becauſe I ſeem not ſatisfied with the vulgar doctrines, either of the Peripatetic or Paracelſian ſchools many of thoſe, that know me (and perhaps among them, *Eleutherius* himſelf) have thought me wedded to the Epicurean hypotheſis (as others have miſtaken me for an Helmontian) yet if you knew how little converſant I have been with Epicurean authors, and how great a part of *Lucretius* himſelf I never yet had the curioſity to read, you would perchance be of another mind: eſpecially if I were to entertain you at large, I ſay not with my preſent notions, but with my former thoughts concerning the principles of things. But, as I ſaid above, fully to clear my apprehenſions would require a longer diſcourſe than we can now have.

FOR I ſhould tell you, that I have ſometimes thought it not unfit, that to the principles, which may be aſſigned to things, as the world is now conſtituted, we ſhould, if we conſider the great maſs of matter, as it was whilſt the univerſe was in making, add another, which may conveniently enough be called an architectonic principle or power; by which I mean thoſe various determinations, and that ſkilful guidance of the motions of the ſmall parts of the univerſal matter by the moſt wiſe author of things, which were neceſſary at the beginning to turn that confuſed chaos into this orderly and beautiful world; and eſpecially to contrive the bodies of animals and plants, and the ſeeds of thoſe things, whoſe kinds were to be propagated. For I confeſs I cannot well conceive, how from matter, barely put into motion, and then left to itſelf, there could emerge ſuch curious fabricks, as the bodies of men and perfect animals, and ſuch yet more admirably contrived parcels of matter, as the ſeeds of living creatures.

I SHOULD likewiſe tell you upon what grounds, and in what ſenſe, I ſuſpected the principles of the world, as it now is, to be three, matter, motion and rest: I ſay, as the world now is, becauſe the preſent fabric of the univerſe, and eſpecially the ſeeds of things, together with the eſtabliſhed courſe of nature, is a requiſite or condition, upon whoſe account divers things may be made out by our three principles, which otherwiſe would be very hard, if poſſible, to explicate.

I SHOULD moreover declare in general (for I pretend not to be able to do it otherwise) not only why I conceive that colours, odours, tastes, fluidness and solidity, and those other qualities, that diversify and denominate bodies, may intelligibly be deduced from these three; but how two of the three Epicurean principles (which, I need not tell you, are magnitude, figure, and weight) are themselves deducible from matter and motion; since the latter of these variously agitating, and, as it were, distracting the former, must needs disjoin its parts; which being actually separated, must each of them necessarily both be of some size, and obtain some shape or other. Nor did I add to our principles the Aristotelian privation, partly for other reasons, which I must not now stay to insist on; and partly, because it seems to be rather an antecedent, or a *terminus à quo*, than a true principle, as the starting-post is none of the horse's legs or limbs.

I SHOULD also explain, why and how I made rest to be, though not so considerable a principle of things, as motion, yet a principle of them; partly, because it is (for aught we know) as antient at least as it, and depends not upon motion, nor any other quality of matter; and partly, because it may enable the body, in which it happens to be, both to continue in a state of rest, till some external force put it out of that state, and to concur to the productions of divers changes in the bodies, that hit against it, by either quite stopping or lessening their motion (whilst the body formerly at rest receives all or part of it into itself) or else by giving a new bias, or some other modification, to motion, that is, to the grand and primary instrument, whereby nature produces all the changes and other qualities, that are to be met with in the world.

I SHOULD likewise, after all this, explain to you how, although matter, motion, and rest, seemed to me to be the catholic principles of the universe, I thought the principles of particular bodies might be commodiously enough reduced to two; namely matter, and (what comprehends the two other, and their effects) the result, or aggregate, or complex of those accidents, which are the motion or rest (for in some bodies both are not to be found) the bigness, figure, texture, and the thence resulting qualities of the small parts, which are necessary to entitle the body, whereto they belong, to this or that peculiar denomination; and discriminating it from others, to appropriate it to a determinate kind of things (as yellowness, fixedness, such a degree of weight, and of ductility, do make the portion of matter wherein they concur, to be reckoned among perfect metals, and obtain the name of gold;) this aggregate or result of accidents you may, if you please, call either structure, or texture (though indeed that do not so properly comprehend the motion of the constituent parts, especially in case some of them be fluid) or what other appellation shall appear most expressive. Or if, retaining the vulgar term, you will call it the form of the thing it denominates, I shall not much oppose it; provided the word be interpreted to mean but what I have expressed, and not a scholastic substantial form, which so many intelligent men profess to be to them altogether unintelligible.

BUT (says *Carneades*) if you remember, that it is a sceptic speaks to you, and that it is not so much my present task to make assertions as to suggest doubts, I hope you will look upon what I have proposed, rather as a narrative of my former conjectures touching the principles of things, than as a resolute declaration of my present opinions of them; especially since although they cannot but appear very much to their disadvantage, if you consider them as they are proposed, without those reasons and explanations, by which I could perhaps make them appear much less extravagant; yet I want

I want time to offer you what may be alleged to clear and countenance these notions; my design in mentioning them unto you at present being, partly, to bring some light and confirmation to divers passages of my discourse to you; partly, to shew you, that I do not (as you seem to have suspected) embrace all *Epicurus*'s principles, but dissent from him in some main things, as well as from *Aristotle* and the chymists, in others; and partly also, or rather chiefly, to intimate to you the grounds, upon which I likewise differ from *Helmont* in this, that whereas he ascribes almost all things, and even diseases themselves, to their determinate seeds, I am of opinion, that besides the peculiar fabrics of the bodies of plants and animals (and perhaps also of some metals and minerals) which I take to be effects of seminal principles, there are many other bodies in nature, which have and deserve distinct and proper names, but yet do but result from such contextures of the matter they are made of, as may without determinate seeds be effected by heat, cold, artificial mixtures and compositions, and divers other causes, which sometimes nature imploys of her own accord, and oftentimes man by his power and skill makes use of, to fashion the matter according to his intentions. This may be exemplified both in the productions of nature, and in those of art: of the first sort I might name multitudes; but to shew how slight a variation of textures, without addition of new ingredients, may procure a parcel of matter divers names, and make it be looked upon as different things;

I SHALL invite you to observe with me, that clouds, rain, hail, snow, frost, and ice, may be but water, having its parts varied as to their size and distance in respect of each other, and as to motion and rest. And among artificial productions we may take notice (to skip the crystals of tartar) of glass, regulus martis stellatus, and particularly of the sugar of lead; which, though made of that insipid metal and sour salt of vinegar, has in it a sweetness surpassing that of common sugar, and divers other qualities, which being not to be found in either of its two ingredients, must be confessed to belong to the concrete itself, upon the account of its texture.

THIS consideration premised, it will be, I hope, the more easy to persuade you, that the fire may as well produce some new textures in a parcel of matter, as destroy the old.

WHEREFORE hoping, that you have not forgot the arguments formerly employed against the doctrine of the *tria prima*; namely, that the salt, sulphur, and mercury, into which the fire seems to resolve vegetable and animal bodies, are yet compounded, not simple and elementary substances; and that (as appeared by the experiment of pompions) the *tria prima* may be made out of water; hoping, I say, that you remember these, and the other things, that I formerly represented to the same purpose, I shall now only add, that if we doubt not the truth of some of *Helmont*'s relations, we may well doubt, whether any of these heterogeneities be (I say, not pre-existent, so as to convene together, when a plant or animal is to be constituted, but) so much as in-existent in the concrete, whence they are obtained, when the chymist first goes about to resolve it. For, not to insist upon the un-inflammable spirit of such concretes, because that may be pretended to be but a mixture of phlegm and salt, the oil or sulphur of vegetables or animals is, according to him, reducible by the help of lixiviate salts into soap; as that soap is by the help of repeated distillations from a *caput mortuum* of chalk into insipid water. And as for the saline substance, that seems separable from mixt bodies, the same * *Helmont*'s trials give us cause to think, that

* *Omne autem alcali addita pinguedine in aqueum liquorem, qui tandem mera & simplex aqua fit, reducitur (ut videre est in sapone, lazurio lapide, &c.) quoties per adjuncta fixa semen pinguedinis deponit.* Helmont.

it may be a production of the fire, which, by transporting and otherwise altering the particles of the matter, does bring it to a saline nature.

FOR I know (says he, in the place formerly alleged to another purpose) a way to reduce all stones into a mere salt of equal weight with the stone, whence it was produced, and that without any of the least either sulphur or mercury; which asseveration of my author would perhaps seem less incredible to you, if I durst acquaint you with all I could say upon that subject. And hence by the way you may also conclude, that the sulphur and mercury, as they call them, that chymists are wont to obtain from compound bodies by the fire, may possibly in many cases be the productions of it; since if the same bodies had been wrought upon by the agents employed by *Helmont*, they would have yielded neither sulphur nor mercury; and those portions of them, which the fire would have presented us in the form of sulphureous and mercurial bodies, would have, by *Helmont*'s method, been exhibited to us in the form of salt.

BUT though (says *Eleutherius*) you have alleged very plausible arguments against the *tria prima*, yet I see not how it will be possible for you to avoid acknowledging, that earth and water are elementary ingredients, though not of mineral concretes, yet of all animal and vegetable bodies; since if any of these, of what sort soever, be committed to distillation, there is regularly and constantly separated from it a phlegm or aqueous part, and a *caput mortuum* or earth.

I READILY acknowledge (answers *Carneades*) it is not so easy to reject water and earth (and especially the former) as it is to reject the *tria prima*, from being the elements of mixt bodies; but it is not every difficult thing, that is impossible.

I CONSIDER then, as to water, that the chief qualities, which make men give that name to any visible substance, are, that it is fluid or liquid, and that it is insipid and inodorous. Now, as for the taste of these qualities, I think you have never seen any of those separated substances, that the chymists call phlegm, which was perfectly devoid both of taste and smell: and if you object, that yet it may be reasonably supposed, that since the whole body is liquid, the mass is nothing but elementary water faintly imbued with some of the saline or sulphureous parts of the same concrete, which it retained with it upon its separation from the other ingredients: to this I answer, that this objection would not appear so strong as it is plausible, if chymists understand the nature of fluidity and compactness; and that, as I formerly observed, to a body's being fluid there is nothing necessary, but that it be divided into parts small enough; and that these parts be put into such a motion among themselves, as to glide some this way, and some that way, along each other's surfaces. So that although a concrete were never so dry, and had not any water or other liquor in-existent in it, yet such a comminution of its parts may be made, by the fire or other agents, as to turn a great portion of them into liquor. Of this truth I will give an instance, employed by our friend here present, as one of the most conducive of his experiments to illustrate the nature of salts. If you take, then, sea-salt, and melt it in the fire, to free it from the aqueous parts, and afterwards distil it with a vehement fire from burnt clay, or any other, as dry a *caput mortuum* as you please, you will, as chymists confess by teaching it, drive over a good part of the salt in the form of a liquor. And to satisfy some ingenious men, that a great part of this liquor was still true sea-salt, brought by the operation of the fire into corpuscles so small, and perhaps so advantageously shaped, as to be capable of the form of a fluid body, he did in my presence pour to such spiritual salts a due proportion of the spirit (or salt and phlegm) of urine, whereby having evaporated the superfluous moisture, he soon obtained such

another

another concrete, both as to taste and smell, and easy sublimableness, as common salt armoniac, which you know is made up of gross and undistilled sea-salt, united with the salts of urine and of soot, which too are very near of kin to each other. And further, to manifest, that the corpuscles of sea-salt and the saline ones of the urine retain their several natures in this concrete, he mixt it with a convenient quantity of salt of tartar, and committing it to distillation, soon regained his spirit of urine in a liquid form by itself, the sea-salt staying behind with the salt of tartar. Wherefore it is very possible, that dry bodies may by the fire be reduced to liquors without any separation of elements, but barely by a certain kind of dissipation and comminution of the matter, whereby its parts are brought into a new state. And if it be still objected, that the phlegm of mixt bodies must be reputed water, because so weak a taste needs but a very small proportion of salt to impart it; it may be replied, that, for aught appears, common salt and divers other bodies, though they be distilled never so dry, and in never so close vessels, will yield each of them pretty store of a liquor, wherein though (as I lately noted) saline corpuscles abound, yet there is besides a large proportion of phlegm, as may easily be discovered by coagulating the saline corpuscles with any convenient body; as I lately told you, our friend coagulated part of the spirit of salt with spirit of urine; and as I have divers times separated a salt from oil of vitriol itself (though a very ponderous liquor, and drawn from a saline body) by boiling it with a just quantity of mercury, and then washing the newly coagulated salt from the precipitate with fair water. Now to what can we more probably ascribe this plenty of aqueous substance afforded us by the distillation of such bodies, than unto this, that among the various operations of the fire upon the matter of a concrete, divers particles of that matter are reduced to such a shape and bigness, as is requisite to compose such a liquor, as chymists are wont to call phlegm or water? How I conjecture this change may be effected, it is neither necessary for me to tell you, nor possible to do so without a much longer discourse than were now seasonable. But I desire you would, with me, reflect upon what I formerly told you, concerning the change of quicksilver into water; for that water having but a very faint taste, if any whit more than divers of those liquors, that chymists refer to phlegm, by that experiment it seems evident, that even a metalline body, and therefore much more such as are but vegetable or animal, may by a simple operation of the fire be turned in great part into water. And since those I dispute with are not yet able out of gold, or silver, or divers other concretes, to separate any thing like water; I hope I may be allowed to conclude against them, that water itself is not an universal and pre-existent ingredient of mixt bodies.

BUT as for those chymists, that, supposing with me the truth of what *Helmont* relates of the alkahest's wonderful effects, have a right to press me with his authority concerning them, and to allege, that he could transmute all reputed mixt bodies into insipid and mere water; to those I shall represent, that though his affirmations conclude strongly against the vulgar chymists (against whom I have not therefore scrupled to employ them) since they evince, that the commonly reputed principles or ingredients of things are not permanent and indestructible, since they may be further reduced into insipid phlegm differing from them all; yet till we can be allowed to examine this liquor, I think it not unreasonable to doubt whether it be not something else than mere water. For I find not any other reason given by *Helmont* of his pronouncing it so, than that it is insipid. Now sapour being an accident or an affection of matter, that relates to our tongue, palate, and other organs of taste, it may very possibly be, that the small parts of a body may be of such a size and shape, as either

by their extreme littleness, or by their slenderness, or by their figure, to be unable to pierce into and make perceptible impression upon the nerves or membranous parts of the organs of taste, and yet may be fit to work otherwise upon divers other bodies than mere water can, and consequently to disclose itself to be of a nature far enough from elementary. In silk dyed red or of any other colour, whilst many contiguous threads make up a skein, the colour of the silk is conspicuous; but if only a very few of them be looked upon, the colour will appear much fainter than before. But if you take out one simple thread, you shall not easily be able to discern any colour at all; so subtile an object having not the force to make upon the optic nerve an impression great enough to be taken notice of. It is also observed, that the best sort of oil-olive is almost tasteless; and yet I need not tell you how exceedingly distant in nature oil is from water. The liquor, into which I told you, upon the relation of *Lully* an eye-witness, that mercury might be transmuted, has sometimes but a very languid, if any taste; and yet its operations, even upon some mineral bodies, are very peculiar. Quicksilver itself also, though the corpuscles it consists of be so very small, as to get into the pores of that closest and compactest of bodies, gold, is yet (you know) altogether tasteless. And our *Helmont* several times tells us, that fair water, wherein a little quantity of quicksilver has lain for some time, though it acquire no certain taste or other sensible quality from the quicksilver; yet it has a power to destroy worms in human bodies; which he does much, but not causelessly extol. And I remember, a great lady, that had been eminent for her beauty in divers courts, confessed to me, that this insipid liquor was of all innocent washes for the face the best, that she ever met with.

AND here let me conclude my discourse, concerning such waters or liquors as I have hitherto been examining, with these two considerations. Whereof the first is, that by reason of our being wont to drink nothing but wine, beer, cyder, or other strongly tasted liquors, there may be in several of those liquors, that are wont to pass for insipid phlegm, very peculiar and distinct tastes, though unheeded (and perhaps not to be perceived) by us. For to omit what naturalists affirm of apes (and which probably may be true of divers other animals) that they have a more exquisite palate than men; among men themselves, those, that are wont to drink nothing but water, may (as I have tried in myself) discern very sensibly a great difference of tastes in several waters, which one unaccustomed to drink water would take to be all alike insipid. And this is the first of my two considerations. The other is, that it is not impossible, that the corpuscles, into which a body is dissipated by the fire, may by the operation of the same fire have their figures so altered, or may be by associations with one another brought into little masses of such a size and shape, as not to be fit to make sensible impressions on the tongue. And that you may not think such alterations impossible, be pleased to consider with me, that not only the sharpest spirit of vinegar having dissolved as much coral as it can, will coagulate with it into a substance, which, though soluble in water like salt, is incomparably less strongly tasted than the vinegar was before; but (what is more considerable) though the acid salts, that are carried up with quicksilver, in the preparation of common sublimate, are so sharp, that being moistened with water it will corrode some of the metals themselves; yet this corrosive sublimate being twice or thrice re-sublimed with a full proportion of insipid quicksilver, constitutes (as you know) that factitious concrete, which the chymists call *mercurius dulcis*; not because it is sweet, but because the sharpness of the corrosive salts is so taken away by their combination with the mercurial corpuscles, that the whole mixture, when it is prepared, is judged to be insipid.

AND

AND thus (continues *Carneades*) having given you ſome reaſons, why I refuſe to admit elementary water for a conſtant ingredient of mixt bodies, it will be eaſy for me to give you an account, why I alſo reject earth.

FOR firſt, it may well be ſuſpected, that many ſubſtances paſs among chymiſts under the name of earth, becauſe, like it, they are dry, and heavy, and fixt; which yet are very far from an elementary nature. This you will not think improbable, if you recall to mind what I formerly told you concerning what chymiſts call the dead earth of things, and eſpecially touching the copper to be drawn from the *caput mortuum* of vitriol; and if alſo you allow me to ſubjoin a caſual, but memorable experiment, made by *Johannes Agricola* upon the *terra damnata* of brimſtone. Our author then tells us (in his notes upon *Popius*) that in the year 1621 he made an oil of ſulphur; the remaining fæces he reverberated in a moderate fire fourteen days; afterwards he put them well luted up in a wind oven, and gave them a ſtrong fire for ſix hours, purpoſing to calcine the fæces to a perfect whiteneſs, that he might make ſomething elſe out of them. But coming to break the pot, he found above but very little fæces, and thoſe grey and not white; but beneath there lay a fine red regulus, which he firſt marvelled at, and knew not what to make of, being well aſſured, that not the leaſt thing, beſides the fæces of the ſulphur, came into the pot; and that the ſulphur itſelf had only been diſſolved in linſeed-oil. This regulus he found heavy and malleable almoſt as lead; having cauſed a goldſmith to draw him a wire of it, he found it to be of the faireſt copper, and ſo rightly coloured, that a Jew of *Prague* offered him a great price for it. And of this metal he ſays he had twelve *loth* (or ſix ounces) out of one pound of aſhes or fæces. And this ſtory may well incline us to ſuſpect, that ſince the *caput mortuum* of the ſulphur was kept ſo long in the fire, before it was found to be any thing elſe than a *terra damnata*, there may be divers other reſidences of bodies, which are wont to paſs only for the terreſtrial fæces of things, and therefore to be thrown away, as ſoon as the diſtillation or calcination of the body that yielded them is ended; which yet, if they were long and ſkilfully examined by the fire, would appear to be differing from elementary earth. And I have taken notice of the unwarrantable forwardneſs of common chymiſts to pronounce things uſeleſs fæces, by obſerving how often they reject the *caput mortuum* of verdigreaſe; which is yet ſo far from deſerving that name, that not only by ſtrong fires, and convenient additaments, it may in ſome hours be reduced into copper, but with a certain flux powder I ſometimes make for recreation, I have in two or three minutes obtained that metal from it. To which I may add, that having for trial's ſake kept Venerian talc in no leſs a heat than that of a glaſs-furnace, I found, after all the brunt of the fire it had indured, the remaining body, though brittle and diſcoloured, had not loſt very much of its former bulk, and ſeemed ſtill to be nearer of kin to talc, than to meer earth. And I remember too, that a candid mineraliſt, famous for his ſkill in trying of ores, requeſting me one day to procure him a certain American mineral earth of a virtuoſo, who, he thought, would not refuſe me; I inquired of him, why he ſeemed ſo greedy of it: he confeſſed to me, that this gentleman having brought that earth to the public ſay-maſters; and upon their being unable by any means to bring it to fuſion, or make it fly away, he (the relator) had procured a little of it; and having tried it with a peculiar flux, ſeparated from it near a third part of pure gold; ſo great miſtakes may be committed in haſtily concluding things to be uſeleſs earth.

NEXT, it may be ſuppoſed, that as in the reſolution of bodies by the fire, ſome of the diſſipated parts may, by their various occurſion occaſioned by the heat, be brought

to ſtick together ſo cloſely, as to conſtitute corpuſcles too heavy for the fire to carry away; the aggregate of which corpuſcles is wont to be called aſhes of earth; ſo other agents may reſolve the concrete into minute parts after ſo differing a manner, as not to produce any *caput mortuum*, or dry and heavy body. As you may remember *Helmont* above informed us, that with his great diſſolvent he divided a coal into two liquid and volatile bodies equiponderant to the coal, without any dry or fixt reſidence at all.

AND indeed, I ſee not, why it ſhould be neceſſary, that all agents, that reſolve bodies into portions of differing qualified matter, muſt work on them the ſame way, and divide them into juſt ſuch parts, both for nature and number, as the fire diſſipates them into. For ſince (as I noted before) the bulk and ſhape of the ſmall parts of bodies, together with their fitneſs and unfitneſs to be eaſily put into motion, may make the liquors, or other ſubſtances ſuch corpuſcles compoſe, as much to differ from each other, as do ſome of the chymical principles: why may not ſomething happen in this caſe, not unlike what is uſual in the groſſer diviſions of bodies by mechanical inſtruments? where we ſee, that ſome tools reduce wood, for inſtance, into parts of ſeveral ſhapes, bigneſs, and other qualities, as hatchets and wedges divide it into groſſer parts; ſome more long and ſlender, as ſplinters; and ſome more thick and irregular, as chips; but all of conſiderable bulk: but files and ſaws make a comminution of it into duſt; which, as all the others, is of the more ſolid ſort of parts; whereas others divide it into long and broad, but thin and flexible parts, as do planes. And of this kind of parts itſelf there is alſo a variety, according to the difference of the tools employed to work on the wood; the ſhavings made by the plane being in ſome things differing from thoſe ſhives or thin and flexible pieces of wood, that are obtained by borers, and theſe from ſome others obtainable by other tools. Some chymical examples applicable to this purpoſe I have elſewhere given you. To which I may add, that whereas, in a mixture of ſulphur and ſalt of tartar well melted and incorporated together, the action of pure ſpirit of wine digeſted on it is to ſeparate the ſulphureous from the alcalizate parts, by diſſolving the former, and leaving the latter, the action of wine (probably upon the ſcore of its copious phlegm) upon the ſame mixture, is to divide it into corpuſcles conſiſting of both alcalizate and ſulphureous parts united. And if it be objected, that this is but a factitious concrete; I anſwer, that however the inſtance may ſerve to illuſtrate what I propoſed, if not to prove it; and that nature herſelf doth in the bowels of the earth make decompounded bodies, as we ſee in vitriol, cinnabar, and even in ſulphur itſelf; I will not urge, that the fire divides new milk into five differing ſubſtances; but rennet and acid liquors divide it into a coagulated matter and a thin whey: and on the other ſide, churning divides it into butter and butter-milk, which may either of them yet be reduced to other ſubſtances differing from the former. I will not preſs this, I ſay, nor other inſtances of this nature, becauſe I cannot in few words anſwer what may be objected, that theſe concretes ſequeſtered without the help of the fire may by it be further divided into hypoſtatical principles. But I will rather repreſent, that whereas the ſame ſpirit of wine will diſſociate the parts of camphire, and make them one liquor with itſelf; aqua fortis will alſo disjoin them, and put them into motion; but ſo as to keep them together, and yet alter their texture into the form of an oil. I know alſo an uncompounded liquor, that an extraordinary chymiſt would not allow to be ſo much as ſaline, which doth (as I have tried) from coral itſelf (as fixt as divers judicious writers aſſert that concrete to be) not only obtain a noble tincture, without the intervention of nitre or other ſalts, but will carry over the tincture in diſtillation.

distillation. And if some reasons did not forbid me, I could now tell you of a menstruum I make myself, that doth more oddly dissociate the parts of minerals very fixt in the fire. So that it seems not incredible, that there may be some agent or way of operation found, whereby this or that concrete, if not all firm bodies, may be resolved into parts so very minute and so apt to stick close to one another, that none of them may be fixt enough to stay behind in a strong fire, and to be incapable of distillation; nor consequently to be looked upon as earth. But to return to *Helmont*, the same author somewhere supplies me with another argument against the earth's being such an element, as my adversaries would have it. For he somewhere affirms, that he can reduce all the terrestrial parts of mixt bodies into insipid water; whence we may argue against the earth's being one of their elements, even from that notion of elements, which you may remember *Philoponus* recited out of *Aristotle* himself, when he lately disputed for his chymists against *Themistius*. And here we may on this occasion consider, that since a body, from which the fire hath driven away its looser parts, is wont to be looked upon as earth, upon the account of its being endowed with both these qualities, tastlessness and fixedness (for salt of tartar, though fixed, passes not among the chymists for earth, because it is strongly tasted) if it be in the power of natural agents to deprive the *caput mortuum* of a body of either of those two qualities, or to give them both to a portion of matter, that had them not both before, the chymists will not easily define what part of a resolved concrete is earth, and make out, that that earth is a primary, simple, and indestructable body. Now there are some cases, wherein the more skilful of the vulgar chymists themselves pretend to be able, by repeated cohobations, and other fit operations, to make the distilled parts of a concrete bring its own *caput mortuum* over the helm, in the form of a liquor; in which state being both fluid and volatile; you will easily believe it would not be taken for earth. And indeed by a skilful, but not vulgar, way of managing some concretes, there may be more effected in this kind, than you perhaps would easily think. And on the other side, that either earth may be generated, or at least bodies, that did not before appear to be near totally earth, may be so altered as to pass for it, seems very possible, if * *Helmont* have done that by art, which he mentions in several places; especially where he says, that he knows ways, whereby sulphur once dissolved is all of it fixed into a terrestrial powder, and the whole body of salt-petre may be turned into earth: which last he elsewhere says is done by the odour only of a certain sulphureous fire. And in another place he mentions one way of doing this, which I cannot give you an account of; because the materials I had prepared for trying it, were by a servant's mistake unhappily thrown away.

AND, these last arguments may be confirmed by the experiment I have often had occasion to mention, concerning the mint I produced out of water. And partly by an observation of *Rondoletius* concerning the growth of animals also, nourished but by water, which I remembered not to mention, when I discoursed to you about the production of things out of water. This diligent writer then, in his instructive book of fishes, affirms that his wife kept a fish in a glass of water without any other food for three years; in which space it was constantly augmented, till at last it could not come out of the place, at which it was put in, and at length was too big for the Lib. 1. cap. 2.

* *Novi item modos, quibus totum sal petræ in terram convertitur, totumque sulphur semel dissolutum fixetur in pulverem terreum.* Helmont. in Compl. atque Mist. Elementor. sect. 24.

glaſs itſelf, though that were of a large capacity. And becauſe there is no juſt reaſon to doubt, that this fiſh, if diſtilled, would have yielded the like differing ſubſtances with other animals; and however, becauſe the mint, which I had out of water, afforded me upon diſtillation a good quantity of charcoal; I think I may from thence infer, that earth itſelf may be produced out of water; or if you pleaſe, that water may be tranſmuted into earth: and conſequently, that though it could be proved, that earth is an ingredient actually in-exiſtent in the vegetable and animal bodies, whence it may be obtained by fire; yet it would not neceſſarily follow, that earth, as a pre-exiſtentt element, does with other principles convene to make up thoſe bodies, whence it ſeems to have been ſeparated.

AFTER all is ſaid (ſays *Eleutherius*) I have yet ſomething to object, that I cannot but think conſiderable, ſince *Carneades* himſelf alleged it as ſuch; for, (continues *Eleutherius* ſmiling) I muſt make bold to try, whether you can as luckily anſwer your own arguments, as thoſe of your antagoniſts; I mean (purſues he) that part of your conceſſions, wherein you cannot but remember, that you ſupplied your adverſaries with an example to prove, that there may be elementary bodies, by taking notice, that gold may be an ingredient in a multitude of differing mixtures, and yet retain its nature, notwithſtanding all that the chymiſts by their fires and corroſive waters are able to do to deſtroy it.

I SUFFICIENTLY intimated to you at that time (replies *Carneades*) that I propoſed this example, chiefly to ſhew you, how nature may be conceived to have made elements, not to prove, that ſhe actually has made any; and you know, that *à poſſe ad eſſe* the inference will not hold. But (continues *Carneades*) to anſwer more directly to the objection drawn from gold, I muſt tell you, that though I know very well, that divers of the more ſober chymiſts have complained of the vulgar chymiſts, as of mountebanks or cheats, for pretending ſo vainly, as hitherto they have done, to deſtroy gold; yet I know a certain menſtruum (which our friend has made, and intends ſhortly to communicate to the ingenious) of ſo piercing and powerful a quality; that if notwithſtanding much care, and ſome ſkill, I did not much deceive myſelf, I have with it really deſtroyed even refined gold, and brought it into a metalline body of another colour and nature, as I found by trials purpoſely made. And if ſome juſt conſiderations did not for the preſent forbid it, I could perchance here ſhew you by another experiment or two of my own trying, that ſuch menſtruums may be made, as to entice away and retain divers parts from bodies, which even the more judicious and experienced Spagyriſts have pronounced irreſoluble by the fire; though (which I deſire you would mark) in neither of theſe inſtances, the gold or precious ſtones be analyſed into any of the *tria prima*, but only reduced to new concretes. And indeed there is a great diſparity betwixt the operations of the ſeveral agents, whereby the parts of a body come to be diſſipated. As if (for inſtance) you diſſolve the purer ſort of vitriol in common water, the liquor will ſwallow up the mineral, and ſo diſſociate its corpuſcles, that they will ſeem to make up but one liquor with thoſe of the water; and yet each of theſe corpuſcles retains its nature and texture, and remains a vitriolate and compounded body. But if the ſame vitriol be expoſed to a ſtrong fire, it will then be divided, not only, as before, into ſmaller parts, but into heterogeneous ſubſtances, each of the vitriolate corpuſcles, that remained entire in the water, being itſelf upon the deſtruction of its former texture diſſipated or divided into new particles of differing qualities. But inſtances more fitly applicable to this purpoſe I have already given you. Wherefore to return to what I told you about the deſtruction

tion of gold; that experiment invites me to represent to you, that though there were either saline, or sulphureous, or terrestrial portions of matter, whose parts were so small, so firmly united together, or of a figure so fit to make them cohere to one another (as we see, that in quicksilver, broken into little globes, the parts brought to touch one another do immediately re-imbody) that neither the fire, nor the usual agents, employed by chymists, are piercing enough to divide their parts, so as to destroy the texture of the single corpuscles; yet it would not necessarily follow, that such permanent bodies were elementary; since it is possible, there may be agents found in nature, some of whose parts may be of such a size and figure, as to take better hold of some parts of these seemingly elementary corpuscles, than these parts do of the rest; and consequently may carry away such parts with them, and so dissolve the texture of the corpuscle by pulling its parts asunder. And if it be said, that at least we may this way discover the elementary ingredients of things, by observing into what substances these corpuscles, that were reputed pure, are divided; I answer, that it is not necessary, that such a discovery should be practicable. For if the particles of the dissolvent do take such firm hold of those of the dissolved body, they must constitute together new bodies, as well as destroy the old; and the strict union, which, according to this hypothesis, may well be supposed betwixt the parts of the emergent bodies will make it as little to be expected, that they should be pulled asunder, but by little parts of matter, that to divide them, associate themselves, and stick extremely close to those of them, which they sever from their former adherents. Besides that it is not impossible, that a corpuscle, supposed to be elementary, may have its nature changed, without suffering a divorce of its parts, barely by a new texture effected by some powerful agent; as I formerly told you, the same portion of matter may easily, by the operation of the fire, be turned at pleasure into the form of a brittle and transparent, or an opacous and malleable body.

AND indeed, if you consider how far the bare change of texture, whether made by art or nature (or rather by nature with or without the assistance of man) can go in producing such new qualities in the same parcel of matter, and how many inanimate bodies (such as are all the chymical productions of the fire) we know are denominated and distinguished, not so much by any imaginary substantial form, as by the aggregate of these qualities; if you consider these things, I say, and that the varying of either figure, or the size, or the motion, or the situation, or connexion of the corpuscles whereof any of these bodies is composed, may alter the fabric of it, you will possibly be invited to suspect with me, that there is no great need, that nature should always have elements before-hand, whereof to make such bodies as we call mixts. And that it is not so easy as chymists and others have hitherto imagined, to discern, among the many differing substances, that may without any extraordinary skill be obtained from the same portion of matter, which ought to be esteemed, exclusively to all the rest, its in-existent elementary ingredients, much less to determine, what primogeneal and simple bodies convened together to compose it. To exemplify this, I shall add, to what I have already on several occasions represented, but this single instance:

You may remember *(Eleutherius)* that I formerly intimated to you, that besides mint and pompions, I produced divers other vegetables of very differing natures out of water. Wherefore you will not, I presume, think it incongruous to suppose, that when a slender vine-slip is set into the ground, and takes root there, it may likewise receive its nutriment from the water attracted out of the earth by its roots, or impell-

ed by the warmth of the ſun, or preſſure of the ambient air into the pores of them. And this you will the more eaſily believe, if you ever obſerved, what a ſtrange quantity of water will drop out of a wound given to the vine, in a convenient place, at a ſeaſonable time in the ſpring; and how little of taſte or ſmell this aqua vitæ, as phyſicians call it, is endowed with, notwithſtanding what concoction or alteration it may receive in its paſſage through the vine, to diſcriminate it from common water. Suppoſing then this liquor, at its firſt entrance into the roots of the vine, to be common water; let us a little conſider, how many various ſubſtances may be obtained from it; though, to do ſo, I muſt repeat ſomewhat, that I had a former occaſion to touch upon. And firſt, this liquor being digeſted in the plant, and aſſimilated by the ſeveral parts of it, is turned into the wood, bark, pith, leaves, *&c.* of the vine; the ſame liquor may be further dried, and faſhioned into vine-buds, and theſe a while after are advanced unto ſour grapes, which expreſſed yield verjuice, a liquor very differing in ſeveral qualities both from wine and other liquors obtainable from the vine: theſe ſour grapes, being by the heat of the ſun concocted and ripened, turn to well taſted grapes; theſe, if dried in the ſun and diſtilled, afford a fœtid oil and a piercing empyreumatical ſpirit, but not a vinous ſpirit; theſe dried grapes or raiſins, boiled in a convenient proportion of water, make a ſweet liquor, which, being betimes diſtilled, afford an oil and ſpirit much like thoſe of the raiſins themſelves; if the juice of the grapes be ſqueezed out, and put to ferment, it firſt becomes a ſweet and turbid liquor, then grows leſs ſweet and more clear, and then affords in common diſtillations not an oil, but a ſpirit, which, though inflammable like oil, differs much from it, in that it is not fat, and that it will readily mingle with water. I have likewiſe without addition obtained in proceſs of time (and by an eaſy way, which I am ready to teach you) from one of the nobleſt ſorts of wine, pretty ſtore of pure and curiouſly figured cryſtals of ſalt, together with a great proportion of a liquor as ſweet almoſt as honey; and theſe I obtained not from muſt, but true and ſprightly wine; beſides the vinous liquor, the fermented juice of grapes is partly turned into liquid drops or lees, and partly into that cruſt or dry feculency, that is commonly called tartar; and this tartar may by the fire be eaſily divided into five differing ſubſtances; four of which are not acid, and the other not ſo manifeſtly acid as the tartar itſelf. The ſame vinous juice after ſome time, eſpecially if it be carefully kept, degenerates into that very ſour liquor called vinegar; from which you may obtain by the fire a ſpirit and a cryſtalline ſalt, differing enough from the ſpirit and lixiviate ſalt of tartar. And if you pour the dephlegmed ſpirit of the vinegar upon the ſalt of tartar, there will be produced ſuch a conflict or ebullition, as if there were ſcarce two more contrary bodies in nature; and oftentimes in this vinegar you may obſerve part of the matter to be turned into an innumerable company of ſwimming animals; which our friend having divers years ago obſerved, hath in one of his papers taught us, how to diſcover clearly without the help of a microſcope.

INTO all theſe various ſchemes of matter, or differingly-qualified bodies, beſides divers others, that I purpoſely forbear to mention, may the water, that is imbibed by the roots of the vine, be brought; partly by the formative power of the plant, and partly by ſupervenient agents or cauſes, without the viſible concurrence of any extraneous ingredient. But if we be allowed to add to the productions of this tranſmuted water a few other ſubſtances, we may much increaſe the variety of ſuch bodies; although in this ſecond ſort of productions, the vinous parts ſeem ſcarce to retain any thing of the much more fixed bodies, wherewith they were mingled, but only to have,

by

by their mixture with them, acquired ſuch a diſpoſition, that in their receſs, occaſioned by the fire, they came to be altered as to ſhape, or bigneſs, or both, and aſſociated after a new manner. Thus, as I formerly told you, I did, by the addition of a *caput mortuum* of antimony, and ſome other bodies unfit for diſtillation, obtain from crude tartar ſtore of a very volatile and cryſtalline ſalt, differing very much in ſmell and other qualities from the uſual ſalts of tartar.

BUT (ſays *Eleutherius*) interrupting him at theſe words) if you have no reſtraint upon you, I would very gladly, before you go any further, be more particularly informed, how you make this volatile ſalt; becauſe, you know, that ſuch multitudes of chymiſts have, by a ſcarce imaginable variety of ways, attempted in vain the volatilization of the ſalt of tartar, that divers learned Spagyriſts ſpeak, as if it were impoſſible to make any thing out of tartar, that ſhall be volatile in a ſaline form, or, as ſome of them expreſs it, *in forma ſicca.* I am very far from thinking (anſwers *Carneades*) that the ſalt I have mentioned is that, which *Paracelſus* and *Helmont* mean, when they ſpeak of *ſal tartari volatile*, and aſcribe ſuch great things to it. For the ſalt I ſpeak of falls extremely ſhort of thoſe virtues, not ſeeming in its taſte, ſmell, and other obvious qualities, to differ very much (though ſomething it does differ) from ſalt of hartſhorn, and other volatile ſalts drawn from the diſtilled parts of animals. Nor have I yet made trials enough to be ſure, that it is a pure ſalt of tartar, without participating any thing at all of the nitre, or antimony. But becauſe it ſeems more likely to proceed from the tartar, than from any of the other ingredients, and becauſe the experiment is in itſelf not ignoble, and luciferous enough (as ſhewing a new way to produce a volatile ſalt, contrary to acid ſalts, from bodies, that otherwiſe are obſerved to yield no ſuch liquor, but either only, or chiefly acid ones;) I ſhall, to ſatisfy you, acquaint you, before any of my other friends, with the way I now uſe (for I have formerly uſed ſome others) to make it.

TAKE then, of good antimony, ſalt-petre, and tartar, of each an equal weight, and of quick-lime half the weight of any one of them; let theſe be powdered and well mingled. This done, you muſt have in readineſs a long neck or retort of earth, which muſt be placed in a furnace for a naked fire, and have at the top of it a hole of a convenient bigneſs, at which you may caſt in the mixture, and preſently ſtop it up again. This veſſel being fitted with a large receiver, muſt have fire made under it, till the bottom of the ſides be red hot, and then you muſt caſt in the above prepared mixture, by about half a ſpoonful (more or leſs) at a time, at the hole made for that purpoſe; which being nimbly ſtopt, the fumes will paſs into the receiver, and condenſe there into a liquor, that being rectified will be of a pure golden colour, and carry up that colour to a great height. This ſpirit abounds in the ſalt I told you of, part of which may eaſily enough be ſeparated by the way I uſe in ſuch caſes, which is, to put the liquor into a glaſs egg, or bolt-head with a long and narrow neck. For if this be placed a little inclining in hot ſand, there will ſublime up a fine ſalt, which, as I told you, I find to be much of kin to the volatile ſalts of animals: for like them it has a ſaltiſh, not an acid ſalt; it hiſſes upon the affuſion of ſpirit of nitre, or oil of vitriol; it precipitates corals diſſolved in ſpirit of vinegar; it turns the blue ſyrup of violets immediately green; it preſently turns the ſolution of ſublimate into a milky whiteneſs; and in ſum, has divers operations like thoſe, that I have obſerved in that ſort of ſalts, to which I have reſembled it; and is ſo volatile, that, for diſtinction-ſake, I call it *ſal tartari fugi-*

fugitivus. What virtues it may have in physic, I have not yet had the opportunity to try; but I am apt to think they will not be despicable. And besides that, a very ingenious friend of mine tells me, he hath done great matters against the stone with a preparation not very much differing from ours; a very experienced German chymist finding, that I was unacquainted with the ways of making this salt, told me, that in a great city in his country, a noted chymist prizes it so highly, that he had a while since procured a privilege from the magistrates, that none but he, or by his licence, should vend a spirit made almost after the same way with mine, save that he leaves out one of the ingredients, namely the quick-lime. But (continues *Carneades*) to resume my former discourse, where your curiosity interrupted it;

IT is also a common practice in *France* to bury thin plates of copper in the marc (as the French call it) or husks of grapes, whence the juice hath been squeezed out in the vine press; and by this means the more saline parts of those husks, working by little and little upon the copper, coagulate themselves with it into that blueish green substance we in English call verdigrease. Of which I therefore take notice, because having distilled it in a naked fire, I found, as I expected, that by the association of the saline with the metalline parts, the former were so altered, that the distilled liquor, even without rectification, seemed by smell and taste, strong almost like aqua fortis, and very much surpassed the purest and most rectified spirit of vinegar, that ever I made. And this spirit I therefore ascribe to the salt of the husks altered by their co-mixture with the copper (though the fire afterwards divorce and transmute them) because I found this latter in the bottom of the retort in the form of a crocus or reddish powder; and because copper is of too sluggish a nature to be forced over in close vessels by no stronger a heat. And that, which is also somewhat remarkable in the distillation of good verdigrease (or at least of that sort that I used) is this, that I never could observe, that it yielded me any oil (unless a little black slime, which was separated in rectification may pass for oil) though both tartar and vinegar (especially the former) will by distillation yield a moderate proportion of it. If likewise you pour spirit of vinegar upon calcined lead, the acid salt of the liquor will by its commixture with the metalline parts, though insipid, acquire in few hours a more than saccharine sweetness; and these saline parts being by a strong fire distilled from the lead, wherewith they were imbodied, will, as I formerly also noted to a different purpose, leave the metal behind them altered in some qualities from what it was, and will themselves ascend, partly in the form of an unctuous body or oil, partly in that of phlegm, but for the greatest part in the form of a subtile spirit, indowed, besides divers new qualities, which I am not now willing to take notice of, with a strong smell very much other than that of vinegar, and a piercing taste quite differing both from the sourness of the spirit of vinegar, and the sweetness of the sugar of lead.

To be short, as the difference of bodies may depend meerly upon that of the schemes, whereinto their common matter is put; so the seeds of things, the fire and the other agents are able to alter the minute parts of a body (either by breaking them into smaller ones of differing shapes, or by uniting together these fragments with the unbroken corpuscles, or such corpuscles among themselves) and the same agents, partly by altering the shape or bigness of the constituent corpuscles of a body, partly by driving away some of them, partly by blending others with them, and partly by some new manner of connecting them, may give the whole portion of matter a new texture of its minute parts, and thereby make it deserve a new and distinct name. So that, according as the small parts of matter recede from each other, or work

upon each other, or are connected together after this or that determinate manner, a body of this or that denomination is produced, as some other body happens thereby to be altered or destroyed.

SINCE then those things, which chymists produce by the help of the fire, are but inanimate bodies; since such fruits of the chymists skill differ from one another but in so few qualities, that we see plainly, that by fire, and other agents we can employ, we can easily enough work as great alterations upon matter, as those, that are requisite to change one of these chymical productions into another; since the same portion of matter may, without being compounded with any extraneous body, or at least element, be made to put on such a variety of forms, and consequently to be (successively) turned into so many differing bodies; and since the matter, cloathed with so many differing forms, was originally but water, and that in its passage through so many transformations, it was never reduced into any of those substances, which are reputed to be the principles or elements of mixt bodies, except the violence of the fire, which itself divides not bodies into perfectly simple or elementary substances, but into new compounds; since, I say, these things are so, I see not, why we must needs believe, that there are any primogeneal and simple bodies, of which, as of pre-existent elements, nature is obliged to compound all others. Nor do I see, why we may not conceive, that she may produce the bodies accounted mixt out of one another, by variously altering and contriving their minute parts, without resolving the matter into any such simple or homogeneous substances, as are pretended. Neither, to dispatch, do I see, why it should be counted absurd to think, that when a body is resolved by the fire into its supposed simple ingredients, those substances, are not true and proper elements, but rather were, as it were, accidentally produced by the fire, which by dissipating a body into minute parts does, if those parts be shut up in close vessels, for the most part necessarily bring them to associate themselves after another manner than before, and so bring them into bodies of such different consistences, as the former texture of the body and concurrent circumstances make such disbanded particles apt to constitute; as experience shews us (and I have both noted it, and proved it already) that as there are some concretes whose parts, when dissipated by fire, are fitted to be put into such schemes of matter as we call oil, and salt, and spirit; so there are others, such as are especially the greatest part of minerals, whose corpuscles being of another size or figure, or perhaps contrived another way, will not in the fire yield bodies of the like consistencies, but rather others of differing textures; not to mention, that from gold and some other bodies, we see not, that the fire separates any distinct substances at all; nor that even those similar parts of bodies, which the chymists obtain by the fire, are the elements, whose names they bear, but compound bodies, upon which, for their resemblance to them in consistence, or some other obvious quality, chymists have been pleased to bestow such appellations.

The CONCLUSION.

THESE last words of *Carneades* being soon after followed by a noise, which seemed to come from the place, where the rest of the company was, he took it for a warning, that it was time for him to conclude or break off his discourse; and told his friend, by this time, I hope, you see, *Eleutherius*, that if *Helmont*'s experiments be true, it is no absurdity to question, whether that doctrine be one, that doth not assert any elements in the sense before explained. But because that as divers of my arguments suppose the marvellous power of the alkahest in the analyzing of bodies, so the effects ascribed to that power are so unparallelled and stupendous, that though I am not sure, but that there may be such an agent, yet little less than αὐτοψία seems requisite to make a man sure there is. And consequently I leave it you to judge, how far those of my arguments, that are built upon alkahestical operations, are weakened by that liquor's being matchless; and shall therefore desire you not to think, that I propose this paradox, that rejects all elements, as an opinion equally probable with the former part of my discourse. For by that, I hope, you are satisfied, that the arguments, wont to be brought by chymists to prove, that all bodies consist of either three principles, or five, are far from being so strong as those, that I have employed to prove, that there is not any certain and determinate number of such principles or elements to be met with universally in all mixt bodies. And, I suppose, I need not tell you, that these anti-chymical paradoxes might have been managed more to their advantage; but that having not confined my curiosity to chymical experiments, I, who am but a young man, and younger chymist, can yet be but slenderly furnished with them, in reference to so great and difficult a task as you imposed upon me: besides that, to tell you the truth, I durst not employ some even of the best experiments I am acquainted with, because I must not yet disclose them. But however, I think I may presume, that what I have hitherto discoursed, will induce you to think, that chymists have been much more happy in finding experiments than the causes of them; or in assigning the principles, by which they may best be explained. And indeed, when in the writings of *Paracelsus* I meet with such phantastic and unintelligible discourses, as that writer often puzzles and tires his reader with, fathered upon such excellent experiments, as though he seldom clearly teaches, I often find he knew; methinks the chymists, in their searches after truth, are not unlike the navigators of *Solomon*'s *Tarshish* fleet, who brought home from their long and tedious voyages, not only gold, and silver, and ivory, but apes and peacocks too: for so the writings of several (for I say not, all) of your hermetic philosophers present us, together with divers substantial and noble experiments, theories, which either like peacocks feathers make a great shew, but are neither solid nor useful; or else like apes, if they have some appearance of being rational, are blemished with some absurdity or other, that, when they are attentively considered, make them appear ridiculous.

CARNEADES having thus finished his discourse against the received doctrines of the elements, *Eleutherius* judging he should not have time to say much to him before their separation, made some haste to tell him; I confess, *Carneades*, that you have said more in favour of your paradoxes than I expected. For though divers of the experiments you have mentioned are no secrets, and were not unknown to me; yet besides

that you have added many of your own unto them, you have laid them together in such a way, and applied them to such purposes, and made such deductions from them, as I have not hitherto met with.

BUT though I be therefore inclined to think, that *Philoponus*, had he heard you, would scarce have been able in all points to defend the chymical hypothesis against the arguments, wherewith you have opposed it; yet methinks, that however your objections seem to evince a great part of what they pretend to, yet they evince it not all; and the numerous trials of those you call the vulgar chymists, may be allowed to prove something too.

WHEREFORE, if it be granted you, that you have made it probable;

FIRST, that the differing substances, into which mixt bodies are wont to be resolved by the fire, are not of a pure and an elementary nature, especially for this reason, that they yet retain so much of the nature of the concrete that afforded them, as to appear to be yet somewhat compounded, and oftentimes to differ in one concrete from principles of the same denomination in another;

NEXT, that as to the number of these differing substances, neither is it precisely three, because in most vegetable and animal bodies earth and phlegm are also to be found among their ingredients; nor is there any one determinate number, into which the fire (as it is wont to be employed) does precisely and universally resolve all compound bodies whatsoever, as well minerals as others, that are reputed perfectly mixt:

LASTLY, that there are divers qualities, which cannot well be referred to any of these substances, as if they primarily resided in it and belonged to it; and some other qualities, which, though they seem to have their chief and most ordinary residence in some one of these principles or elements of mixt bodies, are not yet so deducible from it, but that also some more general principles must be taken in, to explicate them:

IF, I say, the chymists (continues *Eleutherius*) be so liberal as to make you these three concessions, I hope you will, on your part, be so civil and equitable, as to grant them these three other propositions, namely;

FIRST, that divers mineral bodies, and therefore probably all the rest, may be resolved into a saline, a sulphureous, and a mercurial part; and that almost all vegetable and animal concretes may, if not by the fire alone, yet by a skilful artist employing the fire as his chief instrument, be divided into five differing substances, salt, spirit, oil, phlegm and earth; of which the three former, by reason of their being so much more operative than the two latter, deserve to be looked upon as the three active principles, and by way of eminence to be called the three principles of mixt bodies.

NEXT, that these principles, though they be not perfectly devoid of all mixture, yet may without inconvenience be stiled the elements of compounded bodies, and bear the names of those substances, which they most resemble, and which are manifestly predominant in them; and that especially for this reason, that none of these elements is divisible by the fire into four or five differing substances, like the concrete, whence it was separated.

LASTLY, that divers of the qualities of a mixt body, and especially the medical virtues, do for the most part lodge in some one or other of its principles, and may therefore usefully be sought for in that principle severed from the others.

AND in this also (pursues *Eleutherius*) methinks both you and the chymists may easily agree, that the surest way is to learn by particular experiments, what differing parts particular bodies do consist of, and by what ways (either actual or potential fire) they

they may best and most conveniently be separated, as without relying too much upon the fire alone, for the resolving of bodies, so without fruitlessly contending to force them into more elements than nature made them up of, or strip the severed principles so naked, as by making them exquisitely elementary, to make them almost useless.

THESE things (subjoins *Eleutherius*) I propose, without despairing to see them granted by you; not only because I know, that you so much prefer the reputation of candour before that of subtilty, that your having once supposed a truth would not hinder you from imbracing it, when clearly made out to you; but because, upon the present occasion, it will be no disparagement to you to recede from some of your paradoxes, since the nature and occasion of your past discourse did not oblige you to declare your own opinions, but only to personate an antagonist of the chymists. So that (concludes he, with a smile) you may now, by granting what I propose, add the reputation of loving the truth sincerely to that of having been able to oppose it subtilely.

CARNEADES's haste forbidding him to answer this crafty piece of flattery; till I shall (says he) have an opportunity to acquaint you with my own opinions about the controversies I have been discoursing of, you will not, I hope, expect I should declare my own sense of the argument I have employed. Wherefore I shall only tell you thus much at present; that though not only an acute naturalist, but even I myself could take plausible exceptions at some of them; yet divers of them too are such as will not perhaps be readily answered, and will reduce my adversaries, at least, to alter and reform their hypothesis, I perceive I need not mind you, that the objections I made against the quaternary of elements and ternary of principles, needed not to be opposed so much against the doctrines themselves; either of which, especially the latter, may be much more probably maintained than hitherto it seems to have been, by those writers for it (I have met with) as against the unaccurateness and the unconcludingness of the analytical experiments vulgarly relied on to demonstrate them.

AND therefore, if either of the two examined opinions, or any other theory of elements, shall, upon rational and experimental grounds, be clearly made out to me; it is obliging, but not irrational, in you to expect, that I shall not be so far in love with my disquieting doubts, as not to be content to change them for undoubted truths. And (concludes *Carneades* smiling) it were no great disparagement for a sceptic to confess to you, that as unsatisfied as the past discourse may have made you think me with the doctrines of the Peripatetics, and the chymists, about the elements and principles, I can yet so little discover what to acquiesce in, that perchance the enquiries of others have scarce been more unsatisfactory to me, than my own have been to myself.

EXPERIMENTS

AND

NOTES

ABOUT THE

Produciblenefs of Chymical Principles;

Being Parts of an Appendix, defigned to be added to the SCEPTICAL CHYMIST.

The Author's Preface.

HAVING long fince obferved, that a great part of the erroneous reafonings and conclufions of learned men, as well about phyfical, as other fubjects, proceeds not fo much from their making bad illations, as from their affuming falfe or uncertain principles, to draw their confequences from; I thought, I could fcarce mifpend the time I allowed myfelf for chymical ftudies, if I employed fome part of it, in examining the doctrine about the principles of natural bodies. Upon this account I did, in the year 1661, venture abroad my *Sceptical Chymift*, to acquaint the inquifitive with my doubts, and excite them to a more thorough difquifition of a fubject, fo confiderable, as well to natural philofophy, as to phyfic. This difcourfe being once publifhed in Englifh, and foon after in Latin; I thought fit to wait a while, that I might learn what judgment would be made of it, and whether any of the chymifts would return an anfwer to it: and in the mean while, to gratify thofe, that appeared defirous of having it foon reprinted, I gathered divers notes (fome of them confiderable for bulk) to be inferted here and there, as inlargements in the next edition, whofe volume I was not unwilling fomewhat to increafe, not only becaufe I thought truth in general, a thing worthy, that the lovers of it fhould take pains to difcover, and eftablifh it, but becaufe I looked upon the truth inquired after in the *Sceptical Chymift*, as of no mean importance; efpecially fince the miftakes, that very many have made about it, have, I fear, not only been prejudicial to natural philofophy, but have, by feveral men, as well learned as ignorant, been adopted both into the fpeculations, and practice of phyficians; whofe art being converfant about

the health and life of man, doctrinal errors in it cannot but be dangerous, and therefore fit, as much as is possible, to be solicitously avoided, or removed. These inconveniencies I hoped might in some measure be obviated, if it were further made appear by experiments as well as reasonings, that the vulgar doctrine of the *tria prima* is, at least, very questionable, or uncertain and very narrow. For the contrary persuasions about these principles have missed divers learned men to give, and take up with precarious and superficial accounts of divers phænomena of nature; by which means they have been diverted from employing their wits (wherein divers of them are happy) in the investigation of the true and fundamental causes, the discovery whereof would have enabled them, instead of dark and superficial, to give intelligible and particular explications of those phænomena, and many others. The difference between the accounts given of the same phænomena, by the hypostatical and by the mechanical principles, may be seen exemplified by particular instances in other papers; wherefore, I shall proceed to observe as to physic, that besides the mistakes, which, I doubt, divers learned men have, by another valuation of the doctrine of the *tria prima*, been led into, in relation to the causes of divers things, that occur to human bodies, and even in chymical operations; besides this, I say, I fear, that the too confident opinion of the doctrine I question has made divers practitioners of physic make wrong estimates of medicines. But after I had waited a competent time, I perceived no author vouchsafed the *Sceptical Chymist* an answer; but a very ingenious man, from whom I chiefly expected it, told me, that he had indeed designed to write one, but was hindered by considering, that I had so stated the case, that an answer could not confute that book by any mere justification of the chymists principles, since he would be obliged also to defend the chymical doctrine, as it is generally taught by the vulgar chymists; and make good the arguments, by which they are wont to maintain it. Since it is only that doctrine and these arguments, that I declare myself in that discourse to question; and he himself did not think them sound and valid. By these encouragements I was induced to comply with the earnest solicitations of the printer for another edition; but he dying soon after, and the person, to whom the right to dispose of the English copy legally came, having left *England*, and continued out of it, for divers years, the dispute between the Stationers that pretended to it, and treated about it, lasted so long, that a traveller, who passed this way, told an acquaintance of mine, that he had then (which was two or three years ago) seen nine several Latin impressions of it; since when another has been brought me made at *Geneva*. This number of editions (in none of which I have added or altered a word) and the numerous citations I have met with of it, in favourable writers, made me unwilling to confound, or trouble readers, by interweaving additional notes with the body of the discourse; and so by obliging those that should hereafter vouchsafe to mention any of the inserted passages of it, to cite the edition as well as the book. And therefore I was easily inclined, by want of health and leisure, to peruse again deliberately the whole treatise, to suppress all those notes, that I could not readily and conveniently refer to three or four of the chief heads, I intended to enlarge upon; and without altering the form of the book, wherein it has proved so fortunate, to leave it entire, and publish my additions also by themselves by way of appendix. This in my intention was to consist of four heads, The *Producibleness of chymical Principles*; The *Uncertainty of the vulgar Analyses made by Distillation*; The *various Effects of the Fire according to the differing ways of employing it*, and *Doubts whether there be any Elements, or material Principles of mixt Bodies, one or more, in the sense vulgarly received.* But finding by the Stationer's estimate, that the notes referred

to

to the three last titles, are not near so large as those that belong to the first; yet they would make the book, to which they should be added, and which is already printed, of too great a thickness in proportion to its other dimensions; I thought fit to reserve the other papers for another opportunity, and at this time annex nothing, but what concerns *the Producibleness of the chymical Principles.*

BUT yet because there are some general advertisements that do somewhat more belong to this part of our designed Appendix, that now comes forth, than to any of the rest, I must not deny them a room in this preface, which I shall conclude with them.

I MIGHT justly enough allege, in excuse of incoherence of some of the particulars, that follow next after one another in the subsequent discourse, that this being confusedly but a collection (or if you please a rhapsody) of loose notes, it is more pardonable, than strange, that some of them should want apt connections, and the style of the discourse they compose should want uniformity. But it is not so much my present work to make apologies, as to give advertisements; and therefore I shall proceed to tell you in the first place, that though the following discourse have in some places a somewhat dogmatical dress, yet it is chiefly meant (as becomes an appendix to a sceptical book) to excite and assist a further inquiry; and accordingly the reader may perceive it to have been my care, not so much to play the part of a logical opponent, as to take occasion to set down variety of experiments and observations, that whatever hypothesis about the material principles of mixed bodies shall prove fit to be pitched upon, it may be founded on a less insufficient history of matters of fact (relating to that subject) than chymists have been wont to take in; and may be so framed, as not to be liable to those objections and difficulties, that will be here met with, and yet perhaps were not thought of, or at least were not duly taken into consideration, when the vulgar hypothesis of the *tria prima* was established. Upon this account I am not without hope, that the following experiments and considerations, though proposed by way of objections, may do some service to the inquirers into the material principles of things; by obliging the chymists, at least, to reform their doctrine about them, and build it more cautiously, and that upon a larger, as well as more solid foundation of natural history.

THE second thing, whereof I am to advertise the reader, is, that I would not have him infer from any thing, that (prompted by the exigencies of my design) I have said in the following papers, that I do either undervalue, or would decry chymistry, or chymists themselves indiscriminately. For I have a very differing esteem of the notional and of the practical part of chymistry. For divers of the opinions maintained by Spagyrists, without excepting their grand hypothesis of the three principles, I have been inclined to question, not only as a naturalist, but as a chymist; as seeing great cause to doubt, whether they be agreeable, either to the true grounds of philosophy, or the exploring experiments of the fire. But as for chymical operations, such as distillation, solution, sublimation, precipitation, and the rest; especially those seldom sufficiently valued ones, digestion and cohobation; I take them to be excellent tools in the hands of a natural philosopher, and to be by him applicable to many other, and perhaps some nobler uses, than they are wont to be put to, in laboratories; since if they be skilfully employed, they may be successfully so, as well to discover nature, as to correct, to imitate, and, in some cases, to out-do her. Nor do I only thus distinguish between the speculative and operative part of chymistry, but I make a great difference between the avowed cultivators of that art; and look not with the same eyes on the opinions and performances of vulgar chymists, and

chymical

chymical philosophers. For we are told, that there lives concealed in the world a sett of Spagyrists of a much higher order than those, that are wont to write courses of chymistry or other books of that nature; being able to transmute baser metals into perfect ones, and do some other things, that the generality of chymists confess to be extremely difficult, and divers of the more judicious, even among the Spagyrists themselves, have judged impossible. The declaration of what I think of these latent philosophers, belongs to another paper. Yet in this, I shall not deny, but that what I have heard from divers very credible eye-witnesses, and perhaps some more immediate arguments, strongly incline me to think, that there may have been, and may yet be, some such men: and whatever be to be thought of what they call the philosopher's stone, I confess myself convinced by what I have seen, that there are in the world as difficult arcana as divers of those, which have been (perhaps not all of them justly) derided under the name of chymical *non-entia*. Now if there be really such adept philosophers as we are told of; I am apt to think, that among their other arcana, they are masters of extremely potent menstruums (which may, as far as I can guess, be some of their chiefest tools) and may by the help of these and other means peculiar to themselves, of working upon bodies, be able to produce in them such alterations, as we have no examples of, and so obtain from them such similar substances, as either for number, or quality, or both, may be very different from the vulgar *tria prima*, or those substances chymists are wont to obtain (for that word I chuse to employ rather than the word separate or extract) by the common ways of what they call analysis. For if a man have an instrument, which other men have not, and much more, if it be an excellent one, he may be able with it to perform other things, than they can without. The Europeans by the help of so slight an engine as a mill, assisted by a far slighter instrument a sieve, can easily divide corn into bran, and meal, and flour, which even those Americans, for want of those helps, were not able to do, who could do other things, that are thought far more difficult. And he, that has a file and a good turning lath with its appurtenances, may obtain from a piece of iron both filings and shavings, and concave hemispheres, and ellipses, and globes, and cylinders, and other sorts of bodies, which could not be obtained from that iron, even by good artificers, that were not furnished (as till of late very few were) with those instruments. And he, that first found the use of aqua fortis in dissolving silver, and that though it were mixt with gold, had, by his menstruum, an easy way of separating those two metals, though ancienter mineralists, nor chymists themselves had no liquor that would perform that work. But *Helmont*'s writings will supply me with a far nobler instance to my present purpose, if the truth of all that he delivers concerning the effects of his alkahest be admitted; about the possibility of which strange solvent, having elsewhere written a short enquiry, I shall forbear to say any thing of it here, but rather intimate, that if there be such adept philosophers as some speak of, (which I think not incredible) and if they have (which, supposing there be such, I think not unlikely) among other rare things, some alkahestial or other extraordinarily potent menstruum, or way of penetrating and working upon mixt bodies; they may, for aught I know, be able to obtain such substances from them, as may induce me, and perhaps the chymists too, to entertain other thoughts about the constitution of compounded bodies (as they are wont to be called) than either I, or they now have. And therefore, though as to natural philosophy in general, I do not expect to see any principles proposed more comprehensive and intelligible than the corpuscularian or mechanical; yet as to the subordinate theory of mixt bodies in particular, I, that have disputed only against the vulgar hypothesis of the

the chymists, can easily retain a disposition to receive further light in this matter, when those, that are the best able to afford it us, and from whom it will be no disparagement for much greater proficients than I, to learn, shall think fit to oblige us by doing so. In the mean time, to end this advertisement as I begun it; I should not need to say much to satisfy chymists, that I neither hate nor despise their art, even in its present state, if some things, and chiefly want of leisure, would permit me to publish an essay that I wrote many years since, *Of the Usefulness of Chymistry to the Empire of Man.* Nor is it only to the practical part of natural philosophy, that I take chymistry, as it may be managed, to be highly useful, but I confess, I think also, that being ordered by a skilful naturalist, it may far more conduce, than those, that are strangers to it are wont to think, to the speculative part of physicks; and that as the Bolonian stone, without being chymically prepared, would never be made luminous, but being so prepared is brought to shine; so, many other natural bodies never afford much light to philosophy till chymical operations have qualified them to do so.

THE last advertisement I desire to give the reader, concerns the intention, with which I call in question the hypothesis of the *tria prima*, and some other of the chymists doctrines. For though sometimes I have had occasion to discourse like a Sceptick, yet I am far from being one of that sect; which I take to have been little less prejudicial to natural philosophy than to divinity itself. I do not with the true Scepticks propose doubts to persuade men, that all things are doubtful and will ever remain so (at least) to human understandings; but I propose doubts not only with design, but with hope, of being at length freed from them by the attainment of undoubted truth; which I seek, that I may find it; though if I miss of it in one opinion, I proceed to search after it in the opposite, or in any other where it seems more likely I should meet with it. And to declare my mind to the disciples of the fire, by a similitude not alien from their profession; suppose a man, more rich than skilful, should bequeath me a purse of guineas, and that I should have strong presumptions, that some of them are counterfeit, what in this case would a chymist have me do? To take them all for good, in spite of contrary presumptions against some of them, were very imprudent. On the other side, to throw them all away, because it is probable some may prove counterfeit, were downright folly. That then, which common prudence would direct me, would be to take them all out, and examine them one by one, first with the touch-stone, and then, if need be, by the cupel, and by aqua fortis too: and this I should do with desire to find all the pieces true, having also care not only to preserve and put back into the purse those that prove right; but if any be but partly adulterated, to preserve the good portion by purifying it (by the cupel or some other fit way) from the falcifying alloy, by whose admixture it had been imbased. The application of this I leave to be made by chymists. And having, in another paper, purposely discoursed of the cautions and limitations, without which I disallow scepticism, I shall only in general profess, that I more willingly embrace the truths taught by the chymists, than I endeavour to disprove their errors. For I look upon truth, as one of the chief of those goods, that God has of all others laid the most in common; since truth does not only, like desert islands in *America*, belong to him, that first finds it and seizes on it; but even, when another has lighted on it, and is in possession of it, any man may, without trespass or injury, make himself a sharer in it. To conclude; I am glad to find truth in the doctrines of the chymists; but when I cannot discern it there, I chuse rather to seek it elsewhere, than sit down without it. And if I any where seem to be

be somewhat too indulgent to suspicions against their hypothesis, or arguments, I hope the usual confidence, to be met with among most of them, considered, it will be looked upon but as a compliance with the advice of *Aristotle*, of bending a crooked stick the contrary way, to reduce it at length to straitness. And I did, with the less scruple, allow myself this way of writing, because experience having taught me, that some Spagyrists (for I speak not of all) that keep their best things close, will do more to vindicate their art, or oppose their antagonists, than to gratify the curious, or benefit mankind; I thought the rousing style, I sometimes wrote in, might prove no unhopeful way to procure somewhat considerable from those great masters, and orders of chymical arcana, that must be provoked, before they will come out with them; as the sea is observed not to give us one of its preciousest treasures, ambergrease, till it have been agitated by winds and storms.

The Publisher's Advertisement to the Reader.

I SHALL not entertain the reader with any thoughts of my own, about the following Appendix, which, without desiring to prepossess him, I shall willingly (and I think may safely) leave to speak for itself. But yet I think it may not be amiss, if I premise something to the reader, about the publication of these notes, as having been particularly concerned in it.

By the opportunity I had of seeing some papers of the honourable author of the ensuing Appendix; I perceived, that the notes, which he designed it should consist of, were indeed most of them laid together in some (though but a careless) order, and so were without much difficulty fitted for the press: but others of them lay scattered up and down amongst many others, about differing subjects in his philosophical memorials, which particulars not being ready at hand, when the ensuing Notes were sent to *Oxford* to the printer, they could not be published with the rest, but must expect some other opportunity to appear abroad, either alone, or in their company.

Perhaps the reader will not need to be told, that besides the application of some of the experiments contained in the following Notes, most of the experiments themselves are new. But so many years are past betwixt the first edition of the *Sceptical Chymist*, and the second, that now comes forth; that it may be requisite (though otherwise it would be improper) to advertise this reader, that he is not to think, that the author has borrowed from others those experiments and notions, that may be met with in books written in later years, as well as in the *Sceptical Chymist*. For the first English edition having been put forth in the year 1661, and never since by the author at all enlarged, or altered; it will sufficiently shew, that this book could not borrow from those, that never were seen till after, and perhaps long after his was published. Which advertisement may be particularly applied to the late learned treatise, intitled *Philosophia Vetus & Nova*, wherein in one long chapter may be met with an abridgement of a great part of the notions, experiments, and ratiocinations of the *Sceptical Chymist*, without any mention there made, either of the great and famous author's name, or his book, in which they first appear; though the Latin version of that treatise was published many years ago, and reprinted many times since. And though this

this be not the only writer, that hath thought fit to make use of considerable portions of the *Sceptical Chymist*, without owning it, I thought, what he has been pleased to do, required to have particular notice taken of it, because, though his modesty hath persuaded him to conceal his name, his learned book hath made him so justly famous, that if the reader were not advertised, he might easily suspect, that Mr. *Boyle* had not lent to, but borrowed of an author, who appears so capable of enriching the curious with excellent things of his own. And upon the same grounds I think it necessary to observe, that the experiments to be met with in Mr. *Boyle's History of Colours* having been published many years ago, could not be borrowed from that most ingenious treatise, though in that chapter of it, which treats *de coloribus*, between 20 and 30 experiments (if I mis-remember not the number) will be found the same with the like number of Mr. *Boyle*'s; whose name, though elsewhere very civilly taken notice of on some other occasion, is in the whole chapter left unmentioned.

I MIGHT here inform the reader, that the *Sceptical Chymist* having been many years out of print, it chanced, that when the notes, that make up the following Appendix, were drawn together for the press, this author had not a book at hand, by comparing whereof with the particulars of his designed Appendix, he might be sure to avoid (what he now but hopes he hath) the suffering any thing to pass in the latter, that is truly coincident with what was already extant in the former: (I mean, to the same purpose, and on the same occasion; for otherwise an experiment or notion may be more than once employed without mere repetition.)

AND lastly I dare not omit to let the reader know, that since the Appendix was printed, it appears, that by an oversight, some leaves were left behind, that treating of the difference of common mercuries themselves, should have been annexed, as a kind of appendix to the last of the three mercurial tracts, to be met with among the following papers: from whose perusal the reader shall no longer be detained by

His Humble Servant,

I. M.

The Introduction to the following NOTES.

THOUGH the pompous title of *Hypostatical Principles*, which chymists have bestowed upon the ingredients they would have mixt bodies to consist of, has perhaps served to procure them a veneration from vulgar heads, that are wont to esteem things the more because they understand them less; yet the main thing, that has recommended the chymical principles to more discerning men, seems to be, that by the help of a few simple ingredients (whereof nature is supposed to have laid up great magazines at the beginning of things) associated in differing proportions, all mixt bodies may be compounded; and so men may acquaint themselves with the natures of a multitude of bodies, by first knowing the natures but of a few. He therefore, that acknowledges he does not acquiesce in the chymical hypothesis of the *tria prima*, or their's, that add to them water and earth, can scarce imploy a more

proper argument to ſhake it, than, upon good ground, to call in queſtion what they teach when they affirm, that their principles are ingenerable and incorruptible; and that nature does only compound and diſſociate them, without either producing or deſtroying any of them. It will be therefore very well worth while to examine, what evidence there is in an aſſertion, which, in ſo many of the chymiſts reaſonings and explications, is either manifeſtly imployed, or not obſcurely ſuppoſed. And indeed this tenet of theirs is ſo principal a pillar of their hypotheſis, that, in caſe it fail them, the whole ſtructure will be in danger of ruin. For if the bodies they call principles be produced *de novo*, how will it be demonſtrable, that nature was obliged to take thoſe principles made ready to her hand, when ſhe was to compound a mixt body? and how will it appear in every analyſis made by fire, that the ſalt (for inſtance) thereby obtained, was not produced by the chymical operations, but was was pre-exiſtent in the body in minute parts, which, by the action of the fire, were only extricated and ſeparated from the other principles or ingredients, and afterwards brought together? Since, in caſe the chymical ſuppoſition be erroneous, not only the obtained ſalt may be in part due to a new production or tranſmutation, but part of that, which was really ſalt, if any ſuch thing there were antecedently to the analyſis, might be either deſtroyed by the operation, or made to appear under ſome other form.

OF THE

Produciblenes of Chymical Principles.

PART I.

Of the Produciblenes of Salt.

AMONG the ſubſtances, upon which chymiſts have conferred the title of principles, ſalt ſeems in their eſtimate to have had the precedency, ſince they are wont to name it firſt in the enumeration of their *tria prima*. And it is generally granted, that ſalts are wont to be the moſt conſiderable and active parts obtained by chymiſts from mixt bodies. And yet perhaps the inviſible particles, that compoſe the viſible portions of a ſalt, may be ſuch, and ſo contrived, as to be fit to make, and to have perhaps actually made, other portions of matter endowed with thoſe qualities, for which chymiſts are wont to call a body ſulphureous or mercurial, as may be inſtanced in the inflammability of nitre. Wherefore it may deſerve a greater meaſure of curioſity, than ſeems to have been employed, or even deſigned, by vulgar chymiſts, to enquire, whether ſalt, indefinitely ſpeaking, may be produced *de novo*, (as they phraſe it) or deſtroyed; and whether at leaſt the particular, and much differing ſpecies of ſalts may be changed into one another, and thereby, after a manner, be produced in reference to the acquired ſpecies of ſalt, and deſtroyed in relation to that, which the ſame portion of matter belonged to before.

To premise somewhat in general, to render it probable, that salts may be produced *de novo*, I shall briefly represent two things; the first is, that since salts differ much in several other attributes, some being mixt, some volatile, some acid, some urinous, *&c.* the two qualities, wherein they agree, and which therefore make up the common and most received notion of salt in general, are, that it is easily dissoluble in water, and that it affects the palate with a sapor, whether good or evil: and the other thing is, that, whether we allow the Epicurean hypothesis, or the Cartesian, the first saline concretions, that were produced by nature, must be confessed to have been made of atoms, or of particles, that before their conjunction, were not saline. And therefore there appears no absurdity in conceiving, that by the action of the fire or other fit agents, small portions of matter may be so broken into minute parts, and these fragments may be so shaped and connected, as, when they are duly associated, to compose a body capable of being dissolved in water, and of affecting the organs of taste.

THAT a disposition to be dissoluble in this or that liquor may be acquired by mixture, and the new texture of parts, is not without example; for, as I elsewhere observe, though powdered sulphur will lie in well rectified spirit of wine, some weeks or months, without being at all visibly dissolved in it; and though the same liquor will for as long a time swim upon salt of tartar, without making a solution of it; yet if this salt and sulphur be mixt together, spirit of wine will in less than an hour, and sometimes in less than a quarter of that time, dissolve enough of this matter to be richly coloured by it, and this without the help of external heat. And I see not, why it should be impossible, that the action of the fire may reduce the corpuscles of bodies to such a minuteness, and associate them either among themselves, or with the corpuscles of other bodies, which, without preparation, will not dissolve in water, that the pores intercepted between them may be entered, and their loose texture dissolved by that menstruum. Of which conjecture though we have not a perfect instance, yet we have a probable one in that, which I shall hereafter deliver concerning the making of fixt nitre. For though the crystals of well purified salt-petre may be kept many weeks or months in an ordinary lodging-chamber (for I had not occasion to try it in a cellar) without relenting by the moisture of the air; yet if without the addition of any body dissoluble in water, or moist air, it be in great part reduced, as perhaps it may be almost in a trice, to a fixt alkali, this salt will be easily enough penetrable by the vapours, that rove up and down in the air; and will by that moisture, in no long time, be brought to relent, and at length will be resolved into a liquor very analogous to that, which the chymists make of salt of tartar left in moist cellars to deliquate.

As for the sapor, which is the second qualification to be considered in the vulgar notion of a saline body, I doubt, whether the necessity of it be agreeable to another principle of theirs, and to experience. For it is plain, that chymical oils, even those pure ones they call *essential*, or even ethereal ones, are highly sapid: and yet these not dissolving in water, it seems there is no strict connection betwixt being saporous, and being soluble in that liquor; and that, if bodies be reduced into a multitude of parts minute and sharp enough, it is very possible, that some of these, either in part, or in conjunction with others, may acquire a size and shape, that fits them sensibly to affect the organ of taste, though perhaps the bodies themselves, or perhaps those bodies, that afforded them, are more of some other nature than of a saline. This may be illustrated by these gross examples; that a ball of glass, for instance, though, whilst it is entire, it will not prick and hurt the skin, yet if it be

broken and beaten, the little fragments will, not as they are glaſs, but as they have points or edges. And ſo, though a ſtick, being graſped in a man's hand, will not pierce the ſkin, or put him to pain; yet if it be cut into tooth-picks or reduced to ſplinters, their ſharpneſs and ſtiffneſs gives them a power to wound, that they had not before. Something analogous to what we, in theſe examples, ſee to be done upon the organs of touch, may be conceived to be done upon the organs of taſte (which is a kind of touch) of which an almoſt full inſtance may be given in purified ſalt-petre. For though this concrete have but a faint and languid taſte, yet if it be carefully diſtilled with ſome additament, that is not diſſoluble in water, and is inſipid, the parts of it being, by the action of the fire, either broken aſunder, or cleft, or rubbed, or ground againſt one another, till they are reduced to edged and pointed corpuſcles; ſalt-petre, I ſay, thus treated will be reſolved into differing ſubſtances, each of which has an extremely ſtrong and penetrant taſte, which, whence it ſhould proceed but from ſome ſuch mechanical change, as we have been deſcribing, is not eaſy to declare: and perhaps alſo the phlegmatic liquor, that is wont to come over in this analyſis, may, at leaſt as to part of it, be produced by the operation of the fire, and ſo the phlegm being inſipid, the taſte, I mean as much as was in the unanalyzed nitre, may be as well deſtroyed, as thoſe of the ſpirit and alkali are generated by the operation of the fire. But perhaps there needs no other argument, that the ſame parts of matter, according to its differing ſtates, may have the qualities, that chymiſts would have to be proper to this or that principle, than what we have took notice of in chymical oils, which do more ſtrongly affect the taſte, than the moſt of ſalts themſelves are found to do. And to confirm our doctrine of the producibleneſs of ſalts by the authority of *Helmont*, which is very great, at leaſt with the chymiſts of his own ſect, I ſhall obſerve, that he aſſures us, that by *Paracelſus*'s *ſal circulatum* ſolid bodies, among which he particularly, and in the firſt place, inſtances ſtones, may be tranſmuted into actual ſalt equiponderant to the body whereof it was made. So that upon the chymiſts ſuppoſition, that in theſe mixt bodies there is both ſulphur and mercury, beſides a *terra damnata*, the ſame portions of matter, that preexiſted in the form of either of thoſe ſimple ingredients, muſt, by the operation of the fire, and an anomalous menſtruum, have been turned into ſalt; and if the Helmontian experiment be allowed of, whatever becomes of the chymical ſuppoſition, we may ſafely conclude, that ſalt may be made of matter, that was not ſalt before, and conſequently that ſalt may be *de novo* produced. And thus much of the poſſible origination of ſalts in general, which I thought fit to premiſe to what I am going to offer about the production of the particular ſorts of ſalt. Though I have elſewhere enumerated and diſtinguiſhed ſeveral kinds of theſe bodies, whereto chymiſts have given the title of ſalts; yet thoſe, that more properly deſerve that name, and more directly appertain to our preſent diſquiſition, ſeem to be chiefly theſe three: the acid, ſuch as vinegar, ſpirit of ſalt, *&c.* the alcalizate or fixed lixiviate ſalts, made by burning, ſuch as ſalt of tartar, and of wormwood, barilla, pot-aſhes, *&c.* and the volatile and urinous ſalts, ſuch as ſalt of hartſhorn, of urine, of blood, of ſoot, *&c.* which taſte and ſmell like that of urine. Wherefore, if we can ſhew, that theſe may be produced *de novo*, or (which we have intimated to be equivalent for our purpoſe) tranſmuted into one another, we ſhall, I hope, be thought to have ſucceeded in our preſent attempt.

Vide Helmontium in vrollatu diff. Elements, No 11. & alibi.

SECT I.

Of the production of acid ſalts.

AND to begin with acid ſalts, we ſee, that even ſweet wines will but too often, without addition, degenerate into ſour vinegar, which will diſſolve coral and divers ſtones, calcined lead, and ſeveral other minerals. The rain-water, that is imbibed by the roots of trees, is in thoſe, that bear limons and barberries changed into liquors, abounding with ſaline corpuſcles, that enable them to affect the taſte, and act on powdered pearls, and ſeveral other bodies, as acids are wont to do. Alſo guaiacum and divers other woods, that do not at all taſte ſour, will, being diſtilled in retorts, afford ſpirits, that are furniſhed with ſtore of acid particles, which, as I have tried, will hiſs upon alcalies, and will diſſolve coral, and even lead itſelf calcined into minium, and make *ſaccharum Saturni* of it. Many other vegetable bodies alſo do, without addition, afford the like acetous liquors. And if it be objected, that theſe were pre-exiſtent in the bodies, whence they were obtained, and were only extricated by the operation of the fire; it will concern thoſe, that affirm this, to prove it (which no body, that I have met with, hath yet done): and I ſhall the rather require it, becauſe I find, that the ſweeteſt bodies, and thoſe of differing kinds, as (to omit raiſins of the ſun) ſugar and honey themſelves, afford ſuch a ſort of ſpirits, which the trials, I elſewhere mention, ſhew to be ſharp and piercing enough. To which may be added, that in divers caſes, where we are ſure, that acid ſpirits were plentiful ingredients of a compoſition, as in *ſaccharum Saturni*, and that magiſtery, which the chymiſts call ſalt of coral (which are not the only mixtures I have made trial of) experience witneſſeth, that the liquor, which comes over by diſtillation in retorts, is not acid, but quite of another kind. I would not, by what has been ſaid, be concluded to deny, that acid ſalts may, in ſome mixt bodies, be ſo aſſociated with others, and obſcured by them, as not to be diſcernible by the taſte, till they be ſeparated by the operation of the fire. But to ſhew, that ſuch acid ſalts were *de facto* pre-exiſtent, as acid ones in the body, that affords them, there muſt be ſome poſitive proof, other than the liquors diſtilled from them, ſince they, as we have already argued, may be not barely extricated, but may have their acidity produced by the operation of the fire. And we ſee, that ſalt-petre, though it have no acid taſte, may be made to afford, by (a certain way of) diſtillation, above three quarters of its weight of a highly acid liquor; and yet it appears not, that ſuch a great proportion of acid particles, or poſſibly any conſiderable proportion at all, was employed by nature in the compoſition of ſalt-petre. At leaſt, having diſtilled earth, that I cauſed in my own preſence to be dug out of a pigeon-houſe below the dung; I had from it a ſalt indeed, and ſome little ſaline liquor, but of a nature, as far as I obſerved, very differing from that of the acid ſpirit of nitre. (But this experiment I mention occaſionally, without building upon it.)

NOR do I think, it ought to ſeem incredible, that acid ſalts, as well as others, ſhould be producible by the various ſplittings, attritions, coalitions, and changes of texture, which may be cauſed ſeveral ways, and eſpecially by the operations of the fire; which moſt active agent, making a vehement and various agitation of all the minute parts, that a body conſiſts of, may, conſonantly to what hath above been intimated, ſplit or break ſome of them, and as it were grind others againſt one another; and in ſhort, ſo alter their bulk, figure and motions, as to make them fit to

ſtab or cut the tongue, and the other bodies, that they work on, after the manner of thoſe bodies we call acid. But of this you may find more in our notes about the mechanical origin of taſtes; wherefore I now proceed to the ſecond part of my taſk.

SECT. II.

Of the production of volatile ſalts.

AS to the production of volatile ſalts, we have an eminent inſtance of it in the ſalt obtainable by diſtillation from ſoot; for though the woods we burn in our chimneys ſeem not to have any thing of the taſte or ſmell of urinous ſalt, nor have the diſſolutions of the ſaline parts of ſuch woods communicated to water by their infuſion in it, been obſerved (that I know of) to be of affinity in taſte or odour with the ſalt of ſoot; yet when wood is firſt burnt in the fire, and then the ſoot afforded by it is duly diſtilled and rectified in fitly ſhaped veſſels, there is obtained a ſpirit and a white volatile ſalt, that in ſmell, taſte, and divers operations, by which we have examined them, appear to be of great affinity with thoſe of human blood, or urine, and may be eaſily enough miſtaken for them.

BUT this double operation of the fire is not always neceſſary to the production of volatile ſalts out of vegetables. For, though by their diſtillation in retorts we generally obtain from them no dry ſalt at all, but a ſourish ſpirit, with which I have diſſolved coral, lead, and other hard bodies, that urinous ſpirits have not been obſerved to work on; and they will, being put upon urinous ſalts, make ſuch an hiſſing and conflict, as are looked upon as great tokens of antipathy: yet I remember, that ſeveral years ago, I did from muſtard-ſeed, that had been kept for a convenient time, obtain by diſtillation a volatile ſalt, that faſtened itſelf in prettily figured grains to the upper part of the receiver, and this at the very firſt diſtillation; ſo that there was no need of rectifying the diſtilled matter, to ſeparate that ſalt. And to enforce this proof by ſomething more conſiderable than itſelf, I ſhall add, that by an eaſy way by word of mouth communicated to me by a very ingenious perſon (Dr. *D. E.*) one may, out of very many vegetables firſt duly prepared, without adding any thing to them, by bare diſtillations in retorts, obtain good ſtore of volatile ſpirits and ſalts, which by their fugacity, colour, ſmell, taſte, and divers experiments, that I purpoſely made to examine them, were ſo like the ſalt and ſpirit of urine, ſoot, *&c.* that one, that knew nothing of the way they were made by, would readily have concluded they belonged to one or other of the newly named ſorts of bodies.

I REMEMBER, that I have alſo ſometimes produced a volatile ſalt, that one would readily have pronounced urinous, of a mineral itſelf; nor was that the only foſſile, from which experience perſuaded me, that ſalt of this kind might be obtained.

SOME other particulars relating to the production of volatile ſalts, I think fit to reſerve, till I ſhall have occaſion to mention them in another ſection (as inſtances of the production of urinous ſpirits). Only there is one thing, that I think not fit here to pretermit, becauſe I have not met with it in any chymical writer, the contrary being rather generally taken for granted. I ſhall add then, that it is not univerſally true, that ſaline ſubſtances, that are volatile and aſcend in the form of ſalt, are of an urinous nature, and enemies to acids. For I have had from verdigreaſe, diſtilled *per ſe* with a ſtrong fire, a very acid ſpirit; which being warily rectified, afforded firſt a ſour phlegm, and then a penetrant ſpirit ſharper than it, leaving behind it in the veſſel ſome few ſpoonfuls of a dark-coloured liquor, which being ſet aſide, and ſuffered

to

to reſt, did in a great part ſhoot into tranſparent cryſtals, large but thin, almoſt like thoſe of ſilver, diſſolved in aqua fortis. They appeared prettily figured at the edges, but were ſo oddly connected among themſelves, that I was not able to refer them to any of the known geometrical figures; and their brittleneſs made them the leſs tractable, but their ſmell, which was ſtrangely piercing and not inoffenſive, argued them to be of the ſame nature with the acid ſpirit, which had come over with them.

BUT there is a more conſtant and eaſy way of producing ſuch a volatile ſalt, as my obſervation mentions. For if amber be gradually and warily diſtilled, it will afford, beſides the phlegm, ſpirit and oil, a dry ſubſtance, which, though the chymiſts call the volatile ſalt of amber, I found to be really of an acid nature, by ſeveral of thoſe trials, by which we are wont to diſcern, that a body belongs to the family of acids.

