LIFE

AND

LETTERS

OF

CHARLES

DARWIN.

VOL. I.

LONDON:

JOHN MURRAY.

© The Complete Work of Charles Darwin Online

BOUND BY WILSON

CAMBRIDGE PHILOSOPHICAL
LIBRARY.

Deposited by Prof. Newton
1901.

D4 DAR Dar

BALFOUR & NEWTON LIBRARY
2L7EN 027929191

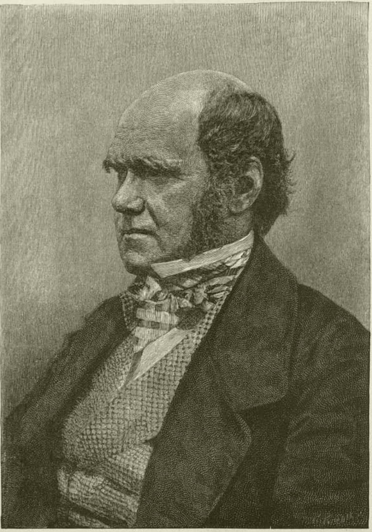
© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

PHILOSOPHICAL LIBRARY
2 AU 1901
CAMBRIDGE

© The Complete Work of Charles Darwin Online



FROM A PHOTOGRAPH (1854?) BY MESSRS. MAULL AND FOX. ENGRAVED FOR 'HARPER'S MAGAZINE,' OCTOBER 1884.

Frontispiece, Vol. I.

© The Complete Work of Charles Darwin Online

THE

LIFE AND LETTERS

OF

CHARLES DARWIN,

INCLUDING

AN AUTOBIOGRAPHICAL CHAPTER.

PHILOSOPHICAL LIBRARY
17 OC 1901
CAMBRIDGE

EDITED BY HIS SON,

FRANCIS DARWIN.

IN THREE VOLUMES:—VOL. I.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1887.

All Rights Reserved.

© The Complete Work of Charles Darwin Online

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.

© The Complete Work of Charles Darwin Online

PREFACE.

In choosing letters for publication I have been largely guided by the wish to illustrate my father's personal character. But his life was so essentially one of work, that a history of the man could not be written without following closely the career of the author. Thus it comes about that the chief part of the book falls into chapters whose titles correspond to the names of his books.

In arranging the letters I have adhered as far as possible to chronological sequence, but the character and variety of his researches make a strictly chronological order an impossibility. It was his habit to work more or less simultaneously at several subjects. Experimental work was often carried on as a refreshment or variety, while books entailing reasoning and the marshalling of large bodies of facts were being written. Moreover, many of his researches were allowed to drop, and only resumed after an interval of years. Thus a rigidly chronological series of letters would present a patchwork of subjects, each of which would be difficult to follow. The Table of Contents will show in what way I have attempted to avoid this result. It will be seen, for instance, that the second

© The Complete Work of Charles Darwin Online

volume is not chronologically continuous with the first. Again, in the third volume, the botanical work, which principally occupied my father during the later years of his life, is treated in a separate series of chapters.

In printing the letters I have followed (except in a few cases) the usual plan of indicating the existence of omissions or insertions. My father's letters give frequent evidence of having been written when he was tired or hurried. In a letter to a friend, or to one of his family, he frequently omitted the articles: these have been inserted without the usual indications, except in a few instances (*e.g.* Vol. I. p. 203), where it is of special interest to preserve intact the hurried character of the letter. Other small words, such as *of*, *to*, &c., have been inserted, usually within brackets. My father underlined many words in his letters; these have not always been given in italics,—a rendering which would have unfairly exaggerated their effect. I have not followed the originals as regards the spelling of names, the use of capital letters, or in the matter of punctuation.

The Diary or Pocket-book, from which quotations occur in the following pages, has been of value as supplying a framework of facts round which letters may be grouped. It is unfortunately written with great brevity, the history of a year being compressed into a page or less, and contains little more than the dates of the principal events of his life, together with entries as to his work, and as to the duration of his more serious illnesses. He rarely dated his letters, so that but for the Diary it would have been all but impossible to unravel the history of his books. It has also enabled me to assign dates to many letters which would otherwise have been shorn of half their value.

© The Complete Work of Charles Darwin Online

Of letters addressed to my father I have not made much use. It was his custom to file all letters received, and when his slender stock of files ("spits" as he called them was exhausted, he would burn the letters of several years, in order that he might make use of the liberated "spits." This process, carried on for years, destroyed nearly all letters received before 1862. After that date he was persuaded to keep the more interesting letters, and these are preserved in an accessible form.

I have attempted to give, in Chapter III., some account of his manner of working. During the last eight years of his life I acted as his assistant, and thus had an opportunity of knowing something of his habits and methods.

I have received much help from my friends in the course of my work. To some I am indebted for reminiscences of my father, to others for information, criticisms, and advice. To all these kind coadjutors I gladly acknowledge my indebtedness. The names of some occur in connection with their contributions, but I do not name those to whom I am indebted for criticisms or corrections, because I should wish to bear alone the load of my short-comings, rather than to let any of it fall on those who have done their best to lighten it.

It will be seen how largely I am indebted to Sir Joseph Hooker for the means of illustrating my father's life. The readers of these pages will, I think, be grateful to Sir Joseph for the care with which he has preserved his valuable collection of letters, and I should wish to add my acknowledgment of the generosity with which he has placed it at my disposal, and for the kindly encouragement given throughout my work.

To Mr. Huxley I owe a debt of thanks, not only for much kind help, but for his willing compliance with my request that

© The Complete Work of Charles Darwin Online

he should contribute a chapter on the reception of the 'Origin of Species.'

Finally, it is a pleasure to acknowledge the courtesy of the publishers of the 'Century Magazine' and of 'Harper's Magazine,' who have freely given me the use of their illustrations. To Messrs. Maull and Fox and Messrs. Elliott and Fry I am also indebted for their kindness in allowing me the use of reproductions of their photographs.

FRANCIS DARWIN.

CAMBRIDGE,
October, 1887.

© The Complete Work of Charles Darwin Online

TABLE OF CONTENTS.

VOLUME I.

LETTERS.

VOLUME II.

© The Complete Work of Charles Darwin Online

VOLUME III.

© The Complete Work of Charles Darwin Online

BOTANICAL LETTERS.

APPENDICES.

© The Complete Work of Charles Darwin Online

ILLUSTRATIONS.

ERRATA.

VOLUME I.

P. 367, line 25 : *for* "Montague" *read* "Montagu."

VOLUME II.

P. 239, line 17 : *for* "[?]" *read* "E. R." The surmise given in the footnote is incorrect. It appears from papers in the possession of Mr. J. Estlin Carpenter, that Dr. Carpenter urged on the Editor of the 'Edinburgh Review' a purely scientific treatment of the 'Origin of Species.'

P. 246, note : *for* "Ichthyology" *read* "Ichnology."

P. 280, line 22 : *for* "Crampton" *read* "Crompton."

P. 356, line 6 : *for* "[illegible]000" *read* "2000."

P. 380, line 3 from foot : *for* "in the Amazons" *read* "on the Amazons."

P. 390, line 4 : *for* "direct in the" *read* "in the direct."

VOLUME III.

P. 40, line 13 : *for* "Magazines" *read* "Magazine."

P. 46, note, last line : *for* "contemporaine" *read* "contemporain."

P. 58, line 8 : *for* "laburnums, Adami-trifacial" *read* "laburnum Adami, trifacial.'

© The Complete Work of Charles Darwin Online

LIFE AND LETTERS

OF

CHARLES DARWIN.

CHAPTER I.

THE DARWIN FAMILY.

THE earliest records of the family show the Darwins to have been substantial yeomen residing on the northern borders of Lincolnshire, close to Yorkshire. The name is now very unusual in England, but I believe that it is not unknown in the neighbourhood of Sheffield and in Lancashire. Down to the year 1600 we find the name spelt in a variety of ways —Derwent, Darwen, Darwynne, &c. It is possible, therefore, that the family migrated at some unknown date from Yorkshire, Cumberland, or Derbyshire, where Derwent occurs as the name of a river.

The first ancestor of whom we know was one William Darwin, who lived, about the year 1500, at Marton, near Gainsborough. His great grandson, Richard Darwyn, inherited land at Marton and elsewhere, and in his will, dated 1584, "bequeathed the sum of 3*s*. 4*d*. towards the settynge up of the Queene's Majestie's armes over the quearie (choir) doore in the parishe churche of Marton." *

The son of this Richard, named William Darwin, and described as "gentleman," appears to have been a successful

* We owe a knowledge of these earlier members of the family to researches amongst the wills at Lincoln, made by the well-known genealogist, Colonel Chester.

© The Complete Work of Charles Darwin Online

man. Whilst retaining his ancestral land at Marton, he acquired through his wife and by purchase an estate at Cleatham, in the parish of Manton, near Kirton Lindsey, and fixed his residence there. This estate remained in the family down to the year 1760. A cottage with thick walls, some fish-ponds and old trees, now alone show where the "Old Hall" once stood, and a field is still locally known as the "Darwin Charity," from being subject to a charge in favour of the poor of Marton. William Darwin must, at least in part, have owed his rise in station to his appointment in 1613 by James I. to the post of Yeoman of the Royal Armoury of Greenwich. The office appears to have been worth only £33 a year, and the duties were probably almost nominal; he held the post down to his death during the Civil Wars.

The fact that this William was a royal servant may explain why his son, also named William, served when almost a boy for the King, as "Captain-Lieutenant" in Sir William Pelham's troop of horse. On the partial dispersion of the royal armies, and the retreat of the remainder to Scotland, the boy's estates were sequestrated by the Parliament, but they were redeemed on his signing the Solemn League and Covenant, and on his paying a fine which must have struck his finances severely; for in a petition to Charles II. he speaks of his almost utter ruin from having adhered to the royal cause.

During the Commonwealth, William Darwin became a barrister of Lincoln's Inn, and this circumstance probably led to his marriage with the daughter of Erasmus Earle, serjeant-at-law; hence his great-grandson, Erasmus Darwin, the Poet, derived his Christian name. He ultimately became Recorder of the city of Lincoln.

The eldest son of the Recorder, again called William, was born in 1655, and married the heiress of Robert Waring, a member of a good Staffordshire family. This lady inherited from the family of Lassells, or Lascelles, the manor and hall of Elston, near Newark, which has remained ever since in the

© The Complete Work of Charles Darwin Online

family.* A portrait of this William Darwin at Elston shows him as a good-looking young man in a full-bottomed wig.

This third William had two sons, William, and Robert who was educated as a barrister. The Cleatham property was left to William, but on the termination of his line in daughters reverted to the younger brother, who had received Elston. On his mother's death Robert gave up his profession and resided ever afterwards at Elston Hall. Of this Robert, Charles Darwin writes †:—

"He seems to have had some taste for science, for he was an early member of the well-known Spalding Club; and the celebrated antiquary Dr. Stukeley, in 'An Account of the almost entire Sceleton of a large Animal,' &c., published in the 'Philosophical Transactions,' April and May 1719, begins the paper as follows: 'Having an account from my friend, Robert Darwin, Esq., of Lincoln's Inn, a person of curiosity, of a human sceleton impressed in stone, found lately by the rector of Elston,' &c. Stukeley then speaks of it as a great rarity, 'the like whereof has not been observed before in this island to my knowledge.' Judging from a sort of litany written by Robert, and handed down in the family, he was a strong advocate of temperance, which his son ever afterwards so strongly advocated:—

From a morning that doth shine,
From a boy that drinketh wine,
From a wife that talketh Latine,
Good Lord deliver me!

* Captain Lassells, or Lascelles, of Elston was military secretary to Monk, Duke of Albemarle, during the Civil Wars. A large volume of account-books, countersigned in many places by Monk, are now in the possession of my cousin Francis Darwin. The accounts might possibly prove of interest to the antiquarian or historian. A portrait of Captain Lassells in armour, although used at one time as an archery-target by some small boys of our name, was not irretrievably ruined.

† What follows is quoted from Charles Darwin's biography of his grandfather, forming the preliminary notice to Ernst Krause's interesting essay, 'Erasmus Darwin,' London, 1879, p. 4.

© The Complete Work of Charles Darwin Online

"It is suspected that the third line may be accounted for by his wife, the mother of Erasmus, having been a very learned lady. The eldest son of Robert, christened Robert Waring, succeeded to the estate of Elston, and died there at the age of ninety-two, a bachelor. He had a strong taste for poetry, like his youngest brother Erasmus. Robert also cultivated botany, and, when an oldish man, he published his 'Principia Botanica.' This book in MS. was beautifully written, and my father [Dr. R. W. Darwin] declared that he believed it was published because his old uncle could not endure that such fine caligraphy should be wasted. But this was hardly just, as the work contains many curious notes on biology — a subject wholly neglected in England in the last century. The public, moreover, appreciated the book, as the copy in my possession is the third edition."

The second son, William Alvey, inherited Elston, and transmitted it to his granddaughter, the late Mrs. Darwin, of Elston and Creskeld. A third son, John, became rector of Elston, the living being in the gift of the family. The fourth son, and youngest child, was Erasmus Darwin, the poet and philosopher.

The table on page 5 shows Charles Darwin's descent from Robert, and his relationship to some other members of the family, whose names occur in his correspondence. Among these are included William Darwin Fox, one of his earliest correspondents, and Francis Galton, with whom he maintained a warm friendship for many years. Here also occurs the name of Francis Sacheverel Darwin, who inherited a love of natural history from Erasmus, and transmitted it to his son Edward Darwin, author (under the name of "High Elms") of a 'Gamekeeper's Manual' (4th Edit. 1863), which shows keen observation of the habits of various animals.

It is always interesting to see how far a man's personal characteristics can be traced in his forefathers. Charles Darwin inherited the tall stature, but not the bulky figure of

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

TABLE OF RELATIONSHIP.

- ROBERT DARWIN of Elston. b. 1682, d. 1754.
  - William Alvey. b. 1726, d. 1783.
    - William Brown. b. 1774, d. 1841.
      - Charlotte, m. Francis Rhodes, now Francis Darwin of Creskeld and Elston.
      - Sarah, m. Edward Noel.
    - Anne m. Samuel Fox.
      - William Darwin Fox.
  - Robert Waring. b. 1724, d. 1816.
  - ERASMUS, b. 1731, d. 1802. m. (1) Mary Howard; b. 1740, d. 1770. m. (2) Eliz. Chandos-Pole. b. 1747, d. 1832.
    - (by Mary Howard) ROBERT WARING. b. 1766, d. 1848, m. Susannah Wedgwood.
      - CHARLES ROBERT DARWIN. b. Feb. 12, 1809, d. Apr. 19, 1882.
    - (by Mary Howard) Charles. b. 1758. d. 1778.
    - (by Eliz. Chandos-Pole) Violetta, m. Samuel Tertius Galton.
      - Francis Galton.
    - (by Eliz. Chandos-Pole) Francis Sacheverel. b. 1786, d. 1859.
      - Reginald Darwin.
      - Edward Darwin, "High Elms."

ZOOLOGICAL DEPT. CAMBRIDGE * NEWTON LIBRARY *

Erasmus; but in his features there is no traceable resemblance to those of his grandfather. Nor, it appears, had Erasmus the love of exercise and of field-sports, so characteristic of Charles Darwin as a young man, though he had, like his grandson, an indomitable love of hard mental work. Benevolence and sympathy with others, and a great personal charm of manner, were common to the two. Charles Darwin possessed, in the highest degree, that "vividness of imagination" of which he speaks as strongly characteristic of Erasmus, and as leading "to his overpowering tendency to theorise and generalise." This tendency, in the case of Charles Darwin, was fully kept in check by the determination to test his theories to the utmost. Erasmus had a strong love of all kinds of mechanism, for which Charles Darwin had no taste. Neither had Charles Darwin the literary temperament which made Erasmus a poet as well as a philosopher. He writes of Erasmus: * "Throughout his letters I have been struck with his indifference to fame, and the complete absence of all signs of any over-estimation of his own abilities, or of the success of his works." These, indeed, seem indications of traits most strikingly prominent in his own character. Yet we get no evidence in Erasmus of the intense modesty and simplicity that marked Charles Darwin's whole nature. But by the quick bursts of anger provoked in Erasmus, at the sight of any inhumanity or injustice, we are again reminded of him.

On the whole, however, it seems to me that we do not know enough of the essential personal tone of Erasmus Darwin's character to attempt more than a superficial comparison; and I am left with an impression that, in spite of many resemblances, the two men were of a different type. It has been shown that Miss Seward and Mrs. Schimmelpenninck have misrepresented Erasmus Darwin's character.† It is, however,

* 'Life of Erasmus Darwin,' p. 68. † Ibid. pp. 77, 79, &c.

© The Complete Work of Charles Darwin Online

extremely probable that the faults which they exaggerate were to some extent characteristic of the man; and this leads me to think that Erasmus had a certain acerbity or severity of temper which did not exist in his grandson.

The sons of Erasmus Darwin inherited in some degree his intellectual tastes, for Charles Darwin writes of them as follows*:—

"His eldest son, Charles (born September 3, 1758), was a young man of extraordinary promise, but died (May 15, 1778) before he was twenty-one years old, from the effects of a wound received whilst dissecting the brain of a child. He inherited from his father a strong taste for various branches of science, for writing verses, and for mechanics . . . He also inherited stammering. With the hope of curing him, his father sent him to France, when about eight years old (1766–67), with a private tutor, thinking that if he was not allowed to speak English for a time, the habit of stammering might be lost; and it is a curious fact, that in after years, when speaking French, he never stammered. At a very early age he collected specimens of all kinds. When sixteen years old he was sent for a year to [Christ Church] Oxford, but he did not like the place, and thought (in the words of his father) that the 'vigour of his mind languished in the pursuit of classical elegance like Hercules at the distaff, and sighed to be removed to the robuster exercise of the medical school of Edinburgh.' He stayed three years at Edinburgh, working hard at his medical studies, and attending 'with diligence all the sick poor of the parish of Waterleith, and supplying them with the necessary medicines.' The Æsculapian Society awarded him its first gold medal for an experimental inquiry on pus and mucus. Notices of him appeared in various journals; and all the writers agree about his uncommon energy and abilities. He seems like his father to have excited the warm affection of his friends. Professor

* 'Life of Erasmus Darwin,' p. 80.

© The Complete Work of Charles Darwin Online

Andrew Duncan . . . . spoke . . . . about him with the warmest affection forty-seven years after his death when I was a young medical student at Edinburgh . . .

"About the character of his second son Erasmus (born 1759), I have little to say, for though he wrote poetry, he seems to have had none of the other tastes of his father. He had, however, his own peculiar tastes, viz. genealogy, the collecting of coins, and statistics. When a boy he counted all the houses in the city of Lichfield, and found out the number of inhabitants in as many as he could; he thus made a census, and when a real one was first made, his estimate was found to be nearly accurate. His disposition was quiet and retiring. My father had a very high opinion of his abilities, and this was probably just, for he would not otherwise have been invited to travel with, and pay long visits to, men so distinguished in different ways as Boulton the engineer, and Day the moralist and novelist." His death by suicide, in 1799, seems to have taken place in a state of incipient insanity.

Robert Waring, the father of Charles Darwin, was born May 30, 1766, and entered the medical profession like his father. He studied for a few months at Leyden, and took his M.D.* at that University on Feb. 26, 1785. "His father" (Erasmus) "brought † him to Shrewsbury before he was twenty-one years old (1787), and left him £20, saying, 'Let me know when you want more, and I will send it you.' His

* I owe this information to the kindness of Professor Rauwenhoff, Director of the Archives at Leyden. He quotes from the catalogue of doctors that "Robertus Waring Darwin, Anglo-britannus," defended (Feb. 26, 1785) in the Senate a Dissertation on the coloured images seen after looking at a bright object, and "Medicinæ Doctor creatus est a clar. Paradijs." The archives of Leyden University are so complete that Professor Rauwenhoff is able to tell me that my grandfather lived together with a certain "Petrus Crompton, Anglus," in lodgings in the Apothekersdijk. Dr. Darwin's Leyden dissertation was published in the 'Philosophical Transactions,' and my father used to say that the work was in fact due to Erasmus Darwin.—F. D.

† 'Life of Erasmus Darwin,' p. 85.

© The Complete Work of Charles Darwin Online

uncle, the rector of Elston, afterwards also sent him £20, and this was the sole pecuniary aid * which he ever received . . . Erasmus tells Mr. Edgeworth that his son Robert, after being settled in Shrewsbury for only six months, 'already had between forty and fifty patients.' By the second year he was in considerable, and ever afterwards in very large, practice."

Robert Waring Darwin married (April 18, 1796) Susannah, the daughter of his father's friend, Josiah Wedgwood, of Etruria, then in her thirty-second year. We have a miniature of her, with a remarkably sweet and happy face, bearing some resemblance to the portrait by Sir Joshua Reynolds of her father; a countenance expressive of the gentle and sympathetic nature which Miss Meteyard ascribes to her.† She died July 15, 1817, thirty-two years before her husband, whose death occurred on November 13, 1848. Dr. Darwin lived before his marriage for two or three years on St. John's Hill, afterwards at the Crescent, where his eldest daughter Marianne was born, lastly at the "Mount," in the part of Shrewsbury known as Frankwell, where the other children were born. This house was built by Dr. Darwin about 1800, it is now in the possession of Mr. Spencer Phillips, and has undergone but little alteration. It is a large, plain, square, red-brick house, of which the most attractive feature is the pretty green-house, opening out of the morning-room.

The house is charmingly placed, on the top of a steep bank leading down to the Severn. The terraced bank is traversed by a long walk, leading from end to end, still called "the Doctor's Walk." At one point in this walk grows a Spanish chestnut, the branches of which bend back parallel to themselves in a curious manner, and this was Charles Darwin's favourite tree as a boy, where he and his sister Catherine had each their special seat.

The Doctor took great pleasure in his garden, planting it

* See errata. † 'A Group of Englishmen,' by Miss Meteyard, 1871.

© The Complete Work of Charles Darwin Online

with ornamental trees and shrubs, and being especially successful in fruit-trees; and this love of plants was, I think, the only taste kindred to natural history which he possessed. Of the "Mount pigeons," which Miss Meteyard describes as illustrating Dr. Darwin's natural-history tastes, I have not been able to hear from those most capable of knowing. Miss Meteyard's account of him is not quite accurate in a few points. For instance, it is incorrect to describe Dr. Darwin as having a philosophical mind; his was a mind especially given to detail, and not to generalising. Again, those who knew him intimately describe him as eating remarkably little, so that he was not "a great feeder, eating a goose for his dinner, as easily as other men do a partridge." * In the matter of dress he was conservative, and wore to the end of his life knee-breeches and drab gaiters, which, however, certainly did not, as Miss Meteyard says, button above the knee—a form of costume chiefly known to us in grenadiers of Queen Anne's day, and in modern wood-cutters and ploughboys.

Charles Darwin had the strongest feeling of love and respect for his father's memory. His recollection of everything that was connected with him was peculiarly distinct, and he spoke of him frequently; generally prefacing an anecdote with some such phrase as, "My father, who was the wisest man I ever knew, &c." It was astonishing how clearly he remembered his father's opinions, so that he was able to quote some maxims or hint of his in most cases of illness. As a rule he put small faith in doctors, and thus his unlimited belief in Dr. Darwin's medical instinct, and methods of treatment was all the more striking.

His reverence for him was boundless and most touching. He would have wished to judge everything else in the world dispassionately, but anything his father had said was received with almost implicit faith. His daughter Mrs. Litchfield remembers him saying that he hoped none of his sons would

* 'A Group of Englishmen,' p. 263.

© The Complete Work of Charles Darwin Online

ever believe anything because he said it, unless they were themselves convinced of its truth,—a feeling in striking contrast with his own manner of faith.

A visit which Charles Darwin made to Shrewsbury in 1869 left on the mind of his daughter who accompanied him a strong impression of his love for his old home. The then tenant of the Mount showed them over the house, &c., and with mistaken hospitality remained with the party during the whole visit. As they were leaving, Charles Darwin said, with a pathetic look of regret, "If I could have been left alone in that green-house for five minutes, I know I should have been able to see my father in his wheel-chair as vividly as if he had been there before me."

Perhaps this incident shows what I think is the truth, that the memory of his father he loved the best, was that of him as an old man. Mrs. Litchfield has noted down a few words which illustrate well his feeling towards his father. She describes him as saying with the most tender respect, "I think my father was a little unjust to me when I was young, but afterwards I am thankful to think I became a prime favourite with him." She has a vivid recollection of the expression of happy reverie that accompanied these words, as if he were reviewing the whole relation, and the remembrance left a deep sense of peace and gratitude.

What follows was added by Charles Darwin to his autobiographical 'Recollections,' and was written about 1877 or 1878.

"I may here add a few pages about my father, who was in many ways a remarkable man.

"He was about 6 feet 2 inches in height, with broad shoulders, and very corpulent, so that he was the largest man whom I ever saw. When he last weighed himself, he was 24 stone, but afterwards increased much in weight. His chief mental characteristics were his powers of obser-

© The Complete Work of Charles Darwin Online

vation and his sympathy, neither of which have I ever seen exceeded or even equalled. His sympathy was not only with the distresses of others, but in a greater degree with the pleasures of all around him. This led him to be always scheming to give pleasure to others, and, though hating extravagance, to perform many generous actions. For instance, Mr. B——, a small manufacturer in Shrewsbury, came to him one day, and said he should be bankrupt unless he could at once borrow £10,000, but that he was unable to give any legal security. My father heard his reasons for believing that he could ultimately repay the money, and from [his] intuitive perception of character felt sure that he was to be trusted. So he advanced this sum, which was a very large one for him while young, and was after a time repaid.

"I suppose that it was his sympathy which gave him unbounded power of winning confidence, and as a consequence made him highly successful as a physician. He began to practise before he was twenty-one years old, and his fees during the first year paid for the keep of two horses and a servant. On the following year his practice was large, and so continued for about sixty years, when he ceased to attend on any one. His great success as a doctor was the more remarkable, as he told me that he at first hated his profession so much that if he had been sure of the smallest pittance, or if his father had given him any choice, nothing should have induced him to follow it. To the end of his life, the thought of an operation almost sickened him, and he could scarcely endure to see a person bled—a horror which he has transmitted to me—and I remember the horror which I felt as a schoolboy in reading about Pliny (I think) bleeding to death in a warm bath. . . .

"Owing to my father's power of winning confidence, many patients, especially ladies, consulted him when suffering from any misery, as a sort of Father-Confessor. He told me that

© The Complete Work of Charles Darwin Online

they always began by complaining in a vague manner about their health, and by practice he soon guessed what was really the matter. He then suggested that they had been suffering in their minds, and now they would pour out their troubles, and he heard nothing more about the body. . . . Owing to my father's skill in winning confidence he received many strange confessions of misery and guilt. He often remarked how many miserable wives he had known. In several instances husbands and wives had gone on pretty well together for between twenty and thirty years, and then hated each other bitterly; this he attributed to their having lost a common bond in their young children having grown up.

"But the most remarkable power which my father possessed was that of reading the characters, and even the thoughts of those whom he saw even for a short time. We had many instances of the power, some of which seemed almost supernatural. It saved my father from ever making (with one exception, and the character of this man was soon discovered) an unworthy friend. A strange clergyman came to Shrewsbury, and seemed to be a rich man; everybody called on him, and he was invited to many houses. My father called, and on his return home told my sisters on no account to invite him or his family to our house; for he felt sure that the man was not to be trusted. After a few months he suddenly bolted, being heavily in debt, and was found out to be little better than an habitual swindler. Here is a case of trustfulness which not many men would have ventured on. An Irish gentleman, a complete stranger, called on my father one day, and said that he had lost his purse, and that it would be a serious inconvenience to him to wait in Shrewsbury until he could receive a remittance from Ireland. He then asked my father to lend him £20, which was immediately done, as my father felt certain that the story was a true one. As soon as a letter could arrive from Ireland, one came with the most profuse thanks, and enclosing, as he said, a £20 Bank

© The Complete Work of Charles Darwin Online

of England note, but no note was enclosed. I asked my father whether this did not stagger him, but he answered 'not in the least.' On the next day another letter came with many apologies for having forgotten (like a true Irishman) to put the note into his letter of the day before. . . . [A gentleman] brought his nephew, who was insane but quite gentle, to my father; and the young man's insanity led him to accuse himself of all the crimes under heaven. When my father afterwards talked over the matter with the uncle, he said, 'I am sure that your nephew is really guilty of . . . a heinous crime.' Whereupon [the gentleman] said, 'Good God, Dr. Darwin, who told you; we thought that no human being knew the fact except ourselves!' My father told me the story many years after the event, and I asked him how he distinguished the true from the false self-accusations; and it was very characteristic of my father that he said he could not explain how it was.

"The following story shows what good guesses my father could make. Lord Shelburne, afterwards the first Marquis of Lansdowne, was famous (as Macaulay somewhere remarks) for his knowledge of the affairs of Europe, on which he greatly prided himself. He consulted my father medically, and afterwards harangued him on the state of Holland. My father had studied medicine at Leyden, and one day [while there] went a long walk into the country with a friend who took him to the house of a clergyman (we will say the Rev. Mr. A——, for I have forgotten his name), who had married an Englishwoman. My father was very hungry, and there was little for luncheon except cheese, which he could never eat. The old lady was surprised and grieved at this, and assured my father that it was an excellent cheese, and had been sent her from Bowood, the seat of Lord Shelburne. My father wondered why a cheese should be sent her from Bowood, but thought nothing more about it until it flashed across his mind many years afterwards, whilst Lord Shelburne was talking about

© The Complete Work of Charles Darwin Online

Holland. So he answered, 'I should think from what I saw of the Rev. Mr. A——, that he was a very able man, and well acquainted with the state of Holland.' My father saw that the Earl, who immediately changed the conversation, was much startled. On the next morning my father received a note from the Earl, saying that he had delayed starting on his journey, and wished particularly to see my father. When he called, the Earl said, 'Dr. Darwin, it is of the utmost importance to me and to the Rev. Mr. A—— to learn how you have discovered that he is the source of my information about Holland.' So my father had to explain the state of the case, and he supposed that Lord Shelburne was much struck with his diplomatic skill in guessing, for during many years afterwards he received many kind messages from him through various friends. I think that he must have told the story to his children; for Sir C. Lyell asked me many years ago why the Marquis of Lansdowne (the son or grandson of the first marquis) felt so much interest about me, whom he had never seen, and my family. When forty new members (the forty thieves as they were then called) were added to the Athenæum Club, there was much canvassing to be one of them; and without my having asked any one, Lord Lansdowne proposed me and got me elected. If I am right in my supposition, it was a queer concatenation of events that my father not eating cheese half-a-century before in Holland led to my election as a member of the Athenæum.

"The sharpness of his observation led him to predict with remarkable skill the course of any illness, and he suggested endless small details of relief. I was told that a young doctor in Shrewsbury, who disliked my father, used to say that he was wholly unscientific, but owned that his power of predicting the end of an illness was unparalleled. Formerly when he thought that I should be a doctor, he talked much to me about his patients. In the old days the practice of bleeding largely was universal, but my father maintained that far more

© The Complete Work of Charles Darwin Online

evil was thus caused than good done; and he advised me if ever I was myself ill not to allow any doctor to take more than an extremely small quantity of blood. Long before typhoid fever was recognised as distinct, my father told me that two utterly distinct kinds of illness were confounded under the name of typhus fever. He was vehement against drinking, and was convinced of both the direct and inherited evil effects of alcohol when habitually taken even in moderate quantity in a very large majority of cases. But he admitted and advanced instances of certain persons who could drink largely during their whole lives without apparently suffering any evil effects, and he believed that he could often beforehand tell who would thus not suffer. He himself never drank a drop of any alcoholic fluid. This remark reminds me of a case showing how a witness under the most favourable circumstances may be utterly mistaken. A gentleman-farmer was strongly urged by my father not to drink, and was encouraged by being told that he himself never touched any spirituous liquor. Whereupon the gentleman said, 'Come, come, Doctor, this won't do—though it is very kind of you to say so for my sake—for I know that you take a very large glass of hot gin and water every evening after your dinner.' * So my father asked him how he knew this. The man answered, 'My cook was your kitchen-maid for two or three years, and she saw the butler every day prepare and take to you the gin and water.' The explanation was that my father had the odd habit of drinking hot water in a very tall and large glass after his dinner; and the butler used first to put some cold water in the glass, which the girl mistook for gin, and then filled it up with boiling water from the kitchen boiler.

"My father used to tell me many little things which he had found useful in his medical practice. Thus ladies often

* This belief still survives, and was mentioned to my brother in 1884 by an old inhabitant of Shrewsbury.—F. D.