SECT. III.

Of the production of alkalies or lixiviate ſalts.

THE third and laſt ſort of ſalts, which we are to endeavour to ſhew to be producible, are the alkalies or fixt ſalts; which ſeem to have an antipathy with acid ones, by making a conflict with them, and exerciſing divers operations contrary to theirs (as I have in another diſcourſe more fully declared.)

As for the origin of theſe fixt ſalts of burnt bodies, the Spagyriſts are not of the ſame mind about it. For, the almoſt univerſal opinion of the chymiſts, that preceded *Helmont*, and the more common opinion even of later chymiſts, ſeem to have been, that theſe fixt alkalies are pre-exiſtent in mixt bodies; and that the fire does but ſeparate or extricate them from the other parts of the compounded body. But *Helmont* (followed in that by ſeveral chymiſts, that diſſent from him in other points) has ingeniouſly conjectured, that theſe lixivial ſalts do not pre-exiſt in their alcalizate form in the bodies, that afford them, but are productions of the fire, by whoſe violent action a part of the ſalt, which, in the concrete, is naturally all volatile, lays hold of ſome parts of the ſulphur of the ſame body, and both together are colliquated and fixt into an alkali; which fixation he ſomewhere exemplifies by that, which happens, when ſalt-petre and arſenic, that are both volatile, being expoſed to the fire, are by its operation fluxt and made to fix one another. But though this account be ingenious, yet I doubt, whether it be ſo clear and ſatisfactory, eſpecially ſince it is applied to all fixt alkalies, as the embracers of it think it. For beſides, that it may be queſtioned, whether it have yet been well proved (what *Helmont* teaches) that all the ſalt of mixt bodies before their combuſtion is volatile, it is not declared, what volatile ſalt is meant; though it be plain, that ſome bodies, that afford a fixt ſalt, do abound in acid ſpirits, as oak, box, and many other vegetables; and others, as hartſhorn, blood, urine, *&c.* abound with urinous ſalts, that exerciſe hoſtility with acids: and I have found, that from ſome bodies I could obtain both acid ſpirits, and ſuch as are wont to be called urinous. It is not eaſy to explain how the volatile ſalt comes to unite itſelf ſo intimately with the oil (or ſulphur) and though it be alſo volatile itſelf, to compoſe with it a body capable of enduring the violence of the fire, ſince we have more than once tried, that the volatile ſalt of urine, or of hartſhorn, and a chymical oil, as of turpentine or any ſuch, being put together, the ſalt will indeed aſſociate to itſelf ſome particles of the oil, but will nevertheleſs in their company ſublime in the form of a ſalt, with a very gentle fire. And the

Vide Helmontium in Blas humano, No. 38, & 43.

Vide Blas humanum, No. 41.

Vide Helmontium Complex. atque Mist. &c.

the example, that *Helmont* somewhere gives of arsenic and nitre, does not satisfy me, because that when I made equal parts of those two bodies to be mingled, and in a strong crucible fulminated together, a great part of the mixture was driven away by the fire, so little altered, that it is very dangerous to be too bold with the fumes, and a good part of what remained was fixt only, in comparison of the crude arsenic, but not comparably to salt of tartar, or some such other true alkali: and the constancy of the part, that was more fixt, may probably be ascribed to the salt-petre, which we know will, without the help of arsenic, afford a great deal of fixt salt, if about half of it be burnt away, by the help of powdered charcoal, or some other convenient additament. It may also serve to weaken this instance of *Helmont*'s, that there are other instances, in which we may observe, that no such thing happens, as his hypothesis may make one expect. For common sulphur is by chymists said to abound in an oily part, upon whose account it is very inflammable, insomuch that they would have other inflammable bodies to be so, by their participating of sulphur. That this concrete also abounds in salt, is evident, according to their principles, by the acid menstruum afforded by it, that goes under the abusive name of *oleum sulphuris per campanam.* And yet these ingredients, combined by nature, make up a concrete, which is so volatile, that both in close vessels and the open fire, it is almost totally volatile. And in that mixture of highly dephlegmed spirits of wine and urine, that *Helmont* calls the *Offa alba*, though the urinous salts do manifestly combine themselves with the spirit of wine, which being totally inflammable, the chymists refer to their oil, or sulphur; yet the coagulated mixture does not, by this association of ingredients, grow fixt, but proves very volatile. I will not here urge, in favour of the common opinion of the chymists of the pre-existence of alkalies in mixt bodies, that a corpuscularian may say two not inconsiderable things; whereof the first is, That there is no need of supposing a colliquation of salts with sulphurs, oils or any thing else to produce fixt salts; since, besides that that supposition does not explain, how two volatile bodies come to compose one that is fixt, it is plain, that a body yet more fixt may be made without any association of differing principles. For the earth, that together with the alkali remains in the ashes of a burnt body, is more fixt than the alkali itself, and yet derives not its fixity from any combination of elements, or principles, but from the grossness, solidity, or weight, and unfitness for avolation of the corpuscles it consists of. And the corpuscularian may add in the second place, that whereas some instances are alleged, wherein there is supposed a lessening of the quantity of the fixt alkali of the concrete, by operations, that are said to carry off the volatile salt, before the body comes to be incinerated; it may be answered, that perhaps those very operations did but rarify and volatilize part of the pre-existent alkali, and so left the less of it to be recovered by burning; as the chymists tell us, that fermentation rarifies the oily parts of the juice of grapes, and subtilizes them into vinous spirits, and so does much lessen the quantity of the oil. And when wood is burnt in a chimney, it is not in the form of an acid salt, which is the only, that is commonly observed to be driven away by distillation in close vessels, but in that of an urinous salt (which is a-kin to alkalies, and an enemy to acids) that the saline part of the wood is made to ascend; as may appear by the distillation of soot. Such arguments as these a corpuscular philosopher might, as I was saying, urge in favour of the more received spagyrical opinions. But instead of insisting on them, I shall only invite you to take notice of what I observe in salt-petre. For, though by distillation, or any other way, that we yet know, there is no oil to be separated from it; yet above half the body of it may be easily and quickly turned into

into a fixt ſalt, in colour, taſte, and operation, much like that of tartar, and other incinerated vegetables. And ſuch an alkali I have made without the help of injected coals, or any other body, furniſhed with a combuſtible ſulphur: ſo that it ſeems not, at leaſt univerſally, true that to the production of an alkali there is neceſſary to be at hand an oil, or ſulphur, to be laid hold on by the volatile ſalt, and fixt together with it. But this experiment is far more congruous to our doctrine, which derives all thoſe ſalts from the ſize, ſhape, and ſolidity or weight of the ſame corpuſcles; ſince the ſame ſalt-petre, whoſe greater portion may, by the operations newly mentioned, be reduced to a fixt alkali, may, by being diſtilled with a convenient bolus, have its greater portion brought over in the form of an acid ſpirit or ſalt, which itſelf may afterwards be made materially to concur to the production of an alkali. I might add, that even from one of * *Helmont*'s own experiments, my concluſion may be inferred, ſince he ſomewhere, and, if I miſtake not, in more places than one, affirms, that by the addition of more alkalizate ſalt and the operation of the fire, the earth itſelf, that is in the aſhes, may be turned into ſalt; which, if true, argues, that a fixt ſalt may be made of that, which was not before, either of ſaline, or of an oleous nature; and conſequently without any ſuch combination of ſalt and oil, or ſulphur, as his hypotheſis ſuppoſes. From which experiment I may alſo infer the poſſible origination of alkalies, by the mechanical changes, that, without the addition of oil or ſulphur, the operations of the fire may produce in the part of a mixt body; ſince earth (excepting water) ſeems the moſt indiſpoſed of any part of the concrete to be turned into fixt ſalt.

I MUST nor here pretermit an obſervation, that I have made, which ſeems to overthrow the opinion of thoſe learned chymiſts, who will have the violence of the fire to be always a neceſſary agent, as I allow it to be ordinarily, to the production of a fixt or lixivial alkali. I ſaid ſeems to overthrow, becauſe I had not the opportunity to repeat my trials, and am not ſure, that the ſalt I employed was altogether genuine, in regard I cannot here in *London* meet with it, at any rate; but I have great cauſe to think it was right, both for other reaſons, and becauſe it was ſent with other things, for a preſent out of the Eaſt, to an inquiſitive nobleman, who had been lately ambaſſador for his Britiſh Majeſty, at the Ottoman court, and who was pleaſed as a rarity to preſent it me.

THIS ſalt was affirmed to be the true Egyptian nitre, mentioned ſo much by antient writers: and indeed I found it to agree much better with the notion, that books had given me of it, than with that ſort, to which chymiſts generally give the name of nitre, and which is indeed the only nitre to be uſually met with in our European ſhops; where it is beſt known by the vulgarly received name of ſalt-petre. But to ſay ſomething of our Egyptian nitre, though it be not pertinent to mention here all that I obſerved about it, yet I muſt not here omit two things, that I made trial of, with that little, which eſcaped the miſfortune that loſt me all the reſt, that ſeem conſiderable in the preſent occaſion.

THE firſt whereof is, that this Nilotic ſalt was very apt to imbibe the moiſt air, as calcined tartar and other fixt alkalies are wont to be; to which reſolution we do not find our ſalt petre, if it be unmixed, diſpoſed. But the other and more important thing I obſerved, was this, that having upon this Egyptian nitre, crude as it was, poured ſpirit of ſalt, this acid liquor did preſently, even in the cold, work briſkly

* *Cinis proprio ſuo alkali fit ſal merum, complex. atque miſt.* No. 12. *Idem Helmontius in Blas humano,* No. 19 *videatur.*

upon it, as if it were a fixt alkali, or at least abounded with such a lixivial salt. And here, upon the by, give me leave to take notice of a text of the holy Scripture, that has sometimes puzzled not only me, but far better critics in the Hebrew tongue than I. And it is a passage to be found in the 25th chap. of *Solomon*'s Proverbs, where, to illustrate things very incongruous to one another, the disagreement of vinegar and nitre is mentioned; for supposing the words to be rightly translated, as besides the authority of I know not how many versions, the very sound of the Hebrew word *neter* or *nethar* argues it to be, it seems very hard to find, what show of antipathy there is between vinegar and the salt-petre, that is commonly sold in our shops for nitre. Wherefore strongly presuming, that *Solomon*, who reigned in *Judæa*, a country near to *Egypt*, and had much commerce with the Egyptians, whose King's daughter he had married, made use of Egyptian nitre as the best known, if not the only in his time and country; and might have found in this nitre some quality very differing
Jer. ii. 22. from any we find in our salt-petre; as I remember, that in the Prophet *Jeremy* nitre is mentioned as a very abstersive thing, and fit to cleanse women's skins, which is a known virtue of our fixed alkalies, but not observed in pure salt-petre: wherefore when once I received the nitre, that I have mentioned, and saw in it signs of an alkalizate nature, I quickly poured upon it some strong vinegar, and found, as I expected, that there presently ensued a manifest conflict, with noise, and store of bubbles; with which experiment I afterwards acquainted some critics, and other learned men, who were not ill pleased with it. But this theological use of the alkalizate nature of nitre not being that, which I chiefly mentioned it for, I shall now make the philosophical use I intended, by taking notice that Egyptian nitre being acknowledged to be a native salt, and made only by the evaporation of the superfluous water of the *Nile* (or some other such liquor) is yet of a lixiviate nature, or at least abounds with particles that are so: though, as I freshly intimated, it was produced without any precedent incineration, and the matter of it suffered not, or at least needed not suffer any violence of the fire to make it afford an alkali.

I HAVE represented these things, not for that I pretend to be sure, that alkalies may not be produced in multitudes of mixt bodies, especially in a good number of vegetables, after the way proposed by *Helmont*, or by some such like; but partly, because it seems not always necessary to the existence of an alkali in nature, and to be the only way it can be produced by; and partly, because I would give you and your chymical friends occasion to clear (as far as they can) and confirm the doctrine I have questioned. It is true, that being a far greater friend to truth, than an enemy to the chymical hypothesis, I would not stifle what may serve to advance that, in favour of *Helmont*'s doctrine, though this would never so well accommodate my present argument. But I have no great temptation to surmount in this case, for it concerns very little the main scope of this discourse, whether *Helmont*'s way, or any other, of the production of alkalies be embraced; since it will suffice for my purpose, if some bodies belonging to this family, or kind of salts, may be produced: I say some, because (as I have already intimated) I will not peremptorily assert, that all fixed alkalies are productions of the fire, or made by the help of it; though I do not readily remember, that I have met with any (except Egyptian nitre) that are not. But I shall wave that question, because my intended brevity calls upon me to proceed to the mention of some particular instances, fit to persuade us, of the producibleness of some alkalizate salts.

IT is known, that chymists generally look upon spirit of nitre and aqua fortis, as liquors containing not alkalies, but acid salts; which they conclude not only from the

taste, but from the great ebullition, that is made, when those liquors are poured on the salt of tartar, fixed nitre, pot-ashes, or other such unquestioned alkalies. That sea-salt likewise does not contain alkalies, is generally taken notice of, the spirit of it being justly reckoned among the acid ones; and when I purposely examined that concrete by distillation, the remaining salt, though the fire had been violent, was very differing from alkalies: and yet my conjectures inclining me to suspect what the event would prove, I several times made the following experiment upon sea-salt, that yet retained all its acid spirit in it.

UPON well dried and powdered sea-salt we put into a retort sometimes an equal, and sometimes (which I prefer) a double weight of good spirit of nitre, or aqua fortis, and leisurely distilling all that would come over, we took out the dry salt remaining at the bottom, which we found much changed both as to colour (a good part of it being usually very reddish) and as to taste, which was differing enough from what it had been before, and might probably have been made much more so, if fresh spirit of nitre had been once or twice more abstracted from it. This salt being again powdered (for it was in a lump when taken out) and put into a crucible, placed in a convenient fire, was by the repeated injection of fragments and well kindled charcoal made to flash divers times almost like melted nitre; and when it would flash no longer, the remaining matter being taken out, did, in great part, appear to be brought to an alcalizate nature, for it had a fiery taste upon the tongue: if spirit of nitre or aqua fortis were poured on it, it would make an ebullition, it would turn syrup of violets green, and in short, exhibit divers phænomena of alkalizate salts.

ANOTHER way there is like that mentioned of making an alkali out of nitre, which is thus done; pour upon it an equal weight, or half the weight of strong oil of vitriol; and having diluted the mixture with a convenient proportion of fair water, distil it by degrees, till there remain a substance very dry. Powder this, and mix it well with about an eighth part of beaten charcoal; keep them in fusion in a strong and covered crucible, till the mass grow very black, and a little of it being taken out with a wire, taste fiery upon the tongue (which may happen in about half an hour or an hour, according to the quantity of matter, and degree of fire:) then take out the blackest or deeply red mixture, which will very easily imbibe the moisture of the air, and you may find it, at least while it is hot and dry, of a more fiery lixivial taste than salt of tartar itself. It will make an ebullition with acid spirits, and precipitate divers solutions made with them; it will turn syrup of violets green, and in short discover itself many ways to be of an alkalizate nature, though it be associated with a sulphur, that may, by divers methods, be made appear to be contained plentifully in it.

IT is also considerable on this occasion, how the same body, without the addition of any other salt, may by the various manner of the fire's application to it be made to afford, either little else than acid salts, or a lesser or greater quantity of alkali: for if fine salt-petre be dexterously distilled with about thrice its weight of some fit earth (but not, as is usual, with powdered bricks) it will sometimes afford very near as much spirit of nitre as the salt weighed; and though this, like other liquors, be not without phlegmatic parts, yet besides that it may be doubted, whether most or many of them were not produced by the transmuting operation of the fire, we may suppose, that five parts of six, or six of seven have been distilled into dephlegmed spirit.

BUT if, according to the way I have elſewhere * circumſtantially delivered (which is by frequent injecting into fluxed ſalt-petre ſmall pieces of kindled charcoal, till one can make it flaſh no more at all) you make fixed nitre, you may obtain from nitre thus handled half its weight, and perhaps better, of an alkalizate ſalt, that many would, by its taſte and operations, gueſs to be ſalt of tartar.

BUT to ſhew yet further, how much the production of this alkali depends upon the operation of the fire, which, as it is variouſly applied, may vary the texture of the ſalt-petre, my conjectures led me to try the following experiment, which I did with ſucceſs from the beginning. We took a pound of good ſalt-petre, which was but groſsly beaten (for it ſhould not be finely powdered) and having laid it on a conical heap upon a flat tile, that the air might on all ſides have acceſs to it, we cauſed the upper part of it to be kindled by a little fragment of burning coal (which may be afterward thrown away.) Then we cauſed the laborant with an iron rod dexterouſly to ſtir the kindled part of the nitre, that the ignition might be preſently communicated to as many parts of the ſalt, as was poſſible; and this nimble ſtirring of the maſs, that the fire might be more diffuſed, and more parts might be obverted to the air, we cauſed him to continue to the end of the operation: by which method, within few minutes, we obtained, more than once, out of ſixteen ounces of ſalt-petre, about ten ounces or better of fixed nitre, very lixivial in taſte and operation; and for the colour, it was of a pleaſant greeniſh blue, and deeper than ſalt of tartar will uſually be brought to, by being (in a crucible) kept twenty times as long, in a good fire.

THE other ſcopes and uſes deſiged in this new and quick way of making the alkali of nitre belong to another diſcourſe, the experiment, which will ſcarce ſucceed without a dextrous management, being here mentioned to ſhew, what quantity of alkalizate ſalt may by a differing operation of the fire be obtained from nitre; which (crude nitre) in diſtillation ſkilfully made for the purpoſe may be in great part driven over, in the form of acid ſpirit, and phlegm, and leaves ſo little true alkali behind it, that I have wondered at it, being ſometimes ſcarce able to find any at all, though I purpoſely tried to ſeparate it from the tobacco-pipe clay, which the petre had been mixed with after a diſtillation, wherein not half of the ſalt had been driven into the receiver, in the form of ſpirit.

AND to ſhew, that to make the fixed ſalt of nitre, the actual inflammation of it, in the open air, is not neceſſary, as very learned men have ſuppoſed; and that it is poſſible, whatever is preſumed to the contrary, to make an alkali nitre, though charcoal, or ſome other combuſtible body be not added to it, to kindle the corpuſcles of the nitre by its ſulphur, and by the aſſociation of ſome part of the ſame ſulphur with the ſaline parts of the nitre, to compoſe an alkali; to ſhew this, I ſay, I more than once made the following experiment: with a convenient quantity of good ſalt-petre we carefully mingled about an eighth part of tobacco-pipe clay; and putting the mixture into a crucible cloſely luted at the top, we kept it by a fitly graduated fire in fuſion for ſome hours, and found, as we expected, that the remaining ſalt (for part would get through the lute, or commiſſures in the form of fumes) was turned into an alkali, of a fair blue colour, like the better ſort of that fixed nitre, which is made with charcoal. This experiment, and that formerly made with tobacco-pipe clay, ſeem plainly to argue, that to the making of fixed nitre, which is confeſſed to be an alkali, a congruous change of texture may ſuffice; whether that change be

* In the *Eſſay of the differing Parts and Redintegration of Nitre.*

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attempted to be made in open veſſels, or in cloſe ones, with, or without the addition of the ſulphur of charcoal, or any other ſuch combuſtible body. Upon the ſame ground, that I had for trying the former experiment, I attempted, and not without ſucceſs, to make an alkali of ſalt-petre, by colliquating moderate quantities of it, ſeveral ways (and keeping it in fuſion) with ſome metals: I ſay, moderate quantities of nitre, becauſe an ingenious gentleman, to whom I communicated this practice, could not make it ſucceed in any conſiderable quantities. And to obviate the ſuſpicion, though perhaps groundleſs, that ſome chymiſts might have of the material concurrence of a good portion of the combuſtible ſulphur, preſumed to be in the ignobler metals to the production of the nitrous alkali; I ſhall add, that our experiment ſucceeded, when we tried it more than once, with more than ordinarily fine ſilver, whoſe ſulphur, if it have any, is granted to be fixt or incombuſtible. And I remember the laſt trials afforded us a bluiſh alkali, though there were employed but a fifth part of ſilver, in reference to the nitre; and though the fire (which was continued for ſome hours) was ſo moderate, that the metal, though thinly laminated, was not melted; and of an ounce, that was put in, there wanted but four grains, which ſmall loſs might eaſily be imputed to divers accidents.

AFTER what is ſaid of the production of lixiviate ſalts and alkalies, it will not be impertinent to add, that as they may, by the operation of the fire, be produced; ſo by the operation of the fire they may be deſtroyed or deſpoiled of their alkalizate form, and turned into a ſubſtance of a nobler nature. This I am induced to think very probable by ſome experiments, amongſt which, that which ſeemed the moſt conſiderable, was this: we took a pretty quantity of good ſalt of tartar, that had been purified by ſolution and coagulation, and having put it into a clean crucible, we kept it in a ſtrong fire (that made the crucible red-hot) for a good while; then giving it at length a ſtronger fire, we poured it or (afterwards) took it out of the pot, and diſſolved again as much of it as we could in cold water, which being ſet to run through cap-paper, there appeared, as I foreſaw, in the filtre a pretty deal of matter, that would not (as the whole ſalt had done at firſt) be diſſolved in the water, but was turned into a kind of earthy ſubſtance. Then coagulating again the ſolution, that had paſſed through the filtre, into dry ſalt; we expoſed it again in the crucible to a ſtrong fire, and putting it again into water, we perceived it would not totally diſſolve, but left in the filtre a ſlime or mud. And in this manner we proceed to ignite, diſſolve, filtre, and coagulate the ſame ſalt of tartar many times; for, if I much miſtake not, it was ſixteen times, and ſtill found ſuch an earthy ſubſtance as has been ſpoken of, remaining in the filtre; and the reſt of the ſalt of tartar ſo little altered, that being ſomewhat tired, and other ways diverted, I deſiſted from proſecuting the operation to the uttermoſt; concluding it highly probable, that the remaining ſalt might, by the ſame way of management, be brought to yield more and more of that ſame ſubſtance, which either was earth, or of kin to it, being at leaſt ſomewhat, that was of a nature very differing from ſalt of tartar, ſince it was not like it, fiery on the tongue, and was indiſſoluble in water, as earth, but not ſalt of tartar, is known to be.

SECT. IV.

IT may bring ſome illuſtration, and add ſome probability to what has been diſcourſed about the produciblenes of ſalts, if we conſider what happens in the compoſitions and decompoſitions of ſaline particles, and their operations on other bodies, and on one another. For if it appear, that, by theſe manifeſt and mechanical ways, ſuch

fuch alterations may be made, and fuch qualities produced, as are either altogether, or very near as confiderable, as thofe, which difcriminate the feveral families of falts formerly fpoken of, from one another, and from this or that chymical principle; it will, I prefume, be judged the more credible, that thefe families of falts may be either tranfmuted into one another, or otherwife produced; and fo may not be primordial and immutable beings, in the fenfe, wherein the chymifts would have them to be fuch. I have elfewhere taken notice of the production of vitriols, fal armoniac, borax *, and divers other factitious falts; for which reafon; I fhall not infift on them here the rather, becaufe it may fuffice for my prefent purpofe to take notice of two falts, whereof the one is meerly factitious, and the other fuch in great part; and yet each of thefe, by a very flight and eafy way of ordering it, afforded me differing faline concretions, fome of which refembled a falt, which many judge the moft fimple and natural, that we yet know of.

To fhew then, that common falt itfelf, that feems the moft primitive and fimple amongft grofs and vifible falts, may be produced by a change of texture made in bodies very differing from common falt, I fhall recite an experiment, which though it have fometimes failed me, yet it has divers times anfwered my defire, though I fhall now relate but that fingle one of my laft trials, that fucceed beft.

THAT, which our Englifh glaffmen call *fandever*, and the French, of whom probably the name was borrowed, *fuindever*, and is, you know, that recrement, that is made, when the materials of glafs, namely fand and a fixt lixiviate alkali, having been firft baked together, aud then kept long in fufion, the mixture cafts up the fuperfluous falt, which the workmen afterwards take off with ladles, and lay by as little worth; this falt feldom ufed by mineralifts, and fcarce wont to be mentioned by the writers of courfes of chymiftry, I have thought fit to employ about feveral purpofes, invited thereto by confidering the ufual way, wherein it is produced. For in fandever we have a falt, which was once altogether lixiviate, but which having been kept long melted in a ftrong fire with fand (or flints or pebbles) muft have had its faline corpufcles variouffly and forcibly ground or rubbed againft one another, and againft the particles of the fand, fome of which it may alfo have diffolved and retained with it; by which rude juftlings and mutual attritions, I thought it very probable, that the alkali muft not only have been confiderably altered, but variouffly too, fome parts being changed more and after a differing manner than others, by which means fandever may confift of portions differingly qualified both in reference to the lixiviate falt, that was at firft employed, and to one another.

WE took a pretty quantity of good fandever, and having diffolved it in fair water, and filtred it, we fet it to evaporate in a digeftive furnace, till a faline cruft, as if it were a thin plate of ice, fpread itfelf upon the top of the liquor; then fuffering it to cool and cryftallize, we broke the mentioned cover, to come at the cryftals, and fet the liquor we had poured off from them to evaporate further and fhoot again: and in this method we proceeded, whilft we judged worth while to do fo. By this means we obtained good ftore of cryftals, whofe figures were not the fame, but many of them differing enough, though moft of them tranfparent and prettily fhaped, as if nature had at once affected variety in their figuration, and yet confined herfelf to geometrize. But the chief thing, for which I mention this experiment, is, by this way of proceeding, I more than once obtained (not on the very furface of the water, as is ufual in the concretion of fea-falt) but in other parts, and chiefly beneath the

* In the *Effay of the Ufefulnefs of Chymiftry to the Empire of Man.*

fur-

ſurface of the ſaline plate formerly mentioned, a conſiderable number of grains of ſalt, that better anſwered to the deſcription of common ſalt, than diſſolved and filtered ſea-ſalt itſelf is wont to do: for theſe grains, that were of no deſpicable bigneſs, were as like little cubes or dice, as if they had been made by a ſkilful jeweller; and their ſurfaces had a ſmoothneſs and gloſſineſs much ſurpaſſing whatever I had obſerved in marine or common ſalt.

I MAY confirm the difference, I have mentioned to be between ſandever and common alkalies, if I here add, that ſome while ago having ſet a good quantity of filtered ſolution of ſandever to coagulate in a cool place, and thereby brought a great part of the ſalt to coagulate into cryſtals, almoſt like thoſe of nitre, but ſo very diaphanous, that divers of them were clear as rock cryſtal itſelf; I did not obſerve them to relent by the moiſture of the air in a long time, though the glaſs they were kept in were negligently enough covered with paper only; which argues their texture to have been remote enough from that, which is proper to alkalies: and to ſhew, that they were alſo ſalts of a peculiar nature, I ſhall further obſerve, that if they were expoſed, though but to a gentle heat, they would in no long time loſe their tranſparency, and be reduced to a white and fine calx; which being weighed and re-diſſolved in water, and made again to cryſtallize, would be diaphanous, and concoagulate with itſelf ſo much of the water, as ſufficed to give a very notable increaſe of the weight.

SANDEVER, which afforded me the firſt of the two inſtances I promiſed you of the production of ſalts, is one of thoſe bodies, that many would reckon amongſt thoſe, that are almoſt meerly artificial. I ſhall now mention a ſecond inſtance of a body, wherein art ſeems to have little to do, ſave the eaſy extrication of its particles from thoſe, wherewith nature had blended it in a human body. If then man's urine, after having been kept ſome weeks in a cloſed veſſel, be expoſed to a moderate fire, firſt it will yield a ſpirit and a volatile ſalt, and then a very copious phlegm, which being totally exhaled, there will remain a dry *caput mortuum*; and this being warily calcined, diſſolved in water, and coagulated, if the experiment ſucceed with you, as it did with me, you will find the ſalt very different from a common lixiviate alkali, rather you will find ſaline concretions of differing forms, if not kinds: for I obſerved ſome to be oblong, and to look like ſome cryſtals of nitre; others, to be of figures reſembling thoſe, that geometricians call *Rhomboides* or *Rhombeides*; and one of the faireſt of theſe lozenges, I remember, for trial-ſake, I kept for many days expoſed to the air, and that in winter, without finding it run *per deliquium*, as a piece of common alkali of that bigneſs would have done in a little part of that time. But beſides thoſe numerous ſaline concretions, that I could not eaſily reduce to any known figure, there was (which I chiefly expected, and would have you take notice of) a conſiderable number of fine grains, that looked like common ſalt, and were indeed more exactly cubical in their figure, than the grains of ſea-ſalt themſelves are wont to be. And I have the leſs cauſe to doubt, that the ſea ſalt abounding in our *caput mortuum* was not a common lixiviate alkali, but conſiſted of parts of other natures, eſpecially of ſuch as compoſed ſea-ſalt; becauſe I obſerved, that the *caput mortuum*, when expoſed to calcination, began early to melt in the fire, before it was near calcined, as not an alkali, but ſea-ſalt, would have done; alſo becauſe the taſte was much nearer to that of brine, than to that of lixivium; and becauſe, laſtly, it would make no conflict with the ſpirit of ſalt, as an alkali would have done, but did make of a ſolution of ſilver in aqua fortis a white precipitate, like that we make of that metal with ſea-ſalt, but not with alkalies.

AND

AND because a mischance unseasonably deprived me of the *caput mortuum* of domestic urine, prepared in my own laboratory, I was fain to procure a supply of fixt salt of urine made by a diligent Spagyrist of my acquaintance, who had wrought much upon that liquor, and having dissolved and filtered a pretty quantity of this salt, and suffered the solution to evaporate slowly, till it began to have a skin, I found the crystals it afforded in a cool place to be some of them an inch or two long, and shaped almost like crystals of nitre, save that they were sharper at both ends, and to many of them were fastened store of minute and oblong crystals prettily shaped, which were placed almost perpendicularly upon the greater proportions of salt, which by this means obtained a shape, not much unlike that, which the *French* engineers call *cheval de frise:* these crystals, as they did not resemble common alkalies in their figures, so they were unlike to them in divers other respects. For though some oil of tartar *per deliquium* being poured upon some of them, there ensued no manifest commotion, as is wont to be produced, when that liquor is mixt with a salt, where an acid is predominant, yet being beaten and mixt with an acid spirit, as that of common salt, they made not the least ebullition or conflict, though they were stirred up and down to excite it; nor did aqua fortis, good enough to be worthy of that name, produce any hissing noise or froth, when it was put upon the salt of urine, though at length it dissolved a good proportion of it. And though strong oil of vitriol being put upon some of the forementioned crystals, did readily work upon them, and in corroding them excite a good number of bubbles, yet that did not surprise me, nor make me conclude the salt to be alkalizate, because I have observed oil of vitriol (though not spirit of salt or aqua fortis) to work after the like manner upon common salt, of which that the fixt salt of urine did participate, I judge very probable, partly upon the account of the phænomenon newly recited, partly because I found, that by impregnating good aqua fortis with a competent quantity of the fixt salt, instead of dissolving in it sea-salt, I could make it capable of corroding foliated gold, even in the cold; and partly also, because that some part of the solution of our fixt salt, that was more slowly coagulable, being mixt with oil of tartar, presently grew thick and muddy, and soon after let fall a precipitate copious enough: and another part of the same solution did readily precipitate silver dissolved in aqua fortis, but would not so much as discolour a strong solution of sublimate (made in fair water) from which a common lixiviate alkali would have immediately struck down an orange-coloured powder.

A LIGHT suspicion I once had, that the common salt, that most men eat to season their aliments, may in some degree impregnate men's urine, gave me the curiosity to examine that of horses, which I found to require rather a shorter than a longer putrefaction than human urine, to make it fit for distillation; but the *caput mortuum* of this also, I was by an accident hindered to examine sufficiently: but by the spirit and volatile salt the liquor after putrefaction easily afforded, it seemed probable enough, that the fixt salt would have been not unlike that of men's urine. Of which last named olid and despicable liquor I chose to make an instance in this place, because chymists are not wont to care for extracting the fixt salt of it (which is therefore commonly presumed to be like other alkalies) but as soon as they have distilled the saline spirits, throw away all the rest as nothing worth. Which practice, as general as it is, I cannot commend; for though I am not altogether of *Helmont*'s mind, where he says, that *Wisdom despises those, that despise the indagation of urine, and refuse by the fire to search out its contents*; yet I think, that those, who understand the mystical writings of some of the best chymical philosophers of former times, will look upon it as a more tolerable hyperbole, than other men or even vulgar chymists imagine it to be.

OF THE

PRODUCIBLENESS OF SPIRITS.

PART II.

SECT. I.

Of the production of vinous spirits.

AS for what the chymists call spirit, they apply the name to so many differing things, that this various and ambiguous use of the word seems to me no mean proof, that they have no clear and settled notion of the thing. Most of them are indeed wont, in the general, to give the name of spirit to any distilled volatile liquor, that is not insipid, as is phlegm, or inflammable, as oil. But under this general term they comprehend liquors, that are not only of a differing, but must be, according to their principles, of a quite contrary nature; some of them being acid, as spirit of nitre, of salt, and of vinegar itself; and some of them urinous, or, as some would have it, volatile alkalies, which are such enemies to the former, that, as soon as they are put together in due proportions, they tumultuate and grow hot, and usually continue to fight, till they have disarmed or mortified each other. Besides these two hostile families of spirits, there is a third sort, which they call vinous or inflammable, which though very subtile and penetrant, is not manifestly either acid, or alkalizate; I say, manifestly, because the taste and smell of this sort of spirit is differing from both the sorts last named; and yet is referred to one, or the other of them, by some learned Spagyrists; with whom I neither need, nor desire to dispute about this matter; since it may suffice for my purpose, if it can be made out, that all the three sorts of spirits above mentioned, the vinous or inflammable, the urinous or alkalizate, and the acid, may be produced, and consequently may be other than primeval bodies.

AND to begin with the first named, these vinous spirits are so producible by art, that we seldom find them produced by nature alone, which does indeed make the juice of grapes, but does not make wine, nor the spirit of wine, unless, by the help of man, that juice be pressed out, and fermented. And the case is yet more plain in the ardent spirits made of ale, beer, and in the like vinous spirits made by the decocting and fermenting the seeds and other parts of vegetables. And it is observable to our purpose, that must (or the juice of grapes newly pressed out) does not in distillation yield a vinous and inflammable spirit. And I remember, I had once the pleasure to laugh at a man, otherwise very ingenious, who, to catch the subtile vinous spirit, that he would have me think was lost in the common way of handling wort, made it work in a huge copper limbec, to catch the spirituous parts, that he thought would otherwise fly away; by which means, instead of the ardent spirit he expected, he got nothing in his receiver but a nauseous phlegm. I have also found by trial, that raisins (which we know are but dried grapes) being distilled alone, afforded an acid and empyreumatical, but not a vinous spirit; whereas, when I carefully

fully fermented them with a due proportion of water, they would afford me in distillation an ardent spirit like that of wine.

If it be objected, as I presume it will, that the vinous and inflammable spirit, that is by fermentation obtained from bodies, was actually in them before, and is only extricated by fermentation; I answer, that this is *gratis dictum*, and is therefore not to be admitted till it be proved; since raisins, and such other fermentable bodies do not, upon the supposed analysis made by distillation, afford a vinous spirit, but one that is very differing from it. And I see not, why the change of texture may not turn some part of the juice of grapes into a vinous and inflammable liquor, since a little further change is able to turn the same juice into a liquor, that is acid, and neither vinous nor inflammable, as it was before. And I have found by trial, that even this vinegar, crude as it is, being satiated with calx of lead made *per se*, would afford a spirit not acid, but of a very differing taste, and inflammable, like the spirituous parts of wine. And if it should be further objected, that these inflammable spirits were not produced by these operations, but pre-existing in the newly expressed juice of grapes, were only extricated by fermentation, and being afterwards covered or disguised by the acid particles of the vinegar, were again extricated by distillation, the acid salts having fixt themselves upon the lead they corroded, and thereby given the spirits leave to forsake them: if I say, this be objected, I might refer you to a more full answer, that I have elsewhere given. And at present it may serve the turn, that I put you in mind again, that the objection alleges no phænomena to make appear the actual pre-existence of vinous spirits, either in the juice of grapes, or in the solution of lead.

And though I need but deny what is barely affirmed, not proved; yet to examine this matter further than I had found others to have done, I did (as I elsewhere mention) make a *saccharum Saturni* with an acid liquor made without fermentation, or the addition of any liquor from wood itself; and then, distilling it also without any additament, I had (as I expected) a spirit, that readily took fire and burnt away in a blue flame, like that of spirit of wine. I know another objection may be framed from the doctrine of some chymists, who would have spirit of wine to consist of the oily parts of the juice of grapes rarefied and subtilized. But with these learned men (for such I know some of them to be) I have not here any need to enter into a dispute; since, without examining, whether their opinion be true or no, if it be admitted, it will be consistent enough with mine. For to say, that by subtiliation, rarefaction, a peculiar kind of commixture with the phlegm, or the like means, the oil contained in the juice of grapes (and separable from it, in the form of oil, if it be distilled before it be fermented) becomes spirit of wine, is but to assign the modus, whereby vinous spirits are produced, but not to deny their production. And all that my purpose requires, is, that it be proved or granted, that inflammable spirits are really produced, by what way soever they come to be so. I shall add, that though experience witnesses, that honey being skilfully fermented with a due proportion of water, will yield a greater plenty of inflammable spirit than the wine itself, that is made in divers countries; yet when we have carefully distilled honey before fermentation, it afforded us a great proportion of considerably acid spirit, that would dissolve some metals, but so little oil, that the paucity seemed strange, and made it appear unlikely enough, that so inconsiderable a proportion of that liquor should be rarefiable into so much ardent spirit, as may be obtained from well fermented honey.

SECT. II.

Of the production of urinous spirits.

I PROCEED now to the other sort of spirits, as those of hartshorn, blood, sal-armoniac, soot of wood, *&c.* that we have formerly called urinous, because of their great affinity in many qualities to the more familiarly known liquor, spirit of urine. But as for these, I know not, whether it will be necessary to treat of their origin apart; since, for aught experience has yet assured me, these spirits are not simple, but compounded bodies, consisting of the volatile salt of the concrete, that afforded them, dissolved in the phlegm, and for the most part accompanied with some little oil, at first undiscerned by the eye, though afterwards it grow visible. The presence of this oil in most spirits, belonging to this family, may be probably argued from the deep tincture, that in process of time, spirit of hartshorn, of blood, and divers other subjects, will acquire by standing, though presently after their distillation, and first or second rectification, they were clear, and colourless as water; the oily portion, which, whilst it was in very minute particles, lay concealed in the mixture, becoming discoverable in process of time by their extricating themselves a little, and associating, though not so far as to emerge and float, yet far enough to disclose themselves by the colour they give the liquor. But in spirit drawn by the help of an alkali from sal armoniac, a concrete not abounding in oily parts, like hartshorn, blood, *&c.* kept not only for many months, but divers years, I observed no such discolouration; which was one inducement to make me, in speaking of the oleaginousness of urinous spirits, to employ the word *most* rather than the word *all*.

HAVING therefore hitherto by rectifications and digestions observed nothing in these urinous spirits but a crystalline volatile salt, most commonly separable in a dry form, and the phlegm it was dissolved in, besides some oleaginous particles, that had (though at first unperceivedly) associated themselves to it; I see no great need to trouble you with particular instances about this sort of spirituous liquors; what has been said making it allowable for me to refer you to what I deliver about the production of salts, where that of volatile ones is mentioned.

SECT. III.

Of the production of acid spirits.

AS for acid spirits, that some of them may be generated or produced *de novo*, seems probably deducible, partly from what has been already delivered (in the first part of these notes) concerning the production of acid salts; and partly from what will be ere long recited of acid, as well as of urinous and of vinous spirits, obtained by distillation from one and the same body. And if we take the word acid, as I usually do in these notes, in a familiar sense, without cryptically distinguishing it, from those sapors, that are a-kin to it, perhaps the spirit of sea-salt and that of nitre may be fitly enough proposed, as instances of the production of acid spirits. For though sea-salt and its distilled liquor have upon some bodies the like operations, as either of them will precipitate silver out of aqua fortis, yet not only the taste of the spirit of salt (especially that which rises last in distillation) is exceeding different from that of crude salt, not only in strength and penetrancy, but in this, that the

ſpirit is highly acid. Whereas the crude ſalt has a taſte not properly acid, but that, which by a diſtinct name is in Latin commonly called *ſalſus*, ſuch as that, which predominates in brine; and it does not appear, that this acid ſpirit did as ſuch pre-exiſt in the ſalt, whence it was obtained, ſo that we may ſuppoſe it to have been made rather by tranſmutation than extrication. And the like, I think, may (and that with greater probability) be ſaid of the ſpirit of nitre; for though this be highly acid, yet the nitre, that afforded it, is not at all ſenſibly acid: and this new vehement taſte of the ſpirituous parts, as well as their great efficacy in diſſolving metals, and divers other bodies, ſeems to have been produced by the violent action of the fire (agreeably to what I formerly noted) which by cleaving the nitrous corpuſcles, or by rubbing them one againſt another, or by both theſe ways, and perhaps by ſome others, makes a comminution of them into fragments or particles, which both becauſe of their ſmallneſs and lightneſs may be elevated by the action of the fire, and becauſe of the ſame minuteneſs and their ſharp and pointed figures may get into the pores of many other bodies, and divide their parts. I know, that chymiſts may object, that all the acid ſpirit, that can be diſtilled from nitre, was really pre-exiſtent in it, and only clogged and diſguiſed by the alkalizate ingredient, wherewith it was aſſociated, as may appear by what I myſelf relate of the ſpeedy way of making ſalt-petre, by putting a due proportion of the ſpirit of nitre to the alkali, or fixt part of nitre, that remains after the ſalt-petre has been fulminated or burned. But to this I anſwer, that this proves indeed (what I readily grant) that ſalt-petre may be artificially compounded of a nitrous ſpirit and a fixt alkali, but does not prove, that nature does always, or ſo much as ordinarily produce nitre by the ſame ways, that is, by compounding it of the ſame ingredients. For it does not appear, that wherever ſalt-petre is generated in the earth, nature has before-hand laid in a proviſion of lixiviate ſalt, which (at leaſt, in theſe countries) is not wont to be made without the violence of an incinerating fire and of corroſive ſpirits, to obtain which, or either of them, artiſts are fain to employ vehement fires; whereas it ſeems, that ſalt-petre is ſlowly generated in the earth by gradual or ſucceſſive alterations of ſome idoneous matter, wherein, for aught I have obſerved, not an acid, but an urinous ſalt is predominant; as may be made probable by what I have formerly related about earth, that had long lain covered with pigeons-dung in a dove-houſe, which I found in diſtillation to yield a volatile ſpirit and ſalt, much like thoſe of urine. Therefore I will not affirm, that nature does never employ fixt alkalies and acid ſpirits to make ſalt-petre; yet I ſee not, that chymiſts have hitherto given us, or perhaps offered us any cogent proof, that ſhe muſt neceſſarily do ſo. I further obſerve this more conſiderable argument, that, according to what I formerly noted, ſalt-petre diſtilled in cloſe veſſels afforded us but an inconſiderable quantity of fixt ſalt, and that too was but a very imperfect alkali; though the quantity of nitrous ſpirit was great enough to perſuade us, that not any thing near ſo much as was wanting of the entire weight of the ſalt-petre had paſſed into the receiver. And elſewhere I relate, that a friend of mine, with the help of a peculiar clay, obtained near a pound of ſpirit of nitre from a pound of ſalt-petre; whereas, on the other ſide, by a differing way of managing it, and without additament, I obtained, as I there relate, about ten ounces of fixt nitre from a pound of ſalt-petre. Whence it ſeems probable, that the ſame ſubſtance, that in crude nitre is almoſt inſipid, may by an operation of the fire be diſtilled into a highly acid ſpirit, as well as, by another operation and way of management, be brought into the nature of a fixt and cauſtic alkali. It may alſo be worth conſidering, whether, the ſpirits of nitre themſelves, when after being made *ſui juris* they compoſe a

Part I.

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diſtinct liquor, and are ſpecificated, may not be deprived of their acid nature, and may become, or at leaſt materially concur to make up, a fixt alkali. For if ſea-ſalt, which chymiſts do not pretend to contain any ſuch alkali, be thoroughly diſſolved in a ſufficient quantity of ſpirit of nitre, and impregnated, this compounded ſalt will, as I have formerly noted, yield a conſiderable proportion of fixt alkali like that of ſalt-petre, which is as likely to proceed from the nitrous, as from the marine part of the reſulting ſalt; and if it do, it will make it the more probable, that it is not neceſſary, that the ſaline corpuſcles of ſpirit of nitre ſhould be primordial bodies, ſince they may be deſtroyed or turned into other ſalts; which is not leſs repugnant to the nature of a principle, than it is to be *de novo* producible from a body, that was not acid before.

SECT. IV.

IT may add much probability to what has been above diſcourſed, concerning the producibleneſs of the differing ſorts of ſpirits, if it ſhall appear, that the ſame body, meerly by different ways of ordering it, may be eaſily enough brought to afford, either acid, or inflammable, or volatile, commonly called urinous ſpirits, as the ſkilful artiſt pleaſes.

AN inſtance of this may be afforded us by ſome legumens, as peas, or beans; which if they be newly gathered and diſtilled in a retort, it will, I preſume, be eaſily granted, that they will, like many other green vegetables, afford, beſides a great deal of phlegm, an acid ſpirit; and if I much miſ-remember not, I had ſuch a ſpirit from either peas, or beans, or both, after they had been kept long enough to loſe their verdure. But if theſe ſeeds be at a fit time duly fermented with common water, I think it will not be doubted, but that they, as well as other mealy ſeeds, will yield an ardent ſpirit; but it will not ſo eaſily be ſuſpected, much leſs believed, that without adding any thing to them, or meddling with them, barely by keeping them in a dry place, for a certain number of months, they will yield a ſpirit, that by one, that did not know whence it proceeded, would be judged near of kin to the ſpirit of urine, or of hartſhorn, and to other ſaline liquors drawn from animal ſubſtances. For having diſtilled theſe legumens by themſelves, without any additament, and without ſo much as breaking them, they afford me ſpirits, not only far more like in ſmell to thoſe, I have compared them to, than they were either to acid, or vinous ſpirits, but very like them, in more intimate qualities; ſince they would, as the ſpirit of urine and hartſhorn, make a conflict with acid ſpirits, turn ſyrup of violets green, diſſolve copper blue, precipitate a ſolution of ſublimate into a white ſubſtance; and in a word, perform thoſe things, which I many years ago delivered in the tract about colours, and ſeveral ingenious writers have ſince embraced, as the diſtinguiſhing marks of volatile and urinous ſalts, or ſpirits. I ſay ſalts or ſpirits, becauſe I found that theſe drawn from vegetables, as well as thoſe afforded by animals, may eaſily by a dexterous ſublimation be brought to exhibit many of their nobler parts in the form of a dry ſalt, as well as in that of a ſpirituous liquor.

ANOTHER inſtance I ſhall take from the juice of grapes, though chymiſts will perhaps think it ſtrange, that I ſhould undertake to accommoda e it to my preſent purpoſe; but there is no great myſtery in the buſineſs: for the freſh juice of grapes or muſt, though ſweet in taſte, will, if it be timely diſtilled to the conſiſtence almoſt of a ſyrup, yield a copious phlegm, but not an ardent ſpirit: if the ſuperfluous moiſture be ſkilfully evaporated, there will remain a kind of rob or ſapa of a pleaſant tart-

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nefs, which I have known ufed in fome places (as efpecially in, or near *Switzerland*) for an excellent ingredient of fauces, and alfo, to be fpread upon bread to be eaten inftead of butter.

BUT if this raifinee or fapa were prefently diftilled, I fuppofe it would yield no vinous, but an acid fpirit: I faid, I fuppofe, becaufe for want of vineyards in *England*, I could not examine any liquor taken out of great vefiels of muft, and therefore cannot fay precifely and experimentally, what diftilled liquors it would afford; fince I know not certainly, whether the great quantity of the fweet liquor, and its continuance for fome time (though not a long one) in the ftate of what they call muft, may not fomewhat alter its productions, but if, as it is probable, that diverfity be not confiderable, I may fafely fuppofe, that the vinous fpirit afforded by the juice of grapes, after fermentation has turned it into wine, is a produced thing, and was not in that form pre-exiftent in the juice; for having purpofely caufed ripe grapes to be moderately preffed, that their juice may, without much dreggy matter, be fqueezed out, we put this liquor into a glafs head and body, and diftilling it with a gentle fire, we obtained a fcarce credible proportion of phlegm: and then transferring the fomewhat infpiffated remainder into a retort, after having kept fome of it (which had a grateful mixture of fweetnefs and acidity) for raifinee, we profecuted the diftillation with a ftronger fire, and obtained not a vinous, but an acid fpirit, as we found, not only by its fmell and tafte, but by its corroding fragments of coral, even in the cold, by its growing fweet upon minium, *&c.* Agreeably to which experiment, I found by trial, that raifins, which confift chiefly of the juice of grapes, infpiffated in the fkins or hufks by the avolition of the fuperfluous moifture through their pores, being diftilled in a retort, did not afford any vinous, but rather an acetous fpirit, that, as an acid liquor, would work upon divers bodies, as fpirit of vinegar would have done, and yet, as it was formerly noted, it is known, that raifins being in a due proportion fermented with common water, will, after a certain time, afford a vinous and inebriating liquor; and though this time in the better fort of the known ways, of making artificial wines, is wont to amount to many months, yet I have practifed a way (which confifts chiefly in a determinate proportion of the water to the raifins) by the help of which, the liquor in very few weeks becomes fit to drink, and confequently to afford by diftillation a vinous fpirit; but this only upon the by.

I SHALL now add, which probably you will think fomewhat ftrange, that from the juice of grapes even after it has been duly fermented, there may be obtained a diftilled liquor, which having not found mentioned in authors, I thought, that I might take the liberty to name, and upon the account of its tafte, and fome other qualities, to call it the acid fpirit of wine: to fatisfy you therefore, that there is fuch a liquor, I will not make ufe of Rhenifh wine or other wines, that are thought to relifh of acidity, but I will acquaint you with an experiment, that I chofe to make upon fack, as a wine fully ripe, and more remote from an acid and tartarous nature, than thofe are wont to be, that are made in lefs hot climates. We took then fome good fack, and having a digeftive furnace, and in a glafs-head and body, flowly drawn off the ardent fpirit firft, and then the phlegm (which even in this generous wine was copious) till there remained a liquor of the confiftence of a fomewhat thin fyrup, we removed it into a retort, and diftilled it by degrees of fire, whereby we obtained, befides a fourifh phlegm, that came firft over, a true acid fpirit, as appeared not only by the tafte, but by the hiffing noife and numerous bubbles, that were produced, when we poured it upon a lixiviate falt; as alfo by this, that having put it upon powdered corals, it began briffkly to diffolve them, even in the cold: we likewife made

made it corrode ſome metalline, and mineral bodies, of which it is not here neceſſary to give you an account, no more than of the black ſubſtance, that remained after the diſtillation: only two things I will here intimate about them: the one, that as this acid ſpirit of wine has its origination differing from that of other known acid ſpirits, and even from ſpirit of vinegar, ſo I thought it not irrational to conjecture, that it might have ſome peculiar qualities, whoſe diſcovery I leave you (if you think it worth the while) to proſecute: only by way of encouragement, as well as hint, I ſhall tell you, that having put ſome of it, for a certain reaſon, upon filings of copper, in ſuch manner, that ſome of them, after being wetted with the ſpirit, ſhould remain expoſed to the air, and others lie beneath the liquor, I found, though the trials were made in the cold, that in a day or two the expoſed filings had gained a fine bluiſh-green colour, but the ſpirit, that ſwam upon the other filings, did in few hours acquire a fine redneſs, which afterwards in two or three days degenerated into a colour like that of the expoſed filings. The other thing I will note, relates to the *caput mortuum* of the diſtilled wine, which I found a more fixt body than one would have expected, and it is, that though probably the finer part belonging as to other vegetable mixts, ſo to the juice of grapes, being attenuated and ſubtilized, was changed into an ardent ſpirit, and therefore appeared not in the diſtillation, in the form of oil, yet it is not unlikely, that the coarſer part of the oleaginous ſubſtance remained ſtill in the *caput mortuum:* for holding it in the flame of a candle, I obſerved, that it would partly exhale in thick ſmoke, partly melt, and as it were fry, and partly burn with an actual flame, which was not only continued, while the flame of the candle cheriſhed it, but would after it was removed from the candle, continue a pretty while to flame upon its own account, and a parcel of it being caſt upon quick (but not upon flaming) coals, burned with a blaze, almoſt as if it had been amber, or bitumen. I could here tell you of fine cryſtals of wine, that I many years ſince made by a peculiar way, of the above mentioned extract of ſack; but this may be elſewhere more fitly mentioned.

To return therefore to our juice of grapes, we ſee, that meerly by a ſeemingly ſlight difference in the management of it, it may be made to afford either a vinous, or acid ſpirit; and I ſhall now add, that it may alſo be brought to yield a volatile or urinous one; for it is known, that in proceſs of time, wine affords tartar; and though chymiſts ſuppoſe the ſpirit of tartar to be of a quite differing nature from that of urine and of ſoot, and though I have elſewhere ſhewn, that tartar diſtilled the common way affords a double ſpirit, namely, an acid, and another, that I thought fit to call anonymous; yet I elſewhere ſhow, that by a peculiar and ſlow way of operating, I have been able to obtain (though perhaps not conſtantly) from crude tartar without any additament, a ſpirituous ſubſtance, that in taſte, ſmell, and divers manifeſt operations, much more reſembled the volatile ſpirit of urine, or rather that of ſoot, obtained as mine of tartar was, by meer diſtillation, than an acid ſpirit: with which (ſo little did they agree) it was diſpoſed to make a conflict as ſoon as they were put together. But ſuch a kind of volatile ſubſtance may be far more eaſily obtained from the lees of wine, than from tartar; for having been accidentally informed, that an expert chymiſt in *Germany* had found the way to get ſtore of volatile ſalt from lees of wine, I reſolved to try, whether it might not be done without any addition; and having procured ſome of the beſt lees (I could get) of Rheniſh wine, I cauſed them to be expoſed in broad veſſels to the ſun, and the free air (which circumſtance yet I am not ſure is neceſſary) that they might leiſurely be dried, if not alſo be impregnated, in order to the volatility of their ſaline parts. Then theſe dried faeces being carefully diſtilled

diſtilled in a retort by degrees of fire, the liquor was ſlowly rectified, by which means there aſcended before the phelgm a ſpirituous part, which would turn ſyrup of violets green, precipitate diſſolved ſublimate into a white powder, ſoon colour itſelf upon copper with a deep blue, and in ſhort do ſeveral things, by the performance of which we have elſewhere diſtinguiſhed volatile ſalts and ſpirits from acid and from vinous ones. By all ſuch trials upon the juice of grapes, we may infer the truth, we intended to prove by them, namely, that the ſame matter, as it is differingly managed, may be made to afford an acid (beſides one, that is truly acetous) a vinous, and a volatile ſpirit: beſides that, *ex abundanti*, it may alſo be made to yield, as I have noted in mentioning the diſtillation of tartar, another ſort of ſpirituous ſubſtance, as yet anonymous.

New obſervations about the adiaphorous ſpirits of Woods and divers other bodies.

AND now having fallen upon the mentioning of this ſort of ſpirits, that I have called anonymous, ſince I remember not that the notice I gave the public of them * has engaged any writer to examine them; I am content on this occaſion to touch upon ſome of the more quick and eaſy trials, that I have made about this kind of liquors, that I may both excite and ſomewhat aſſiſt the curioſity of thoſe enquirers, that ſhall attempt to make a farther diſcovery of the nature of theſe ſpirits; which when I firſt ſeparated from the acid ſpirits, wherewith chymiſts had before confounded them, as ſtyling them, and taking them to be meerly the acid ſpirits of tartar, wood, *&c.* their properties were ſo little known to me, that I contented myſelf to ſtyle them anonymous ſpirits: but ſince having found them to differ in divers qualities, both from vinous, from acid, and from urinous ones, and having not ſufficiently diſcovered their poſitive properties, I was wont to give them a negative appellation, and call each of them the neutral or adiaphorous ſpirit of the body, that affords it, (whether it be tartar, wood, or any other like concrete.)

BUT before I deſcend to particulars, it will not be improper to premiſe in general three or four things not unfit to make way for the obſervations, that are to follow them.

1. I KNOW not, whether it will be requiſite to repeat in the firſt place, that our adiaphorous ſpirit may be obtained by diſtilling the liquor, that is afforded by woods and divers other bodies; by diſtilling this liquor, I ſay, from corals, or calcined lead; for by this means the acid corpuſcles of the menſtruum will work upon the coral or the lead, and ſo faſten themſelves to what they corrode, that they will eaſily enough part with the adiaphorous ſpirits, which by this means are permitted to aſcend by themſelves and fall into the receiver, in the form of a liquor. This, as I was ſaying, I know not, whether it be neceſſary to inſiſt on in this place, becauſe I have already mentioned it in another paper: but I think it may be very pertinent to relate here, that I endeavoured to try, whether there was not a difference in gravity or fixedneſs between the acid and neutral ſpirit of wood, without mortifying the firſt; and whether by the help of this gravity and fixedneſs, I might not be able to ſeparate, at leaſt in great part, the acid from the other, and ſo preſerve it in its diſtinct nature.

In order to this, I cauſed a pretty quantity of the rectified ſpirit of box to be ſlowly diſtilled in a glaſs body and head, placed in a ſand cupel with the flame of a

* This was done in the *Sceptical Chymiſt*.

 lamp,

lamp, as that, which would give a more gentle and regular heat than charcoal; as indeed in the first twenty-four hours or thereabouts, this furnace afforded but about two spoonfuls of liquor; and though the menstruum first put in scarce exceeded, by our guess, one pint or pound (if it were so much) yet it was divers days and nights in drawing over. And in this operation the most observable circumstances were these two: First, that the liquor, that first ascended, was not phlegm, but had a very penetrant taste, yet without any manifest acidity, discoverable by the tongue, though by putting it upon fine powder of coral (whether crude or calcined I remember not) it had some operation, that made me think it not altogether devoid of acid particles. Secondly, having often shifted the receiver, the better to judge, whether the portions of the ascending spirit were considerably different in quantity, I found that, towards the latter end, the liquor, that came over, was sharper than before; and having, at length, distilled all I could make to arise, we found the last parcel of liquor (which was copious enough) to be of a good yellow colour (though those that preceded it were limpid enough) and both to smell strong of vinegar, and to taste more acid upon the tongue than spirit of common vinegar itself: so that if I had not known, how it was obtained, I had suspected it to be what the chymists call *acetum radicatum*; and accordingly I found it to be a very active menstruum in the dissolution of some bodies, that, for trial-sake, were put into it. All which seems to argue, that the acid portion of such distilled liquors, as I have been speaking of, is more ponderous, or more fixt, than the adiaphorous spirit, which, upon this account, may be in great part separated from it by bare distillation, if it be warily enough made.

My second general remark shall be, that I have observed these neutral spirits to be not all of them, in all things, of the same nature; since though they agree in some general attributes, which suffice to entitle them to the same species or denomination, yet they sometimes differ from one another in particular qualities: which advertisement I thought it necessary to premise, that it may not seem strange, and that I may not be blamed, though some of the trials I shall set down do not punctually succeed in their hands, that shall not make use of the anonymous spirit of box, which I employed; not because I think it better than any other, but because amongst divers, that I have made use of, I had then a greater quantity of it at hand. But though for this reason, when I shall speak what I have observed in an adiaphorous spirit, without naming it, I would be understood of the spirit of box, which I had freed from its acid mixture by distilling it from calcined coral; yet I shall not so confine myself to this, as not to mention, now and then, some other spirit of the same family.

The third general observation, that I shall make about our adiaphorous spirits, is, that though the new chymists, that have taken any notice of the distilled liquors, for example, of woods, were wont, by reason of their acid tastes to look upon them, as of a merely acetous nature, and having accordingly called them the vinegars or acetous spirits of wood, yet really the acid portion of these distilled liquors is far from being the greatest: for besides what other trials I have purposely made, I remember I took eight ounces of the rectified spirit of box, wherein the acetous and neutral spirit remained confounded, as they had been in the first distillation; and having poured this upon a quantity of calcined coral, sufficient to satiate the acid corpuscles (which quickly fell to corrode it with noise and bubbles) we gently distilled it to a dryness in a glass head and body, by which means we obtained of adiaphorous spirit but eight grains less than seven ounces and an half, and some of the menstruum having been wasted in the operation, the acid corpuscles remaining in the bottom,

with the coral they had corroded, weighed but between two and three drachms; which shews, that notwithstanding the not contemptible quantity of strong spirit of vinegar, that by our lately recited observation the distilled liquors of box does contain, the corpuscles, that make it so acid, being concentred, take up but a little room. And since it was rational to suspect, that the acetous corpuscles being made without fermentation, might have something peculiar in their nature, I caused them to be gradually distilled with a strong fire from the coral, and thereby obtained a very red spirit, of which, though many chymists would take it for a volatile tincture of coral, I shall only observe, that its smell was very strong, and its taste exceeding penetrant, but very differing from that of acid liquors.

WHETHER our adiaphorous spirit may (as I sometimes suspected it may) be generated, by a commixture of the finer parts of the oil of the wood reduced to an extraordinary smallness, and thereby capable of being exquisitely mixt with the phlegm, and strictly associated with its particles, I shall stay, till I be better furnished with experiments, before I venture to determine.

HAVING premised the foregoing general observations, I shall proceed to particular ones, as soon as I shall have advertised you, that for the better discerning the phænomena to be produced, I chose to make almost all the following trials in cylindrical glasses of about an inch in diameter.

To the phænomenon I am about to take notice of, I therefore give the first place, amongst those produced by the help of our adiaphorous spirit, because it is uncommon, and not unpleasant: for though we have many experiments of the sudden transmutation of colours, whereby we change one into another; yet we have very few of the production of colours *de novo*, in bodies, that were colourless before. And I remember not, that the writers I have since met with, have added any experiments of this kind to those three or four, that I have mentioned in the history of colours.

1. I SHALL begin then with observing, that having into our adiaphorous spirit of box dropt a convenient quantity of strong and transparent oil of vitriol, and shaking the liquor together, there presently emerged a rich and lovely red colour; which at first was diaphanous, but afterwards grew so deep, that it was opacous, though by shaking the glass, the thin liquor, that would slowly glide down the inside of the glass, being held against the light, manifested, that the colour was still red, though much more dark and muscadine-like than before.

2. SOME common aqua fortis being put to our neutral spirit, and shaken a little with it, presently gave it a rich amber or high yellow colour, but not a true red: but if the liquors were not mingled by agitation, the spirit did but slowly and gradually obtain the above mentioned colour, which was somewhat deeper than that of sack: after this change the liquor continued transparent, and which is a circumstance not to be omitted) the change at first was wrought without any manifest precipitation, though afterwards, when the mixture had rested a good while, there appeared some little and light fæculency at the bottom of the glass, and the inside of it, as far as the liquor reached, was sullied with a cloudiness not easy to be washed off. One circumstance more of this trial I must not omit, which is, that notwithstanding the strong and offensive smell, that is wont to be justly complained of in aqua fortis, in our mixture it was either none, or but very faint, being concealed (if I may so speak) or suppressed, and partly perhaps disguised by the predominant odour of the adiaphorous spirit.

3. SOME spirit of salt being mingled with our spirit of box, the mixture became much less diaphanous than the liquors had been before their conjunction, and for a day

day or two was only whitiſh; but when we removed it into a digeſtive furnace, and kept it there for many hours, it acquired a colour high enough, partaking of brown and yellow, and appeared to have let fall ſome little ſediment to the bottom of the glaſs.

4. HAVING put ſome of our adiaphorous liquor on ſalt of tartar, it had not any ſenſible operation on it that we took notice of, ſave that it diſſolved the ſalt; and, after ſome digeſtion, appeared of a yellow colour tending to brown, and faſtened to the inſide of the phial in many little grains of ſalt, that ſeemed to have been firſt diſſolved, and then coagulated again in newly-emergent figures.

5. OUR adiaphorous liquor being confounded with high rectified ſpirit of wine, neither of them appeared to change colour much (for ſome change there was towards yellowneſs) or be opacated by their conjunction, even after ſome days digeſting; but the vinous ſpirit did not hinder the other from being turned red by the action of ſome potent acid, when it was poured on the mixture.

6. RECTIFIED ſpirit of urine being put to our adiaphorous liquor did not make any conflict with it, but joined with it quietly, as the above mentioned ſpirits had done, and did not manifeſtly change the colour of either of the liquors, whilſt they were kept many hours in the cold; but being transferred into a digeſtive furnace, and kept there a night or two, the liquor acquired a high colour, which was almoſt orange-brown, and there appeared ſome little fæces at the bottom. Having made theſe trials upon our ſpirit with ſimple liquors, I thought fit to make ſome with ſuch compounded liquors, as the ſolution of metals are, to ſee if our ſpirit, though neither manifeſtly of an acid, or an urinous, or a lixiviate nature, would procure precipitations of any part of the diſſolved metals.

7. IN proſecuting this enquiry, I dropt into ſome of our ſpirit a little ſolution of refined gold, which at firſt imparted thereto its own colour (perhaps ſomewhat heightened) but the mixture quickly loſt its tranſparency and grew muddy, and, after a while, let fall a conſiderable quantity of ſediment or precipitate, the ſupernatant liquor having acquired a browniſh colour.

8. HAVING mixt our ſpirit with a good ſolution of crude lead, made with an appropriated menſtruum, that diſſolves it readily and clear, almoſt as aqua fortis does common ſilver, the mixture preſently grew muddy, and at length, after ſome days, let fall a copious ſediment, over which ſwam a liquor between brown and red.

9. WE put to our ſpirit of box ſome fine ceruleous tincture or ſolution of copper, made with an urinous ſpirit (as of putrified urine, or ſal armoniac) and ſoon perceived the mixture to grow troubled, which afforded us, though but very ſlowly, a copious reſidence.

10. WE mingled with our ſpirit a convenient quantity of ſtrong infuſion of ſublimate made in fair water, but found not any manifeſt re-action between thoſe liquors, no more than we did between dry and undiſſolved ſublimate and the ſame ſpirit, when we kept them together in this ſame phial.

11. MIXING our ſpirit with oil of tartar *per deliquium*, there did not ſuddenly appear any manifeſt change; but having digeſted the mixture for ſeveral days, there precipitated a light fæculency; and the ſupernatant liquor, which was tranſparent, appeared of a colour inclinable to red.

12. WE alſo mingled with ſome of our ſpirit a convenient quantity of vitriol of copper diſſolved in fair water, till the liquor ſeemed ſatiated with the vitriol; but I remember not that in ſome days, but the ſolution grew manifeſtly opacous or diſcoloured.

13. We put to our ſpirit a ſolution of tin, made in a menſtruum, that diſſolves it clear, and found very little alteration to enſue, though we left the liquors many hours together.

14. But when I put to our ſpirit a convenient quantity of the ſolution of mercury, made in aqua fortis, the colour of the mixture became firſt deeply yellow, and in a minute or two intenſely red; and being digeſted for ſome days, I found at the bottom of the phial a white precipitate, much more copious than I expected, and the tranſparent liquor, that ſwam above it, was of a rich golden colour. Whether phyſicians or ſurgeons ſhould think fit to employ this precipitate, or this tinged liquor for medicinal purpoſes, I ſhall leave them to conſider.

15. Several of the foregoing experiments were tried with the ſpirits of other woods than box, and in particular with thoſe of oak and guaiacum, the phænomena of which experiments were not always the ſame with thoſe above recited, which may probably argue ſome difference in the nature of ſuch ſpirits, as well as there is in the conſtitution of the woods, that afforded them; nor for certain reaſons have I thought fit to recount here all the trials I have made with the adiaphorous ſpirit of box itſelf, of which ſort I ſhall for example-ſake name only two, which I remember as having been the lateſt I made; whereof the firſt was, that having put ſome of our neutral ſpirit upon ſome pieces of fine red coral, and kept them there many days, the liquor did not appear to have extracted any tincture from them, though the upper part of the higheſt fragments ſeemed to be turned white. And the other was, that having taken a parcel of ſpirit, that came over by rectification in a lamp furnace, long before the more fixt acetous ſpirit came to aſcend, and having purpoſely expoſed a phial ſcarce half full of it, in a very ſharp froſty night, in a garden covered with ſnow and ice, it was taken up the next morning, not at all frozen, but leſs limpid than before, and this little opacity did (ſomewhat to our wonder) remain more or leſs for ſome weeks after.

ABOUT THE

PRODUCIBLENESS OF SULPHURS.

PART III.

THOSE ſubſtances, that chymiſts are wont to call the ſulphurs of the mixt bodies, that by the help of the fire, are brought to afford them, are not of ſo uniform a nature, as might be expected in the portions of the ſame principle. For as, on the one ſide, chymiſts make inflammability to be the conſtituent character of ſulphur, ſo on the other ſide, it is obvious enough to thoſe, that are any thing verſed in ſpagyrical operations, that there are at leaſt three ſubſtances manifeſtly differing in conſiſtence, texture, or both, that, according to the notion lately aſſigned, ought to be referred to ſulphur. For ſometimes the inflammable ſubſtance, that is obtained from

from a mixt body, by the intervention of the fire, appears in the form of an oil, that will not mingle with water; sometimes in the form of an ardent spirit, that will readily unite with that liquor; and sometimes also in the form of a consistent body, almost like common sulphur.

NOTWITHSTANDING these various forms, in which it appears, it is not impossible, but that in many mixt bodies, not to say in all, what is called sulphur may be no primordial ingredient, but rather a generated or resulting thing. For that, which is common to these differing bodies, that pass under the name of sulphurs, and which is the constituent quality (if I may so call it) that discriminates them from the other material principles of mixt bodies, must be confessed, if we will speak intelligibly, to be inflammability, or, if you please a disposition to be turned into fire, and usually also into flame. Which being premised I consider here, that sulphur itself is made of the same universal matter, whereof other bodies consist, and is but a coalition of certain particles thereof, whose aggregate, by having such a contexture, motion, *&c.* acquires those properties, for which a body is called sulphur. And therefore if the like contexture happen to be found in other portions of matter, or (to express myself more fully) if art, or chance can frame and bring together particles of matter, and give them such a contexture as is apt and sufficient to dispose them to be kindled and flame or burn away; these qualifications of such an aggregate of corpuscles will suffice to confer on it the nature of a sulphur, whether this portion of matter do, or do not consist, or copiously participate, of the chymists primeval sulphur. For it is not by virtue of the long preceding duration of a thing, but by that of the essential qualities belonging to it, that a body deserves this or that denomination. As the snow, that fell yesterday, and was generated in a trice, is as true snow, as that, which has lain, perhaps for many years, on those *Alps*, that are always covered with snow, or on the highest mountains of the frigid zone. And in the judgment of the chymists themselves, a pound of quicksilver recently transmuted by a grain or two of their elixir into gold becomes as true gold, as that, which was coeval with the mountains, where nature has formed the antientest mines of that metal.

SECT I.

Of the production of oils.

THAT it is not necessary, the oils, or sulphurs obtained by the fire from mixt bodies, should be a primeval element or principle, may be probably argued from the experiment mentioned in the *Sceptical Chymist* about the growth of plants nourished by meer water, which nevertheless, by distillation, afforded an oil. And we see, that in almond trees, walnut trees, and divers others, the rain water, that insinuates itself into their roots, is, by successive changes of texture, reduced into the oil, which the fruit by expression so plentifully affords. And to confirm our experiment from the growth of plants by transmuted or assimilated water, to obviate the suspicion of common water's being impregnated with the grosser juices of the earth, I employed distilled water. About which experiment I find this short memorial among my *adversaria*.

[A SPRIG of mint put into rain-water, distilled, and fed almost wholly with redistilled rain-water, weighed *July* 15. gr. 3. and was taken out *August* the 14th, and being well dried with paper and a cloth, weighed 10 grains and about a quarter: so that within less than a month it grew to be three times as heavy, as when it was first

put

put in. Another put in, and taken out at the same time with the former, had attained within less than a month to near four times its first weight, and had shot out a second sprig much higher than the first, and store of roots, some of them near as long again as the whole plant, when it was first put in.]

If we consider, what a great quantity of oil is afforded by an olive-yard, whose trees are probably, as well as those that bear apples, cherries, and other kinds of aqueous fruits, nourished chiefly by rain-water, that being imbibed by the roots is by various digestions, or preparatory changes, turned into oil in the olive, it will not appear unlikely, that oil may be produced of other substances: since in our instance it seems to have been made by transmutation of water, though this be generally reputed to be of all liquors the most contrary to it, and is evidently of a nature exceeding distant from it.

And here I shall relate an experiment, by which I attempted to produce it, out of only two distilled liquors, that according to the common estimation of chymists are uncompounded bodies; and, whether they be really so or not, are each of them readily dissolvable in water, and in one another. Take then of oil of vitriol, and of such spirit of wine, as is totally inflammable, an equal weight; mix them together by degrees, lest the heat they will produce should breed some inconvenience, and having digested them a good while (which yet is not absolutely necessary) with a very wary management of the fire (for else the experiment will easily miscarry) draw off what will come over: and if you go to work, as I have several times done, you shall obtain, besides a subtile and odoriferous spirit of wine and an acid sulphureous liquor, a considerable quantity of chymical oil, which I have had sometimes deeply coloured, sometimes clear like fair water: and this oil you will perchance look on as an odd liquor, when I tell you, that I have had it, sometimes exceeding fragrant, and (though the oil of vitriol be so highly corrosive) without any acidity at all, the taste of it being very subtile and penetrant, but no way like that of any saline liquor, that we know. This hath sometimes inticed me to doubt, whether it hath been made of the spirit of wine, or of the oil of vitriol. The circumstances last mentioned seem to persuade the former; especially if I add to them, that I found by trial purposely made, that this oil would readily mix with good spirit of wine, that had never had to do with oil of vitriol; but on the other side it seemed considerable, that the oil of vitriol by this operation was much weakened and changed, and it appeared not, whence the spirit of wine should have so great a fragrancy; which considerations were backed by this more weighty argument, that this oil was so ponderous, as to sink not only in common water, which is yet a far more heavy liquor than pure spirit of wine, but in the acid spirit itself, which seemed to be the remains of the altered oil of vitriol, which, by reason of its abounding in salt, you will easily grant to be far heavier than common water. But I need not much trouble myself, to determine, which it is of the two liquors, that affords this strong oil; for it may well be (though not equally) composed of both, by their mutual action, and the operation of the fire, united in the form of oil. And if it be objected, as probably it will, that this inflammable substance is made but by extrication of the parts, that lay concealed in the liquors before they were brought together, it may be answered, that this should not be supposed, but proved; which till it be, our cause will be favoured by our experiment, wherein there appears nothing so likely as a change of texture; to which may be ascribed the production of our anomalous chymical oil, since this plainly seems to result from two bodies, whereof neither was a true oil before. For each of them would readily mingle with water, whereas this produced oil of

of ours, being shaken with water, would break, like common oils, into numerous little globules; which would presently after sink to the bottom, and re-unite there into a liquor, which for trial-sake I have kept divers weeks in the water, and found it at last undissolved by it. Some odd properties of this oil make it seem likely to participate of some of the nobler parts of vitriol, and the sulphur of that metal having extraordinary virtues ascribed to it, by some of the famousest and intelligentest Spagyrists (as *Basilius Valentius*, *Helmont*, &c.) I kept some quantity of this oil by me for several years, to observe, as I did with pleasure, the alterations, that time would produce in it, and afterwards I imparted either some of the medicine itself, (whereof the first trial proved very successful) or the ways of preparing it, or both, to some ingenious men, who (I am told) did not all of them remember me in the free mention they made of it.

But this concerns not our argument, upon occasion whereof I shall observe upon the by, that though chymists should be able to prove, that our oil was but separated from the spirit of wine, or the oil of vitriol, in which it was latent before, yet still the experiment would afford me a considerable reason for questioning a main point in the doctrine of the vulgar chymists, who confidently pretend to prove from the number of similar substances (as they suppose them) obtained from a mixt body, that it was actually compounded of just so many distinct and true material principles, and such a quantity of each. For if from a distilled liquor, as the oil or rather ponderous and acid spirit of vitriol, or from alcohol of wine, which is commonly reputed to be uncompounded, a liquor of quite another kind may be (not made but) separated, how little reason have we, to take it for granted with the chymists, that every distilled liquor, that they look upon as one of the component principles of the body, that afforded it, is a homogeneous substance not further divisible into differing parts?

SECT. II.

Of the production of inflammable spirits.

After what has been hitherto delivered concerning the production of oils, I should now proceed to that of another sort of liquors, referred by the chymists of the principle they call sulphur; though better known to others by the name of inflammable spirits. But of these I shall purposely forbear to discourse in this place, and rather refer to what I have said to them in another, where I thought it more proper to consider them; namely, in one of the sections of that part of these notes, that treat of the producibleness of vinous spirits.

SECT. III.

Of the production of consistent sulphur.

If you should here tell me, as perhaps you will do, that what I have been hitherto saying relates but to inflammable liquors, whereas sulphur, in its most proper and primary acception, signifies a mineral body; I shall answer, that, as I formerly intimated, the chymists use the term sulphur so ambiguously, and so uncertainly, that they have made it difficult for other men's discourses to avoid all appearance of participating of the confusedness, they seem to have affected in theirs. But because the

the moſt intelligible, and leaſt indefinite notion their writings ſuggeſt of ſulphur, is, that it is a combuſtible and inflammable principle; I have hitherto treated of it as ſuch. And as for that ſulphur, that is commonly known by that name, and bought in ſhops, though I know there are ſome chymiſts, that have affirmed, that from vegetables and animals they can ſeparate ſuch a ſulphur; yet ſince they are not wont to teach us the way of doing it, nor give us any proofs, beſides their own word, of their having ever done it themſelves, the thing has ſeemed ſo improbable, that I find few or none of the more judicious of their own party, that look upon it, as other than a brag: only a follower of *Glauber* I find to have undertaken, by his maſter's directions, to produce a real ſulphur, like the mineral, out of vegetable charcoal, by a way, which, becauſe it has deceived more than him, and is ſpecious enough to impoſe upon thoſe, that either are not chymiſts, or, if they be chymiſts, are not cautious men, I ſhall here ſet down, and examine, as after the author I made it.

WE took then equal quantities (ſuppoſe a pound of each) of good oil of vitriol and of common ſea-ſalt, diſſolved in as much water as was requiſite: this mixture was ſlowly diſtilled, till the bottom was throughly dry (which it will not be ſo ſoon as it begins to look white, or appear coagulated) then ſetting aſide the liquor (whereof the firſt part that came over was phlegm, and the other part ſpirit of ſalt) we took out the *caput mortuum* (which, if one pleaſes, may be purified by being diſſolved and philtered) and having beat it to powder with about one fourth or one eighth part of its weight of charcoal, we put it in a ſtrong crucible, and kept the mixture melted in a vehement fire, till it grew of a dark reddiſh colour, for by that time ſuch a change was made in the maſs, that it both ſmelt and taſted rankly enough of ſulphur; and if ſpirit of ſal armoniac were ſeaſonably diſtilled from it, with a competent, but not over-haſty fire, the aſcending ſpirit would be manifeſtly impregnated with ſulphur not difficultly ſeparable, which may alſo be divers other ways obtained from the ſame fixt *caput mortuum*.

BUT for all this ſpecious operation, I do not take the ſulphur, thus produced, to have been the vegetable ſulphur of charcoal, but a mineral ſulphur, that lay concealed in a liquid form among the ſaline parts of the oil of vitriol.

FOR, firſt, it is not likely, that ſo ſmall a quantity of charcoal, as was employed in this experiment, and much leſs that ſo ſmall a quantity, as may ſuffice to make it, could contain ſo much ſulphur, as may this way be obtained.

NEXT, that common vitriol is not deſtitute of mineral ſulphur, may be eaſily conjectured by the ſulphureouſneſs of the marchaſites, whereof it is wont to be made. In ſo much that in divers countries, as about *Liege*, and in ſome parts of *Italy*, from the ſame ſubſtance, that affords them vitriol, they obtain by ſublimation great quantities of common ſulphur, which is ſold for ſuch into divers other countries. And I have found by trial, and do not at all think myſelf in that ſingular, that one may obtain from vitriol an oil, and a *caput mortuum*, which being put together, afforded a ſmell of common ſulphur ſo ſtrong, that I was ſcarce able to indure it.

AND to come yet more cloſe to our experiment, I have (as I have elſewhere mentioned) purpoſely tried, more than once or twice, that by diſtilling together common oil of turpentine and common oil of vitriol, the former of thoſe liquors would make a ſeparation of ſome of the ſulphur, that lay concealed in the latter, and as it were extricate and extract it: ſo that beſides an exceedingly ſulphureous liquor, which ſometimes was made white by the copiouſly diſſolved (and partly precipitated) ſulphur, that paſſed into the receiver, we had in the neck of the retort a yellowiſh conſiſtent body, which being put upon a quick coal, would, after a little yellow flame (probably

(probably proceeding from ſome adhering parts of turpentine) afford good ſtore of bluiſh flame, like that of common ſulphur, which it alſo emulated in its ſmell. And ſuch a kind of ſulphur I have alſo ſeen, in tract of time, ſettle itſelf, in no inconſiderable quantity, at the bottom of the liquor, diſtilled from the mixture of the two above mentioned oils. Nor are theſe the only ways, by which I have obtained from oil of vitriol manifeſt proofs of its containing a mineral ſulphur very like to common ſulphur.

AND in particular it now comes into my mind, that I once put into a retort, together with one part of running mercury, four parts of oil of vitriol, and having diſtilled off the menſtruum, by degrees of fire, there remained at the bottom of the glaſs a very white powder. This calx, being afterwards gradually preſt with a ſtronger fire, afforded in the upper part of the retort a great many ſmall bodies, that looked like half beads of amber, and ſeemed to be of very fine ſulphur, (but were afterwards confounded with many other aſcending corpuſcles.) This amber-like body (which was ſomewhat copious, and, as to ſome portions of it, whitiſh) by its readineſs to be melted, by its ſmell, and by the bluiſh flame it afforded, when it burned, appeared to be a kind of ſulphur, which you will eaſily grant to be far more unlikely to have proceeded from ſo homogeneous a body, as the quickſilver, than from the oil of vitriol, which we have already ſhewn to conſiſt of divers ſulphureous as well as many acid corpuſcles. And on this occaſion I remember, that, whereas upon mingling the oils of turpentine and of vitriol in a due proportion, I have conſtantly obſerved, that they incorporated into a mixture, that was deeply red (and this may eaſily be tried by letting fall two or three drops of oil of vitriol upon ſome drops of that of turpentine, and mixing them in a concave veſſel, or even in a hollowed piece of paper) whereas, I ſay, I obſerved this, I was thereby induced to ſuſpect the chymical (for I ſay not, the optical) cauſe of that phænomenon might be, that the terebinthinate oil had made a ſolution of divers ſulphureous particles it met with in the oil of vitriol, and by that means acquired ſuch a redneſs, as we ſee that common flower of ſulphur gives to the oil of turpentine, when it is diſſolved in it. And to examine this conjecture, I found, that divers other chymical oils, and oil of aniſeeds itſelf, as remote as it is from redneſs, would preſently acquire that colour, being carefully incorporated with a due quantity of oil of vitriol. But this conjecture is propoſed only upon the by.

As for the ſulphur of minerals and metals, I confeſs, I have not yet found enough, either in chymiſts books, or in my own experience, to make me willing to ſpeak dogmatically about them. And this the rather, becauſe firſt, as to the ſulphurs, that are ſometimes obtainable from ſome of the minerals, I think it may be doubted, whether they belonged to thoſe minerals as eſſential ingredients, or were only corpuſcles of common ſulphur, perhaps a little altered, that were mingled in the bowels of the earth, with other parts, that are eſſential to the nature of the mineral. As we ſee, that in native cinnabar the mercury, which, according to chymiſts, is a complete metal by itſelf, is ſo mixt with another body, as not to be diſtinctly diſcernible, till it be ſeparated by the fire. And this cinnabar affords me an inſtance, the more fit for my preſent purpoſe, becauſe I have ſometimes by an eaſy way obtained a ſulphur alſo from it: and ſince we have lately noted, that the vitriolate marchaſites afford great ſtore of common ſulphur, by a groſs way of ſeparation, it ſhould not ſeem irrational to ſuſpect, that ſome common ſulphur may remain more cloſely mixt with the ſaline and metalline parts of the vitriol afforded by the ſame marchaſites; from which latent corpuſcles of ſulphur may in part proceed the ſulphureous ſmell, and

other like things, that we have lately taken notice of in vitriol, and its oil. And perhaps by the same consideration one may account for the sulphureous qualities, that are sometimes to be met with in the liquors, that pass for the vinegars of mineral bodies, and particularly antimony; to which may now and then be added some metalline ores: since, I remember, I have had such a sulphureous liquor from good lead-ore, that I had ordered to be purposely digged out of the mine at *Minedeep*, and being put in close vessels speedily conveyed to me. And that nature herself may blend an imperfect mineral with lead, I have had occasion to observe by an ore, whereof the owner found a mine, but not being able to discover what it was, desired me to inform him. For this gave me occasion to consider the ore (whereof I have yet a lump by me) and to observe, that it was so differing from the other ores of that country, that I did but diffidently guess, that it was a mixture, that nature had made of lead and antimony, till particular trials had justified my suspicions.

But this is not all I had to say about the sulphurs of fossils: for though I know, that chymists pretend to teach us ways of extracting the true sulphurs of minerals and metals; and experience assures me, that a real combustible sulphur may be in a pretty quantity obtained from antimony; yet there are two scruples, that suffer me not fully to acquiesce in what they teach. The first is this, that chymists oftentimes deceive others and themselves too, by mistaking those things for the true sulphurs of minerals and metals, that really are not so: of which I shall give a plain instance in the preparation, that many Spagyrists deliver of the sulphur of antimony.

For when they have boiled that mineral in a strong lixivium of pot-ashes, they suppose, that, as by the same operation, common sulphur is dissolved, so the menstruum seeks out, and takes up, only the sulphureous parts of the antimony: and as common sulphur is precipitated out of the lixivium, wherein it is dissolved, by sprinkling on it vinegar, or some other acid; so they presume, that what is struck down the same way from the solution of antimony, made in the same menstruum, must be the true sulphur of that mineral; in which they are confirmed by the colour: and yet in reality, not only the sulphur (supposing that there is one) but the other parts of the antimony will be dissolved by a strong lixivium, and the yellow powder, that is precipitated, is but a kind of crocus, which will sometimes after a while (at least in part) subside of itself, without the help of an acid. Nor does it convince me, that such a body obtained from a mineral, or metal, is its true sulphur, that it may be made to burn; for unless the colour and smell of the flame concur, I shall be prone to suspect, that the inflammability may be apt to rise, partly from the great comminution made of the prepared body, and partly from the additament employed in preparing it. For these two, and perhaps even one of them, may contribute so much to the inflammable disposition of a body, that little, or no true sulphur, will be necessary to make it burn. Of this I elsewhere give an instance in plates of copper; from which an equal weight of sublimate has been distilled: for the remaining mass will melt and burn at the flame of a candle, almost as readily as sealing-wax. And of these instances I mention more in another paper, where I endeavour to shew, that combustible and inflammable bodies may be made up of parts or ingredients, that singly had not such qualities. And yet the contrary of this is supposed in the chymical argument, that infers from these qualities the presence of sulphur in all those mineral preparations, wherein they are found. Yet by this discourse I would not be thought to derogate from the medical virtues, or other utilities of such supposed sulphurs. For they may be very useful concretes, though they be not true principles; the finer parts of the mineral being in some of these preparations extracted, and

Of the Producibleness of Inflammability.

and further divided, and perhaps very luckily associated with the finer parts of the body, employed to act on them. By which means there may emerge new concretes of great virtue and use. And therefore I intend not to derogate from those metalline sulphurs, which some few masters of chymical arcana reserve with great care among the chiefest they are proud of. And that you may the better examine these fine crocuses, as I am apt to think most of them, and try both what they are, and what they do; if I can light on the process (for I dare not trust my memory) I will at the end of this Appendix impart to you a way of preparing some of those, that are made of metals, those being accounted the most difficult as well as noble. And thus much I now remember of the trials I made according to this way; that I employed not any acid menstruum, or liquor made of any particular salt, but a neutral or compounded salt; which, whilst it was in actual fusion, would dissolve or corrode the very thinly laminated metal.

I DO not look upon these substances as the true sulphurs of the metals, that afford them, but rather (as I lately intimated) suspect them to be a sort of fine crocuses, and perhaps magisteries; which by reason of the subtilty, and sometimes fixtness of their parts, may prove useful to considerable purposes both in alchymy and physic.

BUT there is another sort of bodies obtained from some minerals, and perhaps some metals too, that has a greater resemblance to mineral sulphur, than the newly mentioned crocuses have.

To this purpose I remember, that by putting aqua fortis, in a certain proportion, upon crude antimony, and, after it was almost totally distilled off, increasing the fire till a dry substance began to sublime, we had in the upper part of the retort a yellow and brittle substance, which being carefully separated from the dark-coloured antimonial powder, that was also elevated by the force of the fire, appeared not only by its own colour, but that of its flame, and some other signs, to be much of the nature of common sulphur. Nor is this the only way, whereby we have obtained such a substance from crude antimony, from which (if I much mis-remember not) I have had a yellow and combustible sulphur, even without the help of a menstruum.

PARACELSUS pretends to have a way of drawing sulphurs from all metals; of which process because I have found little or no notice taken by chymists, I shall, for the oddness of it, and the reputation of the author (whom I look upon as a man of great experience in metalline affairs) subjoin it, as I find it among some of his loose papers or fragments.

Sulphur Metallorum Theophrasti.

SULPHUR *metallorum est oleitas ex ipsis extracta, prædita virtutibus pro hominis salute. Sulphur aliud ex metallis, antequam ignem sunt passa, elicitur, ut ex marchasitis aureis, aut argenteis, &c. secundum nobilitatem mineræ, etiam nobile & præstans:* ' & paucis ' interjectis (extractio sulphuris ex mineris metallicis.)' *Etiam fieri potest per lixivium acre & depuratissimum: sed* (vel potius illa) *alia sulphura pro intrinseco corporis usu minus sunt commoda propter alkali cinerum, ex quo clavellatum conficimus erodens, & propter calcem, ex quibus talia fiunt lixivia. Sulphur sic extractum potest ablui aqua dulci, & præcipitari. Digestio duplum requirit temporis. Debet & rectificari lixivium per ipsius sublimationem ab omni residentia terrestri, ne cum ipso incorporentur talia sulphura, & fiant corrosiva, ad perniciem ægrotorum: quod ne fiat dicta, debet fieri separatio. (Tantum de crudis.)*

SED *jam fusis & depuratis elicias ipsorum sulphur: certa nobilior meliorque via non dabitur, quàm per aquam salis seu oleum ipsius præparatum, eo modo, quem in alchymia luculentur descripsi. Talis quippe aqua fundaliter & radicitus extrahit omnibus corporibus metallicis liquorem ipsorum naturalem, seu sulphur & crocum præstantissimum, tam pro operibus medicis, quàm pro chymicis. Resolvit & frangit unumquodlibet metallum, ex natura ipsius metallica deducens in aliam pro varia intentione & industria laborantis.*

THUS far *Paracelsus*'s process; but as I know not whether it be true, because I am not able to reduce it to practice; so, because I do not clearly understand his meaning, and what is the true nature of the instruments he would have us employ, I will not take upon me to determine, whether or no the sulphurs, he teaches us to be obtainable by this method, be genuine ones, and fit to decide the question we are now considering.

BUT whatever become of this obscure Paracelsian process, what I was saying about a sort of bodies less remote than the formerly described crocuses from the true sulphurs of metals (if they have any such) may well subsist. For I remember we have sometimes (though the experiment did not always succeed) by cementing very thin plates of a certain metal with burnt allum, and afterwards dexterously elevating the more disposed parts with sal armoniac, obtained a sublimate, from whence we separated, by ablution with fair water, that dissolved the salt, a substance, which, by its inflammability, appeared a kind of metalline sulphur.

AND this may suffice touching the first scruple I thought fit to propose, concerning the factitious sulphurs of metals and minerals. To proceed therefore now to my second scruple, it may, I think, be suspected, that even this sort of bodies, which I have mentioned to have been drawn from a metal and from antimony, may not be the effects of a bare separation of pre-existent sulphur, from the other ingredients of the bodies, that yielded them, but new concretes produced by the operation of the fire on those bodies, and by the combination of some of their parts with those of the additament, employed to obtain the sulphurs. For, as far as I have yet seen, either salt-petre, crude or distilled, or menstruums made of it, or of other salts, or else oleaginous liquors, are wont to be made use of on these occasions. And it is very possible, that some of the more disposed parts of these additaments may associate themselves with those of the mineral or metal to be wrought upon; and so, from this combination of the ingredients, there may result a body of a new texture, which texture may dispose it to be combustible, or inflammable, whether the ingredients in their separate condition were so or not. As I remember I have elsewhere shown, that though aqua fortis be not inflammable, nor a piece of crude copper inflammable or combustible in a common moderate fire, yet the metal being dissolved in aqua fortis, and the superfluous moisture warily exhaled, there will remain a fusible concrete, wherein the copper being much comminuted, and its small parts fitly associated with the saline ones of the menstruum, compose a kind of vitriol, that being held to the flame of a candle, or even of a piece of paper, will readily burn away in a flame finely coloured; and which may, if one please, be kept in a flame distinct from the other.

To conclude what I have to say about my second scruple; it seems not improbable, that if any of the metals be, by a fit menstruum, or some other congruous additaments, reduced to parts minute enough, and that these parts be fitly associated with some of those of the menstruum; the metal may thereby be brought to burn or flame, as I have successfully tried, by a way elsewhere declared, upon gold itself, whose sulphur the chymists would have us look upon, as (what seems not very agree-

able to the nature of ſulphur) incombuſtible; ſo that, for aught yet appears, it is allowable to ſuſpect, that the ſulphur obtained from this or that metal is not ſo much an elementary or hypoſtatical principle barely extracted, as it is a magiſtery, or ſome other new compound, made by the combination of the metalline particles with all, or ſome of the body, that works on them. But if a chymiſt will have metalline preparations of this kind to be ſulphurs; I may be allowed to make them ſerve for inſtances of the producibleneſs of ſulphurs.

YET theſe doubts concerning the ſulphurs of metals I propoſe but as ſuſpicions, to draw on further and more accurate trials; by which perchance they may be happily removed. And ſpeaking of the ſulphureous principle of mixt bodies, in that general notion of it, wherein the chymiſts often uſe it, and indeed muſt employ it; we may be thought to have ſaid enough to our preſent purpoſe, though we had left mineral ſulphurs untouched; ſince we have ſhewn, that inflammable parts of mixt bodies may be produced, and therefore cannot be ſafely affirmed to have all been pre-exiſtent in them.

OF THE

PRODUCTION OF MERCURY.

PART IV.

THE complaint I have divers times had occaſion to make, of the darkneſs and ambiguity, that chymiſts have allowed themſelves, if not affected, in treating of their three principles, is applicable to nothing more juſtly, than what they have written about that, which they call mercury. For when they would ſeem to tell us, what they mean by that principle, they are wont to do it in terms ſo looſe and ſo ambiguous, that the repreſentations they make of it, are more like to panegyrics, and ſometimes to riddles, than to clear definitions, or ſo much as good deſcriptions. Since then they have given us no ſettled notion of what they call mercury, but have left us to gueſs what they mean by it; I hope a miſtake about it (if I ſhould run into any) would appear pardonable. That which is agreed on by the moſt of chymiſts, when they ſpeak ſomewhat intelligibly of the principle they call mercury, is, that it is a crude ſubſtance, and that it is a volatile liquor; which, by being ſo, may be diſtinguiſhed from the ſaline principles, eſpecially from the alkalizate or fixed ſalt; as it may alſo be from the oil or ſulphur, by its not being inflammable. But theſe marks will not diſcriminate it from phlegm, which is alſo a fugitive and uninflammable liquor; and therefore to make difference, they muſt add ſome other quality, ſuch as ſapor (which yet agrees not to quickſilver itſelf) that is wanting to phlegm. So that, according to this doctrine, the nature of a chymical mercury or ſpirit will conſiſt in its being a liquor volatile, not inflammable like oil or ſulphur, nor yet inſipid like phlegm. How odd a principle this muſt be, that comprifes ſuch differing bodies, as are acid ſpirits, thoſe of nitre and vitriol; urinous, as thoſe of blood,

blood, hartshorn, *&c.* and anonymous ones, as those of guaiacum, honey, raisins, *&c.* forced from their acidity; and the running mercuries of minerals and metals, as cinnabar, antimony, and lead; under one principle, which, to deserve that name, ought to have all the portions of matter belonging to it homogeneous; I may safely leave any considering naturalist to judge. And therefore instead of taking further notice of this, it may suffice for my present purpose to mind you, that as for the mercuries or uninflammable spirits of vegetables and animals, I have endeavoured to show their production, where I discourse of that of spirits and volatile salts. And therefore I need but say something of the production of mercury more properly so called, that is, running mercury: about which perhaps it will not be less acceptable to you, and I am sure it will be less troublesome to me, if I leave you to gather my opinion out of three papers, that were written for differing virtuosi, at several times, and on distinct occasions; upon which account, besides those particulars that relate to our present argument, you will perchance find some things, that you have not elsewhere met with.

Whether Mercury may be obtained from metals and minerals, or (to speak chymically) An dentur mercurii corporum?

THAT there may be extracted or obtained from metals and minerals a fluid substance, in the form of running mercury, is the common opinion of chymists; in whose bodies we may meet with many processes, to make these mercuries: which because they are said to be afforded by mineral, and especially metalline bodies, these writers affect to call (how aptly I now examine not) *mercurios corporum.*

BUT notwithstanding all this, divers of the more learned of the Spagyrists themselves have looked upon the pretension of other chymists to the art of making these mercuries as but a chymical brag; and some judicious modern writers, applauded therein by most of the mechanical philosophers, have proceeded so far, as to explode all these mercuries of bodies as mere *non-entia chymica*; nay, some of them have not scrupled to censure all those, who pretend to have seen or made any of them, as credulous or impostors.

IN the management of this controversy, I confess I am not satisfied with either of the contending parties; and therefore I shall not refuse to comply with your curiosity to receive in a few lines my thoughts, whether there are or may be any such mercuries, as are disputed of; yet I desire leave to premise such a state of the controversy, as I think will avoid some verbal janglings, and at least acquaint you clearly with the sense, wherein I desire to have my opinion understood.

WAVING then, in the present inquiry, the question, that may occur, 'whether 'or no the mercury said to be obtained from metals and minerals are primitive in-'gredients, or hypostatical principles only extracted or separated from the bodies, 'that afforded them?' I shall propose the question in these terms: Whether or no 'from a metal or mineral body there may, without the addition of any body, that 'we may be sure has any common quicksilver in it, be obtained, by the help of art, 'a substance resembling common quicksilver, by being ponderous, fluid when actually 'cold, amalgamable with gold and some other metals, and indisposed to wet or stick 'to one's hand, or to bodies not of a metalline nature.'

To

To give you now my present thoughts about this question, I shall offer them to your consideration, in the following propositions.

There are divers processes of making the mercuries of bodies, that are so darkly delivered, that the generality of chymists cannot sufficiently understand them, to be able to try them. Prop. I. For some of these processes are set down in terms of art, which, for their great darkness or ambiguity, are not to be understood but by the authors themselves, or those, who are versed in the more mysterious parts of hermetic philosophy. And others there are of these processes, that require some menstruum, salts, or other instruments, that it is not in the power of ordinary chymists to procure. Instances of this kind may be frequently enough met with by those, that have the curiosity to peruse heedfully the writings of those, that pass for the adept philosophers. And for a specimen of such processes, I am content to annex to the close of this paper the way delivered by *Lullius* of making mercury of silver, *Helmont*'s way of preparing mercury of lead, and *Paracelsus*'s way of extracting the mercuries of all metals.

There are divers processes to make mercuries of bodies, that are either false, or accompanied with circumstances, that make them unfit to be trusted. Prop. II. For there are of these processes, that having been curiously tried by those, that had a great desire to find them true, have not been found to succeed at all in practice. Hence we have so many complaints of chymists, that have lost their labour in endeavouring to make, according to *Beguinus*'s directions in his *Tyrocinium Chymicum*, the mercury of silver, though I do not take that to be one of the difficultest to be prepared; and he, that converses much among those, that have made attempts to make the mercuries of other bodies, as gold, antimony, *&c.* according to the vulgar processes extant in chymical books, will (if I mistake not) find by their confessions, how little the events of their endeavours answer their labours and expectations. Nor do all the manuscript processes, that are communicated to private friends, as great arcana, much excel those I have been speaking of; as several of my acquaintance have complained to me, that they have found to their cost. And here not to mention my own experience (which by the help of good principles made me timely desist from unlikely attempts) amongst many chymists I have known, I remember not to have found above three or four credible persons, that would affirm to me, that they made, or saw made, the mercury of any metal or mineral (except of native cinnabar, which is the natural ore of quicksilver) in a constant way, by any process he had found in printed books: so that so many of these processes having been upon trial found false, wary men may be excused, if they do not think fit to believe other processes of mercurification; which, though not yet tried, seemed not more probable, than those, that have been already found so unsuccessful, that not only many learned modern naturalists, but *Angelus Sala*, and divers others, eminent chymists themselves, have published to the world, that these mercuries are to be found no where, but in the bragging chymists books and promises; and some have, as has been already intimated, gone so far as to brand all those for cheats, that pretend they can make such mercuries, and those for credulous, that believe they can be made. But what I think of this severe opinion, I shall quickly have occasion to declare.

THERE *are some processes, wherein it is thought, that the mercury of a metal or mineral is obtained; when indeed the obtained substance is misnamed, or the true mercury, that is said to be extracted, was put in, though in a disguised form, by the operator.* Prop. III.

I WILL not here give instances of the subtle cheats, that may be put upon the ignorant and unwary, and sometimes too upon the skilful, if they be not also cautious; but

but ſhall content myſelf to illuſtrate the propoſition by a few known, and therefore innocent, inſtances. And firſt there are ſome, who finding themſelves unable to make the true mercuries of metals or minerals, make bold to aſcribe the name of mercuries to productions, whoſe qualities are very remote from thoſe, that are agreed to be eſſential to quickſilver. Thus *Glauberus* ſpeaks much of his mercury of Luna, which yet is far from being running mercury, or having the ponderoſity and other properties of true quickſilver. So *Angelus Sala* himſelf, in his anatomy of antimony, would have us to look upon the reguline parts of that mineral, as its mercury: becauſe he takes it for granted, it muſt contain mercury, and is pleaſed to fancy no other can be obtained from it. But the difference of the reguline part of antimony, and running mercury in point of conſiſtence, gravity, and other qualities, will, I preſume, indiſpoſe men to confound them. And therefore, I will proceed to confirm the ſecond part of our propoſition, by ſhewing, that the mercury obtained by ſome proceſſes, that may ſucceed, made part of the additament employed by the artiſt, in the operations, and ſo was not properly extracted from the metal, but only recovered from the body, compounded of the metal and the additament. Of this, I remember, I have elſewhere given an eaſy inſtance, in a deluding experiment, that I long ſince ſhewed ſome virtuoſi, in whoſe preſence having mingled the filings of copper with a certain ſalt, and put them in a conveniently ſhaped veſſel of glaſs, I warily held it over a competent fire of well kindled charcoal, till the ſalt was thoroughly melted, and in part ſublimed: by which operation the copper ſeemed to be quite changed, eſpecially in colour, and was really become inflammable: and there remained in the lower part of the glaſs a pretty deal of running mercury, ſo that they would have gone away perſuaded, that they did ſee me make the mercury of Venus, if I had not been careful to undeceive them; which I did by telling them, that this quickſilver was only the common mercury, that lay diſguiſed in the compounded ſublimate I had employed, together with the copper, which ſet the mercury at liberty from the corroſive ſalts it lay concealed in before, by preſenting them a metal more diſpoſed to be wrought on by them, than quickſilver is.

Prop. IV. IT *is poſſible to obtain, at leaſt from ſome metals and minerals, true running mercury, that cannot be juſtly thought to come merely from the additament.* This propoſition a chymiſt might more compendiouſly expreſs by turning it into this ſhort aſſertion, *Dantur mercurii corporum*; but I thought the words I have employed would expreſs my ſenſe more warily and clearly; and yet, *ex abundanti*, I ſhall add this further explication, that though the propoſition ſpeaks affirmatively but of ſome metals and minerals; yet it does not deny, either that more minerals, or that all metals, may afford true running mercury: by which I underſtand (according to what I formerly noted) a mineral body fluid, opacous, exceeding ponderous; amalgamable with gold, and not apt to wet or ſtick to one's fingers, or any other bodies, beſides ſome metalline and mineral ones.

THAT ſuch a mercury may be obtained without the help of additaments, whereof quickſilver is an ingredient, I have been perſuaded to believe by the following obſervations.

I REMEMBER, that many years ago, having had an occaſion to diſtil copper with certain ſaline ſubſtances, I was not a little ſurpriſed to find in the veſſels (that had been luted together) ſome globules of running mercury, which I could not reaſonably ſuſpect to come from the additament, which was not ſublimate, nor any thing I could judge to contain quickſilver. And though the indiſpoſition I had to admit the mercuries

curies of bodies, that so many learned men looked upon as non-entities, made me somewhat diffident of the genuineness of the mercury I had obtained (whereof I had not quantity enough to make just trials) yet afterwards, when I found that accidents of the like nature had happened to several of my friends, I began to think, that what I had kept only for a few days as a questionable rarity, might really have been venereal mercury.

A LABORIOUS chymist of my acquaintance coming to visit me once, when I was not well, was very earnest with me to communicate to him the way of making the mercury of antimony and of Saturn; and when I told him, that I had no such processes of my own, and that I was far from believing those, that I had met with in printed books, to be true ones, he would not acquiesce in this answer; but declaring, that he resolved to make attempts to gain such mercuries, and had rather do it by methods of my proposing, than of his own devising, he pressed me so much to let him know, which way I would go to work, in case I had the same design, that he then had, that to be rid of his importunity, I told him what on a sudden came into my thoughts. And as sometimes the mind being put to such plunges, happens upon a lucky hit, and such as much premeditation would not have led it to; so it happened at that time to me: for when I, because of my distemper, had forgot this affair, the chymist, who was a plain honest man, came to me with great joy to give me thanks for the instructions I had given him, bringing along with him some mercury of antimony, and a little mercury of lead, that he had already made by the help of those instructions; by pursuing which, he expected to obtain much more mercury from the minerals when they should be longer digested with the concourse of the air, in those salts, that I had advised him to grind with them. This pleasing success of directions, which I had as to divers particulars forgotten, made me desire them of the chymist; who, beginning to be proud of his attainment, when he perceived I remembered not so much as he thought I did, ungratefully delayed to bring me the account he promised me at first, till the plague reaching to the place, where he lived, and dispatching him, deprived me of the hopes of satisfying my curiosity.

Two gentlemen of my acquaintance, but unacquainted with each other, working almost at the same time upon silver, did each of them, to his wonder, find parts of his silver turned into running mercury; with which odd accident each of them came to acquaint me, bringing along with them a little portion of the unexpected mercury. One of these portions a servant of mine lost by mistake, before I could try any thing with it; the other I found, by a trial unknown to the maker of it, not to be common, but metalline mercury; of which the chymist complained to me, that he had sometimes had considerable quantities to his great loss, because much of the silver he employed in an operation, that he expected would prove lucriferous, being turned into quicksilver, had swallowed up all his gain; and this was that, which invited him to apply himself to me, hoping to be able to prevent or remedy this inconvenience by my advice; which I willingly gave him, but, because of his departure out of *England*, could not know with what success.

A FELLOW-TRAVELLER of mine, having occasion to employ a saline body about lead, after he had finished his operation, left the lead and salt together for some months, in a vessel, which he laid by in a garret, where the air had access to it: afterwards wanting such a vessel, and not being able to supply himself readily in the country (in which his experiment was made) he remembered this long neglected vessel, and coming to see, whether it was fit for his turn, he found to his wonder,

that though he had employed no mercurial body to work upon the lead, yet part of it was already turned into quickſilver, ſeparable by ſtraining, and more ſeemed in a near diſpoſition to admit the like change: whereupon he brought me, as a rarity, a part of the metal, and a little of the mercury, which I found by experience on gold to be of a nobler kind than common mercury. And I the leſs wondered at this mercurification, becauſe examining the gentleman that chanced to make it, I found the main thing he had employed in the operation was common or ſea-ſalt.

An expert metalliſt of my acquaintance, being deſirous to try, what gold and ſilver he could get out of a fine Engliſh marchaſite I had preſented him at his deſire, he examined it according to his method, without any mercurial preparation, and found to his ſurpriſe, that it yielded him, beſides other things, ſome running mercury, which he brought and gave me, becauſe it was afforded by the marchaſite I had preſented him.

The mercury of antimony more than one of my friends have made, by unſuſpected additaments, ſuch as ſalts, that have nothing to do with ſublimate, or other compoſitions, whereof common mercury is an ingredient. One of theſe antimonial mercuries looked ſo oddly, that, though it were made by diſtillation, I had that curioſity to try, whether it would not operate on gold, in a peculiar manner; and having accordingly put a little fine calx of that metal (as about half a drachm or a drachm) into the palm of my hand, I added to it an equal or double weight of the above mentioned mercury; they immediately incorporated with a very manifeſt heat. And this was the quick way I uſed to examine other mercuries of bodies; for though this alone be not a certain ſign of a mercury's being of that ſort, becauſe I can obtain a mercury ſo qualified by another way than any I have hitherto mentioned; yet as their aſſertions and relations gave me ſufficient ground to conclude, that they had obtained thoſe mercuries from the bodies, that they affirm to have yielded them; ſo the readineſs of theſe mercuries to mix with gold, without the help of fire, and even to grow hot with it, which vulgar mercury will not do, confirmed, that they were metalline mercuries, rather than of the ſame kind with common quickſilver. And my way of obtaining incaleſcent mercury is ſo quite differing from any of thoſe, that there was not the leaſt cauſe to ſuſpect, that the mercuries of bodies we have been mentioning were ſo obtained, eſpecially ſince I knew, that my way was unknown to moſt of the perſons I have mentioned, and was practiſed by none of them.

As for the mercury of gold, though I think I have brought a great many parts of crude gold to aſſume a mercurial form, and to come over in that form by diſtillation (whatever divers learned men think of the inſuperable fixity of gold;) yet I confeſs I have not ſeen any mercury, that I was ſatisfied did deſerve the name of the mercury of that metal. But happening to be once in a place, where a foreigner, that was a ſtranger to me, was ſhowing a friend of his, with whom I had ſome little acquaintance, a metalline experiment, that I confeſs, I could not but admire; (for this foreigner was ſo civil, becauſe I came ſo lucky in, as to let me be preſent at the experiment, though not to diſcover any thing of the drug he employed about it;) I made bold to aſk this civil traveller, who ſeemed a candid man, and I perceived had ſeen uncommon things, whether he had met with any way of making mercury of gold? To which he anſwered, that he knew no ſuch way himſelf, but that he met (ſomewhile before) with a very learned man, in compariſon of whom he confeſſed himſelf but a novice, that put ſome gold into a little phial, full of a certain menſtruum, which my relator owned he knew not how to prepare, and intimated to him, that this menſtruum would have a peculiar operation as well upon gold as ſilver.

Afterward this relator having put the phial well ſtopt into his pocket, and carried it about with him, was, when he came home, and took it out to ſet it aſide, much ſurpriſed to find, inſtead of the gold he had ſeen put in, a pretty quantity of running mercury. Which the artiſt, who only lent him the menſtruum, did not ſeem to think ſtrange, when he was made acquainted with it.

IF I would relate what I have heard from men, that I judge to be either eaſily deceivable themſelves, or concerned in point of intereſt to deceive others, or at leaſt of a vain-glorious bragging humour, I might eaſily ſwell this diſcourſe to a greater bulk; but I have been careful to mention only thoſe relations, to which myſelf, in ſpite of my long backwardneſs to believe ſuch things, ſaw cauſe to give aſſent. And if it be objected, theſe inſtances were but caſual experiments, notwithſtanding which there may be no ſettled way for the obtaining the mercuries of bodies; I might anſwer, that ſome paſſages of what has been lately delivered make it probable enough, that even ſettled ways of making the mercuries of bodies, or at leaſt of ſome of them, are not unknown to ſome artiſts; though for certain reaſons, and particularly for the ingratitude of many men, they do not think fit to divulge them. But to anſwer more home to the objection, I ſhall need only to ſay, that though moſt of the above-recited experiments may be ſaid to have been made by chance, in this reſpect, that thoſe, that made them, did not principally deſign the obtaining of metalline or mineral mercuries; yet the effects produced were as naturally and neceſſarily conſequent to operations ſo managed as they were, as if the artiſt had directly deſigned them, as in ſome of the above mentioned relations they did. And it is not material for us to enquire, whether the quickſilver made by thoſe experiments be to be aſcribed to chance or ſkill; ſince whatever becomes of that queſtion, it is plain, that if metals and minerals could by either way be brought actually to afford running mercury, there needs no more to prove, that ſuch mercuries are really obtainable from them.

Doubts about the pre-exiſtence of running Mercury in metals.

THE propoſed queſtion, Whether or no the mercuries of metals and minerals be principles pre-exiſtent in them, and only extracted from them, may to many ſeem, though it do not to you, a ſuperfluous enquiry; ſince the generality of chymiſts, of differing ages and countries, have reſolutely determined it in the affirmative: which is not at all to be wondered at, ſince according to their hypotheſis of the *tria prima* (or three hypoſtatical principles) whereof they preſume all perfectly mixed bodies to be compoſed, metals, being of this ſort, muſt conſiſt of mercury, as well as of ſalt and ſulphur; and conſequently muſt afford it upon the analyſis of the body into its three primordial ingredients. But notwithſtanding all this, the problem ſeems to me difficult enough to be reſolved, partly becauſe ſuppoſing, that there be true metalline mercuries preparable by chymiſts, they very ſtudiouſly conceal the ways of preparation; and partly, becauſe as it is very difficult to obtain any of the factitious mercuries, wherewith to make ſuch lucriferous trials, as a naturaliſt would deſign; ſo thoſe few authors, that affirm themſelves to have poſſeſſed ſuch mercuries, have given us but an exceeding lame and defective account of them, not mentioning thoſe particulars, which are moſt proper and deſirable, in order to the paſſing a right judgment about them. I pretend not therefore to anſwer your queſtion otherwiſe than conjecturally, till I ſhall be better furniſhed with matters of fact. But in the mean while, that I may comply with your curioſity, as much as I ſafely

can, I shall confess to you, that for the present I am, by as much information as yet I have had, inclined to think, that the mercuries obtained from metals do not clearly appear to have been pre-existent in them, and only separated from them by the artist, but that I think, that at least some of them may be further fluid magisteries of metals, than their extracted principles. One of the most obvious things, that suggested this suspicion to me, was, that whilst some metals, as tin and lead, are in fusion, they would, to one, that should not know of their being melted, appear to be many parcels of mercury; since like it they are fluid and ponderous, and stick not to crucibles, or stones, bricks, or almost any other bodies, except some metalline ones, divers of which they will easily pierce into, as quicksilver does into silver or gold; so that if the fluidity of these metals were permanent, they might pass for mercuries. And if in the moon and some of the other stars, as there are mountains, so there are metalline mines, in case the heat of the climate or of the soil should keep them constantly in such a degree of heat, as we here find sufficient to melt lead (which we know needs not be very intense) these metals would there emulate the nature of mercuries, as I have learned from travellers, that in divers parts of the torrid zone, what would here be butter, is fluid as well as unctuous like oil, and is sold like other liquors, by measure, not by weight. And an inquisitive man, who is a scholar as well as a traveller, assured me, that whilst he was in some parts of the *Indies*, he furnished himself with some liquid substances afforded by wounded plants, that as soon as he came near *Europe*, and not before, turned into consistent and pulverable bodies; it did not therefore seem to me impossible, that the piercing salts, and other subtile bodies, employed by artists about the mercurification (as some stile it) of metals, may either by the agility of their own nature, or by so altering the shapes, and loosing the wonted cohesion of the metalline corpuscles, bring them to have such a texture and such pores, as may enable the ethereal substance, whereto so many other bodies owe their fluidity, to agitate them. These causes, I say, or some other, that may be proposed, may possibly keep the prepared metal fluid; as we see, that though camphire be a consistent and tough body, yet some nitrous spirits of aqua fortis will easily penetrate it, and may be brought to stay so long with it, that I have, for curiosity-sake, kept the camphire several years without loss of its fluidity, which I found that this kind of liquor would retain, though for trial-sake I exposed it to intense degrees of cold, such as would freeze divers other liquors. Nor did it to me seem impossible, that a small quantity of appropriated additament might suffice to put a metal into a state of fluidity; for since we see, that the vapour of lead can arrest quicksilver, and make it a consistent body; and since *Helmont* assures us *, that the liquor alkahest being once abstracted from running mercury, deprives it, and that almost irrecoverably, of its fluidity, so as to make it pulverable; it appears not, why nature or art may not be able to supply some corpuscles, that may expel or disable those, that keep a metal in the form of a fluid body; and especially since, as I have elsewhere shewn, the matter of metals themselves may (at least sometimes) have been a liquor, or some other fluid body.

ANOTHER reason, that induced me to suspect, that the mercuries of metals and minerals are not, as it is presumed, merely extracted principles or ingredients, was, that I have observed a greater dissimilitude between mercuries all of them quick, and

* *Est scilicet (Corallatus Paracelsi) Mercurius, a quo liquor Alkahest semel distillatus est, residetque in fundo coagulatus & pulverabilis, nequicquam in pondere auctus aut diminutus.* Helmontius in Scripto de Arcanis Paracelsi.

furnished

furnished with all, that is requisite to make them pass for true mercuries, than will comport with the supposition, that they are simple and primordial bodies, barely extricated from the others, with which they were at first commixed. But this argument being the subject of an entire, though short, discourse (of the dissimilitude of running mercury) I need not inlarge on it in this place.

It did also strengthen my suspicion to consider, that the chymists, that talk of the mercuries they have drawn from metals, are not wont to tell us, what other ingredients they obtained by their supposed analysis, which left me dubious, whether they obtained any salt and sulphur, or not; and of what nature those substances were, that they did obtain. For if these were not true salt and sulphur, the genuineness of the analysis might be questioned; because it may be alleged, that the chymical operation and the additament turning some parts of the metal into decompounded bodies, which must be acknowledged not to have been (in such) forms preexistent in them, may also have by change of texture turned some other parts of the metal into the form of mercury.

To the foregoing considerations, afforded me chiefly by the nature of the thing, I shall, for the sake of those, that are moved by the authority of adept philosophers, as they call them, add that, which among them ought to pass for a proof from experience. For *Raymund Lully*, whom I take to be one of the greatest chymical philosophers, whose writings are come to our hands, though in many of his other books he speaks of mercury in a dark and allegorical sense; yet in that excellent little tract, which he calls his *Clavicula*, he delivers a process (which is not to be wrought with vulgar menstruums, though they bear the same names with those he prescribes and names) from the close whereof it seems manifest, that his design was not to extract a pre-existent quicksilver out of the metal proposed, but to turn the metal into quicksilver; since he orders and directs us to prosecute the mercurification, till the obtained quicksilver be equal in weight to the silver, that was to be transmuted. And partly upon this I have ventured to ground the foregoing paradox; that the mercuries of bodies are rather magisteries than extracts. For in this Lullian process, it appears not, that the mercurial principle was extracted from the salt and sulphur, but rather that the body of the metal (without being analyzed) was turned into mercury. And though magistery be a term variously enough employed by chymists, and particularly used by *Paracelsus* to signify very different things; yet the best notion I know of it, and that, which I find authorized even by *Paracelsus* in some passages, where he expresses himself more distinctly, is, that it is a preparation, whereby there is not an analysis made of the body assigned, nor an extraction of this or that principle, but the whole, or very near the whole body, by the help of some additament greater or less, is turned into a body of another kind. As when iron or copper by an acid menstruum, that corrodes and associates itself with it, is turned into vitriol of Mars or of Venus; and quicksilver having a sufficient quantity of aqua fortis strongly abstracted from it, is changed into a red precipitate; or by being sublimed up with common sulphur, is turned into cinnabar; or, to give yet a more apposite example, when quicksilver (which is the body we treat of) is by the lasting operation of the fire, without external additaments, at least distinct from the igneous particles, turned into a red powder, that chymists call *Præcipitate per se*.

Vide Lullium in Clavicula, cap. 2.

I have received credible information, and some proof too, that there is a place in *Transylvania*, where portions of running mercury, which, when they fall out of the earth, and lie a while in the air, do of themselves coagulate into permanently hard bodies: so little a distance hath nature herself there put between the mercurial fluidity,

dity, and the ſolid conſiſtence of the ſame portion of matter. So that if ſo ſmall a thing (and perhaps unponderable as well as inviſible) as the contact of the air can expel, is able by its preſence to retain a mineral body in the form of a true running mercury, as well as by its receſs to leave a ſolid body; I ſee not why it ſhould be impoſſible for art, to intercloſe ſome very minute and reſtleſs particles, which, by their various and inceſſant motions, may keep a metalline body in the ſtate of fluidity, much after ſome ſuch way, as I lately noted, that the ſpirits of nitre did, for whole years together, keep camphire in the form of a liquid oil.

HAVING now propoſed ſome of the conſiderations, that inclined me to think, that the mercuries obtained from metals and minerals may not have been pre-exiſtent in them; the impartiality, that, I think, becomes a naturaliſt, obliges me to take notice alſo of thoſe things, that occurred to me, in favour of the received opinion of the chymiſts, in behalf of which I objected to myſelf divers ſpecious arguments.

OF theſe, the firſt was the general conſent of chymiſts, who take it for granted, that all metals are compoſed of mercury as a material principle, and commonly more copious, than any other conſtituent part of thoſe bodies; but this being an argument drawn only from authority was of ſmall weight with me, in a controverſy properly determinable by reaſon and experience.

2. A SECOND objection was afforded me, by many proceſſes I had met with in chymiſts books, to extract the mercuries, as well as the ſulphurs and ſalts of metals. But neither did this argument appear to me of any great moment, for moſt of theſe proceſſes I looked upon as fictitious things; which, if the authors of them had taken the pains to try themſelves, they would have found not to ſucceed in practice; and ſcarce any of them was ſo ſkilfully framed, as to ſatisfy a conſidering naturaliſt, in caſe it had ſucceeded, that the obtained mercury was a pure principle only ſeparated or extracted from the other ingredients of the metals, and not a reſult of ſome metalline parts conjoined with ſome parts of the additament; as it ſeemed manifeſt enough to me, that the ſuppoſed ſalts of metals, that are pretended to be made by ſuch preparations, are not the principles of ſuch metals, but new concretions, and indeed not ſimple, but decompounded bodies; as is evident in the ſalt or ſugar of lead made with the ſpirit of vinegar, and in the ſalt of ſteel made with that, or other acids.

3. A THIRD objection, and of greater weight, ſeemed derivable from this conſideration, that quickſilver eaſily amalgams with metals, becauſe of its cognation with the mercurial part theſe bodies abound with.

4. AND this argument appeared capable of being ſtrengthened by a more conſiderable one; which is, that the gravity of the metals is ſuch, as cannot reaſonably be deduced from any other cauſe, than an abundance of the mercurial principle, there being no other bodies (known to us) beſides quickſilver, that are near ſo ponderous as metals.

THESE two objections I thought fit to couch together, to be able, in fewer words, to anſwer them both: I conſidered then, that amalgamation being, in effect, but a kind of diſſolution of metals in a menſtruum or fluid body (for ſuch mercury is, in reference to them) there is no neceſſity, that the ſolvent ſhould find in the metal a copious ingredient juſt of its own nature; for diſſolution depends not ſo much upon the pretended cognation between the ſolvent and the body it is to work on, as upon the congruity, as to ſize and figure, between the pores of the latter and the corpuſcles of the former; as may appear by the ſolution of ivory and hartſhorn (which belong to the animal kingdom) that may be made with aqua fortis; and by that,

that, which I have elſewhere ſhewn may be made of zink, and even of copper, by the ſpirit of vinegar, the urinous ſpirit of ſal armoniac, and ſpirit of vitriol ſeparately employed; though the firſt of them be a menſtruum drawn from a vegetable, the ſecond from an animal, the third from a mineral ſubſtance. And as for amalgamations themſelves, I obſerve, that the facility mercury finds in joining with a metal, does not barely depend upon the plenty of the mercurial ingredient, contained in the metal; at leaſt if the greater ponderoſity, or ſpecific gravity of the metal depend upon the copiouſneſs of the ſame mercurial principle, or ingredient, as the fourth objection ſuppoſeth: for we find by experience, that mercury will far more eaſily amalgam with tin than with copper, which, yet, is much more heavy than it; nay, than with ſilver, which is a good deal heavier, (in ſpecie) than copper; and is by chymiſts preſumed to be much nearer of kin to mercury than is tin. To which I ſhall add, that although Mars be ſpecifically heavier than tin, yet it is far from being more eaſily amalgamable with mercury; that though tin will readily admit this mineral liquor, without the help of heat, there is no way vulgarly known to chymiſts to make an immediate amalgam between mercury and Mars. So that one of the two objections, I lately joined together, muſt be declined: ſince by the trials I have purpoſely made, it appears, that either the diſpoſition of metals to amalgamate with mercury does not barely depend upon the ſuppoſed plenty of mercury contained in the metal; or elſe that the greatneſs of the ſpecific gravity does not depend upon the more plentiful participation of that mercurial ingredient. Although the fourth objection be built upon a ſuppoſition, that the great ponderouſneſs of metals, in compariſon of other bodies, can proceed from no other cauſe than the great quantity of mercury they contain; I conſidered too, that it might be juſtly demanded, whence mercury itſelf, as well as whence metals, derive their great ponderoſity; and I ſee not, why it may not be ſaid, that both the one and the other owe it to the ſolidity, and cloſe order, of the corpuſcles they conſiſt of; to which qualification it is not eſſential, that the portion of matter endued with them be in a ſtate of fluidity, rather than in one of conſiſtence: as on the contrary we ſee, that gold and lead are exceeding ponderous bodies, as well when they are in fuſion, as when they are cold and hard; and ſo is quickſilver, as well in its wonted and liquid form, as when it is coagulated, as chymiſts ſuppoſe, by the vapour of lead.

BUT this will be ſomewhat further cleared in what I ſhall ſay to the fifth and laſt objection, that my thoughts ſuggeſted to me, and which philoſophical candour forbids me to conceal; though I find it eaſier to be propoſed than anſwered. It may be then alleged in the fifth place; that the mercuries of metals muſt needs be but partial principles of them, ſince quickſilver being confeſſedly heavier than either the ſulphureous or ſaline principle, and being ſpecifically heavier, than almoſt any metal itſelf; the gravity of a metal cannot reaſonably be ſuppoſed to proceed from the whole body of the metal, but only from ſome one ingredient heavier in ſpecie than the reſt, and, than the metal itſelf. And this ingredient or principle can be no other, than the moſt ponderous body, mercury.

THIS difficulty, I confeſs, does keep me yet in ſome ſuſpence, till I have further opportunity, to make ſuch trials, as I think proper to clear it. Yet in the mean time, I ſhall offer ſome few things, which perhaps may leſſen it, if not quite remove it.

I CONSIDER then, that there is no neceſſity to ſuppoſe, that metals, of what denomination ſoever, as tin, iron, ſilver or gold, are bodies perfectly homogeneous, though they ſeem ſuch to our eyes. This ſuppoſition I elſewhere purpoſely diſcourſe of,

of, but in this place I need not ſpend time about it; ſince the chymiſts (who are thoſe I now reaſon with) do not only allow, but teach it, ſince they will have metals as well as other mixt bodies to conſiſt of three hypoſtatical principles, whereof mercury is one, although it muſt be much heavier in ſpecie, than either the ſalt or the ſulphur it is blended with; becauſe it is from the participation of that ingredient, that they derive the great ponderouſneſs, which metals have, in compariſon of other bodies.

And to this granted ſuppoſition, I ſee not why it ſhould be abſurd, to add this other, that the more ſolid and heavy particles or corpuſcles of metal may lie in it, not in the form of fluid or mercurial, but conſiſtent parts, and that theſe may be more diſpoſed than the reſt, to be brought by chymical additaments and the operation of the fire into the form of a running mercury. Nor ought it to be judged incredible, that the fore-mentioned ſolid portion of the metal ſhould be more ponderous than quickſilver, ſince, as I have often tried, gold, though a conſiſtent body, is far heavier than quickſilver, to the bare participation whereof gold cannot owe its ſpecific gravity.

If this hypotheſis be admitted, it will be eaſy to give an account, how the mercury of a metal may be heavier in ſpecie (that is, bulk for bulk) than the metal that afforded it; for the difficulty is eaſily reſolved, by ſaying, that the ſolid parts, which by the chymical operation are reduced into the form of quickſilver, were far more ponderous in kind, than the other parts of the metal, which being alſo aſſociated with them, did, by their comparative lightneſs, make the entire metal leſs heavy (if the bulks be equalled) than an aggregate or convention of all the ſolid parts alone would have been. Which may be illuſtrated by what I have heedfully obſerved, of the decrement of ſpecific gravity, ſuſtained by quickſilver, when it is united by ſublimation either with ſulphur into cinnabar, or with ſalts in corroſive ſublimate.

But I muſt not diſſemble, that againſt the foregoing diſcourſe there occurred to me a couple of arguments (that I have not met with amongſt chymiſts) whereof the latter is very conſiderable For I foreſaw it might be alleged, firſt, that the mercuries of metals being in a liquid form, could not well be ſuppoſed to be ſo cloſe and ponderous bodies, as our hypotheſis requires: and next, that we ourſelves admit an experiment of *Raymund Lully*, whereby he pretends to reduce the whole body of ſilver into mercury, which is a heavier ſubſtance than ſilver; and in this caſe we cannot have recourſe to this anſwer, that the corpuſcles, which aſſume the form of mercury, were far more ponderous than the others, that concurred with them to compoſe the metal.

This twofold objection I do not pretend to anſwer at once, but may perhaps enervate it by degrees.

And firſt, though it be very poſſible, that a pretty quantity of additament may be employed about the mercurification (to ſpeak in the chymiſts language) of a metal, yet there ſhall really and finally adhere to the metalline parts, but a very ſmall proportion of additament, that will continue with them, and keep them in a mercurial flux. And it may appear the more credible, that a very ſmall quantity of additional matter may have a very great ſtroke in altering the conſiſtence of that, which is obtained from a metal, as its moſt ponderous portion, if you conſider with me, that the bare acceſſion of igneous particles, is able in time, to turn running mercury into that red powder, which chymiſts call *Præcipitate per ſe:* and I have found by trials purpoſely made, and elſewhere related, that this powder without any further additament may be reduced into running mercury. Nor muſt I pretermit, on this occaſion, a notable

notable paſſage I remember to have met with in *Helmont*, who relates, that by the abſtraction of the liquor alkaheſt (which is wont to come all over in diſtillation from common quickſilver) he did quite deprive it of its fluidity, and turned it into a conſiſtent body, and even into a fixed one; whereby you may ſee how little a quantity of matter will ſerve to change the conſiſtence of a body of a mercurial nature.

BESIDES that, a fluid form does not always argue the lightneſs of the body, that it is found in, ſince it may conſiſt of particles ſo ſolid and ſo numerous, that, notwithſtanding their inteſtine motion, the body they compoſe may be very ponderous: as may appear by red-hot iron, melted lead, and, which is an inſtance appoſite to our purpoſe, in common quickſilver, which though fluid is heavier than any known body in the world, gold excepted.

BUT I conſider farther, that though the ſolid portion of a metal retain more of the additaments employed to bring it into the form of mercury, than it can be proved to contain; yet this diſadvantage may be compenſated by the new diſpoſition of parts, that the mercurified portion acquires by the operation, that turns it into a liquor, and may be ſuppoſed to bring the parts to a cloſer, or otherwiſe a more expedient order than they were in before: as ice when thawed takes up leſs room in the form of water, than it did before it became a liquor. I ſee no impoſſibility, that the ſpecific gravity of metalline bodies may be increaſed or diminiſhed by ſuch ſmall proportions of additaments, as do not at all conſiderably add to their abſolute gravity. This the chymiſts ought not to deny, if they conſider what themſelves grant of the efficacy of what they call the philoſophers ſtone, whereof they tell us, that one grain, if it be of a nobler order or degree, may tranſmute a whole pound of quickſilver into perfect gold and conſequently the ſpecific gravity of a metal is notably changed by an additament; which (according to the differing pounds uſed in ſeveral countries) amounts not, perhaps to the ſixth or ſeventh thouſandth part of its weight. Beſides, the tranſmuting powder being a compounded body, whereof but part is gold, may probably be ſuppoſed to be more light in ſpecie than the metal, that by addition of it is produced; which being pure gold, is the ponderouſeſt body yet known to us. And to confirm the argument, I ſhall add, that there is a way, though I pretend not to know it, of making a metal far lighter in ſpecie, than it naturally is, by the addition of leſs than a hundredth part of its weight, as experience has convinced me.

WHEREFORE to come now to the grand objection furniſhed by *Lully*'s lately-mentioned experiment, it will not preſently follow, that if the whole body of a metal be brought into a mercurial form, this mercury will ſwallow up and deſtroy our hypotheſis: for though I grant, that, in this caſe, it cannot be ſaid, as in the former caſes (wherein a part only of the metal is mercurified) it may be, that the obtained quickſilver conſiſts of the more ſolid and ponderous parts of the metal; yet it may be ſtill ſaid, that, for aught we know, the mercury, produced by the reduction of the whole metal into a fluid form, may be ſpecifically lighter than common mercury, and ſo cannot be neceſſarily concluded to be ſpecifically heavier than the metal, that afforded it. I lately employed the words, *for aught we know*, becauſe we are now upon the caſe, wherein philoſophical candour invited me to acknowledge, that I wanted further trials to give myſelf full ſatisfaction: for although I have had portions of the mercuries of more than one or two metals, yet it was but in ſmall quantities; ſo that the other trials, I had the curioſity to make with them, kept me from examining their ſpecific gravity, and from finding by an hydroſtatical way, that I have elſewhere declared, whether they were not lighter in ſpecie than inferior metals, and conſequently than common mercury. For that quickſilver may be ſpecifically lighter

than the metal that affords it, I think the chymists cannot reasonably refuse to grant; since they allow, that running mercury may be obtained of gold, and tell us great matters of it, because of its proceeding from so noble a body. Now if this golden mercury be said, because of the supposed resemblance of all mercuries, to be of the same specific weight with common quicksilver, then I have a notable instance of a mercury, that is considerably lighter in specie, than the metal, that afforded it. And therefore, till experience have manifested the contrary, it will not be absurd to presume, that the mercuries of other metals may likewise be lighter in specie, than the respective bodies, from which they were obtained: but if it be said, that this golden mercury may perhaps be as ponderous as gold itself or even more, then it is plain, that it is possible for a metalline body, notwithstanding its being reduced into the form of a fluid, to be equiponderant to the metal, that afforded it. And that I may not seem to argue altogether from the concessions the chymists ought to make, I will add, by way of confirmation, a couple of things, that perhaps you will think somewhat strange. Whereof the former is, that it is possible for a metalline body to resemble another in all the manifest qualities, whereby artists are wont to examine them, and yet they differ much from it in specific gravity: as I had once opportunity to observe in a metal, that was not only white (within and without) like silver, and very malleable, but did, when I purposely examined it, endure cupellation, and passed for, and was reputed by a very eminent artist, that sent it me to examine, to be good silver in all proofs; and yet this metal I found by hydrostatical trials to be much lighter in specie, than common silver. And if the famous person, that sent it me, was not mistaken (for so I must not think he would knowingly misinform me) this odd metal may yield me a notable instance to my present purpose, since he affirmed this metal to be made without the addition of any metalline body of quicksilver; which, if this be so, must, by a change of texture, have made a considerable loss of its specific gravity. But to proceed to my second instance, which will be yet more opposite; I shall add, that once I had a mercury, which amongst other remarkable properties, that belong not to this argument, had a very strange one; namely, that it was considerably heavier in specie than common mercury (as I found and shewed to a great virtuoso by hydrostatical trial) though it was made of a body no heavier than common mercury, and by the help of additaments, which were much lighter than common mercury. And this was so far from being a more gross and sluggish kind of quicksilver than the ordinary, that it looked very fine, and was very agile, and had, before I examined it, been more than once distilled. By this it may appear, that from hence, *that a body is in a mercurial form*, we cannot safely determine what degree of specific gravity it has. For since by this last example it appears, that a sort of quicksilver may be far more ponderous than common quicksilver; it seems not unreasonable, that a sort of quicksilver may be far lighter than common mercury, and so perhaps lighter than the metals, that were reduced into that form: it being far less likely, that the former should be produced than the latter, in regard there is but one mineral body in the world, that we know of, at all heavier than common quicksilver; whereas there are many of those, that are capable of being associated with it, that are far lighter than it.

But, as I intimated above, I am unwilling to speak so positively about this matter as I might do, if I had opportunity to make the trials I would with the mercuries of bodies: only thus much I shall venture to say, that, for aught yet appears, the argument, I have been answering all this while, is not cogent, since it is built upon a supposition, that the mercuries afforded by metals and minerals must be of the same weight

weight with common mercury; which is not only a proofless assertion, but repugnant to the experiment lately mentioned of the distilled mercury, that was heavier than common mercury, and to the presumption derived thence, that there may be bodies, in a mercurial form, more light in specie than common mercury. And whatever becomes of the opinion I incline to; the argument I have been examining of the chymists, may be invalidated by what I have said, where I took notice of the notable excess of ponderosity, that pure gold has in regard of common quicksilver: for by that instance it plainly appears, that it is not to the participation of common mercury, that metals must necessarily owe their great ponderosity, but that nature (and art too) may contrive the parts of a body into so close an order, as to make that body (whether solid or fluid) more ponderous, bulk for bulk, than common quicksilver itself.

HAVING now dispatched what I intended to say in the foregoing discourse, it remains, that I perform the promise I made, of adding the ways of mercurification (as chymists speak) above referred to, as delivered by *Paracelsus*, *Helmont*, and *Lully*: about which I must give you this advertisement, that besides the obscurities, and imperfections, that a moderate degree of attention may enable you to discover in these processes, understood in the literal sense, there are, if I much mistake not, some affected equivocations in terms, that seem very plain, and free from suspicion of ambiguity. As for instance, though the word *sal armoniacum* seem to be of this sort, yet amongst Hermetic philosophers it often signifies, not common sal armoniac, which is far from being able to perform the effects they ascribe to theirs, but a very differing and much more noble and operative thing; which, because it may be sublimed like common sal armoniac, they are pleased to call by that name. And though sometimes they give it the title of *sal armoniacum philosophorum*, yet oftentimes they omit the discriminating epithet, especially in philosophical processes (that is, such as those, wherein they deliver their higher arcana) of which sort are many of *Paracelsus*'s processes, and more (not to say most) of *Lully*'s. What is meant by *sal armoniacum philosophorum*, I think it needless to tell you here (but may perchance do it on another occasion;) since that composition requires an ingredient, that neither of us is furnished with, and that you cannot procure. There may be other ambiguities in the following processes, that will not be easily discovered, but by such as are versed in the mysterious language, which some would call canting, of the Hermetic philosophers. But, I think, I have said enough already to shew, that the annexed processes are fit to confirm, what is delivered upon the first proposition of the foregoing discourse; and therefore without offering to explain them, I shall subjoin them in the proper terms of the respective authors.

Ratio extrahendi ex omnibus metallis mercurium Paracelsica.

Hæc extractio (scilicet mercurii ex metallis) fit per aquam mercurialem, quæ nec *Johanni de Rupescissa*, nec aliis, quicquid etiam jactitent, cognita fuit. Ideoque diligenter est cognoscenda, & indefesso labore tractanda. Hoc ergo pacto paretur dicta aqua mercurialis.

℞. ℔ iii. *Mercurii sublimati septies per vitriolum, sal nitri, & alumen.*

SALIS armoniaci, ter à sale sublimati & clari & albi ℔ß. *Trita simul & alcalizita sublima in sublimatorio per arenam, horis* 9. *Ubi refrixerit, sublimatum cum penna detrahito, & cum reliquo sublima, ut prius. Hanc operationem quater repete, donec amplius non*

non sublimetur, & in fundo massa nigra maneat, instar ceræ, fluens. Refrigeratum exime, & tritam rursus in patina vitrea sæpius cum salis armoniaci aquâ, s. autem præparata, imbibe, & sua sponte coagulatam rursus imbibe & sicca, ad 9. *seu* 10. *usque vices, donec ferè non ulterius coaguletur. Tritum subtiliter supra marmor in loco humido solve in oleum pulchrum, quod rectificabis per distillationem in cineribus, ab omnibus fæcibus & residentia. Hanc aquam omnium facile principem diligenter asservabis, cujus* ℞ *unc.* viii. *& impone laminas opt. solis ac lunæ, optimè mundatas, pondere unc.* iß. *vitro clauso repone ad digestionem in cineres calidos horis* 8. *Corpus tuum videbis in fundo vasis transmutatum in subtilem vaporem seu mercurium. Factâ solutione totius aquæ mercurialis per alembicum lento igne à prima materia sublimando separa, & in vitreo vasculo diligenter asservato. Habebis hoc pacto verissimum mercurium corporis.* Paracelsus in Man. de lapide Philosophorum.

SENSI (says he) *cruditatem Saturni, pinguedine fixorum salium solubilem, solo quandoque igne carptim delebilem, sicque dividi compositi partes, crudumque argentum vivum currere permitti. Sulphur fugitivum superans in Saturno trahere ad volatum, fixum, inseparabiliter junctum. Quodque expediet imprimis Saturni sublimatio. Cujus expressione nulla est elevati ad residens coloris aut substantiæ differentia. Unde etiam caloris, fusionis, & mollitiei causis, post calcinationes & redactiones, residuis medullitus, sine igne fusionem, solitamque mollitiem minimè refutat.* Helmontius, in Potest. medicaminum. num. 40.

Extractio mercurii à corpore perfecto.

REC. unc. i. *calcis lunæ appellatæ, calcinetur modo quo dicitur in fine nostri magisterii operis, quæ quidem calx teratur super porfidum in pulverem subtilem, quem pulverem imbiberis bis, ter, quater in die, cum optimo oleo tartari, facto eo modo, quo dicetur in fine nostri, deficcando ad solem quousque dicta calx absorbuerit de dicto oleo* 4 *aut* 5 *partes, plus quàm fuit ipsa calx, terendo semper super porfidum, ut dictum est, & in fine bene deficcetur calx, ut benè possit in pulverem redigi. Et quando fuerit bene pulverizata, ponatur in metreto cum collo longo. Ponatis de nostro menstruali fætenti, facto de duabus partibus vitrioli rubei, & una salis petræ, & prædictum menstruum prius distilletur septies, & bene rectificetur, separando fæces terrestres in tantum, quod prædictum menstruale sit totum essentiale. Deinde lutetur benè metretum, & ponatur ad ignem cinerum, cum parvo igne carbonum, quousque videris materiam bullire & dissolvi. Deinde sic supra cineres distilla, donec amiserit menstruum, & materia fuerit frigida totaliter: cum frigidum fuerit, vas aperiatur, & materia, quæ frigida est, ponatur in alio vase bene mundo, cum sua cappa bene lutata ad furnum supra cineres, & luto bene disiccato, fiat ignis paulatim in principio, quousque totam recipias aquam ipsius. Postea augeatur ignis, quousque materia bene fuerit deficcato, & spiritus fætentes sint ad cappam; & in receptorio jam exaltati. Et dum tale signum videbis apparere, dimittatur vas infrigidari, ignem minuendo. Et post refrigerationem vasis, extrahatur materia, & in pulverem subtilissimum redigatur super porfidum; ita quod pulvis sit impalpabilis, qui ponatur in vase terreo bene cocto & bene vitreato. Et post ponatur super dictum pulverem de aqua communi bulliente, movendo semper cum baculo mundo materiam, usque materia fuerit spissa sicut sinapi. Et move dictam salsam cum baculo, quousque videris apparere grana mercurii è corpore, & quod vobis appareat quantitas magna prædicti mercurii vivi, secundum quod posueris de corpore perfecto, id est, de luna, & dum habueris magnam quantitatem, interim projice desuper aquam bullientem; & tandem movendo, quoad tota materia resolvatur in materiam similem argento vivo vulgari, toliantur terrestreitates cum aqua frigida, & deficcetur per pannum: postea transeat per corium, & videbis mirabilia.* Lullius in Clavicula, Cap. 2.

Of

Of the dissimilitude of running mercury.

BEFORE I undertake to give you my opinion about your question, I must crave leave to state it somewhat more clearly, by propounding it thus: whether all the bodies, that in the shops, and among chymists, pass for true running mercuries, are homogeneous; or so much of one and the same nature, that the several portions of such mercuries are but numerically different? Now to the question thus stated, the fear of seeming to maintain a paradox, by dissenting from the generality of chymists, as well as naturalists (who are wont to employ indiscriminately all running mercuries not manifestly adulterated) will not keep me from returning a negative answer.

AND though it were not over difficult for me to give you the reasons of my opinion, drawn up into method, and refer several instances I shall produce, some to the depuration of quicksilver, some to the impregnation of it, some to the coction, and others to two, or all these ways of altering it; yet I shall rather present you with them, by way of loose observations, because I presume that freedom will not be unacceptable to you, as it will allow me, to give you some few, but uncommon notices and hints about such noble subjects as prepared mercuries.

1. IN the first place, then, it may be observed, that a running mercury may be brought to differ from common quicksilver by depuration: for there are in most mercuries either recrementitious particles, or at least some loose adherencies, that are separable from the rest of the body; and which being separated, the mercury becomes more homogeneous or clean, than it was before this external depuration (for so I call it, to distinguish it from another, that is internal) that is usually made, by grinding and washing mercury very well with salt and vinegar (for which purpose, I also sometimes use spirit of wine) which one may not unfrequently see somewhat fouled, by what it carries off from the mercury, which is also sometimes attempted to be purified by the more laborious way of distillation; which, though in some cases insufficient (as I shall shew anon) is in some others very convenient; whereto some artists add other probable means, tending to the same purpose. So that I do not wonder to find, that divers philosophical Spagyrists themselves, before they proceed to more intimate preparations of mercury, order it to be several times previously incorporated and sublimed with acid salts or sulphurs, and then revived with alkalies. Since without examining their grounds it may be said, according to mechanical principles, that by diligent commixtures the mercury is divided into exceedingly minute, if not invisible globules, or such like parts; and by this great comminution, it acquires far more of surface than it had before, by which means a great multitude of separable parts come to be touched, almost of every side, by the salts or sulphurs, to which by this means, when the quicksilver is driven from them in the revivification, it is probable, that very many of them stick, that were not superficial, when all these globules made up but one mercurial mass. And it is possible too, that the alkalies employed to revive the quicksilver, may help to tear off from it some of the feculent particles, which the chymists would desire to have it freed from. And here, let me advertise you upon the by, that there is no necessity to have recourse to salt of tartar or quicklime, or such like alkalies for the reviving of quicksilver: and therefore when I would, with ease, obtain a clean and active mercury for some purposes, I do not employ acid and then alcalizate salts, but mix very well common cinnabar finely powdered with a double weight, or at least, an equal weight of filings of iron, or steel. For these being distilled together in a low retort with a smart fire, the sulphur of the cinnabar

will faſten upon the filings, and let the mercury come over fair and vivid, and perhaps ſomewhat impregnated with a martial virtue, upon whoſe ſcore it may be better than if it had been prepared by mere depuration.

2. AND this leads me to the mention of another way of diverſifying mercury, which is by impregnation, either corporeal, or ſpiritual (if for diſtinction ſake I may ſo call them.) But the impregnation being a comprehenſive way, divers particular methods may, after a manner, be referred to it: yet becauſe the true grounds of ſuch references are ſometimes hard enough to be aſſigned, at leaſt in few words, I ſhall allow myſelf, without ſcrupulouſly regarding them, to proceed in my free obſervations.

3. THE next thing then, upon whoſe account a running mercury may come to differ from common quickſilver, is a ſpiritual impregnation. By mercury ſpiritually impregnated I mean that, with which ſome ſubtile parts of another body are ſo intimately aſſociated and united, that not only the additament will paſs with the mercury, when it is ſtrained through leather (though that be the means, by which artiſts uſually ſeparate gold itſelf from the mercury, wherewith it has been amalgamed) but will alſo continue with it after diſtillation, without hindering the mercury from being vivid enough. I know there are many chymiſts, eſpecially among the more cautious, that look upon quickſilver as ſo heteroclite a mineral, that as no body can faſten enough upon it, to alter it intrinſecally, ſo it will not admit any other body to be aſſociated with it any thing intimately, or permanently. And indeed ſince we find, that when gold itſelf, with which of all bodies whatſoever mercury is believed to have the greateſt ſympathy, may yet be ſeparated from it by ſtraining an amalgam of thoſe two metals through leather, which will tranſmit the quickſilver, and retain the gold; and if ſuch an amalgam be diſtilled with a competent fire, the mercury will aſcend, and leave all the gold behind in the retort; ſince mercury I ſay, is ſo ſeparable from gold itſelf, it may ſeem improbable, that it ſhould be more intimately aſſociated with any other bodies. But theſe arguments, though ſpecious, do not, I confeſs, convince me, who muſt not deny, but that the corpuſcles of ſome mineral bodies may be ſo well commixt with quickſilver, as to paſs with it through the pores of leather, and who have found by trial purpoſely made (and elſewhere related) that quickſilver being in a convenient proportion amalgamed with tin, or with lead, and diſtilled with a competent fire, will manifeſtly bring over with it ſome of the aſſociated metal; inſomuch that not only I have found a notable increaſe in the weight of the diſtilled quickſilver, but it would both leave a tail, as they call it, behind it, when it moved upon a ſloping glaſs, and (which was more) when the ſluggiſh mercury had reſted a while, it would appear covered over with a kind of ſcum, made of the emerging corpuſcles of the tin, or lead, either of which, eſpecially the former, is a metal lighter in ſpecie than quickſilver. Hence it appears, that mercury may be ſo ſtrictly united to a not deſpicable proportion of a groſs and ponderous body, and of an ignobler kind, as to carry it along with itſelf in diſtillation; which by this appears not to be near ſo certain a way, as ſome learned chymiſts think it, to try, whether mercury be pure from all adulterating mixtures, and to free it from them, if it had any before. But the chief uſe I will make of this experiment is this, that ſince we ſee, that ſometimes mercury does not refuſe even corporal impregnations (as for diſtinction ſake I call thoſe lately recited) it ought not to appear incredible, that it may in ſome caſes admit ſpiritual impregnations, and ſo intimately aſſociate with itſelf ſome of the finer parts of certain metals and minerals, as not to part with them, though they be diſtilled, and afterwards perhaps ſeveral times waſhed. This brings into

into my mind, that I had once a diſtilled mercury made by an impregnation of common mercury, a drop or globule of which, being evaporated from a thin piece of ſilver, not only ſeemed to have ſomewhat penetrated it, but (as I expected) left upon it a rugged ſubſtance apparently lighter than the ſurface of the piece, and of a colour very near that of gold, from whoſe nature perhaps it was not very remote: but that common mercury may indeed be ſpiritually impregnated, I have been perſuaded by divers effects, that I have tried of ſuch impregnations: and I acknowledge to you, that moſt of the uncommon mercuries, that I am now proceeding to tell you of, have been prepared after ſome ſuch manner.

4. ANOTHER thing, wherein a mercury may differ from common quickſilver, is a facility to amalgam with gold: for it is known to gilders, goldſmiths, and others, that are verſed in ſuch experiments, that to make amalgams with gold and mercury, it is uſual enough to take ſix parts of the latter to one of the former, and ſome take eight or more. Nor is ſo great a proportion of mercury wont to keep them from thinking it requiſite to make both it and the gold ſeparably, and conſiderably hot to facilitate their commixture, but I have divers times had ſpiritually impregnated mercuries with but two parts, of which I would preſently make an amalgam with one part of the calx, or leaves of gold, and that without any other external heat, than that of the palm of my hand. Nay, ſometimes, for trial ſake, I have employed but one part of quickſilver to make in the palm of my hand a mixture, wherein the gold was ſo far from appearing, that the colour of the quickſilver was not ſenſibly ſo much as impaired.

5. ANOTHER difference between ſome mercuries and thoſe that are vulgar, is, that theſe being put to calx of gold, though one do at length bring them to mix (for it is not ſo eaſily done, as men are wont to preſume) yet they will not diſcloſe any ſenſible heat, but the mixture, as each of the incorporated ingredients was, will to the touch be cold: but though I know there are many learned chymiſts, that look upon incaleſcent mercuries, that is, ſuch as will grow hot upon their mixing with gold, as chymical *non-entia*, or chimæras, yet they are not competent judges of the poſſibility of things. For I have more than once, or a few times, both alone, and in the preſence of ſome curious perſons, found and evinced, that a diſtilled mercury may be ſo animated, that a ſingle drachm of it, or perhaps a far leſs quantity, being mixed barely with my finger, with as much, or perchance half as much, calx of gold, would preſently conceive, not only a ſenſible, but a very conſiderable heat; inſomuch that ſometimes it would prove offenſive to the palm of my hand, wherein I made the mixture. Divers phænomena of this experiment may be ſeen in the author's little Tract *Of the Incaleſcence of Quickſilver with Gold*, now extant in the *Philoſophical Tranſactions, Numb.* 112. And I remember, that once being to convince a very eminent chymiſt, that there were ſuch mercuries, as I have been ſpeaking of, I took a remnant of a certain quickſilver, which I intend never to make again (and of which, for the ſake of mankind, I reſolve not to teach the preparation) of whoſe diſpoſition to incaleſcence I had ſuch an opinion, that though we had no calx, nor ſo much as filings of gold, but only ſuch pieces, as he could groſsly prepare with a hammer and a pair of ſciſſors, I ventured to put my mercury to them in a glaſs-mortar; and yet notwithſtanding the thickneſs and cloſeneſs of the beaten metal, and the coldneſs of the veſſel, the mercury, to the artiſt's wonder, penetrated the gold, and grew manifeſtly hot with it. And this faculty of our quickſilver was not a tranſient and eaſily vaniſhing one; for I had already kept the mercury by me for ſeveral years. The incaleſcent mercuries hitherto mentioned were animated by tedious and laborious operations;

operations; but if I had desired only to convince gain-sayers, I could have done it by a very much shorter way: for though this sort of impregnated mercuries be many degrees inferiour to the forementioned animated mercuries, yet as to incalescence with gold, I know by experience a way, which is indeed hard to hit, and requires a dexterous artist, but which, when it succeeds aright, will in an hour, and perhaps a less time, qualify mercury to grow presently hot with gold.

6. When an animated mercury is by due impregnation qualified to amalgam readily and intimately with gold, aud penetrated so as presently to grow hot with it, it is not much to be admired, that it should also differ from common mercury, in the being able to carry up with it part of the gold, wherewith it was so strictly associated. I know, that many learned men, and among them divers chymists themselves, do not think it credible, that at least corporal gold should be volatilized by quicksilver. And indeed that, which is common, may be many times distilled from gold, without carrying up any of it; but this ought not to conclude against such spiritually impregnated mercuries, as I lately mentioned: for with a very small quantity of one of them I have sometimes elevated so much calx of gold, that the inside and neck of the retort were richly gilt by the adherent particles of that metal, which would sometimes stick so close, as not to be without difficulty separated from the glass. And I remember too, that having with one of these noble mercuries amalgamed about half its weight at most (if I mistake not) of calx of gold, though it did not gild the inside of the glass, yet I found, as I expected, that the distilled mercury was manifestly encreased in weight, as well as somewhat changed in colour and consistence: which experiment may be added to those, that I formerly mentioned, to prove, that quicksilver (duly prepared) may be corporally impregnated.

7. In the amalgams made of one of these spiritually impregnated mercuries with calx of gold, I have sometimes observed a thing, that argues such mercuries to be differing from common quicksilver: of whose amalgams with gold such an effect has never (that I know) been taken notice of. The phænomenon I mean was this; that by distilling one of those subtile amalgams in a retort, a good part of the bottom of the vessel, which I have yet by me, was left adorned with a very lovely colour, almost like that of turquois stone, inclining towards the colour of gold, and somewhat changeable; and also so closely fastened to the glass, that it seems to have penetrated into it, though this beautiful stain were left by but a very small quantity of the amalgam, and though this mixture were distilled but in a moderate fire, since it was in a sand-furnace. Nor is this the only experiment of this kind, that I would allege, since I elsewhere mention an amalgam of gold with an animated mercury, which being long decocted, when at length by an excessive fire unskilfully administred the vessel was unluckily broken, left the lower part of the glass permanently tinged a pure and transparent red, that seemed to me to emulate that of a not common ruby.

8. Another difference I found between ordinary quicksilver and spiritually impregnated mercuries, that will perhaps somewhat surprize you. And it is, that though one would expect, that amalgams made with mercuries so penetrant, and so disposed to adhere closely to gold, should make with it amalgams far more easy than those made with ordinary quicksilver to be turned into red precipitate, yet I found the quite contrary upon trial. For whereas chymists are wont to mention about six weeks as the usual time, wherein mercury may be precipitated even *per se*, that is, without additament, and allow but a shorter time to make th s precipitation, when it is amalgamed with gold (whereby some of it is detained, and all more exposed to the action of

of the fire;) I have had the curiosity to keep an animated mercury amalgamed with about a third part of its weight of fine gold above twice six weeks, without having so much as a grain or two of precipitate (perhaps not half so much) that I could perceive, though the mercury grew hot with the gold at their being mingled, and though the mixture were purposely kept in a good heat capable to make quicksilver circulate. Nor did I content myself with one trial, nor with one sort of animated mercury, but in above five or six months I obtained not one grain (that I could discern) of precipitate, though the heat was so strong, as to carry up many parts of the quicksilver, and of the gold with it, to the top of the glaces; nay, in one of them (which was a somewhat odd case) the fire was so violent, that the hermetically sealed glasses beginning to melt, the spirituous matter included in it was so forcibly expanded, as to stretch the weaker side of the glass, and give it as it were a bunch, yet without breaking it; as I can shew you in the glass itself, that I have yet entire by me. Nor do six months make the longest term, that the obstinacy of my curiosity has made me keep gold in decoction with animated mercuries without obtaining a red powder or precipitate, though in the mean time there were produced very pretty vegetations, and sometimes, which is far more considerable, odd changes of colours, about which it is not here necessary to entertain you; the main drift of this observation being to give you notice, that as far as I have yet tried, the more subtle and richly impregnated mercuries are far less apt to afford precipitates with gold, than common quicksilver is. As if that disposition to be calcined (as the chymists are pleased to speak) or turned into powder, required the presence of the recrementitious or more separable part of quicksilver, that a chymist would perhaps call it sulphur, which was a discovery I could willingly enough have missed. For I confess I had some hope, as well as intention, to try, whether a precipitate, made with gold and some of these noble and richly impregnated mercuries, would not prove a nobler medicine than precipitates made with gold, and only common mercury; though even of some of these, when dexterously prepared and kept their due time in decoction, experience invited me to have no slight opinion; especially, if they be exhibited in a just dose, and accompanied with a proper additament, by which they may be kept from raising any salivation, and have their operation either altogether or almost totally determined downwards.

9. THE last difference I shall observe between some distilled mercuries and common quicksilver, shall be their inequality in point of specific gravity. I know you will think this a paradox; but I can tell you, that I had once the opportunity to examine hydrostatically a noble mercury, for the impregnating whereof neither corporal gold nor silver was employed; and yet having carefully weighed this quicksilver in water, according to the method I elsewhere teach, in the presence of a famous and very heedful virtuoso, I found it, as I had foretold, not only manifestly, but very considerably heavier in specie (that is, bulk for bulk) than common quicksilver, though this mercury had been several times distilled, and by other ways depurated: which to me seemed to argue, that even spiritual or volatile gold (for no visible gold was employed, and no metal but gold is so heavy as quicksilver) is able to increase the specific gravity of mercury itself, which, of all the bodies we know, is exceeded or equalled in that quality but by one alone. And the ponderousness of our lately mentioned mercury seems to me the more wonderful, because having by the same method hydrostatically examined a mercury, made after a strange way (without common mercury) I found it scarce at all to differ in gravity from common quicksilver, since it did not weigh full fourteen times as much common water of the same bulk.

BUT here I ſhall obſerve to you upon the by, that it is not a certain conſequence, to infer, that the heavier the mercury is, the more fixt it muſt be; for I remember, that having been once ſo unadviſed as to comply with the earneſt ſolicitations of an inquiſitive gentleman, that afterward behaved himſelf very ungratefully and unworthily to me, I gave him inſtructions, how to make an animated mercury, which I looked upon as very much of the like nature to the ponderous one, I have been ſpeaking of, but leſs tedious, and far leſs difficult to be prepared; and while he found he needed my renewed directions, according as new difficulties occurred to him, he gave me from time to time an account of his progreſs; and when he was advanced far in the proceſs, he informed me, amongſt other things, that following my direction in purifying and animating his quickſilver, he found it ſo altered and ſubtilized, that he would diſtil it in leſs than half the time he had formerly employed to drive it over, with the like fire and veſſels.

THIS is what I thought fit to ſay at preſent, about the differences between common quickſilver and prepared (but yet running) mercuries. And yet I am content to add two or three advertiſements, for which, and eſpecially for the firſt of them, you will perhaps thank me, if ever you ſhould vigorouſly proſecute, in a ſpagyrical way, the more noble ſort of mercurial experiments.

IN the firſt place then, I ſhall obſerve to you, that whatever ſome learned chymiſts, and others, teach to the contrary, it is matter of fact, that mercuries may be animated or ſpiritually impregnated by more ways than one (not to ſay, by more than a few) ſo as to penetrate gold very powerfully, and grow hot with it; and it ſeems to me very probable, upon grounds not meerly notional, that the differing ways, that are employed to prepare theſe animated mercuries, by impregnating them with this, or that mineral, or metal, may much diverſify their qualities and operations, according to the reſpective natures of the bodies they are impregnated with. Nay though there ſeem ſo great a diſtance between quickſilver and vegetable ſubſtances, yet I have ſeen a mercury, that was prepared by the help of vegetables without metals or minerals, which was very different from common quickſilver, by being more noble than it.

THE ſecond thing I am to acquaint you with, is, that as divers bodies and methods may be employed in the preparation of noble mercuries (as I have newly obſerved) ſo it ſeems very probable, that the common mercuries ſo prepared may have differing, as well as noble qualities and uſes, not only in reſpect of alchymy, but of medicine; as being fitted to have potent operations, as well upon human bodies, as the more ſtubborn ones of metals and minerals. I am not indeed at all forward to recommend the needleſs uſe of mercurial medicines, of which we may too often ſee bad effects, if they be not as well prudently and cautiouſly given, as faithfully and ſkilfully prepared: but ſince we ſee, that, in ſpight of *Helmont*, very many learned and experienced phyſicians allow themſelves to employ, frequently enough, even the vulgar preparations of common mercury, ſome of which prove indeed oftentimes in ſome ſtubborn diſeaſes far more efficacious than ordinary medicines; I ſee not, why we may not hope for greater and more innocent effects from a mercury well purified and impregnated with the ſulphur and finer parts of ſuch bodies, as volatile gold, or Venus, or Mars, or antimony, *&c.* And though, as I lately told you, I found ſuch animated mercuries far more indiſpoſed than common quickſilver, to make a precipitate with corporal gold; yet this need not hinder, but that divers other preparations may be made, as well with impregnated, as with vulgar mercury; ſuch as are turbith mineral, the white precipitate, that purges downwards, *mercurius dulcis*, pills of crude

mercury made up with fit ingredients (as in those, that are by some called the blue and the black pill) and especially the cinnabar made by subliming quicksilver and sulphur into a purely red substance, which, though wont to be employed chiefly by painters, ought not perhaps to be neglected by physicians; since even in ordinary cinnabar the vulgar mercury is so bridled by the common sulphur, that unless too frequently given without pauses, or in an indiscreet dose, it has not been usually found to salivate, yet does often lie not idle nor useless in the body: so that it may be well worth trying, whether a noble cinnabar may not be obtained by substituting animated mercury for vulgar, especially if instead of common sulphur one should employ that of antimony, or of antimony and vitriol, which I have elsewhere shewn to make.

THE third and last advertisement I will give you, shall be, that you are not hastily to conclude, that a mercury, that has been carefully depurated and impregnated, has not been well prepared, if you find it not readily to elevate corporal gold, as it may seem by the past discourse, that most of the animated mercuries I have mentioned did. For though it be true, that I have had some mercuries fitted to penetrate gold so far, and mix with it so closely, that it would quickly upon distillation visibly carry up some of that ponderous metal with it; yet so much is not to be expected from all mercuries, that may lay claim to the title of animated or noble. For I have found, that some even of these may require a strong decoction to incorporate them intimately with gold; and I remember, that once, for trial-sake, I made mercury, which upon bare distillation would not at all colour the glass: I made, I say, this mercury, by decocting or circulating it with the gold for ten days or a fortnight, unite so closely with the metal, that it would afterwards elevate enough of it to gild the inside of the glass; and by a much longer decoction I have sometimes had the gold lodged copiously in the upper part, and even in the neck of considerably tall glass-eggs, hermetically sealed, one or two of which I can yet shew you.

OF THE

Producibleness of PHLEGM or WATER.

PART V.

OF the several substances, that chymists obtain by the fire from mixt bodies, that, which they call phlegm or water, and would have men look upon as mere water, separated by a preceding analysis, seems to the Helmontians, and divers other modern artists, to bid the fairest for the title of elementary and primordial. Wherefore it will now be fit to consider, whether, about that also, we may not justly retain some doubts, and rationally suspect, that all, that they call the phlegm of bodies, was not in the form of elementary simple water, pre-existent in the body, whence it is obtained; but that even such portions of matter, as many of those, that pass among chymists for phlegms, may be produced either by the operation of the fire, or by other ways.

IN order to this inquiry, it will be fit to premise something against the presumed simplicity and homogeneity of the liquors, whereto the Spagyrists give, in common, the name of phlegm; that in case some of the produced liquors we speak of shall be denied to be precisely of an elementary nature, it may appear, that that ought not to hinder us from allowing them the name of phlegm, provided that they be not remoter from simplicity, than those, to which chymists grant that appellation.

AND first, I consider, that besides those qualities, that are common with water to divers other liquors confessedly not simple, as transparency, want of colour, aptness to be imbibed by most sorts of vegetable and animal substances; the two qualities upon whose account chymists are wont to call a body phlegm or water, are its appearing to them insipid, and its being of a volatile and fugitive nature.

I FURTHER consider, that not only divers of those liquors, that pass for phlegm, will yield a taste sensible enough to him, that will hold them with attention of mind, for a competent time, in his mouth, but that the criterion of liquors by the taste is nothing near so certain as many think. For, not to mention, that it is plain, that some kinds of dogs, as setters, spaniels, and blood-hounds, take notice of many things by their odours, that we men have no perception of by our smelling, which may argue, that our senses may not be moved with objects, that would affect them, if they were of a more delicate contexture; not to mention this, I say, it is plain, that the subtlety of the sense of tasting differs among men themselves. And those that drink nothing but water, will often tell us of a great disparity betwixt common water, wherein other men find not any. And I remember, that when once I did, though but for some months, confine myself to drink water, I could distinguish the limpid waters of differing places, almost as manifestly as I now distinguish beers, which, after I fell again to drink wine and other liquors, I ceased to be able to do.

THE consideration of quicksilver may, methinks, let us see, that it is possible for a gross and fluid body, that is far from elementary water, to be insipid. For quicksilver is without question a fluid, and at least in reference to some bodies, gold, silver, and some others, a liquor; since it soaks into their pores, and softens the bodies. The same quicksilver may also serve to shew, by its disposition to fly away in the fire, that volatility, even in conjunction with insipidness, is no certain mark of an elementary or simple, nor consequently of a primordial body. And indeed, since volatility depends mainly upon the extraordinary minuteness of the particles, whereof a body consists, and on their being incoherent, and of shapes fitted for motion; this quality may be acquired by so many differing ways, and be found in bodies otherwise of such differing natures, that unless it be found associated with the other qualities proper to phlegm, it will be but an unsure argument, to prove the body, that it belongs to, to be elementary, and not to have been by composition, division, or transposition produced.

IF it be true, as the Cartesians will have it, that water consists of particles, that, like little eels, are long and extremely slender, and consequently flexible; I see not any impossibility, that the various action of the fire, upon the minute parts of a body, and that, which it may cause the corpuscles of one body to have upon those of another, may produce water, that did not in the form of water pre-exist in the body, that affords it: for it seems to me possible enough, that the particles, whereof a corporeal mass is made up, may have such shapes, sizes, and contextures, that by the various agitation, which the pervading corpuscles of the fire may produce amongst them, whatever edges and points they had before, may by mutual attrition of the corpuscles be worn off, and by the same means so much of the substance may be worn away,

away, that what remains, cannot but be very flexible, and by all these qualifications become fit to make a particle of water. As a bar of iron may, by divers strokes of the wedge and hammer skilfully employed, be divided into long and slender parts, whose edges and points being blunted, they may be reduced into slender, and every way flexible wires. But not to build on speculations, let us proceed to some experiments, which afford phænomena, that seem favourable to out hypothesis.

AMONGST the bodies, about which chymistry is conversant, those that seem to be the most indisposed to be turned into water, are the metalline, and mineral ones: so that if it can be made appear, that any of this sort can be changed into an aqueous liquor, it will make it highly probable, that aqueous liquors may be by chymical operations produced, especially in vegetable and animal bodies; which seem far more susceptible of such a change, than the stubborn subjects of the mineral kingdom. And since quicksilver is by many learned men, as well chymists as others, looked upon as one of the few most indestructible bodies in nature, and by its great ponderousness, in which it exceeds all the known bodies of the world save one, is so much the more remote from such a liquor as water, that has not the sixteenth part of its specific weight; if quicksilver itself can in great part be turned into an aqueous liquor, it will not a little favour the doctrine proposed in these notes; for which reason I shall subjoin the ensuing story.

RELATING to a very ingenious and sober physician of my acquaintance what had befallen me in distilling mercury, from whence I once obtained a water without additament, without being able to make the like experiment afterwards succeed; he assured me, that he had a friend of his, had some years past provided a very large Dutch retort of good earth, furnished with a pipe of about a foot long, to cast in the mercury at; and that having by little and little conveyed through that pipe a pound of quicksilver into the cadent retort, they obtained four ounces of water, and lost in spite of their care two ounces of matter (whatever it were) the remaining part of the pound having been elevated in the form of mercury. And when I suggested, that perhaps the water, that came over, was afforded by the aqueous particles of the earthen retort itself, he replied, that, to prevent their being imposed on, they had been careful not to put on the receiver, till the retort had been made throughly glowing hot: and that this liquor was far from common water, he thought to be past doubt by that, which follows. For I having acquainted him with an odd trial or two, I had made with mercurial water, and asked him, whether he found the like effects from his, he told me, that his friend and he poured both their distilled mercury and their water into a kind of China cup, and though it were in *June*, left it open in a garret for two or three days, upon a presumption his friend had, that this mercurial water thus ordered would turn a good part of the quicksilver into its own nature, and and so multiply itself upon it. But when they came to visit their cup again, they were much surprised to find their water all gone, and that the greatest part thereof was turned again into mercury; which they concluded from this, that they missed upon the balance but about half an ounce of the whole matter; which (half ounce) they supposed to have been lost by evaporation; the other three ounces and a half being found in the increased weight of the mercury.

THE mention I have made of quicksilver, puts me in mind of an argument *ad hominem*, that may deserve to be considered by the chief sect of modern chymists, the Helmontians: for if it be true, which their master teaches, that by his liquor alkahest, not only quicksilver, but all other tangible bodies, may be reduced into insipid water, just like rain water, I may be allowed to infer, that water may be produced,

duced, ſince ſalt and ſulphur themſelves may be turned into water. I know the Helmontians may anſwer, that this is not ſo much a production, as a reduction; ſince all things conſiſting originally of water, the alkaheſt does but deprive it of the diſguiſes, that ſeminal principles put it into, to make it appear, under the form of gold, quickſilver, plants, animals, *&c.* But this anſwer may be elſewhere further examined: for the preſent, it may perhaps be ſufficient to reply, that even by this anſwer it is granted to appear by experiment, that water has been copiouſly produced out of mineral bodies, but it has not yet been made appear, that thoſe bodies were produced out of water.

BUT this is not all, nor perhaps the principal thing I have to ſay in favour of the opinion pleaded for in theſe notes. For ſuppoſing bodies by being reduced, by the alkaheſt and the fire, into an inſipid liquor, were really deduced into water, yet the Helmontians would not fully ſatisfy me. For *Helmont* relates, that by abſtracting his immortal liquor from ſtones, or ſuch kinds of bodies, he turns them into ſalt equiponderant to the concrete; which ſalt by further operations he reduces, as he ſuppoſes, into elementary water. Since then he ſtops not at ſalt, but goes to a further tranſmutation, and concludes, that a ſtone doth not conſiſt of ſalt, becauſe that ſalt may, by further operations, be turned into inſipid water; he muſt give me leave, on the ſame ground, to argue, that inſipid water is not the firſt matter of bodies, ſince by a further operation of the fire, that liquor itſelf may be, at leaſt in great part, turned into earth. For I elſewhere relate ſome experiments of my own and a friend's, in which clear water, divers times very ſlowly diſtilled out of clear glaſs bodies, left every time a terreſtrial powder at the bottom: as if (in caſe water be ſo homogeneous a ſubſtance as is ſuppoſed) the whole body of the water might, by reiterated diſtillations, without violence of the fire, be reduced into earth; whereof I remember, in the laſt trial of mine, I had enough to cover the bottom of a large cucurbit, out of which the diſtillations had been made.

AND on this occaſion, I ſhall add a trial, which ſeems to argue, that without the help of often repeated diſtillations in tall cucurbits, clear water itſelf may, by the operation of the fire, be changed into another body.

WE took, then, very pure and limpid water, which had, by our pneumatic engine, been carefully freed from the aërial particles, that are wont to be harboured in the pores of that liquor: this in a new bolt-head of ſuch a ſize, that the matter might have room to play and circulate, we ſealed up hermetically; and placing the veſſel in a digeſtive furnace, we left it there above a year, and obſerved, as we expected, that after it had continued for a good while, there began to form themſelves, in the water, little concretions heavier than it, which in proceſs of time increaſed in magnitude, and, as we thought, in number; making a kind of *terra foliata*, that conſiſted of a multitude of little thin films (or ſcales like thoſe of the ſmaller ſort of fiſhes) which, when the glaſs was ſhaken in an enlightened place, were copiouſly diſperſed through the body of the liquor, and appeared variouſly and vividly coloured, being ſome of them almoſt as big as ſpangles, and more glittering: and when the agitation ceaſed, they preſently fell to the bottom, which they covered in the form of a *terra foliata*; by their ſubſidence manifeſting themſelves to be notably heavier, in ſpecie, than the water they had been formed of. And the longer the glaſs was kept in the digeſtive furnace, the more of this fine terreſtrial ſubſtance was produced: and leſt the effect ſhould be aſcribed to the abſtraction of the air from the water, handled as is before related, I ſhall add, that we produced the like ſubſtance, though, as it ſeemed, not ſo copiouſly, after the like manner, in water, that had not at all been freed from air.

OF THE

PRODUCIBLENESS OF EARTH.

PART VI.

OF all the ſubſtances obtainable from mixt bodies, that which, to perſons not prepoſſeſſed with Helmontian opinions, may ſeem the moſt ſimple, elementary, and unchangeable, is that, which they call earth or *terra damnata*; becauſe there is ſuppoſed to be no doubt, but that the calcining or incinerating violence of the fire, muſt not only have driven away the mercurial and other volatile parts, but muſt alſo have quite burnt out the ſulphurs, which oftentimes are more fixt than the reſt; as the water on the other ſide muſt have diſſolved away all the alkali or fixt ſalt.

THIS ratiocination, I confeſs, is very ſpecious and probable, but yet not ſo ſatiſfactory, but that a ſceptic may retain not irrational doubts about the cogency of it, upon ſuch conſiderations, as I am now going to propoſe.

AND I will begin with conſidering, that, whereas the things, wherein this ſuppoſed ſimplicity, and unchangeableneſs of the earthy part of mixt bodies, is founded, are theſe three: its not diſſolving in water; its not affecting the taſte; and its not having flown away from the incinerated body, that afforded it; it may with probability be doubted, whether any of theſe, or all of them put together, do neceſſarily evince what chymiſts pretend they do.