© The Complete Work of Charles Darwin Online

PHILOSOPHICAL ... 1901

cried much while telling him their troubles, and thus caused much loss of his precious time. He soon found that begging them to command and restrain themselves, always made them weep the more, so that afterwards he always encouraged them to go on crying, saying that this would relieve them more than anything else, and with the invariable result that they soon ceased to cry, and he could hear what they had to say and give his advice. When patients who were very ill craved for some strange and unnatural food, my father asked them what had put such an idea into their heads: if they answered that they did not know, he would allow them to try the food, and often with success, as he trusted to their having a kind of instinctive desire; but if they answered that they had heard that the food in question had done good to some one else, he firmly refused his assent.

"He gave one day an odd little specimen of human nature. When a very young man he was called in to consult with the family physician in the case of a gentleman of much distinction in Shropshire. The old doctor told the wife that the illness was of such a nature that it must end fatally. My father took a different view and maintained that the gentleman would recover: he was proved quite wrong in all respects (I think by autopsy) and he owned his error. He was then convinced that he should never again be consulted by this family; but after a few months the widow sent for him, having dismissed the old family doctor. My father was so much surprised at this, that he asked a friend of the widow to find out why he was again consulted. The widow answered her friend, that 'she would never again see the odious old doctor who said from the first that her husband would die, while Dr. Darwin always maintained that he would recover!' In another case my father told a lady that her husband would certainly die. Some months afterwards he saw the widow who was a very sensible woman, and she said, 'You are a very young man, and allow me to advise you always to give as

© The Complete Work of Charles Darwin Online

long as you possibly can, hope to any near relative nursing a patient. You made me despair, and from that moment I lost strength.' My father said that he had often since seen the paramount importance, for the sake of the patient, of keeping up the hope and with it the strength of the nurse in charge. This he sometimes found difficult to do compatibly with truth. One old gentleman, however, caused him no such perplexity. He was sent for by Mr. P——, who said, 'From all that I have seen and heard of you I believe that you are the sort of man who will speak the truth, and if I ask, you will tell me when I am dying. Now I much desire that you should attend me, if you will promise, whatever I may say, always to declare that I am not going to die.' My father acquiesced on the understanding that his words should in fact have no meaning.

"My father possessed an extraordinary memory, especially for dates, so that he knew, when he was very old, the day of the birth, marriage, and death of a multitude of persons in Shropshire; and he once told me that this power annoyed him; for if he once heard a date, he could not forget it; and thus the deaths of many friends were often recalled to his mind. Owing to his strong memory he knew an extraordinary number of curious stories, which he liked to tell, as he was a great talker. He was generally in high spirits, and laughed and joked with every one—often with his servants—with the utmost freedom; yet he had the art of making every one obey him to the letter. Many persons were much afraid of him. I remember my father telling us one day, with a laugh, that several persons had asked him whether Miss ——, a grand old lady in Shropshire, had called on him, so that at last he enquired why they asked him; and was told that Miss ——, whom my father had somehow mortally offended, was telling everybody that she would call and tell 'that fat old doctor very plainly what she thought of him.' She had already called, but her courage had failed, and no one could have been more

© The Complete Work of Charles Darwin Online

courteous and friendly. As a boy, I went to stay at the house of ——, whose wife was insane; and the poor creature, as soon as she saw me, was in the most abject state of terror that I ever saw, weeping bitterly and asking me over and over again, 'Is your father coming?' but was soon pacified. On my return home, I asked my father why she was so frightened, and he answered he was very glad to hear it, as he had frightened her on purpose, feeling sure that she would be kept in safety and much happier without any restraint, if her husband could influence her, whenever she became at all violent, by proposing to send for Dr. Darwin; and these words succeeded perfectly during the rest of her long life.

"My father was very sensitive, so that many small events annoyed him or pained him much. I once asked him, when he was old and could not walk, why he did not drive out for exercise; and he answered, 'Every road out of Shrewsbury is associated in my mind with some painful event.' Yet he was generally in high spirits. He was easily made very angry, but his kindness was unbounded. He was widely and deeply loved.

"He was a cautious and good man of business, so that he hardly ever lost money by any investment, and left to his children a very large property. I remember a story showing how easily utterly false beliefs originate and spread. Mr. E——, a squire of one of the oldest families in Shropshire, and head partner in a bank, committed suicide. My father was sent for as a matter of form, and found him dead. I may mention, by the way, to show how matters were managed in those old days, that because Mr. E—— was a rather great man, and universally respected, no inquest was held over his body. My father, in returning home, thought it proper to call at the bank (where he had an account) to tell the managing partners of the event, as it was not improbable that it would cause a run on the bank. Well, the story was spread far and wide, that my father went into the bank, drew out all his money, left the

C 2

© The Complete Work of Charles Darwin Online

bank, came back again, and said, 'I may just tell you that Mr. E—— has killed himself,' and then departed. It seems that it was then a common belief that money withdrawn from a bank was not safe until the person had passed out through the door of the bank. My father did not hear this story till some little time afterwards, when the managing partner said that he had departed from his invariable rule of never allowing any one to see the account of another man, by having shown the ledger with my father's account to several persons, as this proved that my father had not drawn out a penny on that day. It would have been dishonourable in my father to have used his professional knowledge for his private advantage. Nevertheless, the supposed act was greatly admired by some persons; and many years afterwards, a gentleman remarked, 'Ah, Doctor, what a splendid man of business you were in so cleverly getting all your money safe out of that bank!'

"My father's mind was not scientific, and he did not try to generalise his knowledge under general laws; yet he formed a theory for almost everything which occurred. I do not think I gained much from him intellectually; but his example ought to have been of much moral service to all his children. One of his golden rules (a hard one to follow) was, 'Never become the friend of any one whom you cannot respect.'"

Dr. Darwin had six children:* Marianne, married Dr. Henry Parker; Caroline, married Josiah Wedgwood; Erasmus Alvey; Susan, died unmarried; Charles Robert; Catherine, married Rev. Charles Langton.

The elder son, Erasmus, was born in 1804, and died unmarried at the age of seventy-seven.

He, like his brother, was educated at Shrewsbury School and at Christ's College, Cambridge. He studied medicine at Edinburgh and in London, and took the degree of Bachelor of Medicine at Cambridge. He never made any pretence of

* Of these Mrs. Wedgwood is now the sole survivor.

© The Complete Work of Charles Darwin Online

practising as a doctor, and, after leaving Cambridge, lived a quiet life in London.

There was something pathetic in Charles Darwin's affection for his brother Erasmus, as if he always recollected his solitary life, and the touching patience and sweetness of his nature. He often spoke of him as "Poor old Ras," or "Poor dear old Philos"—I imagine Philos (Philosopher) was a relic of the days when they worked at chemistry in the tool-house at Shrewsbury—a time of which he always preserved a pleasant memory. Erasmus being rather more than four years older than Charles Darwin, they were not long together at Cambridge, but previously at Edinburgh they lived in the same lodgings, and after the Voyage they lived for a time together in Erasmus' house in Great Marlborough Street. At this time also he often speaks with much affection of Erasmus in his letters to Fox, using words such as "my dear good old brother." In later years Erasmus Darwin came to Down occasionally, or joined his brother's family in a summer holiday. But gradually it came about that he could not, through ill health, make up his mind to leave London, and then they only saw each other when Charles Darwin went for a week at a time to his brother's house in Queen Anne Street.

The following note on his brother's character was written by Charles Darwin at about the same time that the sketch of his father was added to the 'Recollections':—

"My brother Erasmus possessed a remarkably clear mind with extensive and diversified tastes and knowledge in literature, art, and even in science. For a short time he collected and dried plants, and during a somewhat longer time experimented in chemistry. He was extremely agreeable, and his wit often reminded me of that in the letters and works of Charles Lamb. He was very kind-hearted. . . . His health from his boyhood had been weak, and as a consequence he

© The Complete Work of Charles Darwin Online

failed in energy. His spirits were not high, sometimes low, more especially during early and middle manhood. He read much, even whilst a boy, and at school encouraged me to read, lending me books. Our minds and tastes were, however, so different, that I do not think I owe much to him intellectually. I am inclined to agree with Francis Galton in believing that education and environment produce only a small effect on the mind of any one, and that most of our qualities are innate."

Erasmus Darwin's name, though not known to the general public, may be remembered from the sketch of his character in Carlyle's 'Reminiscences,' which I here reproduce in part:—

"Erasmus Darwin, a most diverse kind of mortal, came to seek us out very soon ('had heard of Carlyle in Germany, &c.') and continues ever since to be a quiet house-friend, honestly attached; though his visits latterly have been rarer and rarer, health so poor, I so occupied, &c., &c. He had something of original and sarcastically ingenious in him, one of the sincerest, naturally truest, and most modest of men; elder brother of Charles Darwin (the famed Darwin on Species of these days) to whom I rather prefer him for intellect, had not his health quite doomed him to silence and patient idleness. . . . My dear one had a great favour for this honest Darwin always; many a road, to shops and the like, he drove her in his cab (Darwingium Cabbum comparable to Georgium Sidus) in those early days when even the charge of omnibuses was a consideration, and his sparse utterances, sardonic often, were a great amusement to her. 'A perfect gentleman' she at once discerned him to be, and of sound worth and kindliness in the most unaffected form." *

Charles Darwin did not appreciate this sketch of his brother;

* Carlyle's 'Reminiscences,' vol. ii. p. 208.

© The Complete Work of Charles Darwin Online

he thought Carlyle had missed the essence of his most lovable nature.

I am tempted by the wish of illustrating further the character of one so sincerely beloved by all Charles Darwin's children, to reproduce a letter to the *Spectator* (Sept. 3, 1881) by his cousin Miss Julia Wedgwood.

"A portrait from Mr. Carlyle's portfolio not regretted by any who loved the original, surely confers sufficient distinction to warrant a few words of notice, when the character it depicts is withdrawn from mortal gaze. Erasmus, the only brother of Charles Darwin, and the faithful and affectionate old friend of both the Carlyles, has left a circle of mourners who need no tribute from illustrious pen to embalm the memory so dear to their hearts; but a wider circle must have felt some interest excited by that tribute, and may receive with a certain attention the record of a unique and indelible impression, even though it be made only on the hearts of those who cannot bequeath it, and with whom, therefore, it must speedily pass away. They remember it with the same distinctness as they remember a creation of genius; it has in like manner enriched and sweetened life, formed a common meeting-point for those who had no other; and, in its strong fragrance of individuality, enforced that respect for the idiosyncracies of human character without which moral judgment is always hard and shallow, and often unjust. Carlyle was one to find a peculiar enjoyment in the combination of liveliness and repose which gave his friend's society an influence at once stimulating and soothing, and the warmth of his appreciation was not made known first in its posthumous expression; his letters of anxiety nearly thirty years ago, when the frail life which has been prolonged to old age was threatened by serious illness, are still fresh in my memory. The friendship was equally warm with both husband and wife. I remember well a pathetic little remonstrance from her

© The Complete Work of Charles Darwin Online

elicited by an avowal from Erasmus Darwin, that he preferred cats to dogs, which she felt a slur on her little 'Nero;' and the tones in which she said, 'Oh, but you are fond of dogs! you are too kind not to be,' spoke of a long vista of small, gracious kindnesses, remembered with a tender gratitude. He was intimate also with a person whose friends, like those of Mr. Carlyle, have not always had cause to congratulate themselves on their place in her gallery,—Harriet Martineau. I have heard him more than once call her a faithful friend, and it always seemed to me a curious tribute to something in the friendship that he alone supplied; but if she had written of him at all, I believe the mention, in its heartiness of appreciation, would have afforded a rare and curious meeting-point with the other 'Reminiscences,' so like and yet so unlike. It is not possible to transfer the impression of a character; we can only suggest it by means of some resemblance; and it is a singular illustration of that irony which checks or directs our sympathies, that in trying to give some notion of the man whom, among those who were not his kindred, Carlyle appears to have most loved, I can say nothing more descriptive than that he seems to me to have had something in common with the man whom Carlyle least appreciated. The society of Erasmus Darwin had, to my mind, much the same charm as the writings of Charles Lamb. There was the same kind of playfulness, the same lightness of touch, the same tenderness, perhaps the same limitations. On another side of his nature, I have often been reminded of him by the quaint, delicate humour, the superficial intolerance, the deep springs of pity, the peculiar mixture of something pathetic with a sort of gay scorn, entirely remote from contempt, which distinguish the Ellesmere of Sir Arthur Helps' earlier dialogues. Perhaps we recall such natures most distinctly, when such a resemblance is all that is left of them. The character is not merged in the creation; and what we lose in the power to communicate our impression, we seem to gain in its vividness. Erasmus Darwin

© The Complete Work of Charles Darwin Online

has passed away in old age, yet his memory retains something of a youthful fragrance ; his influence gave much happiness, of a kind usually associated with youth, to many lives besides the illustrious one whose records justify, though certainly they do not inspire, the wish to place this fading chaplet on his grave."

The foregoing pages give, in a fragmentary manner, as much perhaps as need be told of the family from which Charles Darwin came, and may serve as an introduction to the autobiographical chapter which follows.

© The Complete Work of Charles Darwin Online

CHAPTER II.

AUTOBIOGRAPHY.

[My father's autobiographical recollections, given in the present chapter, were written for his children,—and written without any thought that they would ever be published. To many this may seem an impossibility; but those who knew my father will understand how it was not only possible, but natural. The autobiography bears the heading, 'Recollections of the Development of my Mind and Character,' and end with the following note:—"Aug. 3, 1876. This sketch of my life was begun about May 28th at Hopedene,* and since then I have written for nearly an hour on most afternoons." It will easily be understood that, in a narrative of a personal and intimate kind written for his wife and children, passages should occur which must here be omitted; and I have not thought it necessary to indicate where such omissions are made. It has been found necessary to make a few corrections of obvious verbal slips, but the number of such alterations has been kept down to the minimum.—F. D.]

A German Editor having written to me for an account of the development of my mind and character with some sketch of my autobiography, I have thought that the attempt would amuse me, and might possibly interest my children or their children. I know that it would have interested me greatly to have read even

* Mr. Hensleigh Wedgwood's house in Surrey.

© The Complete Work of Charles Darwin Online

so short and dull a sketch of the mind of my grandfather, written by himself, and what he thought and did, and how he worked. I have attempted to write the following account of myself, as if I were a dead man in another world looking back at my own life. Nor have I found this difficult, for life is nearly over with me. I have taken no pains about my style of writing.

I was born at Shrewsbury on February 12th, 1809, and my earliest recollection goes back only to when I was a few months over four years old, when we went to near Abergele for sea-bathing, and I recollect some events and places there with some little distinctness.

My mother died in July 1817, when I was a little over eight years old, and it is odd that I can remember hardly anything about her except her deathbed, her black velvet gown, and her curiously constructed work-table. In the spring of this same year I was sent to a day-school in Shrewsbury, where I stayed a year. I have been told that I was much slower in learning than my younger sister Catherine, and I believe that I was in many ways a naughty boy.

By the time I went to this day-school * my taste

* Kept by Rev. G. Case, minister of the Unitarian Chapel in the High Street. Mrs. Darwin was a Unitarian and attended Mr. Case's chapel, and my father as a little boy went there with his elder sisters. But both he and his brother were christened and intended to belong to the Church of England; and after his early boyhood he seems usually to have gone to church and not to Mr. Case's. It appears (*St. James' Gazette*, Dec. 15, 1883) that a mural tablet has been erected to his memory in the chapel, which is now known as the 'Free Christian Church.'—F. D.

© The Complete Work of Charles Darwin Online

for natural history, and more especially for collecting, was well developed. I tried to make out the names of plants,* and collected all sorts of things, shells, seals, franks, coins, and minerals. The passion for collecting which leads a man to be a systematic naturalist, a virtuoso, or a miser, was very strong in me, and was clearly innate, as none of my sisters or brother ever had this taste.

One little event during this year has fixed itself very firmly in my mind, and I hope that it has done so from my conscience having been afterwards sorely troubled by it; it is curious as showing that apparently I was interested at this early age in the variability of plants! I told another little boy (I believe it was Leighton, who afterwards became a well-known lichenologist and botanist), that I could produce variously coloured polyanthuses and primroses by watering them with certain coloured fluids, which was of course a monstrous fable, and had never been tried by me. I may here also confess that as a little boy I was much given to inventing deliberate falsehoods, and this was always done for the sake of causing excitement. For instance, I once gathered much valuable fruit from my father's trees and hid it in the shrubbery, and then ran in breathless

* Rev. W. A. Leighton, who was a schoolfellow of my father's at Mr. Case's school, remembers his bringing a flower to school and saying that his mother had taught him how by looking at the inside of the blossom the name of the plant could be discovered. Mr. Leighton goes on, "This greatly roused my attention and curiosity, and I inquired of him repeatedly how this could be done?"—but his lesson was naturally enough not transmissible. —F. D.

© The Complete Work of Charles Darwin Online

haste to spread the news that I had discovered a hoard of stolen fruit.

I must have been a very simple little fellow when I first went to the school. A boy of the name of Garnett took me into a cake shop one day, and bought some cakes for which he did not pay, as the shopman trusted him. When we came out I asked him why he did not pay for them, and he instantly answered, "Why, do you not know that my uncle left a great sum of money to the town on condition that every tradesman should give whatever was wanted without payment to any one who wore his old hat and moved [it] in a particular manner?" and he then showed me how it was moved. He then went into another shop where he was trusted, and asked for some small article, moving his hat in the proper manner, and of course obtained it without payment. When we came out he said, "Now if you like to go by yourself into that cake-shop (how well I remember its exact position) I will lend you my hat, and you can get whatever you like if you move the hat on your head properly." I gladly accepted the generous offer, and went in and asked for some cakes, moved the old hat and was walking out of the shop, when the shopman made a rush at me, so I dropped the cakes and ran for dear life, and was astonished by being greeted with shouts of laughter by my false friend Garnett.

I can say in my own favour that I was as a boy humane, but I owed this entirely to the instruction and example of my sisters. I doubt indeed whether humanity is a natural or innate quality. I was very

© The Complete Work of Charles Darwin Online

fond of collecting eggs, but I never took more than a single egg out of a bird's nest, except on one single occasion, when I took all, not for their value, but from a sort of bravado.

I had a strong taste for angling, and would sit for any number of hours on the bank of a river or pond watching the float; when at Maer * I was told that I could kill the worms with salt and water, and from that day I never spitted a living worm, though at the expense probably of some loss of success.

Once as a very little boy whilst at the day school, or before that time, I acted cruelly, for I beat a puppy, I believe, simply from enjoying the sense of power; but the beating could not have been severe, for the puppy did not howl, of which I feel sure, as the spot was near the house. This act lay heavily on my conscience, as is shown by my remembering the exact spot where the crime was committed. It probably lay all the heavier from my love of dogs being then, and for a long time afterwards, a passion. Dogs seemed to know this, for I was an adept in robbing their love from their masters.

I remember clearly only one other incident during this year whilst at Mr. Case's daily school,—namely, the burial of a dragoon soldier; and it is surprising how clearly I can still see the horse with the man's empty boots and carbine suspended to the saddle, and the firing over the grave. This scene deeply stirred whatever poetic fancy there was in me.

In the summer of 1818 I went to Dr. Butler's great

* The house of his uncle, Josiah Wedgwood.

© The Complete Work of Charles Darwin Online

school in Shrewsbury, and remained there for seven years till Midsummer 1825, when I was sixteen years old. I boarded at this school, so that I had the great advantage of living the life of a true schoolboy; but as the distance was hardly more than a mile to my home, I very often ran there in the longer intervals between the callings over and before locking up at night. This, I think, was in many ways advantageous to me by keeping up home affections and interests. I remember in the early part of my school life that I often had to run very quickly to be in time, and from being a fleet runner was generally successful; but when in doubt I prayed earnestly to God to help me, and I well remember that I attributed my success to the prayers and not to my quick running, and marvelled how generally I was aided.

I have heard my father and elder sister say that I had, as a very young boy, a strong taste for long solitary walks; but what I thought about I know not. I often became quite absorbed, and once, whilst returning to school on the summit of the old fortifications round Shrewsbury, which had been converted into a public foot-path with no parapet on one side, I walked off and fell to the ground, but the height was only seven or eight feet. Nevertheless the number of thoughts which passed through my mind during this very short, but sudden and wholly unexpected fall, was astonishing, and seem hardly compatible with what physiologists have, I believe, proved about each thought requiring quite an appreciable amount of time.

Nothing could have been worse for the develop-

© The Complete Work of Charles Darwin Online

ment of my mind than Dr. Butler's school, as it was strictly classical, nothing else being taught, except a little ancient geography and history. The school as a means of education to me was simply a blank. During my whole life I have been singularly incapable of mastering any language. Especial attention was paid to verse-making, and this I could never do well. I had many friends, and got together a good collection of old verses, which by patching together, sometimes aided by other boys, I could work into any subject. Much attention was paid to learning by heart the lessons of the previous day; this I could effect with great facility, learning forty or fifty lines of Virgil or Homer, whilst I was in morning chapel; but this exercise was utterly useless, for every verse was forgotten in forty-eight hours. I was not idle, and with the exception of versification, generally worked conscientiously at my classics, not using cribs. The sole pleasure I ever received from such studies, was from some of the odes of Horace, which I admired greatly.

When I left the school I was for my age neither high nor low in it; and I believe that I was considered by all my masters and by my father as a very ordinary boy, rather below the common standard in intellect. To my deep mortification my father once said to me, "You care for nothing but shooting, dogs, and rat-catching, and you will be a disgrace to yourself and all your family." But my father, who was the kindest man I ever knew and whose memory I love with all my heart, must have been angry and somewhat unjust when he used such words.

© The Complete Work of Charles Darwin Online

Looking back as well as I can at my character during my school life, the only qualities which at this period promised well for the future, were, that I had strong and diversified tastes, much zeal for whatever interested me, and a keen pleasure in understanding any complex subject or thing. I was taught Euclid by a private tutor, and I distinctly remember the intense satisfaction which the clear geometrical proofs gave me. I remember with equal distinctness the delight which my uncle gave me (the father of Francis Galton) by explaining the principle of the vernier of a barometer. With respect to diversified tastes, independently of science, I was fond of reading various books, and I used to sit for hours reading the historical plays of Shakespeare, generally in an old window in the thick walls of the school. I read also other poetry, such as Thomson's 'Seasons,' and the recently published poems of Byron and Scott. I mention this because later in life I wholly lost, to my great regret, all pleasure from poetry of any kind, including Shakespeare. In connection with pleasure from poetry, I may add that in 1822 a vivid delight in scenery was first awakened in my mind, during a riding tour on the borders of Wales, and this has lasted longer than any other æsthetic pleasure.

Early in my school days a boy had a copy of the 'Wonders of the World,' which I often read, and disputed with other boys about the veracity of some of the statements; and I believe that this book first gave me a wish to travel in remote countries, which was ultimately fulfilled by the voyage of the *Beagle*. In

© The Complete Work of Charles Darwin Online

the latter part of my school life I became passionately fond of shooting; I do not believe that any one could have shown more zeal for the most holy cause than I did for shooting birds. How well I remember killing my first snipe, and my excitement was so great that I had much difficulty in reloading my gun from the trembling of my hands. This taste long continued, and I became a very good shot. When at Cambridge I used to practise throwing up my gun to my shoulder before a looking-glass to see that I threw it up straight. Another and better plan was to get a friend to wave about a lighted candle, and then to fire at it with a cap on the nipple, and if the aim was accurate the little puff of air would blow out the candle. The explosion of the cap caused a sharp crack, and I was told that the tutor of the college remarked, "What an extraordinary thing it is, Mr. Darwin seems to spend hours in cracking a horse-whip in his room, for I often hear the crack when I pass under his windows."

I had many friends amongst the schoolboys, whom I loved dearly, and I think that my disposition was then very affectionate.

With respect to science, I continued collecting minerals with much zeal, but quite unscientifically—all that I cared about was a new-*named* mineral, and I hardly attempted to classify them. I must have observed insects with some little care, for when ten years old (1819) I went for three weeks to Plas Edwards on the sea-coast in Wales, I was very much interested and surprised at seeing a large black and scarlet

© The Complete Work of Charles Darwin Online

Hemipterous insect, many moths (Zygæna), and a Cicindela which are not found in Shropshire. I almost made up my mind to begin collecting all the insects which I could find dead, for on consulting my sister I concluded that it was not right to kill insects for the sake of making a collection. From reading White's 'Selborne,' I took much pleasure in watching the habits of birds, and even made notes on the subject. In my simplicity I remember wondering why every gentleman did not become an ornithologist.

Towards the close of my school life, my brother worked hard at chemistry, and made a fair laboratory with proper apparatus in the tool-house in the garden, and I was allowed to aid him as a servant in most of his experiments. He made all the gases and many compounds, and I read with care several books on chemistry, such as Henry and Parkes' 'Chemical Catechism.' The subject interested me greatly, and we often used to go on working till rather late at night. This was the best part of my education at school, for it showed me practically the meaning of experimental science. The fact that we worked at chemistry somehow got known at school, and as it was an unprecedented fact, I was nicknamed "Gas." I was also once publicly rebuked by the head-master, Dr. Butler, for thus wasting my time on such useless subjects; and he called me very unjustly a "poco curante," and as I did not understand what he meant, it seemed to me a fearful reproach.

As I was doing no good at school, my father wisely took me away at a rather earlier age than usual, and

© The Complete Work of Charles Darwin Online

sent me (Oct. 1825) to Edinburgh University with my brother, where I stayed for two years or sessions. My brother was completing his medical studies, though I do not believe he ever really intended to practise, and I was sent there to commence them. But soon after this period I became convinced from various small circumstances that my father would leave me property enough to subsist on with some comfort, though I never imagined that I should be so rich a man as I am; but my belief was sufficient to check any strenuous effort to learn medicine.

The instruction at Edinburgh was altogether by lectures, and these were intolerably dull, with the exception of those on chemistry by Hope; but to my mind there are no advantages and many disadvantages in lectures compared with reading. Dr. Duncan's lectures on Materia Medica at 8 o'clock on a winter's morning are something fearful to remember. Dr. —— made his lectures on human anatomy as dull as he was himself, and the subject disgusted me. It has proved one of the greatest evils in my life that I was not urged to practise dissection, for I should soon have got over my disgust; and the practice would have been invaluable for all my future work. This has been an irremediable evil, as well as my incapacity to draw. I also attended regularly the clinical wards in the hospital. Some of the cases distressed me a good deal, and I still have vivid pictures before me of some of them; but I was not so foolish as to allow this to lessen my attendance. I cannot understand why this part of my medical course did not

© The Complete Work of Charles Darwin Online

interest me in a greater degree; for during the summer before coming to Edinburgh I began attending some of the poor people, chiefly children and women in Shrewsbury: I wrote down as full an account as I could of the case with all the symptoms, and read them aloud to my father, who suggested further inquiries and advised me what medicines to give, which I made up myself. At one time I had at least a dozen patients, and I felt a keen interest in the work. My father, who was by far the best judge of character whom I ever knew, declared that I should make a successful physician,—meaning by this one who would get many patients. He maintained that the chief element of success was exciting confidence; but what he saw in me which convinced him that I should create confidence I know not. I also attended on two occasions the operating theatre in the hospital at Edinburgh, and saw two very bad operations, one on a child, but I rushed away before they were completed. Nor did I ever attend again, for hardly any inducement would have been strong enough to make me do so; this being long before the blessed days of chloroform. The two cases fairly haunted me for many a long year.

My brother stayed only one year at the University, so that during the second year I was left to my own resources; and this was an advantage, for I became well acquainted with several young men fond of natural science. One of these was Ainsworth, who afterwards published his travels in Assyria; he was a Wernerian geologist, and knew a

© The Complete Work of Charles Darwin Online

little about many subjects. Dr. Coldstream was a very different young man, prim, formal, highly religious, and most kind-hearted; he afterwards published some good zoological articles. A third young man was Hardie, who would, I think, have made a good botanist, but died early in India. Lastly, Dr. Grant, my senior by several years, but how I became acquainted with him I cannot remember; he published some first-rate zoological papers, but after coming to London as Professor in University College, he did nothing more in science, a fact which has always been inexplicable to me. I knew him well; he was dry and formal in manner, with much enthusiasm beneath this outer crust. He one day, when we were walking together, burst forth in high admiration of Lamarck and his views on evolution. I listened in silent astonishment, and as far as I can judge without any effect on my mind. I had previously read the 'Zoonomia' of my grandfather, in which similar views are maintained, but without producing any effect on me. Nevertheless it is probable that the hearing rather early in life such views maintained and praised may have favoured my upholding them under a different form in my 'Origin of Species.' At this time I admired greatly the 'Zoonomia;' but on reading it a second time after an interval of ten or fifteen years, I was much disappointed; the proportion of speculation being so large to the facts given.

Drs. Grant and Coldstream attended much to marine Zoology, and I often accompanied the former to collect animals in the tidal pools, which I dissected

© The Complete Work of Charles Darwin Online

as well as I could. I also became friends with some of the Newhaven fishermen, and sometimes accompanied them when they trawled for oysters, and thus got many specimens. But from not having had any regular practice in dissection, and from possessing only a wretched miscroscope, my attempts were very poor. Nevertheless I made one interesting little discovery, and read, about the beginning of the year 1826, a short paper on the subject before the Plinian Society. This was that the so-called ova of Flustra had the power of independent movement by means of cilia, and were in fact larvæ. In another short paper I showed that the little globular bodies which had been supposed to be the young state of *Fucus loreus* were the egg-cases of the worm-like *Pontobdella muricata*.

The Plinian Society was encouraged and, I believe, founded by Professor Jameson: it consisted of students and met in an underground room in the University for the sake of reading papers on natural science and discussing them. I used regularly to attend, and the meetings had a good effect on me in stimulating my zeal and giving me new congenial acquaintances. One evening a poor young man got up, and after stammering for a prodigious length of time, blushing crimson, he at last slowly got out the words, "Mr. President, I have forgotten what I was going to say." The poor fellow looked quite overwhelmed, and all the members were so surprised that no one could think of a word to say to cover his confusion. The papers which were read to our little society were not printed, so that I had not the satis-

© The Complete Work of Charles Darwin Online

faction of seeing my paper in print; but I believe Dr. Grant noticed my small discovery in his excellent memoir on Flustra.

I was also a member of the Royal Medical Society, and attended pretty regularly; but as the subjects were exclusively medical, I did not much care about them. Much rubbish was talked there, but there were some good speakers, of whom the best was the present Sir J. Kay-Shuttleworth. Dr. Grant took me occasionally to the meetings of the Wernerian Society, where various papers on natural history were read, discussed, and afterwards published in the 'Transactions.' I heard Audubon deliver there some interesting discourses on the habits of N. American birds, sneering somewhat unjustly at Waterton. By the way, a negro lived in Edinburgh, who had travelled with Waterton, and gained his livelihood by stuffing birds, which he did excellently: he gave me lessons for payment, and I used often to sit with him, for he was a very pleasant and intelligent man.

Mr. Leonard Horner also took me once to a meeting of the Royal Society of Edinburgh, where I saw Sir Walter Scott in the chair as President, and he apologised to the meeting as not feeling fitted for such a position. I looked at him and at the whole scene with some awe and reverence, and I think it was owing to this visit during my youth, and to my having attended the Royal Medical Society, that I felt the honour of being elected a few years ago an honorary member of both these Societies, more than any other similar honour. If I had been told at that time that I should

© The Complete Work of Charles Darwin Online

one day have been thus honoured, I declare that I should have thought it as ridiculous and improbable, as if I had been told that I should be elected King of England.

During my second year at Edinburgh I attended ——'s lectures on Geology and Zoology, but they were incredibly dull. The sole effect they produced on me was the determination never as long as I lived to read a book on Geology, or in any way to study the science. Yet I feel sure that I was prepared for a philosophical treatment of the subject; for an old Mr. Cotton in Shropshire, who knew a good deal about rocks, had pointed out to me two or three years previously a well-known large erratic boulder in the town of Shrewsbury, called the "bell-stone"; he told me that there was no rock of the same kind nearer than Cumberland or Scotland, and he solemnly assured me that the world would come to an end before any one would be able to explain how this stone came where it now lay. This produced a deep impression on me, and I meditated over this wonderful stone. So that I felt the keenest delight when I first read of the action of icebergs in transporting boulders, and I gloried in the progress of Geology. Equally striking is the fact that I, though now only sixty-seven years old, heard the Professor, in a field lecture at Salisbury Craigs, discoursing on a trap-dyke, with amygdaloidal margins and the strata indurated on each side, with volcanic rocks all around us, say that it was a fissure filled with sediment from above, adding with a sneer that there were men who main-

© The Complete Work of Charles Darwin Online

tained that it had been injected from beneath in a molten condition. When I think of this lecture, I do not wonder that I determined never to attend to Geology.

From attending ——'s lectures, I became acquainted with the curator of the museum, Mr. Macgillivray, who afterwards published a large and excellent book on the birds of Scotland. I had much interesting natural-history talk with him, and he was very kind to me. He gave me some rare shells, for I at that time collected marine mollusca, but with no great zeal.