AND in the firſt place, according to the different conſtitutions of certain ſorts of bodies, I think it fit to make a diſtinction between the dry and heavy parts, that remain after a body has been expoſed to the violence of the fire, and, if need be, freed from its ſalt, as much as it can be, by the affuſion of water. For it is evident, that in ſome bodies, eſpecially of a metalline nature, the fire, that makes that which they call calcination, does not operate as it does in the burning of vegetables. For, beſides that ſometimes almoſt (and ſometimes more than almoſt, the whole weight of a mineral is to be found in that which they call its calx, and is manifeſt in the calcination of lead and tin *per ſe* (if ſkilfully performed) the calx is in great part reducible ſometimes into a body of the ſame nature with that, which afforded it, and ſometimes into ſome other body, very far from being elementary: as is manifeſt partly in the reduction of minium (which is calx of lead made *per ſe*) which, as to the greateſt part of it, we have more than once, by the bare way of ordering the fire, reduced in a very ſhort time, and without additaments, into malleable lead; and partly in the calces or (as they alſo ſpeak) aſhes of antimony, which by bare fuſion are eaſily reduced into glaſs, whence we have ſometimes obtained an antimonial regulus. So that it is manifeſt, that there is a great deal of difference, between the aſhes (taking that word in a large ſenſe) of metals, and of ſome minerals, where almoſt the whole body is by the fire converted into a dry and heavy powder, and the aſhes of incinerated vegetables, which uſually leave but a little quantity of earth behind them, in compariſon of the matter, which the violence of the fire hath driven away.

BUT

BUT ſetting aſide the above-mentioned metalline calces, which without queſtion remain compounded bodies, if metals themſelves be ſo; and to forbear examining, whether they be not further compounded with corpuſcles of the fire, or fuel, that are embodied with them: I conſider, that the qualities, which make other aſhes paſs for elementary earth, may be produced in portions of matter, that are not ſimple, either by compoſition or change of texture.

I HAVE elſewhere occaſion to take notice of bodies, which, though, when they are ſingle, they will eaſily diſſolve in water, yet the reſult of them will not: and you may find inſtances of this kind, among the magiſteries, as chymiſts call them, made of ſeveral bodies, by precipitating their ſolutions (made in acid liquors) with oil of tartar *per deliquium*.

FROM oil of vitriol and ſpirit of wine, though both moſt readily diſſoluble in water, we have, by bare digeſtion and diſtillation, obtained a pretty quantity of a ſubſtance, that we found not to diſſolve in water, and which ſeemed to be inſipid and fixt enough.

THERE are ſtones, ſome more and ſome leſs precious, which though I could by the help of the fire deprive of their colour, and bring to a white powder; yet it did not appear to me, that they were really calcined, or would in water yield any ſalt: ſo that if theſe ſtones be compounded bodies, as Spagyriſts tell us they are, we ſee, that there may be other corpuſcles beſides metalline ones, which though reduced by the help of the fire to a white powder, inſipid, and not diſſoluble in water, are yet remote enough from an elementary nature.

BUT I need ſeek no further, for inſtances of this kind, ſince Spagyriſts themſelves teach us, that the aſhes of wood may, by the violence of the fire, be turned into glaſs; which being a body compoſed of the earthy and ſaline part of the aſhes (for they tell us, that earth ſeparated from the ſalt will never be vitrified) muſt be according to their own confeſſion a compounded body: which being at laſt made by the utmoſt violence of the fire, muſt be fixed, indiſſoluble in water, and conſequently inſipid. And without taking this vitrification upon the chymiſts authority, it is manifeſt, that in glaſs made the common way, there is a great deal of borellia, ſal alkali, or other lixiviate ſalt mixt with the ſand: as is evident, not only becauſe artificers find the ſalt needful to diſſolve the ſand, and bring it to fuſion, but becauſe the glaſs uſes to weigh very much more, ſometimes (as an ingenious maſter of a glaſs-houſe anſwered me) thirty, or forty pound in an hundred, than the ſand, that was put in.

I SHALL add, that, ſince chymiſts aſcribe all odours to ſulphur, I may reaſonably conclude againſt them, that in ſpite of all the violent fire, which is required to the making common glaſs, there is ſtore of ſulphur, as well as ſalt in it. For I have often tried, that by barely rubbing two large pieces of glaſs, one againſt another, there would quickly be produced a ſtrong offenſive ſmell. And yet glaſs, which thus appears to be, not only a compounded, but a decompounded body, ſince the ſand or cugali (as the Venetian glaſs-men call their pebbles) or other ſtones being themſelves mixt bodies, are further compounded with the ſalts that diſſolve them: glaſs, I ſay, is a body, that manifeſtly poſſeſſes all theſe three qualities, which chymiſts require in their earth, being taſteleſs, indiſſoluble in the water, and fixed in the fire. And if aſhes alone be (as chymiſts teach us they are) capable of vitrification (and indeed an inquiſitive owner of a glaſs-houſe anſwered me, that once, if he much miſ-remembered not, he made, but not eaſily, glaſs of aſhes alone without ſand;) how are we ſure, but that in common aſhes, freed the common way, from their fixt ſalt, that

that which is called the ſimple earth, may not be a body compounded of two or more ſubſtances, which by their coalition, and new texture produced by the action of the fire, have been brought to a kind of vitrification, or otherwiſe have acquired the few obvious qualities, that chymiſts are wont to think ſufficient to give a production of the fire, the name of earth?

It is obvious to obſerve, that divers bodies, when they come to be of ſenſible bulk, will ſink in liquors, in which their corpuſcles would freely ſwim, if ſo many of them did not ſtick together. As of ſalt and ſugar, the lumps, and even the grains, whilſt they remain ſuch, will fall to the bottom of water, in which when they are diſperſed into minute and inviſible corpuſcles, they will eaſily ſwim. And I have obſerved, that in ſtopt glaſſes ſome ſalts, and other bodies, that for many months remained undiſtinguiſhed in the liquors, that harboured them, would in tract of time have conventions made of their particles, that would then ſubſide, and be no more carried up and down by the particles of the liquor. And I ſee no impoſſibility, that ſomewhat of this kind may happen to the particles, whereof water conſiſts: for if ſome of theſe, by frequent occurſions and attritions come to apply themſelves to one another, ſo as to have a fuller, and more immediate contact than formerly, and to be intangled among themſelves, and perhaps alſo to exclude ſome very thin and ſubtile air, that may be ſuſpected to lurk about them, and contribute to their ſuſtentation; if, I ſay, this union, or ſtrict adheſion of aqueous corpuſcles, ſhall happen to be made, the aggregates or cluſters, though as to ſenſe but very ſmall, may be too great and unwieldy to be any longer parts of water, but may ſubſide in that liquor; and if their union or adheſion be ſtrict enough, they will upon the ſame account be unfit to be carried up in the form of vapours, and exhalations by heat, and ſo may be like earth fixed in the fire, as well as not diſſoluble in water.

I have ſometimes alſo had a ſuſpicion, that the production of an earthy ſubſtance in water may be furthered by the particles of fire, that are employed to make it circulate; and that of thoſe igneous particles, which, as I am apt to think, pervade the glaſs, ſome of the groſſer, or rather the leſs ſubtile, may in their paſſage faſten themſelves to ſome aqueous particles, fitted to adhere to them, and may with theſe begin to make ſome inviſible concretions; to which afterwards, other congruous particles may, little by little, ſtick, in their paſſage, and ſo at length compoſe ſenſible aggregates of powder: which may be illuſtrated by what happens in the precipitation of quickſilver without addition, where the mercurial particles being aſſociated by, and probably with ſome of thoſe of the fire, begin to form concretions, at firſt very minute, which afterwards increaſe more and more, by the acceſſion of other adhering particles, till all the mercury, or the greateſt part of it be reduced, from a fluid body, to a red powder. And perhaps it may countenance my conjecture, about the production of an earthy ſubſtance, by a briſk concurrence of the particles of fire, if I add, that though I have kept high rectified ſpirit of wine for above a year together hermetically ſealed, and for the moſt part of that time in a digeſtive furnace, without finding any earthy reſidence; yet, when I ordered a bolt-head, that, though it were hermetically ſealed, the alcohol of wine that was put into it might be boiled without breaking the glaſs, I found, that in a ſhort time this liquor would afford a not inconſiderable quantity of ſuch a ſubſiding talcky ſubſtance, as I obtained from the water, formerly mentioned. But theſe things I need propoſe but as illuſtrations, that may ſomewhat help you, to conceive how water may be turned into earth. For, whether it be by theſe, or any other ways, that the thing is performed, yet ſince the experiment formerly recited, that water by frequent cohobations may be more and more

turned into earth, argues the matter of fact, our not being able to explicate the manner, does not hinder the thing from being true, nor the argument we build on it from being good: since even water, to which, by some operations of the fire and the alkahest, it is pleaded, that bodies are ultimately reduced, may itself by a further and very simple operation of the fire be reduced into earth.

I HAVE somewhere mentioned, for I remember not in what paper I have observed, salt-petre distilled with clay, to lose much more of its weight, than can be supposed to have ascended in the form either of spirit or phlegm. And now to make this experiment more short and easy, I shall add, that I lately made it in a crucible (instead of a retort) wherein a double weight of finely powdered tobacco-pipe clay, very well mixt with pulverized crystals of nitre, were kept three hours, in a violent fire, and then the mixture being taken out, the remaining fixt salt was carefully extracted, but amounted to very little in comparison of what nitre was wont to yield, when calcined with charcoal. And that this scant proportion of fixt salt did not proceed chiefly from a very copious avolation of nitrous substance, appeared probable by this, that the *caput mortuum* was much more ponderous, than was to be expected, upon the score of the tobacco-pipe clay, that was first employed, and the alkali, that was extracted; so that the new weight acquired by the clay seemed manifestly to proceed from the accession of a portion of the salt-petre, that by this operation was turned into earth. Insomuch, that of six drachms, that four ounces of clay had acquired in weight, after the crucible was taken out, not so many grains could even by boiling water be obtained from the whole *caput mortuum*: which, when first taken out of the crucible, was almost quite insipid.

THAT earth may be *de novo* produced, may likewise be probably argued from the experiment I formerly related about the destruction of the salt of tartar, by igniting it, and putting it into fair water; for there remained after the numerous filtrations, and, if I misremember not, after every one of them, a substance in the filter, which, for aught appears, may be as well called earth, as that, which was separated from the calcined tartar, the first time it was put in the water, to divide the salt from the earth. For in our experiment, as well as in the common operation I come from mentioning, the way of proceeding is the same, and in both there remains in the filter a substance, which, by its staying there, shews it was not dissoluble in the water, and which before it came thither, shewed, by its enduring a violent fire, that it was also fixt as earth ought to be. Nor would it much move me, if it should be found, that this factitious earth may have some such operation upon some particular body, as is not thought to belong to true elementary earth. For since it is obtained by a chymical analysis, if it have those qualities, that in the general estimations of naturalists make up the notion of what they call earth; I think that ought to suffice us, at least till the chymists give us some accurate notion of genuine earth, and shew us such a thing, and teach us a better way of analysis, to obtain it.

FOR in many bodies, that are, without scruple, looked upon as earth, I observe qualities, that do not belong to the received notion of elementary earth. This I say, because I see not, why such a texture, as will suffice to make a portion of matter indissoluble in water, fixt in the fire, and insipid upon the tongue, may not also make it fit to operate actively upon some bodies, and modify the operation of some others, that act upon it. And if our earth from salt of tartar be rejected as spurious, they ought to confess the insufficiency of their common way of separating a true earth from the bodies they analyze: for it seems calcination, and solution in water, and filtration, which make up their usual method, will not suffice to make our earth of

tartar paſs for true; though it appear not to be near ſo remote from an elementary nature, as ſome other bodies, that are obtained from earth by the vulgar analyſis. Of which I ſhall at the cloſe of theſe notes give an inſtance, in well-dulcified quick-lime, which, according to the doctrine of the chymiſts, ought to be an elementary earth; and yet ſeems not more ſo, (if it be ſo much) as our earth from ſalt of tartar. And for the preſent, I ſhall add, that the *caput mortuum* of vitriol remaining after it had long endured a violent fire, though it were diligently freed from ſaltneſs by reiterated ablutions with hot water, was yet far from being an elementary earth; as appeareth not only by its deep purpliſh colour, and its ponderouſneſs, far exceeding that of earth, but by a trial, that I purpoſely made to examine it.

UPON this occaſion I remember, that a learned man of my acquaintance, who viſited the mines of *Hungary* (and dealt much in Hungarian vitriol) affirmed to me, when I told him I conceived the *caput mortuum* of it to retain much of the metalline nature, that he had, upon a certain occaſion, out of the colcothar of a certain ſort of Hungarian vitriol, not only received a pretty deal of good copper, but ſeparated from that copper a pretty portion of ſilver, and ſome grains of true gold.

BEFORE I put a period to theſe notes about earth, though my argument do not need nor require, that I ſhould do it, yet upon this fair occaſion I ſhall here take leave to doubt, whether ſuch an elementary earth, as the ſchools tell us of, do yet appear to be more than a notional thing. For to what I have already ſaid concerning the earths ſuppoſed to be produced by chymical analyſes, I ſhall now add, that I have not yet ſeen it proved, that nature does, any more than art, afford us a true elementary earth; at leaſt I can ſay, that ſome, which ſeem to be of the more ſimple ſort, I found upon examination to have qualities not aſcribed to pure earth. For though tobacco-pipe clay, by reaſon of its fixity, whiteneſs, and inſipidneſs, and its lying oftentimes deep enough beneath the ſurface of the ground, may, as probably as almoſt any other native earth we know, be looked upon as elementary; yet tobacco-pipes well baked may ſometimes be made to ſtrike fire. And I have more than once tried, that by briſkly rubbing two pieces of a new tobacco-pipe, one againſt another, they would in a minute or two of an hour grow warm; and being immediately ſmelt to, manifeſtly afford a rank odour, between ſulphureous and bituminous, almoſt like that, which proceeds from pebbles or flints, when they are rubbed hard againſt one another. As if tobacco-pipe clay were not a true earth, but a fine white ſand, conſiſting of grains too ſmall to be diſtinctly taken notice of, like thoſe of other ſand. On which occaſion I ſhall add, that I found, by a hydroſtatical trial, that its ſpecific gravity was but little differing from that of pebbles, its proportion in weight to water of the ſame bulk being as two and a quarter to one. A tobacco-pipe may alſo be ſomewhat melted by a very vehement fire (for a leſs will ſcarce ſerve the turn) as may be argued from this, that it may by ſuch a fire be brought to bend.

PORCELANE, or the matter, whereof China diſhes are made, is not, as ſome travellers and learned men have fondly imagined, a compoſition, that requires to be buried under ground, for I know not how many years, to ripen it into porcelane; but as ſome late authors inform us, and as I have been aſſured by a perſon, that went purpoſely to that place in *China*, that is ſo famous for the making of porcelane veſſels, it is a pure ſort of clay; but yet this I find not to be elementary earth. For beſides that I have obſerved, that a violent fire will make it ſomewhat melt; I find, that with ſteel it will eaſily enough ſtrike fire almoſt like a flint. The lik I have obſerved in porcellane very artificially imitated with a ſort of Engliſh clay. And I found too, that the matter even of dark-coloured jugs of the better ſort, and well

baked, would with a ſteel afford ſparks of fire. I forgot to tell you, when I was ſpeaking of fine porcelane, that I found its ſpecific gravity to be very near the ſame with that of flints, and tobacco-pipe clay. But I remember I went once to viſit a grove or pit, where at the depth of divers yards, they were wont to dig up a certain white earth, which diſtilled, by an acquaintance of mine, afforded a liquor, that was put into my hand to try, and which I found to be very rich in a volatile ſalt, that taſted and ſmelt much like ſpirit of urine, or hartſhorn, and had almoſt the ſame effects in changing the colours of ſome bodies, and precipitating of others. I remember too, that I found by the operation of a menſtruum or two upon tripoli, that, as white and pure a virgin earth as it ſeemed, yet it was not elementary: and, on the other ſide, a maſter of ſome Engliſh mines having preſented me, among other minerals, which he knew not what to make of, a very white ſubſtance, whereof he had ſtore, which he thought an earth, and which was judged by an excellent artificer, much converſant with tripoli, to be finer even than that earth, I gueſſed it upon examination to be a kind of talc, whoſe leaves were exceeding fine and minute. The reſult of theſe relations may be, not only, that we may yet retain our doubts, whether the aſſertors of elementary earth can ſhew us any native ſubſtance, that deſerves that name; but alſo, whether thoſe things, that remain after chymical analyſes, though they have all the qualities, that are judged ſufficient to denominate a portion of matter, earth, may not yet either be compounded bodies, or be endowed with qualities, which belong not to ſimple earth. To explain and confirm which, I ſhall give an inſtance in ſome quick-lime, that I purpoſely examined. For though it had been, by frequent ablutions in warm water, carefully dulcified, and ſo was as well inſipid, as fixt, and indiſſoluble in water; yet I found, I could readily diſſolve it in divers menſtruums, and even ſpirit of vinegar: whereas true elementary earth ought to be as well indiſſoluble in ſuch liquors as in water.

If I had not been to deal with chymiſts, but Ariſtotelians, I might have ſaved myſelf the labour of ſolicitouſly endeavouring to prove, that earth and water may each of them be produced from bodies of a differing nature from it. For though the Peripatetics will not allow the whole elements to have been produced, becauſe they look upon them as integrant parts of the world, which *Ariſtotle* laboriouſly (though not ſolidly) maintains to be eternal; yet the portions of the elements they will have to be interchangeably tranſmutable. So that what was once water may be earth, and earth may, by intermediate alterations, be turned into water.

But thoſe I have to do with, being the vulgar chymiſts, who will have the material principles or ſimple ingredients of mixt bodies co-eval with the world, and uncapable of being either deſtroyed or produced; it was not allowable for me to proceed upon the Ariſtotelian hypotheſis, of the tranſmutableneſs of what they call elements; eſpecially becauſe, that though I do admit it, as it is, according to my opinion, a part of a more general truth; yet I do not think, they have well proved it by their arguments: which ſince I myſelf do not like, I think it were diſ-ingenuous to preſs them upon others. And without having recourſe to their doctrine, there will occur ſome other particulars, that may be added to thoſe already mentioned, to countenance the produciblenes of the principles of mixt bodies, in ſome other papers, that are to follow theſe notes; though in ſtrictneſs I was not obliged to ſay ſo much, as I have already ſaid, ſince pretending to call in queſtion no more than the three hypoſtatical principles of the chymiſts, I might eaſily have excuſed myſelf for having let alone the production of water and earth; ſince the generality of chymiſts reckon not thoſe bodies amongſt their hypoſtatical principles, but look upon them, as receptacles, or, as

as others would have them recrements of these, or as on some other account, related to them. But I was not willing to omit water and earth, because some of the more learned of the modern Spagyrists have adopted them into the number of the principles of mixt bodies; and because I find by experience, that in chymical analyses they are at least as often to be met with, as some of the principles confessedly hypostatical. But what has been delivered about earth and water having much added to the bulk of these notes, it is time I should put a period to this part of them, in reference to which I hope it will be considered, that I do not pretend, that every single experiment by me alleged does sufficiently prove, that the body obtained by it was in the strictest sense produced. For if the several experiments, and other proofs, do in conjunction, and, as it were in a body, make up a good argument, that the ingredients they relate to, may be produced; it is as much as will, I hope, be expected from these notes, which having been written by way of appendix to the *Sceptical Chymist*, may be allowed, as well as that book, to employ some proofs, not altogether cogent, and may be judged not improper, though some of the arguments proposed in them, to shew, that chymical principles are not all ingenerable and indestructible, should be but meerly probable. And yet this I shall venture to intimate, that vulgar chymists and Aristotelians may not, perchance, find it so easy a thing, as it is like many of them will imagine, to confute divers passages of the foregoing tract; since probably their objections will suppose something or other, which, though taken for granted among them, and perhaps by several other learned men, I do not admit as true, or think demonstrable, but rather questionable, upon very rational grounds. And though perhaps I should not have brought in some of the experiments mentioned in the preceding notes, if I had not had a mind to throw together what I thought might contribute to so useful a thing, as a natural history of chymical principles, on which others, if not I, may hereafter ground a theory of them; yet this may also deserve to be considered, that if any of the foregoing experiments, though never so few, do really prove (as it is like some of them will be judged to do) that any one of the chymical principles may be, *de novo*, produced; that alone will suffice to destroy the universality and intireness of their hypothesis; and besides give cause to suspect, that by further industry, the producibleness of other principles also may be discovered.

EX-

EXPERIMENTS

AND

CONSIDERATIONS

TOUCHING

COLOURS.

First occasionally written, among some other *Essays*, to a Friend; and now suffered to come abroad as

The BEGINNING of an

EXPERIMENTAL HISTORY of COLOURS.

The PREFACE.

HAVING, in convenient places of the following treatise, mentioned the motives, that induced me to write it, and the scope I proposed to myself in it, I think it superfluous to entertain the reader now with what he will meet with hereafter. And I should judge it needless, to trouble others, or myself, with any thing of preface; were it not, that I can scarce doubt, but this book will fall into the hands of some readers, who being unacquainted with the difficulty of attempts of this nature, will think it strange that I should publish any thing about colours, without a particular theory of them. But I dare expect, that intelligent and equitable readers will consider on my behalf, that the professed design of this treatise is to deliver things rather historical than dogmatical, and consequently, if I have added divers new speculative considerations and hints which perhaps may afford no despicable assistance towards

towards the framing of a ſolid and comprehenſive hypotheſis, I have done at leaſt as much as I promiſed, or as the nature of my undertaking exacted. But another thing there is, which if it ſhould be objected, I fear I ſhould not be able ſo eaſily to anſwer it; and that is, that in the following treatiſe (eſpecially in the third part of it) the experiments might have been better marſhalled, and ſome of them delivered in fewer words. For I muſt confeſs, that this Eſſay was written to a private friend, and that too by ſnatches, at ſeveral times and places, and (after my manner) in looſe ſheets, of which I oftentimes had not all by me that I had already written, when I was writing more; ſo that it needs be no wonder if all the experiments be not ranged to the beſt advantage, and if ſome connexions and conſecutions of them might eaſily have been mended: eſpecially ſince, having careleſsly laid by the looſe papers, for ſeveral years after they were written, when I came to put them together to diſpatch them to the preſs, I found ſome of thoſe I reckoned upon, to be very unſeaſonably wanting. And to make any great change in the order of the reſt was more than the printer's importunity, and that of my own avocations (and perhaps alſo conſiderable ſollicitations) would permit. But though ſome few preambles of the particular experiments might have (perchance) been ſpared, or ſhortened, if I had had all my papers under my view at once; yet in the moſt of thoſe introductory paſſages, the reader will (I hope) find hints, or advertiſements, as well as tranſitions. If I ſometimes ſeem to inſiſt long upon the circumſtances of a trial, I hope I ſhall be eaſily excuſed by thoſe, that both know how nice divers experiments of colours are, and conſider, that I was not barely to relate them, but ſo as to teach a young gentleman to make them. And if I was not ſollicitous to make a nicer diviſion of the whole treatiſe, than into three parts, whereof the one contains ſome conſiderations about colours in general; the other exhibits a ſpecimen of an account of particular colours, exemplified in whiteneſs and blackneſs; and the third, promiſcuous experiments about the remaining colours (eſpecially red) in order to a theory of them: if, I ſay, I contented myſelf with this eaſy diviſion of my diſcourſe, it was perhaps, becauſe I did not think it ſo neceſſary to be curious about the method or contrivance of a treatiſe, wherein I do not pretend to preſent my reader with a compleat fabric, or ſo much as model; but only to bring in materials proper for the building. And if I did not well know, how ingenious the curioſity and civility of friends makes them, to perſuade men by ſpecious allegations, to gratify their deſires; I ſhould have been made to believe by perſons very well qualified to judge of matters of this nature, that the following experiments will not need the addition of accurate method and ſpeculative notions to procure acceptance for the treatiſe that contains them. For it hath been repreſented, that in moſt of them, as the novelty will make them ſurprizing, and the quickneſs of performance keep them from being tedious; ſo the ſenſible changes, that are effected by them, are ſo manifeſt, ſo great, and ſo ſudden, that ſcarce any will be diſpleaſed to ſee them, and thoſe that are any thing curious will ſcarce be able to ſee them, without finding themſelves excited to make reflections upon them. But though with me, who love to meaſure phyſical things by their uſe, not their ſtrangeneſs, or prettineſs, the partiality of others prevails not to make me over-value theſe, or look upon them in themſelves as other than trifles; yet I confeſs, that ever ſince I did divers years ago ſhew ſome of them to a learned company of *Virtuoſi*, ſo many perſons of differing conditions, and even ſexes, have been curious to ſee them, and pleaſed not to diſlike them, that I cannot deſpair, but that by complying with thoſe that urge the publication of them, I may both gratify and excite the curious, and lay perhaps a foundation, whereon either others or myſelf may in

in time superstruct a substantial theory of colours. And if *Aristotle*, after his master *Plato*, have rightly observed admiration to be the parent of philosophy, the wonder, some of these trifles have been wont to produce in all sorts of beholders, and the access they have sometimes gained even to the closets of ladies, seem to promise, that since the subject is so pleasing, that the speculation appears as delightful as difficult, such easy and recreative experiments, which require but little time, or charge, or trouble in the making, and when made are sensible and surprizing enough, may contribute more than others (far more important, but as much more difficult) to recommend those parts of learning (chymistry and corpuscular philosophy) by which they have been produced, and to which they give testimony even to such kind of persons, as value a pretty trick more than a true notion, and would scarce admit philosophy, if it approached them in another dress. Without the strangeness or endearments of pleasantness to recommend it, I know, that I do but ill consult my own advantage in the consenting to the publication of the following treatise: for those things, which, whilst men knew not how they were performed, appeared so strange, will, when the way of making them, and the grounds on which I devised them, shall be public, quickly lose all, that their being rarities, and their being thought mysteries, contributed to recommend them. But it is fitter for mountebanks than naturalists to desire to have their discoveries rather admired than understood; and for my part I had much rather deserve the thanks of the ingenious, than enjoy the applause of the ignorant. And if I can so far contribute to the discovery of the nature of colours, as to help the curious to it, I shall have reached my end, and saved myself some labour, which else I may chance to be tempted to undergo in prosecuting that subject, and adding to this treatise, which I therefore call a history, because it chiefly contains matters of fact, and which history the title declares me to look upon but as begun. Because though that above a hundred, not to say a hundred and fifty experiments (some loose, and others interwoven amongst the discourses themselves) may suffice to give a beginning to a history not hitherto, that I know, begun by any; yet the subject is so fruitful, and so worthy, that those, who are curious of these matters, will be far more wanting to themselves than I can suspect, if what I now publish prove any more than a beginning. For, as I hope my endeavours may afford them some assistance towards this work, so those endeavours are too much unfinished to give them any discouragement, as if there were little left for others to do towards the history of colours.

For (first) I have been willing to leave unmentioned the most part of those phænomena of colours, that nature presents us of her own accord (that is, without being guided or over-ruled by man;) such as the different colours, that several sorts of fruits pass through before they are perfectly ripe, and those that appear upon the fading of flowers and leaves, and the putrefaction (and its several degrees) of fruits, *&c.* together with a thousand other obvious instances of the changes of colours. Nor have I much meddled with those familiar phænomena, wherein man is not an idle spectator; such as the greenness produced by salt in beef much powdered, and the redness produced in the shells of lobsters upon the boiling of those fishes: for I was willing to leave the gathering of observations to those, that have not the opportunity to make experiments. And for the same reasons, among others, I did purposely omit the lucriferous practice of tradesmen about colours; as the ways of making pigments, of blanching wax, of dying scarlet, *&c.* though to divers of them I be not a stranger, and of some I have myself made trial.

NEXT; I did purposely pass by divers experiments of other writers that I had made trial of (and that not without registring some of their events) unless I could some way or other improve them; because I wanted leisure to insert them, and had thoughts of prosecuting the work once begun, of laying together those I had examined by themselves, in case of my not being prevented by others diligence. So that there remains not a little, among the things that are already published, to employ those, that have a mind to exercise themselves in repeating and examining them. And I will not undertake, that none of the things delivered, even in this treatise, though never so faithfully set down, may not prove to be thus far of this sort, as to afford the curious somewhat to add about them. For I remember, that I have somewhere in the book itself acknowledged, that having written it by snatches, partly in the country and partly at unseasonable times of the year, when the want of fit instruments, and of a competent variety of flowers, salts, pigments, and other materials, made me leave some of the following experiments (especially those about emphatical colours) far more unfinished than they should have been, if it had been as easy for me to supply what was wanting to compleat them, as to discern. Thirdly, to avoid discouraging the young gentleman I call *Pyrophilus*, whom the less familiar and more laborious operations of chymistry would probably have frighted, I purposely declined, in what I writ to him, the setting down any number of such chymical experiments, as by being very elaborate or tedious, would either require much skill, or exercise his patience. And yet that this sort of experiments is exceedingly numerous, and might more than a little enrich the history of colours, those that are versed in chymical processes will, I presume, easily allow me.

AND (lastly) for as much as I have occasion more than once in my several writings to treat either purposely or incidentally of matters relating to colours, I did not, perhaps, conceive myself obliged to deliver in one treatise all that I would say concerning that subject.

BUT to conclude, by summing up what I would say concerning what I have, and what I have not done, in the following papers; I shall not (on the one side) deny, that considering, that I pretended not to write an accurate treatise of colours, but an occasional essay to acquaint a private friend with what then occurred to me of the things I had thought or tried concerning them; I might presume I did enough for once, if I did clearly and faithfully set down, though not all the experiments I could, yet at least such a variety of them, that an attentive reader, that shall consider the grounds on which they have been made, and the hints that are purposely (though dispersedly) couched in them, may easily compound them, and otherwise vary them, so as very much to increase their number. And yet (on the other side) I am so sensible both of how much I have, either out of necessity or choice, left undone, and of the fruitfulness of the subject I have begun to handle; that though I had performed far more than it is like many readers will judge I have, I should yet be very free to let them apply to my attempts that of *Seneca*, where having spoken of the study of nature's mysteries, and particularly of the cause of earthquakes, he subjoins; *Nulla res consummata est dum incipit. Nec in hac tantum re omnium maxima ac involutissimâ, in quâ etiam cum multum actum erit, omnis ætas, quod agat, inveniet; sed in omni alio negotio, longè semper à perfecto fuere principia.* L. Anæi Senecæ Natur. Quæst. l. 6. c. 5.

The Publisher to the Reader.

FRIENDLY READER,

HERE is presented to thy view one of the abstrusest as well as the genteelest subjects of natural philosophy, the *Experimental History of Colours*; which, though the noble author be pleased to think but begun, yet I must take leave to say, that I think it so well begun, that the work is more than half dispatched. Concerning which I cannot but give this advertisement to the reader, that I have heard the author express himself, that it would not surprize him, if it should happen to be objected, that some of these experiments have been already published, partly by chymists, and partly by two or three very fresh writers upon other subjects. And though the number of these experiments be but very small, and though they be none of the considerablest, yet it may on this occasion be further represented, that it is easy for our author to name several men (of whose number I can truly name myself) who remember either their having seen him make, or their having read his accounts of the experiments delivered in the following tract several years since, and long before the publication of the books, wherein they are mentioned. Nay, in divers passages (where he could do it without any great inconvenience) he hath struck out experiments, which he had tried many years ago, because he since found them divulged by persons, from whom he had not the least hint of them. Which yet is not touched, with design to reflect upon any ingenious man, as if he were a plagiary: for, though our generous author were not reserved enough in shewing his experiments to those that expressed a curiosity to see them (amongst whom a very learned man hath been pleased publickly to acknowledge it several years *;) yet the same thing may be well enough lighted on by persons, that know nothing of one another. And especially chymical laboratories may many times afford the same phænomenon, about colours, to several persons, at the same or differing times. And as for the few phænomena mentioned in the same chymical writers, as well as in the following treatise, our author hath given an account, why he did not decline rejecting them in the annotations upon the 47th experiment of the third part. Not here to mention, what he elsewhere saith, to shew what use may be justifiably made of experiments not of his own devising by a writer of natural history, if, what he employs of other mens, be well examined or verified by himself.

IN the mean time, this treatise is such, that there needs no other invitation to peruse it, but that it is composed by one of the deepest and most indefatigable searchers of nature, which, I think the world, as far as I know it, affords. For mine own part, I feel a secret joy within me, to see such beginnings upon such themes, it being demonstratively true, *mota facilius moveri*; which causeth me to entertain strong hopes, that this illustrious virtuoso and restless inquirer into nature's secrets will not stop here, but go on and prosper in the disquisition of the other principal colours,

* He that desires more instances of this kind and matter, that according to this doctrine may much help the theory of colours, and particularly the force both of sulphureous and volatile, as likewise of alkalizate and acid salts, and in what particulars colours likely depend not in their causation from any salt at all; may beg his information from Mr. *Boyle*, who hath some while since honoured me with the sight of his papers concerning this subject, containing many excellent experiments, made by him for the elucidation of this doctrine, *&c.* Dr. *R. Sharrock*, in his ingenious and useful *History of the Propagation and Improvement of Vegetables*, published in the year 1660.

green,

green, red, and yellow. The reasoning faculty set once afloat will be carried on, and that with ease; especially, when the productions thereof meet, as they do here, with so greedy an entertainment at home and abroad. I am confident, that the ROYAL SOCIETY, lately constituted by his most Excellent Majesty *for improving Natural Knowledge* will judge it their interest to exhort our author to the prosecution of this argument; considering, how much it is their design and business to accumulate a good stock of such accurate observations and experiments, as may afford them and their offspring genuine matter to raise a masculine philosophy upon, whereby the mind of man may be ennobled with the knowledge of solid truths, and the life of man benefited with ampler accommodations, than it hath been hitherto.

OUR great author, one of the pillars of that illustrious corporation, is constantly furnishing large symbolas to this work; and is now fallen, as you see, upon so comprehensive and important a theme, as will, if insisted on and compleated, prove one of the considerablest pieces of that structure. To which if he shall please to add his treatise of heat and flame, as he is ready to publish his experimental accounts of cold; I esteem, the world will be obliged to him for having shewed them both the right and left-hand of nature, and the operations thereof.

THE considering reader will by this very treatise see abundant cause to sollicit the author for more. Sure I am, that whatever of the productions of his ingeny comes into foreign parts (where I am happy in the acquaintance of many intelligent friends) is highly valued; and to my knowledge, there are those among the French, that have lately begun to learn English, on purpose to enable themselves to read his books, being impatient of their traduction into Latin. If I durst say all I know of the elogies received by me from abroad concerning him, I should perhaps make this preamble too prolix, and certainly offend the modesty of our author.

WHEREFORE I shall leave this, and conclude with desiring the reader, that if he meet with other faults besides those that the Errata take notice of (as I believe he may) he will please to consider both the weakness of the author's eyes for not reviewing, and the manifold avocations of the publisher for not doing his part; who taketh his leave with inviting those, that have also considered this nice subject experimentally, to follow the example of our noble author, and impart such and the like performances to the now very inquisitive world. Farewel.

I *H. O.*

THE EXPERIMENTAL HISTORY OF COLOURS BEGUN.

THE FIRST PART.

CHAP. I.

1. I HAVE ſeen you ſo paſſionately addicted, *Pyrophilus*, to the delightful art of limning and painting, that I cannot but think myſelf obliged to acquaint you with ſome of thoſe things that have occurred to me concerning the changes of colours. And I may expect that I ſhall as well ſerve the Virtuoſi in general, as gratify you in particular, by furniſhing a perſon, who, I hope, will both improve my communications, and communicate his improvements, with ſuch experiments and obſervations as may both invite you to enquire ſeriouſly into the nature of colours, and aſſiſt you in the inveſtigation of it. This being the principal ſcope of the following tract, I ſhould do that which might prevent my own deſign, if I ſhould here attempt to deliver you an accurate and particular theory of colours; for that were to preſent you with what I deſire to receive from you; and, as far as in me lay, to make that ſtudy needleſs, to which I would engage you.

2. Wherefore my preſent work ſhall be but to divert and recreate, as well as excite you by the delivery of matters of fact, ſuch as you may for the moſt part try with much eaſe, and poſſibly not without ſome delight. And leſt you ſhould expect any thing of elaborate or methodical in what you will meet with here, I muſt confeſs to you before-hand, that the ſeaſons I was wont to chuſe to deviſe and try experiments about colours, were thoſe days, wherein having taken phyſic, and finding myſelf as unfit to ſpeculate, as unwilling to be altogether idle, I choſe this diverſion as a kind of mean betwixt the one and the other. And I have the leſs ſcrupled to ſet down the following experiments, as ſome of them came to my mind, and as the notes wherein I had ſet down the reſt, occurred to my hands; that by declining a methodical way of delivering them, I might leave you and myſelf the greater liberty and convenience to add to them, and tranſpoſe them as ſhall appear expedient.

3. Yea, that you may not think me too reſerved, or look upon an inquiry made up of mere narratives, as ſomewhat jejune, I am content to premiſe a few conſiderations,

rations, that now offer themſelves to my thoughts, which relate in a more general way, either to the nature of colours, or to the ſtudy of it. And I ſhall inſert an eſſay, as well ſpeculative as hiſtorical, of the nature of whiteneſs and blackneſs, that you may have a ſpecimen of the hiſtory of colours, I have ſometimes had thoughts of; and if you diſlike not the method I have made uſe of, I hope you, and ſome of the Virtuoſi your friends, may be thereby invited to go thorough with red, blue, yellow, and the reſt of the particular colours, as I have done with white and black, but with far more ſagacity and ſucceſs. And if I can invite ingenious men to undertake ſuch taſks, I doubt not but the curious will quickly obtain a better account of colours, than as yet we have, ſince in our method the theorical part of the inquiry being attended, and as it were interwoven with the hiſtorical conjectures, the philoſophy of colours will be promoted by the indiſputable experiments.

CHAP. II.

1. TO come then in the firſt place to our more general conſiderations, I ſhall begin with ſaying ſomething as to the importance of examining the colours of bodies. For there are ſome, eſpecially chymiſts, who think that a conſiderable diverſity of colours does conſtantly argue an equal diverſity of nature, in the bodies wherein it is conſpicuous; but I confeſs I am not altogether of their mind: for not to mention changeable taffaties, the blue and golden necks of pigeons, and divers water-fowl, rainbows natural and artificial, and other bodies, whoſe colours the philoſophers have been pleaſed to call not real, but apparent and fantaſtical; not to inſiſt on theſe, I ſay (for fear of needleſsly engaging in a controverſy) we ſee in parrots, goldfinches, and divers other birds, not only that the contiguous feathers which are probably as near in properties as place, are ſome of them red, and others white, ſome of them blue, and others yellow, *&c.* but that in the ſeveral parts of the ſelf-ſame feather there may often be ſeen the greateſt diſparity of colours. And ſo in the leaves of tulips, july-flowers, and ſome other vegetables, the ſeveral leaves, and even the ſeveral parts of the ſame leaf, although no difference have been obſerved in their other properties, are frequently found painted with very different colours. And ſuch a variety we have much more admired in that lovely plant which is commonly, and not unjuſtly called the *Marvel of Peru*; for of divers ſcores of fine flowers, which in its ſeaſon that gaudy plant does almoſt daily produce, I have ſcarce taken notice of any two that were dyed perfectly alike. But though, *Pyro*, ſuch things as theſe, among others, keep me from daring to affirm that the diverſity and change of colours does always argue any great difference or alteration betwixt, or in the bodies, wherein it is to be diſcerned; yet that oftentimes the alteration of colours does ſignify conſiderable alterations in the diſpoſition of parts of bodies, may appear in the extraction of tinctures, and divers other chymical operations, wherein the change of colours is the chief, and ſometimes the only thing, by which the artiſt regulates his proceeding; and is taught to know when 'tis ſeaſonable for him to leave off. Inſtances of this ſort are more obvious in divers ſorts of fruits, as cherries, plums, *&c.* wherein, according as the vegetable ſap is ſweetned, or otherwiſe ripened, by paſſing from one degree to another of maturation, the external part of the fruit paſſes likewiſe from one to another colour. But one of the nobleſt inſtances I have met with of this kind, is not ſo obvious; and that is the way of tempering ſteel to make gravers, drills, ſprings, and other mechanical inſtruments, which we have divers times both made artificers practiſe in our preſence, and tried ourſelves after the following

following manner. First, the slender steel to be tempered is to be hardened by heating as much of it as is requisite among glowing coals, till it be glowing hot, but it must not be quenched as soon as it is taken from the fire (for that would make it too brittle, and spoil it) but must be held over a bason of water, till it descend from a white heat to a red one, which as soon as ever you perceive, you must immediately quench as much as you desire to harden in the cold water. The steel thus hardened will, if it be good, look somewhat white, and must be made bright at the end, that its change of colours may be there conspicuous; and then holding it so in the flame of a candle, that the bright end may be, for about half an inch or more, out of the flame, that the smoak do not stain or sully the brightness of it, you shall after a while see that clean end, which is almost contiguous to the flame, pass very nimbly from one colour to another, as from a brighter yellow, to a deeper and reddish yellow, which artificers call a sanguine; and from that to a fainter first, and then a deeper blue. And to bring home this experiment to our present purpose, it is found by daily experience, that each of these succeeding colours argue such a change made in the texture of the steel, that if it be taken from the flame, and immediately quenched in the tallow (whereby it is settled in whatever temper it had before) when it is yellow, it is of such a hardness as makes it fit for gravers, drills, and such like tools; but if it be kept a few minutes longer in the flame till it grow blue, it becomes much softer, and unfit to make gravers for metals, but fit to make springs for watches, and such like instruments, which are therefore commonly of that colour: and if the steel be kept in the flame, after this deep blue hath disclosed itself, it will grow so soft, as to need to be new hardened again, before it can be brought to a temper fit for drills or penknives. And I confess, *Pyro*, I have taken much pleasure to see the colours run along from the parts of the steel contiguous to the flame, to the end of the instrument, and succeed one another so fast, that if a man be not vigilant, to thrust the steel into the tallow at the very nick of time, at which it has attained its due colour, he shall miss of giving his tool the right temper. But because the flame of a candle is offensive to my weak eyes, and because it is apt to either black or sully the contiguous part of the steel which is held in it, and thereby hinder the change of colours from being so long and clearly discerned, I have sometimes made this experiment by laying the steel to be tempered upon a heated bar of iron, which we find also to be employed by some artificers in the tempering of such great instruments, as are too big to be soon heated sufficiently by the flame of a candle. And you may easily satisfy yourself, *Pyro*, of the differing hardness and toughness, which is ascribed to steel tempered at different colours, if you break but some slender wires of steel so tempered, and observe how they differ in brittleness, and if with a file you also make trial of their various degrees of hardness.

2. BUT, *Pyrophilus*, I must not at present any further prosecute the consideration of the importance of experiments about colours, not only because you will in the following papers find some instances, that would here be presented you out of their due place, of the use that may be made of such experiments, in discovering in divers bodies what kind the salt is, that is predominant in them; but also because a speculative Naturalist might justly enough allege, that as light is so pleasing an object, as to be well worth our looking on, though it discovered to us nothing but itself; so modified light, called colour, were worth our contemplation, though by understanding its nature we should be taught nothing else. And however, I need not make either you or myself excuses for entertaining you on the subject I am now about to treat of; since the pleasure *Pyro* takes in mixing and laying on of colours, will I presume keep him,

him, and will (I am sure) keep me from thinking it troublesome to set down, especially after the tedious processes (about other matters) wherewith I fear I may have tired him, some easy, and not unpleasant experiments relating to that subject.

3. But, before we descend to the more particular considerations we are to present you concerning colours, I presume it will be seasonable to propose at the very entrance a distinction; the ignorance or neglect of which, seems to me to have frequently enough occasioned either mistakes or confusion in the writings of divers modern philosophers. For colour may be considered, either as it is a quality residing in the body that is said to be coloured, or to modify the light after such or such a manner; or else as the light itself, which so modified, strikes upon the organ of sight, and so causes that sensation which we call colour: and that this latter may be looked upon as the more proper, though not the usual acceptation of the word colour, will be made probable by divers passages in the ensuing part of our discourse. And indeed it is the light itself, which after a certain manner, either mingled with shades or some other ways troubled, strikes our eyes, that does more immediately produce that motion in the organ, upon whose account men say they see such or such a colour in the object: yet, because there is in the body that is said to be coloured, a certain disposition of the superficial particles, whereby it sends the light reflected, or refracted, to our eyes thus and thus altered, and not otherwise, it may also in some sense be said, that colour depends upon the visible body; and therefore we shall not be against that way of speaking of colours, that is most used among the modern Naturalists, provided we be allowed to have recourse, when occasion shall require, to the premised distinction, and to take the more immediate cause of colour to be the modified light itself, as it affects the sensory; though the disposition also of the coloured body, as that modifies the light, may be called by that name metonymically (to borrow a school-term) or efficiently, that is, in regard of its turning the light, that rebounds from it, or passes through it, into this or that particular colour.

4. I know not whether I may not on this occasion add, that colour is so far from being an inherent quality of the object in the sense that is wont to be declared by the schools, or even in the sense of some modern Atomists, that, if we consider the matter more attentively, we shall see cause to suspect, if not to conclude, that though light do more immediately affect the organ of sight, than do the bodies that send it thither, yet light itself produces the sensation of a colour, but as it produces such a determinate kind of local motion in some part of the brain; which, though it happen most commonly from the motion whereinto the slender string of the retina are put, by the appulse of light; yet if the like motion happen to be produced by any other cause, wherein the light concurs not at all, a man shall think he sees the same colour. For proof of this, I might put you in mind, that it is usual for dreaming men to think they see the images that appear to them in their sleep, adorned some with this, and some with that lively colour, whilst yet, both the curtains of their bed, and those of their eyes, are close drawn. And I might add the confidence with which distracted persons do oftentimes, when they are awake, think they see black fiends in places, where there is no black object in sight without them. But I will rather observe, that not only when a man receives a great stroke upon his eye, or a very great one upon some other part of his head, he is wont to see, as it were, flashes of lightning, and little vivid, but vanishing flames, though perhaps his eyes be shut: but the like apparitions may happen, when the motion proceeds not from something without, but from something within the body, provided the unwonted fumes that wander up and down in the head, or the propagated concussion of any internal part in the body, do cause,

cause, about the inward extremities of the optic nerve, such a motion as is wont to be there produced, when the stroke of the light upon the retina makes us conclude, that we see either light or such and such a colour. This the most ingenious *Des Cartes* hath very well observed; but because he seems not to have exemplified it by any unobvious or peculiar observation, I shall endeavour to illustrate this doctrine by a few instances.

5. AND first, I remember, that having, through God's goodness, been free for several years from troublesome coughs, being afterwards, by an accident, suddenly cast into a violent one, I did often when I was awaked in the night by my distempers, observe, that upon coughing strongly, it would seem to me, that I saw very vivid, but immediately disappearing flames; which I took particular notice of, because of the conjecture I am now mentioning.

6. AN excellent and very discreet person, very near allied both to you and me, was relating to me, that some time since, whilst she was talking with some other ladies, upon a sudden, all the objects she looked upon appeared to her dyed with unusual colours, some of one kind, and some of another, but all so bright and vivid, that she should have been as much delighted, as surprized with them; but that finding the apparition to continue, she feared it portended some very great alteration as to her health: and indeed, the day after she was assaulted with such violence by hysterical and hypocondrical distempers, as both made her rave for some days, and gave her, during that time, a bastard palsy.

7. BEING a while since in a town, where the plague had made great havock, and inquiring of an ingenious man, that was so bold, as without much scruple to visit those that were sick of it, about the odd symptoms of a disease that had swept away so many there; he told me, among other things, that he was able to tell divers patients, to whom he was called, before they took their beds, or had any evident symptoms of the plague, that they were indeed infected, upon peculiar observations, that being asked, they would tell him that the neighbouring objects, and particularly his clothes, appeared to them beautified with most glorious colours, like those of the rainbow, oftentimes succeeding one another: and this he affirmed to be one of the most usual, as well as the most early symptoms, by which this odd pestilence disclosed itself. And when I asked how long the patients were wont to be thus affected, he answered, that it was most commonly for about a day; and when I further inquired whether or no vomits, which in that pestilence were usually given, did not remove this symptom (for some used the taking of a vomit, when they came ashore, to cure themselves of the obstinate and troublesome giddiness caused by the motion of the ship) he replied, that generally, upon the evacuation made by the vomit, that strange apparition of colours ceased, though the other symptoms were not so soon abated; yet he added (to take notice of that upon the by, because the observation may perchance do good) that an excellent physician, in whose company he was wont to visit the sick, did give to almost all those to whom he was called, in the beginning, before nature was much weakened, a pretty odd vomit, consisting of eight or ten drachms of infusion of *Crocus Metallorum*, and about half a drachm, or much more, of white vitriol, with such success, that scarce one of ten to whom it was seasonably administred, miscarried.

8. BUT to return to the consideration of colours: as an apparition of them may be produced by motions from within, without the assistance of an outward object; so I have observed, that it is sometimes possible that the colour that would otherwise be produced by an outward object, may be changed by some motion, or new texture already

already produced in the sensory, as long as that unusual motion, or new disposition lasts; for I have divers times tried, that after I have through a telescope looked upon the sun, though thorough a thick, red, or blue glass, to make its splendor supportable to the eye, the impression upon the retina would be not only so vivid, but so permanent, that if afterwards I turned my eye towards a flame, it would appear to me of a colour very differing from its usual one. And if I did divers times successively shut and open the same eye, I should see the adventitious colour (if I may so call it) changed or impaired by degrees, till at length (for this unusual motion of the eye would not presently cease) the flame would appear to me of the same hue that it did to other beholders. A not unlike effect I found by looking upon the moon, when she was near full, thorough an excellent telescope, without coloured glass to screen my eye with: but that which I desire may be taken notice of, because we may elsewhere have occasion to reflect upon it, and because it seems not agreeable to what Anatomists and optical writers deliver, touching the relation of the two eyes to each other, is this circumstance; that though my right eye, with which I looked thorough the telescope, were thus affected by the over-strong impression of the light, yet when the flame of a candle, or some other bright object appeared to me of a very unusual colour, whilst looked upon with the discomposed eye; or (though not so notably) with both eyes at once; yet if I shut that eye, and looked upon the same object with the other, it would appear with no other than its usual colour, though if I again opened, and made use of the dazzled eye, the vivid adventitious colour would again appear. And on this occasion I must not pretermit an observation which may persuade us, that an over-vehement stroke upon the sensory, especially if it be naturally of a weak constitution, may make a more lasting impression than one would imagine; which impression may in some cases, as it were, mingle with, and vitiate the action of vivid objects for a long time after.

For I know a lady of unquestionable veracity, who having lately, by a desperate fall, received several hurts, and particularly a considerable one upon a part of her face near her eye, had her sight so troubled and disordered, that, as she hath more than once related to me, not only when the next morning one of her servants came to her bed-side, to ask how she did, his clothes appeared adorned with such variety of dazzling colours, that she was fain presently to command him to withdraw; but the images in her hangings did, for many days after, appear to her, if the room were not extraordinarily darkened, embellished with several offensively vivid colours, which no body else could see in them. And when I enquired whether or no white objects did not appear to her adorned with more luminous colours than others, and whether she saw not some which she could not well describe to any, whose eyes had never been distempered, she answered me, that sometimes she thought she saw colours so new and glorious, that they were of a peculiar kind, and such as she could not describe by their likeness to any she had beheld either before or since; and that white objects did so much disorder her sight, that if, several days after her fall, she looked upon the inside of a book, she fancied she saw there colours like those of the rainbow: and even when she thought herself pretty well recovered, and made bold to leave her chamber, the coming into a place where the walls and ceiling were whited over, made those objects appear to her cloathed with such glorious and dazzling colours, as much offended her sight, and made her repent her venturousness. And she added, that this distemper of her eyes lasted not less than five or six weeks, though since that, she hath been able to read and write much without finding the least inconvenience in doing so. I would gladly have known, whether if she had shut the injured

eye, the phænomena would have been the same, when she employed only the other; but I heard not of this accident early enough to satisfy that inquiry.

9. WHEREFORE, I shall now add, that some years before, a person exceedingly eminent for his profound skill in almost all kinds of philological learning, coming to advise with me about a distemper in his eyes, told me, among other circumstances of it, that having upon a time looked too fixedly upon the sun, thorough a telescope, without any coloured glass, to take off from the dazzling splendor of the object, the excess of light did so strongly affect his eye, that ever since, when he turns it towards a window, or any white object, he fancies he seeth a globe of light, of about the bigness the sun then appeared of to him, to pass before his eyes: and having inquired of him, how long he had been troubled with this indisposition, he replied, that it was already nine or ten years since the accident, that occasioned it, first befel him.

10. I COULD here subjoin, *Pyrophilus*, some memorable relations that I have met with in the account given us by the experienced *Epiphanius Ferdinandus*, of the symptoms he observed to be incident to those that are bitten with the Tarantula; by which (relations) I could probably shew, that without any change in the object, a change in the instruments of vision may for a great while make some colours appear charming, and make others provoking, and both to a high degree, though neither of them produced any such effects before. These things, I say, I could here subjoin in confirmation of what I have been saying, to shew that the disposition of the organ is of great importance in the dijudications we make of colours, were it not that these strange stories belonging more properly to another discourse, I had rather (contenting myself to have given you an intimation of them here) that you should meet with them fully delivered there.

CHAP. III.

1. BUT, *Pyrophilus*, I would not, by all that I have hitherto discoursed, be thought to have forgotten the distinction (of colour) that I mentioned to you about the beginning of the third section of the former chapter; and therefore, after all I have said of colour, as it is modified light, and immmediately affects the sensory, I shall now remind you, that I did not deny, but that colour might in some sense be considered as a quality residing in the body that is said to be coloured; and indeed the greatest part of the following experiments refer to colour principally under that notion, for there is in the bodies we call coloured, and chiefly in their superficial parts, a certain disposition, whereby they do so trouble the light that comes from them to our eye, as that it there makes that distinct impression, upon whose account we say, that the seen body is either white or black, or red or yellow, or of any one determinate colour. But because we shall (God permitting) by the experiments that are to follow some pages hence, more fully and particularly shew, that the changes, and consequently in divers places the production and the appearance of colours, depends upon the continuing or altered texture of the object; we shall in this place intimate (and that too but as by the way) two or three things about this matter.

2. AND first, it is not without some reason, that I ascribe colour (in the sense formerly explained) chiefly to the superficial parts of bodies; for not to question how much opacous corpuscles may abound even in those bodies we call diaphanous, it seems plain that of opacous bodies we do indeed see little else than the superficies. For if we found the beams of light that rebound from the object to the eye, to pierce deep into the coloured body, we should not judge it opacous, but either transfucid, or

or at least semi-diaphanous: and though the schools seem to teach us that colour is a penetrative quality, that reaches to the innermost parts of the object, as if a piece of sealing-wax be broken into never so many pieces, the internal fragments will be as red as the external surface did appear; yet that is but a particular example, that will not overthrow the reason lately offered, especially since I can allege other examples of a contrary import, and two or three negative instances are sufficient to overthrow the generality of a positive rule, especially if that be built but upon one or a few examples. Not (then) to mention cherries, plums, and I know not how many other bodies, wherein the skin is of one colour, and what it hides of another, I shall name a couple of instances drawn from the colours of durable bodies that are thought far more homogeneous, and have not parts that are either organical, or of a nature approaching thereunto.

3. To give you the first instance, I shall need but to remind you of what I told you a little after the beginning of this essay, touching the blue and red and yellow, that may be produced upon a piece of tempered steel; for these colours, though they be very vivid, yet if you break the steel they adorn, they will appear to be but superficial; not only the innermost parts of the metal, but those that are within a hair's breadth of the superficies, having not any of these colours, but retaining that of the steel it self. Besides that, we may as well confirm this observation, as some other particulars we elsewhere deliver concerning colours, by the following experiment which we purposely made.

4. We took a good quantity of clean lead, and melted it with a strong fire, and then immediately pouring it out into a clean vessel of a convenient shape and matter (we used one of iron, that the great and sudden heat might not injure it) and then carefully and nimbly taking off the scum that floated on the top, we perceived, as we expected, the smooth and glossy surface of the melted matter to be adorned with a very glorious colour, which being as transitory as delightful, did almost immediately give place to another vivid colour, and that was as quickly succeeded by a third, and this as it were chased away by a fourth; and so these wonderfully vivid colours successively appeared and vanished (yet the same now and then appearing the second time) till the metal ceasing to be hot enough to afford any longer this pleasing spectacle, the colours that chanced to adorn the surface, when the lead thus began to cool, remained upon it; but were so superficial, that how little soever we scraped off the surface of the lead, we did in such places scrape off all the colour, and discover only that which is natural to the metal itself; which receiving its adventitious colours, only when the heat was very intense, and in that part which was exposed to the comparatively very cold air (which by other experiments seems to abound with subtile saline parts, perhaps not uncapable of working upon lead so disposed:) these things, I say, together with my observing that whatever parts of the so strongly melted lead were exposed a while to the air, turned into a kind of scum or litharge, how bright and clean soever they appeared before, suggested to me some thoughts or ravings, which I have not now time to acquaint you with. One that did not know me, *Pyrophilus*, would perchance think I endeavoured to impose upon you by relating this experiment, which I have several times tried; but the reason why the phænomena mentioned have not been taken notice of, may be, that unless lead be brought to a much higher degree of fusion or fluidity than is usual, or than is indeed requisite to make it melt, the phænomena I mentioned will scarce at all disclose themselves; and we have also observed, that this successive appearing and vanishing of vivid colours was wont to be impaired or determined whilst the metal exposed to the air remained

yet hotter than one would readily suspect. And one thing I must further note, of which I leave you to search after the reason, namely, that the same colours did not always and regularly succeed one another, as is usual in steel, but in the diversified order mentioned in this following note, which I was scarce able to write down, the succession of the colours was so very quick: whether that proceeded from the differing degrees of heat in the lead exposed to the cool air, or from some other reason, I leave you to examine.

[*Blue, yellow, purple, blue; green, purple, blue, yellow, red; purple, blue, yellow and blue; yellow, blue, purple, green mixt; yellow, red, blue, green; yellow, red, purple, green.*]

5. THE Atomists of old, and some learned men of late, have attempted to explicate the variety of colours in opacous bodies from the various figures of their superficial parts; the attempt is ingenious, and the doctrine seems partly true: but I confess I think there are divers other things that must be taken in as concurrent to produce those differing forms of asperity, whereon the colours of opacous bodies seem to depend. To declare this a little, we must assume, that the surfaces of all such bodies, how smooth or polite soever they may appear to our dull sight and touch, are exactly smooth only in a popular, or at most in a physical sense, but not in a strict and rigid sense.

6. THIS excellent microscopes shew us in many bodies, that seem smooth to our naked eyes; and this not only as to the little hillocks or protuberances that swell above that which may be conceived to be the plane or level of the considered surface; for it is obvious enough to those that are any thing conversant with such glasses: but as to numerous depressions beneath that level, of which sort of cavities, by the help of a microscope, which the greatest artificer that makes them, judges to be the greatest magnifying glass in *Europe*, except one that equals it, we have on the surface of a thin piece of cork that appeared smooth to the eye, observed about sixty in a row, within the length of less than a 31 and 32d part of an inch (for the glass takes in no longer a space at one view;) and these cavities (which made that little piece of cork almost like an empty honey-comb) were not only very distinct, and figured like one another, but of a considerable bigness, and a scarce credible depth; insomuch that their distinct shadows as well as sides were plainly discerned and easy to be reckoned, and might have been well distinguished, though they had been ten times lesser than they were. Which I thought it not amiss to mention to you, *Pyrophilus*, upon the by, that you may thence make some estimate, what a strange inequality, and what a multitude of little shades there may really be, in a scarce sensible part of the physical superficies, though the naked eye sees no such matter. And as excellent microscopes shew us this ruggedness in many bodies that pass for smooth, so there are divers experiments, though we must not now stay to urge them, which seem to persuade us of the same thing, as to the rest of such bodies as we are now treating of; so that there is no sensible part of an opacous body, that may not be conceived to be made up of a multitude of singly insensible corpuscles. But in the giving these surfaces that disposition, which makes them alter the light that reflects thence to the eye after the manner requisite to make the object appear green, blue, *&c.* the figures of these particles have a great, but not the only stroke. It is true indeed, that the protuberant particles may be of very great variety of figures, spherical, elliptical, polyedrical, and some very irregular; and that according to the nature of these, and the situation of the lucid body, the light must be variously affected, after one manner from surfaces (I now speak of physical surfaces) consisting of spherical, and in another

other from those that are made up of conical or cylindrical corpuscles; some being fitted to reflect more of the incident beams of light, others less, and some towards one part, others towards another. But besides this difference of shape, there may be divers other things that may eminently concur to vary the forms of asperity that colours so much depend on. For, willingly allowing the figure of the particles in the first place, I consider secondly, that the superficial corpuscles, if I may so call them, may be bigger in one body, and less in another, and consequently fitted to allay the light falling on them with greater shades. Next, the protuberant particles may be set more or less close together, that is, there may be a greater or a smaller number of them within the compass of one, than within the compass of another small part of the surface of the same extent; and how much these qualities may serve to produce colour, may be somewhat guessed at, by that which happens in the agitation of water: for if the bubbles that are thereby made be great, and but few, the water will scarce acquire a sensible colour; but if it be reduced to a froth, consisting of bubbles, which being very minute and contiguous to each other, are a multitude of them crouded into a narrow room, the water (turned to froth) does then exhibit a very manifest white colour, to which these last named conditions of the bubbles do, as well as their convex figure, contribute; and that for reasons to be mentioned anon. Besides, it is not necessary that the superficial particles that exhibit one colour should be all of them round, or all conical, or all of any one shape; but corpuscles of differing figures may be mingled on the surface of the opacous body, as when the corpuscles that make a blue colour, and those that make a yellow, come to be accurately and skilfully mixed, they make up a green; which, though it seem one simple colour, yet, in this case, appears to be made by corpuscles of very differing kinds, duly commixed. Moreover, the figure and bigness of the little depressions, cavities, furrows, or pores intercepted betwixt these protuberant corpuscles, are as well to be considered as the sizes and shapes of the corpuscles themselves: for we may conceive the physical superficies of a body, where (as we said) its colour does, as it were, reside, to be cut transversely by a mathematical plane, which you know is conceived to be without any depth or thickness at all; and then, as some parts of the physical superficies will be protuberant, or swell above this last plane, so others may be depressed beneath it, as (to explain myself by a gross comparison) in divers places of the surface of the earth, there are not only neighbouring hills, trees, *&c.* that are raised above the horizontal level of the valley, but rivers, wells, pits and other cavities that are depressed beneath it. And that such protuberant and concave parts of a surface may remit the light so differingly, as much to vary a colour, some examples, and other things that we shall hereafter have occasion to take notice of in this tract, will sufficiently declare; till when, it may suffice to put you in mind, that of two flat sides of the same piece of, for example, red marble, the one being diligently polished, and the other left to its former roughness, the differing degrees or sorts of asperity, for the side that is smooth to the touch wants not its roughness, will so diversify the light reflected from the several planes to the eye, that a painter would employ two differing colours to represent them.

See the Discourse of the nature of whiteness and blackness.

7. And I hope, *Pyrophilus*, you will not think it strange or impertinent, that I employ, in divers passages of these papers, examples drawn from bodies and shadows far more gross than those minute protuberances and shady pores on which, in most cases, the colour of a body, as it is an inherent quality or disposition of its surface, seems to depend. For sometimes I employ such examples, rather to declare my meaning, than prove my conjecture; things, whom their smallness makes insensible, being better

better represented to the imagination by such familiar objects, as being like them enough in other respects, are of a visible bulk. And next, though the beams of light are such subtile bodies, that in respect of them, even surfaces that are sensibly smooth, are not exactly so, have their own degree of roughness, consisting of little protuberances and depressions; and though consequently such inequalities may suffice to give bodies differing colours, as we see in marble that appears white or black, or red or blue, even when the most carefully polished; yet it is plain, by the late instance of red marble, and many others, that even bigger protuberances and greater shades may likewise so diversify the roughness of a body's superficies, as manifestly to concur to the varying of its colour, whereby such examples appear to be proper enough to be employed in such a subject as we have now in hand. And having hinted thus much on this occasion, I now proceed.

8. THE situation also of the superficial particles is considerable, which I distinguish into the posture of the single corpuscles, in respect of the light, and of the eye, and the order of them in reference also to one another; for a body may otherwise reflect the light, when its superficial particles are more erected upon the plane, that may be conceived to pass along their basis, and when the points or extremes of such particles are obverted to the eye, than when those particles are so inclined, that their sides are in great part discernible; as the colour of plush or velvet will appear varied to you, if you carefully stroke part of it one way, and part of it another; the posture of the particular thrids, in reference to the light, or the eye, becoming thereby different. And you may observe in a field of ripe corn blown upon by the wind, that there will appear as it were waves of a colour (at least gradually) differing from that of the rest of the field; the wind, by depressing some of the ears, and not at the same time others, making the one reflect more from the lateral and strawy parts than do the rest. And so, when dogs are so angry as to erect the hairs upon their necks, and upon some other parts of their bodies, those parts seem to acquire a colour varied from that which the same hairs made, when in their usual posture they did far more stoop. And that the order wherein the superficial corpuscles are ranged, is not to be neglected, we may guess by turning of water into froth, the beating of glass, and the scraping of horns, in which cases the corpuscles that were before so marshalled as to be perspicuous, do by the troubling of that order become disposed to terminate and reflect more light, and thereby to appear whitish. And there are other ways in which the order of the protuberant parts, in reference to the eye, may much contribute to the appearing of a particular colour; for I have often observed, that when peas are planted, or set in parallel lines, and are shot up about half a foot above the surface of the ground, by looking on the field or plot of ground from that part towards which the parallel lines tended, the greater part of the ground by far, would appear of its own dirty colour; but if I looked upon it transversely, the plot would appear very green, the upper parts of the peas hindering the intercepted parts of the ground, which, as I said, retained their wonted colour from being discovered by the eye. And I know not, *Pyrophilus*, whether I might not add, that even the motion of the small parts of a visible object may in some cases contribute, though it be not so easy to say how, to the producing, or the varying of a colour: for I have several times made a liquor which, when it has well settled in a close phial, is transparent and colourless; but as soon as the glass is unstopped, begins to fly away very plentifully in a white and opacous fume. And there are other bodies, whose fumes, when they fill a receiver, would make one suspect it contains milk; and yet when these fumes settle into a liquor, that liquor is not white, but transparent; and such white fumes I have seen afforded by unstopping a

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liquor I know, which yet is itself diaphanous and red: nor are these the only instances of this kind, that our trials can supply us with. And if the superficial corpuscles be of the grosser sort, and be so framed, that their differing sides or faces may exhibit differing colours, then the motion or rest of those corpuscles may be considerable, as to the colour of the superficies they compose; upon this account, that sometimes more, sometimes fewer of the sides disposed to exhibit such a colour may by this means become or continue more obverted to the eye than the rest, and compose a physical surface, that will be more or less sensibly interrupted. As, to explain my meaning, by proposing a gross example, I remember, that in some sorts of leafy plants thick set by one another, the two sides of whose leaves were of somewhat differing colours, there would be a notable disparity as to colour, if you looked upon them both, when the leaves, being at rest, had their upper and commonly exposed sides obverted to the eye, and when a breath of wind passing thorough them, made great numbers of the usually hidden sides of the leaves become conspicuous. And though the little bodies we were lately speaking of, may singly and apart seem almost colourless; yet when many of them are placed by one another, so near that the eye does not easily discern an interruption, within a sensible space, they may exhibit a colour: as we see, that though the slenderest thrid of dyed silk does whilst looked on single, seem almost quite devoid of redness (for instance) yet when numbers of these thrids are brought together into one skein, their colour becomes notorious.

9. But the same occasion that invited me to say what I have mentioned concerning the leaves of trees, invites me also to give you some account of what happens in changeable taffaties, where we see differing colours, as it were, emerge and vanish upon the ruffling of the same piece of silk; as I have divers times with pleasure observed, by the help of such a microscope, as though it do not very much magnify the object, has in recompence this great conveniency, that you may easily, as fast as you please, remove it from one part to another of a large object, of which the glass taking a great part at once, you may thereby presently survey the whole. Now by the help of such a microscope I could easily (as I began to say) discern, that in a piece of changeable taffaty (that appeared, for instance, sometimes red, and sometimes green) the stuff was composed of red thrids and green, passing under and over each other, and crossing one another in almost innumerable points: and if I looked through the glass upon any considerable portion of the stuff that (for example sake) to the naked eye appeared to be red, I could plainly see, that in that position, the red thrids were conspicuous, and reflected a vivid light. And though I could also perceive, that there were green ones, yet by reason of their disadvantageous position in the physical surface of the taffaty, they were in part hid by the more protuberant thrids of the other colour: and for the same cause, the reflection from as much of the green as was discovered, was comparatively but dim and faint. And if, on the contrary, I looked through the microscope upon any part that appeared green, I could plainly see that the red thrids were less fully exposed to the eye, and obscured by the green ones, which therefore made up the predominant colour. And by observing the texture of the silken stuff, I could easily so expose the thrids either of the one colour or of the other, to my eye, as at pleasure to exhibit an apparition of red or green, or make those colours succeed one another: so that, when I observed their succession by the help of the glass, I could mark how the predominant colour did as it were start out, when the thrids that exhibited it came to be advantageously placed; and by making little folds in the stuff after a certain manner, the sides that met and terminated in those folds, would appear to the naked eye, one of them red, and the

other green. When thrids of more than two differing colours chance to be interwoven, the resulting changeableness of the taffaty may be also somewhat different. But I chuse to give an instance in the stuff I have been speaking of, because the mixture being more simple, the way whereby the changeableness is produced, may be the more easily apprehended: and though reason alone might readily enough lead a considering man to guess at the explication, in case he knew how changeable taffaties are made; yet I thought it not impertinent to mention it, because both scholars and gentlemen are wont to look upon the inquiry into manufactures, as a mechanic employment, and consequently below them; and because also with such a microscope as I have been mentioning, the discovery is as well pleasant as satisfactory, and may afford hints of the solution of other phænomena of colours. And it were not amiss, that some diligent inquiry were made, whether the microscope would give us an account of the variableness of colour, that is so conspicuous and so delightful in mother of pearl, in opals, and some other resembling bodies. For though I remember I did formerly attempt something of that kind (fruitlessly enough) upon mother of pearl, yet not having then the advantage of my best microscope, nor some conveniencies that might have been wished, I leave it to you, who have better eyes, to try what you can do further; since it will be some discovery to find, that in this case the best eyes and microscopes themselves can make none.

10. I CONFESS, *Pyrophilus*, that a great part of what I have delivered (or proposed rather) concerning the differing forms of asperity in bodies, by which differences, the incident light either comes to be reflected with more or less of shade, and with that shade more or less interrupted, or else happens to be also otherwise modified or troubled, is but conjectural. But I am not sure, that if it were not for the dulness of our senses, either these or some other notions of kin to them, might be better countenanced; for I am apt to suspect, that if we were sharp-sighted enough, or had such perfect microscopes, as I fear are more to be wished than hoped for, our promoted sense might discern in the physical surfaces of bodies, both a great many latent ruggednesses, and the particular sizes, shapes, and situations of the extremely little bodies that cause them, and perhaps might perceive among other varieties that we now can but imagine, how those little protuberances and cavities do interrupt and dilate the light, by mingling with it a multitude of little and singly undiscernable shades, though some of them more, and some of them less minute, some less, and some more numerous, according to the nature and degree of the particular colour we attribute to the visible object. As we see, that in the moon we can with excellent telescopes discern many hills and valleys, and as it were pits and other parts, whereof some are more, and some less vividly illustrated, and others have a fainter, others a deeper shade, though the naked eye can discern no such matter in that planet. And with an excellent microscope, where the naked eye did see but a green powder, the assisted eye, as we noted above, could discern particular granules, some of them of a blue, and some of them of a yellow colour, which corpuscles we had beforehand caused to be exquisitely mixed to compound the green.

11. AND, *Pyrophilus*, that you may not think me altogether extravagant in what I have said of the possibility (for I speak of no more) of discerning the differing forms of asperity in the surfaces of bodies of several colours, I'll here set down a memorable particular that chanced to come to my knowledge, since I writ a good part of this essay; and it is this. Meeting casually the other day with the deservedly famous * Dr. *J. Finch*, extraordinary anatomist to that great patron of the Virtuosi, the now Great Duke of *Tuscany*, and inquiring of this ingenious person, what might be the chief

* *Since, for his eminent qualities and loyalty, graced by his Majesty with the honour of knighthood.*

chief rarity he had seen in his late return out of *Italy* into *England*, he told me, it was a man at *Maestricht* in the *Low-Countries*, who at certain times can discern and distinguish colours by the touch with his fingers. You will easily conclude, that this is far more strange than what I proposed but as not impossible; since the sense of the retina seeming to be much more tender and quick than that of those grosser filaments, nerves or membranes of our fingers, wherewith we use to handle gross and hard bodies, it seems scarce credible, that any accustomance, or diet, or peculiarity of constitution, should enable a man to distinguish, with such gross and unsuitable organs, such nice and subtile differences as those of the forms of asperity, that belong to differing colours; to receive whose languid and delicate impressions by the intervention of light, nature seems to have appointed and contexed into the retina the tender and delicate pith of the optic nerve. Wherefore I confess, I proposed divers scruples, and particularly whether the doctor had taken care to bind a napkin or handkerchief over his eyes so carefully, as to be sure he could make no use of his sight, though he had but counterfeited the want of it; to which I added divers other questions, to satisfy myself, whether there were any likelihood of collusion or other tricks. But I found that the judicious doctor having gone far out of his way, purposely to satisfy himself and his learned prince about this wonder, had been very watchful and circumspect to keep himself from being imposed upon. And that he might not through any mistake in point of memory misinform me, he did me the favour, at my request, to look out the notes he had written for his own and his prince's information, the sum of which memorials, as far as we shall mention them here, was this, that the doctor having been informed at *Utrecht*, that there lived one at some miles distance from *Maestricht*, who could distinguish colours by the touch; when he came to the last named town, he sent a messenger for him, and having examined him, was told upon inquiry these particulars.

That the man's name was *John Vermaasen*, at that time about 33 years of age; that when he was but two years old, he had the small-pox, which rendered him absolutely blind; that at this present he is an organist, and serves that office in a public choir.

That the doctor discoursing with him over night, the blind man affirmed, that he could distinguish colours by the touch, but that he could not do it, unless he were fasting; any quantity of drink taking from him that exquisiteness of touch, which is requisite to so nice a sensation.

That hereupon the doctor provided against the next morning seven pieces of ribbon, of these seven colours, black, white, red, blue, green, yellow, and grey; but as for mingled colours, this *Vermaasen* would not undertake to discern them, though if offered, he would tell that they were mixed.

That to discern the colour of the ribbon, he places it betwixt the thumb and the fore-finger, but his most exquisite perception was in his thumb, and much better in the right thumb than in the left.

That after the blind man had four or five times told the doctor the several colours (though blinded with a napkin for fear he might have some sight) the doctor found he was twice mistaken, for he called the white black, and the red blue; but still, he, before his error, would lay them by in pairs, saying, that though he could easily distinguish them from all others, yet those two pairs were not easily distinguished amongst themselves. Whereupon the doctor desired to be told by him what kind of discrimination he had of colours by his touch, to which he gave a reply, for whose sake chiefly I insert all this narrative in this place; namely, that all the difference

more or less asperity; for says he (I give you the doctor's own words) black feels as if you were feeling needles points, or some harsh sand, and red feels very smooth.

THAT the doctor having desired him to tell in order the difference of colours to his touch, he did as follows.

BLACK and white are the most asperous or unequal of all colours, and so like, that it is very hard to distinguish them; but black is the most rough of the two; green is next in asperity, grey next to green in asperity, yellow is the fifth in degree of asperity: red and blue are so like, that they are as hard to distinguish as black and white; but red is somewhat more asperous than blue, so that red has the sixth place, and blue the seventh in asperity.

12. To these informations the obliging doctor was pleased to add the welcome present of three of those very pieces of ribbon, whose colours in his presence the blind man had distinguished, pronouncing the one grey, the other red, and the third green; which I keep by me as rarities, and the rather, because he feared the rest were miscarried.

13. BEFORE I saw the notes that afforded me the precedent narrative, I confess I suspected this man might have thus discriminated colours rather by the smell than by the touch; for some of the ingredients imployed by dyers to colour things, have scents, that are not so languid, nor so near of kin: but that I thought it not impossible that a very critical nose might distinguish them, and this I the rather suspected, because he required, that the ribbons, whose colours he was to name, should be offered him fasting in the morning; for I have observed in setting dogs, that the feeding of them (especially with some sorts of aliments) does very much impair the exquisite scent of their noses. And though some of the foregoing particulars would have prevented that conjecture, yet I confess to you *(Pyrophilus)* that I would gladly have had the opportunity of examining this man myself, and of questioning him about divers particulars which I do not find to have been yet thought upon. And though it be not incredible to me, that since the liquours that dyers employ to tinge, are qualified to do so by multitudes of little corpuscles of the pigment or dying stuff, which are dissolved and extracted by the liquor, and swim to and fro in it, those corpuscles of colour (as the Atomists call them) insinuating themselves into, and filling all the pores of the body to be dyed, may asperate its superficies more or less according to the bigness and texture of the corpuscles of the pigment; yet I can scarce believe, that our blind man could distinguish all the colours he did, meerly by the ribbons having more or less of asperity; so that I cannot but think, notwithstanding this history, that the blind man distinguished colours not only by the degrees of asperity in the bodies offered to him, but by forms of it, though this (latter) would perhaps have been very difficult for him to make an intelligible mention of, because those minute disparities having not been taken notice of by men for want of touch as exquisite as our blind man's, are things he could not have intelligibly expressed; which will easily seem probable, if you consider, that under the name of sharp, and sweet, and sour, there are abundance of, as it were, immediate peculiar relishes or tastes in differing sorts of wine, which, though critical and experienced palates can easily discern themselves, cannot make them be understood by others; such minute differences not having hitherto any distinct names assigned them. And it seems that there was something in the forms of asperity that was requisite to the distinction of colours, besides the degree of it, since he found it so difficult to distinguish black and white from one another, though not from other colours. For I might urge, that he seems not consonant to himself about the red, which, as you have seen in one place, he represents as somewhat more asperous than the blue; and in another, very smooth:

ſmooth: but becauſe he ſpeaks of this ſmoothneſs in that place, where he mentions the roughneſs of black, we may favourably preſume that he might mean but a comparative ſmoothneſs; and therefore I ſhall not inſiſt on this, but rather countenance my conjecture by this, that he found it ſo difficult, not only to diſcriminate red and blue (though the firſt of our promiſcuous experiments will inform you, that the red reflects by great odds more light than the other) but alſo to diſtinguiſh black and white from one another, though not from other colours. And indeed, though in the ribbons that were offered him, they might be almoſt equally rough, yet in ſuch ſlender corpuſcles, as thoſe of colour, there may eaſily enough be conceived, not only a greater cloſeneſs of parts, or elſe paucity of protuberant corpuſcles, and the little extant particles may be otherwiſe figured, and ranged in the white than in the black, but the cavities may be much deeper in the one than the other.

14. And perhaps (*Pyrophilus*) it may prove ſome illuſtration of what I mean, and help you to conceive how this may be, if I repreſent, that where the particles are ſo exceeding ſlender, we may allow the parts expoſed to the ſight and touch to be a little convex in compariſon of the erected particles of black bodies, as if there were wires I know not how many times ſlenderer than a hair: whether you ſuppoſe them to be figured like needles, or cylindrically, like the hairs of a bruſh, with hemiſpherical (or at leaſt convex) tops, they will be ſo very ſlender, and conſequently the points both of the one ſort and the other ſo very ſharp, that even an exquiſite touch will be able to diſtinguiſh no greater difference between them, than that which our blind man allowed, when comparing black and white bodies, he ſaid, that the latter was the leſs rough of the two. Nor is every kind of roughneſs, though ſenſible enough, inconſiſtent with whiteneſs, there being caſes, wherein the phyſical ſuperficies of a body is made by the ſame operation both rough and white; as when the level ſurface of clear water being by agitation aſperated with a multitude of unequal bubbles, does thereby acquire a whiteneſs; and as a ſmooth piece of glaſs, by being ſcratched with a diamond, does in the aſperated part of its ſurface diſcloſe the ſame colour. But more (perchance) of this elſewhere.

15. And therefore, we ſhall here paſs by the queſtion, whether any thing might be conſidered about the opacity of the corpuſcles of black pigments, and the comparative diaphaneity of thoſe of many white bodies, applied to our preſent caſe; and proceed to repreſent, that the newly mentioned exiguity and ſhape of the extant particles being ſuppoſed, it will then be conſiderable what we lately but hinted (and therefore muſt now ſomewhat explain) that the depth of the little cavities, intercepted between the extant particles, without being ſo much greater in black bodies than in white ones, as to be perceptibly ſo to the groſs organs of touch, may be very much greater in reference to their diſpoſition of reflecting the imaginary ſubtile beams of light. For in black bodies, thoſe little intercepted cavities, and other depreſſions, may be ſo figured, ſo narrow and ſo deep, that the incident beams of light, which the more extant parts of the phyſical ſuperficies are diſpoſed to reflect inwards, may be detained there, and prove unable to emerge; whilſt, in a white body, the ſlender particles may not only by their figure be fitted to reflect the light copiouſly outwards, but the intercepted cavities being not deep, nor perhaps very narrow, the bottoms of them may be ſo conſtituted, as to be fit to reflect outwards much of the light that falls even upon them; as you may poſſibly better apprehend, when we ſhall come to treat of whiteneſs and blackneſs. In the mean time, it may ſuffice, that you take notice with me, that the blind man's relations import no neceſſity of concluding, that though, becauſe, according to the judgment of his touch, black was the roughest,

as it is the darkeſt of colours, therefore white, which (according to us) is the lighteſt, ſhould be alſo the ſmootheſt: ſince I obſerve, that he makes yellow to be two degrees more aſperous than blue, and as much leſs aſperous than green; whereas, indeed, yellow does not only appear to the eye a lighter colour than blue, but (by our firſt experiment hereafter to be mentioned) it will appear, that yellow reflected much more light than blue, and manifeſtly more than green; which we need not much wonder at, ſince in this colour, and the two others (blue and yellow) it is not only the reflected light that is to be conſidered, ſince to produce both theſe, refraction ſeems to intervene, which by its varieties may much alter the caſe: which both ſeems to ſtrengthen the conjecture I was formerly propoſing, that there was ſomething elſe in the kinds of aſperity, as well as in the degrees of it, which enabled our blind man to diſcriminate colours, and does at leaſt ſhow, that we cannot, in all caſes, from the bare difference in the degrees of aſperity betwixt colours, ſafely conclude, that the rougher of any two always reflects the leaſt light.

16. BUT this notwithſtanding *(Pyrophilus)* and whatever curioſity I may have had to move ſome queſtions to our ſagacious blind man; yet thus much I think you will admit us to have gained by his teſtimony, that ſince many colours may be felt with the circumſtances above related, the ſurfaces of ſuch coloured bodies muſt certainly have differing degrees, and in all probability have differing forms or kinds of aſperity belonging to them, which is all the uſe that my preſent attempt obliges me to make of the hiſtory above delivered; that being ſufficient to prove, that colour does much depend upon the diſpoſition of the ſuperficial parts of bodies, and to ſhew in general, wherein it is probable that ſuch a diſpoſition does (principally at leaſt) conſiſt.

17. BUT to return to what I was ſaying, before I began to make mention of our blind organiſt; what we have delivered touching the cauſes of the ſeveral forms of aſperity that may diverſify the ſurfaces of coloured bodies, may perchance ſomewhat aſſiſt us to make ſome conjectures in the general, at ſeveral of the ways whereby it is poſſible for the experiments, hereafter to be mentioned, to produce the ſudden changes of colours that are wont to be conſequent upon them: for moſt of theſe phænomena being produced by the intervention of liquors, and theſe for the moſt part abounding with very minute, active, and variouſly figured ſaline corpuſcles, liquors ſo qualified may well enough very nimbly alter the texture of the body they are employed to work upon, and ſo may change the form of aſperity, and thereby make them remit to the eye the light that falls on them after another manner than they did before, and by that means vary the colour, ſo far forth as it depends upon the texture or diſpoſition of the ſeen parts of the object; which I ſay, *Pyrophilus*, that you may not think I would abſolutely exclude all other ways of modifying the beams of light between their parting from the lucid body, and their reception into the common ſenſory.

18. Now there ſeem to me divers ways, by which we may conceive that liquors may nimbly alter the colour of one another, and of other bodies, upon which they act; but my preſent haſte will allow me to mention but ſome of them, without inſiſting ſo much as upon thoſe I ſhall name.

19. AND firſt, the minute corpuſcles that compoſe a liquor may eaſily inſinuate themſelves into thoſe pores of bodies, whereto their ſize and figure makes them congruous; and theſe pores they may either exactly fill, or but inadequately: and in this latter caſe they will for the moſt part alter the number and figure, and always the bigneſs of the former pores. And in what capacity ſoever theſe corpuſcles of a liquor come to be lodged or harboured in the pores that admit them, the ſurface of the

the body will for the most part have its asperity altered, and the incident light that meets with a grosser liquor in the little cavities that before contained nothing but air, or some yet subtiler fluid, will have its beams either refracted, or imbibed, or else reflected more or less interruptedly than they would be, if the body had been unmoistened: as we see, that even fair water falling on white paper, or linen, and divers other bodies apt to soak it in, will for some such reasons as those newly mentioned, immediately alter the colour of them, and for the most part make it sadder than that of the unwetted parts of the same bodies. And so you may see, that when in the summer the highways are dry and dusty, if there falls store of rain, they will quickly appear of a much darker colour than they did before; and if a drop of oil be let fall upon a sheet of white paper, that part of it, which by the imbibition of the liquor, acquires a greater continuity, and some transparency, will appear much darker than the rest, many of the incident beams of light being now transmitted, that otherwise would be reflected towards the beholder's eyes.

20. Secondly, A liquor may alter the colour of a body, by freeing it from those things that hindered it from appearing in its genuine colour; and though this may be said to be rather a restoration of a body to its own colour, or a retection of its native colour, than a change, yet still there intervenes in it a change of the colour which the body appeared to be of before this operation. And such a change a liquor may work, either by dissolving, or corroding, or by some such way of carrying off that matter, which either veiled or disguised the colour that afterwards appears. Thus we restore old pieces of dirty gold to a clean and nitid yellow, by putting them into the fire, and into aqua-fortis, which take off the adventitious filth that made that pure metal look of a dirty colour: and there is also an easy way to restore silver coins to their due lustre, by fetching off that which discoloured them. And I know a chymical liquor, which I employed to restore pieces of cloth spotted with grease to their proper colour, by imbibing the spotted part with this liquor, which incorporating with the grease, and yet being of a very volatile nature, does easily carry it away with itself. And I have sometimes tried, that by rubbing upon a good touch-stone a certain metalline mixture so compounded, that the impression it left upon the stone appeared of a very differing colour from that of gold, yet a little of aqua fortis would in a trice make the golden colour disclose itself, by dissolving the other metalline corpuscles that concealed those of the gold, which you know that menstruum will leave untouched.

21. Thirdly, A liquor may alter the colour of a body by making a comminution of its parts, and that principally two ways; the first by disjoining and dissipating those clusters of particles, if I may so call them, which stuck more loosely together, being fastened only by some more easily dissoluble cement, which seems to be the case of some of the following experiments, where you will find the colour of many corpuscles brought to cohere by having been precipitated together, destroyed by the affusion of very piercing and incisive liquors. The other of the two ways I was speaking of, is, by dividing the grosser and more solid particles into minute ones, which will be always lesser, and for the most part otherwise shaped than the entire corpuscle so divided, as it will happen in a piece of wood reduced into splinters or chips, or as when a piece of crystal heated red-hot and quenched in cold water is cracked into a multitude of little fragments, which, though they fall not asunder, alter the disposition of a body of the crystal, as to its manner of reflecting the light, as we shall have occasion to shew hereafter.

22. There

22. THERE is a fourth way contrary to the third, whereby a liquor may change the colour of another body, especially of another fluid; and that is, by procuring the coalition of several particles that before lay too scattered and dispersed to exhibit the colour that afterwards appears. Thus sometimes when I have had the solution of gold so dilated, that I doubted whether the liquor had really imbibed any true gold or no, by pouring in a little mercury, I have been quickly able to satisfy myself, that the liquor contained gold; that metal after a little while cloathing the surface of the quicksilver with a thin film of its own livery. And chiefly, though not only by this way of bringing the minute parts of bodies together in such numbers, as to make them become notorious to the eye, many of these colours seem to be generated which are produced by precipitations, especially by such as are wont to be made with fair water; as when resinous gums dissolved in spirit of wine, are let fall again, if the spirit be copiously diluted with that weakening liquor. And so out of the rectified and transparent butter of antimony, by the bare mixture of fair water, there will be plentifully precipitated that milk-white substance, which by having its looser salts well washed off, is turned into that medicine, which vulgar chymists are pleased to call *Mercurius Vitæ*.

23. A FIFTH way, by which a liquor may change the colour of a body, is, by dislocating the parts, and putting them out of their former order into another, and perhaps also altering the posture of the single corpuscles as well as their order or situation in respect of one another. What certain kinds of commotion or dislocation of the parts of a body may do towards the changing its colour, is not only evident in the mutations of colour observable in quicksilver, and some other concretes long kept by chymists in a convenient heat, though in close vessels, but in the obvious degenerations of colour, which every body may take notice of in bruised cherries, and other fruit, by comparing after a while the colour of the injured with that of the sound part of the same fruit. And that also such liquors, as we have been speaking of, may greatly discompose the textures of many bodies, and thereby alter the disposition of their superficial parts, the great commotion made in metals, and several other bodies by aqua fortis, oil of vitriol, and other saline menstruums, may easily persuade us; and what such varied situations of parts may do towards the diversifying of the manner of their reflecting the light, may be guessed in some measure by the beating of transparent glass into a white powder, but far better by the experiments lately pointed at, and hereafter delivered, as the producing and destroying colours by the means of subtile saline liquors, by whose affusion the parts of other liquors are manifestly both agitated, and likewise disposed after another manner than they were before such affusion. And in some chymical oils, as particularly that of lemon peels, by barely shaking the glass that holds it into bubbles, that transposition of the parts which is consequent to the shaking, will shew you on the surfaces of the bubbles exceeding orient and lively colours, which, when the bubbles relapse into the rest of the oil, do immediately vanish.

24. I KNOW not, *Pyrophilus*, whether I should mention as a distinct way, because it is of a somewhat more general nature, that power whereby a liquor may alter the colour of another body, by putting the parts of it into motion; for though possibly the motion so produced does, as such, seldom suddenly change the colour of the body whose parts are agitated, yet this seems to be one of the most general, however not immediate causes of the quick change of colours in bodies. For the parts being put into motion by the adventitious liquor, divers of them that were before united, may become thereby disjoined, and when that motion ceases or decays, others of them

them may ſtick together, and that in a new order, by which means the motion may ſometimes produce permanent changes of colours, as in the experiment you will meet with hereafter, of preſently turning a ſnowy white body into a yellow, by the bare affuſion of fair water, which probably ſo diſſolves the ſaline corpuſcles that remained in the calx, and ſets them at liberty to act upon one another, and the metal, far more powerfully than the water without the aſſiſtance of ſuch ſaline corpuſcles could do. And though you rub blue vitriol, how venereal and unſophiſticated ſoever it be, upon the whetted blade of a knife, it will not impart to the iron its latent colour; but if you moiſten the vitriol with your ſpittle, or common water, the particles of the liquor disjoining thoſe of the vitriol, and thereby giving them the various agitation requiſite to fluid bodies, the metalline corpuſcles of the thus diſſolved vitriol will lodge themſelves in throngs in the ſmall and congruous pores of the iron they are rubbed on, and ſo give the ſurface of it the genuine colour of the copper.

25. There remains yet a way, *Pyrophilus*, to be mentioned, by which a liquor may alter the colour of another body, and this ſeems the moſt important of all, becauſe though it be named but as one, yet it may indeed comprehend many; and that is, by aſſociating the ſaline corpuſcles, or any other ſort of the more rigid ones of the liquor, with the particles of the body that it is employed to work upon. For theſe adventitious corpuſcles aſſociating themſelves with the protuberant particles of the ſurface of a coloured body, muſt neceſſarily alter their bigneſs, and will moſt commonly alter their ſhape. And how much the colours of bodies depend upon the bulk and figure of their ſuperficial particles, you may gueſs by this, that eminent antient philoſophers, and divers moderns, have thought that all colours might, in a general way, be made out by theſe two; whoſe being diverſified will, in our caſe, be attended with theſe two circumſtances; the one, that the protuberant particles being increaſed in bulk, they will oftentimes be varied as to the cloſeneſs or laxity of their order, fewer of them being contained within the ſame ſenſible (though minute) ſpace than before; or elſe by approaching to one another, they muſt ſtraiten the pores, and it may be too they will, by their manner of aſſociating themſelves with the protuberant particles, intercept new pores. And this invites me to conſider farther, that the adventitious corpuſcles I have been ſpeaking of, may likewiſe produce a great change, as well in the little cavities or pores, as in the protuberances of a coloured body; for, beſides what we have juſt now taken notice of, they may, by lodging themſelves in thoſe little cavities, fill them up, and it may well happen, that they may not only fill the pores they inſinuate themſelves into, but likewiſe have their upper parts extant above them; and partly by theſe new protuberances, partly by increaſing the bulk of the former, theſe extraneous corpuſcles may much alter the number and bigneſs of the ſurface's pores, changing the old and intercepting new ones. And then it is odds, but the order of the little extancies, and conſequently that of the little depreſſions in point of ſituation will be altered likewiſe: as if you diſſolve quickſilver in ſome kind of aqua fortis, the ſaline particles of the menſtruum, aſſociating themſelves with the mercurial corpuſcles, will make a green ſolution, which afterwards eaſily enough degenerates. And red lead, or minium, being diſſolved in ſpirit of vinegar, yields not a red, but a clear ſolution, the redneſs of the lead being by the liquor deſtroyed. But a better inſtance may be taken from copper; for I have tried, that if upon a copper-plate, you let ſome drops of weak aqua fortis reſt for a while, the corpuſcles of the menſtruum joining with thoſe of the metal, will produce a very ſenſible aſperity upon the ſurface of the plate, and will concoagulate that way into very minute grains of a pale blue vitriol; whereas, if upon another

another part of the ſame plate you ſuffer a little ſtrong ſpirit of urine to reſt a competent time, you ſhall find the aſperated ſurface adorned with a deeper and richer blue. And the ſame aqua fortis, that will quickly change the redneſs of red lead into a darker colour, will, being put upon crude lead, produce a whitiſh ſubſtance, as with copper it did a blueiſh. And as with iron it will produce a reddiſh, and on white quills a yellowiſh, ſo much may the coalition of the parts of the ſame liquor, with the differingly figured particles of ſtable bodies, divers ways aſperate the differingly diſpoſed ſurfaces, and ſo diverſify the colour of thoſe bodies. And you will eaſily believe, that in many changes of colour, that happen upon the diſſolutions of metals, and precipitations made with oil of tartar, and the like fixed ſalts, there may intervene a coalition of ſaline corpuſcles with the particles of the body diſſolved or precipitated, if you examine how much the vitriol of a metal may be heavier than the metalline part of it alone, upon the ſcore of the ſaline parts concoagulated therewith; and, that in ſeveral precipitations the weight of the calx does for the ſame reaſon much exceed that of the metal, when it was firſt put in to be diſſolved.

26. BUT, *Pyrophilus*, to conſider theſe matters more particularly would be to forget that I declared againſt adventuring, at leaſt for this time, at particular theories of colours, and that accordingly you may juſtly expect from me rather experiments than ſpeculations: and therefore I ſhall diſmiſs this ſubject of the forms of ſuperficial aſperity in coloured bodies, as ſoon as I ſhall but have named to you, by way of ſupplement to what we have hitherto diſcourſed in this ſhort ſection, a couple of particulars (which you will eaſily grant me); the one, that there are divers other ways for the ſpeedy production even of true and permanent colours in bodies, beſides thoſe practicable by the help of liquors: for proof of which advertiſement, though ſeveral examples might be alledged, yet I ſhall need but remind you of what I mentioned to you above, touching the change of colours ſuddenly made on tempered ſteel, and on lead, by the operation of heat, without the intervention of a liquor. But the other particular I am to obſerve to you, is of more importance to our preſent ſubject; and it is, that though nature and art may in ſome caſes ſo change the aſperity of the ſuperficial parts of a body, as to change its colour by either of the ways I have propoſed, ſingle or unaſſiſted; yet for the moſt part it is by two or three, or perhaps by more of the forementioned ways aſſociated together, that the effect is produced. And if you conſider how variouſly theſe ſeveral ways and ſome others allied unto them, which I have left unmentioned, may be compounded and applied, you will not much wonder that ſuch fruitful, whether principles (or manners of diverſification) ſhould be fitted to change or generate no ſmall ſtore of differing colours.

27. HITHERTO, *Pyrophilus*, we have in diſcourſing of the aſperity of bodies conſidered the little protuberances of other ſuperficial particles which make up that roughneſs, as if we took it for granted, that they muſt be perfectly opacous and impenetrable by the beams of light, and ſo muſt contribute to the variety of colours, as they terminate more or leſs light, and reflect it to the eye mixed with more or leſs of thus or thus mingled ſhades. But to deal ingenuouſly with you, *Pyrophilus*, before I proceed any further, I muſt not conceal from you, that I have often thought it worth a ſerious inquiry, whether or no particles of matter, each of them ſingly inſenſible, and therefore ſmall enough to be capable of being ſuch minute particles, as the Atomiſts both of old and of late have (not abſurdly) called *Corpuſcula Coloris*, may not yet conſiſt each of them of divers yet minuter particles, betwixt which we may conceive little commiſſures where they adhere to one another, and, however, may

not be porous enough to be, at least in some degree, pervious to the unimaginably subtile corpuscles that make up the beams of light, and consequently to be in such a degree diaphanous. For, *Pyrophilus*, that the proposed inquiry may be of moment to him that searches after the nature of colour, you will easily grant, if you consider, that whereas perfectly opacous bodies can but reflect the incident beams of light, those that are diaphanous are qualified to refract them too; and that refraction has such a stroke in the production of colours, as you cannot but have taken notice of, and perhaps admired in the colours generated by the trajection of light through drops of water that exhibit a rainbow, through prismatical glasses, and through divers other transparent bodies. But 'tis like, *Pyrophilus*, you will more easily allow that about this matter it is rather important to have a certainty, than that it is rational to entertain a doubt; wherefore I must mention to you some of the reasons that make me think it may need a further inquiry: for I find that in a darkened room, where the light is permitted to enter but at one hole, the little wandering particles of dust, that are commonly called motes, and, unless in the sun-beams, are not taken notice of by the unassisted sight; I have, I say, often observed that these roving corpuscles being looked on by an eye placed on the one side of the beams that entered the little hole, and by the darkness having its pupil much enlarged, I could discern that these motes as soon as they came within the compass of the luminous, whether cylinder or inverted cone, if I may so call it, that was made up by the unclouded beams of the sun, did in certain positions appear adorned with very vivid colours, like those of the rainbow, or rather like those of very minute, but sparkling fragments of diamonds: and as soon as the continuance of their motion had brought them to an inconvenient position in reference to the light and the eye, they were only visible without darting any lively colours as before. Which seems to argue, that these little motes, or minute fragments of several sorts of bodies reputed opacous, and only crumbled as to their exterior and looser parts into dust, did not barely reflect the beams that fell upon them, but remit them to the eye refracted too. We may also observe, that several bodies (as well some of a vegetable, as others of an animal nature) which are wont to pass for opacous, appear in great part transparent, when they are reduced into thin parts, and held against a powerful light. This I have not only taken notice of in pieces of ivory reduced into thick leaves, as also in divers considerable thick shells of fishes, and in shaving of wood; but I have also found that a piece of deal, far thicker than one would easily imagine, being purposely interposed betwixt my eye placed in a room, and the clear day-light, was not only somewhat transparent, but (perhaps by reason of its gummous nature) appeared quite through of a lovely red. And in the darkened room above mentioned, bodies held against the hole at which the light entered, appeared far less opacous than they would elsewhere have done; insomuch that I could easily and plainly see, through the whole thickness of my hand, the motions of a body placed (at a very near distance indeed, but yet) beyond it. And even in minerals, the opacity is not always so great as many think, if the body be made thin: for white marble, though of a pretty thickness, being within a due distance placed betwixt the eye and a convenient light, will suffer the motions of one's finger to be well discerned through it, and so will pieces, thick enough, of many common flints. But above all, that instance is remarkable, that is afforded us by *Muscovy* glass (which some call *Selenites*, others *Lapis Specularis*;) for though plates of this mineral, though but of a moderate thickness, do often appear opacous, yet if one of these be dexterously split into the thinnest leaves it is made up of, it will yield such a number of them, as scarce any thing but experience could have persuaded

me; and these leaves will afford the most transparent sort of consistent bodies, that, for aught I have observed, are yet unknown; and a single leaf or plate will be so far from being opacous, that it will scarce be so much as visible. And multitudes of bodies there are, whose fragments seem opacous to the naked eye, which yet, when I have included them in good microscopes, appeared transparent; but, *Pyrophilus*, on the other side I am not yet sure that there are no bodies, whose minute particles even in such a microscope as that of mine, which I was lately mentioning, will not appear diaphanous. For having considered mercury precipitated *per se*, the little granules that made up the powder, looked like little fragments of coral beheld by the naked eye at a distance (for very near at hand coral will sometimes, especially if it be good, shew some transparency). Filings likewise of steel and copper, though in an excellent microscope, and a fair day, they showed like pretty big fragments of those metals, and had considerable brightness on some of their surfaces, yet I was not satisfied, that I perceived any reflection from the inner parts of any of the filings. Nay, having looked in my best microscope upon the red calx of lead (commonly called *Minium*) neither I, nor any I shewed it to, could discern it to be other than opacous, though the day were clear, and the object strongly enlightened. And the deeply red colour of vitriol appeared in the same microscope (notwithstanding the great comminution effected by the fire) but like grossy beaten brick. So that, *Pyrophilus*, I shall willingly resign you the care of making some further inquiries into the subject we have now been considering; for I confess, as I told you before, that I think that the matter may need a further scrutiny, nor would I be forward to determine how far or in what cases the transparency or semi-diaphaneity of the superficial corpuscles of bigger bodies may have an interest in the production of their colours; especially because that even in divers white bodies, as beaten glass, snow and froth, where it seems manifest that the superficial parts are singly diaphanous (being either water, or air, or glass) we see not that such variety of colours are produced as usually are by the refraction of light even in those bodies, when by their bigness, shape, *&c.* they are conveniently qualified to exhibit such various and lively colours as those of the rainbow, and of prismatical glasses.

28. By what has been hitherto discoursed, *Pyrophilus*, we may be assisted to judge of that famous controversy which was of old disputed betwixt the Epicureans and other Atomists on one side, and most other philosophers on the other side; the former denying bodies to be coloured in the dark, and the latter making colour to be an inherent quality, as well as figure, hardness, weight, or the like. For though this controversy be revived, and hotly agitated among the moderns, yet I doubt whether it be not in great part a nominal dispute; and therefore let us, according to the doctrine formerly delivered, distinguish the acceptations of the word colour, and say, that if it be taken in the stricter sense, the Epicureans seem to be in the right; for if colour be indeed, though not according to them, but light modified, how can we conceive that it can subsist in the dark, that is, where it must be supposed there is no light: but, on the other side, if colour be considered as a certain constant disposition of the superficial parts of the object to trouble the light they reflect after such and such a determinate manner, this constant, and if I may so speak, modifying disposition persevering in the object, whether it be shined upon or no, there seems no just reason to deny, but that in this sense, bodies retain their colour as well in the night as day; or, to speak a little otherwise, it may be said, that bodies are potentially coloured in the dark, and actually in the light. But of this matter discoursing more fully elsewhere, as it is a difficulty that concerns qualities in general, I shall forbear to insist on it here.

CHAP. IV.

1. OF greater moment in the inveſtigation of the nature of colours is the controverſy, whether thoſe of the rainbow, and thoſe that are often ſeen in clouds, before the riſing, or after the ſetting of the ſun; and in a word, whether thoſe other colours, that are wont to be called emphatical, ought or ought not to be accounted true colours. I need not tell you that the negative is the common opinion, eſpecially in the ſchools, as may appear by that vulgar diſtinction of colours, whereby theſe under conſideration are termed apparent, by way of oppoſition to thoſe that in the other member of the diſtinction are called true or genuine. This queſtion I ſay ſeems to me of importance, upon this account, that it being commonly granted (or however, eaſy enough to be proved) that emphatical colours are light itſelf modified by refractions chiefly, with a concurrence ſometimes of reflections, and perhaps ſome other accidents depending on theſe two; if theſe emphatical colours be reſolved to be genuine, it will ſeem conſequent, that colours, or at leaſt divers of them, are but diverſified light, and not ſuch real and inherent qualities as they are commonly thought to be.

2. Now ſince we are wont to eſteem the echoes and other ſounds of bodies, to be true ſounds, all their odours to be true odours, and (to be ſhort) ſince we judge other ſenſible qualities to be true ones, becauſe they are the proper objects of ſome or other of our ſenſes; I ſee not why emphatical colours, being the proper and peculiar objects of the organ of ſight, and capable to affect it as truly and as powerfully as other colours, ſhould be reputed but imaginary ones.

AND if we have (which perchance you will allow) formerly evinced colour (when the word is taken in its more proper ſenſe) to be but modified light, there will be ſmall reaſon to deny theſe to be true colours, which more manifeſtly than others diſcloſe themſelves to be produced by diverſifications of the light.

3. THERE is indeed taken notice of, a difference betwixt theſe apparent colours, and thoſe that are wont to be eſteemed genuine, as to the duration, which has induced ſome learned men to call the former rather evanid than fantaſtical. But as the ingenious *Gaſſendus* does ſomewhere judiciouſly obſerve, if this way of arguing were good, the greenneſs of a leaf ought to paſs for apparent, becauſe, ſoon fading into a yellow, it ſcarce laſts at all, in compariſon of the greenneſs of an emerald. I ſhall add, that if the ſun-beams be in a convenient manner trajected through a glaſs priſm, and thrown upon ſome well ſhaded object within a room, the rainbow thereby painted on the ſurface of the body that terminates the beams, may oftentimes laſt longer than ſome colours I have produced in certain bodies, which would juſtly, and without ſcruple be accounted genuine colours, and yet ſuddenly degenerate, and loſe their nature.

4. A GREATER diſparity betwixt emphatical colours, and others, may perhaps be taken from this, that genuine colours ſeem to be produced in opacous bodies by reflection, but apparent ones in diaphanous bodies, and principally by refraction; I ſay principally, rather than ſolely, becauſe in ſome caſes reflection alſo may concur: but ſtill this ſeems not to conclude theſe latter colours not to be true ones. Nor muſt what has been newly ſaid of the differences of true and apparent colours, be interpreted in too unlimited a ſenſe, and therefore it may perhaps ſomewhat aſſiſt you, both to reflect upon the two foregoing objections, and to judge of ſome other paſſages which you will meet with in this tract, if I take this occaſion to obſerve to you,

that if water be agitated into froth, it exhibits, you know, a white colour, which soon after it loses upon the resolution of the bubbles into air and water. Now in this case either the whiteness of the froth is a true colour, or not; if it be, then true colours, supposing the water pure and free from mixtures of any thing tenacious, may be as short-lived as those of the rainbow; also the matter, wherein the whiteness did reside, may in a few moments perfectly lose all footsteps or remains of it. And besides, even diaphanous bodies may be capable of exhibiting true colours by reflection; for that whiteness is so produced, we shall anon make it probable. But if on the other side it be said, that the whiteness of froth is an emphatical colour, then it must no longer be said, that fantastical colours require a certain position of the luminary and the eye, and must be varied or destroyed by the change thereof, since froth appears white, whether the sun be rising or setting, or in the meridian, or any where between it and the horizon, and from what (neighbouring) place soever the beholder's eye looks upon it. And since by making a liquor tenacious enough, yet without destroying its transparency, or staining it with any colour, you may give the little films, whereof the bubbles consist, such a texture as may make the froth last very many hours, if not some days, or even weeks, it will render it somewhat improper to assign duration for the distinguishing character to discriminate genuine from fantastical colours. For such froth may much outlast the undoubtedly true colours of some of nature's productions, as in that gaudy plant, not undeservedly called the *Marvel of Peru*, the flowers do often fade the same day they are blown; and I have often seen a Virginian flower, which usually withers within the compass of a day; and I am credibly informed, that not far from hence, a curious herborist has a plant, whose flowers perish in about an hour. But, if the whiteness of water turned into froth must therefore be reputed emphatical, because it appears not that the nature of the body is altered, but only that the disposition of its parts, in reference to the incident light, is changed, why may not the whiteness be accounted emphatical too; which I shall shew anon to be producible, barely by such another change in black horn: and yet this so easily acquired whiteness seems to be as truly its colour as the blackness was before, and at least is more permanent than the greenness of leaves, the redness of roses, and in short, than the genuine colours of the most part of nature's productions. It may indeed be further objected, that according as the sun or other luminous body changes place, these emphatical colours alter or vanish. But not to repeat what I have just now said, I shall add, that if a piece of cloth in a draper's shop (in such the light being seldom primary) be variously folded, it will appear of differing colours, as the parts happen to be more illuminated, or more shaded; and if you stretch it flat, it will commonly exhibit some one uniform colour: and yet these are not wont to be reputed emphatical, so that the difference seems to be chiefly this, that in the case of the rainbow, and the like, the position of the luminary varies the colour, and in the cloth I have been mentioning, the position of the object does it. Nor am I forward to allow, that in all cases, the apparition of emphatical colours requires a determinate position of the eye; for if men will have the whiteness of froth emphatical, you know what we have already inferred from thence. Besides, the sunbeams trajected through a triangular glass, after the manner lately mentioned, will, upon the body that terminates them, paint a rainbow, that may be seen, whether the eye be placed on the right hand of it, or the left, or above, or beneath it, or before or behind it: and though there may appear some little variation in the colours of the rainbow beheld from differing parts of the room, yet such a diversity may be also observed by an attentive eye in real colours, looked upon under the like circumstances.

Nor

Nor will it follow, that because there remain no footsteps of the colour upon the object, when the prism is removed, that therefore the colour was not real, since the light was truly modified by the refraction and reflection it suffered in its trajection through the prism; and the object in our case served for a specular body, to reflect that colour to the eye. And that you may not be startled, *Pyrophilus*, that I should venture to say, that a rough and coloured object may serve for a speculum to reflect the artificial rainbow I have been mentioning, consider what usually happens in darkened rooms, where a wall, or other body conveniently situated within, may so reflect the colours of bodies without the room, that they may very clearly be discerned and distinguished; and yet it is taken for granted, that the colours seen in a darkened room, though they leave no traces of themselves upon the wall or body that receives them, are the true colours of the external objects, together with which the colours of the images are moved, or do rest. And the error is not in the eye, whose office is only to perceive the appearances of things, and which does truly so; but in the judging or estimative faculty, which mistakingly concludes that colour to belong to the wall, which does indeed belong to the object, because the wall is that from whence the beams of light, that carry the visible species, do come in strait lines directly to the eye: as for the same reason we are wont, at a certain distance from concave spherical glasses, to persuade ourselves, that we see the image come forth to meet us, and hang in the air betwixt the glass and us, because the reflected beams, that compose the image cross in that place where the image seems to be, and thence, and not from the glass, do in direct lines take their course to the eye. And upon the like cause it is, that divers deceptions in sounds and other sensible objects do depend, as we elsewhere declare.

5. I know not whether I need add, that I have purposely tried (as you will find some pages hence, and will perhaps think somewhat strange) that colours, that are called emphatical, because not inherent in the bodies in which they appear, may be compounded with one another, as those that are confessedly genuine may. But when all this is said, *Pyrophilus*, I must advertise you, that it is but problematically spoken; and that though I think the opinion I have endeavoured to fortify probable, yet a great part of our discourse concerning colours may be true, whether that opinion be so or not.

CHAP. V.

1. THERE are, you know, *Pyrophilus*, besides those obsolete opinions about colours, which have been long since rejected, very various theories, that have each of them, even at this day, eminent men for their abettors: for the Peripatetic schools, though they dispute amongst themselves divers particulars concerning colours, yet in this they seem unanimously enough to agree, that colours are inherent and real qualities, which the light doth but disclose, and not concur to produce. Besides, there are moderns, who with a slight variation adopt the opinion of *Plato*; and as he would have colour to be nothing but a kind of flame consisting of minute corpuscles, as it were darted by the object against the eye, to whose pores their littleness and figure made them congruous; so these would have colour to be an internal light of the more lucid parts of the object, darkened, and consequently altered by the various mixtures of the less luminous parts. There are also others, who, in imitation of some of the antient Atomists, make colour not to be lucid steam, but yet a corporeal effluvium issuing out of the coloured body; but the knowingest of these have of late reformed their hypothesis, by acknowledging and adding, that some external light

light is neceſſary to excite, and, as they ſpeak, ſolicit theſe corpuſcles of colour, as they call them, and bring them to the eye. Another and more principal opinion of the modern philoſophers, to which this laſt named may by a favourable explication be reconciled, is that, which derives colours from the mixture of light and darkneſs, or rather light and ſhadows. And as for the Chymiſts, it is known, that the generality of them aſcribe the origin of colours to the ſulphureous principle in bodies; though I find, as I elſewhere largely ſhew, that ſome of the chieſeſt of them derive colours rather from ſalt than ſulphur, and others from the third hypoſtatical principle, mercury. And as for the Carteſians, I need not tell you, that they, ſuppoſing the ſenſation of light to be produced by the impulſe made upon the organs of ſight, by certain extremely minute and ſolid globules, to which the pores of the air and other diaphanous bodies are pervious, endeavour to derive the varieties of colours from the various proportion of the direct progreſs or motion of theſe globules to their circumvolution or motion about their own center, by which varying proportion they are by this hypotheſis ſuppoſed qualified to ſtrike the optic nerve after ſeveral diſtinct manners, ſo as to produce the perception of differing colours.

2. BESIDES theſe ſix principal hypotheſes, *Pyrophilus*, there may be ſome others, which though leſs known, may perhaps as well as theſe deſerve to be taken into conſideration by you; but that I ſhould copiouſly debate any of them at preſent, I preſume you will not expect, if you conſider the ſcope of theſe papers, and the brevity I have deſigned in them; and therefore I ſhall at this time only take notice to you in the general of two or three things, that do more peculiarly concern the treatiſe you have now in your hands.

3. AND firſt, though the embracers of the ſeveral hypotheſes I have been naming to you, by undertaking each ſect of them to explicate colours indefinitely by the particular hypotheſes they maintain, ſeem to hold it forth as the only needful theory about that ſubject; yet for my part I doubt, whether any one of all theſe hypotheſes have a right to be admitted excluſively to all others: for I think it probable, that whiteneſs and blackneſs may be explicated by reflection alone without refraction, as you will find endeavoured in the diſcourſe you will meet with ere long, of the origin of whiteneſs and blackneſs; and on the other ſide, ſince I have not found, that by any mixture of white and true black (for there is a blueiſh black, which many miſtake for a genuine) there can be a blue, a yellow, or a red, to name no other colours, produced; and ſince we do find, that theſe colours may be produced in the glaſs priſm and other tranſparent bodies, by the help of refractions, it ſeems, that refraction is to be taken in, into the explication of ſome colours, to whoſe generation they ſeem to concur, either by making a further or other commixture of ſhades with the refracted light, or by ſome other way not now to be diſcourſed. And as it ſeems not improbable, that in caſe the pores of the air, and other diaphanous bodies be every where almoſt filled with ſuch globuli, as the Carteſians ſuppoſe, the various kinds of motion of theſe globuli may in many caſes have no ſmall ſtroke in varying our perception of colour; ſo without the ſuppoſition of theſe globuli, which it is not ſo eaſy to evince, I think we may probably enough conceive in general, that the eye may be variouſly affected, not only by the entire beams of light that fall upon it, as they are ſuch; but by the order, and by the degree of ſwiftneſs, and in a word, by the manner, according to which the particles that compoſe each particular beam arrive at the ſenſory: ſo that whatever be the figure of the little corpuſcles, of which the beams of light conſiſt, not only the celerity or ſlowneſs of their revolution or rotation, in reference to their progreſſive motion, by their more abſolute celerity, their direct or undulating

undulating motion, and other accidents, which may attend their appulse to the eye, may fit them to make differing impressions on it.

4. Secondly, For these and the like considerations, *Pyrophilus*, I must desire, that you would look upon this little treatise, not as a discourse written principally to maintain any of the fore-mentioned theories, exclusively to all others, or substitute a new one of my own; but as the beginning of a history of colours, upon which, when you and your ingenious friends shall have enriched it, a solid theory may be safely built. But yet because this history is not meant barely for a register of the things recorded in it, but for an apparatus to a sound and comprehensive hypothesis, I thought fit so to temper the whole discourse, as to make it as conducible as conveniently I can to that end: and therefore I have not scrupled to let you see, that I was willing to save you the labour of cultivating some theories, that I thought would never enable you to reach the ends you aim at, so to contract your enquiries into a narrow compass. For both which purposes I thought it requisite to do these two things; the one, to set down some experiments, which by the help of the reflections and insinuations that attend them, may assist you to discover the infirmness and insufficiency both of the common Peripatetic doctrine, and of the now more applauded theory of the chymists about colour; because these two doctrines having possessed themselves, the one of the most part of the schools, and the other of the esteem of the generality of physicians and other learned men, whose professions and ways of study do not exact, that they should scrupulously examine the very first and simplest principles of nature: I feared it would be to little purpose, without doing something to discover the insufficiency of these hypotheses, that I should (which was the other thing I thought requisite for me to do) set down among my other experiments those in the greatest number, that may let you see, that, till I shall be better informed, I incline to take colour to be a modification of light; and would invite you chiefly to cultivate that hypothesis, and improve it to the making out of the generation of particular colours, as I have endeavoured to apply it to the explication of whiteness and blackness.

5. Thirdly, But *Pyrophilus*, though this be at present the hypothesis I prefer, yet I propose it but in a general sense, teaching only, that the beams of light, modified by the bodies whence they are sent (reflected or refracted) to the eye, produce there that kind of sensation men commonly call colour. But whether I think this modification of the light to be performed by mixing it with shades, or by varying the proportion of the progress and rotation of the Cartesian *Globuli Cælestes*, or by some other way, which I am not now to mention, I pretend not here to declare; much less do I pretend to determine, or scarce so much as to hope to know all that were requisite to be known, to give you, or even myself, a perfect account of the theory of vision and colours. For in order to such an undertaking, I would first know what light is, and if it be a body (as a body or the motion of a body it seems to be) what kind of corpuscles for size and shape it consists of, with what swiftness they move forwards, and whirl about their own centers. Then I would know the nature of refraction, which I take to be one of the abstrusest things (not to explicate plausibly, but to explicate satisfactorily) that I have met with in physicks. I would further know, what kind and what degree of commixture of darkness or shades is made by refractions, or reflections, or both, in the superficial particles of those bodies, that being shined upon, constantly exhibit the one, for instance, a blue, the other a yellow, the third a red colour. I would further know, why this contemperation of light and shade, that is made, for example, by the skin of a ripe cherry, should exhibit a red,

red, and not a green, and the leaf of the same tree should exhibit a green rather than a red. And indeed, lastly, why since the light that is modified into these colours consists but of corpuscles moved against the retina or pith of the optic nerve, it should there not barely give a stroke, but produce a colour; whereas a needle wounding likewise the eye would not produce colour, but pain. These, and perhaps other things I should think requisite to be known, before I should judge myself to have fully comprehended the true and whole nature of colours: and therefore, though by making the experiments and reflections delivered in this paper, I have endeavoured somewhat to lessen my ignorance in this matter, and think it far more desirable to discover a little, than to discover nothing; yet I pretend but to make it probable by the experiments I mention, that some colours may be plausibly enough explicated in the general by the doctrine here proposed. For whensoever I would descend to the minute and accurate explication of particulars, I find myself very sensible of the great obscurity of things, without excepting those, which we never see but when they are enlightened, and confess with *Scaliger*, *Latet natura hæc* (says he, speaking of that colour) *& sicut aliarum rerum species in profundissima caligine inscitiæ humanæ.*

Exercitat. 305. Parag. 4.

THE

EXPERIMENTAL HISTORY

OF

COLOURS

PART II.

Of the Nature of Whiteness and Blackness.

CHAP. I.

1. THOUGH after what I have acknowledged, *Pyrophilus*, of the abstruse nature of colours in particular, you will easily believe, that I pretend not to give you a satisfactory account of whiteness and blackness; yet not wholly to frustrate your expectation of my offering something by way of specimen towards the explication of some colours in particular, I shall make choice of these as the most simple ones (and by reason of their mutual opposition the least hardly explicable) about which to present you my thoughts, upon condition you will take them at most to be my conjectures, not my opinions.

2. WHEN I applied myself to consider, how the cause of whiteness might be explained by intelligible and mechanical principles, I remembered not to have met with

any thing among the antient corpuscularian philosophers, touching the quality we call whiteness, save that *Democritus* is by *Aristotle* said to have ascribed the whiteness of bodies to their smoothness, and on the contrary their blackness to their asperity. But though about the latter of those qualities his opinion be allowable, as we shall see anon; yet that he needs a favourable interpretation in what is delivered concerning the first (at least if his doctrine be not misrepresented in this point, as it has been in many others) we shall quickly have occasion to manifest. But amongst the moderns, the most learned *Gassendus* in his ingenious epistle published in the year 1642, *De apparente magnitudine solis humilis & sublimis*, reviving the atomical philosophy, has, though but incidentally, delivered something towards the explication of whiteness upon mechanical principles. And because no man, that I know of, has done so before him, I shall, to be sure to do him right, give you his sense in his own words: *Cogites velim* (says he) *lucem quidem in diaphano nullius coloris videri, sed in opaco tamen terminante candicare, ac tantò magis, quantò densior seu collectior fuerit. Deinde aquam non esse quidam coloris ex se candidi, & radium tamen ex eâ reflexum versus oculum candicare. Rursus cum plana aquæ superficies non nisi ex una parte eam reflexionem faciat: si contigerit tamen illam in aliquot bullas intumescere, bullam unamquamque reflectionem facere, & candoris speciem creare certa superficiei parte. Ad hæc spumam ex aqua pura non alia ratione videri candescere & albescere, quam quod sit congeries confertissima minutissimarum bullarum, quarum unaquæque suum radium reflectit, unde continens candor alborve apparet. Denique nivem nihil aliud videri quam speciem purissimæ spumæ ex bullulis quam minutissimis & confertissimis cohærentis. Sed ridiculum me exhibeam, si tales meas nugas uberius proponem.*

Album quippe & nigrum, hoc quidem asperum esse dicit, hoc verò lævis. De sensu 3. & sensil. 3.

Epist. 2. pag. 45.

3. But though in this passage, that very ingenious person has anticipated part of what I should say; yet I presume you will for all that expect, that I should give you a fuller account of that notion of whiteness, which I have the least exceptions to, and of the particulars whence I deduce it; which to do, I must mention to you the following experiments and observations.

Whiteness then considered as a quality in the object seems chiefly to depend upon this, that the superficies of the body, that is called white, is asperated by almost innumerable small surfaces; which being of an almost specular nature, are also so placed, that some looking this way, and some that way, they yet reflect the rays of light that fall on them, not towards one another, but outwards towards the spectator's eye. In this rude and general account of whiteness, it seems, that besides those qualities, which are common to bodies of other colours, as for instance the minuteness and number of the superficial parts, the two chief things attributed to bodies as white are made to be, first, that little protuberances and superficial parts be of somewhat a specular nature, that they may, as little looking-glasses, each of them reflect the beams it receives (or the little picture of the sun made on it) without otherwise considerably altering them; whereas in most other colours, they are wont to be much changed, by being also refracted, or by being returned to the eye, mixt with shades or otherwise. And next, that its superficial parts be so situated, that they retain not the incident rays of light by reflecting them inwards, but send them almost all back; so that the outermost corpuscles of a white body, having their various little surfaces of a specular nature, a man can from no place behold the body, but that there will be among those innumerable *superficieculæ*, that look some one way, and some another, enough of them obverted to his eye, to afford, like a broken looking-glass, a confused idea, or representation of light, and make such an impression on the organ, as that for which men are wont to call a body white. But this motion will perhaps be best

 explained

explained by the ſame experiments and obſervations, on which it is built, and therefore I ſhall now advance to them.

4. AND in the firſt place I conſider, that the ſun, and other powerfully lucid bodies, are not only wont to offend, which we call to dazzle our eyes; but that if any colour be to be aſcribed to them as they are lucid, it ſeems it ſhould be whiteneſs. For the ſun at noon-day, and in clear weather, and when his face is leſs troubled, and as it were ſtained by the ſteams of ſublunary bodies, and when his beams have much leſs of the atmoſphere to traject in their paſſage to our eyes, appears of a colour more approaching to white, than when nearer the horizon: the interpoſition of certain ſorts of fumes and vapours make him oftentimes appear either red, or at leaſt more yellow. And when the ſun ſhines upon that natural looking-glaſs, a ſmooth water, that part of it, which appears to this or that particular beholder the moſt ſhined on, does to his eye ſeem far whiter than the reſt. And here I ſhall add, that I have ſometimes had the opportunity to obſerve a thing, that may make to my preſent purpoſe; namely, that when the ſun was veiled over as it were, with a thin white cloud, and yet was too bright to be looked upon directly without dazzling, by caſting my eyes upon a ſmooth water, as we ſometimes do to obſerve eclipſes without prejudice to our eyes, the ſun then not far from the meridian appeared to me not red, but ſo white, that it was not without ſome wonder, that I made the obſervation. Beſides, though we in *Engliſh* are wont to ſay, a thing is red-hot, as an expreſſion of its being ſuperlative *ignitum* (if I may ſo ſpeak for want of a proper *Engliſh* word) yet in the forges of ſmiths, and the furnaces of other artificers, by that which they call a white heat, they mean a further degree of ignition, than by that which both they and we call a red heat.

5. SECONDLY, I conſider, that common experience informs us, that as much light overpowers the eye, ſo when the ground is covered with ſnow (a body extremely white) thoſe that have weak eyes are wont to complain of too much light: and even thoſe, that have not, are generally ſenſible of an extraordinary meaſure of light in the air; and if they are fain to look very long upon ſnow, find their ſight offended by it. On which occaſion we may call to mind what *Xenophon* relates, that his *Cyrus* marching his army for divers days through mountains covered with ſnow, the dazzling ſplendor of its whiteneſs prejudiced the ſight of very many of his ſoldiers, and blinded ſome of them; and other ſtories of that nature may be met with in writers of good note. And the like has been affirmed to me by credible perſons of my own acquaintance, and eſpecially by one, who, though ſkilled in phyſic, and not antient, confeſſed to me, when I purpoſely aſked him, that not only during his ſtay in *Muſcovy* he found his eyes much impaired, by being much reduced frequently to travel in the ſnow; but that the weakneſs of his eyes did not leave him when he left that country, but has followed him into theſe parts, and yet continues to trouble him. And to this doth agree what I as well as others have obſerved, namely, that when I travelled by night, when the ground was all covered with ſnow, though the night otherwiſe would not have been lightſome, yet I could very well ſee to chuſe my way. But much more remarkable to my preſent purpoſe is that, which I have met with in *Olaus Magnus*, concerning the way of travelling in winter in the Northern regions, where the days of that ſeaſon are ſo very ſhort: for after other things not needful to be here tranſcribed; *Iter*, ſays he, *diurnum duo ſcilicet montana milliaria (quæ 12 Italica ſunt) conficiunt. Nocte verò ſub ſplendidiſſima luna, duplatum iter conſumunt aut triplatum. Neque id incommodè fit, cum nivium reverberatione lunaris ſplendor ſublimes & declives campos illuſtret, ac etiam montium præcipitia ac noxias feras à longè proſpiciant evitandas.*

Gent. Septent. Hiſtor. lib. 4. cap. 13.

evitandas. Which testimony I the less scruple to allege, because it agrees very well with what has been affirmed to me by a physician of *Muscow*, whom the notion I have been treating of concerning whiteness invited me to ask, whether he could not see much farther, when he travelled by night in *Russia* than he could do in *England*, or elsewhere, when there was no snow upon the ground; for this ingenious person informed me, that he could see things at a far greater distance, and with more clearness, when he travelled by night on the *Russian* snow, though without the assistance of moon-shine, than we in these parts would easily be persuaded. Though it seems not unlikely to me, that the intenseness of the cold may contribute something to the considerableness of the effect, by much clearing the air of darkish steams, which in these more temperate climates are wont to thicken it in snowy weather: for having purposely enquired of this doctor, and consulted that ingenious navigator Captain *James*'s voyage hereafter to be further mentioned, I find both their relations agree in this, that in dark frosty nights they could discover more stars, and see the rest clearer, than we in *England* are wont to do.

6. I know indeed, that divers learned men think, that snow so strongly affects our eyes, not by a borrowed, but a native light; but I venture to give it as a proof, that white bodies reflect more light than others, because having once purposely placed a parcel of snow in a room carefully darkened, that no celestial light might come to fall upon it, neither I, nor an ingenious person (skilled in optics) whom I desired for a witness, could find, that it had any other light than what it received. And however, it is usual among those that travel in dark nights, that the guides wear something of white to be discerned by, there being scarce any night so dark, but that in the free air there remains some light, though broken and debilitated perhaps by a thousand reflections from the opacous corpuscles that swim in the air, and send it to one another before it comes to arrive at the eye.

7. Thirdly, And the better to shew that white bodies reflect store of light, in comparison of those that are otherwise coloured, I did in the darkened room, formerly mentioned, hold not far from the hole, at which the light was admitted, a sheet only of white paper, from whence casting the sun-beams upon a white wall, whereunto it was obverted, it manifestly appeared both to me, and to the person I took for a witness of the experiment, that it reflected a far greater light, than any of the other colours formerly mentioned; the light so thrown upon the wall notably enlightning it, and by it a good part of the room. And yet further to shew you, that white bodies reflect the beams from them, and not towards themselves, let me add, that ordinary burning glasses, such as are wont to be employed to light tobacco, will not in a great while burn, or so much as discolour a sheet of white paper. Insomuch that even when I was a boy, and loved to make trials with burning-glasses, I could not but wonder at this odd phænomenon, which set me very early upon guessing at the nature of whiteness; especially because I took notice, that the image of the sun upon a white paper was not so well defined (the light seeming too diffused) as upon black, and because I tried, that blacking over the paper with ink, not only the ink would be quickly dried up, but the paper, that I could not burn before, would be quickly set on fire. I have also tried, that my exposing my hand with a thin black glove over it to the warm sun, it was thereby very quickly and considerably more heated, than if I took off the glove, and held my hand naked, or put on it another glove of thin but white leather. And having thus shewn you, *Pyrophilus*, that white bodies reflect the most light of any, let us now proceed to consider, what is further to be taken notice of in them, in order to our present enquiry.

8. AND fourthly, whereas among the dispositions we attributed to white bodies, we also intimated this, that such bodies are apt, like speculums, though but imperfect ones, to reflect the light that falls on them untroubled or unstained, we shall, besides other particulars to be met with in these papers, offer you this in favour of the conjecture; that in the darkened room several times mentioned in this treatise, we tried, that the sun beams being cast from a coloured body upon a neighbouring white wall, the determinate colour of the body was from the wall reflected to the eye; whereas we could in divers cases manifestly alter the colour arriving at the eye, by substituting at a convenient distance, a (conveniently) coloured (and glossy) body, instead of the white wall: as by throwing the beams from a yellow body upon a blue, there would be exhibited a kind of green, as in the experiments about colours is more fully declared.

9. I KNOW not whether I should on this occasion take notice, that when, as when looking upon the calm and smooth surface of a river betwixt my eye and the sun, it appeared to be a natural speculum, wherein that part, which reflected to my eye the entire and defined image of the sun, and the beams less remote from those which exhibited that image, appeared indeed of a great and whitish brightness, but the rest comparatively dark enough; if afterwards the superficies chanced to be a little, but not much troubled by a gentle breath of wind, and thereby reduced into a multitude of small and smooth speculums, the surface of the river would, suitably to the doctrine lately delivered, at a distance appear very much of kin to white, though it would lose that brightness or whiteness upon the return of the surface to calmness and an uniform level. And I have sometimes, for trial sake, brought by a lenticular glass the image of a river, shined upon by the sun, into an upper room darkened, and distant about a quarter of a mile from the river; by which means the numerous declining surfaces of the water appeared so contracted, that upon the body, that received the images, the whole river appeared a very white object at two or three paces distance. But if we drew near it, this whiteness appeared to proceed from an innumerable company of lucid reflections, from the several gently waved superficies of the water, which looked near at hand like a multitude of very little, but shining scales of fish, of which many did every moment disappear, and as many were by the sun, wind and river generated anew. But though this observation seemed sufficiently to discover, how the appearing whiteness in that case was produced, yet in some other cases water may have the same, though not so vivid a colour upon other accounts; for oftentimes it happens, that the smooth surface of the water does appear bright or whitish, by reason of the reflection not immediately of the images of the sun, but of the brightness of the sky; and in such cases a convenient wind may, where it passes along, make the surface look black, by causing many such furrows and cavities, as may make the inflected superficies of the water reflect the brightness of the sky rather inward than outward. And again, if the wind increase into a storm, the water may appear white, especially near the shore and the ship; namely because the rude agitation breaks it into foam or froth. So much do whiteness and blackness depend upon the disposition of the superficial parts of a body, to reflect the beams of light inward or outward. But that as white bodies reflect the most light of any, so their superficial particles are, in the sense newly delivered, of a specular nature; I shall now further endeavour to shew, both by the making of specular bodies white, and the making of a white body specular.

10. IN the fifth place then, I will inform you, that (not to repeat what *Gassendus* observes concerning water) I have for curiosity sake distilled quicksilver in a cucurbit,

bit, fitted with a capacious glaſs-head, and obſerved, that when the operation was performed by the degrees of fire requiſite for my purpoſe, there would ſtick to the inſide of the alembic a multitude of little round drops of mercury: and as you know, that mercury is a ſpecular body, ſo each of theſe little drops was a ſmall round looking-glaſs; and a multitude of them lying thick and near one another, they did both in my judgment, and that of thoſe invited to ſee it, make the glaſs they were faſtened to, appear manifeſtly a white body. And yet, as I ſaid, this whiteneſs depended upon the minuteneſs and nearneſs of the little mercurial globuli, the convexity of whoſe ſurfaces fitted them to repreſent in a narrow compaſs a multitude of little lucid images to differingly ſituated beholders. And here let me obſerve a thing, that ſeems much to countenance the notion I have been recommending; namely, that whereas divers parts of the ſky, and eſpecially the milky way, do to the naked eye appear white (as the name itſelf imports) yet the galaxy looked upon through the teleſcope does not ſhew white, but appears to be made up of a vaſt multitude of little ſtars; ſo that a multitude of lucid bodies, if they be ſo ſmall, that they cannot ſingly or apart be diſcerned by the eye, and if they be ſufficiently thick ſet by one another, may by their confuſed beams appear to the eye one white body. And why is it not poſſible, that the like may be done, when a multitude of bright and little corpuſcles being crouded together, are made to ſend together vivid beams to the eye, though they ſhine but, as the planets, by a borrowed light?

11. But to return to our experiments. We may take notice, that the white of an egg, though in part tranſparent, yet by its power of reflecting ſome incident rays of light, is in ſome meaſure a natural ſpeculum, being long agitated with a whiſk or ſpoon, loſes its tranſparency, and becomes very white, by being turned into froth, that is, into an aggregate of numerous ſmall bubbles, whoſe convex ſuperficies fits them to reflect the light every way outwards. And it is worth noting, that when water, for inſtance, is agitated into froth, if the bubbles be great and few, the whiteneſs will be but faint, becauſe the number of ſpecula within a narrow compaſs is but ſmall, and they are not thick ſet enough to reflect ſo many little images or beams of the lucid body, as are requiſite to produce a vigorous ſenſation of whiteneſs. And partly, leſt it ſhould be ſaid, that the whiteneſs of ſuch globulous particles proceeds from the air included in the froth (which to make good, it ſhould be proved that the air itſelf is white); and partly, to illuſtrate the better the notion we have propoſed of whiteneſs, I ſhall add, that I purpoſely made this experiment: I took a quantity of fair water, and put to it, in a clear glaſs phial, a convenient quantity of oil or ſpirit of turpentine, becauſe that liquor will not incorporate with water, and yet is almoſt as clear and colourleſs as it. Theſe being gently ſhaken together, the agitation breaks the oil (which, as I ſaid, is indiſpoſed to mix like wine or milk *per minima* with the water) into a multitude of little globes, which each of them reflecting outwards a lucid image, make the imperfect mixture of the two liquors appear whitiſh; but if by vehemently ſhaking the glaſs, for a competent time, you make a further comminution of the oil into far more numerous and ſmaller globuli, and thereby confound it alſo better with the water, the mixture will appear of a much greater whiteneſs, and almoſt like milk: whereas if the glaſs be a while let alone, the colour will by degrees impair, as the oily globes grow fewer and bigger, and at length will quite vaniſh, leaving both the liquors diſtinct and diaphanous as before. And ſuch a trial hath not ill ſucceeded, when inſtead of the colourleſs oil of turpentine, I took a yellow mixture made of a good proportion of crude turpentine diſſolved in that liquor; and (if I miſ-remember not) it alſo ſucceeded better than one would expect, when I

employed an oil brought by filings of copper, infused in it, to a deep green. And this (by the way) may be the reason, why oftentimes when the oils of some spices and of aniseeds, *&c.* are distilled in a limbec with water, the water (as I have several times observed) comes over whitish, and will perhaps continue so for a good while; because if the fire be made too strong, the subtile chymical oil is thereby much agitated and broken, and blended with the water in such numerous and minute globules, as cannot easily in a short time emerge to the top of the water, and whilst they remain in it, make it, for the reason newly intimated, look whitish. And perhaps upon the same ground a cause may be rendered, why hot water is observed to be usually more opacous and whitish, than the same water cold; the agitation turning the more spirituous or otherwise conveniently disposed particles of the water into vapours, thereby producing in the body of the liquor a multitude of small bubbles, which interrupt the free passage, that the beams of light would else have every way, and from the innermost parts of the water reflect many of them outwards. These and the like examples, *Pyrophilus*, have induced me to suspect, that the superficial particles of white bodies may for the most part be as well convex as smooth: I content myself to say, *suspect*, and *for the most part*, because it seems not easy to prove, that when diaphanous bodies, as we shall see by and by, are reduced into white powders, each corpuscle must needs be of a convex superficies, since perhaps it may suffice that specular surfaces look several ways. For (as we have seen) when a diaphanous body comes to be reduced to very minute parts, it thereby acquires a multitude of little surfaces within a narrow compass. And though each of these should not be of a figure convenient to reflect a round image of the sun, yet even from such an inconveniently figured body there may be reflected some (either streight or crooked) physical line of light; which line I call physical, because it has some breadth in it, and in which line in many cases some refraction of the light falling upon the body it depends on may contribute to the brightness: as if a slender wire, or solid cylinder of glass, be exposed to the light, you shall see in some part of it a vivid line of light; and if we were able to draw out and lay together a multitude of these little wires or thrids of glass, so slender, that the eye could not discern a distance betwixt the luminous lines, there is little doubt (as far as I can guess by a trial purposely made with very slender, but far less slender thrids of glass, whose aggregate was looked upon one way white) but the whole physical superficies composed of them would to the eye appear white; and if so, it will not be always necessary that the figure of those corpuscles, that make a body appear white, should be globulous. And as for snow itself, though the learned *Gassendus* (as we have seen above) makes it to seem nothing else but a pure frozen froth, consisting of exceedingly minute and thick-set bubbles; yet I see no necessity of admitting that, since not only by the variously and curiously figured snow, that I have divers times had the opportunity with pleasure to observe, but also by the common snow, it rather doth appear both to the naked eye, and in a microscope, often, if not most commonly, to consist principally of little slender icicles of several shapes, which afford such numerous lines of light, as we have been newly speaking of.

12. SIXTHLY, If you take a diaphanous body, as for instance a piece of glass, and reduce it to powder, the same body, which, when it was entire, freely transmitted the beams of light, acquiring by contusion a multitude of minute surfaces, each of which is as it were a little, but imperfect speculum, is qualified to reflect, in a confused manner, so many either beams, or little and singly unobservable images of the lucid body, that from a diaphanous it degenerates into a white body. And I remember,

ber, I have for trial's sake taken lumps of rock crystal, and heating them red-hot in a crucible, I found, according to my expectation, that being quenched in fair water, even those, that remained in seemingly entire lumps, exchanged their transluceney for whiteness, the ignition and extinction having as it were cracked each lump into a multitude of minute bodies, and thereby given it a great multitude of new surfaces. And even with diaphanous bodies, that are coloured, there may be this way a greater degree of whiteness produced, than one would lightly think; as I remember, I have by contusion obtained whitish powders of granates, glass of antimony, and emeralds finely beaten; and you may more easily make the experiment, by taking good venereal vitriol of a deep blue, and comparing with some of the entire crystals purposely reserved some of the subtile powder of the same salt, which will comparatively exhibit a very considerable degree of whiteness.

13. SEVENTHLY, And as by a change of position in the parts, a body that is not white may be made white; so by a slight change of the texture of its surface, a white body may be deprived of its whiteness. For if (as I have tried in goldsmiths shops) you take a piece of silver, that has been freshly boiled, as the artificers call it (which is done by first brushing, and then decocting it with salt and tartar, and perhaps some other ingredients) you shall find it to be of a lovely white. But if you take a piece of smooth steel, and therewith burnish a part of it, which may be presently done, you shall find, that part will lose its whiteness, and turn a speculum, looking almost every where dark, as other looking-glasses do; which may not a little confirm our doctrine. For by this we may guess, what it is chiefly that made the body white before, by considering that all, that was done to deprive it of that whiteness, was only to depress the little protuberances, that were before on the surface of the silver, into one continued superficies, and thereby effect this, that now the image of the lucid body, and consequently a kind of whiteness shall appear to your eye; but in some place of the greater silver looking-glass (whence the beams reflected at an angle equal to that wherewith they fall on it, may reach your eye) whilst the asperity remained undestroyed, the light falling on innumerable little specula obverted some one way, and some another, did from all sensibly distinguishable parts of the superficies reflect confused beams or representations of light to the beholder's eye, from whence soever he chance to look upon it. And among the experiments annexed to this discourse, you will find one, wherein, by the change of texture in bodies, whiteness is in a trice both generated and destroyed.

CHAP. II.

1. WHAT we have discoursed of whiteness, may somewhat assist us to form a notion of blackness, those two qualities being contrary enough to illustrate each other. Yet among the antient philosophers I find less assistance to form a notion of blackness than of whiteness; only *Democritus* in the passage above recited out of *Aristotle* has given a general hint of the cause of this colour, by referring the blackness of bodies to their asperity. But this I call but a general hint, because those bodies that are green, and purple, and blue, seem to be so, as well as black ones, upon the account of their superficial asperity. But among the moderns, the formerly mentioned *Gassendus*, perhaps invited by this hint of *Democritus*, has incidentally in another epistle given us, though a very short, yet a somewhat clearer account of the nature of blackness in these words: *Existimare par est corpora suâpte naturâ nigra constare ex particulis, quarum superficiculæ scabræ sint, nec facilè lucem extrorsum reflectant.*

I wish

I wish this ingenious man had enlarged himself upon this subject; for indeed it seems, that as that, which makes a body white, is chiefly such a disposition of its parts, that it reflects (I mean without much interruption) more of the light that falls on it, than bodies of any other colour do; so that, which makes a body black, is principally a peculiar kind of texture, chiefly of its superficial particles, whereby it does as it were dead the light that falls on it, so that very little is reflected outwards to the eye.

2. AND this texture may be explicated two, and perhaps more than two several ways; whereof the first is by supposing in the superficies of the black body a particular kind of asperity, whereby the superficial particles reflect but few of the incident beams outwards, and the rest inwards towards the body itself. As if, for instance, we should conceive the surface of a black body to be asperated by an almost numberless throng of little cylinders, pyramids, cones, and other such corpuscles, which, by their being thick set and erected, reflect the beams of light from one to another inwards, and send them to and fro so often, that at length they are lost, before they can come to rebound out again to the eye. And this is the first of the two mentioned ways of explicating blackness. The other way is by supposing the texture of black bodies to be such, that either by their yielding to the beams of light, or upon some other account, they do as it were dead the beams of light, and keep them from being reflected in any plenty, or with any considerable vigour or motion, outwards. According to this notion it may be said, that the corpuscles, that make up the beams of light, whether they be solary effluviums, or minute particles of some ætherial substance, thrusting on one another from the lucid body, do, falling on black bodies, meet with such a texture, that such bodies receive into themselves, and retain almost all the motion communicated to them by the corpuscles that make up the beams of light, and consequently reflect but few of them, or those but languidly, towards the eye; it happening here almost in like manner as to a ball, which thrown against a stone or floor would rebound a great way upwards, but rebounds very little or not at all, when it is thrown against water, or mud, or a loose net, because the parts yield, and receive into themselves the motion, on whose account the ball should be reflected outwards. But this last way of explicating blackness I shall content myself to have proposed, without either adopting it, or absolutely rejecting it. For the hardness of touch-stones, black marble, and other bodies, that being black are solid, seem to make it somewhat improbable, that such bodies should be of so yielding a texture, unless we should say, that some bodies may be more disposed to yield to the impulses of the corpuscles of light by reason of a peculiar texture, than other bodies, that in other trials appear to be softer than they. But though the former of these two explications of blackness be that, by which we shall endeavour to give an account of it; yet, as we said, we shall not absolutely reject this latter, partly because they both agree in this, that black bodies reflect but little of the light that falls on them, and partly because it is not impossible, that in some cases both the disposition of the superficial particles, as to figure and position, and the yielding of the body, or some of its parts, may jointly, though not in an equal measure concur to the rendering of a body black. The considerations, that induced me to propose this notion of blackness, as I explained it, are principally these:

3. FIRST, That as I lately said, whiteness and blackness being generally reputed to be contrary qualities, whiteness depending, as I said, upon the disposition of the parts of a body to reflect much light, it seems likely, that blackness may depend upon a contrary disposition of the black bodies surface; but upon this I shall not insist.

4. NEXT

4. Next then we ſee, that if a body of one and the ſame colour be placed, part in the ſun-beams, and part in the ſhade, that part which is not ſhined on will appear more of kin to blackneſs than the other, from which more light rebounds to the eye; and dark colours ſeem the blacker, the leſs light they are looked upon in; and we think all things black in the dark, when they ſend no beams to make impreſſions on our organs of ſight: ſo that ſhadows and darkneſs are near of kin, and ſhadow, we know, is but a privation of light: and accordingly blackneſs ſeems to proceed from the paucity of beams reflected from the black body to the eye; I ſay, the paucity of beams, becauſe thoſe bodies, that we call black, as marble, jett, *&c.* are ſhort of being perfectly ſo, elſe we ſhould not ſee them at all. But though the beams, that fall on the ſides of thoſe erected particles, that we have been mentioning, do few of them return outwards, yet thoſe, that fall upon the points of thoſe cylinders, cones, or pyramids, may thence rebound to the eye, though they make there but a faint impreſſion, becauſe they arrive not there, but mingled with a great proportion of little ſhades. This may be confirmed by my having procured a large piece of black marble well poliſhed, and brought to the form of a large ſpherical and concave ſpeculum; for on the inſide this marble being well poliſhed, was a kind of dark looking-glaſs, wherein I could plainly ſee a little image of the ſun, when that ſhined upon it. But this image was very far from offending and dazzling my eyes, as it would have done from another ſpeculum; nor, though the ſpeculum were large, could I in a long time, or in a hot ſun, ſet a piece of wood on fire, though a far leſs ſpeculum of the ſame form, and of a more reflecting matter, would have made it flame in a trice.

5. And on this occaſion we may as well in reference to ſomething formerly delivered concerning whiteneſs, as in reference to what has been newly ſaid, ſubjoin what we further obſerved touching the differing reflections of light from white and black marble; namely, that having taken a pretty large mortar of white marble, new, and poliſhed in the inſide, and expoſed it to the ſun, we found, that it reflected a great deal of glaring light, but ſo diſperſed, that we could not make the reflected beams concur in any ſuch conſpicuous focus, as that newly taken notice of in the black marble; though perhaps there may enough of them be made to meet near the bottom, to make ſome kind of focus, eſpecially ſince by holding in the night-time a candle at a convenient diſtance, we were able to procure a concourſe of ſome, though not many of the reflected beams, at about two inches diſtant from the bottom of the mortar: but we found the heat even of the ſun-beams ſo diſperſedly reflected to be very languid, even in compariſon of the black marble's focus. And the little picture of the ſun, that appeared upon the white marble as a ſpeculum, was but very faint and exceeding ill defined. Secondly, that taking two pieces of plain and poliſhed ſurfaces, and caſting on them ſucceſſively the beams of the ſame candle, in ſuch manner, as that the neighbouring ſuperficies being ſhaded by an opacous and perforated body, the incident beams were permitted to paſs but through a round hole of about half an inch diameter, the circle of light, that appeared on the white marble, was in compariſon very bright, but very ill defined; whereas that on the black marble was far leſs luminous, but much more preciſely defined.

6. Thirdly, when you look upon a piece of linen, that has ſmall holes in it, thoſe holes appear very black, and men are often deceived in taking holes for ſpots of ink; and painters, to repreſent holes, make uſe of black; the reaſon of which ſeems to be, that the beams, that fall on thoſe holes, fall into them ſo deep, that none of them is reflected back to the eye. And in narrow wells part of the mouth ſeems black,

black, becauſe the incident beams are reflected downwards from one ſide to another, till they can no more rebound to the eye.

WE may conſider too, that if differing parts of the ſame piece of black velvet be ſtroaked oppoſite ways, the piece of velvet will appear of two diſtinct kinds of blackneſs, the one far darker than the other; of which diſparity the reaſon ſeems to be, that in the leſs obſcure part of the velvet, the little ſilken piles, whereof it is made up, being inclined, there is a greater part of each of them obverted to the eye; whereas in the other part the piles of ſilk being more erected, there are far fewer beams reflected outwards from the lateral parts of each pile; ſo that moſt of thoſe, that rebound to the eye, come from the tops of the piles, which make but a ſmall part of the whole ſuperficies, that may be covered by the piece of velvet. Which explication I propoſe, not that I think the blackneſs of the velvet proceeds from the cauſe aſſigned, ſince each ſingle pile of ſilk is black by reaſon of its texture, in what poſition ſoever you look upon it; but that the greater blackneſs of one of theſe tufts ſeems to proceed from the greater paucity of beams reflected from it, and that from the fewneſs of thoſe parts of a ſurface, that reflect beams, and the multitude of thoſe ſhaded parts, that reflect none. And I remember, that I have oftentimes obſerved, that the poſition of particular bodies far greater than piles of ſilk in reference to the eye, may, notwithſtanding their having each of them a colour of its own, make one part of their aggregate appear far darker than the other; for I have near great towns often take notice, that a cart-load of carrots packed up appeared of a much darker colour when looked upon, where the points of the carrots were obverted to the eye, than where the ſides of them were ſo.

7. FOURTHLY, in a darkened room, I purpoſely obſerved, that if the ſun-beams, which came in at the hole, were received upon white or any other colour, and directed to a convenient place of the room, they would manifeſtly, though not all equally, increaſe the light of that part; whereas if we ſubſtituted, either a piece of black cloth or black velvet, it would ſo dead the incident beams, that the place (newly mentioned) whereto I obverted the black body, would be leſs enlightened than it was before, when it received its light but from the weak and oblique reflections of the floor and walls of a pretty large room, through which the beams, that came in at the hole, were confuſedly and brokenly diſperſed.

8. FIFTHLY, And to ſhew, that the beams, that fall on black bodies, as they do not rebound outwards to the eye, ſo they are reflected towards the body itſelf, as the nature of thoſe erected particles, to which we have imputed blackneſs, requires, we will add an experiment, that will alſo confirm our doctrine touching whiteneſs; namely, that we took a broad and large tile, and having whitened over one half of the ſuperficies of it, and blacked the other, we expoſed it to the ſummer's ſun; and having let it lie there a convenient time (for the difference is more apparent, if it have not lain there too long) we found, as we expected, that whilſt the whited part of the tile remained cool enough, the blacked part of the ſame tile was grown not only ſenſible, but very hot (ſometimes to a ſtrong degree.) And to ſatisfy ſome of our friends the more, we have ſometimes left upon the ſurface of the tile, beſides the white and black parts thereof, a part, that retained the native red of the tile itſelf; and expoſing them to the ſun, we obſerved this laſt mentioned to have contracted a heat in compariſon of the white, but a heat inferior to that of the black; of which the reaſon ſeems to be, that the ſuperficial particles of black bodies, being, as we ſaid, more erected, than thoſe of white or red ones, the corpuſcles of light falling on their ſides, being for the moſt part reflected inward from one particle to another, and

7 thereby

thereby engaged as it were, and kept from rebounding upwards, they communicate their brisk motion, wherewith they were impelled against the black body (upon whose account, had they fallen upon a white body, they would have been reflected outwards) to the small parts of the black body, and thereby produce in those small parts such an agitation, as (when we feel it) we are wont to call heat. I have been lately informed, that an observation near of kin to ours has been made by some learned men in *France* and *Italy*, by long exposing to a very hot sun two pieces of marble, the one white and the other black. But though the observation be worthy of them, and may confirm the same truth with our experiment, yet besides that our trial needs not the summer, nor any great heat to succeed, it seems to have this advantage above the other, that whereas bodies more solid, and of a closer texture, though they use to be more slowly heated, are wont to receive a greater degree of heat from the sun or fire, than *(cæteris paribus)* bodies of a slighter texture. I have found by the information of stone-cutters, and by other ways of enquiry, that black marble is much solider and harder than white; so that possibly the difference betwixt the degrees of heat, they receive from the sun-beams, will by many be ascribed to the difference of their texture, rather than to that of their colour; though I think our experiment will make it probable enough, that the greater part of that difference may well be ascribed to that disposition of parts, which makes the one reflect the sun-beams inward, and the other outwards. And with this doctrine accords very well, that rooms hung with black are not only darker than else they would be, but are wont to be warmer too; insomuch that I have known a great lady, whose constitution whas somewhat tender, complain, that she was wont to catch cold, when she went out into the air, after having made any long visits to persons, whose rooms were hung with black. And this is not the only lady I have heard complain of the warmth of such rooms; which though perhaps it may be partly imputed to the effluvia of those materials, wherewith the hangings were dyed, yet probably the warmth of such rooms depends chiefly upon the same cause, that the darkness does; as (not to repeat what I formerly noted touching my gloves) to satisfy some curious persons of that sex, I have convinced them, by trial, that of two pieces of silken stuff given to me by themselves, and exposed in their presence to the same window, shined on by the sun, the white was considerably heated, when the black was not so much as sensibly so.

9. Sixthly, I remember, that acquainting one day a Virtuoso of unsuspected credit, that had visited hot countries, with part of what I have here delivered concerning blackness, he related to me, by way of confirmation of it, a very notable experiment, which he had both seen others make, and made himself in a warm climate; namely, that having carefully blacked over eggs, and exposed them to the hot sun, they were thereby in no very long time well roasted; to which effect I conceive the heat of the climate must have concurred with the disposition of the black surface to reflect the sun-beams inward: for I remember, that having made that among other trials in *England*, though in summer-time, the eggs I exposed acquired indeed a considerable degree of heat, but yet not so intense a one, as proved sufficient to roast them.

10. Seventhly, and lastly, our conjectures at the nature of blackness may be somewhat confirmed by the (formerly mentioned observation of the blind *Dutchman*, that discerns colours with his fingers; for he says, that he feels a greater roughness upon the surfaces of black bodies, than upon those of red, or yellow, or green. And I remember, that the diligent *Bartholinus* says, that a blind Earl of *Mansfield* could distinguish white from black only by the touch; which would sufficiently argue a great disparity *Hist. Anatom. Cent. 3.*

disparity in the asperities, or other superficial textures of bodies of those two colours, if the learned relator had affirmed the matter upon his own knowledge.

11. THESE, *Pyrophilus*, are the chief things, that occur to me at present, about the nature of whiteness and blackness; which if they have rendered it so much as probable, that in most, or at least many cases, the causes of these qualities may be such as I have adventured to deliver, it is as much as I pretend to. For till I have opportunity to examine the matter by some further trials, I am not sure, but that in some white and black bodies, there may concur to the colour some peculiar texture or disposition of the body, whereby the motion of the small corpuscles, that make up the incident beams of light, may be differingly modified, before they reach the eye; especially in this, that white bodies do not only copiously reflect those incident corpuscles outwards, but reflect them briskly, and do not otherwise alter them in the manner of their motion. Nor shall I now stay to inquire, whether some of those other ways (as a disposition to alter the velocity, the rotation, or the order and manner of appulse to the eye of the reflected corpuscles, that composed the incident beams of light) which we mentioned, when we considered the production of colours in general, may not in some cases be applicable to those of white and black bodies: for I am yet so much a seeker in this matter, and so little wedded to the opinions I have proposed, that what I am to add, shall be but the beginning of a collection of experiments and observations towards the history of whiteness and blackness, without at present interposing my explications of them; that so I may assist your enquiries, without much forestalling or byassing your judgment.

EXPERIMENT in CONSORT,

Touching WHITENESS *and* BLACKNESS.

EXPERIMENT I.

HAVING promised in the 27th page of the foregoing discourse of whiteness and blackness, to shew, that those two colours may, by a change of texture in bodies, each of them apart diaphanous and colourless, be at pleasure and in a trice as well generated as destroyed, we shall begin with experiments, that may acquit us of that promise.

TAKE then what quantity you please of fair water, and having heated it, put into it as much good common sublimate, as it is able to dissolve, and (to be sure of having it well glutted) continue putting in the sublimate, till some of it lie untouched in the bottom of the liquor. Filter this solution through cap-paper, to have it clear and limpid, and into a spoonful or two thereof (put into a clean glass-vessel) shake about four or five drops (according as you took more or less of this solution) of good limpid spirits of urine, and immediately the whole mixture will appear white like milk; to which mixture if you presently add a convenient proportion of rectified aqua fortis (for

(for the number of drops is hard to determine, because of the differing strength of the liquor, but easily found by trial) the whiteness will presently disappear, and the whole mixture become transparent; which you may, if you please, again reduce to a good degree of whiteness (though inferior to the first) only by a more copious affusion of fresh spirit of urine. *N. B.* First, that it is not so necessary to employ either aqua fortis or spirit of urine about this experiment, but that we have made it with other liquors instead of these; of which perhaps more elsewhere. Secondly, that this experiment, though not made with the same menstruums, nor producing the same colour, is yet much of kin to that other to be mentioned in this tract, among our other experiments of colours, about turning a solution of precipitate into an orange colour; and the chymical reason being much alike in both, the annexing it to one of them may suffice for both.

EXPERIMENT II.

MAKE a strong infusion of broken galls in fair water; and having filtered it into a clean phial, add more of the same liquor to it, till you have made it somewhat transparent, and sufficiently diluted the colour, for the credit of the experiment, lest otherwise the darkness of the liquor might make it be objected, that it was already almost ink. Into this infusion shake a convenient quantity of a clear, but very strong solution of vitriol; and you shall immediately see the mixture turn black almost like ink, and such a way of producing blackness is vulgar enough; but if presently after you do upon this mixture drop a small quantity of good oil of vitriol, and, by shaking the phial, disperse it nimbly through the two other liquors, you shall (if you perform your part well, and have employed oil of vitriol clear and strong enough) see the darkness of the liquor presently begin to be discussed, and grow pretty clear and transparent, losing its inky blackness, which you may again restore to it by the affusion of a small quantity of a very strong solution of salt of tartar. And though neither of these atramentous liquors will seem other than very pale ink, if you write with a clean pen dipt in them; yet that is common to them with some sorts of ink, that prove very good when dry; as I have also found, that when I made these carefully, what I wrote with either of them, especially with the former, would, when thoroughly dry, grow black enough not to appear bad ink. This experiment of taking away, and restoring blackness from and to the liquors, we have likewise tried in common ink; but there it succeeds not so well, and but very slowly, by reason that the gum wont to be employed in the making it does by its tenacity oppose the operations of the above-mentioned saline liquors. But to consider gum no more, what some kind of precipitation may have to do in the producing and destroying the inks without it, I have elsewhere given you some occasion and assistance to inquire: but I must not now stay to do so myself, only I shall take notice to you, that though it be taken for granted, that bodies will not be precipitated by alcalizate salts, that have not first been dissolved in some acid menstruums; yet I have found upon trials, which my conjectures led me to make on purpose, that divers vegetables, barely infused, or, but slightly decocted in common water, would, upon the affusion of a strong and clear lixivium of pot-ashes, and much more of some other precipitating liquors that I sometimes employ, afford good store of a curdled matter, such as I have had in the precipitations of vegetable substances, by the intervention of acid things; and that this matter was easily separable from the rest of the liquor, being left behind it in the filtre. And in making the first ink mentioned in this experiment, I found,

I found, that I could by filtration separate pretty store of a black pulverable substance, that remained in the filtre; and when the ink was made clear again by the oil of vitriol, the affusion of dissolved *sal tartari* seemed but to precipitate, and thereby to unite and render conspicuous the particles of the black mixture, that had before been dispersed into very minute and singly invisible particles by the incisive and resolving power of the highly corrosive oil of vitriol.

AND to manifest, *Pyrophilus*, that galls are not so requisite as many suppose to the making atramentous liquors, we have sometimes tried the following experiment: we took dried rose-leaves, and decocted them for a while in fair water; into two or three spoonfuls of this decoction we shook a few drops of a strong and well filtrated solution of vitriol (which perhaps, had it been green, would have done as well) and immediately the mixture did turn black, and when into this mixture, presently after it was made, we shook a just proportion of aqua fortis, we turned it from a black ink to a deep red one, which by the affusion of a little spirit of urine may be reduced immediately to an opacous and blackish colour. And in regard, *Pyrophilus*, that in the former experiments, both the infusion of galls, and the decoction of roses, and the solution of copperas, employed about them, are endowed each of them with its own colour, there may be a more noble experiment of the sudden production of blackness made by the way mentioned in the second section of the second part of our essays; for though upon the confusion of the liquors there mentioned, there do immediately emerge a very black mixture; yet both the infusion of orpiment and the solution of minium were, before their being joined together, limpid and colourless.

EXPERIMENT III.

IF pieces of white hartshorn be with a competent degree of fire distilled in a glass-retort, they will, after the avolation of the phlegm, spirit, volatile salt, and the looser and the lighter parts of the oleaginous substance, remain behind of a coal-black colour. And even ivory itself being skilfully burnt (how I am wont to do it, I have elsewhere set down) affords painters one of the best and deepest blacks they have. And yet in the instance of distilled hartshorn, the operation being made in glass-vessels carefully closed, it appears there is no extraneous black substance, that insinuates itself into white harshorn, and thereby makes it turn black; but that the whiteness is destroyed, and the blackness generated, only by a change of texture, made in the burnt body, by the recess of some parts, and the transposition of others. And though I remember not, that in many distillations of hartshorn I ever found the *Cap. mort.* to pass from black to a true whiteness, whilst it continued in closed vessels; yet having taken out the coal-black fragments, and calcined them in open vessels, I could in few hours quite destroy that blackness, and without sensibly changing their bulk or figure, reduce them to great whiteness. So much do these two colours depend upon the disposition of the little parts, that the bodies, wherein they are to be met with, do consist of. And we find, that if white wine-tartar, or even the white crystals of such tartar be burnt without being truly calcined, the *Cap. mortuum* (as the chymists call the more fixt part) will be black. But if you further continue the calcination, till you have perfectly incinerated the tartar, and kept it long enough in a strong fire, the remaining calx will be white. And so we see, that not only other vegetable substances, but even white woods, as the hazel, will yield a black charcoal, and afterwards whitish ashes; and so animal substances naturally white, as bones and egg shells, will grow black upon the being burnt, and white again, when they are perfectly calcined.

EXPERIMENT IV.

BUT yet I much queſtion, whether that rule delivered by divers, as well philoſophers as chymiſts, *aduſta nigra, ſed peruſta alba*, will hold as univerſally as is preſumed, ſince I have ſeveral examples to allege againſt it. For I have found, that by burning alabaſter, ſo as both to make it appear to boil almoſt like milk, and to reduce it to a very fine powder, it would not at all grow black, but retain its pure and native whiteneſs; and though by keeping it longer than is uſual in the fire, I produced but a faint yellow, even in that part of the powder, that lay neareſt the top of the crucible; yet having purpoſely inquired of an experienced ſtone-cutter, who is curious enough in trying concluſions in his own trade, he told me he had found, that if alabaſter or plaiſter of *Paris* be very long kept in a ſtrong fire, the whole heap of burnt powder would exchange its whiteneſs for a much deeper colour than the yellow I obſerved. Lead being calcined with a ſtrong fire turns (after having perhaps run through divers other colours) into minium, whoſe colour we know is a deep red; and if you urge this minium, as I have purpoſely done with a ſtrong fire, you may much eaſier find a glaſſy and brittle body darker than minium, than any white calx or glaſs. It is known among chymiſts, that the white calx of antimony, by the further and more vehement operation of the fire, may be melted into glaſs, which we have obtained of a red colour, which is far deeper than that of the calx of burnt antimony. And though common glaſs of antimony being uſually adulterated with borax, have its colour thereby diluted, oftentimes to a very pale yellow; yet not only ours made more ſincerely was, as we ſaid, of a colour leſs remote from black, than was the calx; but we obſerved, that by melting it once or twice more, and ſo expoſing it to the further operation of the fire, we had, as we expected, the colour heightened. To which we ſhall add but this one inſtance (which is worth the taking notice of, in reference to colours) that, if you take blue, but unſophiſticated, vitriol, and burn it very ſlowly, and with a gentle degree of heat, you may obſerve, that when it is burnt but a little, and yet ſo far as that you may rub it to powder betwixt your fingers, it will be of a white or whitiſh colour; but if you proſecute the calcination, this body, which by a light aduſtion was made white, will paſs through other colours, as gray, yellowiſh, and red; and if you further burn it with a long and vehement fire, by that time it comes to be *peruſtum*, it will be of a dark purple, nearer to black, not only than the firſt calx, but than the vitriol before it at all felt the fire. I might add, that *Crocus Martis (per ſe*, as they call it) made by the laſting violence of the reverberated flames is not ſo near a-kin to white, as the iron or ſteel that afforded it was before its calcinations; but that I ſuppoſe, theſe inſtances may ſuffice to ſatisfy you, that minerals are to be excepted out of the forementioned rule, which perhaps, though it ſeldom fail in ſubſtances belonging to the vegetable or animal kingdom, may yet be queſtioned even in ſome of theſe, if that be true, which the judicious traveller *Bellonius* affirms, that charcoals made out of the wood of oxycedar are white: and I could not find, that though in retorts hart's-horn and other white bodies will be denigrated by heat, yet camphire would not at all loſe its whiteneſs, though I have purpoſely kept it in ſuch a heat, as made it melt and boil.

EXPERIMENT V.

AND now I ſpeak of camphire, it puts me in mind of adding this experiment, that though, as I ſaid, in cloſed glaſſes I could not denigrate it by heat, but it would ſublime to the ſides and top of the glaſs, as it was before; yet not only it will, being ſet on fire in the free air, ſend forth a copious ſmoke, but having purpoſely upon ſome of it that was flaming, clapt a large glaſs, almoſt in the form of a hive (but more ſlender only) with a hole at the top (which I cauſed to be made to try experiments of fire and flame in) it continued ſo long burning, that it lined all the inſide of the glaſs with a ſoot as black as ink, and ſo copious, that, the cloſeneſs of the veſſel conſidered, almoſt all that part of the white camphire, that did take fire, ſeemed to have been changed into that deep black ſubſtance.

EXPERIMENT VI.

AND this alſo brings into my mind another experiment, that I made about the production of blackneſs, whereof, for reaſons too long to be here deduced, I expected and found a good ſucceſs; and it was this: I took rectified oil of vitriol (that I might have the liquor clean as well as ſtrong) and by degrees mixt with it a convenient proportion of the eſſential oil, as chymiſts call it, of wormwood, drawn over with ſtore of water in a limbec; and warily diſtilling the mixture in a retort, there remained a ſcarce credible quantity of dry matter, black as a coal. And becauſe the oil of wormwood, though a chymical oil drawn by a Virtuoſo, ſeemed to have ſomewhat in it of the colour of the plant, I ſubſtituted in its room the pure and ſubtile eſſential oil of winter-ſavory, and mixing little by little this liquor with (if I miſ-remember not) an equal weight of the formerly mentioned rectified oil of vitriol, and diſtilling them as before in a retort, beſides what there paſſed over into the receiver, even theſe two clear liquors left me a conſiderable proportion (though not ſo great as the two former) of a ſubſtance black as pitch, which I yet keep by me as a rarity.

EXPERIMENT VII.

A WAY of whiting wax cheaply and in great quantity may be a thing of good œconomical uſe; and we have elſewhere ſet down the practice of tradeſmen that blanch it; but here treating of whiteneſs only, in order to the philoſophy of colours, I ſhall not examine, which of the ſlow ways may be beſt employed, to free wax from the yellow melleous parts, but ſhall rather ſet down a quick way of making it white, though but in very ſmall quantities. Take then a little yellow wax, ſcraped or thinly ſliced, and putting it into a bolt's-head or ſome other convenient glaſs, pour to it a pretty deal of ſpirit of wine, and placing the veſſel in warm ſand, increaſe the heat by degrees, till the ſpirit of wine begin to ſimmer or to boil a little; and continuing that degree of fire, if you have put liquor enough, you will quickly have the wax diſſolved: then taking it off the fire, you may either ſuffer it to cool as haſtily as with ſafety to the glaſs you can, or pour it, whilſt it is yet hot, into a filtre of paper; and either in the glaſs where it cools, or in the filtre, you will ſoon find the wax and menſtruum together reduced into a white ſubſtance, almoſt like butter,

 which

which by letting the ſpirit exhale will ſhrink into a much leſſer bulk, but ſtill retaining its whiteneſs. And that, which is pretty in the working of this magiſtery of wax, is, that the yellowneſs vaniſhes, neither appearing in the ſpirit of wine, that paſſes limpid through the filtre, nor in the butter of wax, if I may ſo call it, that, as I ſaid, is white.

EXPERIMENT VIII.

THERE is an experiment, *Pyrophilus*, which though I do not ſo exactly remember; and though it be ſomewhat nice to make, yet I am willing to acquaint you with, becauſe the thing produced, though it be but a curioſity, is wont not a little to pleaſe the beholders; and it is a way of turning, by the help of a dry ſubſtance, an almoſt golden-coloured concrete into a white one. The ſeveral trials are not at preſent ſo freſh in my memory to enable me to tell you certainly, whether an equal only or a double weight of common ſublimate muſt be taken in reference to the tin-glaſs; but, if I miſtake not, there was in the experiment, that ſucceeded beſt, two parts of the former taken to one of the latter. Theſe ingredients being finely powdered and exactly mixed, we ſublimed together by degrees of fire (the due gradation of which is in this experiment a thing of main importance;) there aſcended a matter of a very peculiar texture; for it was for the moſt part made up of very thin, ſmooth, ſoft and ſlippery plates, almoſt like the fineſt ſort of ſcales of fiſhes, but of ſo lovely a white inclining to pearl-colour, and of ſo curious a colour and ſhining a gloſs, that they appeared in ſome reſpect little inferior to orient pearls, and in other regards, they ſeemed to ſurpaſs them, and were applauded for a ſort of the prettieſt trifles, that we had ever prepared to amuſe the eye. I will not undertake, that though you will hardly miſs changing the colour of your ſhining tin-glaſs, yet you will the firſt or perhaps the ſecond time hit right upon the way of making the gliſtering ſublimate I have been mentioning.

EXPERIMENT IX.

WHEN we diſſolve in aqua fortis a mixture of gold and ſilver melted into one lump, it uſually happens, that the powder of gold, that falls to the bottom, as not being diſſoluble by that menſtruum, will not have its own yellow, but appear of a black colour, though neither the gold, nor the ſilver, nor the aqua fortis did before manifeſt any blackneſs. And divers alchymiſts, when they make ſolutions of minerals they would examine, are very glad, if they ſee a black powder precipitated to the bottom, taking it for a hopeful ſign, that thoſe particles are of a golden nature, which appear in a colour ſo ordinary to gold parted from other metals by aqua fortis, that it is a trouble to the refiner to reduce the precipitated calx to its native colour. For though (as we have tried) that may be quickly enough done by fire, which will make this gold look very glorioufly (as indeed it is at leaſt one of the beſt ways, that is practiſed for the refining of gold) yet it requires both watchfulneſs and ſkill, to give it ſuch a degree of fire, as will ſerve to reſtore it to its luſtre, without giving it ſuch a one, as may bring it to fuſion, to which the minuteneſs of the corpuſcles it conſiſts of makes the powder very apt. And this brings into my mind, that having taken a flat and bright piece of gold, that was refined by a curious and ſkilful perſon on purpoſe to try to what height of purity gold could be brought by art, I found that this very piece, as glorious as it looked, being rubbed a little upon

a piece of fine clean linen, did fully it with a kind of black: and the like I have observed in refined silver, which I therefore mention, because I formerly suspected, that the impurity of the metal might have been the only cause of what I have divers times observed in wearing silver-hilted swords, namely, that where they rubbed upon my clothes, if they were of a light-coloured cloth, the affriction would quickly black them; and congruously hereunto I have found pens blacked almost all over, when I had a while carried them about me in a silver ink-case. To which I shall only add, that whereas in these several instances of denigration, the metals are worn off, or otherwise reduced into very minute parts, that circumstance may prove not unworthy your notice.

EXPERIMENT X.

THAT a solution of silver does dye hair of a black colour, is a known experiment, which some persons, more curious than dexterous, have so unluckily made upon themselves, as to make their friends very merry. And I remember, that the other day I made myself some sport by an improvement of this observation; for having dissolved some pure silver in aqua fortis, and evaporated the *menstruum ad siccitatem*, as they speak, I caused a quantity of fair water to be poured upon the calx two or three several times, and to be at each evaporated, till the calx was very dry, and all the greenish bluenes, that is wont to appear in common crystals of silver, was quite carried away. Then I made those I meant to deceive, moisten some part of their skin with their own spittle, and slightly rub the moistened parts with a little of this prepared silver; whereupon they admired to see, that a snow-white body laid upon the white skin should presently produce a deep blackness, as if the stains had been made with ink; especially considering, that this blackness could not, like that produced by ordinary ink, be readily washed off, but required many hours, and part of it some days to its obliteration. And with the same white calx and a little fair water we likewise stained the white hafts of knives, with a lasting black in those parts, where the calx was plentifully enough laid on; for where it was laid on but very thinly, the stain was not quite of so deep a colour.

EXPERIMENT XI.