My summer vacations during these two years were wholly given up to amusements, though I always had some book in hand, which I read with interest. During the summer of 1826 I took a long walking tour with two friends with knapsacks on our backs through North Wales. We walked thirty miles most days, including one day the ascent of Snowdon. I also went with my sister a riding tour in North Wales, a servant with saddle-bags carrying our clothes. The autumns were devoted to shooting chiefly at Mr. Owen's, at Woodhouse, and at my Uncle Jos's,* at Maer. My zeal was so great that I used to place my shooting-boots open by my bed-side when I went to bed, so as not to lose half a minute in putting them on in the morning; and on one occasion I reached a distant part of the Maer estate, on the 20th of August for black-game shooting, before I could see: I then toiled on with the gamekeeper the whole day through thick heath and young Scotch firs.

* Josiah Wedgwood, the son of the founder of the Etruria Works.

© The Complete Work of Charles Darwin Online

I kept an exact record of every bird which I shot throughout the whole season. One day when shooting at Woodhouse with Captain Owen, the eldest son, and Major Hill, his cousin, afterwards Lord Berwick, both of whom I liked very much, I thought myself shamefully used, for every time after I had fired and thought that I had killed a bird, one of the two acted as if loading his gun, and cried out, "You must not count that bird, for I fired at the same time," and the gamekeeper, perceiving the joke, backed them up. After some hours they told me the joke, but it was no joke to me, for I had shot a large number of birds, but did not know how many, and could not add them to my list, which I used to do by making a knot in a piece of string tied to a button-hole. This my wicked friends had perceived.

How I did enjoy shooting! but I think that I must have been half-consciously ashamed of my zeal, for I tried to persuade myself that shooting was almost an intellectual employment; it required so much skill to judge where to find most game and to hunt the dogs well.

One of my autumnal visits to Maer in 1827 was memorable from meeting there Sir J. Mackintosh, who was the best converser I ever listened to. I heard afterwards with a glow of pride that he had said, "There is something in that young man that interests me." This must have been chiefly due to his perceiving that I listened with much interest to everything which he said, for I was as ignorant as a pig about his subjects of history, politics, and moral

© The Complete Work of Charles Darwin Online

philosophy. To hear of praise from an eminent person, though no doubt apt or certain to excite vanity, is, I think, good for a young man, as it helps to keep him in the right course.

My visits to Maer during these two or three succeeding years were quite delightful, independently of the autumnal shooting. Life there was perfectly free; the country was very pleasant for walking or riding; and in the evening there was much very agreeable conversation, not so personal as it generally is in large family parties, together with music. In the summer the whole family used often to sit on the steps of the old portico, with the flower-garden in front, and with the steep wooded bank opposite the house reflected in the lake, with here and there a fish rising or a water-bird paddling about. Nothing has left a more vivid picture on my mind than these evenings at Maer. I was also attached to and greatly revered my Uncle Jos; he was silent and reserved, so as to be a rather awful man; but he sometimes talked openly with me. He was the very type of an upright man, with the clearest judgment. I do not believe that any power on earth could have made him swerve an inch from what he considered the right course. I used to apply to him in my mind the well-known ode of Horace, now forgotten by me, in which the words "nec vultus tyranni, &c.," * come in.

Cambridge 1828–1831.—After having spent two

* Justum et tenacem propositi virum
Non civium ardor prava jubentium,
Non vultus instantis tyranni
Mente quatit solidâ.

© The Complete Work of Charles Darwin Online

sessions in Edinburgh, my father perceived, or he heard from my sisters, that I did not like the thought of being a physician, so he proposed that I should become a clergyman. He was very properly vehement against my turning into an idle sporting man, which then seemed my probable destination. I asked for some time to consider, as from what little I had heard or thought on the subject I had scruples about declaring my belief in all the dogmas of the Church of England; though otherwise I liked the thought of being a country clergyman. Accordingly I read with care 'Pearson on the Creeds,' and a few other books on divinity; and as I did not then in the least doubt the strict and literal truth of every word in the Bible, I soon persuaded myself that our Creed must be fully accepted.

Considering how fiercely I have been attacked by the orthodox, it seems ludicrous that I once intended to be a clergyman. Nor was this intention and my father's wish ever formally given up, but died a natural death when, on leaving Cambridge, I joined the *Beagle* as naturalist. If the phrenologists are to be trusted, I was well fitted in one respect to be a clergyman. A few years ago the secretaries of a German psychological society asked me earnestly by letter for a photograph of myself; and some time afterwards I received the proceedings of one of the meetings, in which it seemed that the shape of my head had been the subject of a public discussion, and one of the speakers declared that I had the bump of reverence developed enough for ten priests.

LOGICAL DEPT. CAMBRIDGE

© The Complete Work of Charles Darwin Online

As it was decided that I should be a clergyman, it was necessary that I should go to one of the English universities and take a degree; but as I had never opened a classical book since leaving school, I found to my dismay, that in the two intervening years I had actually forgotten, incredible as it may appear, almost everything which I had learnt, even to some few of the Greek letters. I did not therefore proceed to Cambridge at the usual time in October, but worked with a private tutor in Shrewsbury, and went to Cambridge after the Christmas vacation, early in 1828. I soon recovered my school standard of knowledge, and could translate easy Greek books, such as Homer and the Greek Testament, with moderate facility.

During the three years which I spent at Cambridge my time was wasted, as far as the academical studies were concerned, as completely as at Edinburgh and at school. I attempted mathematics, and even went during the summer of 1828 with a private tutor (a very dull man) to Barmouth, but I got on very slowly. The work was repugnant to me, chiefly from my not being able to see any meaning in the early steps in algebra. This impatience was very foolish, and in after years I have deeply regretted that I did not proceed far enough at least to understand something of the great leading principles of mathematics, for men thus endowed seem to have an extra sense. But I do not believe that I should ever have succeeded beyond a very low grade. With respect to Classics I did nothing except attend a few compulsory college

© The Complete Work of Charles Darwin Online

lectures, and the attendance was almost nominal. In my second year I had to work for a month or two to pass the Little-Go, which I did easily. Again, in my last year I worked with some earnestness for my final degree of B.A., and brushed up my Classics, together with a little Algebra and Euclid, which latter gave me much pleasure, as it did at school. In order to pass the B.A. examination, it was also necessary to get up Paley's 'Evidences of Christianity,' and his 'Moral Philosophy.' This was done in a thorough manner, and I am convinced that I could have written out the whole of the 'Evidences' with perfect correctness, but not of course in the clear language of Paley. The logic of this book and, as I may add, of his 'Natural Theology,' gave me as much delight as did Euclid. The careful study of these works, without attempting to learn any part by rote, was the only part of the academical course which, as I then felt and as I still believe, was of the least use to me in the education of my mind. I did not at that time trouble myself about Paley's premises; and taking these on trust, I was charmed and convinced by the long line of argumentation. By answering well the examination questions in Paley, by doing Euclid well, and by not failing miserably in Classics, I gained a good place among the οἱ πολλοὶ or crowd of men who do not go in for honours. Oddly enough, I cannot remember how high I stood, and my memory fluctuates between the fifth, tenth, or twelfth, name on the list.*

Public lectures on several branches were given in

* Tenth in the list of January 1831.

© The Complete Work of Charles Darwin Online

the University, attendance being quite voluntary; but I was so sickened with lectures at Edinburgh that I did not even attend Sedgwick's eloquent and interesting lectures. Had I done so I should probably have become a geologist earlier than I did. I attended, however, Henslow's lectures on Botany, and liked them much for their extreme clearness, and the admirable illustrations; but I did not study botany. Henslow used to take his pupils, including several of the older members of the University, field excursions, on foot or in coaches, to distant places, or in a barge down the river, and lectured on the rarer plants and animals which were observed. These excursions were delightful.

Although, as we shall presently see, there were some redeeming features in my life at Cambridge, my time was sadly wasted there, and worse than wasted. From my passion for shooting and for hunting, and, when this failed, for riding across country, I got into a sporting set, including some dissipated low-minded young men. We used often to dine together in the evening, though these dinners often included men of a higher stamp, and we sometimes drank too much, with jolly singing and playing at cards afterwards. I know that I ought to feel ashamed of days and evenings thus spent, but as some of my friends were very pleasant, and we were all in the highest spirits, I cannot help looking back to these times with much pleasure.

But I am glad to think that I had many other friends of a widely different nature. I was very in-

© The Complete Work of Charles Darwin Online

timate with Whitley,* who was afterwards Senior Wrangler, and we used continually to take long walks together. He inoculated me with a taste for pictures and good engravings, of which I bought some. I frequently went to the Fitzwilliam Gallery, and my taste must have been fairly good, for I certainly admired the best pictures, which I discussed with the old curator. I read also with much interest Sir Joshua Reynolds' book. This taste, though not natural to me, lasted for several years, and many of the pictures in the National Gallery in London gave me much pleasure; that of Sebastian del Piombo exciting in me a sense of sublimity.

I also got into a musical set, I believe by means of my warm-hearted friend, Herbert,† who took a high wrangler's degree. From associating with these men, and hearing them play, I acquired a strong taste for music, and used very often to time my walks so as to hear on week days the anthem in King's College Chapel. This gave me intense pleasure, so that my backbone would sometimes shiver. I am sure that there was no affectation or mere imitation in this taste, for I used generally to go by myself to King's College, and I sometimes hired the chorister boys to sing in my rooms. Nevertheless I am so utterly destitute of an ear, that I cannot perceive a discord, or keep time and hum a tune correctly; and it is a mystery how I could possibly have derived pleasure from music.

* Rev. C. Whitley, Hon. Canon of Durham, formerly Reader in Natural Philosophy in Durham University.

† The late John Maurice Herbert, County Court Judge of Cardiff and the Monmouth Circuit.

© The Complete Work of Charles Darwin Online

My musical friends soon perceived my state, and sometimes amused themselves by making me pass an examination, which consisted in ascertaining how many tunes I could recognise, when they were played rather more quickly or slowly than usual. 'God save the King,' when thus played, was a sore puzzle. There was another man with almost as bad an ear as I had, and strange to say he played a little on the flute. Once I had the triumph of beating him in one of our musical examinations.

But no pursuit at Cambridge was followed with nearly so much eagerness or gave me so much pleasure as collecting beetles. It was the mere passion for collecting, for I did not dissect them, and rarely compared their external characters with published descriptions, but got them named anyhow. I will give a proof of my zeal: one day, on tearing off some old bark, I saw two rare beetles, and seized one in each hand; then I saw a third and new kind, which I could not bear to lose, so that I popped the one which I held in my right hand into my mouth. Alas! it ejected some intensely acrid fluid, which burnt my tongue so that I was forced to spit the beetle out, which was lost, as was the third one.

I was very successful in collecting, and invented two new methods; I employed a labourer to scrape during the winter, moss off old trees and place it in a large bag, and likewise to collect the rubbish at the bottom of the barges in which reeds are brought from the fens, and thus I got some very rare species. No poet ever felt more delighted at seeing his first poem

© The Complete Work of Charles Darwin Online

published than I did at seeing, in Stephens' 'Illustrations of British Insects,' the magic words, "captured by C. Darwin, Esq." I was introduced to entomology by my second cousin, W. Darwin Fox, a clever and most pleasant man, who was then at Christ's College, and with whom I became extremely intimate. Afterwards I became well acquainted, and went out collecting, with Albert Way of Trinity, who in after years became a well-known archæologist; also with H. Thompson of the same College, afterwards a leading agriculturist, chairman of a great railway, and Member of Parliament. It seems therefore that a taste for collecting beetles is some indication of future success in life!

I am surprised what an indelible impression many of the beetles which I caught at Cambridge have left on my mind. I can remember the exact appearance of certain posts, old trees and banks where I made a good capture. The pretty *Panagæus crux-major* was a treasure in those days, and here at Down I saw a beetle running across a walk, and on picking it up instantly perceived that it differed slightly from *P. crux-major*, and it turned out to be *P. quadripunctatus*, which is only a variety or closely allied species, differing from it very slightly in outline. I had never seen in those old days Licinus alive, which to an uneducated eye hardly differs from many of the black Carabidous beetles; but my sons found here a specimen, and I instantly recognised that it was new to me; yet I had not looked at a British beetle for the last twenty years.

© The Complete Work of Charles Darwin Online

I have not as yet mentioned a circumstance which influenced my whole career more than any other. This was my friendship with Professor Henslow. Before coming up to Cambridge, I had heard of him from my brother as a man who knew every branch of science, and I was accordingly prepared to reverence him. He kept open house once every week when all undergraduates and some older members of the University, who were attached to science, used to meet in the evening. I soon got, through Fox, an invitation, and went there regularly. Before long I became well acquainted with Henslow, and during the latter half of my time at Cambridge took long walks with him on most days; so that I was called by some of the dons "the man who walks with Henslow;" and in the evening I was very often asked to join his family dinner. His knowledge was great in botany, entomology, chemistry, mineralogy, and geology. His strongest taste was to draw conclusions from long-continued minute observations. His judgment was excellent, and his whole mind well balanced; but I do not suppose that any one would say that he possessed much original genius.

He was deeply religious, and so orthodox, that he told me one day he should be grieved if a single word of the Thirty-nine Articles were altered. His moral qualities were in every way admirable. He was free from every tinge of vanity or other petty feeling; and I never saw a man who thought so little about himself or his own concerns. His temper was imperturbably good, with the most winning and

© The Complete Work of Charles Darwin Online

courteous manners; yet, as I have seen, he could be roused by any bad action to the warmest indignation and prompt action.

I once saw in his company in the streets of Cambridge almost as horrid a scene as could have been witnessed during the French Revolution. Two body-snatchers had been arrested, and whilst being taken to prison had been torn from the constable by a crowd of the roughest men, who dragged them by their legs along the muddy and stony road. They were covered from head to foot with mud, and their faces were bleeding either from having been kicked or from the stones; they looked like corpses, but the crowd was so dense that I got only a few momentary glimpses of the wretched creatures. Never in my life have I seen such wrath painted on a man's face as was shown by Henslow at this horrid scene. He tried repeatedly to penetrate the mob; but it was simply impossible. He then rushed away to the mayor, telling me not to follow him, but to get more policemen. I forget the issue, except that the two men were got into the prison without being killed.

Henslow's benevolence was unbounded, as he proved by his many excellent schemes for his poor parishioners, when in after years he held the living of Hitcham. My intimacy with such a man ought to have been, and I hope was, an inestimable benefit. I cannot resist mentioning a trifling incident, which showed his kind consideration. Whilst examining some pollen-grains on a damp surface, I saw the tubes exserted, and instantly rushed off to communicate my

© The Complete Work of Charles Darwin Online

surprising discovery to him. Now I do not suppose any other professor of botany could have helped laughing at my coming in such a hurry to make such a communication. But he agreed how interesting the phenomenon was, and explained its meaning, but made me clearly understand how well it was known ; so I left him not in the least mortified, but well pleased at having discovered for myself so remarkable a fact, but determined not to be in such a hurry again to communicate my discoveries.

Dr. Whewell was one of the older and distinguished men who sometimes visited Henslow, and on several occasions I walked home with him at night. Next to Sir J. Mackintosh he was the best converser on grave subjects to whom I ever listened. Leonard Jenyns,* who afterwards published some good essays in Natural History,† often stayed with Henslow, who was his brother-in-law. I visited him at his parsonage on the borders of the Fens [Swaffham Bulbeck], and had many a good walk and talk with him about Natural History. I became also acquainted with several other men older than me, who did not care much about science, but were friends of Henslow. One was a Scotchman, brother of Sir Alexander Ramsay, and tutor of Jesus College ; he was a delightful man, but did not live for many years. Another was Mr. Dawes, afterwards Dean of Hereford, and famous for his success in the education of the poor.

* The well-known Soame Jenyns was cousin to Mr. Jenyns' father.

† Mr. Jenyns (now Blomefield) described the fish for the Zoology of the *Beagle;* and is author of a long series of papers, chiefly Zoological.

© The Complete Work of Charles Darwin Online

These men and others of the same standing, together with Henslow, used sometimes to take distant excursions into the country, which I was allowed to join, and they were most agreeable.

Looking back, I infer that there must have been something in me a little superior to the common run of youths, otherwise the above-mentioned men, so much older than me and higher in academical position, would never have allowed me to associate with them. Certainly I was not aware of any such superiority, and I remember one of my sporting friends, Turner, who saw me at work with my beetles, saying that I should some day be a Fellow of the Royal Society, and the notion seemed to me preposterous.

During my last year at Cambridge, I read with care and profound interest Humboldt's 'Personal Narrative.' This work, and Sir J. Herschel's 'Introduction to the Study of Natural Philosophy,' stirred up in me a burning zeal to add even the most humble contribution to the noble structure of Natural Science. No one or a dozen other books influenced me nearly so much as these two. I copied out from Humboldt long passages about Teneriffe, and read them aloud on one of the above-mentioned excursions, to (I think) Henslow, Ramsay, and Dawes, for on a previous occasion I had talked about the glories of Teneriffe, and some of the party declared they would endeavour to go there; but I think that they were only half in earnest. I was, however, quite in earnest, and got an introduction to a merchant in London to enquire about ships; but the

© The Complete Work of Charles Darwin Online

scheme was, of course, knocked on the head by the voyage of the *Beagle*.

My summer vacations were given up to collecting beetles, to some reading, and short tours. In the autumn my whole time was devoted to shooting, chiefly at Woodhouse and Maer, and sometimes with young Eyton of Eyton. Upon the whole the three years which I spent at Cambridge were the most joyful in my happy life; for I was then in excellent health, and almost always in high spirits.

As I had at first come up to Cambridge at Christmas, I was forced to keep two terms after passing my final examination, at the commencement of 1831; and Henslow then persuaded me to begin the study of geology. Therefore on my return to Shropshire I examined sections, and coloured a map of parts round Shrewsbury. Professor Sedgwick intended to visit North Wales in the beginning of August to pursue his famous geological investigations amongst the older rocks, and Henslow asked him to allow me to accompany him.* Accordingly he came and slept at my father's house.

A short conversation with him during this evening produced a strong impression on my mind. Whilst examining an old gravel-pit near Shrewsbury, a

* In connection with this tour my father used to tell a story about Sedgwick: they had started from their inn one morning, and had walked a mile or two, when Sedgwick suddenly stopped, and vowed that he would return, being certain "that damned scoundrel" (the waiter) had not given the chambermaid the sixpence intrusted to him for the purpose. He was ultimately persuaded to give up the project, seeing that there was no reason for suspecting the waiter of especial perfidy.—F. D.

© The Complete Work of Charles Darwin Online

labourer told me that he had found in it a large worn tropical Volute shell, such as may be seen on the chimney-pieces of cottages; and as he would not sell the shell, I was convinced that he had really found it in the pit. I told Sedgwick of the fact, and he at once said (no doubt truly) that it must have been thrown away by some one into the pit; but then added, if really embedded there it would be the greatest misfortune to geology, as it would overthrow all that we know about the superficial deposits of the Midland Counties. These gravel-beds belong in fact to the glacial period, and in after years I found in them broken arctic shells. But I was then utterly astonished at Sedgwick not being delighted at so wonderful a fact as a tropical shell being found near the surface in the middle of England. Nothing before had ever made me thoroughly realise, though I had read various scientific books, that science consists in grouping facts so that general laws or conclusions may be drawn from them.

Next morning we started for Llangollen, Conway, Bangor, and Capel Curig. This tour was of decided use in teaching me a little how to make out the geology of a country. Sedgwick often sent me on a line parallel to his, telling me to bring back specimens of the rocks and to mark the stratification on a map. I have little doubt that he did this for my good, as I was too ignorant to have aided him. On this tour I had a striking instance how easy it is to overlook phenomena, however conspicuous, before they have been observed by any one. We spent

© The Complete Work of Charles Darwin Online

many hours in Cwm Idwal, examining all the rocks with extreme care, as Sedgwick was anxious to find fossils in them; but neither of us saw a trace of the wonderful glacial phenomena all around us; we did not notice the plainly scored rocks, the perched boulders, the lateral and terminal moraines. Yet these phenomena are so conspicuous that, as I declared in a paper published many years afterwards in the 'Philosophical Magazine,'* a house burnt down by fire did not tell its story more plainly than did this valley. If it had still been filled by a glacier, the phenomena would have been less distinct than they now are.

At Capel Curig I left Sedgwick and went in a straight line by compass and map across the mountains to Barmouth, never following any track unless it coincided with my course. I thus came on some strange wild places, and enjoyed much this manner of travelling. I visited Barmouth to see some Cambridge friends who were reading there, and thence returned to Shrewsbury and to Maer for shooting; for at that time I should have thought myself mad to give up the first days of partridge-shooting for geology or any other science.

Voyage of the 'Beagle' from December 27, 1831, *to October* 2, 1836.

On returning home from my short geological tour in North Wales, I found a letter from Henslow, informing me that Captain Fitz-Roy was willing to give

* 'Philosophical Magazine,' 1842.

© The Complete Work of Charles Darwin Online

up part of his own cabin to any young man who would volunteer to go with him without pay as naturalist to the Voyage of the *Beagle*. I have given, as I believe, in my MS. Journal an account of all the circumstances which then occurred; I will here only say that I was instantly eager to accept the offer, but my father strongly objected, adding the words, fortunate for me, "If you can find any man of common sense who advises you to go I will give my consent." So I wrote that evening and refused the offer. On the next morning I went to Maer to be ready for September 1st, and, whilst out shooting, my uncle* sent for me, offering to drive me over to Shrewsbury and talk with my father, as my uncle thought it would be wise in me to accept the offer. My father always maintained that he was one of the most sensible men in the world, and he at once consented in the kindest manner. I had been rather extravagant at Cambridge, and to console my father, said, "that I should be deuced clever to spend more than my allowance whilst on board the *Beagle*;" but he answered with a smile, "But they tell me you are very clever."

Next day I started for Cambridge to see Henslow, and thence to London to see Fitz-Roy, and all was soon arranged. Afterwards, on becoming very intimate with Fitz-Roy, I heard that I had run a very narrow risk of being rejected, on account of the shape of my nose! He was an ardent disciple of Lavater, and was convinced that he could judge of a man's

* Josiah Wedgwood.

© The Complete Work of Charles Darwin Online

character by the outline of his features; and he doubted whether any one with my nose could possess sufficient energy and determination for the voyage. But I think he was afterwards well satisfied that my nose had spoken falsely.

Fitz-Roy's character was a singular one, with very many noble features: he was devoted to his duty, generous to a fault, bold, determined, and indomitably energetic, and an ardent friend to all under his sway. He would undertake any sort of trouble to assist those whom he thought deserved assistance. He was a handsome man, strikingly like a gentleman, with highly courteous manners, which resembled those of his maternal uncle, the famous Lord Castlereagh, as I was told by the Minister at Rio. Nevertheless he must have inherited much in his appearance from Charles II., for Dr. Wallich gave me a collection of photographs which he had made, and I was struck with the resemblance of one to Fitz-Roy; and on looking at the name, I found it Ch. E. Sobieski Stuart, Count d'Albanie, a descendant of the same monarch.

Fitz-Roy's temper was a most unfortunate one. It was usually worst in the early morning, and with his eagle eye he could generally detect something amiss about the ship, and was then unsparing in his blame. He was very kind to me, but was a man very difficult to live with on the intimate terms which necessarily followed from our messing by ourselves in the same cabin. We had several quarrels; for instance, early in the voyage at Bahia, in Brazil, he defended and praised

© The Complete Work of Charles Darwin Online

slavery, which I abominated, and told me that he had just visited a great slave-owner, who had called up many of his slaves and asked them whether they were happy, and whether they wished to be free, and all answered "No." I then asked him, perhaps with a sneer, whether he thought that the answer of slaves in the presence of their master was worth anything? This made him excessively angry, and he said that as I doubted his word we could not live any longer together. I thought that I should have been compelled to leave the ship; but as soon as the news spread, which it did quickly, as the captain sent for the first lieutenant to assuage his anger by abusing me, I was deeply gratified by receiving an invitation from all the gun-room officers to mess with them. But after a few hours Fitz-Roy showed his usual magnanimity by sending an officer to me with an apology and a request that I would continue to live with him.

His character was in several respects one of the most noble which I have ever known.

The voyage of the *Beagle* has been by far the most important event in my life, and has determined my whole career; yet it depended on so small a circumstance as my uncle offering to drive me thirty miles to Shrewsbury, which few uncles would have done, and on such a trifle as the shape of my nose. I have always felt that I owe to the voyage the first real training or education of my mind; I was led to attend closely to several branches of natural history, and thus my powers of observation

© The Complete Work of Charles Darwin Online

were improved, though they were always fairly developed.

The investigation of the geology of all the places visited was far more important, as reasoning here comes into play. On first examining a new district nothing can appear more hopeless than the chaos of rocks; but by recording the stratification and nature of the rocks and fossils at many points, always reasoning and predicting what will be found elsewhere, light soon begins to dawn on the district, and the structure of the whole becomes more or less intelligible. I had brought with me the first volume of Lyell's 'Principles of Geology,' which I studied attentively; and the book was of the highest service to me in many ways. The very first place which I examined, namely St. Jago in the Cape de Verde islands, showed me clearly the wonderful superiority of Lyell's manner of treating geology, compared with that of any other author, whose works I had with me or ever afterwards read.

Another of my occupations was collecting animals of all classes, briefly describing and roughly dissecting many of the marine ones; but from not being able to draw, and from not having sufficient anatomical knowledge, a great pile of MS. which I made during the voyage has proved almost useless. I thus lost much time, with the exception of that spent in acquiring some knowledge of the Crustaceans, as this was of service when in after years I undertook a monograph of the Cirripedia.

During some part of the day I wrote my Journal,

© The Complete Work of Charles Darwin Online

and took much pains in describing carefully and vividly all that I had seen; and this was good practice. My Journal served also, in part, as letters to my home, and portions were sent to England whenever there was an opportunity.

The above various special studies were, however, of no importance compared with the habit of energetic industry and of concentrated attention to whatever I was engaged in, which I then acquired. Everything about which I thought or read was made to bear directly on what I had seen or was likely to see; and this habit of mind was continued during the five years of the voyage. I feel sure that it was this training which has enabled me to do whatever I have done in science.

Looking backwards, I can now perceive how my love for science gradually preponderated over every other taste. During the first two years my old passion for shooting survived in nearly full force, and I shot myself all the birds and animals for my collection; but gradually I gave up my gun more and more, and finally altogether, to my servant, as shooting interfered with my work, more especially with making out the geological structure of a country. I discovered, though unconsciously and insensibly, that the pleasure of observing and reasoning was a much higher one than that of skill and sport. That my mind became developed through my pursuits during the voyage is rendered probable by a remark made by my father, who was the most acute observer whom I ever saw, of a sceptical disposition, and far from being a believer

© The Complete Work of Charles Darwin Online

in phrenology; for on first seeing me after the voyage, he turned round to my sisters, and exclaimed, "Why, the shape of his head is quite altered."

To return to the voyage. On September 11th (1831), I paid a flying visit with Fitz-Roy to the *Beagle* at Plymouth. Thence to Shrewsbury to wish my father and sisters a long farewell. On October 24th I took up my residence at Plymouth, and remained there until December 27th, when the *Beagle* finally left the shores of England for her circumnavigation of the world. We made two earlier attempts to sail, but were driven back each time by heavy gales. These two months at Plymouth were the most miserable which I ever spent, though I exerted myself in various ways. I was out of spirits at the thought of leaving all my family and friends for so long a time, and the weather seemed to me inexpressibly gloomy. I was also troubled with palpitation and pain about the heart, and like many a young ignorant man, especially one with a smattering of medical knowledge, was convinced that I had heart disease. I did not consult any doctor, as I fully expected to hear the verdict that I was not fit for the voyage, and I was resolved to go at all hazards.

I need not here refer to the events of the voyage—where we went and what we did—as I have given a sufficiently full account in my published Journal. The glories of the vegetation of the Tropics rise before my mind at the present time more vividly than anything else; though the sense of sublimity, which the great deserts of Patagonia and the forest-clad mountains of

© The Complete Work of Charles Darwin Online

Tierra del Fuego excited in me, has left an indelible impression on my mind. The sight of a naked savage in his native land is an event which can never be forgotten. Many of my excursions on horseback through wild countries, or in the boats, some of which lasted several weeks, were deeply interesting: their discomfort and some degree of danger were at that time hardly a drawback, and none at all afterwards. I also reflect with high satisfaction on some of my scientific work, such as solving the problem of coral islands, and making out the geological structure of certain islands, for instance, St. Helena. Nor must I pass over the discovery of the singular relations of the animals and plants inhabiting the several islands of the Galapagos archipelago, and of all of them to the inhabitants of South America.

As far as I can judge of myself, I worked to the utmost during the voyage from the mere pleasure of investigation, and from my strong desire to add a few facts to the great mass of facts in Natural Science. But I was also ambitious to take a fair place among scientific men,—whether more ambitious or less so than most of my fellow-workers, I can form no opinion.

The geology of St. Jago is very striking, yet simple: a stream of lava formerly flowed over the bed of the sea, formed of triturated recent shells and corals, which it has baked into a hard white rock. Since then the whole island has been upheaved. But the line of white rock revealed to me a new and important fact, namely, that there had been afterwards subsi-

© The Complete Work of Charles Darwin Online

dence round the craters, which had since been in action, and had poured forth lava. It then first dawned on me that I might perhaps write a book on the geology of the various countries visited, and this made me thrill with delight. That was a memorable hour to me, and how distinctly I can call to mind the low cliff of lava beneath which I rested, with the sun glaring hot, a few strange desert plants growing near, and with living corals in the tidal pools at my feet. Later in the voyage, Fitz-Roy asked me to read some of my Journal, and declared it would be worth publishing; so here was a second book in prospect!

Towards the close of our voyage I received a letter whilst at Ascension, in which my sisters told me that Sedgwick had called on my father, and said that I should take a place among the leading scientific men. I could not at the time understand how he could have learnt anything of my proceedings, but I heard (I believe afterwards) that Henslow had read some of the letters which I wrote to him before the Philosophical Society of Cambridge,* and had printed them for private distribution. My collection of fossil bones, which had been sent to Henslow, also excited considerable attention amongst palæontologists. After reading this letter, I clambered over the mountains of Ascension with a bounding step, and made the volcanic rocks resound under my geological hammer. All this shows how ambitious I was; but I think that I can

* Read at the meeting held November 16, 1835, and printed in a pamphlet of 31 pp. for distribution among the members of the Society.

© The Complete Work of Charles Darwin Online

say with truth that in after years, though I cared in the highest degree for the approbation of such men as Lyell and Hooker, who were my friends, I did not care much about the general public. I do not mean to say that a favourable review or a large sale of my books did not please me greatly, but the pleasure was a fleeting one, and I am sure that I have never turned one inch out of my course to gain fame.

From my return to England (*October* 2, 1836) *to my marriage* (*January* 29, 1839).

These two years and three months were the most active ones which I ever spent, though I was occasionally unwell, and so lost some time. After going backwards and forwards several times between Shrewsbury, Maer, Cambridge, and London, I settled in lodgings at Cambridge * on December 13th, where all my collections were under the care of Henslow. I stayed here three months, and got my minerals and rocks examined by the aid of Professor Miller.

I began preparing my 'Journal of Travels,' which was not hard work, as my MS. Journal had been written with care, and my chief labour was making an abstract of my more interesting scientific results. I sent also, at the request of Lyell, a short account of my observations on the elevation of the coast of Chile to the Geological Society.†

On March 7th, 1837, I took lodgings in Great Marlborough Street in London, and remained there for

* In Fitzwilliam Street.

† 'Geolog. Soc. Proc.' ii. 1838, pp. 446–449.

© The Complete Work of Charles Darwin Online

nearly two years, until I was married. During these two years I finished my Journal, read several papers before the Geological Society, began preparing the MS. for my 'Geological Observations,' and arranged for the publication of the 'Zoology of the Voyage of the *Beagle*.' In July I opened my first note-book for facts in relation to the Origin of Species, about which I had long reflected, and never ceased working for the next twenty years.

During these two years I also went a little into society, and acted as one of the honorary secretaries of the Geological Society. I saw a great deal of Lyell. One of his chief characteristics was his sympathy with the work of others, and I was as much astonished as delighted at the interest which he showed when, on my return to England, I explained to him my views on coral reefs. This encouraged me greatly, and his advice and example had much influence on me. During this time I saw also a good deal of Robert Brown; I used often to call and sit with him during his breakfast on Sunday mornings, and he poured forth a rich treasure of curious observations and acute remarks, but they almost always related to minute points, and he never with me discussed large or general questions in science.

During these two years I took several short excursions as a relaxation, and one longer one to the Parallel Roads of Glen Roy, an account of which was published in the 'Philosophical Transactions.'* This paper was a great failure, and I am ashamed of it.

* 1839, pp. 39–82.

© The Complete Work of Charles Darwin Online

Having been deeply impressed with what I had seen of the elevation of the land in South America, I attributed the parallel lines to the action of the sea; but I had to give up this view when Agassiz propounded his glacier-lake theory. Because no other explanation was possible under our then state of knowledge, I argued in favour of sea-action; and my error has been a good lesson to me never to trust in science to the principle of exclusion.

As I was not able to work all day at science, I read a good deal during these two years on various subjects, including some metaphysical books; but I was not well fitted for such studies. About this time I took much delight in Wordsworth's and Coleridge's poetry; and can boast that I read the 'Excursion' twice through. Formerly Milton's 'Paradise Lost' had been my chief favourite, and in my excursions during the voyage of the *Beagle*, when I could take only a single volume, I always chose Milton.

From my marriage, January 29, 1839, *and residence in Upper Gower Street, to our leaving London and settling at Down, September* 14, 1842.

After speaking of his happy married life, and of his children, he continues:—

During the three years and eight months whilst we resided in London, I did less scientific work, though I worked as hard as I possibly could, than during any other equal length of time in my life. This was owing to frequently recurring unwellness, and to one long and serious illness. The greater part of my

© The Complete Work of Charles Darwin Online

time, when I could do anything, was devoted to my work on 'Coral Reefs,' which I had begun before my marriage, and of which the last proof-sheet was corrected on May 6th, 1842. This book, though a small one, cost me twenty months of hard work, as I had to read every work on the islands of the Pacific and to consult many charts. It was thought highly of by scientific men, and the theory therein given is, I think, now well established.

No other work of mine was begun in so deductive a spirit as this, for the whole theory was thought out on the west coast of South America, before I had seen a true coral reef. I had therefore only to verify and extend my views by a careful examination of living reefs. But it should be observed that I had during the two previous years been incessantly attending to the effects on the shores of South America of the intermittent elevation of the land, together with denudation and the deposition of sediment. This necessarily led me to reflect much on the effects of subsidence, and it was easy to replace in imagination the continued deposition of sediment by the upward growth of corals. To do this was to form my theory of the formation of barrier-reefs and atolls.

Besides my work on coral-reefs, during my residence in London, I read before the Geological Society papers on the Erratic Boulders of South America,* on Earthquakes,† and on the Formation by the Agency of Earth-worms of Mould.‡ I also continued to superin-

* 'Geolog. Soc. Proc.' iii. 1842. † 'Geolog. Trans.' v. 1840.
‡ 'Geolog. Soc. Proc.' ii. 1838.

© The Complete Work of Charles Darwin Online

tend the publication of the 'Zoology of the Voyage of the *Beagle*.' Nor did I ever intermit collecting facts bearing on the origin of species; and I could sometimes do this when I could do nothing else from illness.

In the summer of 1842 I was stronger than I had been for some time, and took a little tour by myself in North Wales, for the sake of observing the effects of the old glaciers which formerly filled all the larger valleys. I published a short account of what I saw in the 'Philosophical Magazine.'* This excursion interested me greatly, and it was the last time I was ever strong enough to climb mountains or to take long walks such as are necessary for geological work.

During the early part of our life in London, I was strong enough to go into general society, and saw a good deal of several scientific men, and other more or less distinguished men. I will give my impressions with respect to some of them, though I have little to say worth saying.

I saw more of Lyell than of any other man, both before and after my marriage. His mind was characterised, as it appeared to me, by clearness, caution, sound judgment, and a good deal of originality. When I made any remark to him on Geology, he never rested until he saw the whole case clearly, and often made me see it more clearly than I had done before. He would advance all possible objections to my suggestion, and even after these were exhausted would long remain dubious. A second characteristic

* 'Philosophical Magazine,' 1842.

© The Complete Work of Charles Darwin Online

was his hearty sympathy with the work of other scientific men.*

On my return from the voyage of the *Beagle*, I explained to him my views on coral-reefs, which differed from his, and I was greatly surprised and encouraged by the vivid interest which he showed. His delight in science was ardent, and he felt the keenest interest in the future progress of mankind. He was very kind-hearted, and thoroughly liberal in his religious beliefs, or rather disbeliefs ; but he was a strong theist. His candour was highly remarkable. He exhibited this by becoming a convert to the Descent theory, though he had gained much fame by opposing Lamarck's views, and this after he had grown old. He reminded me that I had many years before said to him, when discussing the opposition of the old school of geologists to his new views, "What a good thing it would be if every scientific man was to die when sixty years old, as afterwards he would be sure to oppose all new doctrines." But he hoped that now he might be allowed to live.

The science of Geology is enormously indebted to Lyell—more so, as I believe, than to any other man who ever lived. When [I was] starting on the voyage of the *Beagle*, the sagacious Henslow, who, like all other geologists, believed at that time in successive cataclysms, advised me to get and study the first volume of the 'Principles,' which had then just been published,

* The slight repetition here observable is accounted for by the notes on Lyell, &c., having been added in April, 1881, a few years after the rest of the 'Recollections' were written.

© The Complete Work of Charles Darwin Online

but on no account to accept the views therein advocated. How differently would any one now speak of the 'Principles'! I am proud to remember that the first place, namely, St. Jago, in the Cape de Verde archipelago, in which I geologised, convinced me of the infinite superiority of Lyell's views over those advocated in any other work known to me.

The powerful effects of Lyell's works could formerly be plainly seen in the different progress of the science in France and England. The present total oblivion of Elie de Beaumont's wild hypotheses, such as his 'Craters of Elevation' and 'Lines of Elevation' (which latter hypothesis I heard Sedgwick at the Geological Society lauding to the skies), may be largely attributed to Lyell.

I saw a good deal of Robert Brown, "facile Princeps Botanicorum," as he was called by Humboldt. He seemed to me to be chiefly remarkable for the minuteness of his observations, and their perfect accuracy. His knowledge was extraordinarily great, and much died with him, owing to his excessive fear of ever making a mistake. He poured out his knowledge to me in the most unreserved manner, yet was strangely jealous on some points. I called on him two or three times before the voyage of the *Beagle*, and on one occasion he asked me to look through a microscope and describe what I saw. This I did, and believe now that it was the marvellous currents of protoplasm in some vegetable cell. I then asked him what I had seen; but he answered me, "That is my little secret."

© The Complete Work of Charles Darwin Online

He was capable of the most generous actions. When old, much out of health, and quite unfit for any exertion, he daily visited (as Hooker told me) an old man-servant, who lived at a distance (and whom he supported), and read aloud to him. This is enough to make up for any degree of scientific penuriousness or jealousy.

I may here mention a few other eminent men, whom I have occasionally seen, but I have little to say about them worth saying. I felt a high reverence for Sir J. Herschel, and was delighted to dine with him at his charming house at the Cape of Good Hope, and afterwards at his London house. I saw him, also, on a few other occasions. He never talked much, but every word which he uttered was worth listening to.

I once met at breakfast at Sir R. Murchison's house the illustrious Humboldt, who honoured me by expressing a wish to see me. I was a little disappointed with the great man, but my anticipations probably were too high. I can remember nothing distinctly about our interview, except that Humboldt was very cheerful and talked much.

—— reminds me of Buckle whom I once met at Hensleigh Wedgwood's. I was very glad to learn from him his system of collecting facts. He told me that he bought all the books which he read, and made a full index, to each, of the facts which he thought might prove serviceable to him, and that he could always remember in what book he had read anything, for his memory was wonderful. I asked him how at first he could judge what facts would be

© The Complete Work of Charles Darwin Online

serviceable, and he answered that he did not know, but that a sort of instinct guided him. From this habit of making indices, he was enabled to give the astonishing number of references on all sorts of subjects, which may be found in his 'History of Civilisation.' This book I thought most interesting, and read it twice, but I doubt whether his generalisations are worth anything. Buckle was a great talker, and I listened to him saying hardly a word, nor indeed could I have done so for he left no gaps. When Mrs. Farrer began to sing, I jumped up and said that I must listen to her; after I had moved away he turned round to a friend and said (as was overheard by my brother), "Well, Mr. Darwin's books are much better than his conversation."

Of other great literary men, I once met Sydney Smith at Dean Milman's house. There was something inexplicably amusing in every word which he uttered. Perhaps this was partly due to the expectation of being amused. He was talking about Lady Cork, who was then extremely old. This was the lady who, as he said, was once so much affected by one of his charity sermons, that she *borrowed* a guinea from a friend to put in the plate. He now said "It is generally believed that my dear old friend Lady Cork has been overlooked," and he said this in such a manner that no one could for a moment doubt that he meant that his dear old friend had been overlooked by the devil. How he managed to express this I know not.

I likewise once met Macaulay at Lord Stanhope's

© The Complete Work of Charles Darwin Online

(the historian's) house, and as there was only one other man at dinner, I had a grand opportunity of hearing him converse, and he was very agreeable. He did not talk at all too much ; nor indeed could such a man talk too much, as long as he allowed others to turn the stream of his conversation, and this he did allow.

Lord Stanhope once gave me a curious little proof of the accuracy and fulness of Macaulay's memory : many historians used often to meet at Lord Stanhope's house, and in discussing various subjects they would sometimes differ from Macaulay, and formerly they often referred to some book to see who was right ; but latterly, as Lord Stanhope noticed, no historian ever took this trouble, and whatever Macaulay said was final.

On another occasion I met at Lord Stanhope's house, one of his parties of historians and other literary men, and amongst them were Motley and Grote. After luncheon I walked about Chevening Park for nearly an hour with Grote, and was much interested by his conversation and pleased by the simplicity and absence of all pretension in his manners.

Long ago I dined occasionally with the old Earl, the father of the historian ; he was a strange man, but what little I knew of him I liked much. He was frank, genial, and pleasant. He had strongly marked features, with a brown complexion, and his clothes, when I saw him, were all brown. He seemed to believe in everything which was to others utterly incredible. He said one day to me, " Why don't you

© The Complete Work of Charles Darwin Online

give up your fiddle-faddle of geology and zoology, and turn to the occult sciences?" The historian, then Lord Mahon, seemed shocked at such a speech to me, and his charming wife much amused.

The last man whom I will mention is Carlyle, seen by me several times at my brother's house, and two or three times at my own house. His talk was very racy and interesting, just like his writings, but he sometimes went on too long on the same subject. I remember a funny dinner at my brother's, where, amongst a few others, were Babbage and Lyell, both of whom liked to talk. Carlyle, however, silenced every one by haranguing during the whole dinner on the advantages of silence. After dinner Babbage, in his grimmest manner, thanked Carlyle for his very interesting lecture on silence.

Carlyle sneered at almost every one: one day in my house he called Grote's 'History' "a fetid quagmire, with nothing spiritual about it." I always thought, until his 'Reminiscences' appeared, that his sneers were partly jokes, but this now seems rather doubtful. His expression was that of a depressed, almost despondent yet benevolent, man; and it is notorious how heartily he laughed. I believe that his benevolence was real, though stained by not a little jealousy. No one can doubt about his extraordinary power of drawing pictures of things and men—far more vivid, as it appears to me, than any drawn by Macaulay. Whether his pictures of men were true ones is another question.

He has been all-powerful in impressing some grand

© The Complete Work of Charles Darwin Online

moral truths on the minds of men. On the other hand, his views about slavery were revolting. In his eyes might was right. His mind seemed to me a very narrow one; even if all branches of science, which he despised, are excluded. It is astonishing to me that Kingsley should have spoken of him as a man well fitted to advance science. He laughed to scorn the idea that a mathematician, such as Whewell, could judge, as I maintained he could, of Goethe's views on light. He thought it a most ridiculous thing that any one should care whether a glacier moved a little quicker or a little slower, or moved at all. As far as I could judge, I never met a man with a mind so ill adapted for scientific research.

Whilst living in London, I attended as regularly as I could the meetings of several scientific societies, and acted as secretary to the Geological Society. But such attendance, and ordinary society, suited my health so badly that we resolved to live in the country, which we both preferred and have never repented of.

Residence at Down from September 14, 1842, *to the present time*, 1876.

After several fruitless searches in Surrey and elsewhere, we found this house and purchased it. I was pleased with the diversified appearance of the vegetation proper to a chalk district, and so unlike what I had been accustomed to in the Midland counties; and still more pleased with the extreme quietness and rusticity of the place. It is

© The Complete Work of Charles Darwin Online

not, however, quite so retired a place as a writer in a German periodical makes it, who says that my house can be approached only by a mule-track! Our fixing ourselves here has answered admirably in one way, which we did not anticipate, namely, by being very convenient for frequent visits from our children.

Few persons can have lived a more retired life than we have done. Besides short visits to the houses of relations, and occasionally to the seaside or elsewhere, we have gone nowhere. During the first part of our residence we went a little into society, and received a few friends here; but my health almost always suffered from the excitement, violent shivering and vomiting attacks being thus brought on. I have therefore been compelled for many years to give up all dinner-parties; and this has been somewhat of a deprivation to me, as such parties always put me into high spirits. From the same cause I have been able to invite here very few scientific acquaintances.

My chief enjoyment and sole employment throughout life has been scientific work; and the excitement from such work makes me for the time forget, or drives quite away, my daily discomfort. I have therefore nothing to record during the rest of my life, except the publication of my several books. Perhaps a few details how they arose may be worth giving.

My several Publications.—In the early part of 1844, my observations on the volcanic islands visited during the voyage of the *Beagle* were published. In 1845, I took much pains in correcting a new edition of my 'Journal of Researches,' which was originally published

© The Complete Work of Charles Darwin Online

in 1839 as part of Fitz-Roy's work. The success of this my first literary child always tickles my vanity more than that of any of my other books. Even to this day it sells steadily in England and the United States, and has been translated for the second time into German, and into French and other languages. This success of a book of travels, especially of a scientific one, so many years after its first publication, is surprising. Ten thousand copies have been sold in England of the second edition. In 1846 my 'Geological Observations on South America' were published. I record in a little diary, which I have always kept, that my three geological books ('Coral Reefs' included) consumed four and a half years' steady work; "and now it is ten years since my return to England. How much time have I lost by illness?" I have nothing to say about these three books except that to my surprise new editions have lately been called for.*

In October, 1846, I began to work on 'Cirripedia.' When on the coast of Chile, I found a most curious form, which burrowed into the shells of Concholepas, and which differed so much from all other Cirripedes that I had to form a new sub-order for its sole reception. Lately an allied burrowing genus has been found on the shores of Portugal. To understand the structure of my new Cirripede I had to examine and dissect many of the common forms; and this gradually led me on to take up the whole group. I worked steadily on this subject for the next eight years, and ultimately

* 'Geological Observations,' 2nd Edit. 1876. 'Coral Reefs,' 2nd Edit. 1874.

© The Complete Work of Charles Darwin Online

published two thick volumes,* describing all the known living species, and two thin quartos on the extinct species. I do not doubt that Sir E. Lytton Bulwer had me in his mind when he introduced in one of his novels a Professor Long, who had written two huge volumes on limpets.

Although I was employed during eight years on this work, yet I record in my diary that about two years out of this time was lost by illness. On this account I went in 1848 for some months to Malvern for hydropathic treatment, which did me much good, so that on my return home I was able to resume work. So much was I out of health that when my dear father died on November 13th, 1848, I was unable to attend his funeral or to act as one of his executors.

My work on the Cirripedia possesses, I think, considerable value, as besides describing several new and remarkable forms, I made out the homologies of the various parts—I discovered the cementing apparatus, though I blundered dreadfully about the cement glands—and lastly I proved the existence in certain genera of minute males complemental to and parasitic on the hermaphrodites. This latter discovery has at last been fully confirmed; though at one time a German writer was pleased to attribute the whole account to my fertile imagination. The Cirripedes form a highly varying and difficult group of species to class; and my work was of considerable use to me, when I had to discuss in the 'Origin of Species' the principles of a natural classification. Nevertheless, I doubt whether

* Published by the Ray Society.

© The Complete Work of Charles Darwin Online

the work was worth the consumption of so much time.

From September 1854 I devoted my whole time to arranging my huge pile of notes, to observing, and to experimenting in relation to the transmutation of species. During the voyage of the *Beagle* I had been deeply impressed by discovering in the Pampean formation great fossil animals covered with armour like that on the existing armadillos; secondly, by the manner in which closely allied animals replace one another in proceeding southwards over the Continent; and thirdly, by the South American character of most of the productions of the Galapagos archipelago, and more especially by the manner in which they differ slightly on each island of the group; none of the islands appearing to be very ancient in a geological sense.

It was evident that such facts as these, as well as many others, could only be explained on the supposition that species gradually become modified; and the subject haunted me. But it was equally evident that neither the action of the surrounding conditions, nor the will of the organisms (especially in the case of plants) could account for the innumerable cases in which organisms of every kind are beautifully adapted to their habits of life—for instance, a woodpecker or a tree-frog to climb trees, or a seed for dispersal by hooks or plumes. I had always been much struck by such adaptations, and until these could be explained it seemed to me almost useless to endeavour to prove by indirect evidence that species have been modified.

© The Complete Work of Charles Darwin Online

After my return to England it appeared to me that by following the example of Lyell in Geology, and by collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject. My first note-book was opened in July 1837. I worked on true Baconian principles, and without any theory collected facts on a wholesale scale, more especially with respect to domesticated productions, by printed enquiries, by conversation with skilful breeders and gardeners, and by extensive reading. When I see the list of books of all kinds which I read and abstracted, including whole series of Journals and Transactions, I am surprised at my industry. I soon perceived that selection was the keystone of man's success in making useful races of animals and plants. But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me.

In October 1838, that is, fifteen months after I had begun my systematic enquiry, I happened to read for amusement 'Malthus on Population,' and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of animals and plants, it at once struck me that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. The result of this would be the formation of new species. Here then I had at last got a theory by which to work; but I was so anxious to avoid prejudice, that I determined not for

© The Complete Work of Charles Darwin Online

some time to write even the briefest sketch of it. In June 1842 I first allowed myself the satisfaction of writing a very brief abstract of my theory in pencil in 35 pages; and this was enlarged during the summer of 1844 into one of 230 pages, which I had fairly copied out and still possess.

But at that time I overlooked one problem of great importance; and it is astonishing to me, except on the principle of Columbus and his egg, how I could have overlooked it and its solution. This problem is the tendency in organic beings descended from the same stock to diverge in character as they become modified. That they have diverged greatly is obvious from the manner in which species of all kinds can be classed under genera, genera under families, families under sub-orders and so forth; and I can remember the very spot in the road, whilst in my carriage, when to my joy the solution occurred to me; and this was long after I had come to Down. The solution, as I believe, is that the modified offspring of all dominant and increasing forms tend to become adapted to many and highly diversified places in the economy of nature.

Early in 1856 Lyell advised me to write out my views pretty fully, and I began at once to do so on a scale three or four times as extensive as that which was afterwards followed in my 'Origin of Species;' yet it was only an abstract of the materials which I had collected, and I got through about half the work on this scale. But my plans were overthrown, for early in the summer of 1858 Mr. Wallace, who was

© The Complete Work of Charles Darwin Online

then in the Malay archipelago, sent me an essay "On the Tendency of Varieties to depart indefinitely from the Original Type;" and this essay contained exactly the same theory as mine. Mr. Wallace expressed the wish that if I thought well of his essay, I should send it to Lyell for perusal.

The circumstances under which I consented at the request of Lyell and Hooker to allow of an abstract from my MS., together with a letter to Asa Gray, dated September 5, 1857, to be published at the same time with Wallace's Essay, are given in the 'Journal of the Proceedings of the Linnean Society,' 1858, p. 45. I was at first very unwilling to consent, as I thought Mr. Wallace might consider my doing so unjustifiable, for I did not then know how generous and noble was his disposition. The extract from my MS. and the letter to Asa Gray had neither been intended for publication, and were badly written. Mr. Wallace's essay, on the other hand, was admirably expressed and quite clear. Nevertheless, our joint productions excited very little attention, and the only published notice of them which I can remember was by Professor Haughton of Dublin, whose verdict was that all that was new in them was false, and what was true was old. This shows how necessary it is that any new view should be explained at considerable length in order to arouse public attention.

In September 1858 I set to work by the strong advice of Lyell and Hooker to prepare a volume on the transmutation of species, but was often interrupted by ill-health, and short visits to Dr. Lane's delightful

© The Complete Work of Charles Darwin Online

hydropathic establishment at Moor Park. I abstracted the MS. begun on a much larger scale in 1856, and completed the volume on the same reduced scale. It cost me thirteen months and ten days' hard labour. It was published under the title of the 'Origin of Species,' in November 1859. Though considerably added to and corrected in the later editions, it has remained substantially the same book.

It is no doubt the chief work of my life. It was from the first highly successful. The first small edition of 1250 copies was sold on the day of publication, and a second edition of 3000 copies soon afterwards. Sixteen thousand copies have now (1876) been sold in England; and considering how stiff a book it is, this is a large sale. It has been translated into almost every European tongue, even into such languages as Spanish, Bohemian, Polish, and Russian. It has also, according to Miss Bird, been translated into Japanese,* and is there much studied. Even an essay in Hebrew has appeared on it, showing that the theory is contained in the Old Testament! The reviews were very numerous; for some time I collected all that appeared on the 'Origin' and on my related books, and these amount (excluding newspaper reviews) to 265; but after a time I gave up the attempt in despair. Many separate essays and books on the subject have appeared; and in Germany a catalogue or bibliography on "Darwinismus" has appeared every year or two.

* Miss Bird is mistaken, as I learn from Prof. Mitsukuri.—F.D.

© The Complete Work of Charles Darwin Online

The success of the 'Origin' may, I think, be attributed in large part to my having long before written two condensed sketches, and to my having finally abstracted a much larger manuscript, which was itself an abstract. By this means I was enabled to select the more striking facts and conclusions. I had, also, during many years followed a golden rule, namely, that whenever a published fact, a new observation or thought came across me, which was opposed to my general results, to make a memorandum of it without fail and at once; for I had found by experience that such facts and thoughts were far more apt to escape from the memory than favourable ones. Owing to this habit, very few objections were raised against my views which I had not at least noticed and attempted to answer.

It has sometimes been said that the success of the 'Origin' proved "that the subject was in the air," or "that men's minds were prepared for it." I do not think that this is strictly true, for I occasionally sounded not a few naturalists, and never happened to come across a single one who seemed to doubt about the permanence of species. Even Lyell and Hooker, though they would listen with interest to me, never seemed to agree. I tried once or twice to explain to able men what I meant by Natural Selection, but signally failed. What I believe was strictly true is that innumerable well-observed facts were stored in the minds of naturalists ready to take their proper places as soon as any theory which would receive them was sufficiently explained. Another element in the success

© The Complete Work of Charles Darwin Online

of the book was its moderate size; and this I owe to the appearance of Mr. Wallace's essay; had I published on the scale in which I began to write in 1856, the book would have been four or five times as large as the 'Origin,' and very few would have had the patience to read it.

I gained much by my delay in publishing from about 1839, when the theory was clearly conceived, to 1859; and I lost nothing by it, for I cared very little whether men attributed most originality to me or Wallace; and his essay no doubt aided in the reception of the theory. I was forestalled in only one important point, which my vanity has always made me regret, namely, the explanation by means of the Glacial period of the presence of the same species of plants and of some few animals on distant mountain summits and in the arctic regions. This view pleased me so much that I wrote it out in extenso, and I believe that it was read by Hooker some years before E. Forbes published his celebrated memoir* on the subject. In the very few points in which we differed, I still think that I was in the right. I have never, of course, alluded in print to my having independently worked out this view.

Hardly any point gave me so much satisfaction when I was at work on the 'Origin,' as the explanation of the wide difference in many classes between the embryo and the adult animal, and of the close resemblance of the embryos within the same class. No notice of this point was taken, as far as I re-

* 'Geolog. Survey Mem.,' 1846.

© The Complete Work of Charles Darwin Online

member, in the early reviews of the 'Origin,' and I recollect expressing my surprise on this head in a letter to Asa Gray. Within late years several reviewers have given the whole credit to Fritz Müller and Häckel, who undoubtedly have worked it out much more fully, and in some respects more correctly than I did. I had materials for a whole chapter on the subject, and I ought to have made the discussion longer; for it is clear that I failed to impress my readers; and he who succeeds in doing so deserves, in my opinion, all the credit.

This leads me to remark that I have almost always been treated honestly by my reviewers, passing over those without scientific knowledge as not worthy of notice. My views have often been grossly misrepresented, bitterly opposed and ridiculed, but this has been generally done as, I believe, in good faith. On the whole I do not doubt that my works have been over and over again greatly overpraised. I rejoice that I have avoided controversies, and this I owe to Lyell, who many years ago, in reference to my geological works, strongly advised me never to get entangled in a controversy, as it rarely did any good and caused a miserable loss of time and temper.

Whenever I have found out that I have blundered, or that my work has been imperfect, and when I have been contemptuously criticised, and even when I have been overpraised, so that I have felt mortified, it has been my greatest comfort to say hundreds of times to myself that "I have worked as hard and as well as I could, and no man can do more than this." I

© The Complete Work of Charles Darwin Online

remember when in Good Success Bay, in Tierra del Fuego, thinking (and, I believe, that I wrote home to the effect) that I could not employ my life better than in adding a little to Natural Science. This I have done to the best of my abilities, and critics may say what they like, but they cannot destroy this conviction.

During the two last months of 1859 I was fully occupied in preparing a second edition of the 'Origin,' and by an enormous correspondence. On January 1st, 1860, I began arranging my notes for my work on the 'Variation of Animals and Plants under Domestication;' but it was not published until the beginning of 1868; the delay having been caused partly by frequent illnesses, one of which lasted seven months, and partly by being tempted to publish on other subjects which at the time interested me more.

On May 15th, 1862, my little book on the 'Fertilisation of Orchids,' which cost me ten months' work, was published: most of the facts had been slowly accumulated during several previous years. During the summer of 1839, and, I believe, during the previous summer, I was led to attend to the cross-fertilisation of flowers by the aid of insects, from having come to the conclusion in my speculations on the origin of species, that crossing played an important part in keeping specific forms constant. I attended to the subject more or less during every subsequent summer; and my interest in it was greatly enhanced by having procured and read in November 1841, through the advice of Robert Brown, a copy of C. K. Sprengel's wonderful book, 'Das entdeckte Geheimniss

© The Complete Work of Charles Darwin Online

der Natur.' For some years before 1862 I had specially attended to the fertilisation of our British orchids; and it seemed to me the best plan to prepare as complete a treatise on this group of plants as well as I could, rather than to utilise the great mass of matter which I had slowly collected with respect to other plants.

My resolve proved a wise one; for since the appearance of my book, a surprising number of papers and separate works on the fertilisation of all kinds of flowers have appeared; and these are far better done than I could possibly have effected. The merits of poor old Sprengel, so long overlooked, are now fully recognised many years after his death.

During the same year I published in the 'Journal of the Linnean Society' a paper "On the Two Forms, or Dimorphic Condition of Primula," and during the next five years, five other papers on dimorphic and trimorphic plants. I do not think anything in my scientific life has given me so much satisfaction as making out the meaning of the structure of these plants. I had noticed in 1838 or 1839 the dimorphism of *Linum flavum*, and had at first thought that it was merely a case of unmeaning variability. But on examining the common species of Primula I found that the two forms were much too regular and constant to be thus viewed. I therefore became almost convinced that the common cowslip and primrose were on the high-road to become diœcious;—that the short pistil in the one form, and the short stamens in the other form were tending towards abortion. The plants

© The Complete Work of Charles Darwin Online

were therefore subjected under this point of view to trial; but as soon as the flowers with short pistils fertilised with pollen from the short stamens, were found to yield more seeds than any other of the four possible unions, the abortion-theory was knocked on the head. After some additional experiment, it became evident that the two forms, though both were perfect hermaphrodites, bore almost the same relation to one another as do the two sexes of an ordinary animal. With Lythrum we have the still more wonderful case of three forms standing in a similar relation to one another. I afterwards found that the offspring from the union of two plants belonging to the same forms presented a close and curious analogy with hybrids from the union of two distinct species.

In the autumn of 1864 I finished a long paper on 'Climbing Plants,' and sent it to the Linnean Society. The writing of this paper cost me four months; but I was so unwell when I received the proof-sheets that I was forced to leave them very badly and often obscurely expressed. The paper was little noticed, but when in 1875 it was corrected and published as a separate book it sold well. I was led to take up this subject by reading a short paper by Asa Gray, published in 1858. He sent me seeds, and on raising some plants I was so much fascinated and perplexed by the revolving movements of the tendrils and stems, which movements are really very simple, though appearing at first sight very complex, that I procured various other kinds of climbing plants, and studied the whole subject. I was all the more attracted to it,

© The Complete Work of Charles Darwin Online

from not being at all satisfied with the explanation which Henslow gave us in his lectures, about twining plants, namely, that they had a natural tendency to grow up in a spire. This explanation proved quite erroneous. Some of the adaptations displayed by Climbing Plants are as beautiful as those of Orchids for ensuring cross-fertilisation.

My 'Variation of Animals and Plants under Domestication' was begun, as already stated, in the beginning of 1860, but was not published until the beginning of 1868. It was a big book, and cost me four years and two months' hard labour. It gives all my observations and an immense number of facts collected from various sources, about our domestic productions. In the second volume the causes and laws of variation, inheritance, &c., are discussed as far as our present state of knowledge permits. Towards the end of the work I give my well-abused hypothesis of Pangenesis. An unverified hypothesis is of little or no value; but if any one should hereafter be led to make observations by which some such hypothesis could be established, I shall have done good service, as an astonishing number of isolated facts can be thus connected together and rendered intelligible. In 1875 a second and largely corrected edition, which cost me a good deal of labour, was brought out.

My 'Descent of Man' was published in February 1871. As soon as I had become, in the year 1837 or 1838, convinced that species were mutable productions, I could not avoid the belief that man must come under the same law. Accordingly I collected notes on the

© The Complete Work of Charles Darwin Online

subject for my own satisfaction, and not for a long time with any intention of publishing. Although in the 'Origin of Species' the derivation of any particular species is never discussed, yet I thought it best, in order that no honourable man should accuse me of concealing my views, to add that by the work "light would be thrown on the origin of man and his history." It would have been useless and injurious to the success of the book to have paraded, without giving any evidence, my conviction with respect to his origin.

But when I found that many naturalists fully accepted the doctrine of the evolution of species, it seemed to me advisable to work up such notes as I possessed, and to publish a special treatise on the origin of man. I was the more glad to do so, as it gave me an opportunity of fully discussing sexual selection—a subject which had always greatly interested me. This subject, and that of the variation of our domestic productions, together with the causes and laws of variation, inheritance, and the intercrossing of plants, are the sole subjects which I have been able to write about in full, so as to use all the materials which I have collected. The 'Descent of Man' took me three years to write, but then as usual some of this time was lost by ill-health, and some was consumed by preparing new editions and other minor works. A second and largely corrected edition of the 'Descent' appeared in 1874.

My book on the 'Expression of the Emotions in Men and Animals' was published in the autumn of 1872. I had intended to give only a chapter on the

© The Complete Work of Charles Darwin Online

subject in the 'Descent of Man,' but as soon as I began to put my notes together, I saw that it would require a separate treatise.

My first child was born on December 27th, 1839, and I at once commenced to make notes on the first dawn of the various expressions which he exhibited, for I felt convinced, even at this early period, that the most complex and fine shades of expression must all have had a gradual and natural origin. During the summer of the following year, 1840, I read Sir C. Bell's admirable work on expression, and this greatly increased the interest which I felt in the subject, though I could not at all agree with his belief that various muscles had been specially created for the sake of expression. From this time forward I occasionally attended to the subject, both with respect to man and our domesticated animals. My book sold largely; 5267 copies having been disposed of on the day of publication.

In the summer of 1860 I was idling and resting near Hartfield, where two species of Drosera abound; and I noticed that numerous insects had been entrapped by the leaves. I carried home some plants, and on giving them insects saw the movements of the tentacles, and this made me think it probable that the insects were caught for some special purpose. Fortunately a crucial test occurred to me, that of placing a large number of leaves in various nitrogenous and non-nitrogenous fluids of equal density; and as soon as I found that the former alone excited energetic movements, it was obvious that here was a fine new field for investigation.

© The Complete Work of Charles Darwin Online

During subsequent years, whenever I had leisure, I pursued my experiments, and my book on 'Insectivorous Plants' was published in July 1875—that is sixteen years after my first observations. The delay in this case, as with all my other books, has been a great advantage to me; for a man after a long interval can criticise his own work, almost as well as if it were that of another person. The fact that a plant should secrete, when properly excited, a fluid containing an acid and ferment, closely analogous to the digestive fluid of an animal, was certainly a remarkable discovery.