THE cause of the blackness of those many nations, which by one common name we are wont to call Negroes, has been long since disputed of by learned men, who possibly had not done amiss, if they had also taken into consideration, why some whole races of other animals besides men, as foxes and hares, are distinguished by a blackness not familiar to the generality of animals of the same species. The general opinion (to be mentioned a little lower) has been rejected even by some of the antient geographers, and among the moderns *Ortelius* and divers other learned men have questioned it. But this is no place to mention what thoughts I have had to and fro about these matters: only as I shall freely acknowledge, that to me the inquiry seems more abstruse than it does to many others, and that because consulting with authors, and with books of voyages, and with travellers, to satisfy myself in matters of fact, I have met with some things among them, which seem not to agree very well with the notions of the most classic authors concerning these things; for it being my present work to deliver rather matters historical than theories, I shall annex some few of my collections, instead of a solemn disputation. It is commonly presumed, that the heat of

of the climates, wherein they live, is the reason, why so many inhabitants of the scorching regions of *Africa* are black; and there is this familiar observation to countenance this conjecture, that we plainly see that mowers, reapers, and other country-people, who spend the most part of the hot summer days exposed to the sun, have the skin of their hands and faces, which are the parts immediately exposed to the sun and air, made of a darker colour than before, and consequently tending to blackness: and contrariwise we observe, that the Danes and some other people that inhabit cold climates, and even the English who feel not so rigorous a cold, have usually whiter faces than the Spaniards, Portugals and other European inhabitants of hotter climates. But this argument I take to be far more specious than convincing; for though the heat of the sun may darken the colour of the skin by that operation, which we in English call sun-burning; yet experience doth not evince, that I remember, that that heat alone can produce a discolouring, that shall amount to a true blackness, like that of Negroes; and we shall see by and by, that even the children of the Negroes not yet ten days old (perhaps not so much by three quarters of that time) will notwithstanding their infancy be of the same hue with their parents. Besides, there is a strong argument to be alleged against the vulgar opinion, that in divers places in *Asia* under the same parallel, or even of the same degree of latitude with the African regions inhabited by the Blacks, the people are at most but tawny; and in *Africa* itself divers nations in the empire of *Ethiopia* are not Negroes, though situated in the torrid zone, and as near the equinoctial, as other nations that are so (as the black inhabitants of *Zeylan* and *Malabar* are not in our globes placed so near the line as *Amara* the famousest place in *Ethiopia*). Moreover (that which is of no small moment in our present disquisition) I find not by the best navigators and travellers to the *West-Indies*, whose books or themselves I have consulted on this subject, that excepting perhaps one place or two of small extent, there are any Blacks originally natives of any part of *America* (for the Blacks now there have been by the Europeans long transplanted thither) though the new world contain in it so great a variety of climates, and particularly reaches quite cross the torrid zone from one tropic to another. And though it be true, that the Danes be a whiter people than the Spaniards, yet that may proceed rather from other causes (not here to be enquired into) than from the coldness of the climate, since not only the Swedes and other inhabitants of those cold countries, are not usually so white as the Danes, nor whiter than other nations in proportion to their vicinity to the pole. [And since the writing of the former part of this essay, having an opportunity on a solemn occasion to take notice of the numerous train of some extraordinary ambassadors sent from the Russian emperor to a great monarch, I observed, that (though it were then winter) the colour of their hair and skin was far less whitish than the Danes who inhabit a milder region is wont to be, but rather from the most part of a darkish brown; and the physician to the ambassador, with whom those Russes came, being asked by me, whether in *Muscovy* itself the generality of the people were more inclined to have dark-coloured hair than flaxen, he answered affirmatively; but seemed to suspect, that the true and antient Russians, a sept of whom he told me he had met with in one of the provinces of that vast empire, were rather white like the Danes than any thing near so brown as the present Muscovites, whom he guesses to be descended of the Tartars, and to have inherited their colour from them.] But to prosecute our former discourse, I shall add for further proof the conjecture I was countenancing, that good authors inform us, that there are Negroes in *Africa* not far from the *Cape of Good Hope*, and consequently beyond the southern tropic, and without the torrid zone: much about the same northern

Olearius Voyage de Moscov & de Perse liv. 3.

thern latitude (or very little more) wherein there are divers American nations, that are not Negroes, and wherein the inhabitants of *Candia*, some parts of *Sicily*, and even of *Spain*, are not so much as Tawny-moors. But (which is a fresh and strong argument against the common opinion) I find by our recent relations of *Greenland* (our accounts whereof we owe to the curiosity of that royal Virtuoso the present King of *Denmark*) that the inhabitants are olive-coloured, or rather of a darker hue. But if the case were the same with men, and those other kinds of animals I formerly named, I should offer something as a considerable proof, that cold may do much towards the making men white or black; and however I shall set down the observation as I have met with it, as worthy to come into the history of whiteness and blackness; and it is, that in some parts of *Russia* and of *Livonia* it is affirmed by *Olaus Magnus* and others, that hares and foxes (some add partridges) which before were black, or red, or grey, do in the depth of winter become white by reason of the great cold; (for that it should be, as some conceive, by looking upon the snow, seems improbable upon divers accounts): and I remember, that having purposely inquired of a Virtuoso, who lately travelled through *Livonia* to *Mosco*, concerning the truth of this tradition, he both told me, he believed it, and added, that he saw divers of those lately named animals either in *Russia* or *Livonia* (for I do not very well remember whether of the two) which, though white, when he saw them in winter, they assured him had been black, or other colours, before the winter began, and would be so again when it was over. But for further satisfaction, I also consulted one, that had for some years been an eminent physician in *Russia*, who though he rejected some other traditions, that are generally enough believed concerning that country, told me nevertheless, that he saw no cause to doubt of this tradition of *Olaus Magnus*, as to foxes and hares; not only because it is a common and uncontrouled assertion of the natives; but also because he himself in the winter could never, that he remembered, see foxes and hares of any other colour than white. And I myself having seen a small white fox, brought out of *Russia* into *England*, towards the latter end of winter, foretold those, that shewed him me, that he would change colour in summer; and accordingly coming to look upon him again in *July*, I found, that the back and sides, together with the upper part of the head and tail, were already grown of a dark colour, the lower part of the head and belly containing as yet a whiteness. Let me add, that were it not for some scruple I have, I should think more than what *Olaus* relates confirmed by the judicious *Olearius*, who was twice employed into those parts as a public minister, who in his account of *Muscovy* has this passage: *The hares there are grey; but in some provinces they grow white in the winter.* And within some few lines after; *It is not very difficult to find the cause of this change, which certainly proceeds only from the outward cold, since I know, that even in summer hares will change colour, if they be kept a competent time in a cellar.* I say, were it not for some scruple, because I take notice, that in the same page the author affirms, that the like change of colour, that happens to hares in some provinces of *Muscovy*, happens to them also in *Livonia*; and yet immediately subjoins, that in *Courland* the hares vary not their colour in winter; though these two last named countries be contiguous, that is, severed only by the river of *Dugna*. For it is scarce conceivable how cold alone should have, in countries so near, so strangely differing an operation, though no less strange a thing is confessed by many, that ascribe the complexion of Negroes to the heat of the sun, when they would have the river of *Senega* so to bound the *Moors*, that though on the north-side they are but tawny, on the other side they are black.

THERE

THERE is another opinion concerning the complexion of Negroes, that is not only embraced by many of the more vulgar writers, but likewise by that ingenious traveller Mr. *Sandys*, and by a late most learned critic, besides other men of note; and these would have the blackness of Negroes an effect of *Noah*'s curse ratified by God's, upon *Cham*. But though I think, that even a Naturalist may without disparagement believe all the miracles attested by the Holy Scriptures, yet in this case, to fly to a supernatural cause, will, I fear, look like shifting off the difficulty, instead of resolving it; for we inquire not the first and universal, but the proper, immediate, and physical cause of the jetty colour of Negroes; and not only we do not find expressed in the scripture, that the curse meant by *Noah* to *Cham* was the blackness of his posterity, but we do find plainly enough there, that the curse was quite another thing, namely, that he should be a servant of servants, that is, by an Hebraism, a very abject servant to his brethren: which accordingly did in part come to pass, when the Israelites of the posterity of *Sem* subdued the Canaanites, that descended from *Cham*, and kept them in great subjection. Nor is it evident, that blackness is a curse; for navigators tell us of black nations who think so much otherwise of their own condition, that they paint the devil white. Nor is blackness inconsistent with beauty, which even to our European eyes consists not so much in colour, as an advantageous stature, a comely symmetry of the parts of the body, and good features in the face. So that I see not, why blackness should be thought such a curse to the Negroes, unless perhaps it be, that being wont to go naked in those hot climates, the colour of their skin does probably, according to the doctrine above delivered, make the sun-beams more scorching to them, than they would prove to a people of a white complexion.

GREATER probability there is, that the principal cause (for I would not exclude all concurrent ones) of the blackness of Negroes is some peculiar and seminal impression: for not only we see, that Blackmoor boys, brought over into these colder climates, lose not their colour; but good authors inform us, that the offspring of Negroes transplanted out of *Africa*, above a hundred years ago, retain still the complexion of their progenitors, though possibly in tract of time it will decay; as, on the other side, the white people removing into very hot climates, have their skins by the heat of the sun scorched into dark colours; yet neither they, nor their children have been observed, even in the countries of negroes, to descend to a colour amounting to that of the natives. Whereas I remember I have read in *Piso*'s excellent account of *Brasil*, that betwixt the Americans and Negroes are generated a distinct sort of men, which they call *Cabocles*; and betwixt Portugals and Æthiopian women, he tells us, he has sometimes seen twins, whereof one had a white skin, the other a black: not to mention here some other instances he gives, that the productions of the mixtures of differing people, that is (indeed) the effects of seminal impressions, which they consequently argue to have been their causes. And we shall not much scruple at this, if we consider, that even organical parts may receive great differences from such peculiar impressions, upon what account soever they came to be settled in the first individual persons, from whom they are propagated to posterity, as we see in the blobber-lips and flat-noses of most nations of Negroes. And if we may credit what learned men deliver concerning the little feet of the Chinese, the *Macrocephali* taken notice of by *Hippocrates* will not be the only instance we might apply to our present purpose. And on this occasion it will not perchance be impertinent to add something of what I have observed in other animals, as there are a sort of hens, that want rumps; and that (not to mention, that in several places there is a sort of crows or daws, that are not coal black as ours, but partly of a whitish colour) in spight of *Porphyry*'s examples

Piso *Nat. & Med. Hist.* Brasil. lib. 1. in *fine*.

of inſeparable accidents, I have ſeen a perfectly white raven, as to bill as well as feathers, which I attentively conſidered, for fear of being impoſed upon. And this recalls into my memory, what a very ingenious phyſician has divers times related to me of a young lady, to whom being called, he found, that though ſhe much complained of want of health, yet there appeared ſo little cauſe either in her body, or her condition, to gueſs, that ſhe did any more than fancy herſelf ſick; that ſcrupling to give her phyſic, he perſuaded her friends rather to divert her mind by little journeys of pleaſure: in one of which going to viſit St. *Winifred*'s well, this lady, who was a catholic, and devout in her religion, and a pretty while in the water to perform ſome devotions, and had occaſion to fix her eyes very attentively upon the red pebble-ſtones, which in a ſcattered order made up a good part of thoſe that appeared through the water; and a while after growing big, ſhe was delivered of a child, whoſe white ſkin was copiouſly ſpeckled with ſpots of the colour and bigneſs of thoſe ſtones; and though now this child have already lived ſeveral years, yet ſhe ſtill retains them. I have but two things to add concerning the blackneſs of Negroes; the one is, that the ſeat of that colour ſeems to be but the thin epidermis, or outward ſkin; for I knew a young Negro, who having been lightly ſick of the ſmall-pox or meaſles (for it was doubted, which of the two was his diſeaſe) I found by inquiry of a perſon, that was concerned for him, that in thoſe places the little tumours had broke their paſſage through the ſkin, when they were gone, they left whitiſh ſpecks behind them; and the lately commended *Piſo* aſſures us, that having the opportunity in *Braſil* to diſſect many Negroes, he clearly found, that their blackneſs went no deeper than the very outward ſkin, which cuticula or epidermis being removed, the undermoſt ſkin or cutis appeared juſt as white as that of European bodies. And the like has been affirmed to me by a phyſician of our own, whom hearing he had diſſected a Negroe here in *England*, I conſulted about this particular. The other thing to be here taken notice of concerning Negroes is, that having inquired of an intelligent acquaintance of mine (who keeps in the *Indies* about three hundred of them, as well women as men, to work in his plantations) whether their children come black into the world; he anſwered, that they did not, but were brought forth of almoſt the like reddiſh colour with our European children: and having further enquired, how long it was before theſe infants appeared black, he replied, that it was not wont to be many days. And agreeable to this account I find that given us in a freſhly publiſhed French book, written by a Jeſuit, that had good opportunity of knowing the truth of what he delivers; for being one of the miſſionaries of his order into the ſouthern *America* upon the laudable deſign of converting infidels to Chriſtianity, he baptized ſeveral infants, which when newly born were much of the ſame colour with European babes, but within about a week began to appear of the hue of their parents. But more pregnant is the teſtimony of our countryman *Andrew Battel*, who being ſent priſoner by the Portugals to *Angola*, lived there, and in the adjoining regions, partly as a priſoner, partly as a pilot, and partly as a ſoldier, near eighteen years; and he mentioning the *African* kingdom of *Longo*, peopled with Blacks, has this paſſage: *The children in this country are born white, and change their colour in two days to a perfect black.* As for example, *The* Portugals, *which dwell in the kingdom of* Longo, *have ſometimes children by the Negroe-women; and many times the fathers are deceived, thinking, when the child is born, that it is theirs, and within two days it proves the ſon or daughter of a Negroe, which the Portugals greatly grieve at.* And the ſame perſon has elſewhere a relation, which, if he have made no uſe at all of the liberty of a traveller, is very well worth our notice; ſince this, together with that we have formerly mentioned

Purchas Pilgrim, *ſecond part, ſeventh Book, Chap.* 3. *Sect.* 5.

tioned of ſeminal impreſſions, ſhews a poſſibility, that a race of Negroes might be begun, though none of the ſons of *Adam* for many precedent generations were of that complexion. For I ſee not, why it ſhould not be at leaſt as poſſible, that white parents may ſometimes have black children, as that African Negroes ſhould ſometimes have laſtingly white ones; eſpecially ſince concurrent cauſes may eaſily more befriend the productions of the former kind, than under the ſcorching heat of *Africa* thoſe of the latter. And I remember on the occaſion of what he delivers, that of the white raven formerly mentioned, the poſſeſſor affirmed to me, that in the neſt, out of which he was taken white, they found with him but one other young one, and that he was of as jetty a black as any common raven. But let us hear our author himſelf: *Here are* (ſays he, ſpeaking of the formerly mentioned regions) *born in this country white children, which is very rare among them, for their parents are Negroes; and when any of them are born, they are preſented to the king, and are called* Dondos; *theſe are as white as any white men. Theſe are the king's witches, and are brought up in witchcraft, and always wait on the king: there is no man, that dares meddle with theſe* Dondos; *if they go to the market, they may take what they liſt, for all men ſtand in awe of them. The king of* Longo *hath four of them.* And yet this country in our globes is placed almoſt in the midſt of the torrid zone (four or five degrees ſouthward of the line.) And our author elſewhere tells us of the inhabitants, that they are ſo fond of their blackneſs, that they will not ſuffer any, that is not of that colour (as the Portugals that come to trade thither) to be ſo much as buried in their land; of which he annexes a particular example, that may be ſeen in his voyage preſerved by our induſtrious countryman Mr. *Purchas*. But it is high time for me to diſmiſs obſervations, and go on with experiments.

Purchas. ibid.

Purchas ibid. in fin.

EXPERIMENT XII.

THE way, *Pyrophilus*, of producing whiteneſs by chymical precipitations is very well worth our obſerving; for thereby bodies, of very differing colours as well as natures, though diſſolved in ſeveral liquors, are all brought into calces or powders that are white. Thus we find, that not only crabs-eyes, that are of themſelves white, and pearls that are almoſt ſo, but coral and minium that are red, being diſſolved in ſpirit of vinegar, may be uniformly precipitated by oil of tartar into white powders. Thus ſilver and tin ſeparately diſſolved in aqua fortis will the one precipitate itſelf, and the other be precipitated by common ſalt-water into a white calx, and ſo will crude lead and quickſilver firſt diſſolved likewiſe in aqua fortis. The like calx will be afforded, as I have tried, by a ſolution of that ſhining mineral tin-glaſs diſſolved in aqua fortis, and precipitated out of it; and divers of theſe calces may be made at leaſt as fair and white, if not better coloured, if inſtead of oil of tartar they were precipitated with oil of vitriol, or with another liquor I could name. Nay, that black mineral antimony itſelf, being reduced by and with the ſalts, that concur to the compoſition of common ſublimate, into that clear though unctuous liquor, that chymiſts commonly call rectified butter of antimony, will, by the bare affuſion of ſtore of fair water, be ſtruck down into that ſnow-white powder, which when the adhering ſaltneſs is well waſhed off, chymiſts are pleaſed to call *Mercurius Vitæ*; though the like powder may be made of antimony, without the addition of any mercury at all. And this lacteſcence, if I may ſo call it, does alſo commonly enſue, when ſpirit of wine, being impregnated with thoſe parts of gums or other vegetable concretions, that are ſuppoſed to abound with ſulphureous corpuſcles, fair water

water is suddenly poured upon the tincture or solution. And I remember, that very lately I did, for trial sake, on a tincture of Benjamin drawn with spirit of wine, and brought to be as red as blood, pour some fair water; which presently mingling with the liquor, immediately turned the whole mixture white. But if such seeming milks be suffered to stand unstirred for a convenient while, they are wont to let fall to the bottom a resinous substance, which the spirit of wine diluted and weakened by the water poured into it is unable to support any longer. And something of kin to this change of colour in vegetables is that, which chymists are wont to observe upon the pouring of acid spirits upon the red solution of sulphur, dissolved in an infusion of pot-ashes, or in some other sharp lixivium; the precipitated sulphur, before it subsides, immediately turning the red liquor into a white one. And other examples might be added of this way of producing whiteness in bodies by precipitating them out of the liquors, wherein they have been dissolved. But I think it may be more useful to admonish you, *Pyrophilus*, that this observation admits of restrictions, and is not so universal, as by this time perhaps you have begun to think it: for though most precipitated bodies are white, yet I know some that are not; for gold dissolved in aqua regis, whether you precipitate it with oil of tartar, or with spirit of sal armoniac, will not afford a white, but a yellow calx. Mercury also, though reduced into sublimate, and precipitated with liquors abounding with volatile salts, as the spirits drawn from urine, hartshorn, and other animal substances, yet will afford, as we noted in our first experiment about whiteness and blackness, a white precipitate; yet with the solution of pot-ashes and other lixiviate salts, it will let fall an orange-tawny powder. And so will crude antimony, it being dissolved in a strong lye, if you pour (as far as I remember) any acid liquor upon the solution newly filtrated, whilst it is yet warm. And if upon the filtrated solution of vitriol, you pour a solution of one of these fixed salts, there will subside a copious substance, very far from having any whiteness, which the chymists are pleased to call (how properly I have elsewhere examined) the sulphur of vitriol. So that most dissolved bodies being by precipitation brought to white powders, and yet some affording precipitates of other colours, the reason of both the phænomena may deserve to be enquired into.

EXPERIMENT XIII.

See Scaliger *Exercit.* 305. Sect. 9.

SOME learned modern writers are of opinion, that the account, upon which whiteness and blackness ought to be called, as they commonly are, the two extreme colours, is, that blackness (by which I presume is meant the bodies endowed with it) receives no other colours; but whiteness very easily receives them all: whence some of them compare whiteness to the *Aristotelian Materia prima*, that being capable of any sort of forms, as they suppose white bodies to be of every kind of colour. But not to dispute about names or expressions, the thing itself that is affirmed as matter of fact, seems to be true enough in most cases, not in all, or so as to hold universally. For though it be a common observation among dyers, that cloaths, which, have once been thoroughly imbued with black, cannot so well afterwards be dyed into lighter colours, the pre-existent dark colour infecting the ingredients, that carry the lighter colour to be introduced, and making it degenerate into some more sad one; yet the experiments lately mentioned may shew us, that where the change of colour in black bodies is attempted, not by mingling bodies of lighter colours with them, but by addition of such things as are proper to alter the texture of those corpuscles that contain the black colour, it is no such difficult matter, as the lately mentioned learned men

men imagine, to alter the colour of black bodies. For we saw, that inks of several kinds might in a trice be deprived of all their blackness; and those made with log-wood and red-roses might also be changed, the one into a red, the other into a reddish liquor; and with oil of vitriol I have sometimes turned black pieces of silk in o a kind of yellow; and though the taffaty were thereby made rotten, yet the spoiling of that does no way prejudice the experiment, the change of black silk into yellow being nevertheless true, because the yellow silk is the less good. And as for whiteness I think the general affirmation of its being so easily destroyed, or transmuted by any other colour, ought not to be received without some cautions and restrictions. For whereas, according to what I formerly noted, lead is by calcination turned into that red powder we call minium, and tin by calcination reduced to a white calx; the common putty, that is sold and used so much in shops, instead of being, as it is pretended and ought to be, only the calx of tin, is, by the artificers that make it, to save the charge of tin, made (as some of themselves have confessed, and as I long suspected by the cheap rate it may be bought for) but of half tin and half lead, if not far more lead than tin; and yet the putty, in spite of so much lead, is a very white powder, without disclosing any mixture of minium. And so if you take two parts of copper, which is a high coloured metal, to but one of tin, you may by fusion bring them into one mass, wherein the whiteness of the tin is much more conspicuous and predominant than the reddishness of the copper. And on this occasion it may not be impertinent to mention an experiment, which I relate upon the credit of a very honest man, whom I purposely inquired of about it, being myself not very fond of making trials with arsenic: the experiment is this; that if you colliquate arsenic and copper in a due proportion, the arsenic will blanch the copper both within and without, which is an experiment well enough known. But when I inquired, whether or no this white mixture being skilfully kept a while upon the cupel would not let go its arsenic, which made whiteness its predominant colour, and return to the reddishness of copper, I was assured of the affirmative. So that among mineral bodies, some of those that are white, may be far more capable, than those I am reasoning with seem to have known, of eclipsing others, and of making their colour predominant in mixtures. In further confirmation of which may be added, that I remember, that I also took a lump of silver and gold melted together, wherein, by the estimate of a very experienced refiner, there might be about a fourth or third part of gold; and yet the yellow colour of the gold was so hid by the white of the silver, that the whole mass appeared to be but silver; and when it was rubbed upon the touch-stone, an ordinary beholder could scarce have distinguished it from the touch of common silver; though if I put a little aqua fortis upon any part of the white surface it had given the touch-stone, the silver in the moistened part being immediately taken up and concealed by the liquor, the golden particles would presently disclose that native yellow, and look rather as if gold, than if the above-mentioned mixture had been rubbed upon the stone.

EXPERIMENT XIV.

I TOOK a piece of black horn (polished as being part of a comb) this with a piece of broken glass I scraped into many thin and curled flakes, some shorter and some longer; and having laid a pretty quantity of these scrapings together, I found, as I looked for, that the heap they composed was white; and though, if I laid it upon a clean piece of white-paper, its colour seemed somewhat eclipsed by the greater

whiteneſs of the body it was compared with, looking ſomewhat like linen, that had been ſullied by a little wearing; yet if I laid it upon a very black body, as upon a beaver hat, it then appeared to be of a good white. Which experiment, that you may in a trice make when you pleaſe, ſeems very much to disfavour both their doctrine, that would have colours to flow from the ſubſtantial forms of bodies; and that of the chymiſts alſo, who aſcribe them to one or other of their three hypoſtatical principles: for though in our caſe there was ſo great a change made, that the ſame body, without being ſubſtantially either increaſed or leſſened, paſſes immediately from one extreme colour to another (and that too from black to white) yet this ſo great and ſudden change is effected by a ſlight mechanical tranſpoſition of parts, there being no ſalt or ſulphur or mercury, that can be pretended to be added or taken away, nor yet any ſubſtantial form, that can reaſonably be ſuppoſed to be generated and deſtroyed, the effect proceeding only from a local motion of the parts, which ſo varied their poſition, as to multiply their diſtinct ſurfaces, and to qualify them to reflect far more light to the eye, than they could before they were ſcraped off from the entire piece of black horn.

EXPERIMENT XV.

AND now, *Pyrophilus*, it will not be improper for us to take ſome notice of an opinion touching the cauſe of blackneſs, which I judged it not ſo ſeaſonable to queſtion, till I had ſet down ſome of the experiments, that might juſtify my diſſent from it. You know, that of late divers learned men, having adopted the three hypoſtatical principles, beſides other notions of the chymiſts, are very inclinable to reduce all qualities of bodies to one or other of thoſe three principles; and particularly aſſign for the cauſe of blackneſs the ſooty ſteam of aduſt or torrified ſulphur. But I hope, that what we have delivered above to countenance the opinion we have propoſed about the cauſe of blackneſs, will ſo eaſily ſupply you with ſeveral particulars, that may be made uſe of againſt this opinion, that I ſhall now repreſent to you but two things concerning it.

AND firſt, it ſeems, that the favourers of the chymical theories might have pitched upon ſome more proper term, to expreſs the efficient of blackneſs than ſulphur aduſt; for we know, that common ſulphur, not only when melted, but even when ſublimed. does not grow black by ſuffering the action of the fire, but continues and aſcends yellow, and rather more than leſs white, than it was before its being expoſed to the fire. And if it be ſet on fire, as when we make that acid liquor, that chymiſts call *Oleum Sulphuris per campanam*, it affords very little ſoot: and indeed the flame yields ſo little, that it will ſcarce in any degree black a ſheet of white paper, held a pretty while over the flame and ſmoke of it, which is obſerved rather to whiten than infect linen, and which does plainly make red roſes grow very pale, but not at all black, as far as the ſmoke is permitted to reach the leaves. And I can ſhew you a ſort of fixt ſulphur made by an induſtrious laborant of your acquaintance, who aſſured me, that he was wont to keep it for divers weeks together night and day in a naked and violent fire, almoſt like that of the glaſs-houſe; and when, to ſatisfy my curioſity, I made him take out a lump of it, though it were glowing hot (and yet not melted) it did not, when I had ſuffered it to cool, appear black, the true colour of it being a true red. I know it may be ſaid, that chymiſts in the opinion above recited mean the principle of ſulphur, and not common ſulphur, which receives its name, not from its being all perfectly of a ſulphureous nature, but for that plenty and predominancy of the ſulphureous

ſulphureous principle in it. But allowing this, it is eaſy to reply, that according to this very reaſon, torrified ſulphur ſhould afford more blackneſs than moſt other concretes, wherein that principle is confeſſed to be far leſs copious. Alſo when I have expoſed camphire to the fire in cloſe veſſels, as inflammable, and conſequently (according to the chymiſts) as ſulphureous a body as it is, I could not by ſuch a degree of heat as brought it to fuſion, and made it to boil in the glaſs, impreſs any thing of blackneſs, or of any other colour, than its own pure white, upon this vegetable concrete. But what ſhall we ſay to ſpirit of wine, which being made by a chymical analyſis of the liquor that affords it, and being totally inflammable, ſeems to have a full right to the title they give it of *Sulphur Vegetabile?* and yet this fluid ſulphur not only contracts not any degree of blackneſs by being often ſo heated, as to be made to boil, but when it burns away with an actual flame, I have not found, that it would diſcolour a piece of white paper held over it, with any diſcernable ſoot. Tin alſo, that wants not, according to the chymiſts, a *Sulphur Joviale*, when thoroughly burned by the fire into a calx, is not black, but eminently white. And I lately noted to you out of *Bellonius*, that the charcoals of oxy-cedar are not of the former of theſe two colours, but of the latter. And the ſmoke of our *Tinby* coals here in *England* has been uſually obſerved rather to blanch linen than to black it. To all which other particulars of the like nature might be added; but I rather chuſe to put you in mind of the third experiment, about making black liquors, or ink, of bodies, that were none of them black before. For how can it be ſaid, that when thoſe liquors are put together actually cold, and continue ſo after their mixture, there intervenes any new aduſtion of ſulphur to produce the emergent blackneſs? (and the ſame queſtion will be applicable to the blackneſs produced upon the blade of a knife, that has cut lemons and ſome kind of ſour apples, if the juice, though both actually and potentially cold, be not quickly wiped off.) And when by the inſtilling either of a few drops of oil of vitriol, as in the ſecond experiment, or of a little of the liquor mentioned in the paſſage pointed at in the fourth experiment (where I teach at once to deſtroy one black ink, and make another) the blackneſs produced by thoſe experiments is preſently deſtroyed; if the colour proceeded only from the plenty of ſulphureous parts, torrified in the black bodies, I demand what becomes of them, when the colour ſo ſuddenly diſappears? For it cannot reaſonably be ſaid, that all thoſe, that ſufficed to make ſo great a quantity of black matter, ſhould reſort to ſo very ſmall a proportion of the clarifying liquor (if I may ſo call it) as to be diluted by it, without at all denigrating it. And if it be ſaid, that the inſtilled liquor diſperſed thoſe black corpuſcles, I demand, how that diſperſion comes to deſtroy their blackneſs, but by making ſuch a local motion of their parts, as deſtroys their former texture? Which may be a matter of ſuch moment in caſes like ours, that I remember, that I have in few hours, without addition, from ſoot itſelf, attained pretty ſtore of cryſtalline ſalt, and good ſtore of tranſparent liquor; and (which I have on another occaſion noted as remarkable) this ſo black ſubſtance had its colour ſo altered, by the change of texture it received from the fire, wherewith it was diſtilled, that it did for a great while afford ſuch plenty of very white exhalations, that the receiver, though large, ſeemed to be almoſt filled with milk.

Secondly, But were it granted, as it is in ſome caſes not improbable, that divers bodies may receive a blackneſs from a ſooty exhalation, occaſioned by the aduſtion of their ſulphur, which (for the reaſons lately mentioned) I ſhould rather call their oily parts; yet ſtill this account is applicable but to ſome particular bodies, and will afford us no general theory of blackneſs. For if, for example, white hartſhorn being,

in veſſels well luted to each other, expoſed to the fire, be ſaid to turn black by the infection of its own ſmoke, I think I may juſtly demand, what it is that makes the ſmoke or ſoot itſelf black, ſince no ſuch colour, but its contrary, appeared before in the hartſhorn? And with the ſame reaſon, when we are told, that torrified ſulphur makes bodies black, I deſire to be told alſo, why torrefaction makes ſulphur itſelf black? Nor will there be any ſatisfactory reaſon aſſigned of theſe queries, without taking in thoſe fertile as well as intelligible mechanical principles of the poſition and texture of the minute parts of the body in reference to the light and the eye; and theſe applicable principles may ſerve the turn in many caſes, where the aduſtion of ſulphur cannot be pretended; as in the appearing blackneſs of an open window looked upon at a ſomewhat remote diſtance from the houſe; as alſo in the blackneſs men think they ſee in the holes, that happen to be in white linen, or paper of the like colour; and in the increaſing blackneſs immediately produced barely by ſo rubbing velvet, whoſe piles were inclined before, as to reduce them to a more erect poſture; in which and in many other caſes formerly alleged, there appears nothing requiſite to the production of the blackneſs, but the hindering of the incident beams of light from rebounding plentifully enough to the eye. To be ſhort, thoſe I reaſon with, do concerning blackneſs what the chymiſts are wont alſo to do concerning other qualities; namely, to content themſelves to tell us, in what ingredient of a mixt body, the quality enquired after does reſide, inſtead of explicating the nature of it, which (to borrow a compariſon from their own laboratories) is much as if in an inquiry after the cauſe of ſalivation, they ſhould think it enough to tell us, that the ſeveral kinds of precipitates of gold and mercury, as likewiſe of quickſilver and ſilver (for I know the make and uſe of ſuch precipitates alſo) do ſalivate upon the account of the mercury, which though diſguiſed abounds in them; whereas the difficulty is as much to know upon what account mercury itſelf, rather than other bodies, has that power of working by ſalivation. Which I ſay not, as though it were not ſomething (and too often the moſt we can arrive at) to diſcover in which of the ingredients of a compounded body the quality, whoſe nature is ſought, reſides; but becauſe, though this diſcovery itſelf may paſs for ſomething, and is oftentimes more than what is taught us about the ſame ſubjects in the ſchools, yet we ought not to think it enough, when more clear and particular accounts are to be had.

The Experimental Hiſtory of COLOURS begun.

PART III.

Containing promiſcuous Experiments about COLOURS.

EXPERIMENT I.

BECAUSE that, according to the conjectures I have above propoſed, one of the moſt general cauſes of the diverſity of colours in opacous bodies, is, that ſome reflect the light mingled with more, others with leſs of ſhade (either as to quantity, or as

as to interruption ;) I hold it not unfit to mention, in the first place, the experiments, that I thought upon to examine this conjecture. And though coming to transcribe them out of some physiological *Adversaria* I had written in loose papers, I cannot find one of the chief records I had of my trials of this nature, yet the papers, that escaped miscarrying, will, I presume, suffice to manifest the main thing, for which I now allege them. I find then among my *Adversaria* the following narrative.

October the 11th, About ten in the morning in sun-shiny weather (but not without fleeting clouds) we took several sorts of paper stained, some of one colour, and some of another; and in a darkened room, whose window looked southward, we cast the beams, that came in at a hole about three inches and a half in diameter, upon a white wall, that was placed on one side, about five foot distance from them.

The white gave much the brightest reflection.

The green, red, and blue being compared together, the red gave much the strongest reflection, and manifestly enough also threw its colour upon the wall: the green and blue were scarce discernable by their colours, and seemed to reflect an almost equal light.

The yellow, compared with the two last named, reflected somewhat more light.

The red and purple being compared together, the former manifestly reflected a good deal more light.

The blue and purple compared together, the former seemed to reflect a little more light, though the purple colour were more manifestly seen.

A sheet of very well sleeked marbled paper being applied as the others, did not cast any of its distinct colours upon the wall, nor throw its light upon it with an equal diffusion; but threw the beams unstained and bright to this and that part of the wall, as if its polish had given it the nature of a specular body. But comparing it with a sheet of white paper, we found the reflection of the latter to be much stronger, it diffusing almost as much light to a good extent as the marble paper did to one part of the wall.

The green and purple left us somewhat in suspense, which reflected the most light; only the purple seemed to have some little advantage over the green, which was dark in its kind.

Thus much I find in our above-mentioned collections; among which there are also some notes concerning the production of compounded colours, by reflection from bodies differingly coloured. And these notes we intended should supply us with what we should mention as our second experiment: but having lost the paper, that contained the particulars, and remembering only in general, that if the objects, which reflected the light, were not strongly coloured and somewhat glossy, the reflected beams would not manifestly make a compounded colour upon the wall, and even then but very faintly; we shall now say no more of that matter, only reserving ourselves to mention hereafter the composition of a green, which we still retain in memory.

EXPERIMENT II.

We may add, *Pyrophilus*, on this occasion, that though a darkened room be generally thought requisite to make the colour of a body appear by reflection from another body, that is not one of those, that are commonly agreed upon to be specular (as polished metal, quicksilver, glass, water, *&c.*) yet I have often observed, that when I wore doublets lined with some silken stuff, that was very glossy and vividly coloured, especially red, I could in an enlightened room plainly enough discern

cern the colour upon the pure white linen, that came out at my ſleeve, and reached to my cuffs; as if that fine white body were more ſpecular than coloured and unpoliſhed bodies are thought capable of being.

EXPERIMENT III.

WHILST we were making the newly mentioned experiments, we thought fit to try alſo what compoſition of colours might be made by altering the light in its paſſage to the eye, by the interpoſition not of perfectly diaphanous bodies (that having been already tried by others as well as by us, as we ſhall ſoon have occaſion to take notice) but of ſemi-opacous bodies, and thoſe ſuch as looked upon in an ordinary light, and not held betwixt it and the eye, are not wont to be diſcriminated from the reſt of opacous bodies. Of this trial our mentioned *Adverſaria* preſent us the following account:

HOLDING theſe ſheets, ſometimes one, ſometimes the other of them, before the hole betwixt the ſun and the eye, with the coloured ſides obverted to the ſun; we found them ſingle to be ſomewhat tranſparent, and appear of the ſame colour as before, only a little altered by the great light they were placed in: but laying two of them one over another, and applying them ſo to the hole, the colours were compounded as follows.

THE blue and yellow ſcarce exhibited any thing but a darker yellow, which we aſcribed to the coarſeneſs of the blue paper, and its darkneſs in its kind. For applying the blue parts of the marbled paper with the yellow paper after the ſame manner, they exhibited a good green.

THE yellow and red looked upon together gave us but a dark red, ſomewhat (and but a little) inclining to an orange colour.

THE purple and red looked on together appeared more ſcarlet.

THE purple and yellow made an orange.

THE green and red made a dark orange-tawny.

THE green and purple made the purple appear more dirty.

THE blue and purple made the purple more lovely, and far more deep.

THE red parts of the marbled paper, looked upon with the yellow, appeared of a red far more like ſcarlet than without it.

BUT the fineneſs or coarſeneſs of the papers, their being carefully or ſlightly coloured, and divers other circumſtances, may ſo vary the events of ſuch experiments as theſe, that if, *Pyrophilus*, you would build much on them, you muſt carefully repeat them.

EXPERIMENT IV.

THE triangular priſmatical glaſs being the inſtrument, upon whoſe effects we may the moſt commodiouſly ſpeculate the nature of emphatical colours (and perhaps that of others too;) we thought it might be uſeful to obſerve the ſeveral reflections and refractions, which the incident beams of light ſuffer in rebounding from it, and paſſing through it. And this we thought might be beſt done, not (as is uſual) in an ordinary enlightened room, where (by reaſon of the difficulty of doing otherwiſe) even the curious have left particulars unheeded, which may in a convenient place be eaſily taken notice of; but in a darkened room, where by placing the glaſs in a convenient poſture, the various reflections and refractions may be diſtinctly obſerved;

and where it may appear, what beams are untinged, and which they are, that, upon the bodies, that terminate them, do paint either the primary or ſecondary iris. In purſuance of this we did, in the above-mentioned darkened room, make obſervation of no leſs than four reflections, and three refractions, that were afforded us by the ſame priſm; and thought, that, notwithſtanding what was taught us by the rules of catoptricks and dioptricks, it would not be amiſs to find alſo, by hiding ſometimes one part of the priſm, and ſometimes another, and obſerving where the light or colour vaniſhed thereupon, by which reflection and by which refraction each of the ſeveral places whereon the light rebounding from, or paſſing through, the priſm, appeared either ſincere or tinctcd, was produced. But becauſe it would be tedious, and not ſo intelligible to deliver this in words, I have thought fit to refer you to the annexed ſcheme, where the newly mentioned particulars may be at one view taken notice of.

EXPERIMENT V.

I Know not whether you will think it inconſiderable to annex to this experiment, that we obſerved in a room not darkened, that the priſmatical iris (if I may ſo call it) might be reflected without loſing any of its ſeveral colours (for we now conſider not their order) not only from a plain looking-glaſs and from the calm ſurface of fair water, but alſo from a concave looking-glaſs; and that refraction did as little deſtroy thoſe colours as reflection. For by the help of a large (double convex) burning-glaſs, through which we refracted the ſun-beams, we found, that one part of the iris might be made to appear either beyond, or on this ſide of the other parts of the ſame iris; but yet the ſame vivid colours would appear in the diſplaced part (if I may ſo term it) as in the other. To which I ſhall add, that having, by hiding the ſide of the priſm, obverted to the ſun with an opacous body, wherein only one ſmall hole was left for the light to paſs though, reduced the priſmatical iris (caſt upon white paper) into a very narrow compaſs, and looked upon it through a microſcope; the colours appeared the ſame as to kind, that they did to the naked eye.

EXPERIMENT VI.

IT may afford matter of ſpeculation to the inquiſitive, ſuch as you, *Pyrophilus*, that as the colours of outward objects brought into a darkened room do ſo much depend for their viſibility upon the dimneſs of the light they are there held by, that the ordinary light of the day being freely let in upon them, they immediately diſappear; ſo our trials have informed us, that as to the priſmatical iris painted on the floor by the beams of the ſun trajected thorough a triangular glaſs, though the colours of it appear very vivid even at noon-day, and in ſun-ſhiny weather, yet by a more powerful light they may be made to diſappear. For having ſometimes, (in proſecution of ſome conjectures of mine not now to be inſiſted on) taken a large metalline concave ſpeculum, and with it caſt the converging beams of the ſun upon a priſmatical iris, which I had cauſed to be projected upon the floor, I found, that the over-powerful light made the colours of the iris diſappear. And if I ſo reflected the light, as that it croſſed but the middle of the iris, in that part only the colours vaniſhed or were made inviſible; thoſe parts of the iris, that were on the right and left hand of the reflected light (which ſeemed to divide them, and cut the iris aſunder) continuing to exhibit the ſame colours as before. But upon this we muſt not now ſtay to ſpeculate.

EXPERIMENT VII.

I HAVE ſometimes thought it worth while to take notice, whether or no the colours of opacous bodies might not appear to the eye ſomewhat diverſified, not only by the diſpoſition of the ſuperficial parts of the bodies themſelves, and by the poſition of the eye in reference to the object and the light, (for theſe things are notorious enough) but according alſo to the nature of the lucid body, that ſhines upon them. And I remember, that in proſecution of this curioſity, I obſerved a manifeſt difference in ſome kinds of coloured bodies looked on by day-light, and afterwards by the light of the moon, either directly falling on them, or reflected upon them from a concave looking-glaſs. But not finding at preſent, in my collections about colours, any thing ſet down of this kind, I ſhall, till I have opportunity to repeat them, content myſelf to add what I find regiſtered concerning colours looked on by candle-light, in regard that not only the experiment is more eaſy to be repeated, but the objects being the ſame ſorts of coloured paper laſtly mentioned, the collation of the two experiments may help to make the conjectures they will ſuggeſt ſomewhat the leſs uncertain.

WITHIN a few days of the time above-mentioned, divers ſheets of coloured paper, that had been looked upon before in the ſun-ſhine, were looked upon at night by the light of a pretty big candle (ſnuffed) and the changes that were obſerved were theſe:

THE yellow ſeemed much fainter than in the day, and inclinable to a pale ſtraw-colour.

THE red ſeemed little changed; but ſeemed to reflect light more ſtrongly than any other colour (for white was none of them).

A FAIR deep green looked upon by itſelf, ſeemed to be a dark blue: but being looked upon together with a dark blue, appeared greeniſh; and beheld together with a yellow, appeared more blue than at firſt.

THE blue looked more like a deep purple or murray, than it had done in the day-light.

THE purple ſeemed very little altered.

THE red looked upon with the yellow made the yellow look almoſt like brown cap-paper.

N. B. THE caution ſubjoined to the third experiment is alſo applicable to this.

EXPERIMENT VIII.

BUT here I muſt not omit to ſubjoin, that to ſatisfy ourſelves, whether or no the light of a candle were not made unſincere, and as it were tinged with a yellow colour, by the admixtion of the corpuſcles it aſſumes from its fuel; we did not content ourſelves with what appears to the naked eye, but taking a pretty thick rod or cylinder (for thin pieces would not ſerve the turn) of deep blue glaſs, and looking upon the candle's flame at a convenient diſtance through it, we perceived, as we expected, the flame to look green: which, as we often note, is the colour wont to emerge from the compoſition of opacous bodies, which were apart one of them blue, and the other yellow. And this perchance may be the main reaſon of that, which ſome obſerve, that a ſheet of very white paper being looked upon by candle-light, it is not eaſy at firſt to diſcern it from a light yellow or lemon colour; white bodies (as we have elſewhere obſerved) having more than thoſe, that are otherwiſe coloured, of

of a ſpecular nature; in regard that though they exhibit not (unleſs they be poliſhed) the ſhape of the luminary, that ſhines on them, yet they reflect its light more ſincere and untroubled, by either ſhades or refractions, than bodies of other colours, (as blue, or green, or yellow, or the like.)

EXPERIMENT IX.

WE took a leaf of ſuch foliated gold, as apothecaries are wont to gild their pills with; and with the edge of a knife, (lightly moiſtened by drawing it over the ſurface of the tongue, and afterwards) laid upon the edge of the gold leaf, we ſo faſtened it to the knife, that being held againſt the light, it continued extended like a little flag. This leaf being held very near the eye, and obverted to the light, appeared ſo full of pores, that it ſeemed to have ſuch a kind of tranſparency, as that of a ſieve, or a piece of cypreſs, or a love-hood; but the light that paſſed by theſe pores was in its paſſages ſo tempered with ſhadow, and modified, that the eye diſcerned no more a golden colour, but a greeniſh blue. And for others ſatisfaction, we did in the night look upon a candle through ſuch a leaf of gold; and by trying the effect of ſeveral proportions of diſtance betwixt the leaf, the eye and the light, we quickly hit upon ſuch a poſition for the leaf of gold, as that the flame looked on through it, appeared of a greeniſh blue, as we have ſeen in the day-time. The like experiment tried with a leaf of ſilver ſucceeded not well.

EXPERIMENT X.

WE have ſometimes found in the ſhops of our druggiſts a certain wood, which is there called *Lignum Nephriticum*, becauſe the inhabitants of the country, where it grows, are wont to uſe the infuſion of it made in fair water againſt the ſtone of the kidneys. And indeed an eminent phyſician of our acquaintance, who has very particularly inquired into that diſeaſe, aſſures me, that he has found ſuch an infuſion one of the moſt effectual remedies, which he has ever tried againſt that formidable diſeaſe. The ancienteſt account I have met with of this ſimple, is given us by the experienced *Monardes* in theſe words: *Nobis*, ſays he, *Nova Hiſpana mittit quoddam ligni genus craſſum & enode, cujus uſus jam diu receptus fuit in his regionibus ad renum vitia, & urinæ difficultates ac arenulas pellendas. Fit autem hac ratione; lignum aſſulatim & minutim conciſum in limpidiſſima aqua fontana maceratur, inque ea relinquitur, donec aqua à bibentibus abſumpta ſit; dimidia hora poſt injectum lignum aqua cæruleum colorem contrahit, qui ſenſim intenditur pro temporis diuturnitate, tametſi lignum candidum ſit.* This wood, *Pyrophilus*, may afford us an experiment, which, beſides the ſingularity of it, may give no ſmall aſſiſtance to an attentive conſiderer towards the detection of the nature of colours. The experiment, as we made it, is this: Take *Lignum Nephriticum*, and with a knife cut it into thin ſlices; put about a handful of theſe ſlices into two, three or four pounds of the pureſt ſpring-water; let them infuſe there a night; but if you be in haſte, a much ſhorter time may ſuffice. Decant this impregnated water into a clear glaſs phial; and if you hold it directly between the light and your eye, you ſhall ſee it wholly tincted, (excepting the very top of the liquor, wherein you will ſometimes diſcern a ſky-coloured circle) with an almoſt golden colour, unleſs your infuſion have been made too ſtrong of the wood; for in that caſe it will againſt the light appear ſomewhat dark and reddiſh, and requires to be diluted by the addition of a convenient quantity of water. But if you hold this phial from the

Nicolaus Monardes lib. *ſimplic. ex India allator.* cap. 27.

the light, so that your eye be placed betwixt the window and the phial, the liquor will appear of a deep and lovely ceruleous colour, of which also the drops, if any be lying on the outside of the glass, will seem to be very perfectly. And thus far we have tried the experiment, and found it to succeed even by the light of candles of the larger size. If you so hold the phial over against your eyes, that it may have a window on one side of it, and a dark part of the room both before it and on the other side, you shall see the liquor partly of a bluish and partly of a golden colour. If turning your back to the window, you pour out some of the liquor towards the light and towards your eyes, it will seem at the coming out of the glass to be perfectly ceruleous; but when it is fallen down a little way, the drops may seem particoloured, according as the beams of light do more or less fully penetrate and illustrate them. If you take a bason about half full of water, and having placed it so in the sun-beams shining into a room, that one part of the water may be freely illustrated by the beams of light, and the other part of it darkened by the shadow of the brim of the bason; if then, I say, you drop of our tincture, made somewhat strong, both into the shade and illuminated parts of the water, you may by looking upon it from several places, and by a little agitation of the water, observe divers pleasing phænomena, which were tedious to particularize. If you pour a little of this tincture upon a sheet of white paper, so as the liquor may remain of some depth upon it, you may perceive the neighbouring drops to be partly of one colour, and partly of the other, according to the position of your eye in reference to the light when it looks upon them; but if you pour off the liquor, the paper will seem dyed of an almost yellow colour. And if a sheet of paper with some of this liquor in it be placed in a window where the sun-beams may shine freely on it, then if you turn your back to the sun and take a pen or some such slender body, and hold it overthwart betwixt the sun and the liquor, you may perceive, that the shadow projected by the pen upon the liquor will not all of it be a vulgar and dark, but in part a curiously coloured shadow; that edge of it, which is next the body that makes it, being almost of a lively golden colour, and the remoter verge of a ceruleous one.

THESE and other phænomena, which I have observed in this delightful experiment, divers of my friends have looked upon not without some wonder; and I remember an excellent oculist, finding by accident in a friend's chamber a fine phial full of this liquor, which I had given that friend, and having never heard any thing of the experiment, nor having any body near him, that could tell him what this strange liquor might be, was a great while apprehensive, as he presently after told me, that some strange new distemper was invading his eyes. And I confess, that the unusualness of the phænomena made me very solicitous to find out the cause of this experiment; and though I am far from pretending to have found it, yet my inquiries have, I suppose, enabled me to give such hints, as may lead your greater sagacity to the discovery of the cause of this wonder. And first finding that this tincture, if it were too copious in the water, kept the colours from being so lively, and their change from being so discernable, and finding also that the impregnating virtue of this wood did by its being frequently infused in new water by degrees decay; I conjectured that the tincture afforded by the wood must proceed from some subtiler parts of it drawn forth by the water, which swimming to and fro in it, did so modify the light, as to exhibit such and such colours: and because these subtile parts were so easily soluble even in cold water, I concluded that they must abound with salts, and perhaps contain much of the essential salt, as the chymists call it, of the wood. And to try whether these subtile parts were volatile enough to be distilled, without

without the diſſolution of their texture, I carefully diſtilled ſome of the tincted liquor in very low veſſels, and the gentle heat of a lamp furnace; but found all that came over to be as limpid and colourleſs as rock-water, and the liquor remaining in the veſſel to be ſo deeply ceruleous, that it required to be oppoſed to a very ſtrong light to appear of any other colour. I took likewiſe a phial with ſpirit of wine, and a little ſalt of hartſhorn, and found that there was a certain proportion to be met with betwixt the liquor and the ſalt, which made the mixture fit to exhibit ſome little variety of colours not obſervable in ordinary liquors, as it was variouſly directed in reference to the light and the eye; but this change of colour was very far ſhort from that which we had admired in our tincture. But, however, I ſuſpected that the tinging particles did abound with ſuch ſalts, whoſe texture, and the colour ſpringing from it, would probably be altered by piercing acid ſalts, which would in likelihood either make ſome diſſipation of their parts, or aſſociate themſelves to the like bodies, and either way alter the colour exhibited by them; whereupon pouring into a ſmall phial, full of impregnated water, a very little ſpirit of vinegar, I found that, according to my expectation, the ceruleous colour immediately vaniſhed, but was deceived in the expectation I had, that the golden colour would do ſo too; for, which way ſoever I turned the phial, either to or from the light, I found the liquor to appear always of a yellowiſh colour and no other. Upon this I imagined that the acid ſalts of the vinegar having been able to deprive the liquor of its ceruleous colour, a ſulphureous ſalt being of a contrary nature, would be able to mortify the ſaline particles of vinegar, and deſtroy their effects; and accordingly having placed myſelf betwixt the window, and the phial, and into the ſame liquor dropt a few drops of oil of tartar *per deliquium*, (as chymiſts call it) I obſerved with pleaſure, that immediately upon the diffuſion of this liquor, the impregnated water was reſtored to its former ceruleous colour; and this liquor of tartar being very ponderous, and falling at firſt to the bottom of the phial, it was eaſy to obſerve that for a little while the lower part of the liquor appeared deeply ceruleous, whilſt all the upper part retained its former yellowneſs, which it immediately loſt as ſoon as either agitation or time had made a competent diffuſion of the liquor of tartar through the body of the former tincture; and this reſtored liquor did, as it was looked upon againſt or from the light, exhibit the ſame phænomena as the tincted water did, before either of the adventitious liquors was poured into it.

Having made, *Pyrophilus*, divers trials upon this nephritick wood, we found mention made of it by the induſtrious Jeſuit *Kircherus*, who having received a cup turned of it from the Mexican procurator of his ſociety, has probably received alſo from him the information he gives us concerning that exotick plant; and therefore partly for that reaſon, and partly becauſe what he writes concerning it, does not perfectly agree with what we have delivered, we ſhall not ſcruple to acquaint you in his own words, with as much of what he writes concerning our wood, as is requiſite to our preſent purpoſe. *Hoc loco* (ſays he) *neutiquam omittendum duximus quoddam ligni candidi Mexicani genus, quod indigenæ Coalle & Tlapazatli vocant, quod etſi experientia hucuſque non niſi cæruleo aquam colore tingere docuerit, nos tamen continua experientia invenimus id aquam in omne colorum genus transformare, quod merito cuipiam paradoxum videri poſſet; ligni frutex grandis, ut aiunt, non rarò in molem arboris excreſcit, truncus illius eſt craſſus, enodis, inſtar piri arboris, folia ciceris foliis, aut rutæ haud abſimilia, flores exigui, oblongi, lutei & ſpicatim digeſti; eſt frigida & humida planta, licet parum recedat à medio temperamento. Hujus itaque deſcriptæ arboris lignum in poculum efformatum, aquam eidem infuſam primo in aquam intenſe cæruleam, colore floris bugloſſæ, tingit, &* Kircher. Ars. Magnæ lucis & umbræ, lib. 1. part 3.

quo diutius in eo steterit, tanto intensiorem colorem acquirit. Hanc igitur aquam si vitriæ sphæræ infuderis lucique exposueris, ne ullum quidem cærulei coloris vestigium apparebit, sed instar aquæ puræ putæ fontanæ limpidam claramque aspicientibus se præbebit. Porro si hanc phialam vitream versus locum magis umbrosum direxeris, totus humor gratissimum virorem referet; si adhuc umbrosioribus locis, subrubrum, & sic pro rerum objectarum conditione, mirum dictu, colorem mutabit; in tenebris verò vel in vase opaco posita cæruleum colorem suum resumet.

In this passage we may take notice of the following particulars. And first, he calls it a white Mexican wood, whereas (not to mention that *Monardes* informs us that it is brought out of *Nova Hispania*) the wood that we have met with in several places, and employed as *Lignum Nephriticum*, was not white, but for the most part of a much darker colour, not unlike that of the sadder coloured wood of juniper. It is true, that *Monardes* himself also says, that the wood is white; and it is affirmed, that the wood which is of a sadder colour is adulterated by being imbued with the tincture of a vegetable, in whose decoction it is steeped. But having purposely inquired of the eminentest of our English druggists, he peremptorily denied it. And indeed, having considered some of the fairest round pieces of this wood that I could meet with in these parts, I had opportunity to take notice that in one or two of them it was the external part of the wood that was white, and the more inward part that was of the other colour; the contrary of which would probably have appeared, if the wood had been adulterated after the aforementioned manner. And I have at present by me a piece of such wood, which for about an inch next the bark is white, and then, as it were, abruptly passes to the abovementioned colour; and yet this wood, by the tincture it afforded us in water, appears to have its coloured part genuine enough: for as for the white part, it appears, upon trial of both at once, much less enriched with the tingent property.

Next, whereas our author tells us, that the infusion of this wood exposed in a phial to the light, looks like spring-water, in which, he afterwards adds, that there is no tincture to be seen in it; our observation and his agree not: for the liquor which opposed to the darker part of a room exhibits a sky-colour, did constantly, when held against the light, appear yellowish or reddish, according as its tincture was more dilute or deep; and then, whereas it has been already said, that the ceruleous colour was by acid salts abolished, this yellowish one survived without any considerable alteration, so that unless our author's words be taken in a very limited sense, we must conclude, that either his memory misinformed him, or that his white nephritick wood, and the sadder coloured one which we employed, were not altogether of the same nature. What he mentions of the cup made of *Lignum Nephriticum*, we have not had opportunity to try, not having been able to procure pieces of that wood great enough, and otherwise fit to be turned into cups; but as for what he says in the title of his experiment, that this wood tinges the water with all sorts of colours, that is much more than any of those pieces of nephritick wood that we have hitherto employed, was able to make good; the change of colours discernable in a phial full of water, impregnated by any of them, as it is directed towards a place more lightsome or obscure, being far from affording a variety answerable to so promising a title. And as for what he tells us, that in the dark the infusion of our wood will resume a ceruleous colour, I wish he had informed us how he tried it.

But this brings into my mind, that having sometimes, for curiosity sake, brought a round phial with a long neck filled with the tincture of *Lignum Nephriticum* into the darkened room already often mentioned, and holding it sometimes in, sometimes near

near the ſun-beams that entered at the hole, and ſometimes partly in them, and partly out of them, the glaſs being held in ſeveral poſtures, and looked upon from ſeveral neighbouring parts of the room, diſcloſed a much greater variety of colours than in ordinary enlightened rooms it is wont to do; exhibiting, beſides the uſual colours, a red in ſome parts, and a green in others, beſides intermediate colours produced by the differing degrees, and odd mixtures of light and ſhade.

By all this you may ſee, *Pyrophilus*, the reaſonableneſs of what we elſewhere had occaſion to mention, when we have divers times told you, that it is uſeful to have new experiments tried over again, though they were, at firſt, made by knowing and candid men; ſuch reiterations of experiments commonly exhibiting ſome new phænomena, detecting ſome miſtake, or hinting ſome truth, in reference to them, that was not formerly taken notice of. And ſome of our friends have been pleaſed to think, that we have made no unuſual addition to this experiment, by ſhewing a way, how in a moment our liquor may be deprived of its blueneſs, and reſtored to it again by the affuſion of a few drops of liquors, which have neither of them any colour at all of their own. And that which deſerves ſome particular wonder, is, that the ceruleous tincture of our wood is ſubject by the former method to be deſtroyed or reſtored, the yellowiſh or reddiſh tincture continuing what it was. And that you may ſee, that ſalts are of a conſiderable uſe in the ſtriking of colours, let me add to the many experiments which may be afforded us to this purpoſe by the dyers trade, this obſervation; that as far as we have hitherto tried, thoſe liquors in general that are ſtrong of acid ſalts have the power of deſtroying the blueneſs of the infuſion of our wood, and thoſe liquors indiſcriminately that abound with ſulphureous ſalts (under which I comprehend the urinous and volatile ſalts of animal ſubſtances, and the alcalizate or fixed ſalts that are made by incineration) have the virtue of reſtoring it.

A Corollary of the Tenth Experiment.

That this experiment, *Pyrophilus*, may be as well uſeful as delightful to you, I muſt mind you, *Pyrophilus*, that in the newly mentioned obſervation, I have hinted to you a new and eaſy way of diſcovering in many liquors (for I dare not ſay in all) whether it be an acid or ſulphureous ſalt, that is predominant; and that ſuch a diſcovery is oftentimes of great difficulty, and may frequently be of great uſe, he that is not a ſtranger to the various properties and effects of ſalts, and of how great moment it is to be able to diſtinguiſh their tribes, may readily conceive. But to proceed to the way of trying other liquors by an infuſion of our wood, take it briefly thus. Suppoſe I have a mind to try whether I conjecture aright, when I imagine that allom, though it be plainly a mixt body, does abound rather with acid than ſulphureous ſalt: to ſatisfy myſelf herein, I turn my back to the light, and holding a ſmall phial full of the tincture of *Lignum Nephriticum*, which, looked upon in that poſition, appears ceruleous, I drop into it a little of a ſtrong ſolution of allom made in fair water; and finding upon the affuſion and ſhaking of this new liquor, that the blueneſs formerly conſpicuous on our tincture does preſently vaniſh, I am thereby incited to ſuppoſe, that the ſalt predominant in allom belongs to the family of ſour ſalts. But if on the other ſide I have a mind to examine whether or no I rightly conceive that ſalt of urine, or of hartſhorn is rather of a ſaline ſulphureous (if I may ſo ſpeak) than of an acid nature, I drop a little of the ſaline ſpirit of either into the nephritic tincture, and finding that the ceruleous colour is rather thereby deepened than deſtroyed,

ſtroyed, I collect that the ſalts, which conſtitute theſe ſpirits, are rather ſulphureous than acid. And to ſatisfy myſelf yet farther in this particular, I take a ſmall phial of freſh tincture, and placing both it and myſelf in reference to the light as formerly, I drop into the infuſion juſt as much diſtilled vinegar, or other acid liquor as will ſerve to deprive it of its bluenes (which a few drops, if the ſour liquor be ſtrong, and phial ſmall, will ſuffice to do;) then without changing my poſture, I drop and ſhake into the ſame phial a ſmall proportion of ſpirit of hartſhorn or urine, and finding that upon this affuſion the tincture immediately recovers its ceruleous colour, I am thereby confirmed in my former opinion, of the ſulphureous nature of theſe ſalts. And ſo, whereas it is much doubted by ſome modern chymiſts to what ſort of ſalt, that which is predominant in quick-lime belongs, we have been perſuaded to refer it rather to lixiviate than acid ſalts; and having obſerved, that though an evaporated infuſion of it will ſcarce yield ſuch a ſalt, as aſhes and other alcalizate bodies are wont to do, yet if we deprive our nephritic tincture of its bluenes by juſt ſo much diſtilled vinegar as is requiſite to make that colour vaniſh, the lixivium of quick-lime will immediately upon its affuſion recall the baniſhed colour, but not ſo powerfully as either of the ſulphureous liquors formerly mentioned. And therefore I allow myſelf to gueſs at the ſtrength of the liquors examined by this experiment, by the quantity of them which is ſufficient to deſtroy or reſtore the ceruleous colour of our tincture. But whether concerning liquors, wherein neither acid nor alcalizate ſalts are eminently predominant, our tincture will enable us to conjecture any thing more than that ſuch ſalts are not predominant in them, I take not upon me to determine here, but leave to further trial; for I find not that ſpirit of wine, ſpirit of tartar freed from acidity, or chymical oil of turpentine (although liquors which muſt be conceived very ſaline, if chymiſts have, which is here no place to diſpute, rightly aſcribed taſtes to the ſaline principle of bodies) have any remarkable power either to deprive our tincture of its ceruleous colour, or reſtore it, when upon the affuſion of ſpirit of vinegar it has diſappeared.

EXPERIMENT XI.

AND here I muſt not omit, *Pyrophilus*, to inform you, that we can ſhew you even in a mineral body ſomething that may ſeem very near of kin to the changeable quality of the tincture of *Lignum Nephriticum*; for we have ſeveral flat pieces of glaſs, of the thickneſs of ordinary panes for windows, one of which being interpoſed betwixt the eye and a clear light, appears of a golden colour, not much unlike that of the moderate tincture of our wood; but being ſo looked upon as that the beams of light are not ſo much trajected through it as reflected from it to the eye, that yellow ſeems to degen rate into a pale blue, ſomewhat like that of a turquoiſe. And that which may alſo appear ſtrange, is this, that if in a certain poſture you hold one of theſe plates perpendicular to the horizon, ſo that the ſun-beams ſhine upon half of it, the other half being ſhaded, you may ſee that the part ſhined upon will be of a much diluter yellow than the ſhaded part, which will appear more richly coloured; and if you alter the poſture of the glaſs, ſo that it be not held perpendicular, but parallel in reference to the horizon, you may ſee (which perhaps you will admire) the ſhaded part look of a golden colour, but the other that the ſun ſhines freely on, will appear conſiderably blue, and as you remove any part of the glaſs thus held horizontally into the ſun-beams or ſhade, it will in the twinkling of an eye ſeem to paſs from one of the above mentioned colours to the other; the ſun-beams trajected through it upon

a ſheet of white paper held near it, do colour it with yellow, ſomewhat bordering upon a red, but yet the glaſs may be ſo oppoſed to the ſun, that it may upon paper project a mixed colour here and there more inclined to yellow, and here and there more to blue. The other phænomena of this odd glaſs, I fear it would be ſcarce worth while to record; and therefore I ſhall rather advertiſe you, firſt, that in the trying of theſe experiments with it, you muſt take notice that one of the ſides has either alone, or at leaſt principally, its ſuperficial parts diſpoſed to the reflection of the blue colour above named, and that therefore you muſt have a care to keep that ſide neareſt to the eye. And next, that we have ourſelves made glaſſes not unfit to exhibit an experiment not unlike that I have been ſpeaking of, by laying upon pieces of glaſs ſome very finely foliated ſilver, and giving it by degrees a much ſtronger fire than is requiſite or uſual for the tinging of glaſſes of other colours. And this experiment, not to mention that it was made without a furnace, in which artificers that paint glaſs are wont to be very curious, is the more conſiderable, becauſe, that though a ſkilful painter could not deny to me that it was with ſilver he coloured his glaſſes yellow; yet he told me, that when to burn them (as they ſpeak) he lays on the plates of glaſs nothing but a calx of ſilver calcined without corroſive liquors, and tempered with fair water, the plates are tinged of a fine yellow that looks of a golden colour, which part ſoever of it you turn to or from the light; whereas (whether it be what an artificer would call over-doing, or burning, or elſe the employing the ſilver crude that makes the difference) we have found more than once, that ſome pieces of glaſs prepared as we have related, though held againſt the light they appeared of a tranſparent yellow, yet looked on with one's back turned to the light, they exhibited an untranſparent blue.

EXPERIMENT XII.

IF you will allow me, *Pyrophilus*, for the avoiding of ambiguity, to employ the word pigments, to ſignify ſuch prepared materials (as cochineal, vermilion, orpiment) as painters, dyers, and other artificers make uſe of to impart or imitate particular colours; I ſhall be the better underſtood in divers paſſages of the following papers, and particularly when I tell you, that the mixing of pigments being no inconſiderable part of the painters art, it may ſeem an incroachment in me to meddle with it. But I think I may eaſily be excuſed (though I do not altogether paſs it by) if I reſtrain myſelf to the making of a tranſient mention of ſome few of their practices about this matter; and that only ſo far forth, as may warrant me to obſerve to you, that there are but few ſimple and primary colours (if I may ſo call them) from whoſe various compoſitions all the reſt do as it were reſult. For though painters can imitate the hues (though not always the ſplendor) of thoſe almoſt numberleſs differing colours that are to be met with in the works of nature, and of art, I have not yet found, that to exhibit this ſtrange variety they need imploy any more than white, and black, and red, and blue, and yellow; theſe five, variouſly compounded, and (if I may ſo ſpeak) decompounded, being ſufficient to exhibit a variety and number of colours, ſuch as thoſe that are altogether ſtrangers to the painters pallets can hardly imagine.

THUS (for inſtance) black and white differingly mixed, make a vaſt company of lighter and darker greys.

BLUE and yellow make a huge variety of greens.

RED and yellow make orange-tawny.

RED

RED with a little white makes a carnation.

RED with an eye of blue, makes a purple; and by these simple compositions again compounded among themselves, the skilful painter can produce what kind of colour he pleases, and a great many more than we have yet names for. But, as I intimated above, it is not my design to prosecute this subject, though I thought it not unfit to take some notice of it, because we may hereafter have occasion to make use of what has been now delivered, to illustrate the generation of intermediate colours; concerning which we must yet subjoin this caution, that to make the rules about the emergency of colours fit to be relied upon, the corpuscles whereof the pigments consist must be such as do not destroy one another's texture; for in case they do, the produced colour may be very different from that which would result from the mixture of other harmless pigments of the same colours, as I shall have occasion to shew ere long.

EXPERIMENT XIII.

IT may also give much light to an inquirer into the nature of colours, to know that not only in green, but in many (if not all) other colours, the light of the sun passing through diaphanous bodies of differing hues may be tinged of the same compound colour, as if it came from some painters colours of the same denomination, though this latter be exhibited by reflection, and be (as the former experiment declares) manifestly compounded of material pigments. Wherefore to try the composition of colours by trajection, we provided several plates of tinged glass, which being laid two at a time, one on the top of another, the object looked upon through them both, appeared of a compounded colour, which agrees well with what we have observed in the second experiment, of looking against the light through differingly coloured papers. But we thought the experiment would be more satisfactory, if we procured the sun-beams to be so tinged in their passage through plates of glass, as to exhibit the compounded colour upon a sheet of white paper. And though by reason of the thickness of the glasses, the effect was but faint, even when the sun was high and shined forth clear, yet, we easily remedied that by contracting the beams we cast on them by means of a convex burning-glass, which, where it made the beams much converge, increased the light enough to make the compounded colour very manifest upon the paper. By this means we observed, that the beams trajected through blue and yellow composed a green; that an intense and moderate red did with yellow make differing degrees of saffron, and orange-tawny colours; that green and blue made a colour partaking of both, such as that which some Latin writers call *Pavonaceus*; that red and blue made a purple; to which we might add other colours, that we produced by the combinations of glasses differingly tinged, but that I want proper words to express them in our language, and had not, when we made the trials, the opportunity of consulting with a painter, who perchance might have supplied me with some of the terms I wanted.

I KNOW not whether it will be requisite to subjoin on this occasion, what I tried concerning reflections from coloured glasses, and other transparent bodies; namely, that having exposed four or five sorts of them to the sun, and cast the reflected beams upon white paper held near at hand, the light appeared not manifestly tinged, but as if it had been reflected from the impervious parts of a colourless glass; only that reflected from the yellow was here and there stained with the same colour, as if those beams were not all reflected from the superficial, but some from the internal parts of the

the glaſs; upon which occaſion you may take notice, that a ſkilful tradeſman, who makes ſuch coloured glaſs, told me, that whereas the red pigment was but ſuperficial, the yellow penetrated to the very midſt of the plate. But for further ſatisfaction, not having the opportunity to foliate thoſe plates, and ſo turn them into looking-glaſſes, we foliated a plate of *Muſcovy* glaſs, and then laying on it a little tranſparent varniſh of a gold colour, we expoſed it to the ſun-beams, ſo as to caſt them upon a body fit to receive them; on which the reflected light appearing, as we expected, yellow, manifeſted that rebounding from the ſpecular part of the ſelenitis, it was tinged in its return with the colour of the tranſparent varniſh through which it paſſed.

EXPERIMENT XIV.

AFTER what we have ſaid of the compoſition of colours, it will now be ſeaſonable to annex ſome experiments that we made in favour of thoſe colours, that are taught in the ſchools not to be real, but only apparent and fantaſtical; for we found by trials, that theſe colours might be compounded, both with true and ſtable colours, and with one another, as well as unqueſtionably genuine and laſting colours, and that the colours reſulting from ſuch compoſitions, would reſpectively deſerve the ſame denominations.

For firſt, having by the trajection of the ſun-beams through a glaſs priſm thrown an iris on the floor, I found that by placing a blue glaſs at a convenient diſtance betwixt the priſm and the iris, that part of the iris that was before yellow, might be made to appear green, though not of a graſs green, but of one more dilute and yellowiſh. And it ſeems not improbable, that the narrow greeniſh liſt (if I may ſo call it) that is wont to be ſeen between the yellow and blue parts of the iris, is made by the confuſion of thoſe two bordering colours.

Next, I found, that though the want of a ſufficient livelineſs in either of the compounding colours, or a light error in the manner of making the following trials, was enough to render ſome of them unſucceſsful, yet, when all neceſſary circumſtances were duly obſerved, the event was anſwerable to our expectation and deſire.

And (as I formerly noted) that red and blue compound a purple, ſo I could produce this laſt named colour, by caſting at ſome diſtance from the glaſs the blue part of the priſmatical iris (as I think it may be called for diſtinction ſake) upon a lively red (or elſe the experiment ſucceeds not ſo well.) And I remember, that ſometimes when I tried this upon a piece of red cloth, that part of the iris which would have been blue, (as I tried by covering that part of the cloth with a piece of white paper) and compounded with the red, wherewith the cloth was imbued before, appeared of a fair purple, did, when I came to view it near at hand, look very oddly, as if there were ſome ſtrange reflection or refraction, or both, made in the hairs of which that cloth was compoſed.

Caſting likewiſe the priſmatical iris upon a very vivid blue, I found that part of it, which would elſe have been the yellow, appear green. (Another ſomewhat differing trial, and yet fit to confirm this, you will find in the fifteenth experiment.)

But it may ſeem ſomewhat more ſtrange, that though the priſmatical iris being made by the refraction of light through a body that has no colour at all, muſt according to the doctrine of the ſchools, conſiſt of as purely emphatical colours as may be, yet even theſe may be compounded with one another, as well as real colours in the groſſeſt pigments. For I took at once two triangular glaſſes, and one of them being kept fixt in the ſame poſture, that the iris it projected on the floor might not waver, I caſt

on the ſame floor another iris with the other priſm, and moving it to and fro to bring what part of the ſecond iris I pleaſed, to fall upon what part of the firſt I thought fit, we did ſometimes (for a ſmall error ſuffices to hinder the ſucceſs) obtain by this means a green colour in that part of the more ſtable iris, that before was yellow, or blue; and frequently by caſting thoſe beams, that in one of the iriſes made the blue upon the red parts of the other iris, we were able to produce a lovely purple, which we can deſtroy or recompoſe at pleaſure, by ſevering and re-approaching the edges of the two iriſes.

EXPERIMENT XV.

ON this occaſion, *Pyrophilus*, I ſhall add, that finding the glaſs priſm to be the uſefulleſt inſtrument men have yet employed about the contemplation of colours, and conſidering that priſms, hitherto in uſe, are made of glaſs tranſparent and colourleſs, I thought it would not be amiſs to try, what change the ſuperinduction of a colour, without the deſtruction of the diaphaneity, would produce in the colours exhibited by the priſm. But being unable to procure one to be made of coloured glaſs, and fearing alſo that if it were not carefully made, the thickneſs of it would render it too opacous, I endeavoured to ſubſtitute one made of clarified roſin, or of turpentine brought (as I elſewhere teach) to the conſiſtence of a tranſparent gum. But though theſe endeavours were not wholly loſt, yet we found it ſo difficult to give theſe materials their true ſhape, that we choſe rather to varniſh over an ordinary priſm with ſome of thoſe few pigments that are to be had tranſparent; as accordingly we did firſt with yellow, and then with red, or rather crimſon, made with lake tempered with a convenient oil; and the event was, that for want of good tranſparent colours (of which you know there are but very few) both the yellow and the red made the glaſs ſo opacous (though the pigment were laid on but upon two ſides of the glaſs, no more being abſolutely neceſſary) that unleſs I looked upon an enlightened window, or the flame of a candle, or ſome other luminous or very vivid object, I could ſcarce diſcern any colours at all, eſpecially when the glaſs was covered with red. But when I did look on ſuch objects, it appeared (as I expected) that the colour of the pigment had vitiated or drowned ſome of thoſe which the priſm would, according to its wont, have exhibited, and mingling with others, altered them: as I remember, that both to my eyes, and others to whom I ſhewed it, when the priſm was covered with yellow, it made thoſe parts of bright objects, where the blue would elſe have been conſpicuous, appear of a light green. But, *Pyrophilus*, both the nature of the colours, and the degree of tranſparency, or of darkneſs in the pigment, beſides divers other circumſtances, did ſo vary the phænomena of theſe trials, that till I can procure ſmall coloured priſms, or hollow ones that may be filled with tincted liquor, or obtain ſome better pigments than thoſe I was reduced to employ, I ſhall forbear to build any thing upon what has been delivered, and ſhall make no other uſe of it, than to invite you to proſecute the enquiry further.

EXPERIMENT XVI.

AND here, *Pyrophilus*, ſince we are treating of emphatical colours, we ſhall add what we think not unworthy your obſervation, and not unfit to afford ſome exerciſe to the ſpeculative. For there are ſome liquors which, though colourleſs themſelves, when they come to be elevated, and diſperſed into exhalations, exhibit a conſpicuous

conſpicuous colour, which they loſe again, when they come to be reconjoined into a liquor; as good ſpirit of nitre, or upon its account ſtrong aqua fortis, though devoid of all appearance of redneſs whilſt they continue in the form of a liquor, if a little heat chance to turn the minute parts of them into vapours, the ſteam will appear of a reddiſh or deep yellow colour, which will vaniſh when thoſe exhalations come to reſume the form of a liquor.

And not only if you look upon a glaſs half full of aqua fortis, or ſpirit of nitre, and half full of nitrous ſteams proceeding from it, you will ſee the upper part of the glaſs of the colour freſhly mentioned, if through it you look upon the light: but which is much more conſiderable, I have tried, that putting aqua fortis in a long clear glaſs, and adding a little copper or ſome ſuch open metal to it, to excite heat and fumes, the light trajected through thoſe fumes, and caſt upon a ſheet of white paper, did upon that appear of the colour that the fumes did, when directly looked upon, as if the light were as well tinged in its paſſage through theſe fumes, as it would have been by paſſing through ſome glaſs or liquor in which the ſame colour was inherent.

To which I ſhall further add, that having ſometimes had the curioſity to obſerve whether the beams of the ſun near the horizon, trajected through a very red ſky, would not (though ſuch redneſſes are taken to be but emphatical colours) exhibit the like colour; I found that the beams falling within a room upon a very white object, placed directly oppoſite to the ſun, diſcloſed a manifeſt redneſs, as if they had paſſed through a coloured medium.

EXPERIMENT XVII.

THE emergency, *Pyrophilus*, of colours upon the coalition of the particles of ſuch bodies as were neither of them of the colour of that mixture whereof they are the ingredients, is very well worth our attentive obſervation, as being of good uſe both ſpeculative and practical: for much of the mechanical uſe of colours among painters and dyers doth depend upon the knowledge of what colours may be produced by the mixtures of pigments ſo and ſo coloured. And (as we lately intimated) it is of advantage to the contemplative Naturaliſt, to know how many and which colours are primitive (if I may ſo call them) and ſimple, becauſe it both eaſes his labour by confining his moſt ſolicitous enquiry to a ſmall number of colours upon which the reſt depend, and aſſiſts him to judge of the nature of particular compounded colours, by ſhewing him, from the mixture of what more ſimple ones, and of what proportions of them to one another, the particular colour to be conſidered does reſult. But becauſe, to inſiſt on the proportions, the manner and the effects of ſuch mixtures, would oblige me to conſider a greater part of the painter's art and dyer's trade, than I am well acquainted with, I confined myſelf to make trial of ſeveral ways to produce green, by the compoſition of blue and yellow: and ſhall in this place both recapitulate moſt of the things I have diſperſedly delivered already concerning that ſubject, and recruit them.

And firſt, whereas painters (as I noted above) are wont to make green by tempering blue and yellow, both of them made into a ſoft conſiſtence, with either water or oil, or ſome liquor of kin to one of thoſe two, according as the picture is to be drawn with thoſe they call water-colours, or thoſe they term oil-colours; I found, that by chuſing fit ingredients, and mixing them in the form of dry powders, I could do, what I could not if the ingredients were tempered up with a liquor: but the blue and yellow powders muſt not only be finely ground, but ſuch as that the corpuſcles of

the one may not be too unequal to those of the other, lest by the disproportionate minuteness the smaller cover and hide the greater. We used with good success a slight mixture of the fine powder of bise, with that of orpiment, or that of good yellow oker; I say, a slight mixture, because we found that an exquisite mixture did not do so well: but by lightly mingling the two pigments in several little parcels, those of them in which the proportion and manner of mixture was more lucky, afforded us a good green.

2. We also learned in the dye-houses, that cloth being dyed blue with woad, is afterwards by the yellow decoction of woud-wax or wood-wax dyed into a green colour.

3. You may also remember what we above related, where we intimated, that having in a darkened room taken two bodies, a blue and a yellow, and cast the light reflected from the one upon the other, we likewise obtained a green.

4. And you may remember, that we observed a green to be produced, when in the same darkened room we looked at the hole at which alone the light entered, through the green and yellow parts of a sheet of marbled paper laid over one another.

5. We found too, that the beams of the sun being trajected through two pieces of glass, the one blue and the other yellow, laid over one another, did upon a sheet of white paper, on which they were made to fall, exhibit a lovely green.

6. I hope also, that you have not already forgot, what was so lately delivered, concerning the composition of a green, with a blue and yellow: of which most authors would call the one a real, and the other an emphatical.

7. And I presume, you may have yet fresh in your memory, what the fourteenth experiment informs you, concerning the exhibiting of a green, by the help of a blue and yellow, that were both of them emphatical.

8. Wherefore we will proceed to take notice, that we also devised a way of trying whether or no metalline solutions, though one of them at least had its colour adventitious, by the mixture of the menstruum employed to dissolve it, might not be made to compound a green after the manner of other bodies. And though this seemed not easy to be performed by reason of the difficulty of finding metalline solutions of the colour requisite, that would mix without precipitating each other; yet after a while having considered the matter, the first trial afforded me the following experiment. I took a high yellow solution of good gold in aqua regis (made of aqua fortis, and as I remember half its weight of spirit of salt); to this I put a due proportion of a deep and lovely blue solution of crude copper (which I have elsewhere taught to be readily dissolvable in strong spirit of urine.) And these two liquors, though at first they seemed a little to curdle one another, yet being thoroughly mingled by shaking, they presently, as had been conjectured, united into a transparent green liquor, which continued so for divers days, that I kept it in a small glass wherein it was made, only letting fall a little blackish powder to the bottom. The other phænomena of this experiment belong not to this place, where it may suffice to take notice of the production of a green, and that the experiment was more than once repeated with success.

9. And lastly, to try whether this way of compounding colours would hold even in ingredients actually melted by the violence of the fire, provided their texture were capable of safely enduring fusion, we caused some blue and yellow ammel to be long and well wrought together in the flame of a lamp, which being strongly and incessantly blown on them, kept them in some degree of fusion, and at length (for the experiment requires some patience as well as skill) we obtained the expected ammel of a green colour.

I know

I know not, *Pyrophilus*, whether it be worth while to acquaint you with the ways that came into my thoughts, whereby in some measure to explicate the first of the mentioned ways of making a green; for I have sometimes conjectured, that the mixture of the bise and the orpiment produced a green by so altering the superficial asperity, which each of those ingredients had apart, that the light incident on the mixture was reflected with differing shades, as to quantity, or order, or both, from those of either of the ingredients, and such as the light is wont to be modified with, when it reflects from grass, or leaves, or some of those other bodies that we are wont to call green. And sometimes too I have doubted, whether the produced green might not be partly at least derived from this, that the beams that rebound from the corpuscles of the orpiment, giving one kind of stroke upon the retina, whose perception we call yellow, and the beams reflected from the corpuscles of the bise giving another stroke upon the same retina, like to objects that are blue; the contiguity and minuteness of these corpuscles may make the appulse of the reflected light fall upon the retina within so narrow a compass, that the part they beat upon being as it were a physical point, they may give a compounded stroke, which may consequently exhibit a compounded and new kind of sensation: as we see that two strings of a musical instrument being struck together, making two noises that arrive at the ear at the same time as to sense, yield a sound differing from either of them, and as it were compounded of both; insomuch that if they be discordantly tuned, though each of them struck apart would yield a pleasing sound, yet being struck together they make but a harsh and troublesome noise. But this not being so fit a place to prosecute speculations, I shall not insist, neither upon these conjectures nor any others, which the experiment we have been mentioning may have suggested to me. And I shall leave it to you, *Pyrophilus*, to derive what instruction you can from comparing together the various ways whereby a yellow and a blue can be made to compound a green: that which I now pretend to, being only to shew that the first of those mentioned ways (not to take at present notice of the rest) does far better agree with our conjectures about colours, than either with the doctrine of the schools, or with that of the chymists, both which seem to be very much disfavoured by it.

For first, since in the mixture of the two mentioned powders I could by the help of a very excellent microscope (for ordinary ones will scarce serve the turn) discover that which seemed to the naked eye a green body, to be but a heap of distinct, though very small grains of yellow orpiment and blue bise confusedly enough blended together, it appears that the coloured corpuscles of either kind did each retain its own nature and colour; by which it may be guessed, what meer transposition and juxtaposition of minute and singly unchanged particles of matter can do to produce a new colour. For that this local motion and new disposition of the small parts of the orpiment did intervene, is much more manifest than it is easy to explicate how they should produce this new green, otherwise than by the new manner of their being put together, and consequently by their new disposition to modify the incident light, by reflecting it otherwise than they did before they were mingled together.

Secondly, The green thus made, being (if I may so speak) mechanically produced, there is no pretence to derive it from I know not what incomprehensible substantial form, from which yet many would have us believe that colours must flow; nor does this green, though a real and permanent, not a phantastical and vanid colour, seem to be such an inherent quality as they would have it, since not only each part of the mixture remains unaltered in colour, and consequently of a differing colour from the heap they compose; but if the eye be assisted by a microscope to dis-

cern

cern things better and more distinctly than before it could, it sees not a green body, but a heap of blue and yellow corpuscles.

And in the third place, I demand what either sulphur, or salt, or mercury has to do in the production of this green; for neither the bise nor the orpiment were endued with that colour before; and the bare juxtaposition of the corpuscles of the two powders that work not upon each other, but might, if we had convenient instruments, be separated, unaltered, cannot, with any probability, be imagined either to increase or diminish any of the three hypostatical principles (to which of them soever the chymists are pleased to ascribe colours); nor does there here intervene so much as heat to afford them any colour to pretend, that at least there is made an extraversion (as the Helmontians speak) of the sulphur, or of any of the two other supposed principles. But upon this experiment we have already reflected enough, if not more than enough for once.

EXPERIMENT XVIII.

But here, *Pyrophilus*, I must advertise you, that it is not every yellow and every blue that, being mingled, will afford a green; for in case one of the ingredients do not act only as endowed with such a colour, but as having a power to alter the texture of the corpuscles of the other, so as to indispose them to reflect the light, as corpuscles that exhibit a blue or a yellow are wont to reflect it; the emergent colour may be not green, but such as the change of texture in the corpuscles of one or both of the ingredients qualifies them to shew forth: as for instance, if you let fall a few drops of syrup of violets upon a piece of white paper, though the syrup being spread will appear blue, yet mingling with it two or three drops of the lately mentioned solution of gold, I obtained not a green but a reddish mixture, which I expected from the remaining power of the acid salts abounding in the solution, such salts or saline spirits being wont, as we shall see anon, though weakened, so to work upon that syrup as to change it into a red or reddish colour. And to confirm that for which I allege the former experiment, I shall add this other, that having made a very strong and high-coloured solution of filings of copper with spirit of urine, though the menstruum seemed glutted with the metal, because I put in so much filings, that many of them remained for divers days undissolved at the bottom; yet having put three or four drops of syrup of violets upon white paper, I found that the deep blue solution proportionably mingled with this other blue liquor, did not make a blue mixture, but, as I expected, a fair green, upon the account of the urinous salt that was in the menstruum.

EXPERIMENT XIX.

To shew the chymists, that colours may be made to appear or vanish, where there intervenes no accession or change either of the sulphureous, or the saline, or the mercurial principle (as they speak) of bodies; I shall not make use of the iris afforded by the glass-prism, nor of the colours to be seen in a fair morning in those drops of dew that do in a convenient manner reflect and refract the beams of light to the eye: but I will rather mind them of what they may observe in their own laboratories, namely, that divers, if not all, chymical essential oils, as also good spirit of wine, being shaken till they have good store of bubbles, those bubbles will (if attentively considered) appear adorned with various and lovely colours, which all immediately

diately vanish, upon the relapsing of the liquor that affords those bubbles their skins, into the rest of the oil, or spirit of wine; so that a colourless liquor may be made in a trice to exhibit variety of colours, and may lose them in a moment without the accession or diminution of any of its hypostatical principles. And, by the way, it is not unworthy our notice, that some bodies, as well colourless as coloured, by being brought to a great thinness of parts, acquire colours though they had none before, or colours differing from them they were before endued with: for, not to insist on the variety of colours, that water, made somewhat glutinous by soap, acquires when it is blown into such spherical bubbles as boys are wont to make and play with; turpentine (though it have a colour deep enough of its own) may (by being blown into after a certain manner) be brought to afford bubbles adorned with variety of orient colours, which though they vanish some while upon the breaking of the bubbles, yet they would in all likelihood always exhibit colours upon their superficies (though not always the same in the same parts of them, but varied according to the incidence of the sight, and the position of the eye) if their texture were durable enough. For I have seen one that was skilled at fashioning glasses by the help of a lamp, blowing some of them so strongly as to burst them; whereupon it was found, that the tenacity of the metal was such, that before it broke, it suffered itself to be reduced into films so extremely thin, that being kept clean they constantly shewed on their surfaces (but after the manner newly mentioned) the varying colours of the rainbow, which were exceedingly vivid, as I had often opportunity to observe in some, that I caused purposely to be made, to keep by me.

But lest it should be objected, that the above-mentioned instances are drawn from transparent liquors, it may possibly appear not impertinent to add, what I have sometimes thought upon, and several times tried, when I was considering the opinions of the chymists about colours. I took then a feather of a convenient bigness and shape, and holding it at a fit distance betwixt my eye and the sun when he was near the horizon, methought there appeared to me a variety of little rainbows, with differing and very vivid colours, of which none was constantly to be seen in the feather; the like phænomenon I have at other times (though not with altogether so good success) produced, by interposing at a due distance a piece of black ribband betwixt the almost setting sun and my eye; not to mention the trials I have made to the same purpose, with other bodies.

EXPERIMENT XX.