During this autumn of 1876 I shall publish on the 'Effects of Cross and Self-Fertilisation in the Vegetable Kingdom.' This book will form a complement to that on the 'Fertilisation of Orchids,' in which I showed how perfect were the means for cross-fertilisation, and here I shall show how important are the results. I was led to make, during eleven years, the numerous experiments recorded in this volume, by a mere accidental observation; and indeed it required the accident to be repeated before my attention was thoroughly aroused to the remarkable fact that seedlings of self-fertilised parentage are inferior, even in the first generation, in height and vigour to seedlings of cross-fertilised parentage. I hope also to republish a revised edition of my book on Orchids, and hereafter my papers on dimorphic and trimorphic plants, together with some additional observations on allied points which I never have had time to arrange. My strength will then probably be exhausted, and I shall be ready to exclaim "Nunc dimittis."

© The Complete Work of Charles Darwin Online

Written May 1st, 1881.—'The Effects of Cross and Self-Fertilisation' was published in the autumn of 1876; and the results there arrived at explain, as I believe, the endless and wonderful contrivances for the transportal of pollen from one plant to another of the same species. I now believe, however, chiefly from the observations of Hermann Müller, that I ought to have insisted more strongly than I did on the many adaptations for self-fertilisation; though I was well aware of many such adaptations. A much enlarged edition of my 'Fertilisation of Orchids' was published in 1877.

In this same year 'The Different Forms of Flowers, &c.,' appeared, and in 1880 a second edition. This book consists chiefly of the several papers on Heterostyled flowers originally published by the Linnean Society, corrected, with much new matter added, together with observations on some other cases in which the same plant bears two kinds of flowers. As before remarked, no little discovery of mine ever gave me so much pleasure as the making out the meaning of heterostyled flowers. The results of crossing such flowers in an illegitimate manner, I believe to be very important, as bearing on the sterility of hybrids; although these results have been noticed by only a few persons.

In 1879, I had a translation of Dr. Ernst Krause's 'Life of Erasmus Darwin' published, and I added a sketch of his character and habits from material in my possession. Many persons have been much interested by this little life, and I am surprised that only 800 or 900 copies were sold.

© The Complete Work of Charles Darwin Online

In 1880 I published, with [my son] Frank's assistance, our 'Power of Movement in Plants.' This was a tough piece of work. The book bears somewhat the same relation to my little book on 'Climbing Plants,' which 'Cross-Fertilisation' did to the 'Fertilisation of Orchids;' for in accordance with the principle of evolution it was impossible to account for climbing plants having been developed in so many widely different groups unless all kinds of plants possess some slight power of movement of an analogous kind. This I proved to be the case; and I was further led to a rather wide generalisation, viz. that the great and important classes of movements, excited by light, the attraction of gravity, &c., are all modified forms of the fundamental movement of circumnutation. It has always pleased me to exalt plants in the scale of organised beings; and I therefore felt an especial pleasure in showing how many and what admirably well adapted movements the tip of a root possesses.

I have now (May 1, 1881) sent to the printers the MS. of a little book on 'The Formation of Vegetable Mould, through the Action of Worms.' This is a subject of but small importance; and I know not whether it will interest any readers,* but it has interested me. It is the completion of a short paper read before the Geological Society more than forty years ago, and has revived old geological thoughts.

I have now mentioned all the books which I have published, and these have been the milestones in my

* Between November 1881 and February 1884, 8500 copies have been sold.

© The Complete Work of Charles Darwin Online

life, so that little remains to be said. I am not conscious of any change in my mind during the last thirty years, excepting in one point presently to be mentioned; nor, indeed, could any change have been expected unless one of general deterioration. But my father lived to his eighty-third year with his mind as lively as ever it was, and all his faculties undimmed; and I hope that I may die before my mind fails to a sensible extent. I think that I have become a little more skilful in guessing right explanations and in devising experimental tests; but this may probably be the result of mere practice, and of a larger store of knowledge. I have as much difficulty as ever in expressing myself clearly and concisely; and this difficulty has caused me a very great loss of time; but it has had the compensating advantage of forcing me to think long and intently about every sentence, and thus I have been led to see errors in reasoning and in my own observations or those of others.

There seems to be a sort of fatality in my mind leading me to put at first my statement or proposition in a wrong or awkward form. Formerly I used to think about my sentences before writing them down; but for several years I have found that it saves time to scribble in a vile hand whole pages as quickly as I possibly can, contracting half the words; and then correct deliberately. Sentences thus scribbled down are often better ones than I could have written deliberately.

Having said thus much about my manner of writing, I will add that with my large books I spend a good

© The Complete Work of Charles Darwin Online

deal of time over the general arrangement of the matter. I first make the rudest outline in two or three pages, and then a larger one in several pages, a few words or one word standing for a whole discussion or series of facts. Each one of these headings is again enlarged and often transferred before I begin to write *in extenso*. As in several of my books facts observed by others have been very extensively used, and as I have always had several quite distinct subjects in hand at the same time, I may mention that I keep from thirty to forty large portfolios, in cabinets with labelled shelves, into which I can at once put a detached reference or memorandum. I have bought many books, and at their ends I make an index of all the facts that concern my work ; or, if the book is not my own, write out a separate abstract, and of such abstracts I have a large drawer full. Before beginning on any subject I look to all the short indexes and make a general and classified index, and by taking the one or more proper portfolios I have all the information collected during my life ready for use.

I have said that in one respect my mind has changed during the last twenty or thirty years. Up to the age of thirty, or beyond it, poetry of many kinds, such as the works of Milton, Gray, Byron, Wordsworth, Coleridge, and Shelley, gave me great pleasure, and even as a schoolboy I took intense delight in Shakespeare, especially in the historical plays. I have also said that formerly pictures gave me considerable, and music very great delight. But now for many years I cannot endure to read a line of poetry : I have tried

© The Complete Work of Charles Darwin Online

lately to read Shakespeare, and found it so intolerably dull that it nauseated me. I have also almost lost my taste for pictures or music. Music generally sets me thinking too energetically on what I have been at work on, instead of giving me pleasure. I retain some taste for fine scenery, but it does not cause me the exquisite delight which it formerly did. On the other hand, novels which are works of the imagination, though not of a very high order, have been for years a wonderful relief and pleasure to me, and I often bless all novelists. A surprising number have been read aloud to me, and I like all if moderately good, and if they do not end unhappily—against which a law ought to be passed. A novel, according to my taste, does not come into the first class unless it contains some person whom one can thoroughly love, and if a pretty woman all the better.

This curious and lamentable loss of the higher æsthetic tastes is all the odder, as books on history, biographies, and travels (independently of any scientific facts which they may contain), and essays on all sorts of subjects interest me as much as ever they did. My mind seems to have become a kind of machine for grinding general laws out of large collections of facts, but why this should have caused the atrophy of that part of the brain alone, on which the higher tastes depend, I cannot conceive. A man with a mind more highly organised or better constituted than mine, would not, I suppose, have thus suffered; and if I had to live my life again, I would have made a rule to read some poetry and listen to some music at

© The Complete Work of Charles Darwin Online

least once every week; for perhaps the parts of my brain now atrophied would thus have been kept active through use. The loss of these tastes is a loss of happiness, and may possibly be injurious to the intellect, and more probably to the moral character, by enfeebling the emotional part of our nature.

My books have sold largely in England, have been translated into many languages, and passed through several editions in foreign countries. I have heard it said that the success of a work abroad is the best test of its enduring value. I doubt whether this is at all trustworthy; but judged by this standard my name ought to last for a few years. Therefore it may be worth while to try to analyse the mental qualities and the conditions on which my success has depended; though I am aware that no man can do this correctly.

I have no great quickness of apprehension or wit which is so remarkable in some clever men, for instance, Huxley. I am therefore a poor critic: a paper or book, when first read, generally excites my admiration, and it is only after considerable reflection that I perceive the weak points. My power to follow a long and purely abstract train of thought is very limited; and therefore I could never have succeeded with metaphysics or mathematics. My memory is extensive, yet hazy: it suffices to make me cautious by vaguely telling me that I have observed or read something opposed to the conclusion which I am drawing, or on the other hand in favour of it; and after a time I can generally recollect where to search for my authority. So poor in one sense is my memory,

© The Complete Work of Charles Darwin Online

that I have never been able to remember for more than a few days a single date or a line of poetry.

Some of my critics have said, "Oh, he is a good observer, but he has no power of reasoning!" I do not think that this can be true, for the 'Origin of Species' is one long argument from the beginning to the end, and it has convinced not a few able men. No one could have written it without having some power of reasoning. I have a fair share of invention, and of common sense or judgment, such as every fairly successful lawyer or doctor must have, but not, I believe, in any higher degree.

On the favourable side of the balance, I think that I am superior to the common run of men in noticing things which easily escape attention, and in observing them carefully. My industry has been nearly as great as it could have been in the observation and collection of facts. What is far more important, my love of natural science has been steady and ardent.

This pure love has, however, been much aided by the ambition to be esteemed by my fellow naturalists. From my early youth I have had the strongest desire to understand or explain whatever I observed,—that is, to group all facts under some general laws. These causes combined have given me the patience to reflect or ponder for any number of years over any unexplained problem. As far as I can judge, I am not apt to follow blindly the lead of other men. I have steadily endeavoured to keep my mind free so as to give up any hypothesis, however much beloved (and I cannot resist forming one on every subject), as soon

© The Complete Work of Charles Darwin Online

as facts are shown to be opposed to it. Indeed, I have had no choice but to act in this manner, for with the exception of the Coral Reefs, I cannot remember a single first-formed hypothesis which had not after a time to be given up or greatly modified. This has naturally led me to distrust greatly deductive reasoning in the mixed sciences. On the other hand, I am not very sceptical,—a frame of mind which I believe to be injurious to the progress of science. A good deal of scepticism in a scientific man is advisable to avoid much loss of time, for I have met with not a few men, who, I feel sure, have often thus been deterred from experiment or observations, which would have proved directly or indirectly serviceable.

In illustration, I will give the oddest case which I have known. A gentleman (who, as I afterwards heard, is a good local botanist) wrote to me from the Eastern counties that the seeds or beans of the common field-bean had this year everywhere grown on the wrong side of the pod. I wrote back, asking for further information, as I did not understand what was meant; but I did not receive any answer for a very long time. I then saw in two newspapers, one published in Kent and the other in Yorkshire, paragraphs stating that it was a most remarkable fact that "the beans this year had all grown on the wrong side." So I thought there must be some foundation for so general a statement. Accordingly, I went to my gardener, an old Kentish man, and asked him whether he had heard anything about it, and he answered, "Oh, no, sir, it must be a mistake, for the beans grow on the

© The Complete Work of Charles Darwin Online

wrong side only on leap-year, and this is not leap-year." I then asked him how they grew in common years and how on leap-years, but soon found that he knew absolutely nothing of how they grew at any time, but he stuck to his belief.

After a time I heard from my first informant, who, with many apologies, said that he should not have written to me had he not heard the statement from several intelligent farmers; but that he had since spoken again to every one of them, and not one knew in the least what he had himself meant. So that here a belief—if indeed a statement with no definite idea attached to it can be called a belief—had spread over almost the whole of England without any vestige of evidence.

I have known in the course of my life only three intentionally falsified statements, and one of these may have been a hoax (and there have been several scientific hoaxes) which, however, took in an American Agricultural Journal. It related to the formation in Holland of a new breed of oxen by the crossing of distinct species of Bos (some of which I happen to know are sterile together), and the author had the impudence to state that he had corresponded with me, and that I had been deeply impressed with the importance of his result. The article was sent to me by the editor of an English Agricultural Journal, asking for my opinion before republishing it.

A second case was an account of several varieties, raised by the author from several species of Primula, which had spontaneously yielded a full complement of

© The Complete Work of Charles Darwin Online

seed, although the parent plants had been carefully protected from the access of insects. This account was published before I had discovered the meaning of heterostylism, and the whole statement must have been fraudulent, or there was neglect in excluding insects so gross as to be scarcely credible.

The third case was more curious: Mr. Huth published in his book on 'Consanguineous Marriage' some long extracts from a Belgian author, who stated that he had interbred rabbits in the closest manner for very many generations, without the least injurious effects. The account was published in a most respectable Journal, that of the Royal Society of Belgium; but I could not avoid feeling doubts—I hardly know why, except that there were no accidents of any kind, and my experience in breeding animals made me think this very improbable.

So with much hesitation I wrote to Professor Van Beneden, asking him whether the author was a trustworthy man. I soon heard in answer that the Society had been greatly shocked by discovering that the whole account was a fraud.* The writer had been publicly challenged in the Journal to say where he had resided and kept his large stock of rabbits while carrying on his experiments, which must have consumed several years, and no answer could be extracted from him.

My habits are methodical, and this has been of not

* The falseness of the published statements on which Mr. Huth relied has been pointed out by himself in a slip inserted in all the copies of his book which then remained unsold.

© The Complete Work of Charles Darwin Online

a little use for my particular line of work. Lastly, I have had ample leisure from not having to earn my own bread. Even ill-health, though it has annihilated several years of my life, has saved me from the distractions of society and amusement.

Therefore my success as a man of science, whatever this may have amounted to, has been determined, as far as I can judge, by complex and diversified mental qualities and conditions. Of these, the most important have been—the love of science—unbounded patience in long reflecting over any subject—industry in observing and collecting facts—and a fair share of invention as well as of common sense. With such moderate abilities as I possess, it is truly surprising that I should have influenced to a considerable extent the belief of scientific men on some important points.

© The Complete Work of Charles Darwin Online

THE STUDY AT DOWN.*

CHAPTER III.

REMINISCENCES OF MY FATHER'S EVERYDAY LIFE.

It is my wish in the present chapter to give some idea of my father's everyday life. It has seemed to me that I might carry out this object in the form of a rough sketch of a day's life at Down, interspersed with such recollections as are called up by the record. Many of these recollections, which have a meaning for those who knew my father, will seem colourless or trifling to strangers. Nevertheless, I give them in the hope that they may help to preserve that impression of his personality which remains on the minds of those who knew

* From the 'Century Magazine,' January 1883.

© The Complete Work of Charles Darwin Online

and loved him—an impression at once so vivid and so untranslatable into words.

Of his personal appearance (in these days of multiplied photographs) it is hardly necessary to say much. He was about six feet in height, but scarcely looked so tall, as he stooped a good deal; in later days he yielded to the stoop; but I can remember seeing him long ago swinging his arms back to open out his chest, and holding himself upright with a jerk. He gave one the idea that he had been active rather than strong; his shoulders were not broad for his height, though certainly not narrow. As a young man he must have had much endurance, for on one of the shore excursions from the *Beagle*, when all were suffering from want of water, he was one of the two who were better able than the rest to struggle on in search of it. As a boy he was active, and could jump a bar placed at the height of the "Adam's apple" in his neck.

He walked with a swinging action, using a stick heavily shod with iron, which he struck loudly against the ground, producing as he went round the "Sand-walk" at Down, a rhythmical click which is with all of us a very distinct remembrance. As he returned from the midday walk, often carrying the waterproof or cloak which had proved too hot, one could see that the swinging step was kept up by something of an effort. Indoors his step was often slow and laboured, and as he went upstairs in the afternoon he might be heard mounting the stairs with a heavy footfall, as if each step were an effort. When interested in his work he moved about quickly and easily enough, and often in the middle of dictating he went eagerly into the hall to get a pinch of snuff, leaving the study door open, and calling out the last words of his sentence as he went. Indoors he sometimes used an oak stick like a little alpenstock, and this was a sign that he felt giddiness.

In spite of his strength and activity, I think he must always have had a clumsiness of movement. He was naturally awk-

© The Complete Work of Charles Darwin Online

ward with his hands, and was unable to draw at all well.* This he always regretted much, and he frequently urged the paramount necessity of a young naturalist making himself a good draughtsman.

He could dissect well under the simple microscope, but I think it was by dint of his great patience and carefulness. It was characteristic of him that he thought many little bits of skilful dissection something almost superhuman. He used to speak with admiration of the skill with which he saw Newport dissect a humble bee, getting out the nervous system with a few cuts of a fine pair of scissors, held, as my father used to show, with the elbow raised, and in an attitude which certainly would render great steadiness necessary. He used to consider cutting sections a great feat, and in the last year of his life, with wonderful energy, took the pains to learn to cut sections of roots and leaves. His hand was not steady enough to hold the object to be cut, and he employed a common microtome, in which the pith for holding the object was clamped, and the razor slid on a glass surface in making the sections. He used to laugh at himself, and at his own skill in section-cutting, at which he would say he was "speechless with admiration." On the other hand, he must have had accuracy of eye and power of co-ordinating his movements, since he was a good shot with a gun as a young man, and as a boy was skilful in throwing. He once killed a hare sitting in the flower-garden at Shrewsbury by throwing a marble at it, and, as a man, he once killed a cross-beak with a stone. He was so unhappy at having uselessly killed the cross-beak that he did not mention it for years, and then explained that he should never have thrown at it if he had not felt sure that his old skill had gone from him.

When walking he had a fidgeting movement with his

* The figure representing the aggregated cell-contents in 'Insectivorous Plants' was drawn by him.

© The Complete Work of Charles Darwin Online

fingers, which he has described in one of his books as the habit of an old man. When he sat still he often took hold of one wrist with the other hand; he sat with his legs crossed, and from being so thin they could be crossed very far, as may be seen in one of the photographs. He had his chair in the study and in the drawing-room raised so as to be much higher than ordinary chairs; this was done because sitting on a low or even an ordinary chair caused him some discomfort. We used to laugh at him for making his tall drawing-room chair still higher by putting footstools on it, and then neutralising the result by resting his feet on another chair.

His beard was full and almost untrimmed, the hair being grey and white, fine rather than coarse, and wavy or frizzled. His moustache was somewhat disfigured by being cut short and square across. He became very bald, having only a fringe of dark hair behind.

His face was ruddy in colour, and this perhaps made people think him less of an invalid than he was. He wrote to Dr. Hooker (June 13, 1849), "Every one tells me that I look quite blooming and beautiful; and most think I am shamming, but you have never been one of those." And it must be remembered that at this time he was miserably ill, far worse than in later years. His eyes were bluish grey under deep overhanging brows, with thick bushy projecting eyebrows. His high forehead was much wrinkled, but otherwise his face was not much marked or lined. His expression showed no signs of the continual discomfort he suffered.

When he was excited with pleasant talk his whole manner was wonderfully bright and animated, and his face shared to the full in the general animation. His laugh was a free and sounding peal, like that of a man who gives himself sympathetically and with enjoyment to the person and the thing which have amused him. He often used some sort of gesture with his laugh, lifting up his hands or bringing one down with

© The Complete Work of Charles Darwin Online

a slap. I think, generally speaking, he was given to gesture, and often used his hands in explaining anything (*e.g.* the fertilisation of a flower) in a way that seemed rather an aid to himself than to the listener. He did this on occasions when most people would illustrate their explanations by means of a rough pencil sketch.

He wore dark clothes, of a loose and easy fit. Of late years he gave up the tall hat even in London, and wore a soft black one in winter, and a big straw hat in summer. His usual out-of-doors dress was the short cloak in which Elliot and Fry's photograph represents him leaning against the pillar of the verandah. Two peculiarities of his indoor dress were that he almost always wore a shawl over his shoulders, and that he had great loose cloth boots lined with fur which he could slip on over his indoor shoes. Like most delicate people he suffered from heat as well as from chilliness; it was as if he could not hit the balance between too hot and too cold; often a mental cause would make him too hot, so that he would take off his coat if anything went wrong in the course of his work.

He rose early, chiefly because he could not lie in bed, and I think he would have liked to get up earlier than he did. He took a short turn before breakfast, a habit which began when he went for the first time to a water-cure establishment. This habit he kept up till almost the end of his life. I used, as a little boy, to like going out with him, and I have a vague sense of the red of the winter sunrise, and a recollection of the pleasant companionship, and a certain honour and glory in it. He used to delight me as a boy by telling me how, in still earlier walks, on dark winter mornings, he had once or twice met foxes trotting home at the dawning.

After breakfasting alone about 7.45, he went to work at once, considering the 1½ hour between 8 and 9.30 one of his best working times. At 9.30 he came into the drawing-room for his letters—rejoicing if the post was a light one and being

© The Complete Work of Charles Darwin Online

sometimes much worried if it was not. He would then hear any family letters read aloud as he lay on the sofa.

The reading aloud, which also included part of a novel, lasted till about half-past ten, when he went back to work till twelve or a quarter past. By this time he considered his day's work over, and would often say, in a satisfied voice, "*I've* done a good day's work." He then went out of doors whether it was wet or fine; Polly, his white terrier, went with him in fair weather, but in rain she refused or might be seen hesitating in the verandah, with a mixed expression of disgust and shame at her own want of courage; generally, however, her conscience carried the day, and as soon as he was evidently gone she could not bear to stay behind.

My father was always fond of dogs, and as a young man had the power of stealing away the affections of his sisters' pets; at Cambridge, he won the love of his cousin W. D. Fox's dog, and this may perhaps have been the little beast which used to creep down inside his bed and sleep at the foot every night. My father had a surly dog, who was devoted to him, but unfriendly to every one else, and when he came back from the *Beagle* voyage, the dog remembered him, but in a curious way, which my father was fond of telling. He went into the yard and shouted in his old manner; the dog rushed out and set off with him on his walk, showing no more emotion or excitement than if the same thing had happened the day before, instead of five years ago. This story is made use of in the 'Descent of Man,' 2nd Edit. p. 74.

In my memory there were only two dogs which had much connection with my father. One was a large black and white half-bred retriever, called Bob, to which we, as children, were much devoted. He was the dog of whom the story of the "hot-house face" is told in the 'Expression of the Emotions.'

But the dog most closely associated with my father was the above-mentioned Polly, a rough, white fox-terrier. She was

© The Complete Work of Charles Darwin Online

a sharp-witted, affectionate dog; when her master was going away on a journey, she always discovered the fact by the signs of packing going on in the study, and became low-spirited accordingly. She began, too, to be excited by seeing the study prepared for his return home. She was a cunning little creature, and used to tremble or put on an air of misery when my father passed, while she was waiting for dinner, just as if she knew that he would say (as he did often say) that "she was famishing." My father used to make her catch biscuits off her nose, and had an affectionate and mock-solemn way of explaining to her before-hand that she must "be a very good girl." She had a mark on her back where she had been burnt, and where the hair had re-grown red instead of white, and my father used to commend her for this tuft of hair as being in accordance with his theory of pangenesis; her father had been a red bull-terrier, thus the red hair appearing after the burn showed the presence of latent red gemmules. He was delightfully tender to Polly, and never showed any impatience at the attentions she required, such as to be let in at the door, or out at the verandah window, to bark at "naughty people," a self-imposed duty she much enjoyed. She died, or rather had to be killed, a few days after his death.*

My father's midday walk generally began by a call at the greenhouse, where he looked at any germinating seeds or experimental plants which required a casual examination, but he hardly ever did any serious observing at this time. Then he went on for his constitutional—either round the "Sand-walk," or outside his own grounds in the immediate neighbourhood of the house. The "Sand-walk" was a narrow strip of land 1½ acres in extent, with a gravel-walk round it. On one side of it was a broad old shaw with fair-sized

* The basket in which she usually lay curled up near the fire in his study is faithfully represented in Mr. Parson's drawing given at the head of the chapter.

© The Complete Work of Charles Darwin Online

oaks in it, which made a sheltered shady walk; the other side was separated from a neighbouring grass field by a low quickset hedge, over which you could look at what view there was, a quiet little valley losing itself in the upland country towards the edge of the Westerham hill, with hazel coppice and larch wood, the remnants of what was once a large wood, stretching away to the Westerham road. I have heard my father say that the charm of this simple little valley helped to make him settle at Down.

The Sand-walk was planted by my father with a variety of trees, such as hazel, alder, lime, hornbeam, birch, privet, and dogwood, and with a long line of hollies all down the exposed side. In earlier times he took a certain number of turns every day, and used to count them by means of a heap of flints, one of which he kicked out on the path each time he passed. Of late years I think he did not keep to any fixed number of turns, but took as many as he felt strength for. The Sand-walk was our play-ground as children, and here we continually saw my father as he walked round. He liked to see what we were doing, and was ever ready to sympathize in any fun that was going on. It is curious to think how, with regard to the Sand-walk in connection with my father, my earliest recollections coincide with my latest; it shows how unvarying his habits have been.

Sometimes when alone he stood still or walked stealthily to observe birds or beasts. It was on one of these occasions that some young squirrels ran up his back and legs, while their mother barked at them in an agony from the tree. He always found birds' nests even up to the last years of his life, and we, as children, considered that he had a special genius in this direction. In his quiet prowls he came across the less common birds, but I fancy he used to conceal it from me, as a little boy, because he observed the agony of mind which I endured at not having seen the siskin or goldfinch, or whatever it might have been. He used to tell us how, when

© The Complete Work of Charles Darwin Online

he was creeping noiselessly along in the "Big-Woods," he came upon a fox asleep in the daytime, which was so much astonished that it took a good stare at him before it ran off. A Spitz dog which accompanied him showed no sign of excitement at the fox, and he used to end the story by wondering how the dog could have been so faint-hearted.

Another favourite place was "Orchis Bank," above the quiet Cudham valley, where fly- and musk-orchis grew among the junipers, and Cephalanthera and Neottia under the beech boughs; the little wood "Hangrove," just above this, he was also fond of, and here I remember his collecting grasses, when he took a fancy to make out the names of all the common kinds. He was fond of quoting the saying of one of his little boys, who, having found a grass that his father had not seen before, had it laid by his own plate during dinner, remarking, "I are an extraordinary grass-finder!"

My father much enjoyed wandering slowly in the garden with my mother or some of his children, or making one of a party, sitting out on a bench on the lawn; he generally sat, however, on the grass, and I remember him often lying under one of the big lime-trees, with his head on the green mound at its foot. In dry summer weather, when we often sat out, the big fly-wheel of the well was commonly heard spinning round, and so the sound became associated with those pleasant days. He used to like to watch us playing at lawn-tennis, and often knocked up a stray ball for us with the curved handle of his stick.

Though he took no personal share in the management of the garden, he had great delight in the beauty of flowers—for instance, in the mass of Azaleas which generally stood in the drawing-room. I think he sometimes fused together his admiration of the structure of a flower and of its intrinsic beauty; for instance, in the case of the big pendulous pink and white flowers of Dielytra. In the same way he had an affection, half-artistic, half-botanical, for the little blue Lobelia. In admiring

© The Complete Work of Charles Darwin Online

flowers, he would often laugh at the dingy high-art colours, and contrast them with the bright tints of nature. I used to like to hear him admire the beauty of a flower; it was a kind of gratitude to the flower itself, and a personal love for its delicate form and colour. I seem to remember him gently touching a flower he delighted in; it was the same simple admiration that a child might have.

He could not help personifying natural things. This feeling came out in abuse as well as in praise—*e.g.* of some seedlings —"The little beggars are doing just what I don't want them to." He would speak in a half-provoked, half-admiring way of the ingenuity of a Mimosa leaf in screwing itself out of a basin of water in which he had tried to fix it. One might see the same spirit in his way of speaking of Sundew, earth-worms, &c.*

Within my memory, his only outdoor recreation, besides walking, was riding, which he took to on the recommendation of Dr. Bence Jones, and we had the luck to find for him the easiest and quietest cob in the world, named "Tommy." He enjoyed these rides extremely, and devised a number of short rounds which brought him home in time for lunch. Our country is good for this purpose, owing to the number of small valleys which give a variety to what in a flat country would be a dull loop of road. He was not, I think, naturally fond of horses, nor had he a high opinion of their intelligence, and Tommy was often laughed at for the alarm he showed at passing and repassing the same heap of hedge-clippings as he went round the field. I think he used to feel surprised at himself, when he remembered how bold a rider he had been, and how utterly old age and bad health had taken away his nerve. He would say that riding prevented him thinking

* Cf. Leslie Stephen's 'Swift,' 1882, p. 200, where Swift's inspection of the manners and customs of servants are compared to my father's observations on worms, "The difference is," says Mr. Stephen, "that Darwin had none but kindly feelings for worms."

© The Complete Work of Charles Darwin Online

much more effectually than walking—that having to attend to the horse gave him occupation sufficient to prevent any really hard thinking. And the change of scene which it gave him was good for spirits and health.

Unluckily, Tommy one day fell heavily with him on Keston common. This, and an accident with another horse upset his nerves, and he was advised to give up riding.

If I go beyond my own experience, and recall what I have heard him say of his love for sport, &c., I can think of a good deal, but much of it would be a repetition of what is contained in his 'Recollections.' At school he was fond of bat-fives, and this was the only game at which he was skilful. He was fond of his gun as quite a boy, and became a good shot; he used to tell how in South America he killed twenty-three snipe in twenty-four shots. In telling the story he was careful to add that he thought they were not quite so wild as English snipe.

Luncheon at Down came after his midday walk; and here I may say a word or two about his meals generally. He had a boy-like love of sweets, unluckily for himself, since he was constantly forbidden to take them. He was not particularly successful in keeping the "vows," as he called them, which he made against eating sweets, and never considered them binding unless he made them aloud.

He drank very little wine, but enjoyed, and was revived by, the little he did drink. He had a horror of drinking, and constantly warned his boys that any one might be led into drinking too much. I remember, in my innocence as a small boy, asking him if he had been ever tipsy; and he answered very gravely that he was ashamed to say he had once drunk too much at Cambridge. I was much impressed, so that I know now the place where the question was asked.

After his lunch, he read the newspaper, lying on the sofa in the drawing-room. I think the paper was the only non-scientific matter which he read to himself. Everything else,

© The Complete Work of Charles Darwin Online

novels, travels, history, was read aloud to him. He took so wide an interest in life, that there was much to occupy him in newspapers, though he laughed at the wordiness of the debates; reading them, I think, only in abstract. His interest in politics was considerable, but his opinion on these matters was formed rather by the way than with any serious amount of thought.

After he had read his paper, came his time for writing letters. These, as well as the MS. of his books, were written by him as he sat in a huge horse-hair chair by the fire, his paper supported on a board resting on the arms of the chair. When he had many or long letters to write, he would dictate them from a rough copy; these rough copies were written on the backs of manuscript or of proof-sheets, and were almost illegible, sometimes even to himself. He made a rule of keeping *all* letters that he received; this was a habit which he learnt from his father, and which he said had been of great use to him.

He received many letters from foolish, unscrupulous people, and all of these received replies. He used to say that if he did not answer them, he had it on his conscience afterwards, and no doubt it was in great measure the courtesy with which he answered every one, which produced the universal and wide-spread sense of his kindness of nature, which was so evident on his death.

He was considerate to his correspondents in other and lesser things, for instance when dictating a letter to a foreigner he hardly ever failed to say to me, "You'd better try and write well, as it's to a foreigner." His letters were generally written on the assumption that they would be carelessly read; thus, when he was dictating, he was careful to tell me to make an important clause begin with an obvious paragraph "to catch his eye," as he often said. How much he thought of the trouble he gave others by asking questions, will be well enough shown by his letters. It is difficult to say anything about the general

© The Complete Work of Charles Darwin Online

tone of his letters, they will speak for themselves. The unvarying courtesy of them is very striking. I had a proof of this quality in the feeling with which Mr. Hacon, his solicitor, regarded him. He had never seen my father, yet had a sincere feeling of friendship for him, and spoke especially of his letters as being such as a man seldom receives in the way of business:—"Everything I did was right, and everything was profusely thanked for."

He had a printed form to be used in replying to troublesome correspondents, but he hardly ever used it; I suppose he never found an occasion that seemed exactly suitable. I remember an occasion on which it might have been used with advantage. He received a letter from a stranger stating that the writer had undertaken to uphold Evolution at a debating society, and that being a busy young man, without time for reading, he wished to have a sketch of my father's views. Even this wonderful young man got a civil answer, though I think he did not get much material for his speech. His rule was to thank the donors of books, but not of pamphlets. He sometimes expressed surprise that so few people thanked him for his books which he gave away liberally; the letters that he did receive gave him much pleasure, because he habitually formed so humble an estimate of the value of all his works, that he was genuinely surprised at the interest which they excited.

In money and business matters he was remarkably careful and exact. He kept accounts with great care, classifying them, and balancing at the end of the year like a merchant. I remember the quick way in which he would reach out for his account-book to enter each cheque paid, as though he were in a hurry to get it entered before he had forgotten it. His father must have allowed him to believe that he would be poorer than he really was, for some of the difficulty experienced in finding a house in the country must have arisen from the modest sum he felt prepared to give. Yet he knew,

© The Complete Work of Charles Darwin Online

of course, that he would be in easy circumstances, for in his 'Recollections' he mentions this as one of the reasons for his not having worked at medicine with so much zeal as he would have done if he had been obliged to gain his living.

He had a pet economy in paper, but it was rather a hobby than a real economy. All the blank sheets of letters received were kept in a portfolio to be used in making notes; it was his respect for paper that made him write so much on the backs of his old MS., and in this way, unfortunately, he destroyed large parts of the original MS. of his books. His feeling about paper extended to waste paper, and he objected, half in fun, to the careless custom of throwing a spill into the fire after it had been used for lighting a candle.

My father was wonderfully liberal and generous to all his children in the matter of money, and I have special cause to remember his kindness when I think of the way in which he paid some Cambridge debts of mine—making it almost seem a virtue in me to have told him of them. In his later years he had the kind and generous plan of dividing his surplus at the year's end among his children.

He had a great respect for pure business capacity, and often spoke with admiration of a relative who had doubled his fortune. And of himself would often say in fun that what he really *was* proud of was the money he had saved. He also felt satisfaction in the money he made by his books. His anxiety to save came in great measure from his fears that his children would not have health enough to earn their own livings, a foreboding which fairly haunted him for many years. And I have a dim recollection of his saying, "Thank God, you'll have bread and cheese," when I was so young that I was rather inclined to take it literally.

When letters were finished, about three in the afternoon, he rested in his bedroom, lying on the sofa and smoking a cigarette, and listening to a novel or other book not scientific. He only smoked when resting, whereas snuff

© The Complete Work of Charles Darwin Online

was a stimulant, and was taken during working hours. He took snuff for many years of his life, having learnt the habit at Edinburgh as a student. He had a nice silver snuff-box given him by Mrs. Wedgwood of Maer, which he valued much —but he rarely carried it, because it tempted him to take too many pinches. In one of his early letters he speaks of having given up snuff for a month, and describes himself as feeling "most lethargic, stupid and melancholy." Our former neighbour and clergyman, Mr. Brodie Innes, tells me that at one time my father made a resolve not to take snuff except away from home, "a most satisfactory arrangement for me," he adds, "as I kept a box in my study to which there was access from the garden without summoning servants, and I had more frequently, than might have been otherwise the case, the privilege of a few minutes' conversation with my dear friend." He generally took snuff from a jar on the hall table, because having to go this distance for a pinch was a slight check; the clink of the lid of the snuff jar was a very familiar sound. Sometimes when he was in the drawing-room, it would occur to him that the study fire must be burning low, and when some of us offered to see after it, it would turn out that he also wished to get a pinch of snuff.

Smoking he only took to permanently of late years, though on his Pampas rides he learned to smoke with the Gauchos, and I have heard him speak of the great comfort of a cup of *maté* and a cigarette when he halted after a long ride and was unable to get food for some time.

The reading aloud often sent him to sleep, and he used to regret losing parts of a novel, for my mother went steadily on lest the cessation of the sound might wake him. He came down at four o'clock to dress for his walk, and he was so regular that one might be quite certain it was within a few minutes of four when his descending steps were heard.

From about half-past four to half-past five he worked; then he came to the drawing-room, and was idle till it was time

© The Complete Work of Charles Darwin Online

(about six) to go up for another rest with novel-reading and a cigarette.

Latterly he gave up late dinner, and had a simple tea at half-past seven (while we had dinner), with an egg or a small piece of meat. After dinner he never stayed in the room, and used to apologise by saying he was an old woman, who must be allowed to leave with the ladies. This was one of the many signs and results of his constant weakness and ill-health. Half an hour more or less conversation would make to him the difference of a sleepless night, and of the loss perhaps of half the next day's work.

After dinner he played backgammon with my mother, two games being played every night; for many years a score of the games which each won was kept, and in this score he took the greatest interest. He became extremely animated over these games, bitterly lamenting his bad luck and exploding with exaggerated mock-anger at my mother's good fortune.

After backgammon he read some scientific book to himself, either in the drawing-room, or, if much talking was going on, in the study.

In the evening, that is, after he had read as much as his strength would allow, and before the reading aloud began, he would often lie on the sofa and listen to my mother playing the piano. He had not a good ear, yet in spite of this he had a true love of fine music. He used to lament that his enjoyment of music had become dulled with age, yet within my recollection his love of a good tune was strong. I never heard him hum more than one tune, the Welsh song "Ar hyd y nos," which he went through correctly; he used also, I believe, to hum a little Otaheitan song. From his want of ear he was unable to recognize a tune when he heard it again, but he remained constant to what he liked, and would often say, when an old favourite was played, "That's a fine thing; what is it?" He liked especially parts of Beethoven's symphonies, and bits of Handel. He made a little list of all the pieces

© The Complete Work of Charles Darwin Online

which he especially liked among those which my mother played—giving in a few words the impression that each one made on him—but these notes are unfortunately lost. He was sensitive to differences in style, and enjoyed the late Mrs. Vernon Lushington's playing intensely, and in June 1881, when Hans Richter paid a visit at Down, he was roused to strong enthusiasm by his magnificent performance on the piano. He much enjoyed good singing, and was moved almost to tears by grand or pathetic songs. His niece Lady Farrer's singing of Sullivan's "Will he come" was a never-failing enjoyment to him. He was humble in the extreme about his own taste, and correspondingly pleased when he found that others agreed with him.

He became much tired in the evenings, especially of late years, and left the drawing-room about ten, going to bed at half-past ten. His nights were generally bad, and he often lay awake or sat up in bed for hours, suffering much discomfort. He was troubled at night by the activity of his thoughts, and would become exhausted by his mind working at some problem which he would willingly have dismissed. At night, too, anything which had vexed or troubled him in the day would haunt him, and I think it was then that he suffered if he had not answered some troublesome person's letter.

The regular readings, which I have mentioned, continued for so many years, enabled him to get through a great deal of the lighter kinds of literature. He was extremely fond of novels, and I remember well the way in which he would anticipate the pleasure of having a novel read to him, as he lay down, or lighted his cigarette. He took a vivid interest both in plot and characters, and would on no account know before-hand, how a story finished; he considered looking at the end of a novel as a feminine vice. He could not enjoy any story with a tragical end, for this reason he did not keenly appreciate George Eliot, though he often spoke warmly in praise of 'Silas

© The Complete Work of Charles Darwin Online

Marner.' Walter Scott, Miss Austen, and Mrs. Gaskell, were read and re-read till they could be read no more. He had two or three books in hand at the same time—a novel and perhaps a biography and a book of travels. He did not often read out-of-the-way or old standard books, but generally kept to the books of the day obtained from a circulating library.

I do not think that his literary tastes and opinions were on a level with the rest of his mind. He himself, though he was clear as to what he thought good, considered that in matters of literary taste, he was quite outside the pale, and often spoke of what those within it liked or disliked, as if they formed a class to which he had no claim to belong.

In all matters of art he was inclined to laugh at professed critics, and say that their opinions were formed by fashion. Thus in painting, he would say how in his day every one admired masters who are now neglected. His love ot pictures as a young man is almost a proof that he must have had an appreciation of a portrait as a work of art, not as a likeness. Yet he often talked laughingly of the small worth of portraits, and said that a photograph was worth any number of pictures, as if he were blind to the artistic quality in a painted portrait. But this was generally said in his attempts to persuade us to give up the idea of having his portrait painted, an operation very irksome to him.

This way of looking at himself as an ignoramus in all matters of art, was strengthened by the absence of pretence, which was part of his character. With regard to questions of taste, as well as to more serious things, he always had the courage of his opinions. I remember, however, an instance that sounds like a contradiction to this: when he was looking at the Turners in Mr. Ruskin's bedroom, he did not confess, as he did afterwards, that he could make out absolutely nothing of what Mr. Ruskin saw in them. But this little pretence was not for his own sake, but for the sake of courtesy to his host. He was pleased and amused when subsequently

© The Complete Work of Charles Darwin Online

Mr. Ruskin brought him some photographs of pictures (I think Vandyke portraits), and courteously seemed to value my father's opinion about them.

Much of his scientific reading was in German, and this was a great labour to him; in reading a book after him, I was often struck at seeing, from the pencil-marks made each day where he left off, how little he could read at a time. He used to call German the "Verdammte," pronounced as if in English. He was especially indignant with Germans, because he was convinced that they could write simply if they chose, and often praised Dr. F. Hildebrand for writing German which was as clear as French. He sometimes gave a German sentence to a friend, a patriotic German lady, and used to laugh at her if she did not translate it fluently. He himself learnt German simply by hammering away with a dictionary; he would say that his only way was to read a sentence a great many times over, and at last the meaning occurred to him. When he began German long ago, he boasted of the fact (as he used to tell) to Sir J. Hooker, who replied, "Ah, my dear fellow, that's nothing; I've begun it many times."

In spite of his want of grammar, he managed to get on wonderfully with German, and the sentences that he failed to make out were generally really difficult ones. He never attempted to speak German correctly, but pronounced the words as though they were English; and this made it not a little difficult to help him, when he read out a German sentence and asked for a translation. He certainly had a bad ear for vocal sounds, so that he found it impossible to perceive small differences in pronunciation.

His wide interest in branches of science that were not specially his own was remarkable. In the biological sciences his doctrines make themselves felt so widely that there was something interesting to him in most departments of it. He read a good deal of many quite special works, and large parts of text books, such as Huxley's 'Invertebrate Anatomy,' or

© The Complete Work of Charles Darwin Online

such a book as Balfour's 'Embryology,' where the detail, at any rate, was not specially in his own line. And in the case of elaborate books of the monograph type, though he did not make a study of them, yet he felt the strongest admiration for them.

In the non-biological sciences he felt keen sympathy with work of which he could not really judge. For instance, he used to read nearly the whole of 'Nature,' though so much of it deals with mathematics and physics. I have often heard him say that he got a kind of satisfaction in reading articles which (according to himself) he could not understand. I wish I could reproduce the manner in which he would laugh at himself for it.

It was remarkable, too, how he kept up his interest in subjects at which he had formerly worked. This was strikingly the case with geology. In one of his letters to Mr. Judd he begs him to pay him a visit, saying that since Lyell's death he hardly ever gets a geological talk. His observations, made only a few years before his death, on the upright pebbles in the drift at Southampton, and discussed in a letter to Mr. Geikie, afford another instance. Again, in the letters to Dr. Dohrn, he shows how his interest in barnacles remained alive. I think it was all due to the vitality and persistence of his mind—a quality I have heard him speak of as if he felt that he was strongly gifted in that respect. Not that he used any such phrases as these about himself, but he would say that he had the power of keeping a subject or question more or less before him for a great many years. The extent to which he possessed this power appears when we consider the number of different problems which he solved, and the early period at which some of them began to occupy him.

It was a sure sign that he was not well when he was idle at any times other than his regular resting hours; for, as long as he remained moderately well, there was no break in the regularity of his life. Week-days and Sundays passed by

© The Complete Work of Charles Darwin Online

alike, each with their stated intervals of work and rest. It is almost impossible, except for those who watched his daily life, to realise how essential to his well-being was the regular routine that I have sketched: and with what pain and difficulty anything beyond it was attempted. Any public appearance, even of the most modest kind, was an effort to him. In 1871 he went to the little village church for the wedding of his elder daughter, but he could hardly bear the fatigue of being present through the short service. The same may be said of the few other occasions on which he was present at similar ceremonies.

I remember him many years ago at a christening; a memory which has remained with me, because to us children it seemed an extraordinary and abnormal occurrence. I remember his look most distinctly at his brother Erasmus's funeral, as he stood in the scattering of snow, wrapped in a long black funeral cloak, with a grave look of sad reverie.

When, after an interval of many years, he again attended a meeting of the Linnean Society, it was felt to be, and was in fact, a serious undertaking; one not to be determined on without much sinking of heart, and hardly to be carried into effect without paying a penalty of subsequent suffering. In the same way a breakfast-party at Sir James Paget's, with some of the distinguished visitors to the Medical Congress (1881), was to him a severe exertion.

The early morning was the only time at which he could make any effort of the kind, with comparative impunity. Thus it came about that the visits he paid to his scientific friends in London were by preference made as early as ten in the morning. For the same reason he started on his journeys by the earliest possible train, and used to arrive at the houses of relatives in London when they were beginning their day.

He kept an accurate journal of the days on which he worked and those on which his ill health prevented him from working, so that it would be possible to tell how many were idle days

© The Complete Work of Charles Darwin Online

in any given year. In this journal—a little yellow Letts's Diary, which lay open on his mantel-piece, piled on the diaries of previous years—he also entered the day on which he started for a holiday and that of his return.

The most frequent holidays were visits of a week to London, either to his brother's house (6 Queen Anne Street), or to his daughter's (4 Bryanston Street). He was generally persuaded by my mother to take these short holidays, when it became clear from the frequency of "bad days," or from the swimming of his head, that he was being overworked. He went unwillingly, and tried to drive hard bargains, stipulating, for instance, that he should come home in five days instead of six. Even if he were leaving home for no more than a week, the packing had to be begun early on the previous day, and the chief part of it he would do himself. The discomfort of a journey to him was, at least latterly, chiefly in the anticipation, and in the miserable sinking feeling from which he suffered immediately before the start; even a fairly long journey, such as that to Coniston, tired him wonderfully little, considering how much an invalid he was; and he certainly enjoyed it in an almost boyish way, and to a curious extent.

Although, as he has said, some of his æsthetic tastes had suffered a gradual decay, his love of scenery remained fresh and strong. Every walk at Coniston was a fresh delight, and he was never tired of praising the beauty of the broken hilly country at the head of the lake.

One of the happy memories of this time [1879] is that of a delightful visit to Grasmere: "The perfect day," my sister writes, "and my father's vivid enjoyment and flow of spirits, form a picture in my mind that I like to think of. He could hardly sit still in the carriage for turning round and getting up to admire the view from each fresh point, and even in returning he was full of the beauty of Rydal Water, though

© The Complete Work of Charles Darwin Online

he would not allow that Grasmere at all equalled his beloved Coniston."

Besides these longer holidays, there were shorter visits to various relatives—to his brother-in-law's house, close to Leith Hill, and to his son near Southampton. He always particularly enjoyed rambling over rough open country, such as the commons near Leith Hill and Southampton, the heath-covered wastes of Ashdown Forest, or the delightful "Rough" near the house of his friend Sir Thomas Farrer. He never was quite idle even on these holidays, and found things to observe. At Hartfield he watched Drosera catching insects, &c.; at Torquay he observed the fertilisation of an orchid (*Spiranthes*), and also made out the relations of the sexes in Thyme.

He was always rejoiced to get home after his holidays; he used greatly to enjoy the welcome he got from his dog Polly, who would get wild with excitement, panting, squeaking, rushing round the room, and jumping on and off the chairs; and he used to stoop down, pressing her face to his, letting her lick him, and speaking to her with a peculiarly tender, caressing voice.

My father had the power of giving to these summer holidays a charm which was strongly felt by all his family. The pressure of his work at home kept him at the utmost stretch of his powers of endurance, and when released from it, he entered on a holiday with a youthfulness of enjoyment that made his companionship delightful; we felt that we saw more of him in a week's holiday than in a month at home.

Some of these absences from home, however, had a depressing effect on him; when he had been previously much overworked it seemed as though the absence of the customary strain allowed him to fall into a peculiar condition of miserable health.

Besides the holidays which I have mentioned, there were his

© The Complete Work of Charles Darwin Online

visits to water-cure establishments. In 1849, when very ill, suffering from constant sickness, he was urged by a friend to try the water-cure, and at last agreed to go to Dr. Gully's establishment at Malvern. His letters to Mr. Fox show how much good the treatment did him; he seems to have thought that he had found a cure for his troubles, but, like all other remedies, it had only a transient effect on him. However, he found it, at first, so good for him, that when he came home he built himself a douche-bath, and the butler learnt to be his bathman.

He paid many visits to Moor Park, Dr. Lane's water-cure establishment in Surrey, not far from Aldershot. These visits were pleasant ones, and he always looked back to them with pleasure. Dr. Lane has given his recollections of my father in Dr. Richardson's 'Lecture on Charles Darwin,' October 22, 1882, from which I quote:—

"In a public institution like mine, he was surrounded, of course, by multifarious types of character, by persons of both sexes, mostly very different from himself—commonplace people, in short, as the majority are everywhere, but like to him at least in this, that they were fellow-creatures and fellow-patients. And never was any one more genial, more considerate, more friendly, more altogether charming than he universally was." . . . . He "never aimed, as too often happens with good talkers, at monopolising the conversation. It was his pleasure rather to give and take, and he was as good a listener as a speaker. He never preached nor prosed, but his talk, whether grave or gay (and it was each by turns), was full of life and salt—racy, bright, and animated."

Some idea of his relation to his family and his friends may be gathered from what has gone before; it would be impossible to attempt a complete account of these relationships, but a slightly fuller outline may not be out of place. Of his

K 2

© The Complete Work of Charles Darwin Online.

married life I cannot speak, save in the briefest manner. In his relationship towards my mother, his tender and sympathetic nature was shown in its most beautiful aspect. In her presence he found his happiness, and through her, his life,—which might have been overshadowed by gloom,—became one of content and quiet gladness.

The 'Expression of the Emotions' shows how closely he watched his children; it was characteristic of him that (as I have heard him tell), although he was so anxious to observe accurately the expression of a crying child, his sympathy with the grief spoiled his observation. His note-book, in which are recorded sayings of his young children, shows his pleasure in them. He seemed to retain a sort of regretful memory of the childhoods which had faded away, and thus he wrote in his 'Recollections':—"When you were very young it was my delight to play with you all, and I think with a sigh that such days can never return."

I may quote, as showing the tenderness of his nature, some sentences from an account of his little daughter Annie, written a few days after her death:—

"Our poor child, Annie, was born in Gower Street, on March 2, 1841, and expired at Malvern at mid-day on the 23rd of April, 1851.

"I write these few pages, as I think in after years, if we live, the impressions now put down will recall more vividly her chief characteristics. From whatever point I look back at her, the main feature in her disposition which at once rises before me, is her buoyant joyousness, tempered by two other characteristics, namely, her sensitiveness, which might easily have been overlooked by a stranger, and her strong affection. Her joyousness and animal spirits radiated from her whole countenance, and rendered every movement elastic and full of life and vigour. It was delightful and cheerful to behold her. Her dear face now rises before me, as she used sometimes to

© The Complete Work of Charles Darwin Online

come running downstairs with a stolen pinch of snuff for me her whole form radiant with the pleasure of giving pleasure. Even when playing with her cousins, when her joyousness almost passed into boisterousness, a single glance of my eye, not of displeasure (for I thank God I hardly ever cast one on her), but of want of sympathy, would for some minutes alter her whole countenance.

"The other point in her character, which made her joyousness and spirits so delightful, was her strong affection, which was of a most clinging, fondling nature. When quite a baby, this showed itself in never being easy without touching her mother, when in bed with her; and quite lately she would, when poorly, fondle for any length of time one of her mother's arms. When very unwell, her mother lying down beside her, seemed to soothe her in a manner quite different from what it would have done to any of our other children. So, again, she would at almost any time spend half an hour in arranging my hair, 'making it,' as she called it, 'beautiful,' or in smoothing, the poor dear darling, my collar or cuffs—in short, in fondling me.

"Besides her joyousness thus tempered, she was in her manners remarkably cordial, frank, open, straightforward, natural, and without any shade of reserve. Her whole mind was pure and transparent. One felt one knew her thoroughly and could trust her. I always thought, that come what might, we should have had in our old age, at least one loving soul, which nothing could have changed. All her movements were vigorous, active, and usually graceful. When going round the Sand-walk with me, although I walked fast, yet she often used to go before, pirouetting in the most elegant way, her dear face bright all the time with the sweetest smiles. Occasionally she had a pretty coquettish manner towards me, the memory of which is charming. She often used exaggerated language, and when I quizzed her by exaggerating what she had said, how clearly can

© The Complete Work of Charles Darwin Online

I now see the little toss of the head, and exclamation of, 'Oh, papa, what a shame of you!' In the last short illness, her conduct in simple truth was angelic. She never once complained; never became fretful; was ever considerate of others, and was thankful in the most gentle, pathetic manner for everything done for her. When so exhausted that she could hardly speak, she praised everything that was given her, and said some tea 'was beautifully good.' When I gave her some water, she said, 'I quite thank you;' and these, I believe, were the last precious words ever addressed by her dear lips to me.

"We have lost the joy of the household, and the solace of our old age. She must have known how we loved her. Oh, that she could now know how deeply, how tenderly, we do still and shall ever love her dear joyous face! Blessings on her!

"April 30, 1851."

We his children all took especial pleasure in the games he played at with us, but I do not think he romped much with us; I suppose his health prevented any rough play. He used sometimes to tell us stories, which were considered specially delightful, partly on account of their rarity.

The way he brought us up is shown by a little story about my brother Leonard, which my father was fond of telling. He came into the drawing-room and found Leonard dancing about on the sofa, which was forbidden, for the sake of the springs, and said, "Oh, Lenny, Lenny, that's against all rules," and received for answer, "Then I think you'd better go out of the room." I do not believe he ever spoke an angry word to any of his children in his life; but I am certain that it never entered our heads to disobey him. I well remember one occasion when my father reproved me for a piece of carelessness; and I can still recall the feeling of depression which came over me, and the care which he took to disperse it by speaking to me soon afterwards with especial kindness. He

© The Complete Work of Charles Darwin Online

kept up his delightful, affectionate manner towards us all his life. I sometimes wonder that he could do so, with such an undemonstrative race as we are; but I hope he knew how much we delighted in his loving words and manner. How often, when a man, I have wished when my father was behind my chair, that he would pass his hand over my hair, as he used to do when I was a boy. He allowed his grown-up children to laugh with and at him, and was generally speaking on terms of perfect equality with us.

He was always full of interest about each one's plans or successes. We used to laugh at him, and say he would not believe in his sons, because, for instance, he would be a little doubtful about their taking some bit of work for which he did not feel sure that they had knowledge enough. On the other hand, he was only too much inclined to take a favourable view of our work. When I thought he had set too high a value on anything that I had done, he used to be indignant and inclined to explode in mock anger. His doubts were part of his humility concerning what was in any way connected with himself; his too favourable view of our work was due to his sympathetic nature, which made him lenient to every one.

He kept up towards his children his delightful manner of expressing his thanks; and I never wrote a letter, or read a page aloud to him, without receiving a few kind words of recognition. His love and goodness towards his little grandson Bernard were great; and he often spoke of the pleasure it was to him to see "his little face opposite to him" at luncheon. He and Bernard used to compare their tastes; *e.g.*, in liking brown sugar better than white, &c.; the result being, "We always agree, don't we?"

My sister writes:—

"My first remembrances of my father are of the delights of his playing with us. He was passionately attached to his

© The Complete Work of Charles Darwin Online

own children, although he was not an indiscriminate child-lover. To all of us he was the most delightful play-fellow, and the most perfect sympathiser. Indeed it is impossible adequately to describe how delightful a relation his was to his family, whether as children or in their later life.

"It is a proof of the terms on which we were, and also of how much he was valued as a play-fellow, that one of his sons when about four years old tried to bribe him with sixpence to come and play in working hours. We all knew the sacredness of working time, but that any one should resist sixpence seemed an impossibility.

"He must have been the most patient and delightful of nurses. I remember the haven of peace and comfort it seemed to me when I was unwell, to be tucked up on the study sofa, idly considering the old geological map hung on the wall. This must have been in his working hours, for I always picture him sitting in the horse-hair arm-chair by the corner of the fire.

"Another mark of his unbounded patience was the way in which we were suffered to make raids into the study when we had an absolute need of sticking-plaster, string, pins, scissors, stamps, foot-rule, or hammer. These and other such necessaries were always to be found in the study, and it was the only place where this was a certainty. We used to feel it wrong to go in during work-time; still, when the necessity was great, we did so. I remember his patient look when he said once, 'Don't you think you could not come in again, I have been interrupted very often.' We used to dread going in for sticking-plaster, because he disliked to see that we had cut ourselves, both for our sakes and on account of his acute sensitiveness to the sight of blood. I well remember lurking about the passage till he was safe away, and then stealing in for the plaster.

"Life seems to me, as I look back upon it, to have been very regular in those early days, and except relations (and a few

© The Complete Work of Charles Darwin Online

intimate friends), I do not think any one came to the house. After lessons, we were always free to go where we would, and that was chiefly in the drawing-room and about the garden, so that we were very much with both my father and mother. We used to think it most delightful when he told us any stories about the *Beagle*, or about early Shrewsbury days—little bits about school-life and his boyish tastes. Sometimes too he read aloud to his children such books as Scott's novels, and I remember a few little lectures on the steam-engine.

"I was more or less ill during the five years between my thirteenth and eighteenth years, and for a long time (years it seems to me) he used to play a couple of games of back-gammon with me every afternoon. He played them with the greatest spirit, and I remember we used at one time to keep account of the games, and as this record came out in favour of him, we kept a list of the doublets thrown by each, as I was convinced that he threw better than myself.

"His patience and sympathy were boundless during this weary illness, and sometimes when most miserable I felt his sympathy to be almost too keen. When at my worst, we went to my aunt's house at Hartfield, in Sussex, and as soon as we had made the move safely he went on to Moor Park for a fortnight's water-cure. I can recall now how on his return I could hardly bear to have him in the room, the expression of tender sympathy and emotion in his face was too agitating, coming fresh upon me after his little absence.

"He cared for all our pursuits and interests, and lived our lives with us in a way that very few fathers do. But I am certain that none of us felt that this intimacy interfered the least with our respect or obedience. Whatever he said was absolute truth and law to us. He always put his whole mind into answering any of our questions. One trifling instance makes me feel how he cared for what we cared for. He had no special taste for cats, though he admired the pretty ways of a kitten. But yet he knew and remembered the individu-

© The Complete Work of Charles Darwin Online

alities of my many cats, and would talk about the habits and characters of the more remarkable ones years after they had died.

"Another characteristic of his treatment of his children was his respect for their liberty, and for their personality. Even as quite a girl, I remember rejoicing in this sense of freedom. Our father and mother would not even wish to know what we were doing or thinking unless we wished to tell. He always made us feel that we were each of us creatures whose opinions and thoughts were valuable to him, so that whatever there was best in us came out in the sunshine of his presence.

"I do not think his exaggerated sense of our good qualities, intellectual or moral, made us conceited, as might perhaps have been expected, but rather more humble and grateful to him. The reason being no doubt that the influence of his character, of his sincerity and greatness of nature, had a much deeper and more lasting effect than any small exaltation which his praises or admiration may have caused to our vanity."

As head of a household he was much loved and respected; he always spoke to servants with politeness, using the expression, "would you be so good," in asking for anything. He was hardly ever angry with his servants; it shows how seldom this occurred, that when, as a small boy, I overheard a servant being scolded, and my father speaking angrily, it impressed me as an appalling circumstance, and I remember running up stairs out of a general sense of awe. He did not trouble himself about the management of the garden, cows, &c. He considered the horses so little his concern, that he used to ask doubtfully whether he might have a horse and cart to send to Keston for Drosera, or to the Westerham nurseries for plants, or the like.

As a host my father had a peculiar charm: the presence of visitors excited him, and made him appear to his best advan-

© The Complete Work of Charles Darwin Online

tage. At Shrewsbury, he used to say, it was his father's wish that the guests should be attended to constantly, and in one of the letters to Fox he speaks of the impossibility of writing a letter while the house was full of company. I think he always felt uneasy at not doing more for the entertainment of his guests, but the result was successful; and, to make up for any loss, there was the gain that the guests felt perfectly free to do as they liked. The most usual visitors were those who stayed from Saturday till Monday; those who remained longer were generally relatives, and were considered to be rather more my mother's affair than his.

Besides these visitors, there were foreigners and other strangers, who came down for luncheon and went away in the afternoon. He used conscientiously to represent to them the enormous distance of Down from London, and the labour it would be to come there, unconsciously taking for granted that they would find the journey as toilsome as he did himself. If, however, they were not deterred, he used to arrange their journeys for them, telling them when to come, and practically when to go. It was pleasant to see the way in which he shook hands with a guest who was being welcomed for the first time; his hand used to shoot out in a way that gave one the feeling that it was hastening to meet the guest's hands. With old friends his hand came down with a hearty swing into the other hand in a way I always had satisfaction in seeing. His good-bye was chiefly characterised by the pleasant way in which he thanked his guests, as he stood at the door, for having come to see him.

These luncheons were very successful entertainments, there was no drag or flagging about them, my father was bright and excited throughout the whole visit. Professor De Candolle has described a visit to Down, in his admirable and sympathetic sketch of my father.* He speaks of his manner

* 'Darwin considéré au point de vue des causes de son succès.'—Geneva, 1882.

© The Complete Work of Charles Darwin Online

as resembling that of a "savant" of Oxford or Cambridge. This does not strike me as quite a good comparison; in his ease and naturalness there was more of the manner of some soldiers; a manner arising from total absence of pretence or affectation. It was this absence of pose, and the natural and simple way in which he began talking to his guests, so as to get them on their own lines, which made him so charming a host to a stranger. His happy choice of matter for talk seemed to flow out of his sympathetic nature, and humble, vivid interest in other people's work.

To some, I think, he caused actual pain by his modesty; I have seen the late Francis Balfour quite discomposed by having knowledge ascribed to himself on a point about which my father claimed to be utterly ignorant.

It is difficult to seize on the characteristics of my father's conversation.

He had more dread than have most people of repeating his stories, and continually said, "You must have heard me tell," or "I dare say I've told you." One peculiarity he had, which gave a curious effect to his conversation. The first few words of a sentence would often remind him of some exception to, or some reason against, what he was going to say; and this again brought up some other point, so that the sentence would become a system of parenthesis within parenthesis, and it was often impossible to understand the drift of what he was saying until he came to the end of his sentence. He used to say of himself that he was not quick enough to hold an argument with any one, and I think this was true. Unless it was a subject on which he was just then at work, he could not get the train of argument into working order quickly enough. This is shown even in his letters; thus, in the case of two letters to Prof. Semper about the effect of isolation, he did not recall the series of facts he wanted until some days after the first letter had been sent off.

When puzzled in talking, he had a peculiar stammer on the

© The Complete Work of Charles Darwin Online

first word of a sentence. I only recall this occurring with words beginning with w; possibly he had a special difficulty with this letter, for I have heard him say that as a boy he could not pronounce w, and that sixpence was offered him if he could say "white wine," which he pronounced "rite rine." Possibly he may have inherited this tendency from Erasmus Darwin, who stammered.*

He sometimes combined his metaphors in a curious way, using such a phrase as "holding on like life,"—a mixture of "holding on for his life," and "holding on like grim death." It came from his eager way of putting emphasis into what he was saying. This sometimes gave an air of exaggeration where it was not intended; but it gave, too, a noble air of strong and generous conviction; as, for instance, when he gave his evidence before the Royal Commission on vivisection and came out with his words about cruelty, "It deserves detestation and abhorrence." When he felt strongly about any similar question, he could hardly trust himself to speak, as he then easily became angry, a thing which he disliked excessively. He was conscious that his anger had a tendency to multiply itself in the utterance, and for this reason dreaded (for example) having to scold a servant.

It was a great proof of the modesty of his style of talking, that, when, for instance, a number of visitors came over from Sir John Lubbock's for a Sunday afternoon call, he never seemed to be preaching or lecturing, although he had so much of the talk to himself. He was particularly charming when "chaffing" any one, and in high spirits over it. His manner at such times was light-hearted and boyish, and his refinement of nature came out most strongly. So, when he was talking to a lady who pleased and amused him, the combina-

* My father related a Johnsonian answer of Erasmus Darwin's: "Don't you find it very inconvenient stammering, Dr. Darwin?" "No, sir, because I have time to think before I speak, and don't ask impertinent questions."

© The Complete Work of Charles Darwin Online

tion of raillery and deference in his manner was delightful to see.

When my father had several guests he managed them well, getting a talk with each, or bringing two or three together round his chair. In these conversations there was always a good deal of fun, and, speaking generally, there was either a humorous turn in his talk, or a sunny geniality which served instead. Perhaps my recollection of a pervading element of humour is the more vivid, because the best talks were with Mr. Huxley, in whom there is the aptness which is akin to humour, even when humour itself is not there. My father enjoyed Mr. Huxley's humour exceedingly, and would often say, "What splendid fun Huxley is!" I think he probably had more scientific argument (of the nature of a fight) with Lyell and Sir Joseph Hooker.

He used to say that it grieved him to find that for the friends of his later life he had not the warm affection of his youth. Certainly in his early letters from Cambridge he gives proofs of very strong friendship for Herbert and Fox; but no one except himself would have said that his affection for his friends was not, throughout life, of the warmest possible kind. In serving a friend he would not spare himself, and precious time and strength were willingly given. He undoubtedly had, to an unusual degree, the power of attaching his friends to him. He had many warm friendships, but to Sir Joseph Hooker he was bound by ties of affection stronger than we often see among men. He wrote in his 'Recollections,' "I have known hardly any man more lovable than Hooker."