TAKE good syrup of violets, impregnated with the tincture of the flowers, drop a little of it upon a white paper (for by that means the change of colour will be more conspicuous, and the experiment may be practised in smaller quantities) and on this liquor let fall two or three drops of spirit either of salt or vinegar, or almost any other eminently acid liquor, and upon the mixture of these you shall find the syrup immediately turned red: and the way of effecting such a change has not been unknown to divers persons, who have produced the like, by spirit of vitriol, or juice of lemons, but have groundlessly ascribed the effect to some peculiar quality of those two liquors, whereas (as we have already intimated) almost any acid salt will turn syrup of violets red. But to improve the experiment, let me add what has not (that I know of) been hitherto observed, and has, when we first shewed it them, appeared something strange, even to those that have been inquisitive into the nature of colours; namely, that if instead of spirit of salt, or that of vinegar, you drop upon

the ſyrup of violets a little oil of tartar *per deliquium*, or the like quantity of ſolution of pot-aſhes, and rub them together with your finger, you ſhall find the blue colour of the ſyrup turned in a moment into a perfect green; and the like may be performed by divers other liquors, as we may have occaſion elſewhere to inform you.

Annotation upon the Twentieth EXPERIMENT.

THE uſe of what we lately delivered concerning the way of turning ſyrup of violets red or green, may be this; that, though it be a far more common and procurable liquor than the infuſion of *lignum nephriticum*, it may yet be eaſily ſubſtituted in its room, when we have a mind to examine, whether or no the ſalt predominant in a liquor or other body, wherein it is looſe and abundant, belong to the tribe of acid ſalts or not. For if ſuch a body turn the ſyrup of a red or reddiſh purple colour, it does for the moſt part argue the body (eſpecially if it be a diſtilled liquor) to abound with acid ſalt. But if the ſyrup be made green, that argues the predominant ſalt to be of a nature repugnant to that of the tribe of acids. For, as I find that either ſpirit of ſalt, or oil of vitriol, or aqua fortis, or ſpirit of vinegar, or juice of lemons, or any of the acid liquors I have yet had occaſion to try, will turn ſyrup of violets of a red, or at leaſt of a reddiſh colour; ſo I have found, that not only the volatile ſalts of all animal ſubſtances I have uſed, as ſpirit of hartſhorn, of urine, of ſal armoniac, of blood, *&c.* but alſo all the alcalizate ſalts I have employed, as the ſolution of ſalt of tartar, of pot-aſhes, of common wood-aſhes, lime-water, *&c.* will immediately change the blue ſyrup into a perfect green. And by the ſame way (to hint that upon the by) I elſewhere ſhow you, both the changes that nature and time produce, in the more ſaline parts of ſome bodies, may be diſcovered, and alſo how even ſuch chymically prepared bodies, as belong not either to the animal kingdom, or to the tribe of alcalies, may have their new and ſuperinduced nature ſucceſsfully examined. In this place I ſhall only add, that not alone the changing the colour of the ſyrup requires, that the changing body be more ſtrong of the acid, or other ſort of ſalt, that is predominant in it, than is requiſite for the working upon the tincture of *lignum nephriticum*; but that in this alſo, the operation of the formerly mentioned ſalts upon our ſyrup, differs from their operation upon our tinctures; that in this liquor, if the ceruleous colour be deſtroyed by an acid ſalt, it may be reſtored by one that is either volatile, or lixiviate; whereas in ſyrup of violets, though one of theſe contrary ſalts will deſtroy the action of the other, yet neither of them will reſtore the ſyrup to its native blue; but each of them will change it into the colour which itſelf doth (if I may ſo ſpeak) affect, as we ſhall have occaſion to ſhew in the notes on the twenty-fifth experiment.

EXPERIMENT XXI.

Herbaliſts are wont to call this plant Cyanus vulgaris minor.

THERE is a weed, more known to plowmen than beloved by them, whoſe flowers from their colour are commonly called blue-bottles, and corn-weed from their growing among corn. Theſe flowers, ſome ladies do, upon the account of their lovely colour, think worth the being candied, which when they are, they will long retain ſo fair a colour, as makes them a very fine ſallad in the winter. But I have tried, that when they are freſhly gathered, they will afford a juice, which when newly expreſſed (for in ſome caſes it will ſoon enough degenerate) affords a very deep and pleaſant blue. Now (to draw this to our preſent ſcope) by dropping on this freſh

juice a little ſpirit of ſalt (that being the acid ſpirit I had then at hand) it immediately turned (as I predicted) into a red. And if, inſtead of the ſour ſpirit, I mingled with it a little ſtrong ſolution of an alcalizate ſalt, it did preſently diſcloſe a lovely green; the ſame changes being, by thoſe differing ſorts of ſaline liquors, producible in this natural juice, that we lately mentioned to have happened to that factitious mixture, the ſyrup of violets. And I remember, that finding this blue liquor, when freſhly made, to be capable of ſerving in a pen for an ink of that colour, I attempted by moiſtening one part of a piece of white paper with the ſpirit of ſalt I have been mentioning, and another with ſome alcalizate or volatile liquor, to draw a line on the leiſurely dried paper, that ſhould even before the ink was dry appear partly blue, partly red, and partly green: but though the latter part of the experiment ſucceeded not well (whether becauſe volatile ſalts are too fugitive to be retained in the paper, and alcalizate ones are too unctuous, or ſo apt to draw moiſture from the air, that they keep the paper from drying well) yet the former part ſucceeded well enough; the blue and the red being conſpicuous enough to afford a ſurprizing ſpectacle to thoſe, I acquaint not with (what I willingly allow you to call) the trick.

Annotation upon the one and twentieth EXPERIMENT.

BUT leſt you ſhould be tempted to think *(Pyrophilus)* that volatile or alcalizate ſalts change blues into green, rather upon the ſcore of the eaſy tranſition of the former colour into the latter, than upon the account of the texture, wherein moſt vegetables, that afford a blue, ſeem, though otherwiſe differing, to be allied; I will add, that when I purpoſely diſſolved blue vitriol in fair water, and thereby imbued ſufficiently that liquor with that colour, a lixiviate liquor, and a urinous ſalt being copiouſly poured upon diſtinct parcels of it, did each of them, though perhaps with ſome difference, turn the liquor not green, but of a deep yellowiſh colour, almoſt like that of yellow oker; which colour, the precipitated corpuſcles retained, when they had leiſurely ſubſided in the bottom. What this precipitated ſubſtance is, it is not needful now to inquire in this place, and in another I have ſhewn you, that notwithſtanding its colour, and its being obtainable from an acid menſtruum by the help of ſalt of tartar, it is yet far enough from being the true ſulphur of vitriol.

EXPERIMENT XXII.

OUR next experiment *(Pyrophilus)* will perhaps ſeem to be of a contrary nature to the two former, made upon ſyrup of violets and juice of blue-bottles. For as in them, by affuſion of oil of tartar, a blueiſh liquor is made green, ſo in this, by the ſole mixture of the ſame oil, a greeniſh liquor becomes blue. The hint of this experiment was given us by the practice of ſome Italian painters, who being wont to counterfeit *Ultra-marine Azure* (as they call it) by grinding verdigreaſe with ſal-armoniac, and ſome other ſaline ingredients, and letting them rot (as they imagine) for a good while together in a dunghill, we ſuppoſed that the change of colour wrought in the verdigreaſe by this way of preparation muſt proceed from the action of certain volatile and alcalizate ſalts, abounding in ſome of the mingled concretes, and brought to make a further diſſolution of the copper abounding in the verdigreaſe; and therefore we conjectured, that if both the verdigreaſe, and ſuch ſalts were diſſolved in fair water, the ſmall parts of both being therein more ſubdivided and ſet at liberty, would have better acceſs to each other, and thereby incorporate much the more ſuddenly.

And accordingly we found, that if upon a ſtrong ſolution of good French verdigreaſe (for it is that we are wont to employ, as the beſt) you pour a juſt quantity of oil of tartar, and ſhake them well together, you ſhall immediately ſee a notable change of colour, and the mixture will grow thick, and not tranſparent; but if you ſtay a while, till the groſſer part be precipitated to, and ſettled in the bottom, you may obtain a clear liquor of a very lovely colour, and exceeding delightful to the eye. But, you muſt have a care to drop in a competent quantity of oil of tartar, for elſe the colour will not be ſo deep and rich; and if inſtead of this oil you employ a clear lixivium of pot-aſhes, you may have an azure ſomewhat lighter or paler than, and therefore differing from, the former. And if inſtead of either of theſe liquors, you make uſe of ſpirit of urine, or of hartſhorn, you may, according to the quantity and quality of the ſpirit you pour in, obtain ſome further variety (though ſcarce conſiderable) of ceruleous liquors. And yet lately by the help of this urinous ſpirit we made a blue liquor, which not a few ingenious perſons, and among them, ſome, whoſe profeſſion makes them very converſant with colours, have looked upon with ſome wonder. But theſe azure-coloured liquors ſhould be freed from the ſubſiding matter, which the ſalts of tartar or urine precipitate out of them, rather by being decanted, than by filtration. For by the latter of theſe ways, we have ſometimes found the colour of them very much impaired, and little ſuperior to that of the groſſer ſubſtance, that is left in the filtre.

EXPERIMENT XXIII.

THAT roſes held over the fume of ſulphur, may quickly by it be deprived of their colour, and have as much of their leaves, as the fume works upon, burned pale, is an experiment, that divers others have tried, as well as I. But *(Pyrophilus)* it may ſeem ſomewhat ſtrange to one that has never conſidered the compounded nature of brimſtone, that whereas the fume of ſulphur will, as we have ſaid, whiten the leaves of roſes; that liquor, which is commonly called, oil of ſulphur *per campanam*, becauſe it is ſuppoſed to be made by the condenſation of theſe fumes, in glaſſes ſhaped like bells, into a liquor, does powerfully heighten the tincture of red roſes, and make it more red and vivid, as we have eaſily tried by putting ſome red-roſe leaves, that had been long dried (and ſo had loſt much of their colour) into a phial of fair water. For a while after the affuſion of a convenient quantity of the liquor we are ſpeaking of, both the leaves themſelves, and the water they were ſteeped in, diſcovered a very freſh and lovely colour.

EXPERIMENT XXIV.

IT may *(Pyrophilus)* ſomewhat ſerve to illuſtrate, not only the doctrine of pigments, and of colours, but divers other parts of the corpuſcular philoſophy, as that explicates odours, and many other things, not as the ſchools by airy qualities, but by real, though extremely minute bodies; to examine, how much of the colourleſs liquor a very ſmall parcel of a pigment may imbue with a diſcernable colour. And though there be ſcarce any thing of preciſeneſs to be expected from ſuch trials, yet I preſumed, that (at leaſt) I ſhould be able to ſhow a much further ſubdiviſion of the parts of matter into viſible particles, than I have hitherto found taken notice of, and than moſt men would imagine; nobody, that I know of, having yet attempted to reduce this matter to any meaſure.

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THE bodies, the most promising for such a purpose, might seem to be the metals, especially gold, because of the multitude and minuteness of its parts, which might be argued from the incomparable closeness of its texture: but though we tried a solution of gold made in aqua regia first, and then in fair water, yet in regard we were to determine the pigment we employed, not by bulk, but weight, and because also, that the yellow colour of gold is but a faint one in comparison of the deep colour of cochineal, we rather chose this to make our trials with. But among divers of these it would suffice to set down one, which was carefully made in vessels conveniently shaped, (and that in the presence of a witness, and an assistant) the sum whereof I find among my *Adversaria*, registered in the following words. To which I shall only premise, (to lessen the wonder of so strange a diffusion of the pigment) that cochineal will be better dissolved, and have its colour far more heightened by spirit of urine, than (I say not by common water, but) by rectified spirit of wine itself.

THE note I spoke of is this: [One grain of cochineal dissolved in a pretty quantity of spirit of urine, and then dissolved further by degrees in fair water, imparted a discernable, though but a very faint colour, to about six glass fulls of water, each of them containing about forty three ounces and a half, which amounts to above a hundred twenty five thousand times its own weight.]

EXPERIMENT XXV.

IT may afford a considerable hint *(Pyrophilus)* to him, that would improve the art of dying, to know what change of colours may be produced by the three several sorts of salts already often mentioned, (some or other of which may be procured in quantity at reasonable rates) in the juices, decoctions, infusions, and (in a word) the more soluble parts of vegetables. And, though the design of this discourse be the improvement of knowledge, not of trades; yet thus much I shall not scruple to intimate here, that the blue liquors, mentioned in the twentieth and one and twentieth experiments, are far from being the only vegetable substances upon which acid, urinous, and alcalizate salts have the like operations to those recited in those two experiments. For ripe privet berries (for instance) being crushed upon white paper, though they stain it with a purplish colour, yet if we let fall on some part of it two or three drops of spirit of salt, and on the other part a little more of the strong solution of pot-ashes, the former liquor immediately turned that part of the thick juice or pulp, on which it fell, into a lovely red, and the latter turned the other part of it into a delightful green. Though I will not undertake, that those colours in that substance shall not be much more orient than lasting; and though *(Pyrophilus)* this experiment may seem to be almost the same with those already delivered concerning syrup of violets, and the juice of blue-bottles, yet I think it not amiss to take occasion to inform you, that this experiment reaches much farther than perhaps you yet imagine, and may be of good use to those, whom it concerns to know how dying stuffs may be wrought upon by saline liquors. For, I have found this experiment to succeed in so many various berries, flowers, blossoms, and other finer parts of vegetables, that neither my memory, nor my leisure serves me to enumerate them. And it is somewhat surprizing to see, by how differingly-coloured flowers, or blossoms, (for example) the paper being stained, will by an acid spirit be immediately turned red, and by any alcaly or any urinous spirit turned green; insomuch that even the crushed blossoms of meserion, (which I gathered in winter and frosty weather) and those of peas, crushed upon white paper, how remote soever their colours be

from green, would in a moment pass into a deep degree of that colour, upon the touch of an alcalizate liquor. To which let us add, that either of those new pigments (if I may so call them) may, by the affusion of enough of a contrary liquor, be presently changed from red into green, and from green into red: which observation will hold also in syrup of violets, juices of blue-bottles, *&c.*

ANNOTATION.

AFTER what I have formerly delivered to evince that there are many instances, wherein new colours are produced or acquired by bodies, which chymists are wont to think destitute of salt, or to whose change of colours no new accession of saline particles does appear to contribute; I think we may safely enough acknowledge, that we have taken notice of so many changes made by the intervention of salts in the colours of mixed bodies, that it has lessened our wonder, that though many chymists are wont to ascribe the colours of such bodies to their sulphureous, and the rest to their mercurial principle; yet *Paracelsus* himself directs us in the indagation of colours, to have an eye principally upon salts, as we find in that passage of his, wherein he takes upon him to oblige his readers much by instructing them, of what things they are to expect the knowledge from each of the three distinct principles of bodies. *Alias* (says he) *colorum similis ratio est: de quibus brevem institutionem hanc attendite, quod scilicet colores omnes ex sale prodeant. Sal enim dat colorem, dat balsamum.* And a little beneath; *Jam natura ipsa colores protrahit ex sale, cuique speciei dans illum, qui ipsi competit, &c.* After which he concludes; *Itaque qui rerum omnium corpora cognoscere, vult, huic opus est, ut ante omnia cognoscat sulphur; ab hoc, qui desiderat novisse colores, is scientiam istorum petat à sale; qui scire vult virtutes, is scrutetur arcana Mercurii. Sic nimirum fundamentum hauserit mysteriorum, in quolibet crescenti indagandorum, prout natura cuilibet specie ea ingessit.* But though *Paracelsus* ascribes to each of his beloved hypostatical principles much more than I fear will be found to belong to it; yet if we please to consider colours, not as philosophers, but as dyers, the concurrence of salts to the striking and change of colours, and their efficacy, will, I suppose, appear so considerable, that we shall not need to quarrel much with *Paracelsus*, for ascribing in this place (for I dare not affirm that he uses to be still of one mind) the colours of bodies to their salts, if by salts he here understood not only elementary salts, but such also as are commonly taken for salts, as allom, crystals of tartar, vitriol, *&c.* because the saline principle does chiefly abound in them, though indeed they be, as we elsewhere declare, mixed bodies, and have most of them, besides what is saline, both sulphureous, aqueous, and gross or earthy parts.

Paracelsus de Mineral. tract. 1. pag. m. 243.

BUT though *(Pyrophilus)* I have observed a red and green to be produced, the former, by acid salts, the latter by salts not acid, in the exprest juices of so many differing vegetable substances, that the observation, if pursued, may prove (as I said) of good use: yet to show you how much even these effects depend upon the particular texture of bodies, I must subjoin some cases wherein I (who am somewhat backwards to admit observations for universal) had the curiosity to discover, that the experiments would not uniformly succeed; and of these exceptions, the chief that I now remember, are reducible to the following three.

EXPERIMENT XXVI.

AND, (first) I thought fit to try the operation of acid salts upon vegetable substances, that are already and by their own nature red. And accordingly I made trial upon syrup of clove-julyflowers, the clear expressed juice of the succulent berries of *Spina Cervina*, or buckthorn, (which I had long kept by me for the sake of its deep colour) upon red roses, infusion of Brazil, and divers other vegetable substances, on some of which crushed (as is often mentioned) upon white paper (which is also to be understood in most of these experiments, if no circumstance of them argue otherwise) spirit of salt either made no considerable change, or altered the colour but from a darker to a lighter red. How it will succeed in many other vegetable juices, and infusions of the same colour, I have at present so few at hand, that I must leave you to find it out yourself. But as for the operation of the other sort of salts upon these red substances, I found it not very uniform, some red, or reddish infusions, as of roses, being turned thereby into a dirty colour, but yet inclining to green. Nor was the syrup of clove-julyflowers turned by the solution of potashes to a much better, though somewhat a greener colour. Another sort of red infusions was by an alcaly not turned into a green, but advanced into a crimson, as I shall have occasion to note ere long. But there were other sorts, as particularly the lovely coloured juice of buckthorn berries, that readily passed into a lovely green.

EXPERIMENT XXVII.

AMONG other vegetables, which we thought likely to afford exceptions to the general observation about the differing changes of colours produced by acid and sulphureous salts, we thought fit to make trial upon the flowers of jasmin, they being both white as to colour, and esteemed to be of a more oily nature than other flowers. Whereupon having taken the white parts only of the flowers, and rubbed them somewhat hard with my finger, upon a piece of clean paper, it appeared very little discoloured. Nor had spirit of salt, wherewith I moistened one part of it, any considerable operation upon it. But spirit of urine, and somewhat more effectually a strong alcalizate solution, did immediately turn the almost colourless paper moistened by the juice of the jasmin, not as those liquors are wont to do, when put upon the juices of other flowers, of a good green, but of a deep, though somewhat greenish yellow; which experiment I did afterwards at several times repeat with the like success. But it seems not that a great degree of unctuousness is necessary to the production of the like effects, for when we tried the experiment with the leaves of those purely white flowers that appear about the end of winter, commonly called snow-drops, the event was not much unlike that, which we have been newly mentioning.

EXPERIMENT XXVIII.

ANOTHER sort of instances to show how much changes of colours, effected by salts, depend upon the particular texture of the coloured bodies, has been afforded me by several yellow flowers, and other vegetables, as mary-gold leaves, early primroses, fresh madder, *&c.* For being rubbed upon white paper, till they imbued it with their colour, I found not, that by the addition of alcalizate liquors, nor yet by that of an urinous spirit, they would be turned either green or red: nor did

did so acid a spirit as that of salt considerably alter their colour, save that it seemed a little to dilute it: only in some early primroses it destroyed the greatest part of the colour, and made the paper almost white again. And madder also afforded something peculiar, and very differing from what we have newly mentioned: for having gathered some roots of it, and (whilst they were recent) expressed upon white paper, the yellow juice, an alcalizate solution dropt upon it did not turn it either green or white, but red. And the bruised madder itself being drenched with the like alcalizate solution, exchanged also its yellowness for a redness.

An admonition touching the four preceding EXPERIMENTS.

HAVING thus *(Pyrophilus)* given you divers instances, to countenance the general observation delivered in the twenty-fifth experiment, and divers exceptions whereby it ought to be limited; I must leave the further inquiry into these matters to your own industry. For not remembering at present many of those other trials, long since made to satisfy myself about particulars, and not having now the opportunity to repeat them, I must content myself to have given you the hint, and the ways of prosecuting the search yourself; and only declare to you in general, that, as I have made many trials, unmentioned in this treatise, whose events were agreeable to those mentioned in the twenty-fifth experiment, so (to name now no other instances) what I have tried with acid and sulphureous salts upon the pulp of juniper berries, rubbed upon white paper, inclines me to think, that among that vast multitude, and strange variety of plants that adorn the face of the earth, perhaps many other vegetables may be found, on which such menstruums may not have such operations, as upon the juice of violets, peas-blossoms, *&c.* no nor upon any of those three other sorts of vegetables, that I have taken notice of in the three foregoing experiments: it sufficiently appearing even by these, that the effects of a salt upon the juices of particular vegetables do very much depend upon their particular textures.

EXPERIMENT XXIX.

IT may be of some use towards the discovery of the nature of these changes, which the alimental juice receives in some vegetables, according to the differing degrees of their maturity, and according to the differing kinds of plants of the same denomination, to observe what operation acid, urinous, and alcalizate salts will have upon the juices of the several sorts of the vegetable substances I have been mentioning.

To declare my meaning by an example; I took from the same cluster one blackberry full ripe, and another that had not yet gone beyond a redness; and rubbing a piece of white paper with the former, I observed that the juice adhering to it was of a dark reddish colour, full of little black specks, and that this juice, by a drop of a strong lixivium, was immediately turned into a greenish colour deep enough; by as much urinous spirit, into a colour much of kin to the former, though somewhat differing, and fainter; and by a drop of spirit of salt, into a fine and lightsome red: whereas the red berry being in like manner rubbed upon paper, left on it a red colour, which was very little altered by the acid spirit newly named, and by the urinous and lixiviate salts received changes of colour, differing from those that had been just before produced in the dark juice of the ripe black berry.

I REMEMBER also, that though the infusion of damask roses would as well, though not so much, as that of red, be heightened by acid spirits to an intense degree of redness,

nefs, and by lixiviate falts be brought to a darkifh green; yet having for trial's fake taken a rofe, whofe leaves, which were large and numerous, like thofe of a *Provence* rofe, were perfectly yellow, though in a folution of falt of tartar, they afforded a green blueifh tincture, yet I did not by an acid liquor obtain a red one; all that the faline fpirit I employed performed, being (if I much mif-remember not) to dilute fomewhat the yellownefs of the leaves. I would alfo have tried the tincture of yellow violets, but could procure none. And if I were in thofe iflands of *Banda*, which are made famous as well as rich, by being the almoft only place where cloves will profper, I fhould think it worth my curiofity to try, what operation the three differing kinds of falts, I have fo often mentioned, would have upon the juice of this fpice (expreffed at the feveral feafons of it) as it grows upon the tree. Since good authors inform us (of what is remarkable) that thefe, whether fruits, or rudiments of fruits, are at firft white, afterward green, and then reddifh, before they be beaten off the tree; after which being dried before they are put up, they grow blackifh, as we fee them. And one of the recenteft Herbarifts informs us, that the flower grows upon the top of the clove itfelf, confifting of four fmall leaves, like a cherry-bloffom, but of an excellent blue. But *(Pyrophilus)* to return to our own obfervations, I fhall add, that I the rather chufe to mention to you an example drawn from rofes, becaufe that though I am apt to think, as I elfewhere advertife, that fomething may be guefed at about fome of the qualities of the juices of vegetables, by the refemblance or difparity that we meet with in the changes made of their colours, by the operation of the fame kinds of falts; yet that thofe conjectures fhould be very warily made, may appear, among other things, by the inftance I have chofen to give in rofes. For though (as I formerly told you) the dried leaves, both of the damafk, and of red ones, give a red tincture to water fharpened with acid falts, yet the one fort of leaves is known to have a purgative faculty, and the other are often, and divers ways employed for binding. *See Parkinfon, Th. Farm. Trib. c. cap. 28.*

And I alfo chufe *(Pyrophilus)* to fubjoin this twenty-ninth experiment to thofe that precede it, about the change of the colours of vegetables by falts, for thefe two reafons: the firft, that you may not eafily entertain fufpicions, if in the trials of an experiment of fome of the kinds formerly mentioned, you fhould meet with an event fomewhat differing from what my relations have made you expect. And the fecond, that you may hereby be invited to difcern, that it may not be amifs to take notice of the particular feafons wherein you gather the vegetables which in nicer experiments you make ufe of. For, if I were not hindered both by hafte and fome juftifiable confiderations, I could perhaps add confiderable inftances, to thofe lately delivered, for the making out of this obfervation; but for certain reafons I fhall at prefent fubftitute a remarkable paffage to be met with in that laborious Herbarift Mr. *Parkinfon*, where treating of the virtues of the (already divers times mentioned) buckthorn berries, he fubjoins the following account of feveral pigments that are made of them, not only according to the feveral ways of handling them, but according to the differing feafons of maturity, at which they are gathered. *Of thefe berries* (fays he) *are made three feveral forts of colours as they fhall be gathered, that is, being gathered while they are green, and kept dry, are called fapberries, which being fteeped into fome allom-water, or frefh bruifed into allom-water, they give a reafonable fair yellow colour which painters ufe for their work, and book-binders to colour the edges of books, and leather-dreffers to colour leather; as they ufe alfo to make a green colour, called fap-green, taken from the berries when they are black, being bruifed and put into a brafs or copper kettle or pan, and there fuffered to abide three or four days, or a little heated upon the fire, and fome beaten-*

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allom put unto them, and afterwards pressed forth; the juice or liquor is usually put into great bladders tied with strong thread at the head and hung up until it be dry, which is dissolved in water or wine, but sack (he affirms) *is the best to preserve the colour from starving (as they call it) that is, from decaying, and make it hold fresh the longer. The third colour (whereof none,* says he, *that I can find have made mention but only* Tragus) *is a purplish colour, which is made of the berries suffered to grow upon the bushes until the middle or end of* November, *that they are ready to drop from the trees.*

AND, I remember *(Pyrophilus)* that I tried, with a success that pleased me well enough, to make such a kind of pigment, as the painters call sap-green, by a way not unlike that delivered here by our author, but I cannot now find any thing relating to that matter among my loose papers. And my trials were made so many years ago, that I dare not trust my memory for circumstances, but will rather tell you, that in a noted colour-shop I brought them by questions to confess to me, that they made their sap-green much after the ways by our Botanist here mentioned. And on this occasion I shall add an observation, which though it does not strictly belong to this place, may well enough be mentioned here; namely, that I find by an account given us by the learned *Clusius*, of alaternus, that even the grosser parts of the same plant are some of them one colour, and some another: for speaking of that plant, he tells us, that the Portugals use the bark to dye their nets into a red colour, and with the chips of the wood, which are whitish, they dye a blackish blue.

EXPERIMENT XXX.

AMONG the experiments that tend to shew that the change of colours in bodies may proceed from the varied texture of their parts, and the consequent change of their disposition to reflect or refract the light, that sort of experiments must not be left unmentioned, which is afforded us by chymical digestions. For, if chymists will believe several famous writers about what they call the philosopher's stone, they must acknowledge that the same matter, sealed up hermetically in a philosophical egg, will, by the continuance of digestion, or if they will have it so (for it is not material in our case which of the two it be) of decoction, run through a great variety of differing colours, before it come to that of the noblest elixir; whether that be scarlet, or purple, or whatever other kind of red. But without building any thing on so abstruse and questionable an operation (which yet may be pertinently represented to those that believe the thing) we may observe, that divers bodies digested in carefully closed vessels, will in tract of time change their colour: as I have elsewhere mentioned my having observed even in rectified spirit of hartshorn, and as is evident in the precipitations of amalgams of gold and mercury, without addition, where, by the continuance of a due heat, the silver-coloured amalgam is reduced into a shining red powder. Further instances of this kind you may find here and there in divers places of my other essays. And indeed it has been a thing, that has much contributed to deceive many chymists, that there are more bodies than one, which by digestion will be brought to exhibit that variety and succession of colours, which they imagine to be peculiar to what they call the *true matter of the philosophers*. But concerning this, I shall refer you to what you may elsewhere find in the discourse written touching the passive deceptions of chymists, and more about the production of colours by digestion you will meet with presently. Wherefore I shall now make only this observation from what has been delivered, that in these operations there appears not any cause to attribute the new colours emergent to the action of a new substantial form, nor to

any increaſe or decrement of either the ſalt, ſulphur, or mercury of the matter that acquires new colours: for the veſſels are cloſed, and theſe principles, according to the chymiſts, are ingenerable and incorruptible; ſo that the effect ſeems to proceed from hence, that the heat agitating and ſhuffling the corpuſcles of the body expoſed to it, does in proceſs of time ſo change its texture, as that the tranſpoſed parts do modify the incident light otherwiſe than they did when the matter appeared of another colour.

EXPERIMENT XXXI.

AMONG the ſeveral changes of colour, which bodies acquire or diſcloſe by digeſtion, it is very remarkable, that chymiſts find a redneſs rather than any other colour in moſt of the tinctures they draw, and even in the more groſs ſolutions they make of almoſt all concretes, that abound either with mineral or vegetable ſulphur, though the menſtruum employed about theſe ſolutions or tinctures be ever ſo limpid or colourleſs.

THIS we have obſerved in I know not how many tinctures drawn with ſpirit of wine from jalap, guaiacum, and ſeveral other vegetables; and not only in the ſolutions of amber, benzoin, and divers other concretes made with the ſame menſtruum, but alſo in divers mineral tinctures. And, not to urge that familiar inſtance of the ruby of ſulphur, as chymiſts upon the ſcore of its colour call the ſolution of flowers of brimſtone, made with the ſpirit of turpentine, nor to take notice of other more known examples of the aptneſs of chymical oils to produce a red colour with the ſulphur they extract, or diſſolve; not to inſiſt (I ſay) upon inſtances of this nature, I ſhall further repreſent to you, as a thing remarkable, that both acid and alcalizate ſalts, though in moſt other caſes of ſuch contrary operations, in reference to colours, will, with many bodies that abound with ſulphureous, or with oily parts, produce a red; as is manifeſt partly in the more vulgar inſtances of the tinctures, or ſolutions of ſulphur made with lixiviums, either of calcinated tartar or pot-aſhes, and other obvious examples, partly by this, that the true glaſs of antimony extracted with ſome acid ſpirits, with or without wine, will yield a red tincture, and that I know an acid liquor, which in a moment will turn oil of turpentine into a deep red. But among the many inſtances I could give you of the eaſy production of redneſs by the operation of ſaline ſpirit, as well as of ſpirit of wine; I remember two or three of thoſe I have tried, which ſeem remarkable enough to deſerve to be mentioned to you apart.

EXPERIMENT XXXII.

BUT before we ſet them down, it will not perhaps appear impertinent to premiſe,

THAT there ſeems to be a manifeſt diſparity betwixt red liquors, ſo that ſome of them may be ſaid to have a genuine redneſs in compariſon of others, that have a yellowiſh redneſs: for if you take (for example) a good tincture of cochineal, dilute it ever ſo much with fair water, you will not (as far as I can judge by what I have tried) be able to make it a yellow liquor. Inſomuch that a ſingle drop of a rich ſolution of cochineal in ſpirit of urine, being diluted with above an ounce of fair water, exhibited no yellowiſhneſs at all, but a fair (though ſomewhat faint) pink or carnation; and even when cochineal was by degrees diluted much beyond the newly mentioned

 colour,

colour, by the way formerly related to you in the twenty fourth experiment, I remember not, that there appeared in the whole trial any yellow. But if you take balſam of ſulphur (for inſtance) though it may appear in a glaſs, where it has a good thickneſs, to be of a deep red; yet if you ſhake the glaſs, or pour a few drops on a ſheet of white paper, ſpreading them on it with your finger, the balſam that falls back along the ſides of the glaſs, and that which ſtains the paper, will appear yellow, not red. And there are divers tinctures, ſuch as that of amber made with ſpirit of wine (to name now no more) that will appear either yellow or red, according as the veſſels that they fill are ſlender or broad.

EXPERIMENT XXXIII.

BUT to proceed to the experiments I was about to deliver: firſt, oil or ſpirit of turpentine, though clear as fair water, being digeſted upon the purely white ſugar of lead, has, in a ſhort time, afforded us a high red tincture, that ſome artiſts are pleaſed to call the balſam of *Saturn*, which they very much (and probably not altogether without cauſe) extol as an excellent medicine in divers outward affections.

EXPERIMENT XXXIV.

NEXT, take of common brimſtone finely powdered five ounces, of ſal-armoniac likewiſe pulverized an equal weight, of beaten quicklime ſix ounces, mix theſe powders exquiſitely, and diſtil them through a retort placed in ſand by degrees of fire, giving at length as intenſe a heat as you well can in ſand; there will come over (if you have wrought well) a volatile tincture of ſulphur, which may probably prove an excellent medicine, and ſhould have been mentioned among the other preparations of ſulphur, which we have elſewhere imparted to you, but that it is very pertinent to our preſent ſubject, the change of colours. For though none of the ingredients be red, the diſtilled liquor will be ſo: and this liquor, if it be well drawn, will, upon a little agitation of the phial firſt unſtopped (eſpecially if it be held in a warmer hand) ſend forth a copious fume, not red, like that of nitre, but white; and ſometimes this liquor may be ſo drawn, that I remember, not long ſince, I took pleaſure to obſerve in a parcel of it, that ingredients not red, did not only yield by diſtillation a volatile ſpirit that was red, but though that liquor did upon the bare opening of the bottle it was kept in, drive us away with the plenty and ſulphureous ſcent of a white ſteam which it ſent forth, yet the liquor itſelf being touched by our fingers, did immediately dye them black.

EXPERIMENT XXXV.

THE third and laſt experiment I ſhall now mention, to ſhew how prone bodies abounding in ſulphureous parts are to afford a red colour, is one, wherein by the operation of a ſaline ſpirit upon a white or whitiſh body, which according to the chymiſts ſhould be altogether ſulphureous, a redneſs may be produced, not (as in the former experiments) ſlowly, but in the twinkling of an eye. We took then of the eſſential oil of aniſeeds, which has this peculiarity, that in cold weather it loſes its fluidity and the greateſt part of its tranſparency, and looks like a white or whitiſh ointment, and near at hand ſeems to conſiſt of a multitude of little ſoft ſcales: of this

this coagulated ſtuff was ſpread a little with a knife upon a piece of white paper, and letting fall on it, and mixing with it, a drop or two of oil of vitriol, immediately (as we foreſaw) there emerged together with ſome heat and ſmoke, a blood-red colour which therefore was in a trice produced by two bodies, whereof the one had but a whitiſh colour, and the other (if carefully rectified) had no colour at all.

EXPERIMENT XXXVI.

BUT on this occaſion (*Pyrophilus*) we muſt add once for all, that in many of the above recited experiments, though the changes of colour happened as we have mentioned them; yet the emergent or produced colour is oft-times very ſubject to degenerate, both quickly and much. Notwithſtanding which, ſince the changes, we have ſet down, do happen preſently upon the operation of the bodies upon each other, or at the times by us ſpecified, that is ſufficient both to juſtify our veracity, and to ſhew what we intend; it not being eſſential to the genuineneſs of a colour to be durable. For a fading leaf, that is ready to rot, and moulder into duſt, may have as true a yellow, as a wedge of gold, which ſo obſtinately reſiſts both time and fire. And the reaſon why I take occaſion from the former experiment to ſubjoin this general advertiſement, is, that I have ſeveral times obſerved, that the mixture reſulting from the oils of vitriol, and of aniſeeds, though it acquire a thicker conſiſtence than either of the ingredients had, has quickly loſt its colour, turning in a very ſhort time into a dirty grey, at leaſt in the ſuperficial parts, where it is expoſed to the air: which laſt circumſtance I therefore mention, becauſe that, though it ſeem probable, that this degeneration of colours may oft-times and in divers caſes proceed from the further action of the ſaline corpuſcles, and the other ingredients upon one another, yet in many caſes much of the quick change of colours ſeems aſcribeable to the air, as may be made probable by ſeveral reaſons; the firſt whereof may be fetched from the newly recited example of the two oils; the next may be, that we have ſometimes obſerved long window-curtains of light colours to have that part of them, which was expoſed to the air, when the window was open of one colour, and the lower part, that was ſheltered from the air by the wall, of another colour: and the third argument may be fetched from divers obſervations, both of others, and our own; for of that pigment ſo well known in painters ſhops, by the name of Turnſol, our induſtrious *Parkinſon*, in the particular account he gives of the plant that bears it, tells us alſo, That *the berries, when they are at their full maturity, have within them between the outer ſkin and the inward kernel or ſeed, a certain juice or moiſture, which being rubbed upon paper or cloth, at the firſt appears of a freſh and lovely green colour, but preſently changeth into a kind of bluiſh purple, upon the cloth or paper; and the ſame cloth afterwards wet in water, and wrung forth, will colour the water into a claret-wine colour; and theſe* (concludes he) *are thoſe rags of cloth, which are uſually called* turnſol *in the druggiſts or grocers ſhops.* And to this obſervation of our Botaniſt we will add an experiment of our own (made before we met with that) which, though in many circumſtances very differing, ſerves to prove the ſame thing. For having taken of the deeply red juice of buckthorn berries, which I bought of the man that uſes to ſell it to the apothecaries, to make their ſyrup *de ſpina cervina*, I let ſome of it drop upon a piece of white paper, and having left it there for many hours, till the paper was grown dry again, I found what I was inclined to ſuſpect, namely, that this juice was degenerated from a deep red to a dirty kind of greyiſh colour, which, in a great part of the ſtained paper, ſeemed not to have ſo much as an eye of red: though a little

Parkinſon, *Thea. Bot. Trib.* cap. 4. 12.

ſpirit of ſalt or diſſolved alcali would turn this unpleaſant colour (as formerly I told you it would change the not yet altered juice) into a red or green. And to ſatisfy myſelf, that this degeneration of colour did not proceed from the paper, I dropped ſome of the deep red or crimſon juice upon a white glazed tile, and ſuffering it to dry on there, I found that even in that body, on which it could not ſoak, and by which it could not be wrought, it nevertheleſs loſt its colour. And theſe inſtances *(Pyrophilus)* I am the more careful to mention to you, that you may not be much ſurpriſed or diſcouraged, if you ſhould ſometimes miſs of performing punctually what I affirm myſelf to have done in point of changing colours; ſince in theſe experiments the over-ſight or neglect of ſuch little circumſtances, as in many others would not be perhaps conſiderable, may occaſion the miſcarrying of a trial. And I was willing alſo to take this occaſion of advertiſing you in the repeating of the experiments mentioned in the treatiſe, to make uſe of the juices of vegetables, and other things prepared for your trials, as ſoon as ever they are ready, leſt one or other of them grow leſs fit, if not quite unfit by delay; and to eſtimate the event of trials by the change, that is produced preſently upon the due and ſufficient application of actives to paſſives (as they ſpeak) becauſe in many caſes the effects of ſuch mixtures may not be laſting, and the newly produced colour may in a little time degenerate. But *(Pyrophilus)* I forgot to add to the former obſervations lately made about vegetables, a third of the ſame import, made in mineral ſubſtances, by telling you, that the better to ſatisfy a friend or two in this particular, I ſometimes made, according to ſome conjectures of mine, this experiment; that having diſſolved good ſilver in aqua fortis, and precipitated it with ſpirit of ſalt, upon the firſt decanting of the liquor, the remaining matter would be purely white; but after it had lain a while uncovered, that part of it that was contiguous to the air, would not only loſe its whiteneſs, but appear of a very dark and almoſt blackiſh colour; I ſay, that part that was contiguous to the air, becauſe if that were gently taken off, the ſubjacent part of the ſame maſs would appear very white, till that alſo, having continued a while expoſed to the air, would likewiſe degenerate. Now whether the air performs theſe things by the means of a ſubtile ſalt, which we elſewhere ſhew it not to be deſtitute of, or by a piercing moiſture, that is apt eaſily to inſinuate itſelf into the pores of ſome bodies, and thereby change their texture, and ſo their colour; or by ſoliciting the avolation of certain parts of the bodies, to which it is contiguous; or by ſome other way (which poſſibly I may elſewhere propoſe and conſider) I have not now the leiſure to diſcourſe. And for the ſame reaſon, though I could add many other inſtances, of what I formerly noted touching the emergency of redneſs upon the digeſtion of many bodies, inſomuch that I have often ſeen upon the borders of *France* (and probably we may have the like in *England)* a ſort of pears, which digeſted for ſome time with a little wine, in a veſſel exactly cloſed, will in not many hours appear throughout of a deep red colour (as alſo that of the juice, wherein they are ſtewed, becomes) but even on pure and white ſalt of tartar, pure ſpirit of wine, as clear as rock-water, will (as we elſewhere declare) by long digeſtion acquire a redneſs: though I ſay ſuch inſtances might be multiplied, and though there be ſome other obvious changes of colours, which happen ſo frequently, that they cannot but be as well conſiderable as notorious; ſuch as is the blacknes of almoſt all bodies burned in the open air; yet our haſte invites us to reſign you the exerciſe of inquiring into the cauſes of theſe changes. And certainly, the reaſon both why the ſoots of ſuch differing bodies are almoſt all of them all black, why ſo much the greater part of vegetables ſhould be rather green than of any other colour, and particularly (which more directly concerns the place) why gentle-

gentle heats do so frequently in chymical operations produce rather a redness than any other colour in digested menstruums, not only sulphureous, as spirit of wine, but saline, as spirit of vinegar, may be very well worth a serious enquiry; which I shall therefore recommend to *Pyrophilus* and his ingenious friends.

EXPERIMENT XXXVII.

IT may seem somewhat strange, that if you take the crimson solution of cochineal, or the juice of black cherries, and of some other vegetables that afford the like colour (which because many take but for a deep red, we do with them sometimes call it so) and let some of it fall upon a piece of paper, a drop or two of an acid spirit, such as spirit of salt, or aqua fortis, will immediately turn it into a fair red. Whereas, if you make an infusion of brazil in fair water, and drop a little spirit of salt or aqua fortis into it, that will destroy its redness, and leave the liquor of a yellow (sometimes pale) I might perhaps plausibly enough say on this occasion, that if we consider the case a little more attentively, we may take notice, that the action of the acid spirit seems in both cases but to weaken the colour of the liquor on which it falls. And so though it destroy redness in the tincture of brazil, as well as produce red in the tincture of cochineal, its operations may be uniform enough, since as crimson seems to be little else than a very deep red, with (perhaps) an eye of blue, so some kinds of red seem (as I have lately noted) to be little else than heightened yellow. And consequently in such bodies, the yellow seems to be but a diluted red. And accordingly alcalizate solutions and urinous spirits, which seem disposed to deepen the colours of the juices and liquors of most vegetables, will not only restore the solution of cochineal and the infusion of brazil to crimson, whence the spirit of salt had changed them into a truer red; but will also (as I lately told you) not only heighten the yellow juice of madder into red, but advance the red infusion of brazil to a crimson. But I know not whether it will not be much safer to derive these changes from varied textures, than certain kinds of bodies; and you will perhaps think it worth while, that I should add on this occasion, that it may deserve some speculation, why, notwithstanding what we have been observing, though blue and purple seem to be deeper colours than red, and therefore the juices of plants of either of the two former colours may (congruously enough to what has been just now noted) be turned red by spirit of salt or aqua fortis, yet blue syrup of violets and some purples should both by oil of tartar and spirit of urine be changed into green, which seems to be not a deeper, but a more diluted colour than blue, if not also than purple.

EXPERIMENT XXXVIII.

IT would much contribute to the history of colours, if chymists would in their laboratories take a heedful notice, and give us a faithful account of the colours observed in the steams of bodies either sublimed or distilled, and of the colours of those productions of the fire, that are made up by the coalition of those steams. As (for instance) we observe in the distilling of pure salt-petre, that at a certain season of the operation, the body, though it seem either crystalline, or white, affords very red fumes: whereas though vitriol be green or blue, the spirit of it is observed to come over in whitish fumes. The like colour I have taken notice of in the fumes of several other concretes of differing colours, and natures, especially when distilled with strong fires. And we elsewhere note, that even soot, as black as it is, has filled our receivers

ceivers with ſuch copious white fumes, that they ſeemed to have had their inſides waſhed with milk. And no leſs obſervable may be the diſtilled liquors, into which ſuch fumes convene (for though we will not deny, that by ſkill and care a reddiſh liquor may be obtained from nitre) yet the common ſpirit of it, in the making even of which, ſtore of theſe red fumes are wont to paſs over into the receiver, appears not to be at all red. And beſides, that neither the ſpirit of vitriol, nor that of ſoot is any thing white; and, beſides alſo, that as far as I have obſerved, moſt (for I ſay not all) of the empyreumatical oils of wood, and other concretes, are either of a deep red, or of a colour between red and black; beſides this, I ſay, it is very remarkable, that notwithſtanding that great variety of colours to be met with in the herbs, flowers, and other bodies wont to be diſtilled *in balneo*; yet (as far at leaſt as our common diſtillers' experience reacheth) all the waters and ſpirits that firſt come over by that way of diſtillation, leave the colours of their concretes behind them, though indeed there be one or two vegetables not commonly taken notice of, whoſe diſtilled liquors I elſewhere obſerve to carry over the tincture of the concrete with them. And as in diſtillations, ſo in ſublimations, it were worth while to take notice of what comes up, in reference to our preſent ſcope, but purpoſely performing them (as I have in ſome caſes done) in conveniently ſhaped glaſſes, that the colour of the aſcending fumes may be diſcerned; for it may afford a Naturaliſt good information to obſerve the congruities or the differences betwixt the colours of the aſcending fumes, and thoſe of the flowers they compoſe by their convention. For it is evident, that theſe flowers do many of them, in point of colour, much differ, not only from one another, but oftimes from the concretes that afforded them. Thus (not here to repeat what I formerly noted of the black ſoots of very differingly coloured bodies) though camphire and brimſtone afford flowers much of their own colour, ſave that thoſe of brimſtone are wont to be a little paler, than the lumps that yielded them; yet even of red benzoin, that ſublimed ſubſtance, which chymiſts call its flowers, is wont to be white or whitiſh. And to omit other inſtances, even one and the ſame black mineral, antimony, may be made to afford flowers, ſome of them red, and ſome grey, and, which is more ſtrange, ſome of them purely white. And it is the preſcription of ſome glaſs-men by exquiſitely mingling a convenient proportion of brimſtone, ſal armoniac, and quickſilver, and ſubliming them together, to make a ſublimate of an excellent blue; and though having cauſed the experiment to be made, we found the produced ſublimate to be far from being of a lovely colour (as was promiſed) that here and there it ſeemed blueiſh, and at leaſt was of a colour differing enough from either of the ingredients, which is ſufficient for our preſent purpoſe. But a much finer colour is promiſed by ſome of the empirics, that pretend to ſecrets, who tell us, that orpiment being ſublimed, will afford among the parts of it that fly upward, ſome little maſſes, which, though the mineral itſelf be of a good yellow, will be red enough to emulate rubies, both in colour and tranſlucency. And this experiment may, for aught I know, ſometimes ſucceed; for I remember, that having in a ſmall bolt-head purpoſely ſublimed ſome powdered orpiment, we could in the lower part of the ſublimate diſcern here and there ſome reddiſh lines, though much of the upper part of the ſublimate conſiſted of a matter, which was not alone purely yellow, but tranſparent almoſt like a powder. And we have alſo this way obtained a ſublimate, the lower part whereof though it conſiſted not of rubies, yet the ſmall pieces of it, which were numerous enough, were of a pleaſant reddiſh colour, and gliſtered very prettily. But to inſiſt on ſuch kind of trials and obſervations (where the aſcending fumes of bodies differ in colour from the bodies themſelves) though it might indeed enrich the

history of colours, would rob me of too much of the little time I have to dispatch what I have further to tell you concerning them.

EXPERIMENT XXXIX.

TAKE the dried buds (or blossoms) of the pomegranate tree (which are commonly called in the shops *Balaustiums*;) pull off the reddish leaves, and by a gentle ebullition of them in fair water, or by a competent infusion of them in like water well heated, extract a faint reddish tincture; which, if the liquor be turbid, you may clarify it by filtrating it. Into this, if you pour a little good spirit of urine, or some other spirit abounding in the like sort of volatile salts, the mixture will presently turn of a darkish green colour; but if instead of the forementioned liquor, you drop into the simple infusion a little rectified spirit of sea-salt, the pale and almost colourless liquor will immediately not only grow more transparent, but acquire a high redness, like that of rich claret wine; which so suddenly acquired colour may as quickly be destroyed, and turned into a dirty blueish green, by the affusion of a competent quantity of the abovementioned spirit of urine.

ANNOTATION.

THIS experiment may bring some light to, and receive some from a couple of other experiments, that I remember I have met with in the ingenious *Gassendus*'s animadversions upon *Epicurus*'s philosophy, whilst I was turning over the leaves of those learned commentaries, (my eyes being too weak to let me read such voluminous books quite through) and I the less scruple (nowithstanding my contrary custom in this treatise) to set down these experiments of another, because I shall a little improve the latter of them, and because by comparing therewith that which I have last cited, we may be assisted to conjecture upon what account it is, that oil of vitriol heightens the tincture of red-rose leaves, since spirit of salt, which is a highly acid menstruum, but otherwise differing enough from oil of vitriol, does the same thing. Our author's experiments then, as we made them, are these: We took about a glass-full of lukewarm water, and in it immerged a quantity of the leaves of senna, and presently upon the immersion there did not appear any redness in the water, but dropping into it a little oil of tartar, the liquor soon discovered a redness to the watchful eye; whereas by a little of that acid liquor of vitriol, which is like the former undeservedly called oil, such a colour would not be extracted from the infused senna. On the other side, we took some red-rose leaves dried, and having shaken them into a glass of fair water, they imparted to it no redness, but upon the affusion of a little oil of vitriol the water was immediately turned red, which it would not have been, if instead of oil of vitriol, we had employed oil of tartar to produce that colour. That these were *Gassendus* his experiments, I partly remember, and was assured by a friend, who lately transcribed them out of *Gassendus* his book, which, I therefore add, because I have not now that book at hand. And the design of *Gassendus* in these experiments our friend affirms to be, to prove, that of things not red a redness may be made only by mixture, and the varied position of parts, wherein the doctrine of that subtile philosopher doth not a little authorize what we have formerly delivered concerning the emergency and change of colours. But the instances, that we have out of him set down, seem not to be the most eminent, that may be produced of this truth: for our next experiment will shew the production of several colours;

lours out of liquors, which have not any of them any ſuch colour, nor indeed any diſcernable one at all. And whereas though our author tells us, that there was no redneſs either in the water, or the leaves of ſenna, or the oil of tartar; and though it be true, that the predominant colour of the leaves of ſenna be another than red, yet we have tried, that by ſteeping that plant a night even in cold water, it would afford a very deep yellow or reddiſh tincture without the help of the oil of tartar, which ſeems to do little more than aſſiſt the water to extract more nimbly a plenty of that red tincture, wherewith the leaves of ſenna do of themſelves abound, and having taken off the tincture of ſenna, made only with fair water, before it grew to be reddiſh, and decanted it from the leaves, we could not perceive, that by dropping ſome oil of tartar into it, that colour was conſiderable, though it were a little heightened into a redneſs, which might have been expected, if the particles of the oil did eminently co-operate, otherwiſe than we have expreſſed, to the production of this redneſs.

AND as for the experiment with red-roſe leaves, the ſame thing may be alledged; for we found that ſuch leaves, by bare infuſion for a night and day in fair water, did afford us a tincture bordering at leaſt upon redneſs; and that colour being conſpicuous in the leaves themſelves, would not by ſome ſeem ſo much to be produced as to be extracted by the affuſion of oil of vitriol. And the experiment tried with the dry leaves of damaſk roſes ſucceeded but imperfectly, but that is indeed obſervable to our author's pupoſe, that oil of tartar will not perform in this experiment what oil of vitriol doth: but becauſe this laſt named liquor is not ſo eaſily to be had, give me leave to advertiſe you, that the experiment will ſucceed, if inſtead of it you employ aqua fortis. And though ſome trials of our own formerly made, and others eaſily deducible from what we have already delivered, about the different families and operations of ſalt, might enable us to preſent you an experiment upon red-roſe leaves, more accommodated to our author's purpoſe than that which he hath given us; yet our reverence to ſo candid a philoſopher, invites us rather to improve his experiment, than ſubſtitute another in its place. Take therefore of the tincture of red-roſe leaves, (for with damaſk-roſe leaves the experiment ſucceedeth not well) made as before hath been taught with a little oil of vitriol, and a good quantity of fair water; pour off this liquor into a clear phial half filled with limpid water, till the water held againſt the light have acquired a competent redneſs, without loſing its tranſparency; into this tincture drop leiſurely a little good ſpirit of urine, and ſhaking the phial, which you muſt ſtill hold againſt the light, you ſhall ſee the red liquor immediately turned into a fine greeniſh blue, which colour was not to be found in any of the bodies, upon whoſe mixture it emerged: and this change is the more obſervable, becauſe in many bodies the degenerating of blue into red is uſual enough, but the turning of red into blue is very unfrequent. If at every drop of ſpirit of urine you ſhake the vial containing the red tincture, you may delightfully obſerve a pretty variety of colours in the paſſage of that tincture from a red to a blue, and ſometimes we have this way hit upon ſuch a liquor, as being looked upon againſt and from the light, did ſeem faintly to emulate the abovementioned tincture of *Lignum Nephriticum*. And if you make the tincture of red-roſes very high, and, without diluting it with fair water, pour on the ſpirit of urine, you may have a blue ſo deep as to make the liquor opacous; but being dropt upon white paper, the colour will ſoon diſcloſe itſelf. Alſo having made the red, and conſequently the blue tincture very tranſparent, and ſuffered it to reſt in a ſmall open phial for a day or two, we found, according to our conjecture, that not only the blue but the red colour

lour also vanished; the clear liquor being of a bright amber colour, at the bottom of which subsided a light, but copious feculency of almost the same colour, which seems to be nothing but the tincted parts of the rose-leaves drawn out by the acid spirits of the oil of vitriol, and precipitated by the volatile salt of the spirit of urine: which makes it the more probable, that the redness drawn by the oil of vitriol, was at least as well an extraction of the tinging parts of the roses, as a production of redness. And lastly, if you be destitute of spirit of urine, you may change the colour of the tincture of roses with many other sulphureous salts, as a strong solution of pot-ashes, oil of tartar, *&c.* which yet are seldom so free from feculency, as the spirituous parts of urine become by repeated distillation.

ANNOTATION.

ON this occasion, I call to mind, that I found a way of producing, though not the same kind of blue, as I have been mentioning, yet a colour near of kin to it, namely, a fair purple, by employing a liquor not made red by art, instead of the tincture of red-roses made with an acid spirit: and my way was, only to take log-wood (a wood very well known to dyers) having by infusing the powder of it a while in fair water made that liquor red, I dropt into it a tantillum of an urinous spirit, as that of sal armoniac (and I have done the same thing with an alcali) by which the colour was in a moment turned into a rich, and lovely purple. But care must be had, that you let not fall into a spoonful above two or three drops, lest the colour become so deep, as to make the liquor too opacous. And (to answer the other part of *Gassendus* his experiment) if, instead of fair water, I infused the logwood in water made somewhat sour by the acid spirit of salt, I should obtain neither a purple liquor, nor a red, but only a yellow one.

EXPERIMENT XL.

THE experiment I am now to mention to you, *Pyrophilus*, is that which both you, and all the other Virtuosi that have seen it, have been pleased to think very strange; and indeed of all the experiments of colours I have yet met with, it seems to be the fittest to recommend the doctrine proposed in this treatise, and to shew that we need not suppose, that all colours must necessarily be inherent qualities, flowing from the substantial forms of the bodies they are said to belong to, since by a bare mechanical change of texture in the minute parts of bodies, two colours may in a moment be generated quite *de novo*, and utterly destroyed. For there is this difference betwixt the following experiment, and most of the others delivered in these papers, that in this, the colour that the body already had, is not changed into another, but betwixt two bodies, each of them apart devoid of colour, there is in a moment generated a very deep colour, and which, if it were let alone, would be permanent; and yet by a very small parcel of a third body, that has no colour of its own (lest some may pretend, I know not what antipathy betwixt colours) this otherwise permanent colour will be in another trice so quite destroyed, that there will remain no footsteps either of it or of any other colour in the whole mixture.

THE experiment is very easy, and it is thus performed: take good common sublimate, and fully satiate with it what quantity of water you please, filtre the solution carefully through clean and close paper, that it may drop down as clear and colourless as fountain water. Then, when you'll shew the experiment, put of it about a spoonful

ſpoonful into a ſmall wine-glaſs, or any other convenient veſſel made of clear glaſs, and dropping in three or four drops of good oil of tartar *per deliquium*, well filtred, that it may likewiſe be without colour: theſe two limpid liquors will in the twinkling of an eye turn into an opacous mixture of a deep orange colour, which by keeping the glaſs continually ſhaking in your hand, you muſt preſerve from ſettling too ſoon to the bottom; and when the ſpectators have a little beheld this firſt change, then you muſt preſently drop in about four or five drops of the oil of vitriol, and continuing to ſhake the glaſs pretty ſtrongly, that it may the nimbler diffuſe itſelf, the whole colour, if you have gone ſkilfully to work, will immediately diſappear, and all the liquor in the glaſs will be clear and colourleſs as before, without ſo much as a ſediment at the bottom. But for the more graceful trial of this experiment, it will not be amiſs to obſerve, firſt, that there ſhould not be taken too much of the ſolution of ſublimate, nor too much of the oil of tartar dropped in, to avoid the neceſſity of putting in ſo much oil of vitriol as may make an ebullition, and perhaps run over the glaſs. Secondly, that it is convenient to keep the glaſs always a little ſhaking, both for the better mixing of the liquors, and to keep the yellow ſubſtance from ſubſiding, which elſe it would in a ſhort time do; though when it is ſubſided it will retain its colour, and alſo be capable of being deprived of it by the oil newly mentioned. Thirdly, that if any yellow matter ſtick at the ſides of the glaſs, it is but inclining the glaſs, till the clarified liquor can waſh along it, and the liquor will preſently imbibe it, and deprive it of its colour.

MANY have ſometimes wondered, how I came to light upon this experiment; but the notions or conjectures I have about the differing natures of the ſeveral tribes of ſalts, having led me to deviſe the experiment, it will not be difficult for me to give you the chymical reaſon, if I may ſo ſpeak, of the phænomenon. Having then obſerved, that mercury, being diſſolved in ſome menſtruums, would yield a dark yellow precipitate, and ſuppoſing that, as to this, common water, and the ſalts that ſtick to the mercury would be equivalent to thoſe acid menſtruums, which work upon the quickſilver, upon the account of their ſaline particles, I ſubſtituted a ſolution of ſublimate in fair water, inſtead of a ſolution of mercury in aqua fortis, or ſpirit of nitre, that ſimple ſolution being both clearer and free from that very offenſive ſmell, which accompanies the ſolutions of mercury made with thoſe other corroſive liquors. Then I conſidered, that that which makes the yellow colour, is indeed but a precipitate made by the means of the oil of tartar, which we drop in, and which, as the chymiſts know, does generally precipitate metalline bodies corroded by acid ſalts: ſo that the colour in our caſe reſults from the coalition of the mercurial particles with the ſaline ones, wherewith they were formerly aſſociated, and with the alcalizate particles of the ſalt of tartar that ſwim up and down in the oil. Wherefore conſidering alſo, that very many of the effects of lixiviate liquors, upon the ſolutions of other bodies, may be deſtroyed by acid menſtruums, as I elſewhere more particularly declare, I concluded, that if I choſe a very potently acid liquor, which by its inciſive power might undo the work of the oil of tartar, and diſperſe again thoſe particles, which the other had by precipitation aſſociated, into ſuch minute corpuſcles as were before ſingly inconſpicuous, they would become inconſpicuous again, and conſequently leave the liquor as colourleſs as before the precipitation was made.

THIS, as I ſaid, *Pyrophilus*, ſeems to be the chymical reaſon of this experiment; that is, ſuch a reaſon, as, ſuppoſing the truth of thoſe chymical notions I have elſewhere I hope evinced, may give ſuch an account of the phænomena as chymical notions can ſupply us with: but I both here and elſewhere make uſe of this way of ſpeaking

ſpeaking, to intimate that I am ſufficiently aware of the difference betwixt a chymical explication of a phænomenon, and one that is truly philoſophical or mechanical; as in our preſent caſe, I tell you ſomething, when I tell you that the yellowneſs of the mercurial ſolution, and the oil of tartar, is produced by the precipitation occaſioned by the affuſion of the latter of thoſe liquors, and that the deſtruction of the colour proceeds from the diſſipation of that curdled matter, whoſe texture is deſtroyed, and which is diſſolved into minute and inviſible particles by the potently acid menſtruum: which is the reaſon, why there remains no ſediment in the bottom, becauſe the infuſed oil takes it up, and reſolves it into hidden or inviſible parts, as water does ſalt or ſugar. But when I have told you all this, I am far from thinking I have told all that ſuch an inquiſitive perſon as yourſelf would know: for I preſume you would deſire, as well as I, to learn (at leaſt) why the particles of the mercury, of the tartar, and of the acid ſalts convening together, ſhould make rather an orange colour than a red, or a blue, or a green. For it is not enough to ſay what I related a little before, that divers mercurial ſolutions, though otherwiſe made, would yield a yellow precipitate, becauſe the queſtion will recur concerning them; and to give it a ſatisfactory anſwer, is, I freely acknowledge, more than I dare as yet pretend to.

But to confirm my conjecture about the chymical reaſon of our experiment, I may add, that as I have (*viz.* pag. 11. of this treatiſe) elſewhere (on another occaſion) told you, with ſaline liquors of another kind and nature than ſalt of tartar (namely, with ſpirit of urine, and liquors of kin to that) I can make the mercury precipitate out of the firſt ſimple ſolution quite of another colour than that hitherto mentioned; nay, if inſtead of altering the precipitating liquor, I altered the texture of the ſublimate in ſuch a way as my notions about ſalt required, I could produce the ſame phænomenon. For having purpoſely ſublimed together equal parts (or thereabout) of ſal armoniac and ſublimate, firſt diligently mixed, the aſcending flowers being diſſolved in fair water, and filtred, gave a ſolution limpid and colourleſs, like that of the other ſublimates, and yet an alcaly dropped into this liquor did not turn it yellow but white. And upon the ſame grounds we may with quickſilver, without the help of common ſublimate, prepare another ſort of flowers diſſoluble in water without diſcolouring it, with which I could likewiſe do what I newly mentioned; to which I ſhall add (what poſſibly you will ſomewhat wonder at) that ſo much does the colour depend upon the texture reſulting from the convention of the ſeveral ſorts of corpuſcles, that though, in our experiment, oil of vitriol deſtroys the yellow colour, yet with quickſilver and fair water, by the help of oil of vitriol alone, we may eaſily make a kind of precipitate of a fair and permanent yellow, as you will ere long (in the forty-ſecond experiment of this third part) be taught. And I may further add, that I choſe oil of vitriol, not ſo much for any other or peculiar quality, as for its being, when it is well rectified (which it is ſomewhat hazardous to bring it to be) not only devoid of colour and ill ſmells, but extremely ſtrong and inciſive. For though common and undephlegmated aqua fortis will not perform the ſame thing well, yet that which is made exceeding ſtrong, by being carefully dephlegmed, will do it pretty well, though not ſo well as oil of vitriol; which is ſo ſtrong, that even without rectification it may for a need be made uſe of. I will not here tell you what I have tried, that I may be able to deprive at pleaſure the precipitate that one of the ſulphureous liquors had made, by the copious affuſion of the other; becauſe I found, though this experiment is too ticklish to let me give a full account of it in few words, I ſhall therefore tell you, that it is not only for once, that the other above-mentioned experiment may be made, the ſame numerical parcels of liquor being ſtill employed in

it. For after I have clarified the orange-coloured liquor, by the addition of as little of the oil of vitriol as will suffice to perform the effect, I can again at pleasure reproduce the opacous colour, by the dropping in of fresh oil of tartar, and destroy it again by the re-affusion of more of the acid menstruum; and yet oftener, if I please, can I with these two contrariant liquors recall and disperse the colour, though by reason of the addition of so much new liquor, in reference to the mercurial particles, the colour will at length appear more dilute and faint.

An Improvement of the fortieth EXPERIMENT.

AND, *Pyrophilus*, to confirm yet further the notions that led me to think on the proposed experiment, I shall acquaint you with another; which, when I had conveniency, I have sometimes added to it, and which has to the spectators appeared little less odd than the first. And though because the liquor, requisite to make the trial succeed well, must be on purpose prepared anew a while before, because it will not long retain its fitness for this work, I do but seldom annex this experiment to the other; yet I shall tell you how I devised it, and how I make it. If you boil crude antimony in a strong and clear lixivium, you shall separate a substance from it, which some modern chymists are pleased to call its sulphur, but how deservedly I shall not here examine, having elsewhere done it in an opportune place; wherefore I shall now but need to take notice, that when this supposed sulphur (not now to call it rather a kind of crocus) is let fall by the liquor upon its refrigeration, it often settles in flakes, or such-like parcels of a yellow substance (which being by the precedent dissolution reduced into minute parts, may peradventure be made to take fire much more easily than the grosser powder of unprepared antimony would have done.) Considering therefore, that common sulphur boiled in a lixivium, may be precipitated out of it by rhenish-wine or white-wine, which are sourish liquors, and have in them, as I elsewhere shew, an acid salt; and having found also by trial, that with other acid liquors I could precipitate out of lixiviate solvents some other mineral concretions abounding with sulphureous parts, of which sort is crude antimony; I concluded it to be easy to precipitate the antimony dissolved, as was lately mentioned, with the acid oil of vitriol. And though common sulphur yields a white precipitate, which the chymists call *lac sulphuris*, yet I supposed the precipitated antimony would be of a deep yellow colour, as well if made with oil of vitriol, as if made only by refrigeration and length of time. From this it was easy to deduce this experiment, that if you put into one glass some of the freshly impregnated and filtrated solution of antimony, and into another some of the orange-coloured mixture (which I formerly shewed you how to make with a mercurial solution and oil of tartar) a few drops of oil of vitriol dropped into the last mentioned glass would, as I told you before, turn the deep yellow mixture into a clear liquor; whereas a little of the same oil dropped out of the same phial into the other glass, would presently (but not without some ill scent) turn the moderately clear solution into a deep yellow substance. But this, as I said, succeeds not well, unless you employ a lixivium that has but newly dissolved antimony, and has not yet let it fall. But yet in summer-time, if your lixivium have been duly impregnated and well filtred after it is quite cold, it will for some days (perhaps much longer than I had occasion to try) retain antimony enough to exhibit, upon the affusion of the corrosive oil, as much of a good yellow substance as is necessary to satisfy the beholders of the possibility of the experiment.

Reflections upon the XLth experiment, compared with the Xth and XXth.

THE knowledge of the distinction of salts which we have proposed, whereby they are discriminated into acid, volatile, or salfuginous (if I may for distinction sake so call the fugitive salts of animal substances) and fixed or alcalizate, may possibly (by that little part which we have already delivered, of what we could say of its applicableness) appear of so much use in natural philosophy (especially in the practic part of it) that I doubt not but it will be no unwelcome corollary of the preceding experiment, if by the help of it I teach you to distinguish which of those salts is predominant in chymical liquors, as well as whether any of them be so or not. For though in our notes upon the tenth and twentieth experiments I have shown you a way, by means of the tincture of *Lignum Nephriticum*, or of syrup of violets, to discover whether a propounded salt be acid or not; yet you can thereby only find in general that such and such salts belong not to the tribe of acids, but cannot determine whether they belong to the tribe of urinous salts (under which, for distinction sake, I comprehend all those volatile salts of animal or other substances that are contrary to acids) or to that of alcalies. For as well the one as the other of these salino-sulphureous salts will restore the ceruleous colour to the tincture of *Lignum Nephriticum*, and turn that of syrup of violets into green. Wherefore this XLth experiment does opportunely supply the deficiency of those. For being solicitous to find out some ready ways of discriminating the tribes of chymical salts, I found that all those I thought fit to make trial of, would, if they were of a lixiviate nature, make with sublimate dissolved in fair water an orange tawny precipitate; whereas, if they were of an urinous nature, the precipitate would be white and milky. So that having always by me some syrup of violets and some solution of sublimate, I can by the help of the first of those liquors discover in a trice, whether the propounded salt or saline body be of an acid nature or no; if it be, I need (you know) inquire no further; but if it be not, I can very easily, and as readily distinguish between the other two kinds of salts, by the white or orange-colour that is immediately produced, by letting fall a few drops or grains of the salt to be examined, into a spoonful of the clear solution of sublimate. For example, it has been supposed by some eminently learned, that when sal armoniac being mingled with an alcali is forced from it by the fire in close vessels, the volatile salt that will thereby be obtained (if the operation be skilfully performed) is but a more fine and subtile sort of sal armoniac, which, it is presumed, this operation does but more exquisitely purify than common solutions, filtrations, and coagulations. But this opinion may be easily shown to be erroneous, as by other arguments, so particularly by the lately delivered method of distinguishing the tribes of salts. For the saline spirit of sal-armoniac, as it is in many other manifest qualities very like the spirit of urine, so like, that it will in a trice make syrup of violets of a lovely green, turn a solution of good verdigrease into an excellent azure, and make the solution of a sublimate yield a white precipitate; insomuch that in most (for I say not all of the experiments) where I aim only at producing a sudden change of colour, I scruple not to use spirit of sal-armoniac when it is at hand, instead of spirit of urine, as indeed it seems chiefly to consist (besides the phlegm that helps to make it fluid) of the volatile urinous salt (yet not excluding that of soot) that abounds in the sal armoniac and is set at liberty from the sea-salt wherewith it was formerly associated, and clogged, by the operation of the alcali, that divides the ingredients of sal armoniac, and retains that sea-salt with itself: what use may be made

made of the like way of exploration in that inquiry which puzzles so many modern Naturalists, whether the rich pigment (which we have often had occasion to mention) belongs to the vegetable or animal kingdom, you may find in another place, where I give you some account of what I tried about cochineal. But I think it needless to exemplify here our method by any other instances, many such being to be met with in divers parts of this treatise; but I will rather advertise you, that by this way of examining chymical liquors, you may not only in most cases conclude affirmatively, but in some cases negatively. As since spirit of wine, and, as far as I have tried, those chymical oils which artists call essential, did not (when I used them as I had used the several families of salts upon that syrup) turn syrup of violets red or green, nor the solution of sublimate white or yellow; I inferred it may thence be probably argued, that either they are destitute of salt, or have such as belongs not to either of the three grand families often already mentioned. When I went to examine the spirit of oak, or of such like concretes forced over through a retort, I found by this means amongst others, that (as I elsewhere show) those chymists are much mistaken in it, that account it a simple liquor, and one of their hypostatical principles. For not to mention what phlegm it may have, I found that with a few drops of one of this sort of spirits mixed with a good proportion of syrup of violets, I could change the colour and make it purplish, by the affinity of which colour to redness, I conjectured that this spirit had some acid corpuscles in it; and accordingly I found, that as it would destroy the blueness of a tincture of *Lignum Nephriticum*, so being put upon corals, it would corrode them, as common spirit of vinegar, and other acid lquors are wont to do. And farther to examine whether there were not a great part of the liquor that was not of an acid nature, having separated the sour or vinegar-like part from the rest, which (if I mistake not) is far the more copious; we concluded, as we had conjectured, the other or remaining part, though it had a strong taste as well as smell, to be of a nature differing from that of either of the three sorts of salts above-mentioned, since it did as little as spirit of wine, and chymical oils, alter the colour either of syrup of violets or solution of sublimate: whence we also inferred, that the change that had been made of that syrup into a purple colour, was effected by the vinegar, that was one of the two ingredients of the liquor, which was wont to pass for a simple or uncompounded spirit. And, upon this account, it was of the spirit of oak (and the like concretes) freed from its vinegar, that I elsewhere told you, that I had not then observed it (and I have repeated the trial but very lately) to destroy the ceruleous tincture of *Lignum Nephriticum*. But this only *en passant*; for the chief thing I had to add was this: that by the same way may be examined and discovered divers changes that are produced in bodies, either by nature only, or by art; either of them being able, by changing the texture of some concretes I could name, to qualify them to operate after a new manner upon the above mentioned syrup, or solution, or both: and by this means, to tell you that, upon the by, I have been able to discover, that there may be made bodies, which though they run *per deliquium*, as readily as salt of tartar, belong in other respects, not to the family of alcalies, much less to that of salfuginous or that of acid salts. Perhaps too, I may know a way of making a highly operative saline body, that shall neither change the colour of syrup of violets nor precipitate the solution of sublimate; and I can likewise, if I please, conceal by what liquors I perform such changes of colour, as I have been mentioning to you, by quite altering the texture of some ordinary chymical productions, the exploration of which is the main use of the fortieth experiment, which I think teaches not a little, if it teach us to discover the nature of those things (in reference to salt) that are ob-

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tained by the ordinary chymical analysis of mixed bodies, though perhaps there may be other bodies prepared by chymistry, which may have the same effects in the change of colours, and yet be produced not from what chymists call the resolution of bodies, but from their composition. But the discoursing of things of this nature is more proper for another place. I shall now only add, what might perhaps have been more seasonably told you before; that the reason why the way of exploration of salts hitherto delivered succeeds in the solution of sublimate, depends upon the particular texture of that solution, as well as upon the differing natures of the saline liquors employed to precipitate it. For gold dissolved in aqua regia, whether you precipitate it with oil of tartar, which is an alcali, or with spirit of urine, or sal armoniac, which belongs to the family of volatile salts, will either way afford a yellow substance: though with such an acid liquor as, I say not spirit of salt, the body that yields it, being upon the matter an ingredient of aqua regis, but oil of vitriol itself, I did not find that I could precipitate the metal out of the solution, or destroy the colour of it; though the same oil of vitriol would readily precipitate silver dissolved in aqua fortis. And if you dissolve pure silver in aqua fortis, and suffer it to shoot into crystals, the clear solution of these made in fair water, will afford a very white precipitate, whether it be made with an alcali, or an acid spirit, as that of salt; whereas, which may seem somewhat strange, with spirit of sal armoniac (that I used was made of quick-lime) I could obtain no such white precipitate: that volatile spirit, nor (as I remember) that of urine, scarce doing any more than striking down a very small quantity of matter, which was neither white nor whitish; so that the remaining liquor being suffered to evaporate till the superfluous moisture was gone, the greatest part of the metalline corpuscles with the saline ones that had imbibed them, concoagulated into salt, as is usual in such solutions, wherein the metal has not been precipitated.

EXPERIMENT XLI.

OF kin to the last or fortieth experiment is another which I remember I have sometimes shewn to Virtuosi, that were pleased not to dislike it. I took spirit of urine made by fermentation, and with a due proportion of copper brought into small parts, I obtained a very lovely azure solution; and when I saw the colour was such as was requisite, pouring into a clean glass about a spoonful of this tincted liquor (of which I used to keep a quantity by me) I could, by shaking into it some drops of strong oil of vitriol, deprive it in a trice of its deep colour, and make it look like common water.

ANNOTATION.

THIS experiment brings into my mind this other, which oftentimes succeeds well enough, though not quite so well as the former; namely, that if into about a small spoonful of a solution of good French verdigrease made in fair water, I dropt and shaked some strong spirit of salt, or rather dephlegmed aqua fortis, the greenness of the solution would be made in a trice almost totally to disappear, and the liquor held against the light would scarce seem other than clear or limpid, to any but an attentive eye: which is therefore remarkable, because we know that aqua fortis corroding copper, which is it that gives the colour to verdigrease, is wont to reduce it to a green blue solution. But if into the other altogether or almost colour-

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less liquor I was speaking of, you drop a just quantity either of oil of tartar or spirit of urine, you shall find that after the ebullition is ceased, the mixture will disclose a lively colour, though somewhat differing from that which the solution of verdigrease had at first.

EXPERIMENT XLII.

THAT the colour *(Pyrophilus)* of a body may be changed by a liquor which of itself is of no colour, provided it be saline, we have already manifested by a multitude of instances. Nor doth it seem so strange, because saline particles swimming up and down in liquors, have been by many observed to be very operative in the production and change of colours. But divers of our friends, that are not acquainted with chymical operations, have thought it very strange that a white body, and a dry one too, should immediately acquire a rich new colour upon the bare affusion of spring-water destitute as well of adventitious salt as of tincture. And yet *(Pyrophilus)* the way of producing such a change of colours may be easily enough lighted on, by those that are conversant in the solutions of mercury. For we have tried, that though by evaporating a solution of quicksilver in aqua fortis, and abstracting the liquor till the remaining matter began to be well, but not too strongly dried, fair water poured on the remaining calx made it but somewhat yellowish; yet when we took good quicksilver, and three or four times its weight of oil of vitriol, in case we in a glass retort placed in sand drew off the saline menstruum from the metalline liquor, till there remained a dry calx at the bottom, though this precipitate were a snow-white body, yet upon pouring on it a large quantity of fair water, we did almost in a moment perceive it to pass from a milky colour to one of the loveliest light yellows that ever we had beheld. Nor is the turbith mineral, that chymists extol for its power to salivate, and for other virtues, of a colour much inferior to this, though it be often made with a differing proportion of the ingredients, a more troublesome way. For *Beguinus*, who calls it *Mercurius præcipitatus optimus*, takes to one part of quicksilver but two of liquor, and that is rectified oil of sulphur, which is (in *England* at least) far more scarce and dear than oil of vitriol; he also requires a previous digestion, two or three cohobations, and frequent ablutions with hot distilled water; with other prescriptions, which though they may conduce to the goodness of the medicine, which is that he aims at, are troublesome, and, our trials have informed you, unnecessary to the obtaining the lemon colour, which he regards not. But though we have very rarely seen either in painters shops, or elsewhere, a finer yellow than that which we have divers times this way produced (which is the more considerable, because durable and pleasant yellows are very hard to be met with, as may appear by the great use which painters are for its colour's sake fain to make of that pernicious and heavy mineral, orpiment;) yet I fear our yellow is too costly, to be like to be employed by painters, unless about choice pieces of work, nor do I know how well it will agree with every pigment, especially, with oiled colours. And whether this experiment, though it have seemed somewhat strange to most we have shewn it to, be really of another nature than those wherein saline liquors are employed, may, as we formerly also hinted, be so plausibly doubted, that whether the water poured on the calx, do barely by imbibing some of its saline parts alter its colour, by altering its texture, or whether by dissolving the concoagulated salts, it does become a saline menstruum, and, as such, work upon the mercury, I freely leave to you, *Pyrophilus*, to consider. And that I may give you some assistance in your inquiry, I will not only tell

Beguinus Tyr. Chy. Lib. 2. cap. 13.

tell you, that I have ſeveral times with fair water waſhed from this calx, good ſtore of ſtrongly taſted corpuſcles, which by the abſtraction of the menſtruum, I could reduce into ſalt; but I will alſo ſubjoin an experiment, which I deviſed, to ſhew among other things, how much a real and permanent colour may be as it were drawn forth by a liquor that has neither colour, nor ſo much as ſaline or other active parts, provided it can but bring the parts of the body it imbibes to convene into cluſters diſpoſed after the manner requiſite to the exhibiting of the emergent colour. The experiment was this.

EXPERIMENT XLIII.

WE took good common vitriol, and having beaten it to powder, and put it into a crucible, we kept it melted in a gentle heat, till by the evaporation of ſome parts, and the ſhuffling of the reſt, it had quite loſt its former colour; what remained we took out, and found it to be a friable calx, of a dirty grey. On this we poured fair water, which it did not colour green or blue, but only ſeemed to make a muddy mixture with it, then ſtopping the phial wherein the ingredients were put, we let it ſtand in a quiet place for ſome days, and after many hours the water having diſſolved a good part of the imperfectly calcined body, the vitriolate corpuſcles ſwimming to and fro in the liquor, had time by their opportune occurſions to conſtitute many little maſſes of vitriol, which gave the water they impregnated a fair vitriolate colour; and this liquor being poured off, the remaining dirty powder did in proceſs of time communicate the like colour, but not ſo deep, to a ſecond parcel of clear water that we poured on it. But this experiment, *Pyrophilus*, is (to give you that hint by the way) of too luciferous a nature to be fit to be fully proſecuted, now that I am in haſte, and willing to diſpatch what remains. And we have already ſaid of it, as much as is requiſite to our preſent purpoſe.

EXPERIMENT XLIV.

IT may *(Pyrophilus)* ſomewhat contribute towards the ſhewing how much ſome colours depend upon the leſs or greater mixture, and (as it were) contemperation of the light with ſhades, to obſerve how that ſometimes the number of particles, of the ſame colour, received into the pores of a liquor, or ſwimming up and down in it, do ſeem much to vary the colour of it. I could here preſent you with particular inſtances to ſhow, how in many (if not moſt) conſiſtent bodies, if the colour be not a light one, as white, yellow, or the like, the cloſeneſs of parts in the pigments makes it look blackiſh, though when it is diſplayed and laid on thinly, it will perhaps appear to be either blue, or green, or red. But the colours of conſiſtent pigments, not being thoſe which the preamble of this experiment has led you to expect examples in, I ſhall take the inſtances I am now to give you, rather from liquors than dry bodies. If then you put a little fair water into a clear and ſlender phial (or rather into one of thoſe pipes of glaſs, which we ſhall by and by mention;) and let fall into it a few drops of a ſtrong decoction or infuſion of cochineal, or (for want of that) of brazil; you may ſee the tincted drops deſcend like little clouds into the liquor; through which, if, by ſhaking the phial, you diffuſe them, they will turn the water either of a pink colour, or like that which is wont to be made by the waſhing of raw fleſh in fair water; by dropping a little more of the decoction, you may heighten the colour into a fine red, almoſt like that which ennobles rubies; by continuing the affuſion, you

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may bring the liquor to a kind of a crimson, and afterwards to a dark and opacous redness, somewhat like that of clotted blood. And in the passage of the liquor from one of these colours to the other, you may observe, if you consider it attentively, divers other less noted colours belonging to red, to which it is not easy to give names; especially considering how much the proportion of the decoction to the fair water; and the strength of that decoction, together with that of the trajected light and other circumstances, may vary the phænomena of this experiment. For the convenienter making whereof, we use, instead of a phial, any slender pipe of glass of about a foot or more in length, and about the thickness of a man's little finger; for, if leaving one end of this pipe open, you seal up the other hermetically (or at least stop it exquisitely with a cork well fitted to it, and overlaid with hard sealing wax melted, and rubbed upon it;) you shall have a glass, wherein may be observed the variations of the colours of liquors much better than in large phials, and wherein experiments of this nature may be well made with very small quantities of liquor. And if you please, you may in this pipe produce variety of colours in the various parts of the liquor, and keep them swimming upon one another unmixed for a good while. And some have marvelled to see, what variety of colours we have sometimes (but I confess rather by chance than skill) produced in those glasses, by the bare infusion of brazil, variously diluted with fair water, and altered by the infusion of several chymical spirits and other saline liquors devoid themselves of colour: and when the whole liquor is reduced to an uniform degree of colour, I have taken pleasure to make that very liquor seem to be of colours gradually differing, by filling with it glasses of a conical figure (whether the glass have its basis in the ordinary position, or turned upwards). And yet you need not glasses of an extraordinary shape, to see an instance of what the various mixture of light and shadow can do in the diversifying of the colour. For if you take but a large round phial, with a somewhat long and slender neck, and filling it with our red infusion of brazil, hold it against the light you will discern a notable disparity betwixt the colour of that part of the liquor which is in the body of the phial, and that which is more pervious to the light in the neck. Nay, I remember, that I once had a glass and a blue liquor (consisting chiefly, or only, if my memory deceive me not, of a certain solution of verdigrease) so fitted for my purpose, that though in other glasses the experiment would not succeed, yet when that particular glass was filled with that solution, in the body of the phial it appeared of a lovely blue, and in the neck (where the light did more dilute the colour) of a manifest green; and though I suspected there might be some latent yellowness in the substance of the neck of the glass, which might with the blue compose that green, yet was I not satisfied myself with my conjecture, but the thing seemed odd to me, as well as to divers curious persons to whom it was shown. And I lately had a broad piece of glass, which being looked on against the light seemed clear enough, and held from the light appeared very lightly discoloured; and yet it was a piece knocked off from a great lump of glass, to which if we rejoined it, where it had been broken off, the whole mass was as green as grass. And I have several times used bottles and stopples that were both made (as those, I had them from, assured me) of the very same metal; and whilst the bottle appeared but inclining towards a green, the stopple (by reason of its great thickness) was of so deep a colour, that you would hardly believe they could possibly be made of the same materials. But to satisfy some ingenious men, on another occasion, I provided myself of a flat glass (which I yet have by me) with which if I look against the light with the broad side obverted to the eye, it appears like a good ordinary window-glass; but if I turn the

the edge of it to my eye, and place my eye in a convenient posture in reference to the light, it may contend for deepness of colour with an emerald. And this greenness puts me in mind of a certain thickish, but not consistent pigment I have sometimes made, and can show you when you please, which being dropped on a piece of white paper appears, where any quantity of it is fallen, of a somewhat crimson colour; but being with one's finger spread thinly on the paper, does presently exhibit a fair green: which seems to proceed only from its disclosing its colour upon the extenuation of its depth into superficies, if the change be not somewhat helped by the colours degenerating upon one or other of the accounts formerly mentioned. Let me add, that having made divers trials with that blue substance, which in painters shops is called *Litmase*, we have sometimes taken pleasure to observe, that being dissolved in a due proportion of fair water, the solution either opposed to the light, or dropped upon white paper, did appear of a deep colour betwixt crimson and purple; and yet that being spread very thin on the paper, and suffered to dry on there, the paper was wont to appear stained of a fine blue. And to satisfy myself, that the diversity came not from the paper, which one might suspect capable of imbibing the liquor, and altering the colour, I made the trial upon a flat piece of purely white glazed earth (which I sometimes make use of about experiments of colours) with an event not unlike the former.

And now I speak of litmase, I will add, that having this very day taken a piece of it, that I had kept by me these several years, to make trials about colours, and having let fall a few drops of the strong infusion of it in fair water, into a fine crystal glass, shaped like an inverted cone, and almost full of fair water, I had now (as formerly) the pleasure to see, and to shew others, how these few tincted drops variously dispersing themselves through the limpid water, exhibited divers colours, or varieties of purple and crimson. And when the corpuscles of the pigment seemed to have equally diffused themselves through the whole liquor, I then by putting two or three drops of spirit of salt, first made an odd change in the colour of the liquor, as well as a visible commotion among its small parts, and in a short time changed it wholly into a very glorious yellow, like that of a topaz. After which if I let fall a few drops of the strong and heavy solution of pot-ashes, whose weight would quickly carry it to the sharp bottom of the glass, there would soon appear four very pleasant and distinct colours; namely, a bright, but dilute colour at the picked bottom of the glass; a purple, a little higher; a deep and glorious crimson (which crimson seemed to terminate the operation of the salt upward) in the confines betwixt the purple and the yellow; and an excellent yellow, the same that before ennobled the whole liquor, reaching from thence to the top of the glass. And if I pleased to pour very gently a little spirit of sal-armoniac upon the upper part of this yellow, there would also be a purple or a crimson, or both generated there, so that the unaltered part of the yellow liquor appeared intercepted betwixt the two neighbouring colours.

My scope in this third experiment *(Pyrophilus)* is manifold, as first to invite you to be wary in judging of the colour of liquors in such glasses as are therein recommended to you, and consequently as much, if not more, when you employ other glasses. Secondly, that you may not think it strange, that I often content myself to rub upon a piece of white paper the juice of bodies I would examine; since not only I could not easily procure a sufficient quantity of the juices of divers of them, but in several cases the trials of the quantities of such juices in glasses would make us more liable to mistakes, than the way that in those cases I have made use of. Thirdly, I hope you will by these and divers other particulars delivered in this trea-

tise, be easily induced to think that I may have set down many phænomena very faithfully, and just as they appeared to me, and yet by reason of some unheeded circumstance in the conditions of the matter, and in the degree of light, or the manner of trying the experiment, you may find some things to vary from the relations I make of them. Lastly, I designed to give you an opportunity to free yourself from the amazement which possesses most men, at the tricks of those mountebanks that are commonly called water-drinkers. For though not only the vulgar, but even many persons that are far above that rank, have so much admired to see a man, after having drunk a great deal of fair water, to spurt it out again in the form of claret-wine, sack and milk, that they have suspected the intervening of magic, or some forbidden means to effect what they conceived above the power of art; yet having once by chance had occasion to oblige a wanderer that made profession of that and other juggling tricks, I was easily confirmed by his ingenuous confession to me, that this so much admired art, indeed consisted rather in a few tricks, than in any great skill, in altering the nature and colours of things. And I am easy to be persuaded, that there may be a great deal of truth in a little pamphlet printed divers years ago in English, wherein the author undertakes to discover, and that (if I mistake not) by the confession of some of the accomplices themselves, that a famous water-drinker, then much admired in *England*, performed his pretended transmutations of liquors by the help of two or three inconsiderable preparations and mixtures of not unobvious liquors, and chiefly of an infusion of brazil variously diluted and made pale or yellowish (and otherwise altered) with vinegar; the rest of their work being performed by the shape of the glasses, by craft and legerdemain. And for my part, that which I marvel at in this business, is the drinkers being able to take down so much water, and spout it out with that violence; though custom, and a vomit seasonably taken before hand, may in some of them much facilitate the work. But as for the changes made in the liquors, they were but few and slight in comparison of those, that the being conversant in chymical experiments, and dextrous in applying them to the transmuting of colours, may easily enough enable a man to make, as even what has been newly delivered in this, and the foregoing experiment; especially if we add to it the things contained in the twentieth, the thirty-ninth, and the fortieth experiments, may perhaps have already persuaded you.