His relationship to the village people was a pleasant one; he treated them, one and all, with courtesy, when he came in contact with them, and took an interest in all relating to their welfare. Some time after he came to live at Down he helped to found a Friendly Club, and served as treasurer for thirty years. He took much trouble about the club, keep-

© The Complete Work of Charles Darwin Online

ing its accounts with minute and scrupulous exactness, and taking pleasure in its prosperous condition. Every Whit-Monday the club used to march round with band and banner, and paraded on the lawn in front of the house. There he met them, and explained to them their financial position in a little speech seasoned with a few well-worn jokes. He was often unwell enough to make even this little ceremony an exertion, but I think he never failed to meet them.

He was also treasurer of the Coal Club, which gave him some work, and he acted for some years as a County Magistrate.

With regard to my father's interest in the affairs of the village, Mr. Brodie Innes has been so good as to give me his recollections :—

"On my becoming Vicar of Down in 1846, we became friends, and so continued till his death. His conduct towards me and my family was one of unvarying kindness, and we repaid it by warm affection.

"In all parish matters he was an active assistant; in matters connected with the schools, charities, and other business, his liberal contribution was ever ready, and in the differences which at times occurred in that, as in other parishes, I was always sure of his support. He held that where there was really no important objection, his assistance should be given to the clergyman, who ought to know the circumstances best, and was chiefly responsible."

His intercourse with strangers was marked with scrupulous and rather formal politeness, but in fact he had few opportunities of meeting strangers.

Dr. Lane has described * how, on the rare occasion of my father attending a lecture (Dr. Sanderson's) at the Royal Institution, "the whole assembly . . . rose to their feet to welcome him," while he seemed "scarcely conscious that such an outburst of applause could possibly be intended for himself."

* Lecture by Dr. B. W. Richardson, in St. George's Hall, Oct. 22, 1882.

© The Complete Work of Charles Darwin Online

The quiet life he led at Down made him feel confused in a large society; for instance, at the Royal Society's *soirées* he felt oppressed by the numbers. The feeling that he ought to know people, and the difficulty he had in remembering faces in his latter years, also added to his discomfort on such occasions. He did not realise that he would be recognised from his photographs, and I remember his being uneasy at being obviously recognised by a stranger at the Crystal Palace Aquarium.

I must say something of his manner of working: one characteristic of it was his respect for time; he never forgot how precious it was. This was shown, for instance, in the way in which he tried to curtail his holidays; also, and more clearly, with respect to shorter periods. He would often say, that saving the minutes was the way to get work done; he showed this love of saving the minutes in the difference he felt between a quarter of an hour and ten minutes' work; he never wasted a few spare minutes from thinking that it was not worth while to set to work. I was often struck by his way of working up to the very limit of his strength, so that he suddenly stopped in dictating, with the words, "I believe I mustn't do any more." The same eager desire not to lose time was seen in his quick movements when at work. I particularly remember noticing this when he was making an experiment on the roots of beans, which required some care in manipulation; fastening the little bits of card upon the roots was done carefully and necessarily slowly, but the intermediate movements were all quick; taking a fresh bean, seeing that the root was healthy, impaling it on a pin, fixing it on a cork, and seeing that it was vertical, &c.; all these processes were performed with a kind of restrained eagerness. He always gave one the impression of working with pleasure, and not with any drag. I have an image, too, of him as he recorded the result of some experiment, looking eagerly at each root, &c., and then writing with equal eagerness. I

© The Complete Work of Charles Darwin Online

remember the quick movement of his head up and down as he looked from the object to the notes.

He saved a great deal of time through not having to do things twice. Although he would patiently go on repeating experiments where there was any good to be gained, he could not endure having to repeat an experiment which ought, if complete care had been taken, to have succeeded the first time—and this gave him a continual anxiety that the experiment should not be wasted; he felt the experiment to be sacred, however slight a one it was. He wished to learn as much as possible from an experiment, so that he did not confine himself to observing the single point to which the experiment was directed, and his power of seeing a number of other things was wonderful. I do not think he cared for preliminary or rough observations intended to serve as guides and to be repeated. Any experiment done was to be of some use, and in this connection I remember how strongly he urged the necessity of keeping the notes of experiments which failed, and to this rule he always adhered.

In the literary part of his work he had the same horror of losing time, and the same zeal in what he was doing at the moment, and this made him careful not to be obliged unnecessarily to read anything a second time.

His natural tendency was to use simple methods and few instruments. The use of the compound microscope has much increased since his youth, and this at the expense of the simple one. It strikes us nowadays as extraordinary that he should have had no compound microscope when he went his *Beagle* voyage; but in this he followed the advice of Robt. Brown, who was an authority in such matters. He always had a great liking for the simple microscope, and maintained that nowadays it was too much neglected, and that one ought always to see as much as possible with the simple before taking to the compound microscope. In one of his letters he speaks on this point, and remarks that he always

© The Complete Work of Charles Darwin Online

suspects the work of a man who never uses the simple microscope.

His dissecting table was a thick board, let into a window of the study; it was lower than an ordinary table, so that he could not have worked at it standing; but this, from wishing to save his strength, he would not have done in any case. He sat at his dissecting-table on a curious low stool which had belonged to his father, with a seat revolving on a vertical spindle, and mounted on large castors, so that he could turn easily from side to side. His ordinary tools, &c., were lying about on the table, but besides these a number of odds and ends were kept in a round table full of radiating drawers, and turning on a vertical axis, which stood close by his left side, as he sat at his microscope-table. The drawers were labelled, "best tools," "rough tools," "specimens," "preparations for specimens," &c. The most marked peculiarity of the contents of these drawers was the care with which little scraps and almost useless things were preserved; he held the well-known belief, that if you threw a thing away you were sure to want it directly—and so things accumulated.

If any one had looked at his tools, &c., lying on the table, he would have been struck by an air of simpleness, make-shift, and oddness.

At his right hand were shelves, with a number of other odds and ends, glasses, saucers, tin biscuit boxes for germinating seeds, zinc labels, saucers full of sand, &c., &c. Considering how tidy and methodical he was in essential things, it is curious that he bore with so many make-shifts: for instance, instead of having a box made of a desired shape, and stained black inside, he would hunt up something like what he wanted and get it darkened inside with shoe-blacking; he did not care to have glass covers made for tumblers in which he germinated seeds, but used broken bits of irregular shape, with perhaps a narrow angle sticking uselessly out on one side. But so much of his experimenting was of a simple

© The Complete Work of Charles Darwin Online

kind, that he had no need for any elaboration, and I think his habit in this respect was in great measure due to his desire to husband his strength, and not waste it on inessential things.

His way of marking objects may here be mentioned. If he had a number of things to distinguish, such as leaves, flowers, &c., he tied threads of different colours round them. In particular he used this method when he had only two classes of objects to distinguish; thus in the case of crossed and self-fertilised flowers, one set would be marked with black and one with white thread, tied round the stalk of the flower. I remember well the look of two sets of capsules, gathered and waiting to be weighed, counted, &c., with pieces of black and of white thread to distinguish the trays in which they lay. When he had to compare two sets of seedlings, sowed in the same pot, he separated them by a partition of zinc-plate; and the zinc-label, which gave the necessary details about the experiment, was always placed on a certain side, so that it became instinctive with him to know without reading the label which were the "crossed" and which the "self-fertilised."

His love of each particular experiment, and his eager zeal not to lose the fruit of it, came out markedly in these crossing experiments—in the elaborate care he took not to make any confusion in putting capsules into wrong trays, &c., &c. I can recall his appearance as he counted seeds under the simple microscope with an alertness not usually characterising such mechanical work as counting. I think he personified each seed as a small demon trying to elude him by getting into the wrong heap, or jumping away altogether; and this gave to the work the excitement of a game. He had great faith in instruments, and I do not think it naturally occurred to him to doubt the accuracy of a scale or measuring glass, &c. He was astonished when we found that one of his micrometers differed from the other. He did not require any

© The Complete Work of Charles Darwin Online

great accuracy in most of his measurements, and had not good scales; he had an old three-foot rule, which was the common property of the household, and was constantly being borrowed, because it was the only one which was certain to be in its place—unless, indeed, the last borrower had forgotten to put it back. For measuring the height of plants, he had a seven-foot deal rod, graduated by the village carpenter Latterly he took to using paper scales graduated to millimeters. For small objects he used a pair of compasses and an ivory protractor. It was characteristic of him that he took scrupulous pains in making measurements with his somewhat rough scales. A trifling example of his faith in authority is that he took his "inch in terms of millimeters" from an old book, in which it turned out to be inaccurately given. He had a chemical balance which dated from the days when he worked at chemistry with his brother Erasmus. Measurements of capacity were made with an apothecary's measuring glass: I remember well its rough look and bad graduation. With this, too, I remember the great care he took in getting the fluid-line on to the graduation. I do not mean by this account of his instruments that any of his experiments suffered from want of accuracy in measurement, I give them as examples of his simple methods and faith in others—faith at least in instrument-makers, whose whole trade was a mystery to him.

A few of his mental characteristics, bearing especially on his mode of working, occur to me. There was one quality of mind which seemed to be of special and extreme advantage in leading him to make discoveries. It was the power of never letting exceptions pass unnoticed. Everybody notices a fact as an exception when it is striking or frequent, but he had a special instinct for arresting an exception. A point apparently slight and unconnected with his present work is passed over by many a man almost unconsciously with some half-considered explanation, which is in fact no explanation. It

© The Complete Work of Charles Darwin Online

was just these things that he seized on to make a start from. In a certain sense there is nothing special in this procedure, many discoveries being made by means of it. I only mention it because, as I watched him at work, the value of this power to an experimenter was so strongly impressed upon me.

Another quality which was shown in his experimental work, was his power of sticking to a subject; he used almost to apologise for his patience, saying that he could not bear to be beaten, as if this were rather a sign of weakness on his part. He often quoted the saying, "It's dogged as does it;" and I think doggedness expresses his frame of mind almost better than perseverance. Perseverance seems hardly to express his almost fierce desire to force the truth to reveal itself. He often said that it was important that a man should know the right point at which to give up an inquiry. And I think it was his tendency to pass this point that inclined him to apologise for his perseverance, and gave the air of doggedness to his work.

He often said that no one could be a good observer unless he was an active theoriser. This brings me back to what I said about his instinct for arresting exceptions: it was as though he were charged with theorising power ready to flow into any channel on the slightest disturbance, so that no fact, however small, could avoid releasing a stream of theory, and thus the fact became magnified into importance. In this way it naturally happened that many untenable theories occurred to him; but fortunately his richness of imagination was equalled by his power of judging and condemning the thoughts that occurred to him. He was just to his theories, and did not condemn them unheard; and so it happened that he was willing to test what would seem to most people not at all worth testing. These rather wild trials he called "fool's experiments," and enjoyed extremely. As an example I may mention that finding the cotyledons of Biophytum to be highly sensitive to vibrations of the table, he fancied that they

© The Complete Work of Charles Darwin Online

might perceive the vibrations of sound, and therefore made me play my bassoon close to a plant.*

The love of experiment was very strong in him, and I can remember the way he would say, "I shan't be easy till I have tried it," as if an outside force were driving him. He enjoyed experimenting much more than work which only entailed reasoning, and when he was engaged on one of his books which required argument and the marshalling of facts, he felt experimental work to be a rest or holiday. Thus, while working upon the 'Variations of Animals and Plants,' in 1860–61, he made out the fertilisation of Orchids, and thought himself idle for giving so much time to them. It is interesting to think that so important a piece of research should have been undertaken and largely worked out as a pastime in place of more serious work. The letters to Hooker of this period contain expressions such as, "God forgive me for being so idle; I am quite sillily interested in the work." The intense pleasure he took in understanding the adaptations for fertilisation is strongly shown in these letters. He speaks in one of his letters of his intention of working at Drosera as a rest from the 'Descent of Man.' He has described in his 'Recollections' the strong satisfaction he felt in solving the problem of heterostylism. And I have heard him mention that the Geology of South America gave him almost more pleasure than anything else. It was perhaps this delight in work requiring keen observation that made him value praise given to his observing powers almost more than appreciation of his other qualities.

For books he had no respect, but merely considered them as tools to be worked with. Thus he did not bind them, and even when a paper book fell to pieces from use, as happened to Müller's 'Befruchtung,' he preserved it from complete dissolution by putting a metal clip over its back. In the same

* This is not so much an example of superabundant theorising from a small cause, but only of his wish to test the most improbable ideas.

© The Complete Work of Charles Darwin Online

way he would cut a heavy book in half, to make it more convenient to hold. He used to boast that he had made Lyell publish the second edition of one of his books in two volumes, instead of in one, by telling him how he had been obliged to cut it in half. Pamphlets were often treated even more severely than books, for he would tear out, for the sake of saving room, all the pages except the one that interested him. The consequence of all this was, that his library was not ornamental, but was striking from being so evidently a working collection of books.

He was methodical in his manner of reading books and pamphlets bearing on his own work. He had one shelf on which were piled up the books he had not yet read, and another to which they were transferred after having been read, and before being catalogued. He would often groan over his unread books, because there were so many which he knew he should never read. Many a book was at once transferred to the other heap, either marked with a cypher at the end, to show that it contained no marked passages, or inscribed, perhaps, "not read," or "only skimmed." The books accumulated in the "read" heap until the shelves overflowed, and then, with much lamenting, a day was given up to the cataloguing. He disliked this work, and as the necessity of undertaking the work became imperative, would often say, in a voice of despair, "We really must do these books soon."

In each book, as he read it, he marked passages bearing on his work. In reading a book or pamphlet, &c., he made pencil-lines at the side of the page, often adding short remarks, and at the end made a list of the pages marked. When it was to be catalogued and put away, the marked pages were looked at, and so a rough abstract of the book was made. This abstract would perhaps be written under three or four headings on different sheets, the facts being sorted out and added to the previously collected facts in

© The Complete Work of Charles Darwin Online

different subjects. He had other sets of abstracts arranged, not according to subject, but according to periodical. When collecting facts on a large scale, in earlier years, he used to read through, and make abstracts, in this way, of whole series of periodicals.

In some of his early letters he speaks of filling several note-books with facts for his book on species; but it was certainly early that he adopted his plan of using portfolios, as described in the 'Recollections.'* My father and M. de Candolle were mutually pleased to discover that they had adopted the same plan of classifying facts. De Candolle describes the method in his 'Phytologie,' and in his sketch of my father mentions the satisfaction he felt in seeing it in action at Down.

Besides these portfolios, of which there are some dozens full of notes, there are large bundles of MS. marked "used" and put away. He felt the value of his notes, and had a horror of their destruction by fire. I remember, when some alarm of fire had happened, his begging me to be especially careful, adding very earnestly, that the rest of his life would be miserable if his notes and books were to be destroyed.

He shows the same feeling in writing about the loss of a manuscript, the purport of his words being, "I have a copy, or the loss would have killed me." In writing a book he would spend much time and labour in making a skeleton or plan of the whole, and in enlarging and sub-classing each heading, as described in his 'Recollections.' I think this careful arrangement of the plan was not at all essential to the building up of his argument, but for its presentment, and for the arrangement of his facts. In his 'Life of Erasmus Darwin,' as it was first printed in slips, the growth of the book from a skeleton was plainly visible. The arrangement

* The racks in which the portfolios were placed are shown in the illustration at the head of the chapter, in the recess at the right-hand side of the fire-place.

© The Complete Work of Charles Darwin Online

was altered afterwards, because it was too formal and categorical, and seemed to give the character of his grandfather rather by means of a list of qualities than as a complete picture.

It was only within the last few years that he adopted a plan of writing which he was convinced suited him best, and which is described in the 'Recollections'; namely, writing a rough copy straight off without the slightest attention to style. It was characteristic of him that he felt unable to write with sufficient want of care if he used his best paper, and thus it was that he wrote on the backs of old proofs or manuscript. The rough copy was then reconsidered, and a fair copy was made. For this purpose he had foolscap paper ruled at wide intervals, the lines being needed to prevent him writing so closely that correction became difficult. The fair copy was then corrected, and was recopied before being sent to the printers. The copying was done by Mr. E. Norman, who began this work many years ago when village schoolmaster at Down. My father became so used to Mr. Norman's handwriting, that he could not correct manuscript, even when clearly written out by one of his children, until it had been recopied by Mr. Norman. The MS., on returning from Mr. Norman, was once more corrected, and then sent off to the printers. Then came the work of revising and correcting the proofs, which my father found especially wearisome.

It was at this stage that he first seriously considered the style of what he had written. When this was going on he usually started some other piece of work as a relief. The correction of slips consisted in fact of two processes, for the corrections were first written in pencil, and then re-considered and written in ink.

When the book was passing through the "slip" stage he was glad to have corrections and suggestions from others. Thus my mother looked over the proofs of the 'Origin.' In some of the later works my sister, Mrs. Litchfield, did much

© The Complete Work of Charles Darwin Online

of the correction. After my sister's marriage perhaps most of the work fell to my share.

My sister, Mrs. Litchfield, writes :—

"This work was very interesting in itself, and it was inexpressibly exhilarating to work for him. He was always so ready to be convinced that any suggested alteration was an improvement, and so full of gratitude for the trouble taken. I do not think that he ever used to forget to tell me what improvement he thought I had made, and he used almost to excuse himself if he did not agree with any correction. I think I felt the singular modesty and graciousness of his nature through thus working for him in a way I never should otherwise have done.

"He did not write with ease, and was apt to invert his sentences both in writing and speaking, putting the qualifying clause before it was clear what it was to qualify. He corrected a great deal, and was eager to express himself as well as he possibly could."

Perhaps the commonest corrections needed were of obscurities due to the omission of a necessary link in the reasoning, something which he had evidently omitted through familiarity with the subject. Not that there was any fault in the sequence of the thoughts, but that from familiarity with his argument he did not notice when the words failed to reproduce his thought. He also frequently put too much matter into one sentence, so that it had to be cut up into two.

On the whole, I think the pains which my father took over the literary part of the work was very remarkable. He often laughed or grumbled at himself for the difficulty which he found in writing English, saying, for instance, that if a bad arrangement of a sentence was possible, he should be sure to adopt it. He once got much amusement and satisfaction out of the difficulty which one of the family found in writing a short circular. He had the pleasure of correcting and laughing

© The Complete Work of Charles Darwin Online

at obscurities, involved sentences, and other defects, and thus took his revenge for all the criticism he had himself to bear with. He used to quote with astonishment Miss Martineau's advice to young authors, to write straight off and send the MS. to the printer without correction. But in some cases he acted in a somewhat similar manner. When a sentence got hopelessly involved, he would ask himself, "now what *do* you want to say?" and his answer written down, would often disentangle the confusion.

His style has been much praised; on the other hand, at least one good judge has remarked to me that it is not a good style. It is, above all things, direct and clear; and it is characteristic of himself in its simplicity, bordering on naïveté, and in its absence of pretence. He had the strongest disbelief in the common idea that a classical scholar must write good English; indeed, he thought that the contrary was the case. In writing, he sometimes showed the same tendency to strong expressions as he did in conversation. Thus in the 'Origin,' p. 440, there is a description of a larval cirripede, "with six pairs of beautifully constructed natatory legs, a pair of magnificent compound eyes, and extremely complex antennæ." We used to laugh at him for this sentence, which we compared to an advertisement. This tendency to give himself up to the enthusiastic turn of his thought, without fear of being ludicrous, appears elsewhere in his writings.

His courteous and conciliatory tone towards his reader is remarkable, and it must be partly this quality which revealed his personal sweetness of character to so many who had never seen him. I have always felt it to be a curious fact, that he who has altered the face of Biological Science, and is in this respect the chief of the moderns, should have written and worked in so essentially a non-modern spirit and manner. In reading his books one is reminded of the older naturalists rather than of the modern school of writers. He was a Naturalist in the old sense of

© The Complete Work of Charles Darwin Online

the word, that is, a man who works at many branches of science, not merely a specialist in one. Thus it is, that, though he founded whole new divisions of special subjects —such as the fertilisation of flowers, insectivorous plants, dimorphism, &c.—yet even in treating these very subjects he does not strike the reader as a specialist. The reader feels like a friend who is being talked to by a courteous gentleman, not like a pupil being lectured by a professor. The tone of such a book as the 'Origin' is charming, and almost pathetic; it is the tone of a man who, convinced of the truth of his own views, hardly expects to convince others; it is just the reverse of the style of a fanatic, who wants to force people to believe. The reader is never scorned for any amount of doubt which he may be imagined to feel, and his scepticism is treated with patient respect. A sceptical reader, or perhaps even an unreasonable reader, seems to have been generally present to his thoughts. It was in consequence of this feeling, perhaps, that he took much trouble over points which he imagined would strike the reader, or save him trouble, and so tempt him to read.

For the same reason he took much interest in the illustrations of his books, and I think rated rather too highly their value. The illustrations for his earlier books were drawn by professional artists. This was the case in 'Animals and Plants,' the 'Descent of Man,' and the 'Expression of the Emotions.' On the other hand, 'Climbing Plants,' 'Insectivorous Plants,' the 'Movements of Plants,' and 'Forms of Flowers,' were, to a large extent, illustrated by some of his children—my brother George having drawn by far the most. It was delightful to draw for him, as he was enthusiastic in his praise of very moderate performances. I remember well his charming manner of receiving the drawings of one of his daughters-in-law, and how he would finish his words of praise by saying, "Tell A——, Michael Angelo is nothing to it." Though he praised so generously, he always

© The Complete Work of Charles Darwin Online

looked closely at the drawing, and easily detected mistakes or carelessness.

He had a horror of being lengthy, and seems to have been really much annoyed and distressed when he found how the 'Variations of Animals and Plants' was growing under his hands. I remember his cordially agreeing with 'Tristram Shandy's' words, "Let no man say, 'Come, I'll write a duodecimo.'"

His consideration for other authors was as marked a characteristic as his tone towards his reader. He speaks of all other authors as persons deserving of respect. In cases where, as in the case of ——'s experiments on Drosera, he thought lightly of the author, he speaks of him in such a way that no one would suspect it. In other cases he treats the confused writings of ignorant persons as though the fault lay with himself for not appreciating or understanding them. Besides this general tone of respect, he had a pleasant way of expressing his opinion on the value of a quoted work, or his obligation for a piece of private information.

His respectful feeling was not only morally beautiful, but was I think of practical use in making him ready to consider the ideas and observations of all manner of people. He used almost to apologise for this, and would say that he was at first inclined to rate everything too highly.

It was a great merit in his mind that, in spite of having so strong a respectful feeling towards what he read, he had the keenest of instincts as to whether a man was trustworthy or not. He seemed to form a very definite opinion as to the accuracy of the men whose books he read; and made use of this judgment in his choice of facts for use in argument or as illustrations. I gained the impression that he felt this power of judging of a man's trustworthiness to be of much value.

He had a keen feeling of the sense of honour that ought to reign among authors, and had a horror of any kind of laxness

© The Complete Work of Charles Darwin Online

in quoting. He had a contempt for the love of honour and glory, and in his letters often blames himself for the pleasure he took in the success of his books, as though he were departing from his ideal—a love of truth and carelessness about fame. Often, when writing to Sir J. Hooker what he calls a boasting letter, he laughs at himself for his conceit and want of modesty. There is a wonderfully interesting letter which he wrote to my mother bequeathing to her, in case of his death, the care of publishing the manuscript of his first essay on evolution. This letter seems to me full of the intense desire that his theory should succeed as a contribution to knowledge, and apart from any desire for personal fame. He certainly had the healthy desire for success which a man of strong feelings ought to have. But at the time of the publication of the 'Origin' it is evident that he was overwhelmingly satisfied with the adherence of such men as Lyell, Hooker, Huxley, and Asa Gray, and did not dream of or desire any such wide and general fame as he attained to.

Connected with his contempt for the undue love of fame, was an equally strong dislike of all questions of priority. The letters to Lyell, at the time of the 'Origin,' show the anger he felt with himself for not being able to repress a feeling of disappointment at what he thought was Mr. Wallace's forestalling of all his years of work. His sense of literary honour comes out strongly in these letters; and his feeling about priority is again shown in the admiration expressed in his 'Recollections' of Mr. Wallace's self-annihilation.

His feeling about reclamations, including answers to attacks and all kinds of discussions, was strong. It is simply expressed in a letter to Falconer (1863), "If I ever felt angry towards you, for whom I have a sincere friendship, I should begin to suspect that I was a little mad. I was very sorry about your reclamation, as I think it is in every case a mistake and should be left to others. Whether I should so act myself under provocation is a different question." It was a feeling

© The Complete Work of Charles Darwin Online

partly dictated by instinctive delicacy, and partly by a strong sense of the waste of time, energy, and temper thus caused. He said that he owed his determination not to get into discussions * to the advice of Lyell,—advice which he transmitted to those among his friends who were given to paper warfare.

If the character of my father's working life is to be understood, the conditions of ill-health, under which he worked, must be constantly borne in mind. He bore his illness with such uncomplaining patience, that even his children can hardly, I believe, realise the extent of his habitual suffering. In their case the difficulty is heightened by the fact that, from the days of their earliest recollections, they saw him in constant ill-health,—and saw him, in spite of it, full of pleasure in what pleased them. Thus, in later life, their perception of what he endured had to be disentangled from the impression produced in childhood by constant genial kindness under conditions of unrecognised difficulty. No one indeed, except my mother, knows the full amount of suffering he endured, or the full amount of his wonderful patience. For all the latter years of his life she never left him for a night; and her days were so planned that all his resting hours might be shared with her. She shielded him from every avoidable annoyance, and omitted nothing that might save him trouble, or prevent him becoming overtired, or that might alleviate the many discomforts of his ill-health. I hesitate to speak thus freely of a thing so sacred as the life-long devotion which prompted all this constant

* He departed from his rule in his "Note on the Habits of the Pampas Woodpecker, *Colaptes campestris*," 'Proc. Zool. Soc.,' 1870, p. 705: also in a letter published in the 'Athenæum' (1863, p. 554), in which case he afterwards regretted that he had not remained silent. His replies to criticisms, in the later editions of the 'Origin,' can hardly be classed as infractions of his rule.

© The Complete Work of Charles Darwin Online

and tender care. But it is, I repeat, a principal feature of his life, that for nearly forty years he never knew one day of the health of ordinary men, and that thus his life was one long struggle against the weariness and strain of sickness. And this cannot be told without speaking of the one condition which enabled him to bear the strain and fight out the struggle to the end.

© The Complete Work of Charles Darwin Online

LETTERS.

THE earliest letters to which I have access are those written by my father when an undergraduate at Cambridge.

The history of his life, as told in his correspondence, must therefore begin with this period.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

CHAPTER IV.

CAMBRIDGE LIFE.

[My father's Cambridge life comprises the time between the Lent Term, 1828, when he came up as a Freshman, and the end of the May Term, 1831, when he took his degree and left the University.

It appears from the College books, that my father "admissus est pensionarius minor sub Magistro Shaw" on Oct. 15, 1827. He did not come into residence till the Lent Term, 1828, so that, although he passed his examination in due season, he was unable to take his degree at the usual time,—the beginning of the Lent Term, 1831. In such a case a man usually took his degree before Ash-Wednesday, when he was called "Baccalaureus ad Diem Cinerum," and ranked with the B.A.'s of the year. My father's name, however, occurs in the list of Bachelors "ad Baptistam," or those admitted between Ash-Wednesday and St. John Baptist's Day (June 24th); * he therefore took rank among the Bachelors of 1832.

He "kept" for a term or two in lodgings, over Bacon the tobacconist's; not, however, over the shop in the Market Place, now so well known to Cambridge men, but in Sidney Street. For the rest of his time he had pleasant rooms on the south side of the first court of Christ's.†

What determined the choice of this college for his brother

* "On Tuesday last Charles Darwin, of Christ's College, was admitted B.A."—*Cambridge Chronicle*, Friday, April 29, 1831.

† The rooms are on the first floor, on the west side of the middle staircase. A medallion (given by my brother) has recently been let into the wall of the sitting-room.

© The Complete Work of Charles Darwin Online

Erasmus and himself I have no means of knowing. Erasmus the elder, their grandfather, had been at St. John's, and this college might have been reasonably selected for them, being connected with Shrewsbury School. But the life of an undergraduate at St. John's seems, in those days, to have been a troubled one, if I may judge from the fact that a relative of mine migrated thence to Christ's to escape the harassing discipline of the place. A story told by Mr. Herbert * illustrates the same state of things :—

"In the beginning of the October Term of 1830, an incident occurred which was attended with somewhat disagreeable, though ludicrous consequences to myself. Darwin asked me to take a long walk with him in the Fens, to search for some natural objects he was desirous of having. After a very long, fatiguing day's work, we dined together, late in the evening, at his rooms in Christ's College; and as soon as our dinner was over we threw ourselves into easy chairs and fell sound asleep. I was the first to awake, about three in the morning, when, having looked at my watch, and knowing the strict rule of St. John's, which required men *in statu pupillari* to come into college before midnight, I rushed homeward at the utmost speed, in fear of the consequences, but hoping that the Dean would accept the excuse as sufficient when I told him the real facts. He, however, was inexorable, and refused to receive my explanations, or any evidence I could bring; and although during my undergraduateship I had never been reported for coming late into College, now, when I was a hard-working B.A., and had five or six pupils, he sentenced me to confinement to the College walls for the rest of the term. Darwin's indignation knew no bounds, and the stupid injustice and tyranny of the Dean raised not only a perfect ferment among my friends, but was the subject of expostulation from some of the leading members of the University."

My father seems to have found no difficulty in living at

* See footnote, p. 49.

© The Complete Work of Charles Darwin Online

peace with all men in and out of office at Lady Margaret's other foundation. The impression of a contemporary of my father's is that Christ's in their day was a pleasant, fairly quiet college, with some tendency towards "horsiness"; many of the men made a custom of going to Newmarket during the races, though betting was not a regular practice. In this they were by no means discouraged by the Senior Tutor, Mr. Shaw, who was himself generally to be seen on the Heath on these occasions. There was a somewhat high proportion of Fellow-Commoners,—eight or nine, to sixty or seventy Pensioners, and this would indicate that it was not an unpleasant college for men with money to spend and with no great love of strict discipline.

The way in which the service was conducted in chapel shows that the Dean, at least, was not over zealous. I have heard my father tell how at evening chapel the Dean used to read alternate verses of the Psalms, without making even a pretence of waiting for the congregation to take their share. And when the Lesson was a lengthy one, he would rise and go on with the Canticles after the scholar had read fifteen or twenty verses.

It is curious that my father often spoke of his Cambridge life as if it had been so much time wasted, forgetting that, although the set studies of the place were barren enough for him, he yet gained in the highest degree the best advantages of a University life—the contact with men and an opportunity for his mind to grow vigorously. It is true that he valued at its highest the advantages which he gained from associating with Professor Henslow and some others, but he seemed to consider this as a chance outcome of his life at Cambridge, not an advantage for which *Alma Mater* could claim any credit. One of my father's Cambridge friends was the late Mr. J. M. Herbert, County Court Judge for South Wales, from whom I was fortunate enough to obtain some notes which help us to gain an idea of how my father impressed

© The Complete Work of Charles Darwin Online

his contemporaries. Mr. Herbert writes: "I think it was in the spring of 1828 that I first met Darwin, either at my cousin Whitley's rooms in St. John's, or at the rooms of some other of his old Shrewsbury schoolfellows, with many of whom I was on terms of great intimacy. But it certainly was in the summer of that year that our acquaintance ripened into intimacy, when we happened to be together at Barmouth, or the Long Vacation, reading with private tutors,—he with Betterton of St. John's, his Classical and Mathematical Tutor, and I with Yate of St. John's."

The intercourse between them practically ceased in 1831, when my father said good-bye to Herbert at Cambridge, on starting on his *Beagle* voyage. I once met Mr. Herbert, then almost an old man, and I was much struck by the evident warmth and freshness of the affection with which he remembered my father. The notes from which I quote end with this warm-hearted eulogium: "It would be idle for me to speak of his vast intellectual powers . . . but I cannot end this cursory and rambling sketch without testifying, and I doubt not all his surviving college friends would concur with me, that he was the most genial, warm-hearted, generous, and affectionate of friends; that his sympathies were with all that was good and true; and that he had a cordial hatred for everything false, or vile, or cruel, or mean, or dishonourable. He was not only great, but pre-eminently good, and just, and loveable."

Two anecdotes told by Mr. Herbert show that my father's feeling for suffering, whether of man or beast, was as strong in him as a young man as it was in later years: "Before he left Cambridge he told me that he had made up his mind not to shoot any more; that he had had two days' shooting at his friend's, Mr. Owen of Woodhouse; and that on the second day, when going over some of the ground they had beaten on the day before, he picked up a bird not quite dead, but lingering from a shot it had received on the pre-

© The Complete Work of Charles Darwin Online

vious day; and that it had made and left such a painful impression on his mind, that he could not reconcile it to his conscience to continue to derive pleasure from a sport which inflicted such cruel suffering."