EXPERIMENT XLV.

YOU may, I presume, *(Pyrophilus)* have taken notice, that in this whole treatise I purposely decline (as far as I well can) the mentioning of elaborate chymical experiments, for fear of frighting you by their tediousness and difficulty; but yet, in confirmation of what I have been newly telling you about the possibility of varying the colours of liquors, better than the water-drinkers are wont to do, I shall add, that *Helmont* used to make a preparation of steel, which a very ingenious chymist, his son's friend, whom you know, sometimes employs for a succedaneum to the Spaw-waters, by diluting this *essentia martis liquida* (as he calls it) with a due proportion of water. Now that for which I mention to you this preparation (which as he communicated to me, I know he will not refuse to *Pyrophilus)* is this, that though the liquor (as I can shew you when you please) be almost of the colour of a German (not an Oriental) amethist, and consequently remote enough from green, yet a very few drops being let fall into a large proportion of good Rhenish, or (in want of that) white-wine (which yet does not quite so well) immediately turned the liquor

into a lovely green, as I have not without delight shown several curious persons. By which phænomenon you may learn, among other things, how requisite it is in experiments about the changes of colours heedfully to mind the circumstances of them: for water will not, as I have purposely tried, concur to the production of any such green, nor did it give that colour to moderate spirit of wine, wherein I purposely dissolved it, and wine itself is a liquor that few would suspect of being able to work suddenly any such change in a metalline preparation of this nature. And to satisfy myself that this new colour proceeds rather from the peculiar texture of the wine, than from any greater acidity, that Rhenish or white-wine (for that may not absurdly be suspected) has in comparison of water; I purposely sharpened the solution of this essence in fair water, with a good quantity of spirit of salt, notwithstanding which, the mixture acquired no greenness. And to vary the experiment a little, I tried, that if into a glass of Rhenish wine made green by this essence, I dropped an alcalizate solution, or urinous spirit, the wine would presently grow turbid, and of an odd dirty colour: but if instead of dissolving the essence in wine, I dissolved it in fair water, sharpened perhaps with a little spirit of salt, then either the urinous spirit of sal armoniac, or the solution of the fixed salt of pot-ashes, would immediately turn it of a yellowish colour, the fixed or urinous salt precipitating the vitriolate substance contained in the essence. But here I must not forget to take notice of a circumstance that deserves to be compared with some part of the foregoing experiment; for whereas our essence imparts a greenness to wine, but not to water, the industrious *Olaus Wormius* in his late *Musæum* tells us of a rare kind of turnsol, which he calls *Bezetta Rubra*, given him by an apothecary that knew not how it was made, whose lovely redness would be easily communicated to water, if it were immersed in it; but scarce to wine, and not at all to spirit of wine: in which last circumstance it agrees with what I lately told you of our essence, notwithstanding their disagreement in other particulars. *Libr. 2. Cap. 34.*

EXPERIMENT XLVI.

WE have often taken notice, as of a remarkable thing, that metals, as they appear to the eye, before they come to be farther altered by other bodies, do exhibit colours very different from those which the fire and the menstruum, either apart, or both together, do produce in them; especially considering that these metalline bodies are after all these disguises reducible not only to their former metalline consistence, and other more radical properties, but to their colour too; as if nature had given divers metals to each of them a double colour, an external, and an internal. But though upon a more attentive consideration of this difference of colours, it seemed probable to me that divers (for I say not all) of those colours which we have just now called internal, are rather produced by the coalition of metalline particles with those of the salts, or other bodies employed to work on them, than by the bare alteration of the parts of the metals themselves; and though therefore we may call the obvious colours natural or common, and the others adventitious: yet because such changes of colours, from whatsoever cause they be resolved to proceed, may be properly enough taken in to illustrate our present subject, we shall not scruple to take notice of some of them, especially because there are among them such as are produced without the intervention of saline menstruums. Of the adventitious colours of metalline bodies the chief sorts seem to be these three: the first, such colours as are

are produced without other additaments by the action of the fire upon metals. The next, such as emerge from the coalition of metalline particles with those of some menstruum employed to corrode a metal or precipitate it; and the last, the colours afforded by metalline bodies either colliquated with, or otherwise penetrating into, other bodies, especially fusible ones. But these *(Pyrophilus)* are only, as I told you, the chief sorts of the adventitious colours of metals, for there may others belong to them, of which I shall hereafter have occasion to take notice of some, and of which also there possibly may be others that I never took notice of.

AND to begin with the first sort of colours, it is well enough known to chymists, that tin being calcined by fire alone is wont to afford a white calx, and lead calcined by fire alone affords that most common red powder we call minium: copper also calcined *per se*, by a long or violent fire, is wont to yield (as far as I have had occasion to take notice of it) a very dark or blackish powder; that iron likewise may by the action of reverberated flames be turned into a colour almost like that of saffron, may be easily deduced from the preparation of that powder, which by reason of its colour and of the metal it is made of, is by chymists called *Crocus Martis per se*. And that mercury, made by the stress of fire, may be turned into a red powder, which chymists call precipitate *per se*, I elsewhere more particularly declare.

ANNOTATION I.

IT is not unworthy the admonishing you *(Pyrophilus)* and it agrees very well with our conjectures about the dependence of the change of a body's colour upon that of its texture, that the same metal may by the successive operation of the fire receive divers adventitious colours, as is evident in lead, which before it come to so deep a colour as that of minium, may pass through divers others.

ANNOTATION II.

NOT only the calces, but the glasses of metals, vitrified *per se*, may be of colours differing from the natural or obvious colour of the metal; as I have observed in the glass of lead, made by long exposing crude lead to a violent fire, and what I have observed about the glass or slag of copper (of which I can show you some of an odd kind of texture) may be elsewhere more conveniently related. I have likewise seen a piece of very dark glass, which an ingenious artificer that showed it me professed himself to have made of silver alone by an extreme violence (which seems to be no more than is needful) of the fire.

ANNOTATION III.

MINERALS also by the action of the fire may be brought to afford colours very differing from their own, as I not long since noted to you about the variously coloured flowers of antimony; to which we may add the whitish grey-colour of its calx, and the yellow or reddish colour of the glass, whereinto that calx may be fluxed.

AND I remember, that I elsewhere told you, that vitriol calcined with a very gentle heat, and afterwards with higher and higher degrees of it, may be made to pass through several colours before it descends to a dark purplish colour, whereto a strong fire is wont

wont at length to reduce it. But to insist on the colours produced by the operation of fire upon several minerals, would take up far more time than I have now to spare.

EXPERIMENT XLVII.

THE adventitious colours produced upon metals, or rather with them, by saline liquors, are many of them so well known to chymists, that I would not here mention them, but that besides a not un-needed testimony, I can add something of my own, to what I shall repeat about them; and divers experiments which are familiar to chymists, are as yet unknown to the greatest part of ingenious men.

THAT gold dissolved in aqua regia ennobles the menstruum with its own colour, is a thing that you cannot *(Pyrophilus)* but have often seen. The solutions of mercury in aqua fortis are not generally taken notice of, to give any notable tincture to the menstruum; but sometimes when the liquor first falls upon the quicksilver, I have observed a very remarkable, though not durable greenness, or blueness to be produced; which is a phænomenon not unfit for you to consider, though I have not now the leisure to discourse upon it. Tin corroded by aqua fortis till the menstruum will work no farther on it, becomes exceeding white; but, as we elsewhere note, does very easily of itself acquire the consistence, not of a metalline calx, but of a coagulated matter, which we have observed with pleasure to look so like, either to curdled milk, or curdled whites of eggs, that a person unacquainted with such solutions may easily be mistaken in it. But when I purposely prepared a menstruum that would dissolve it as aqua fortis dissolves silver, and not barely corrode it, and quickly let it fall again, I remember not that I took notice of any particular colour in the solution, as if the more whitish metals did not much tinge their menstruums, though the conspicuously coloured metals as gold, and copper, do. For lead dissolved in spirit of vinegar or aqua fortis gives a solution clear enough, and if the menstruum be abstracted appears either diaphanous or white. Of the colour of iron we have elsewhere said something: and it is worth noting, that though if that metal be dissolved in oil of vitriol diluted with water, it affords a salt or magistery so like in colour, as well as some other qualities, to other green vitriol, that chymists do not improperly call it *Vitriolum Martis*; yet I have purposely tried, that, by changing the menstruum, and pouring upon the filings of steel, instead of oil of vitriol, aqua fortis (whereof, as I remember, I used four parts to one of the metal) I obtained not a green, but a saffron coloured solution; or rather a thick liquor of a deep but yellowish red. Common silver, such as is to be met with in coins, being dissolved in aqua fortis, yields a solution tincted like that of copper, which is not to be wondered at, because in the coining of silver, they are wont (as we elsewhere particularly inform you) to give it an allay of copper, and that, which is sold in shops for refined silver, is not (so far as we have tried) so perfectly free from that ignobler metal, but that a solution of it in aqua fortis will give a venereal tincture to the menstruum. But we could not observe upon the solution of some silver, which was perfectly refined (such as some that we have, from which eight or ten times its weight of lead has been blown off) that the menstruum, though held against the light in a crystal phial, did manifestly disclose any tincture, only it seemed sometimes not to be quite destitute of a little, but very faint blueishness.

BUT here I must take notice, that of all the metals, there is not any, which doth so easily and constantly disclose its unobvious colour, as copper doth. For not only in acid menstruums, as aqua fortis and spirit of vinegar, it gives a blueish green solution,

ſolution, but if it be almoſt any way corroded, it appears of one of thoſe two colours, as may be obſerved in verdigreaſe made ſeveral ways, in that odd preparation of Venus, which we elſewhere teach you to make with ſublimate, and in the common vitriols of Venus delivered by chymiſts. And ſo conſtant is the diſpoſition of copper, notwithſtanding the diſguiſe artiſts put upon it, to diſcloſe the colour we have been mentioning, that we have by forcing it up with ſal armoniac obtained a ſublimate of a blueiſh colour. Nay, a famous Spagyriſt affirms, that the very mercury of it is green; but till he teach us an intelligible way of making ſuch a mercury, we muſt content ourſelves to inform you, that we have had a cupreous body, that was precipitated out of a diſtilled liquor, that ſeemed to be the ſulphur of Venus, and ſeemed, even when flaming, of a greeniſh colour. And indeed copper is a metal ſo eaſily wrought upon by liquors of ſeveral kinds, that I ſhould tell you, I know not any mineral, that will concur to the production of ſuch a variety of colours as copper diſſolved in ſeveral menſtruums, as ſpirit of vinegar, aqua fortis, aqua regis, ſpirit of nitre, of urine, of ſoot, oils of ſeveral kinds, and I know not how many other liquors, if the variety of ſomewhat differing colours (that copper will be made to aſſume, as it is wrought upon by ſeveral liquors) were not comprehended within the limits of greeniſh blue, or blueiſh green.

AND yet I muſt advertiſe you *(Pyrophilus)* that being deſirous to try, if I could not make with crude copper a green ſolution without the blueiſhneſs, that is wont to accompany its vulgar ſolutions, I bethought myſelf of uſing two menſtruums, which I had not known employed to work on this metal, and which I had certain reaſons to make trial of, as I ſucceſsfully did. The one of theſe liquors (if I much miſremember not) was ſpirit of ſugar diſtilled in a retort, which muſt be warily done (if you will avoid breaking your glaſſes;) and the other, oil or ſpirit of turpentine, which affords a fine green ſolution, that is uſeful to me on ſeveral occaſions. And yet to ſhew, that the adventitious colour may reſult, as well from the true and permanent copper itſelf, as the ſalts wherewith it is corroded, I ſhall add, that if you take a piece of good *Dantzick* copperas, or any other vitriol, wherein Venus is predominant, and having moiſtened it in your mouth, or with fair water, rub it upon a whetted knife, or any other bright piece of ſteel or iron, it will (as we have formerly told you) preſently ſtain the ſteel with a reddiſh colour, like that of copper; the reaſon of which we muſt not now ſtay to inquire.

ANNOTATION I.

I PRESUME you may have taken notice *(Pyrophilus)* that I have borrowed ſome of the inſtances mentioned in this 47th experiment from the laboratories of chymiſts; and becauſe in ſome (though very few) other paſſages of this eſſay, I have likewiſe made uſe of experiments mentioned alſo by ſome ſpagyrical writers, I think it not amiſs to repreſent to you on this occaſion once for all ſome things, beſides thoſe which I intimated in the preamble of this preſent experiment. For beſides, that it is very allowable for a writer to repeat an experiment, which he invented not, in caſe he improve it; and beſides that many experiments familiar to chymiſts are unknown to the generality of learned men, who either never read chymical proceſſes, or never underſtood their meaning, or never durſt believe them; beſides theſe things, I ſay, I ſhall repreſent, that, as to the few experiments I have borrowed from the chymiſts, if they be very vulgar, it would perhaps be difficult to aſcribe each of them its own author, and it is more than the generality of chymiſts themſelves can do: and if they be not of

of very known and familiar practice among them, unless the authors, wherein I found them, had given me cause to believe themselves had tried them, I know not why I might not set them down, as a part of the phænomena of colours, which I present you; many things unanimously enough delivered as matters of fact by I know not how many chymical writers, being not to be relied on, upon the single authority of such authors: for instance, as some Spagyrists deliver (perhaps amongst several deceitful processes) that *saccharum Saturni* with spirit of turpentine will afford a balsam, so *Beguinus* and many more tell us, that the same concrete *(saccharum Saturni)* will yield an incomparably fragrant spirit, and a pretty quantity of two several oils. And yet since many have complained, as well as I have done, that they could find no such odoriferous, but rather an ill-scented liquor, and scarce any oil in their distillation of that sweet vitriol, a wary person would as little build any thing on what they say of the former experiment, as upon what they aver of the latter; and therefore I scrupled not to mention this red balsam, of which I have not seen any (but what I made) among my other experiments about redness.

ANNOTATION II.

WE have sometimes had the curiosity to try, what colours minerals, as tin-glass, antimony, spelter, *&c.* would yield in several menstruums; nor have we forborn to try the colours of stones, of which that famous one (which *Helmont* calls *Paracelsus*'s *Ludus)* though it be digged out of the earth, and seem a true stone, has afforded in menstruums capable to dissolve so solid a stone, sometimes a yellowish, sometimes a red solution, of both which I can shew you. But though I have from minerals obtained with several menstruums very differing colours, and some such as, perhaps, you would be surprized to see drawn from such bodies; yet I must now pass by the particulars, being desirous to put an end to this treatise, before I put an end to your patience and my own.

ANNOTATION III.

AND yet before I pass to the next experiment, I must put you in mind, that the colours of metals may in many cases be further altered by employing, either precipitating salts, or other convenient substances to act upon their solutions. Of this you may remember that I have given you several instances already, to which may be added such as these; that if quicksilver be dissolved in aqua fortis, and precipitated out of the solution, either with water impregnated with sea-salt, or with the spirit of the concrete, it falls to the bottom in the form of a white powder; whereas if it be precipitated with an alcali, it will afford a yellowish or tawny powder; and if there be no precipitation made, and the menstruum be drawn off with a convenient fire, the corroded mercury will remain in the bottom, in the form of a substance, that may be made to appear of differing colours by differing degrees of heat: as I remember, that lately having purposely abstracted aqua fortis from some quicksilver, that we had dissolved in it, so that there remained a white calx, exposing that to several degrees of fire, and afterwards to a naked one, we obtained some new colours, and at length the greatest part of the calx lying at the bottom of the phial, and being brought partly to a deep yellow, and partly to a red colour, the rest appeared elevated to the upper part and neck of the phial, some in the form of reddish, and some of an ash-colour sublimate. But of the differing colours, which by differing ways and working

of quickſilver with fire, and ſaline bodies, may be produced in precipitates, I may elſewhere have ocaſion to take further notice. I alſo told you not long ſince, that if you corrode quickſilver with oil of vitriol inſtead of aqua fortis, and abſtract the menſtruum, there will remain a white calx, which by the affuſion of fair water preſently turns into a lemon colour. And even the ſuccedaneum to a menſtruum may ſometimes ſerve the turn, to change the colours of a metal. The lovely red, which painters call vermilion, is made of mercury, which is of the colour of ſilver; and of brimſtone, which is of kin to that of gold, ſublimed up together in a certain proportion, as is vulgarly known to Spagyriſts.

EXPERIMENT XLVIII.

THE third chief ſort of the adventitious colours of metals is that, which is produced by aſſociating them (eſpecially when calcined) with other fuſible bodies, and principally *Venice*, and other fine glaſs, devoid of colour.

I HAVE formerly given you an example, whereby it may appear, that a metal may impart to glaſs a colour much differing from its own, when I told you how with ſilver I had given glaſs a lovely golden colour. And I ſhall now add, that I have learned from one of the chief artificers, that ſells painted glaſs, that thoſe of his trade colour it yellow with a preparation of the calx of ſilver. Though having lately had occaſion among other trials to mingle a few grains of ſhell-ſilver (ſuch as is employed with the pencil and pen) with a convenient proportion of powdered cryſtal glaſs, having kept them two or three hours in fuſion, I was ſurprized to find the colliquated maſs to appear, upon breaking the crucible, of a lovely ſaphirine blue; which made me ſuſpect my ſervant might have brought me a wrong crucible: but he conſtantly affirmed it to be the ſame, wherein the ſilver was put, and conſiderable circumſtances countenanced his aſſertion, ſo that till I have opportunity to make farther trial, I cannot but ſuſpect, either that ſilver, which is not (which is not very probable) brought to a perfect fuſion and colliquation with glaſs, may impart to it other colours than when nealed upon it; or elſe (which is leſs unlikely) that though ſilver-beaters uſually chuſe the fineſt coin they can get, as that which is moſt extenſive under the hammer, yet the ſilver leaves, of which this ſhell-ſilver was made, might retain ſo much copper, as to enable it to give the predominant tincture to the glaſs.

FOR, I muſt proceed to tell you *(Pyrophilus)* as another inſtance of the adventitious colours of metals, that, which is ſomething ſtrange, namely, that, though copper calcined *per ſe* affords but a dark and baſely coloured calx, yet the glaſs-men do with it, as themſelves inform me, tinge their glaſs green. And I remember, that when once we took ſome crude copper, and by frequent ignition quenching it in water had reduced it to a dark and ill-coloured powder, and afterwards kept it in fuſion in about a hundred times its weight of fine glaſs, we had, though not a green, yet a blue coloured maſs; which would perhaps have been green, if we had hit right upon the proportion of the materials, and the degree of fire, and the time wherein it ought to be kept in fuſion; ſo plentifully does that metal abound in a venereal tincture, as artiſts call it, and in ſo many ways does it diſcloſe that richneſs. But though copper do, as we have ſaid, give ſomewhat near the like colour to glaſs, which it does to aqua fortis, yet it ſeems worthy to enquire, whether theſe new colours, which mineral bodies diſcloſe in melted glaſs, proceed from the coalition of the corpuſcles of the mineral with the particles of the glaſs as ſuch, or from the action (excited or actuated by fire) of the alcalizate ſalt (which is a main ingredient of glaſs) upon the mineral body,

body, or from the concurrence of both these causes, or else from any other. But to return to that which we were saying, we may observe, that putty made by calcining together a proportion of tin and lead, as it is itself a white calx, so does it turn the *pitta di crystello* (as the glass-men call the matter of the purer sort of glass, wherewith it is colliquated) into a white mass; which, if it be opacous enough, is employed, as we elsewhere declare, for white amel. But of the colours, which the other metals may be made to produce in colourless glass, and other vitrifiable bodies, that have native colours of their own, I must leave you to inform yourself upon trial; or at least must forbear to do it till another time, considering how many annotations are to follow, upon what has in this and the two former experiments been said already.

ANNOTATION I.

WHEN the materials of glass being melted with calcined tin, have composed a mass undiaphanous and white, this white amel is as it were the basis of all those fine concretes, that goldsmiths and several artificers employ in the curious art of enamelling. For this white and fusible substance will receive into itself, without spoiling them, the colours of divers other mineral substances, which like it will endure the fire.

ANNOTATION II.

SO that as by the present (XLVIII) experiment it appears, that divers minerals will impart to fusible masses colours differing from their own; so by the making and compounding of amels, it may appear, that divers bodies will both retain their colour in the fire, and impart the same to some others wherewith they were vitrified, and in such trials as that mentioned in the seventeenth experiment, where I told you, that even in amels a blue and yellow will compound a green. It is pretty to behold not only that some colours are of so fixed a nature, as to be capable of mixture without receiving any detriment by the fire, that does so easily destroy or spoil those of other bodies; but mineral pigments may be mingled by fire little less regularly and successfully, than in ordinary dying fatts, the vulgar colours are wont to be mingled by the help of water.

ANNOTATION III.

IT is not only metalline, but other mineral bodies, that may be employed, to give tinctures unto glass; and it is worth noting how small a quantity of some mineral substances will tinge a comparatively vast proportion of glass; and we have sometimes attempted to colour glass, even with precious stones, and had cause to think the experiment not cast away. And it is known by them, that have looked into the art of glass, that the artificers used to tinge their glass blue with that dark mineral zaffora (some of my trials on which I elsewhere acquaint you) which some would have to be a mineral earth, others of stone, and others neither the one nor the other, but which is confessedly of a dark, but not a blue colour, though it be not agreed of what particular colour it is. It is likewise though a familiar yet a remarkable practice among those, that deal in the making of glass, to employ (as some of themselves have informed me) what they call manganess, and some authors call *Magnesia* (of which I make particular mention in another treatise) to exhibit in glass not only other colours

than its own (which is so like in darkness or blackishness to the loadstone, that is given by mineralists for one of the reasons of its Latin name) but colours differing from one another. For, though they use it (which is somewhat strange) to clarify their glass, and free it from that blueish greenish colour, which else it would too often be subject to; yet they also employ it in certain proportions, to tinge their glass both with red colour, and with a purplish or murry; and putting in a greater quantity, they also make with it that deep obscure glass, which is wont to pass for black, which agrees very well with, and may serve to confirm what we noted near the beginning of the 44th experiment, of the seeming blackness of those bodies, that are over-charged with the corpuscles of such colours, as red, or blue, or green, *&c.* And as by several metals and other minerals we can give various colours to glass, so on the other side, by the differing colours, that mineral ores, or other mineral powders, being melted with glass, disclose in it, a good conjecture may be oftentimes made of the metal or known mineral, that the ore proposed either holds, or is most of kin to. And this easy way of examining ores may be in some cases of good use, and is not ill delivered by *Glauber*, to whom I shall at present refer you for a more particular account of it: unless your curiosity command also what I have observed about these matters. Only I must here advertise you that great circumspection is requisite to keep this way from proving fallacious, upon the account of the variations of colour, that may be produced by the differing proportions, that may be used betwixt the ore and the glass, by the richness or poorness of the ore itself, by the degree of fire, and especially by the length of time, during which the matter is kept in fusion; as you will easily gather from what you will quickly meet with in the following annotation upon this 48th experiment.

ANNOTATION IV.

THERE is another way, and differing enough from those already mentioned, by which metals may be brought to exhibit adventitious colours: for by this, the metal does not so much impart a colour to another body, as receive a colour from it, or rather both bodies do by the new texture resulting from their mistion produce a new colour. I will not insist to this purpose upon the examples afforded us by yellow orpiment, and common sea-salt, from which, sublimed together, chymists unanimously affirm their white or crystalline arsenic to be made: but it is not unworthy our noting, that though yellow orpiment be acknowledged to be the copiousest by far of the two ingredients of arsenic, yet this last named body being duly added to the highest coloured metal copper, when it is in fusion, gives it whiteness both within and without. Thus *Lapis Calaminaris* changes and improves the colour of copper, by turning it into brass. And I have sometimes, by the help of zink duly mixed after a certain manner, given copper one of the richest golden colours, that ever I have seen the best true gold ennobled with. But pray have a care, that such hints fall not into any hands, that may misemploy them.

ANNOTATION V.

UPON the knowledge of the differing ways of making minerals and metals produce their adventitious colours in bodies capable of vitrification, depends the pretty art of making what chymists by a barbarous word are pleased to call *Amanses*, that is, counterfeit or factitious gems, as emeralds, rubies, saphires, topazes, and the

the like. For in the making of these, though pure sand or calcined crystal give the body, yet it is for the most part some metalline or mineral calx, mingled in a small proportion, that gives the colour. But though I have many years since taken delight to divert myself with this pleasing art, and have seen very pretty productions of it, yet besides that I fear I have now forgot most of the little skill I had in it, this is no place to entertain you with what would rather take up an entire discourse than be comprehended in an annotation. Wherefore the few things, which I shall here take notice of to you, are only what belong to the present argument, namely,

First, That I have often observed, that calcined lead colliquated with fine white sand or crystal, reduced by ignitions and subsequent extinctions in water to a subtile powder, will of itself be brought by a due decoction to give a clear mass coloured like a German amethyst. For though this glass of lead is looked upon by them, that know no better way of making *Amanses*, as the grand work of them all; yet, which is an inconvenience, that much blemishes this way, the calcined lead itself does not only afford matter to the *Amanses*, but has also as well as other metals a colour of its own, which, as I was saying, I have often found to be like that of German (as many call them) not Eastern amethysts.

Secondly, That nevertheless this colour may be easily overpowered by those of divers other mineral pigments (if I may so call them) so that with glass of lead you may emulate (for instance) the fresh and lovely greenness of an emerald, though in divers cases the colour, which the lead itself upon vitrification tends to, may vitiate that of the pigment, which you would introduce into the mass.

Thirdly, That so much even these colours depend upon texture, that in the glass of lead itself made of about three parts of litharge or minium colliquated with one of very finely powdered crystal or sand, we have taken pleasure to make the mixture pass through differing colours, as we kept it more or less in the fusion. For it was not usually till after a pretty long decoction, that the mass attained to the amethystine colour.

Fourthly and lastly, That the degrees of coction and other circumstances may so vary the colour produced in the same mass, that in a crucible that was not great I have had fragments of the same mass, in some of which, perhaps not so big as a hazel-nut, you may discern four distinct colours.

ANNOTATION VI.

YOU may remember *(Pyrophilus)* that when I mentioned the three sorts of adventitious colours of metals, I mentioned them but as the chief, not the only. For there may be other ways, which though they do not in so strict a sense belong to the adventitious colours of metals, may not inconveniently be reduced to them. And of these I shall name now a couple, without denying, that there may be more.

The first may be drawn from the practice of those, that dye scarlet. For the famousest master in that art, either in *England* or *Holland*, has confessed to me, that neither others nor he can strike that lovely colour, which is now wont to be called the Bow-dye, without their materials be boiled in vessels, either made of, or lined with a particular metal. But of what I have known attempted in this kind, I must not as yet, for fear of prejudicing or displeasing others, give you any particular account. *See the latter end of the fifteenth experiment.*

The other way *(Pyrophilus)* of making metals afford unobvious colours, is by imbuing divers bodies with solutions of them made in their proper menstruums: as (for instance)

instance) though copper plentifully dissolved in aqua fortis, will imbue several bodies with the colour of the solution; yet some other metals will not (as I elsewhere tell you) and have often tried. Gold dissolved in aqua regia will (which is not commonly known) dye the nails and skin, and hafts of knives, and other things made of ivory, not with a golden, but a purple colour, which though it manifest but slowly, is very durable, and scarce ever to be washed out. And if I misremember not, I have already told you in this treatise, that the purer crystals of fine silver made with aqua fortis, though they appear white, will presently dye the skin and nails with a black, or at least a very dark colour, which water will not wash off, as it will ordinary ink from the same parts. And divers other bodies may the same way be dyed, some of a black, and others of a blackish colour.

AND as metalline, so likewise mineral solutions may produce colours differing enough from those of the liquors themselves. I shall not fetch an example of this, from what we daily see happen in the powdering of beef, which by the brine employed about it (especially if the flesh be over-salted) does oftentimes appear at our tables of a green, and sometimes of a reddish colour (deep enough;) nor shall I insist on the practice of some that deal in salt-petre, who (as I suspected, and as themselves acknowledged to me) do, with the mixture of a certain proportion of that, and common salt, give a fine redness, not only to neats tongues, but, which is more pretty as well as difficult, to such flesh, as would otherwise be purely white: these examples, I say, I shall decline insisting on, as chusing rather to tell you, that I have several times tried, that a solution of the sulphur of vitriol, or even of common sulphur, though the liquor appeared clear enough, would immediately tinge a piece of new coin, or other clean silver, sometimes with a golden, sometimes with a deeper and more reddish colour, according to the strength of the solution, and the quantity of it, that chanced to adhere to the metal; which may take off your wonder, that the water of the hot spring at *Bath*, abounding with dissolved substances of a very sulphureous nature, should for a while as it were gild the new or clean pieces of silver coin, that are for a due time immersed in it. And to these may be added those formerly mentioned examples of the adventitious colours of mineral bodies: which brings into my mind, that even vegetable liquors, whether by degeneration, or by altering the texture of the body that imbibes them, may stain other bodies with colours differing enough from their own, of which very good herbarists have afforded us a notable example, by affirming, that the juice of alcanna being green (in which state I could never here procure it) does yet dye the skin and nails of a lasting red. But I see this treatise is like to prove too bulky, without the addition of further instances of this nature.

EXPERIMENT XLIX.

MEETING the other day, *Pyrophilus*, in an Italian book, that treats of other matters, with a way of preparing what the author calls a lacca of vegetables, by which the Italians mean a kind of extract fit for painting, like that rich lacca in English commonly called lake, which is employed by painters as a glorious red: and finding the experiment not to be inconsiderable, and very defectively set down; it will not be amiss to acquaint you with what some trials have informed us, in reference to this experiment, which both by our Italian author, and by divers of his countrymen, is looked upon as no trifling secret.

TAKE

TAKE then the root called in Latin *Curcuma*, and in English turmerick (which I made use of, because it was then at hand, and is among vegetables fit for that purpose one of the most easiest to be had): and when it is beaten, put what quantity of it you please into fair water, adding to every pound of water about a spoonful or better of as strong a lixivium or solution of pot ashes as you can well make, clarifying it by filtration before you put it to the decocting water. Let these things boil, or rather simmer over a soft fire in a clean glazed earthen vessel, till you find by the immersion of a sheet of white paper (or by some other way of trial) that the liquor is sufficiently impregnated with the golden tincture of the turmeric; then take the decoction off the fire, and filter or strain it, that it may be clean; and leisurely dropping into it a strong solution of roch-allom, you shall find the decoction as it were curdled, and the tincted part of it either to emerge, to subside, or to swim up and down, like little yellow flakes: and if you pour this mixture into a tunnel lined with cap-paper, the liquor, that filtred formerly so yellow, will now pass clean through the filtre, leaving its tincted and as it were curdled parts in the filtre, upon which fair water must be so often poured, till you have dulcified the matter therein contained, the sign of which dulcification is (you know) when the water, that has passed through it, comes from it as tasteless as it was poured on it. And if without filtration you would gather together the flakes of this vegetable lake, you must pour a great quantity of fair water upon the decoction after the affusion of the alluminous solution, and you shall find the liquor to grow clearer, and the lake to settle together at the bottom, or emerge to the top of the water; though sometimes having not poured out a sufficient quantity of fair water, we have observed the lake partly to subside, and partly to emerge, leaving all the middle of the liquor clear. But to make this lake fit for use, it must, by repeated affusions of fresh water, be dulcified from the adhering salts, as well as that separated by filtration, and be spread and suffered to dry leisurely upon pieces of cloth, with brown paper, or chalk, or bricks under them to imbibe the moisture *.

ANNOTATION I.

WHEREAS it is presumed, that the magistery of vegetables obtained this way consists but of the more soluble and coloured parts of the plants that afford it, I must take the the liberty to question the supposition; and for my so doing, I shall give you this account.

ACCORDING to the notions (such as they were) that I had concerning salts; allom, though to sense a homogeneous body, ought not to be reckoned among true salts, but to be itself looked upon as a kind of magistery, in regard that as native vitriol (for such I have had) contains both a saline substance and a metal, whether copper, or iron, corroded by it, and associated with it; so allom, which may be of so near a kin to vitriol, that in some places of *England* (as we are assured by good authority the same stone will sometimes afford both) seems manifestly to contain a peculiar kind of acid spirit, generated in the bowels of the earth, and some kind of stony matter dissolved by it. And though in making our ordinary allom the workmen use the ashes of a sea-weed (vulgarly called kelp) and urine; yet those, that should know, inform us,

* The curious reader that desires further information concerning lakes, may resort to the 7th Book of *Neri*'s art of glass, Englished, (6 or 7 years since the writing of this 40th experiment) and illustrated with learned observations by the inquisitive and experienced Dr. *Charles Merret*.

that,

that, here in *England*, there is besides the factitious allom, allom made by nature without the help of those additaments. Now, *Pyrophilus*, when I consider this composition of allom, and that alcalizate salts are wont to precipitate what acid salts have dissolved, I could not but be prone to suspect, that the curdled matter, which is called the magistery of vegetables, may have in it no inconsiderable proportion of a stony substance precipitated out of the allom by the lixivium, wherein the vegetable had been decocted. And to shew you, that there is no necessity, that all the curdled substance must belong to the vegetable, I shall add, that I took a strong solution of allom, and having filtred it, by pouring in a convenient quantity of a strong solution of pot-ashes, I presently, as I expected, turned the mixture into a kind of white curds, which being put to filtre, the paper retained a stony calx, copious enough, very white, and which seemed to be of a mineral nature, both by some other signs and this, that little bits of it being put upon a live coal, which was gently blown whilst they were on it, they did neither melt nor fly away, and you may keep a quantity of this white substance for a good while (nay, for aught I can guess, for a very long one) in a red hot crucible, without losing or spoiling it; nor did hot water, wherein I purposely kept another parcel of such calx, seem to do any more than wash away the looser adhering salts from the stony substance, which therefore seemed unlikely to be separable by ablutions (though reiterated) from the precipitated parts of the vegetable, whose lake is intended. And to shew you, that there is likewise in allom a body, with which the fixed salt of the alcalizate solution will concoagulate into a saline substance differing from either of them, I shall add, that I have taken pleasure to recover out of the slowly exhaled liquor, that passed through the filtre, and left the aforementioned calx behind, a body, that at least, seemed a salt very pretty to look on, as being very white, and consisting of an innumerable company of exceeding slender and shining particles, which would in part easily melt at the flame of a candle, and in part fly away with some little noise. But of this substance, and its odd qualities, more, perhaps, elsewhere; for now I shall only take notice to you, that I have likewise with urinous salts, such as the spirit of sal armoniac, as well as with the spirit of urine itself, nay (if I much mistake not) even with stale urine undistilled, easily precipitated such a white calx, as I was formerly speaking of, out of a limpid solution of allom: so that there is need of circumspection in judging of the natures of liquors by precipitations, wherein allom intervenes; else we may sometimes mistakingly imagine, that to be precipitated out of a liquor by allom, which is rather precipitated out of allom by the liquor. And this puts me in mind to tell you, that it is not unpleasant to behold, how quickly the solution of allom (or injected lumps of allom) does occasion the severing of the coloured parts of the decoction from the liquor, that seemed to have so perfectly imbibed them.

ANNOTATION II.

THE above-mentioned way of making lakes we have tried not only with turmeric, but also with madder, which yielded us a red lake; and with rue, which afforded us an extract, of (almost, if not altogether) the same colour with that of the leaves.

BUT in regard that it is principally the alcalizate salt of the pot-ashes, which enables the water to extract so powerfully the tincture of the decocted vegetables, I fear, that our author may be mistaken, by supposing, that the decoction will always be of the very same colour with the vegetable it is made of. For lixiviate salts, to which pot-ashes

ashes eminently belong, though by piercing and opening the bodies of vegetables, they prepare and dispose them to part readily with their tincture; yet some tinctures they do not only draw out, but likewise alter them, as may be easily made appear by many of the experiments already set down in this treatise. And though allom being of an acid nature, its solutions may in some cases destroy the adventitious colours produced by the alcali, and restore the former; yet besides that allom is not, as I have lately shown, a mere acid salt, but a mixt body; and besides that its operations are languid in comparison of the activity of salts freed by distillation, or by incineration and dissolution, from the most of their earthy parts, we have seen already examples that in divers cases an acid salt will not restore a vegetable substance to the colour, of which an alcalizate one had deprived it, but makes it assume a third very differing from both; as we formerly told you, that if syrup of violets were by an alcali turned green (which colour, as I have tried, may be the same way produced in the violet-leaves themselves without any relation to a syrup) an acid salt would not make it blue again, but red. And though I have, by this way of making lakes, made magisteries (for such they seem to be) of brazil, and as I remember of cochineal itself, and of other things, red, yellow, or green, which lakes were ennobled with a rich colour, and others had no bad one; yet in some the colour of the lake seemed rather inferior than otherwise to that of the plant, and in others it seemed both very differing, and much worse. But writing this in a time and place, where I cannot provide myself of flowers and other vegetables to prosecute such trials in a competent variety of subjects, I am content not to be positive in delivering a judgment of this way of lakes, till experience, or you, *Pyrophilus*, shall have afforded me a fuller and more particular information.

ANNOTATION III.

AND on this occasion, *Pyrophilus*, I must here (having forgot to do it sooner) advertise you, once for all, that having written several of the foregoing experiments, not only in haste, but at seasons of the year, and in places wherein I could not furnish myself with such instruments, and such a variety of materials, as the design of giving you an introduction into the history of colours required; it can scarce be otherwise, but that divers of the experiments, that I have set down, may afford you some matter of new trials, if you think fit to supply the deficiencies of some of them (especially the freshly mentioned about lakes, and those that concern emphatical colours) which deficiencies, for want of being befriended with accommodations, I could better discern than avoid.

ANNOTATION IV.

THE use of allom is very great as well as familiar in the dyers trade, and I have not been ill pleased with the use I have been able to make of it, in preparing other pigments than those they employ with vegetable juices. But the lucriferous practices of dyers and other tradesmen I do, for reasons that you may know when you please, purposely forbear in this essay, though not strictly from pointing at, yet from making it a part of my present work explicitly and circumstantially to deliver; especially since I now find (though late, and not without some blushes at my prolixity) that what I intended but for a short essay, is already swelled into almost a volume.

EXPERIMENT L.

YET here, *Pyrophilus*, I must take leave to insert an experiment, though perhaps you will think its coming in here an intrusion: for I confess its more proper place would have been among those experiments, that were brought as proofs and applications of our notions concerning the differences of salts: but not having remembered to insert it in its fittest place, I had rather take notice of it in this, than leave it quite unmentioned: partly, because it doth somewhat differ from the rest of our experiments about colours, in the way whereby it is made; and partly because the grounds, upon which I devised it, may hint to you somewhat of the method I use in defining and varying experiments about colours. And upon this account I shall inform you, not only what I did, but why I did it.

I CONSIDERED then, that the work of the former experiments was either to change the colour of a body into another, or quite to destroy it, without giving it a successor; but I had a mind to give you also a way, whereby to turn a body endued with one colour into two bodies, of colours as well as consistencies, very distinct from each other, and that by the help of a body that had itself no colour at all. In order to this, I remembered, that finding the acidity of spirit of vinegar to be wholly destroyed by its working upon minium (or calcined lead) whereby the saline particles of the menstruum have their taste and nature quite altered, I had, among other conjectures I had built upon that change, rightly concluded, that the solution of lead in spirit of vinegar would alter the colour of the juices and infusions of several plants, much after the like manner that I had found oil of tartar to do; and accordingly I was quickly satisfied upon trial, that the infusion of rose-leaves would, by a small quantity of this solution well mingled with it, be immediately turned into a somewhat sad green.

AND further, I had often found, that oil of vitriol, though a potently acid menstruum, will yet precipitate many bodies, both mineral and others, dissolved not only in aqua fortis (as some chymists have observed) but particularly in spirit of vinegar. And I have further found, that the calces or powders precipitated by this liquor were usually fair and white.

LAYING these things together, it was not difficult to conclude, that if, upon a good tincture of red rose-leaves made with fair water, I dropped a pretty quantity of a strong and sweet solution of minium, the liquor would be turned into the like muddy green substance, as I have formerly intimated to you, that oil of tartar would reduce it to; and that if then I added a convenient quantity of good oil of vitriol, this last named liquor would have two distinct operations upon the mixture; the one, that it would precipitate that resolved lead in the form of a white powder; the other, that it would clarify the muddy mixture, and both restore and exceedingly heighten the redness of the infusion of roses, which was the most copious ingredient of the green composition. And accordingly trying the experiment in a wine-glass sharp at the bottom (like an inverted cone) that the subsiding power might seem to take up the more room, and be the more conspicuous, I found, that when I had shaken the green mixture, that the coloured liquor might be the more equally dispersed, a few drops of the rectified oil of vitriol did presently turn the opacous liquor into one that was clear and red, almost like a ruby, and threw down good store of a powder, which, when it was settled, would have appeared very white, if some interspersed particles of the red liquor had not a little allayed the purity, though not blemished

the beauty of the colour. And to shew you, *Pyrophilus*, that these effects do not flow from the oil of vitriol, as it is such, but as it is a strongly acid menstruum, that has the property both to precipitate lead, as well as some other concretes out of spirit of vinegar, and to heighten the colour of red rose-leaves; I add, that I have done the same thing, though perhaps not quite so well, with spirit of salt; and that I could not do it with aqua fortis, because though that potent menstruum does, as well as the others, heighten the redness of roses, yet it would not, like them, precipitate lead out of spirit of vinegar, but would rather have dissolved it, if it had not found it dissolved already.

And as by this way we have produced a red liquor, and a white precipitate out of a dirty green magistery of rose-leaves; so by the same method, you may produce a fair yellow, and sometimes a red liquor, and the like precipitate, out of an infusion of a curious purple colour. For you may call to mind, that in the annotation upon the 39th experiment I intimated to you, that I had with a few drops of an alcali turned the infusion of logwood into a lovely purple. Now if instead of this alcali I substituted a very strong and well filtrated solution of minium, made with spirit of vinegar, and put about half as much of this liquor, as there was of the infusion of logwood (that the mixture might afford a pretty deal of precipitate) the affusion of a convenient proportion of spirit of salt would (if the liquors were well and nimbly stirred together) presently strike down a precipitate like that formerly mentioned, and turn the liquor, that swam above it, for the most part, into a lovely yellow.

But for the advancing of this experiment a little further, I considered, that in case I first turned a spoonful of the infusion of logwood purple, by a convenient proportion of the solution of minium, the affusion of spirit of sal-armoniac would precipitate the corpuscles of lead concealed in the solution of minium, and yet not destroy the purple colour of the liquor; whereupon I thus proceeded: I took about a spoonful of the fresh tincture of logwood (for I found, that if it were stale, the experiment would not always succeed) and having put to it a convenient proportion of the solution of minium to turn it into a deep and almost opacous purple, I then dropped in as much spirit of sal armoniac, as I guessed would precipitate about half or more (but not all) of the lead, and immediately stirring the mixture well together, I mingled the precipitated parts with the others, so that they fell to the bottom, partly in the form of a powder, and partly in the form of a curdled substance, that (by reason of the predominancy of the tinged corpuscles over the white) retained, as well as the supernatant liquor, a blueish purple colour sufficiently deep, and then instantly (but yet warily) pouring on a pretty quantity of spirit of salt; the matter first precipitated was, by the above specified figure of the bottom of the glass preserved from being reached by the spirituous salt; which hastily precipitated upon it a new bed (if I may so call it) of white powder, being the remaining corpuscles of the lead, that the urinous spirit had not struck down. So that there appeared in the glass three distinct and very differingly coloured substances; a purple or violet-coloured precipitate at the bottom, a white and carnation (sometimes a variously coloured) precipitate over that, and at the top of all a transparent liquor of a lovely yellow, or red.

Thus you see, *Pyrophilus*, that though to some I may have seemed to have lighted on this (50th) experiment by chance, and though others may imagine, that to have excogitated it must have proceeded from some extraordinary insight into the nature of colours; yet indeed the devising of it need not be looked upon as any great matter, especially to one, that is a little versed in the notions I have in these, and other papers hinted concerning the differences of salts. And perhaps I might add, upon more than conjecture, that these very notions, and some particulars scatteringly

delivered in this treatise, being skilfully put together, may suggest divers matters (at least) about colours, that will not be altogether despicable. But those hinted, *Pyrophilus*, I must now leave such as you to prosecute, having already spent far more time than I intended to allow myself, in acquainting you with particular experiments and observations concerning the changes of colour; to which I might have added many more, but that I hope I may have presented you with a competent number, to make out, in some measure, what I have, at the beginning of this essay, either proposed as my design in this tract, or delivered as my conjectures concerning these matters. And it not being my present design, as I have more than once declared, to deliver any positive hypothesis or solemn theory of colours, but only to furnish you with some experiments towards the framing of such a theory; I shall add nothing to what I have said already, but a request, that you would not be forward to think I have been mistaken in any thing I have delivered as matter of fact concerning the changes of colours, in case you should not, every time you try it, find it exactly to succeed. For besides the contingencies, to which we have elsewhere shewn some other experiments to be obnoxious, the omission or variation of a seemingly inconsiderable circumstance may hinder the success of an experiment, wherein no other fault has been committed. Of which truth I shall only give you that single and almost obvious, but yet illustrious instance of the art of dying scarlets: for though you should see every ingredient, that is used about it; though I should particularly inform you of the weight of each; and though you should be present at the kindling of the fire, and at the increasing and remitting of it, whenever the degree of heat is to be altered; and though (in a word) you should see every thing done so particularly, that you would scarce harbour the least doubt of your comprehending the whole art; yet if I should not disclose to you, that the vessels, that immediately contain the tinging ingredients, are to be made of or to be lined with tin, you would never be able, by all that I could tell you else (at least, if the famousest and candidest artificers do not strangely delude themselves) to bring your tincture of cochineal to dye a perfect scarlet. So much depends upon the very vessel, wherein the tinging matters are boiled, and so great an influence may an unheeded circumstance have on the success of experiments concerning colours.

A short

A ſhort Account of ſome OBSERVATIONS made by Mr. BOYLE, about a DIAMOND, that ſhines in the Dark,

Firſt incloſed in a LETTER written to a FRIEND:

And now, together with it, annexed to the foregoing Treatiſe, upon the ſcore of the affinity between LIGHT and COLOURS.

A Copy of the LETTER, that Mr. BOYLE wrote to Sir ROBERT MORRAY, to accompany the *Obſervations* touching the *Shining Diamond*.

SIR,

THOUGH Sir *Robert Morray* and Monſieur *Zulichem* be perſons, that have deſerved ſo well of the commonwealth of learning, that I ſhould think myſelf unworthy to be looked upon as a member of it, if I declined to obey them, or to ſerve them; yet I ſhould not without reluctancy ſend you the notes you deſire for him, if I did not hope, that you will tranſmit, together with them, ſome account, why they are not leſs unworthy of his peruſal: which that you may do, I muſt inform you, how the writing of them was occaſioned, which in ſhort was thus. As I was juſt going out of town, hearing that an ingenious gentleman of my acquaintance, lately returned from *Italy*, had a diamond, that being rubbed, would ſhine in the dark, and that he was not far off; I ſnatched time from my occaſions to make him a viſit; but finding him ready to go abroad, and having in vain tried to make the ſtone yield any light in the day-time, I borrowed it of him for that night, upon condition to reſtore it him within a day or two at furtheſt, at *Greſham* College, where we appointed to attend the meeting of the ſociety, that was then to be at that place. And hereupon I haſted that evening out of town, and finding after ſupper, that the ſtone, which in the day-time would afford no diſcernible light, was really conſpicuous in the dark, I was ſo taken with the novelty, and ſo deſirous to make ſome uſe of an opportunity, that was like to laſt ſo little a while, that though at that time I had no body to aſſiſt me but a foot-boy, yet ſitting up late, I made a ſhift that night to try a pretty number of ſuch of the things, that then came into my thoughts, as were not in that place and time unpracticable. And the next day, being otherwiſe employed, I was fain to make uſe of a drowſy part of the night to ſet down haſtily in writing what I had obſerved; and without having the time in the morning to ſtay the tranſcribing of it, I ordered the obſervations to be brought after me to *Greſham* College; where

where you may remember, that they were, together with the stone itself, shown to the Royal Society, by which they had the good fortune not to be disliked, though several things were, through haste, omitted, some of which you will find in the margin of the inclosed paper. The substance of this short narrative I hope you will let Monsieur *Zulichem* know, that he may be kept from expecting any thing of finished in the observations, and be disposed to excuse the want of it. But such as they are I hope they will prove (without a clinch) luciferous experiments, by setting the speculations of the curious on work, in a diligent enquiry after the nature of light, towards the discovery of which, perhaps, they have not yet met with so considerable an experiment; since here we see light produced in a dead and opacous body, and that not as in rotten wood, or in fishes, or as in the Bolonian stone, by a natural corruption, or by a violent destruction of the texture of the body, but by so slight a mechanical operation upon its texture, as we seem to know what it is, and as is immediately performed, and that several ways, without at all prejudicing the body, or making any sensible alterations in its manifest qualities. And I am the more willing to expose my hasty trials to Monsieur *Zulichem*, and to you, because he, being upon the consideration of dioptrics, so odd a phænomenon relating to the subject, as probably he treats of, Light, will, I hope, excite a person to consider it, that is wont to consider things he treats of very well. And for you, Sir, I hope you will both recruit and perfect the observations you receive; for you know, that I cannot add to them, having a good while since restored to Mr. *Clayton* the stone, which, though it be now in the hands of a prince, that so highly deserves, by understanding them, the greatest curiosities; yet he vouchsafes you that access to him, as keeps me from doubting, you may easily obtain leave to make further trials with it, of such a monarch as ours, that is not more inquisitive himself, than a favourer of them that are so. I doubt not but these notes will put you in mind of the motion you made to the society, to impose upon me the task bringing in what I had on other occasions observed concerning shining bodies. But though I deny not, that I sometimes made observations about the Bolonian stone, and tried some experiments about some other shining bodies; yet the same reasons, that reduced me then to be unwilling to receive even their commands, must now be my apology for not answering your expectations, namely, the abstruse nature of light, and my being already overburdened, and but too much kept employed by the urgency of the press, as well as by more concerning and distracting occasions. But yet I will tell you some part of what I have met with in reference to the stone, of which I send you an account. Because I I find, on the one side, that a great many think it no rarity, upon a mistaken persuasion, that not only there are a store of carbuncles, of which this is one; but that all diamonds, and other glistering jewels, shine in the dark. Whereas, on the other side, there are very learned men, who (plausibly enough) deny, that there are any carbuncles or shining stones at all.

AND certainly, those judicious men have much more to say for themselves, than the others commonly plead; and therefore did deservedly look upon Mr. *Clayton*'s diamond as a great rarity. For not only *Boëtius de Boot*, who is judged the best author on this subject, ascribes no such virtue to diamonds, but begins what he delivers of carbuncles, with this passage; *Magna fama est carbunculi. Is vulgo putatur in tenebris carbonis instar lucere; fortassis quia pyrepus seu anthrax appellatus à veteribus fuit. Verum hactenus nemo unquam vere asserere ausus fuit, se gemmam noctu lucentem vidisse. Garcias ab Horto pro regis Indiæ medicus refert se allocutum fuisse, qui se vidisse affirmarent. Sed iis fidem non habuit.* And a later author, the diligent and judicious *Johannes de Laet*,

Boëtius de Boot. Gem. & Lapid. Histor. Lib. 2. Cap. 8.

Laet, in his chapter of carbuncles and of rubies, has this passage; *Quia autem carbunculi, pyropi & anthraces à veteribus nominantur, vulgo creditum fuit, carbonis instar in tenebris lucere, quod tamen nullâ gemmâ hactenus deprehensum, licet à quibusdam temerè jactetur.* And the recentest writer I have met with on this subject, *Olaus Wormius*, in his account of his well-furnished *Museum*, does, where he treats of rubies, concur with the former writers by these words: *Sunt, qui rubinum veterum carbunculum esse existimant, sed deest una illa nota, quod in tenebris instar anthracis non luceat: Ast talem carbunculum in rerum naturâ non inveniri major pars authorum existimant. Licet unum aut alterum in India apud magnates quosdam reperiri scribant, cum tamen ex aliorum relatione id habeant saltem, sed ipsi non viderint.* In confirmation of which I shall only add, that hearing of a ruby, so very vivid, that the jewellers themselves have several times begged leave of the fair lady, to whom it belonged, that they might try their choicest rubies by comparing them with that, I had the opportunity, by the favour of this lady and her husband (both which I have the honour to be acquainted with) to make a trial of this famous ruby in the night, and in a room well darkened, but not only could not discern any thing of light, by looking on the stone before any thing had been done to it, but could not, by all my rubbing, bring it to afford the least glimmering of light.

Musei Wormiani, Cap. 17.

But, Sir, though I be very backward to admit strange things for truths, yet I am not very forward to reject them as impossibilities; and therefore I would not discourage any from making further enquiry, whether or no there be really *in rerum natura* any such thing as a true carbuncle or stone, that without rubbing will shine in the dark. For if such a thing can be found, it may afford no small assistance to the curious in the investigation of light, besides the nobleness and rarity of the thing itself. And though *Vartomannus* was not an eye-witness of what he relates, that the king of *Pegu*, one of the chief kings of the *East-Indies*, had a true carbuncle of that bigness and splendour, that it shined very gloriously in the dark; and though *Garcias ab Horto*, the *Indian* Vice-Roy's physician, speaks of another carbuncle, only upon the report of one, that he discoursed with, who affirmed himself to have seen it; yet as we are not sure, that these men, that gave themselves out to be eye-witnesses, speak true, yet they may have done so for aught we know to the contrary. And I could present you with a much considerabler testimony to the same purpose, if I had the permission of a person concerned, without whose leave I must not do it. I might tell you, that *Marcus Paulus Venetus* (whose supposed fables divers of our later travellers and navigators have since found to be truths) speaking of the king of *Zeilan*, that then was, tells us, that he was said to have the best ruby in the world, a palm long, and as big as a man's arm, without spot, shining like a fire: and he subjoins, that the Great Cham, under whom *Paulus* was a considerable officer, sent and offered the value of a city for it; but the king answered, he would not give it for the treasure of the world, nor part with it, having been his ancestors. And I could add, that in the relation made by two Russian Cossacks of their journey into *Catay*, written to their emperor, they mention their having been told by the people of those parts, that their king had a stone, which lights as the sun both day and night, called in their language *Sarra*, which those Cossacks interpret a ruby. But these relations are too uncertain for me to build any thing upon; and therefore I shall proceed to tell you, that there came hither, about two years since, out of *America*, the governor of one of the principal colonies there, an ancient virtuoso, and one that has the honour to be a member of the Royal Society: this gentleman, finding some of the chief affairs of his country committed to another and me, made me divers

Purchas's Pilgrim, lib. 1. cap. 4. pag. 104.

In the year 1619.

divers visits; and in one of them, when I enquired what rare stones they had in those parts of the *Indies* he belonged to, he told me, that the Indians had a tradition, that in a certain hardly accessible hill, a pretty way up in the country, there was a stone, which in the night-time shined very vividly, and to a great distance; and he assured me, that though he thought it not fit to venture himself so far among those savages, yet he purposely sent thither a bold Englishman, with some natives, to be his guides; and that this messenger brought him back word, that at a distance from the hillock he had plainly perceived such a shining substance as the Indians tradition mentioned; and being stimulated by curiosity, had slighted those superstitious fears of the inhabitants, and with much ado, by reason of the difficulty of the way, had made a shift to clamber up to that part of the hill, where, by a very heedful observation, he supposed himself to have seen the light. But whether it were, that he had mistaken the place, or for some other reason, he could not find it there; though when he was returned to his former station, he did again see the light shining in the same place where it shone before. A further account of this light I expect from the gentleman, that gave me this, who lately sent me the news of his being landed in that country. And though I reserve to myself a full liberty of believing no more than I see cause, yet I do the less scruple to relate this, because a good part of it agrees well enough with another story, that I shall in the next place have occasion to subjoin: in order whereunto, I shall tell you, that though the learned authors I formerly mentioned, tell us, that no writer has affirmed his having himself seen a real carbuncle, yet, considering the light of Mr. *Clayton*'s diamond, it recalled into my mind, that some years before, when I was inquisitive about stones, I had met with an old Italian book highly extolled to me by very competent judges; and that, though the book was very scarce, I had purchased it at a dear rate, for the sake of a few considerable passages I met with in it, and particularly one, which being very remarkable in itself, and pertinent to our present argument, I shall put it for you, though not word for word, which I fear I have forgot to do, yet as to the sense, into English.

'HAVING promised (says our author) to say something of that most precious sort 'of jewels, carbuncles, because they are very rarely to be met with, we shall briefly 'deliver what we know of them. In *Clement* the VIIth's time I happened to see one 'of them at a certain Ragusian merchant's, named *Beigeio di Bona*: this was a car- 'buncle white, of that kind of whiteness, which we said was to be found in those 'rubies, of which we made mention a little above,' (where he had said, that those rubies had a kind of livid whiteness, or paleness, like that of a Calcidonian) 'but it 'had in it a lustre so pleasing, and so marvellous, that it shined in the dark, but not 'as much as coloured carbuncles; though it be true, that in an exceeding dark 'place I saw it shine in the manner of fire almost gone out. But as for coloured 'carbuncles, it has not been my fortune to have seen any: wherefore I will only set 'down what I learned about them, discoursing in my youth with a Roman gentle- 'man of ancient experience in matters of jewels, who told me, that one *Jacopo Cola* 'being by night in a vineyard of his, and espying something in the midst of it, that 'shined like a little glowing coal, at the foot of a vine, went near towards the place, 'where he thought himself to have seen that fire; but not finding it, he said, that 'being returned to the same place, whence he had first descried it, and perceiving 'there the same splendor as before, he marked it so heedfully, that he came at length 'to it, where he took up a very little stone, which he carried away with transports 'and joy. And the next day, carrying it about to show it divers of his friends, 'whilst he was relating after what manner he found it, there casually intervened a Venetian

Benvenuto Cellini nell Arte del Gioiellare, lib. 1. pag. 20.

'Venetian embassador, exceedingly expert in jewels, who presently knowing it to be 'a carbuncle, did craftily, before he and the said *Jacopo* parted (so that there was no 'body present, that understood the worth of so precious a gem) purchase it for the 'value of ten crowns, and the next day left *Rome* to shun the being necessitated to 'restore it; and, as he affirmed, it was known within some while after, that the said 'Venetian gentleman did, in *Constantinople*, sell that carbuncle to the then Grand 'Signior, newly come to the empire, for a hundred thousand crowns.'

And this is what I can say concerning carbuncles; and this is not a little at least as to the first part of this account, where our *Cellini* affirms himself to have seen a real carbuncle with his own eyes, especially since this author appears wary in what he delivers, and is inclined rather to lessen, than increase the wonder of it. And his testimony is the more considerable, because though he were born a subject neither to the pope nor the then king of *France* (that royal virtuoso *Francis* I) yet both the one and the other of those princes employed him much about making of their noblest jewels. What is now reported concerning a shining substance to be seen in one of the islands about *Scotland*, were very improper for me to mention to Sir *Robert Morray*, to whom the first information was originally brought, and from whom I expect a farther (for I scarce dare expect a convincing) account of it. But I must not omit, that some virtuosi questioning me the other day at *Whitehall* about Mr. *Clayton*'s diamond, and meeting amongst them an ingenious Dutch gentleman, whose father was long embassador for the *Netherlands* in *England*, I learned of him, that he is acquainted with a person, whose name he told (but I do not well remember it) who was admiral of the Dutch in the *East-Indies*, and who assured this gentleman, Monsieur *Boreel*, that at his return from thence, he brought back with him into *Holland* a stone, which though it looked but like a pale dull diamond, such as he saw Mr. *Clayton*'s to be, yet was it a real carbuncle; and did without rubbing shine so much, that when the admiral had occasion to open a chest, which he kept under deck in a dark place, where it was forbidden to bring candles for fear of mischances, as soon as he opened the trunk, the stone would, by its native light, shine so as to illustrate a great part of it. And this gentleman having very civilly and readily granted me the request I made him, to write to the admiral, who is yet alive in *Holland* (and probably may still have the jewel by him) for a particular account of this stone, I hope ere long to receive it; which will be the more welcome to me, not only because so unlikely a thing needs a clear evidence, but because I have had some suspicion, that (supposing the truth of the thing, what may be a shining stone in a very hot country, as the *East-Indies*, may perhaps cease to be so (at least in certain seasons) in one as cold as *Holland*. For I observed in the diamond I send you an account of, that not only rubbing, but a very moderate degree of warmth, though excited by other ways, would make it shine a little. And it is not impossible, that there may be stones as much more susceptible than that, of the alterations requisite to make a diamond shine, as that appears to be more susceptible of them than ordinary diamonds. And I confess to you, that this is not the only odd suspicion (for they are not so much as conjectures) that what I tried upon this diamond suggested to me. For not here to entertain you with the changes I think may be effected even in harder sort of stones, by ways not vulgar, nor very promising, because I may elsewhere have occasion to speak of them, and this letter is but too prolix already; that which I shall now acknowledge to you is, that I began to doubt, whether there may not in some cases be some truth in what is said of the right turquois, that it often changes colour as the wearer is sick or well, and manifestly loses its splendour at his death. For when I

 found,

found, that even the warmth of an affriction, that lasted not above a quarter of a minute, nay, that of my body (whose constitution, you know, is none of the hottest) would make a manifest change in the solidest of stones, a diamond; it seemed not impossible, that certain warm and saline steams, issuing from the body of a living man, may by their plenty or paucity, or by their peculiar nature, or by the total absence of them, diversify the colour and the splendour of so soft a stone as the turquois. And though I admired to see, that I know not how many men, otherwise learned, should confidently ascribe to jewels such virtues, as seem no way compatible to inanimate agents, if to any corporeal ones at all; yet as to what is affirmed concerning the turquois's changing colour, I know not well how to reject the affirmation of so learned (and which in this case is much more considerable) so judicious a lapidary as *Boetius de Boot* *, who upon his own particular and repeated experience delivers so memorable a narrative of the turquois's changing colour, that I cannot but think it worth your perusal; especially since a much later and very experienced author, *Olaus Wormius*, where he treats of that stone, confirms it with this testimony: *Imprimis memorandum exemplum, quod Anshelmus Boëtius de seipso refert, tam mutati coloris quam à casu preservationis. Cui & ipse haud dissimile adferre possum, nisi ex Anshelmo petitum quis putaret.* I remember, that I saw two or three years since a turquois (worn in a ring) wherein there were some small spots, which the virtuoso, whose it was, assured me he had observed to grow sometimes greater, sometimes less, and to be sometimes in one part of the stone, sometimes in another. And I having encouraged to make pictures from time to time of the stone and of the situation of the cloudy parts, that so their motion may be more indisputable, and better observed, he came to me about the middle of this very week, and assured me, that he had, as I wished, made from time to time schemes or pictures of the differing parts of the stone; whereby the several removes and motions of the above mentioned clouds are very manifest, though the cause seemed to him very occult. These pictures he has promised to shew me, and is very ready to put the stone itself into my hands. But the ring having been the other day casually broken upon his finger, unless it can be taken out, and set again without any considerable heat, he is loth to have it meddled with, for fear its peculiarity should be thereby destroyed. And possibly his apprehension would have been strengthened, if I had had opportunity to tell him what is related by the learned *Wormius* of an acquaintance of his, that had a nephritic stone, of whose eminent virtues he had often experience even in himself, and for that cause wore it still about his wrist; and yet going upon a time into a bath of fair water only wherein certain herbs had been boiled, the stone, by being

Olaus Wormius in Mus. 2d pag. 256.

Museum Worm. pag. 99.

* *The narrative in the author's own words, is this:* Ego *(says he)* sanctè affirmare possum me unam aureo annulo inclusam perpetuo gestare, cujus facultatem (si gemma est) nunquam satis admirari potui. Gestaverat enim ante triginta annos Hispanus quidem non procul à paternis ædibus habitans. Is cum vitâ functus esset, & ipsius supellex (ut moris apud nos est) venum exposita esset, inter cætera etiam turcois exponebatur. Verum nemo (licet complures eo concorrissent, ut eam propter coloris elegantiam, quam vivo domino habuerat, emerent) sibi emptam voluit, pristinum enim nitorem & colorem prorsus amiserat, ut potius malachites, quam turcois videretur. Aderat tum temporis gemmæ habendæ desiderio etiam parens & frater meus, qui antea sæpius gratiam & elegantiam ipsius viderant, mirabundi eam nunc tam esse deformem: emit eam nihilominus pater, satisque vili pretio, qua omnibus contemptui erat, ac præsentes non eam esse quam Hispanus gestarat, arbitrarentur. Domum reversus pater, qui tam turpem gemmam gestare sibi indecorum putabat, eam mihi dono dat, inquiens; Quandoquidem, fili mi, vulgi fama est, turcoidem, ut facultates suas exercere possit, dono dari debere, tibi eam devoveo: ego acceptam gemmam sculptori trado, ut gentilitia mea insignia illi, quemadmodum fieri solet, in jaspide chalcedonio, aliisque ignobilioribus gemmis, insculperet. Torpe enim existimabam, hujusmodi gemmâ ornatus gratia, dum gratiam nullam haberet, uti. Paret sculptor, redditque gemmam, quam gesto pro annulo signatorio. Vix per mensem gestaram, redit illi pristinus color, sed non ita nitens propter sculpturam, ac inæqualem superficiem. Miramur omnes gemmam, atque id præcipuè, quod color indies pulchrior fieret. Id quia observabam, nunquam fere eam à manu deposui, ita ut nunc adhuc eandem gestem.

wetted

wetted with this decoction, was deprived of all its virtue; whence *Wormius* takes occaſion to advertiſe the ſick, to lay by ſuch ſtones, whenſoever they make uſe of a bath. And we might expect to find turquois likewiſe, eaſily to be wrought upon in point of colour, if that were true, which the curious *Antonino Neri*, in his ingenious *Arte Vetraria*, teaches of it; namely, that turquoiſes diſcoloured, and grown white, will regain and acquire an excellent colour, if you but keep them two or three days at moſt covered with oil of ſweet almonds, kept in a temperate heat by warm aſhes: I ſay, if it were true, becauſe I doubt whether it be ſo, and have not as yet had opportunity to ſatisfy myſelf by trials; becauſe I find, by the confeſſion of the moſt ſkilful perſons, among whom I have laid out for turquoiſes, that the true ones are great rarities, though others be not at all ſo. And therefore I ſhall now only mind you of one thing, that you know as well as I, namely, that the rare ſtone, which is called *Oculus Mundi*, if it be good in its kind, will have ſo great a change made in its texture by being barely left a while in the languideſt of liquors, common water, that from opacous it will become tranſparent, and acquire a luſtre, of which it will again be deprived, without uſing any other art or violence, by leaving it a while in the air. And before experience had ſatisfied us of the truth of this, it ſeemed as unlikely, that common water or air ſhould work ſuch great changes in that gem, as it now ſeems that the effluviums of a human body ſhould effect leſſer changes in a turquois, eſpecially if more ſuſceptible of them, than other ſtones of the ſame kind. But both my watch and my eyes tell me, that it is now high time to think of going to ſleep; matters of this nature will be better, as well as more eaſily, cleared by conference than writing. And therefore ſince I think you know me too well to make it needful for me to diſclaim credulity, notwithſtanding my having entertained you with all theſe extravagancies; for you know well, how wide a difference I am wont to put betwixt things that barely may be, and things that are; and between thoſe relations that are but not unworthy to be enquired into, and thoſe that are not worthy to be actually believed; without making apologies for my ravings, I ſhall readily comply with the drowſineſs, that calls upon me to releaſe you: and the rather becauſe Monſieur *Zulichem* being concerned in your deſire to know the few things I have obſerved about the ſhining ſtone; to entertain thoſe with ſuſpicions, that are accuſtomed not to acquieſce but in demonſtrations, were a thing, that cannot be looked upon as other than very improper by

Arte Vetraria, lib. 7. cap. 102.

SIR,

Your moſt affectionate and

moſt faithful Servant,

R. BOYLE.

OBSERVATIONS made * this 27th of *October*, 1663, about Mr. *Clayton*'s Diamond.

BEING looked on in the † day-time, though in a bed, whoſe curtains were carefully drawn, I could not diſcern it to ſhine at all, though well rubbed; but about a little after ſun-ſet, whilſt the twilight yet laſted, nay, this morning a pretty while after ſun-riſing (but before I had been abroad in the more freely enlightened air of the chamber) I could upon a light affriction eaſily perceive the ſtone to ſhine.

2. THE candles being removed, I could not in a dark place diſcern the ſtone to have any light, when I looked on it, without having rubbed or otherwiſe prepared it.

3. BY two white pebbles, though hard rubbed, one againſt another, nor by the long and vehement affriction of rock cryſtal againſt a piece of red cloth, nor yet by rubbing two diamonds ſet in a ring, as I had rubbed this ſtone, I could produce any ſenſible degree of light.

4. I FOUND this diamond hard enough, not only to enable me to write readily with it upon glaſs, but to grave on rock-cryſtal itſelf.

5. I FOUND ‡ this to have, like other diamonds, an electrical faculty.

6. BEING rubbed upon my clothes, as is uſual for the exciting of amber, wax, and other electrical bodies, it did in the dark manifeſtly ſhine like rotten wood, or the ſcales of whitings, or other putrified fiſh.

7. BUT this conſpicuouſneſs was fainter than that of the ſcales and ſlabber (if I may ſo call it) of whitings, and much fainter than the light of a glow-worm, by which I have been ſometimes able to read a ſhort word; whereas after an ordinary affriction of this diamond, I was not able to diſcern diſtinctly by the light of it any of the neareſt bodies. And this glimmering alſo did very manifeſtly and conſiderably decay preſently, upon the ceaſing of the affriction, though the ſtone continued viſible ſome while after.

8. BUT if it were rubbed upon a convenient body for a pretty while, and briſkly enough, I found the light would be for ſome moments much more conſiderable, almoſt like the light of a glow-worm; inſomuch that after I ceaſed rubbing, I could with the chafed ſtone exhibit a little luminous circle, like that, but not ſo bright as that, which children make by moving a ſtick fired at the end: and in this caſe it would continue viſible about ſeven or eight times as long as I had been in rubbing it.

* Theſe were brought in and read before the Royal Society, (the day following) Oct. 28, 1663.

The ſtone itſelf being to be ſhown to the Royal Society, when the obſervations were delivered, I was willing (being in haſte) to omit the deſcription of it; which is in ſhort, that it was a flat or table diamond, of about a third part of an inch in length, and ſomewhat leſs in breadth; that it was a dull ſtone, and of a very bad water, having in the day-time very little of the vividneſs of even ordinary diamonds, and being blemiſhed with a whitiſh cloud about the middle of it, which covered near a third part of the ſtone

† Haſte made me forget to take notice, that I went abroad the ſame morning, the ſun ſhining forth clear enough, to look upon the diamond through a microſcope, that I might try whether by that magnifying glaſs any thing of peculiar could be diſcerned in the texture of the ſtone, and eſpecially of the whitiſh cloud, that poſſeſt a good part of it. But for all my attention I could not diſcover any peculiarity worth mentioning.

‡ 5. For it drew light bodies like amber, jet, and other concretes, that are noted to do ſo: but its attractive power ſeemed inferior to theirs.

9. I FOUND

9. I FOUND, * that holding it a while near the flame of a candle (from which yet I was careful to avert my eyes) and being immediately removed into the dark, it diſcloſed ſome faint glimmering, but inferior to that it was wont to acquire by rubbing. And afterward holding it near a fire, that had but little flame, I found the ſtone to be rather leſs than more excited, than it had been by the candle.

10. I LIKEWISE endeavoured to make it ſhine, by holding it a pretty while in a very dark place, over a thick piece of iron, that was well heated, but not to that degree as to be viſibly ſo. And though at length I found, that by this way alſo the ſtone acquired ſome glimmering, yet it was leſs than by either of the other ways abovementioned.

11. I ALSO brought it to ſome kind of glimmering light, by taking it into bed with me, and holding it a good while upon a warm part of my naked body.

12. To ſatisfy myſelf, whether the motion introduced into the ſtone did generate the light upon the account of its producing heat there, I held it near the flame of a candle, till it was qualified to ſhine pretty well in the dark; and then immediately I applied a ſlender hair, to try whether it would attract it, but found not that it did ſo: though if it were made to ſhine by rubbing, it was, as I formerly noted, electrical. And for further confirmation, though I once purpoſely kept it ſo near the hot iron I juſt now mentioned, as to make it ſenſibly warm, yet it ſhined more dimly than it had done by affriction, or the flame of a candle, though by both thoſe ways it had not acquired any warmth that was ſenſible.

13. HAVING purpoſely rubbed it upon ſeveral bodies differing as to colour and as to texture, there ſeemed to be ſome little diſparity in the excitation (if I may ſo call it) of light. Upon white and red cloaths it ſeemed to ſucceed beſt, eſpecially in compariſon of black ones.

14. BUT to try what it would do rubbed upon bodies more hard, and leſs apt to yield heat, upon a light affriction, than cloth, I firſt rubbed it upon a white wooden box, by which it was excited, and afterwards upon a piece of purely glazed earth; which ſeemed, during the attrition, to make it ſhine better than any of the other bodies had done, without excepting the white ones; which I add, leſt the effect ſhould be wholly aſcribed to the diſpoſition white bodies are wont to have to reflect much light.

15. HAVING † well excited the ſtone, I nimbly plunged it under water, that I had provided for that purpoſe, and perceived it to ſhine whilſt it was beneath the ſurface of that liquor, and this I did divers times. But when I endeavoured to produce a light by rubbing it upon the lately mentioned cover of the box, the ſtone and it being both held beneath the ſurface of the water, I did not well ſatisfy myſelf in the event of the trial: but this I found, if I took the ſtone out, and rubbed it upon a piece of cloth, it would not, as elſe it was wont to do, preſently acquire a luminouſneſs, but needed to be rubbed manifeſtly much longer, before the deſired effect was found.

16. I ALSO ‡ tried ſeveral times, that by covering it with my warm ſpittle (having no warm water at hand) it did not loſe its light.

17. FINDING

* 9. We durſt not hold it in the flame of a candle, no more than put it into a naked fire; for fear too violent a heat (which has been obſerved to ſpoil many other precious ſtones) ſhould vitiate and impair a jewel, that was but borrowed, and was ſuppoſed to be the only one of its kind.

† 15. We likewiſe plunged it, as ſoon as we had excited it, under liquors of ſeveral ſorts, as ſpirit of wine, oils both chymical and expreſſed, an acid ſpirit, and, as I remember, an alcalizate ſolution; and found not any of thoſe various liquors to deſtroy its ſhining property.

‡ 16. Having found by this obſervation, that a warm liquor would not extinguiſh light in the diamond, I thought fit to try, whether, by reaſon of its warmth, it would not excite it; and divers times I found, that

17. FINDING that by rubbing the ſtone with the flat ſide downwards, I did, by reaſon of the opacity of the ring, and the ſudden decay of light upon the ceaſing of the attrition, probably loſe the ſight of the ſtone's greateſt vividneſs; and ſuppoſing that the commotion made in one part of the ſtone will be eaſily propagated all over; I ſometimes held the piece of cloth, upon which I rubbed it, ſo that one ſide of the ſtone was expoſed to my eye, whilſt I was rubbing the other; whereby it appeared more vivid than formerly, and to make luminous tracts by its motions to and fro. And ſometimes holding the ſtone upwards, I rubbed its broad ſide with a fine ſmooth piece of tranſparent horn, by which means the light through that diaphanous ſubſtance did, whilſt I was actually rubbing the ſtone, appear ſo briſk, that ſometimes, and in ſome places, it ſeemed to have little ſparks of fire.

18. I TOOK alſo a piece of flat blue glaſs, and having rubbed the diamond well upon a cloth, and nimbly clapt the glaſs upon it, to try whether, in caſe the light could pierce it, it would by appearing green, or of ſome other colour than blue, aſſiſt me to gueſs, whether itſelf were ſincere or no. But finding the glaſs impervious to ſo faint a light, I then thought it fit to try, whether hard bodies would not by attrition increaſe the diamond's light, ſo as to become penetrable thereby: and accordingly when I rubbed the glaſs briſkly upon the ſtone, I found the light to be conſpicuous enough, and ſomewhat dyed in its paſſage; but found it not eaſy to give a name to the colour it exhibited.

LASTLY, To comply with the ſuſpicion I had upon the whole matter, that the chief manifeſt change wrought in the ſtone was by compreſſion of its parts, rather than incaleſcence; I took a piece of white tile well glazed, and if I preſſed the ſtone hard againſt it, it ſeemed, though I did not rub it to and fro, to ſhine at the ſides. And however it did both very manifeſtly and vigorouſly ſhine, if, whilſt I ſo preſſed it, I moved it any way upon the ſurface of the tile, though I did not make it draw a line of above a quarter of an inch long, or thereabouts, and though I made it not move to and fro, but only from one end of the ſhort line to the other, without any return or lateral motion. Nay, after it had been often rubbed, and ſuffered to loſe its light again, not only it ſeemed more eaſy to be excited than at the beginning of the night; but if I did preſs hard upon it with my finger, at the very inſtant that I drew it briſkly off, it would diſcloſe a very vivid but exceeding ſhort-lived ſplendor, not to call it a little coruſcation *. So that a Carteſian would ſcarce ſcruple to think, he had found in this ſtone no ſlight confirmation of his ingenious maſter's hypotheſis, touching the generation of light in ſublunary bodies, not ſenſibly hot.

that if it were kept therein, till the water had leiſure to communicate ſome of its heat to it, it would often ſhine as ſoon as it was taken out; and probably we ſhould have ſeen it ſhine more, whilſt it was in the water, if ſome degree of opacity, which heated water is wont to acquire, upon the ſcore of the numerous little bubbles generated in it, had not kept us from diſcerning the luſtre of the ſtone.

* I after bethought myſelf of employing a way, which produced the deſired effect, both ſooner and better. For holding betwixt my fingers a ſteel bodkin, near the lower part of it, I preſſed the point hard againſt the ſurface of the diamond, and much more if I ſtruck the point againſt it, the coruſcation would be extremely ſudden, and very vivid, though very vaniſhing too: and this way, which commonly much ſurprized and pleaſed the ſpectators, ſeemed far more proper than the other, to ſhew, that preſſure alone, if forcible enough, though it were ſo ſudden and ſhort, that it could not well be ſuppoſed to give the ſtone any thing near a ſenſible degree of warmth, as may be ſuſpected of rubbing, yet it is ſufficient to generate a very vivid light.

A POST-

A POSTSCRIPT, *annexed ſome Hours after the* Obſervations *were written.*

SO * many particulars taken notice of in one night, may make this ſtone appear a kind of prodigy; and the rather, becauſe having tried, as I formerly noted, not only a fine artificial cryſtal, and ſome alſo that is natural, but a ruby and two diamonds, I did not find, that any of theſe diſcloſed the like glimmering of light; yet after all, perceiving by the hardneſs, and the teſtimony of a ſkilful goldſmith, that this was rather a natural than artificial ſtone; for fear leſt there might be ſome difference in the way of ſetting, or in the ſhape of the diamonds I made uſe of, neither of which was like this, a flat table-ſtone, I thought fit to make a further trial of my own diamonds, by ſuch a briſk and aſſiduous affriction, as might make amends for the diſadvantages above-mentioned, in caſe they were the cauſe of the unſucceſsfulneſs of the former attempts. And accordingly I found, that by this way I could eaſily bring a diamond I wore on my finger to diſcloſe a light, that was ſenſible enough, and continued ſo, though I covered it with ſpittle, and uſed ſome other trials about it. And this will much leſſen the wonder of all the formerly mentioned obſervations, by ſhewing, that the properties, that are ſo ſtrange, are not peculiar to one diamond, but may be found in others alſo, and, perhaps, in divers other hard and diaphanous ſtones. Yet I hope, that what this diſcovery takes away from the wonder of theſe obſervations, it will add to the inſtructiveneſs of them, by affording pregnant hints towards the inveſtigation of the nature of light.

* We afterwards tried precious ſtones, as diamonds, rubies, ſaphires, and emeralds, &c. but found not any of them to ſhine, except ſome diamonds; and of theſe we were not, upon ſo little practice, able to foretel beforehand, which would be brought to ſhine, and which would not: for ſeveral very good diamonds either would not ſhine at all, or much leſs than others, that were far inferior to them. And yet thoſe ingenious men are miſtaken, that think a diamond muſt be foul and cloudy, as Mr. *Clayton*'s was, to be fit for ſhining; for as we could bring ſome ſuch to afford a glimmering light, ſo with ſome clear and excellent diamonds we could do the like. But none of thoſe many, that we tried of all kinds, were equal to the diamond, on which the obſervations were made, not only conſidering the degree of light it afforded, but the eaſineſs wherewith it was excited, and the comparatively great duration of its ſhining.

End of the FIRST VOLUME.

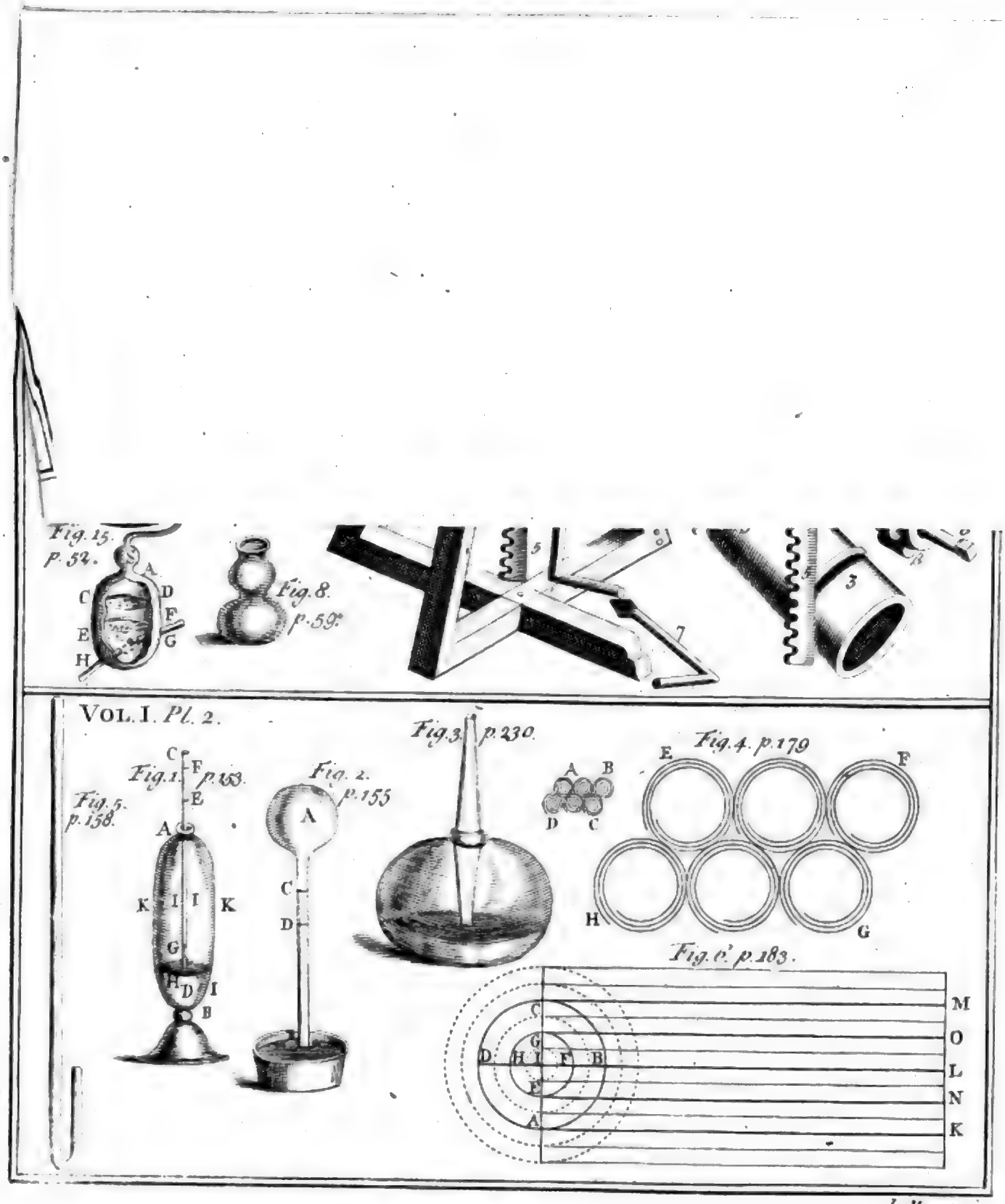
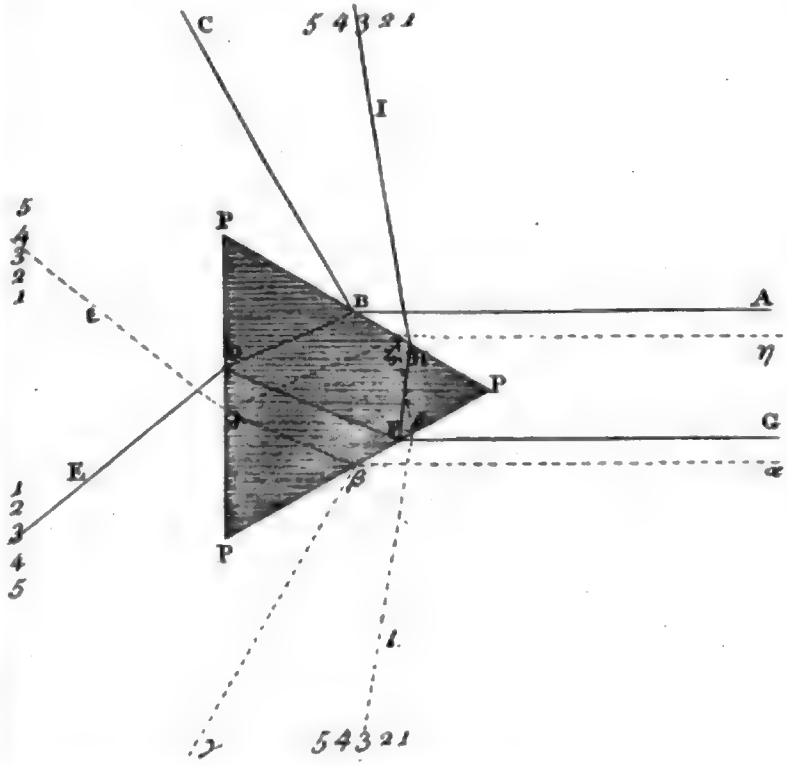


Fig. 15.
p. 52.
Fig. 8.
p. 59.
VOL. I. Pl. 2.
Fig. 1. p. 153.
Fig. 5.
p. 158.
Fig. 2.
p. 155
Fig. 3. p. 230.
Fig. 4. p. 179
Fig. 6. p. 183.

J. Mynde.



The Explication of the Scheme

PPP. *An Æquilaterotriangular Crystalline Prism, one of whose edges P. is placed directly towards the Sun*

AB & αβ *Two rays from the Sun falling on the Prism at* B β. *and thence partly reflected towards* C & γ. *and partly refracted towards* D & δ

BC & βγ. *Those reflected Rays*

BD & βδ. *Those refracted Rays which are partly refracted towards* E & ε. *and there paint an Iris* 12345. *denoting the five consecutions of colours Red, Yellow, Green, Blew, and Purple; & are partly reflected towards* F & ζ.

DF & δζ. *Those Reflected Rays which are partly refracted towards* G & η. *colourless, and partly reflected, towards* H & θ.

FH & ζθ. *Those Reflected Rays which are refracted towards* I & ι *and there paint an other fainter Iris, the colours of which are contrary to the former* 5 4 3 2 1. *signifying Purple, Blew, Green, Yellow, Red, so that the Prism in this posture exhibits four Rainbows.*