To realise the strength of the feeling that led to this resolve, we must remember how passionate was his love of sport. We must recall the boy shooting his first snipe,* and trembling with excitement so that he could hardly reload his gun. Or think of such a sentence as, "Upon my soul, it is only about a fortnight to the 'First,' then if there is a bliss on earth that is it." †

Another anecdote told by Mr. Herbert illustrates again his tenderness of heart:—

"When at Barmouth, he and I went to an exhibition of 'learned dogs.' In the middle of the entertainment one of the dogs failed in performing the trick his master told him to do. On the man reproving him, the dog put on a most piteous expression, as if in fear of the whip. Darwin seeing it, asked me to leave with him, saying, 'Come along, I can't stand this any longer; how those poor dogs must have been licked.'"

It is curious that the same feeling recurred to my father more than fifty years afterwards, on seeing some performing dogs at the Westminster Aquarium; on this occasion he was reassured by the manager telling him that the dogs were taught more by reward than by punishment. Mr. Herbert goes on:—"It stirred one's inmost depth of feeling to hear him descant upon, and groan over, the horrors of the slave trade, or the cruelties to which the suffering Poles were subjected to at Warsaw. . . . These, and other like proofs have left on my mind the conviction that a more humane or tender-hearted man never lived."

* 'Recollections,' p. 34.

† Letter from C. Darwin to W. D. Fox.

© The Complete Work of Charles Darwin Online

His old college friends agree in speaking with affectionate warmth of his pleasant, genial temper as a young man. From what they have been able to tell me, I gain the impression of a young man overflowing with animal spirits—leading a varied healthy life—not over-industrious in the set studies of the place, but full of other pursuits, which were followed with a rejoicing enthusiasm. Entomology, riding, shooting in the fens, suppers and card-playing, music at King's Chapel, engravings at the Fitzwilliam Museum, walks with Professor Henslow—all combined to fill up a happy life. He seems to have infected others with his enthusiasm. Mr. Herbert relates how, during the same Barmouth summer, he was pressed into the service of "the science"—as my father called collecting beetles. They took their daily walks together among the hills behind Barmouth, or boated in the Mawddach estuary, or sailed to Sarn Badrig to land there at low water, or went fly-fishing in the Cors-y-gedol lakes. "On these occasions Darwin entomologised most industriously, picking up creatures as he walked along, and bagging everything which seemed worthy of being pursued, or of further examination. And very soon he armed me with a bottle of alcohol, in which I had to drop any beetle which struck me as not of a common kind. I performed this duty with some diligence in my constitutional walks; but alas! my powers of discrimination seldom enabled me to secure a prize—the usual result, on his examining the contents of my bottle, being an exclamation, 'Well, old Cherbury'* (the nickname he gave me, and by which he usually addressed me), 'none of these will do.'" Again, the Rev. T. Butler, who was one of the Barmouth reading-party in 1828, says: "He inoculated me with a taste for Botany which has stuck by me all my life."

Archdeacon Watkins, another old college friend of my father's, remembers him unearthing beetles in the willows

* No doubt in allusion to the title of Lord Herbert of Cherbury.

© The Complete Work of Charles Darwin Online

between Cambridge and Grantchester, and speaks of a certain beetle the remembrance of whose name is "Crux major." * How enthusiastically must my father have exulted over this beetle to have impressed its name on a companion so that he remembers it after half a century! Archdeacon Watkins goes on: "I do not forget the long and very interesting conversations that we had about Brazilian scenery and tropical vegetation of all sorts. Nor do I forget the way and the vehemence with which he rubbed his chin when he got excited on such subjects, and discoursed eloquently of lianas, orchids, &c."

He became intimate with Henslow, the Professor of Botany, and through him with some other older members of the University. "But," Mr. Herbert writes, "he always kept up the closest connection with the friends of his own standing; and at our frequent social gatherings—at breakfast, wine or supper parties—he was ever one of the most cheerful, the most popular, and the most welcome."

My father formed one of a club for dining once a week, called the Gourmet † Club, the members, besides himself and Mr. Herbert (from whom I quote), being Whitley of St. John's, now Honorary Canon of Durham; ‡ Heaviside of Sidney, now Canon of Norwich; Lovett Cameron of Trinity, now vicar of Shoreham; Blane of Trinity, who held a high post during the Crimean war; H. Lowe § (now Sherbrooke) of Trinity Hall; and Watkins of Emmanuel, now Archdeacon of York. The origin of the club's name seems already to have become involved in obscurity. Mr. Herbert says that it was chosen in derision of another "set of men who called themselves by a long Greek name signifying 'fond of dainties,' but who falsified their claim to such a designation by their weekly practice of dining at some roadside inn, six miles from

* *Panagæus crux-major.*

† Mr. Herbert mentions the name as 'The Glutton Club.'

‡ Formerly Reader in Natural Philosophy at Durham University.

§ Brother of Lord Sherbrooke.

© The Complete Work of Charles Darwin Online

Cambridge, on mutton chops or beans and bacon." Another old member of the club tells me that the name arose because the members were given to making experiments on "birds and beasts which were before unknown to human palate." He says that hawk and bittern were tried, and that their zeal broke down over an old brown owl, "which was indescribable." At any rate, the meetings seemed to have been successful, and to have ended with "a game of mild vingt-et-un."

Mr. Herbert gives an amusing account of the musical examinations described by my father in his 'Recollections.' Mr. Herbert speaks strongly of his love of music, and adds, "What gave him the greatest delight was some grand symphony or overture of Mozart's or Beethoven's, with their full harmonies." On one occasion Herbert remembers "accompanying him to the afternoon service at King's, when we heard a very beautiful anthem. At the end of one of the parts, which was exceedingly impressive, he turned round to me and said, with a deep sigh, 'How's your backbone?'" He often spoke of a feeling of coldness or shivering in his back on hearing beautiful music.

Besides a love of music, he had certainly at this time a love of fine literature; and Mr. Cameron tells me that he used to read Shakespeare to my father in his rooms at Christ's, who took much pleasure in it. He also speaks of his "great liking for first-class line engravings, especially those of Raphael Morghen and Müller; and he spent hours in the Fitzwilliam Museum in looking over the prints in that collection."

My father's letters to Fox show how sorely oppressed he felt by the reading for an examination: "I am reading very hard, and have spirits for nothing. I actually have not stuck a beetle this term." His despair over mathematics must have been profound, when he expressed a hope that Fox's silence is due to "your being ten fathoms deep in the Mathe-

© The Complete Work of Charles Darwin Online

matics; and if you are, God help you, for so am I, only with this difference, I stick fast in the mud at the bottom, and there I shall remain." Mr. Herbert says: "He had, I imagine, no natural turn for mathematics, and he gave up his mathematical reading before he had mastered the first part of algebra, having had a special quarrel with Surds and the Binomial Theorem."

We get some evidence from his letters to Fox of my father's intention of going into the Church. "I am glad," he writes,* "to hear that you are reading divinity. I should like to know what books you are reading, and your opinions about them; you need not be afraid of preaching to me prematurely." Mr. Herbert's sketch shows how doubts arose in my father's mind as to the possibility of his taking Orders. He writes, "We had an earnest conversation about going into Holy Orders; and I remember his asking me, with reference to the question put by the Bishop in the ordination service, 'Do you trust that you are inwardly moved by the Holy Spirit, &c.,' whether I could answer in the affirmative, and on my saying I could not, he said, 'Neither can I, and therefore I cannot take orders.'" This conversation appears to have taken place in 1829, and if so, the doubts here expressed must have been quieted, for in May 1830, he speaks of having some thoughts of reading divinity with Henslow.

The greater number of the following letters are addressed by my father to his cousin, William Darwin Fox. Mr. Fox's relationship to my father is shown in the pedigree given in Chapter I. The degree of kinship appears to have remained a problem to my father, as he signs himself in one letter "$\frac{\text{cousin}}{n^2}$." Their friendship was, in fact, due to their being undergraduates together. My father's letters show clearly enough how genuine the friendship was. In after years, distance, large families, and ill-health on both sides, checked the

* March 18, 1829.

© The Complete Work of Charles Darwin Online

intercourse; but a warm feeling of friendship remained. The correspondence was never quite dropped and continued till Mr. Fox's death in 1880. Mr. Fox took orders, and worked as a country clergyman until forced by ill-health to leave his living in Delamere Forest. His love of natural history remained strong, and he became a skilled fancier of many kinds of birds, &c. The index to 'Animals and Plants,' and my father's later correspondence, show how much help he received from his old College friend.]

C. Darwin to J. M. Herbert.

Saturday Evening
[September 14, 1828].*

MY DEAR OLD CHERBURY,

I am about to fulfil my promise of writing to you, but I am sorry to add there is a very selfish motive at the bottom. I am going to ask you a great favour, and you cannot imagine how much you will oblige me by procuring some more specimens of some insects which I dare say I can describe. In the first place, I must inform you that I have taken some of the rarest of the British Insects, and their being found near Barmouth, is quite unknown to the Entomological world: I think I shall write and inform some of the crack entomologists.

But now for business. *Several* more specimens, if you can procure them without much trouble, of the following insects:—The violet-black coloured beetle, found on Craig Storm, † under stones, also a large smooth black one very like it; a bluish metallic-coloured dung-beetle, which is *very* common on the hill-sides; also, if you *would* be so very kind as to cross the ferry, and you will find a great number under

* The postmark being Derby seems to show that the letter was written from his cousin, W. D. Fox's house, Osmaston, near Derby.

† The top of the hill immediately behind Barmouth was called Craig-Storm, a hybrid Cambro-English word.

© The Complete Work of Charles Darwin Online

the stones on the waste land of a long, smooth, jet-black beetle (a great many of these); also, in the same situation, a very small pinkish insect, with black spots, with a curved thorax projecting beyond the head; also, upon the marshy land over the ferry, near the sea, under old sea-weed, stones, &c., you will find a small yellowish transparent beetle, with two or four blackish marks on the back. Under these stones there are two sorts, one much darker than the other; the lighter-coloured is that which I want. These last two insects are *excessively rare*, and you will really *extremely* oblige me by taking all this trouble pretty soon. Remember me most kindly to Butler, tell him of my success, and I dare say both of you will easily recognise these insects. I hope his caterpillars go on well. I think many of the Chrysalises are well worth keeping. I really am quite ashamed [of] so long a letter all about my own concerns; but do return good for evil, and send me a long account of all your proceedings.

In the first week I killed seventy-five head of game—a very contemptible number—but there are very few birds. I killed, however, a brace of black game. Since then I have been staying at the Fox's, near Derby; it is a very pleasant house, and the music meeting went off very well. I want to hear how Yates likes his gun, and what use he has made of it.

If the bottle is not large you can buy another for me, and when you pass through Shrewsbury you can leave these treasures, and I hope, if you possibly can, you will stay a day or two with me, as I hope I need not say how glad I shall be to see you again. Fox remarked what deuced good-natured fellows your friends at Barmouth must be; and if I did not know that you and Butler were so, I would not think of giving you so much trouble.

Believe me, my dear Herbert,
Yours, most sincerely,
CHARLES DARWIN.

Remember me to all friends.

© The Complete Work of Charles Darwin Online

[In the following January we find him looking forward with pleasure to the beginning of another year of his Cambridge life: he writes to Fox—

"I waited till to-day for the chance of a letter, but I will wait no longer. I must most sincerely and cordially congratulate you on having finished all your labours. I think your place a *very good* one considering by how much you have beaten many men who had the start of you in reading. I do so wish I were now in Cambridge (a very selfish wish, however, as I was not with you in all your troubles and misery), to join in all the glory and happiness, which dangers gone by can give. How we would talk, walk, and entomologise! Sappho should be the best of bitches, and Dash, of dogs: then should be 'peace on earth, good will to men,'—which, by the way, I always think the most perfect description of happiness that words can give."]

C. Darwin to W. D. Fox.

Cambridge, Thursday [February 26, 1829].

MY DEAR FOX,

When I arrived here on Tuesday I found to my great grief and surprise, a letter on my table which I had written to you about a fortnight ago, the stupid porter never took the trouble of getting the letter forwarded. I suppose you have been abusing me for a most ungrateful wretch; but I am sure you will pity me now, as nothing is so vexatious as having written a letter in vain.

Last Thursday I left Shrewsbury for London, and stayed there till Tuesday, on which I came down here by the 'Times.' The first two days I spent entirely with Mr. Hope,* and did little else but talk about and look at insects; his collection is most magnificent, and he himself is the most generous of entomologists; he has given me about 160 new species, and

* Founder of the Chair of Zoology at Oxford.

© The Complete Work of Charles Darwin Online

actually often wanted to give me the rarest insects of which he had only two specimens. He made many civil speeches, and hoped you will call on him some time with me, whenever we should happen to be in London. He greatly compliments our exertions in Entomology, and says we have taken a wonderfully great number of good insects. On Sunday I spent the day with Holland, who lent me a horse to ride in the Park with.

On Monday evening I drank tea with Stephens;* his cabinet is more magnificent than the most zealous entomologist could dream of; he appears to be a very good-humoured pleasant little man. Whilst in town I went to the Royal Institution, Linnean Society, and Zoological Gardens, and many other places where naturalists are gregarious. If you had been with me, I think London would be a very delightful place; as things were, it was much pleasanter than I could have supposed such a dreary wilderness of houses to be.

I shot whilst in Shrewsbury a Dundiver (female Goosander, as I suppose you know). Shaw has stuffed it, and when I have an opportunity I will send it to Osmaston. There have been shot also five Waxen Chatterers, three of which Shaw has for sale; would you like to purchase a specimen? I have not yet thanked you for your last very long and agreeable letter. It would have been still more agreeable had it contained the joyful intelligence that you were coming up here; my two solitary breakfasts have already made me aware how very very much I shall miss you.

* * * * *

Believe me,

My dear old Fox,

Most sincerely yours,

C. DARWIN.

* J. F. Stephens, author of 'A Manual of British Coleoptera,' 1839, and other works.

© The Complete Work of Charles Darwin Online

[Later on in the Lent term he writes to Fox :—

"I am leading a quiet everyday sort of a life ; a little of Gibbon's History in the morning, and a good deal of *Van John* in the evening ; this, with an occasional ride with Simcox and constitutional with Whitley, makes up the regular routine of my days. I see a good deal both of Herbert and Whitley, and the more I see of them increases every day the respect I have for their excellent understandings and dispositions. They have been giving some very gay parties, nearly sixty men there both evenings."]

C. Darwin to W. D. Fox.

Christ's College [Cambridge], April 1 [1829].

MY DEAR FOX,

In your letter to Holden you are pleased to observe "that of all the blackguards you ever met with I am the greatest." Upon this observation I shall make no remarks, excepting that I must give you all due credit for acting on it most rigidly. And now I should like to know in what one particular are you less of a blackguard than I am ? You idle old wretch, why have you not answered my last letter, which I am sure I forwarded to Clifton nearly three weeks ago ? If I was not really very anxious to hear what you are doing, I should have allowed you to remain till you thought it worth while to treat me like a gentleman. And now having vented my spleen in scolding you, and having told you, what you must know, how very much and how anxiously I want to hear how you and your family are getting on at Clifton, the purport of this letter is finished. If you did but know how often I think of you, and how often I regret your absence, I am sure I should have heard from you long enough ago.

I find Cambridge rather stupid, and as I know scarcely any one that walks, and this joined with my lips not being quite so well, has reduced me to a sort of hybernation. . . . I have

© The Complete Work of Charles Darwin Online

caught Mr. Harbour letting —— have the first pick of the beetles; accordingly we have made our final adieus, my part in the affecting scene consisted in telling him he was a d—d rascal, and signifying I should kick him down the stairs if ever he appeared in my rooms again. It seemed altogether mightily to surprise the young gentleman. I have no news to tell you; indeed, when a correspondence has been broken off like ours has been, it is difficult to make the first start again. Last night there was a terrible fire at Linton, eleven miles from Cambridge. Seeing the reflection so plainly in the sky, Hall, Woodyeare, Turner, and myself thought we would ride and see it. We set out at half-past nine, and rode like incarnate devils there, and did not return till two in the morning. Altogether it was a most awful sight. I cannot conclude without telling you, that of all the blackguards I ever met with, you are the greatest and the best.

C. DARWIN.

C. Darwin to W. D. Fox.

[Cambridge, Thursday, April 23, 1829.]

MY DEAR FOX,

I have delayed answering your last letter for these few days, as I thought that under such melancholy circumstances my writing to you would be probably only giving you trouble. This morning I received a letter from Catherine informing me of that event,* which, indeed, from your letter, I had hardly dared to hope would have happened otherwise. I feel most sincerely and deeply for you and all your family; but at the same time, as far as any one can, by his own good principles and religion, be supported under such a misfortune, you, I am assured, will know where to look for such support. And after so pure and holy a comfort as the Bible affords, I am equally assured how useless the sympathy of all friends

* The death of Fox's sister, Mrs. Bristowe.

© The Complete Work of Charles Darwin Online

must appear, although it be as heartfelt and sincere, as I hope you believe me capable of feeling. At such a time of deep distress I will say nothing more, excepting that I trust your father and Mrs. Fox bear this blow as well as, under such circumstances, can be hoped for.

I am afraid it will be a long time, my dear Fox, before we meet; till then, believe me at all times,

Yours most affectionately,
CHARLES DARWIN.

C. Darwin to W. D. Fox.

Shrewsbury, Friday [July 4, 1829].

MY DEAR FOX,

I should have written to you before only that whilst our expedition lasted I was too much engaged, and the conclusion was so unfortunate, that I was too unhappy to write to you till this week's quiet at home. The thoughts of Woodhouse next week has at last given me courage to relate my unfortunate case.

I started from this place about a fortnight ago to take an entomological trip with Mr. Hope through all North Wales; and Barmouth was our first destination. The two first days I went on pretty well, taking several good insects; but for the rest of that week my lips became suddenly so bad,* and I myself not very well, that I was unable to leave the room, and on the Monday I retreated with grief and sorrow back again to Shrewsbury. The first two days I took some good insects. . . . But the days that I was unable to go out, Mr. Hope did wonders . . . . and to-day I have received another parcel of insects from him, such Colymbetes, such Carabi, and such magnificent Elaters (two species of the bright scarlet sort). I am sure you will properly sympathise with my unfortunate situation: I am determined I will go over the

* Probably with eczema, from which he often suffered.

© The Complete Work of Charles Darwin Online

same ground that he does before autumn comes, and if working hard will procure insects I will bring home a glorious stock.

* * * * *

My dear Fox,
Yours most sincerely,
CHAS. DARWIN.

C. Darwin to W. D. Fox.

Shrewsbury, July 18, 1829.

I am going to Maer next week in order to entomologise, and shall stay there a week, and for the rest of this summer I intend to lead a perfectly idle and wandering life. . . . You see I am much in the same state that you are, with this difference, you make good resolutions and never keep them; I never make them, so cannot keep them; it is all very well writing in this manner, but I must read for my Little-go. Graham smiled and bowed so very civilly, when he told me that he was one of the six appointed to make the examination stricter, and that they were determined this would make it a very different thing from any previous examination, that from all this I am sure it will be the very devil to pay amongst all idle men and entomologists. Erasmus, we expect home in a few weeks' time: he intends passing next winter in Paris. Be sure you order the two lists of insects published by Stephens, one printed on both sides, and the other only on one; you will find them very useful in many points of view.

Dear old Fox, yours,
C. DARWIN.

C. Darwin to W. D. Fox.

Christ's College, Thursday [October 16, 1829].

MY DEAR FOX,

I am afraid you will be very angry with me for not having written during the Music Meeting, but really I was

© The Complete Work of Charles Darwin Online

worked so hard that I had no time; I arrived here on Monday and found my rooms in dreadful confusion, as they have been taking up the floor, and you may suppose that I have had plenty to do for these two days. The Music Meeting * was the most glorious thing I ever experienced; and as for Malibran, words cannot praise her enough, she is quite the most charming person I ever saw. We had extracts out of several of the best operas, acted in character, and you cannot imagine how very superior it made the concerts to any I ever heard before. J. de Begnis † acted 'Il Fanatico' in character; being dressed up an extraordinary figure gives a much greater effect to his acting. He kept the whole theatre in roars of laughter. I liked Madame Blasis very much, but nothing will do after Malibran, who sung some comic songs, and [a] person's heart must have been made of stone not to have lost it to her. I lodged very near the Wedgwoods, and lived entirely with them, which was very pleasant, and had you been there it would have been quite perfect. It knocked me up most dreadfully, and I will never attempt again to do two things the same day.

* * * * *

C. Darwin to W. D. Fox.

[Cambridge] Thursday [March, 1830].

MY DEAR FOX,

I am through my Little-Go!!! I am too much exalted to humble myself by apologising for not having written before. But I assure you before I went in, and when my nerves were in a shattered and weak condition, your injured person often rose before my eyes and taunted me with my idleness. But I am through, through, through. I could write the whole sheet full with this delightful word. I went in yesterday, and have

* At Birmingham. † De Begnis's Christian name was Giuseppe.

© The Complete Work of Charles Darwin Online

just heard the joyful news. I shall not know for a week which class I am in. The whole examination is carried on in a different system. It has one grand advantage—being over in one day. They are rather strict, and ask a wonderful number of questions.

And now I want to know something about your plans; of course you intend coming up here: what fun we will have together; what beetles we will catch; it will do my heart good to go once more together to some of our old haunts. I have two very promising pupils in Entomology, and we will make regular campaigns into the Fens. Heaven protect the beetles and Mr. Jenyns, for we won't leave him a pair in the whole country. My new Cabinet is come down, and a gay little affair it is.

And now for the time—I think I shall go for a few days to town to hear an opera and see Mr. Hope; not to mention my brother also, whom I should have no objection to see. If I go pretty soon, you can come afterwards, but if you will settle your plans definitely, I will arrange mine, so send me a letter by return of post. And I charge you let it be favourable—that is to say, come directly. Holden has been ordained, and drove the Coach out on the Monday. I do not think he is looking very well. Chapman wants you and myself to pay him a visit when you come up, and begs to be remembered to you. You must excuse this short letter, as I have no end more to send off by this day's post. I long to see you again, and till then,

My dear good old Fox,

Yours most sincerely,

C. DARWIN.

[In August he was in North Wales and wrote to Fox:—

"I have been intending to write every hour for the last fortnight, but *really* have had no time. I left Shrewsbury this day fortnight ago, and have since that time been

© The Complete Work of Charles Darwin Online

working from morning to night in catching fish or beetles. This is literally the first idle day I have had to myself; for on the rainy days I go fishing, on the good ones entomologising. You may recollect that for the fortnight previous to all this, you told me not to write, so that I hope I have made out some sort of defence for not having sooner answered your two long and very agreeable letters."]

C. Darwin to W. D. Fox.

[Cambridge, November 5, 1830.]

MY DEAR FOX,

I have so little time at present, and am so disgusted by reading that I have not the heart to write to anybody. I have only written once home since I came up. This must excuse me for not having answered your three letters, for which I am really very much obliged. . . .

I have not stuck an insect this term, and scarcely opened a case. If I had time I would have sent you the insects which I have so long promised; but really I have not spirits or time to do anything. Reading makes me quite desperate; the plague of getting up all my subjects is next thing to intolerable. Henslow is my tutor, and a most *admirable* one he makes; the hour with him is the pleasantest in the whole day. I think he is quite the most perfect man I ever met with. I have been to some very pleasant parties there this term. His good-nature is unbounded.

I am sure you will be sorry to hear poor old Whitley's father is dead. In a worldly point of view it is of great consequence to him, as it will prevent him going to the Bar for some time.—(Be sure answer this:) What did you pay for the iron hoop you had made in Shrewsbury? Because I do not mean to pay the whole of the Cambridge man's bill. You need not trouble yourself about the Phallus, as I have bought up both species. I have heard men say that Henslow

© The Complete Work of Charles Darwin Online

has some curious religious opinions. I never perceived anything of it, have you? I am very glad to hear, after all your delays, you have heard of a curacy where you may read all the commandments without endangering your throat. I am also still more glad to hear that your mother continues steadily to improve. I do trust that you will have no further cause for uneasiness. With every wish for your happiness, my dear old Fox,

Believe me yours most sincerely,
CHARLES DARWIN.

C. Darwin to W. D. Fox.

Cambridge, Sunday, January 23, 1831.

MY DEAR FOX,

I do hope you will excuse my not writing before I took my degree. I felt a quite inexplicable aversion to write to anybody. But now I do most heartily congratulate you upon passing your examination, and hope you find your curacy comfortable. If it is my last shilling (I have not many), I will come and pay you a visit.

I do not know why the degree should make one so miserable, both before and afterwards. I recollect you were sufficiently wretched before, and I can assure [you] I am now, and what makes it the more ridiculous is, I know not what about. I believe it is a beautiful provision of nature to make one regret the less leaving so pleasant a place as Cambridge; and amongst all its pleasures—I say it for once and for all—none so great as my friendship with you. I sent you a newspaper yesterday, in which you will see what a good place [10th] I have got in the Poll. As for Christ's, did you ever see such a college for producing Captains and Apostles?* There are no men either at Emmanuel or Christ's plucked. Cameron is

* The "Captain" is at the head of the "Poll": the "Apostles" are the last twelve in the Mathematical Tripos.

© The Complete Work of Charles Darwin Online

gulfed, together with other three Trinity scholars! My plans are not at all settled. I think I shall keep this term, and then go and economise at Shrewsbury, return and take my degree.

A man may be excused for writing so much about himself when he has just passed the examination; so you must excuse [me]. And on the same principle do you write a letter brimful of yourself and plans. I want to know something about your examination. Tell me about the state of your nerves; what books you got up, and how perfect. I take an interest about that sort of thing, as the time will come when I must suffer. Your tutor, Thompson, begged to be remembered to you, and so does Whitley. If you will answer this, I will send as many stupid answers as you can desire.

Believe me, dear Fox,
CHAS. DARWIN.

© The Complete Work of Charles Darwin Online

CHAPTER V.

THE APPOINTMENT TO THE 'BEAGLE.'

[IN a letter addressed to Captain Fitz-Roy, before the *Beagle* sailed, my father wrote, "What a glorious day the 4th of November* will be to me—my second life will then commence, and it shall be as a birthday for the rest of my life."

The circumstances which led to this second birth—so much more important than my father then imagined—are connected with his Cambridge life, but may be more appropriately told in the present chapter. Foremost in the chain of circumstances which led to his appointment to the *Beagle*, was my father's friendship with Professor Henslow. He wrote in a pocket-book or diary, which contains a brief record of dates, &c., throughout his life:—

"1831. *Christmas.*—Passed my examination for B.A. degree and kept the two following terms.

"During these months lived much with Professor Henslow, often dining with him and walking with him; became slightly acquainted with several of the learned men in Cambridge, which much quickened the zeal which dinner parties and hunting had not destroyed.

"In the spring paid Mr. Dawes a visit with Ramsay and Kirby, and talked over an excursion to Teneriffe. In the spring Henslow persuaded me to think of Geology, and introduced me to Sedgwick. During Midsummer geologized a little in Shropshire.

* The *Beagle* did not however make her final and successful start until December 27.

© The Complete Work of Charles Darwin Online

"*August.*—Went on Geological tour * by Llangollen, Ruthin, Conway, Bangor, and Capel Curig, where I left Professor Sedgwick, and crossed the mountain to Barmouth."

In a letter to Fox (May 1831), my father writes:—"I am very busy . . . and see a great deal of Henslow, whom I do not know whether I love or respect most." His feeling for this admirable man is finely expressed in a letter which he wrote to Rev. L. Blomefield (then Rev. L. Jenyns), when the latter was engaged in his 'Memoir of Professor Henslow' (published 1862). The passage † has been made use of in the first of the memorial notices written for 'Nature,' and Mr. Romanes points out that my father, "while describing the character of another, is unconsciously giving a most accurate description of his own":—

"I went to Cambridge early in the year 1828, and soon became acquainted, through some of my brother entomologists, with Professor Henslow, for all who cared for any branch of natural history were equally encouraged by him. Nothing could be more simple, cordial, and unpretending than the encouragement which he afforded to all young naturalists. I soon became intimate with him, for he had a remarkable power of making the young feel completely at ease with him; though we were all awe-struck with the amount of his knowledge. Before I saw him, I heard one young man sum up his attainments by simply saying that he knew everything. When I reflect how immediately we felt at perfect ease with a man older, and in every way so immensely our superior, I think it was as much owing to the transparent sincerity of his character as to his kindness of heart; and, perhaps, even still more, to a highly remarkable absence in him of all self-consciousness. One perceived at once that he never thought of

* Mentioned by Sedgwick in his preface to Salter's 'Catalogue of Cambrian and Silurian Fossils,' 1873.

† 'Memoir of the Rev. John Stevens Henslow, M.A.,' by the Rev. Leonard Jenyns. 8vo. London, 1862, p. 51.

© The Complete Work of Charles Darwin Online

his own varied knowledge or clear intellect, but solely on the subject in hand. Another charm, which must have struck every one, was that his manner to old and distinguished persons and to the youngest student was exactly the same: and to all he showed the same winning courtesy. He would receive with interest the most trifling observation in any branch of natural history; and however absurd a blunder one might make, he pointed it out so clearly and kindly, that one left him no way disheartened, but only determined to be more accurate the next time. In short, no man could be better formed to win the entire confidence of the young, and to encourage them in their pursuits.

"His Lectures on Botany were universally popular, and as clear as daylight. So popular were they, that several of the older members of the University attended successive courses. Once every week he kept open house in the evening, and all who cared for natural history attended these parties, which, by thus favouring inter-communication, did the same good in Cambridge, in a very pleasant manner, as the Scientific Societies do in London. At these parties many of the most distinguished members of the University occasionally attended; and when only a few were present, I have listened to the great men of those days, conversing on all sorts of subjects, with the most varied and brilliant powers. This was no small advantage to some of the younger men, as it stimulated their mental activity and ambition. Two or three times in each session he took excursions with his botanical class; either a long walk to the habitat of some rare plant, or in a barge down the river to the fens, or in coaches to some more distant place, as to Gamlingay, to see the wild lily of the valley, and to catch on the heath the rare natter-jack. These excursions have left a delightful impression on my mind. He was, on such occasions, in as good spirits as a boy, and laughed as heartily as a boy at the misadventures of those who chased the splendid swallow-tail butterflies across the broken and

© The Complete Work of Charles Darwin Online

treacherous fens. He used to pause every now and then and lecture on some plant or other object; and something he could tell us on every insect, shell, or fossil collected, for he had attended to every branch of natural history. After our day's work we used to dine at some inn or house, and most jovial we then were. I believe all who joined these excursions will agree with me that they have left an enduring impression of delight on our minds.

"As time passed on at Cambridge I became very intimate with Professor Henslow, and his kindness was unbounded; he continually asked me to his house, and allowed me to accompany him in his walks. He talked on all subjects, including his deep sense of religion, and was entirely open. I owe more than I can express to this excellent man. . . .

"During the years when I associated so much with Professor Henslow, I never once saw his temper even ruffled. He never took an ill-natured view of any one's character, though very far from blind to the foibles of others. It always struck me that his mind could not be even touched by any paltry feeling of vanity, envy, or jealousy. With all this equability of temper and remarkable benevolence, there was no insipidity of character. A man must have been blind not to have perceived that beneath this placid exterior there was a vigorous and determined will. When principle came into play, no power on earth could have turned him one hair's-breadth. . . .

"Reflecting over his character with gratitude and reverence, his moral attributes rise, as they should do in the highest character, in pre-eminence over his intellect."

In a letter to Rev. L. Blomefield (Jenyns), May 24, 1862, my father wrote with the same feelings that he had expressed in his letters thirty years before:—

"I thank you most sincerely for your kind present of your Memoir of Henslow. I have read about half, and it has interested me much. I did not think that I could have venerated

© The Complete Work of Charles Darwin Online

him more than I did; but your book has even exalted his character in my eyes. From turning over the pages of the latter half, I should think your account would be invaluable to any clergyman who wished to follow poor dear Henslow's noble example. What an admirable man he was."

The geological work mentioned in the quotation from my father's pocket-book was doubtless of importance as giving him some practical experience, and perhaps of more importance in helping to give him some confidence in himself. In July of the same year, 1831, he was "working like a tiger" at Geology, and trying to make a map of Shropshire, but not finding it "as easy as I expected."

In writing to Henslow about the same time, he gives some account of his work:—

"I should have written to you some time ago, only I was determined to wait for the clinometer, and I am very glad to say I think it will answer admirably. I put all the tables in my bedroom at every conceivable angle and direction. I will venture to say I have measured them as accurately as any geologist going could do . . . . I have been working at so many things that I have not got on much with geology. I suspect the first expedition I take, clinometer and hammer in hand, will send me back very little wiser and a good deal more puzzled than when I started. As yet I have only indulged in hypotheses, but they are such powerful ones that I suppose, if they were put into action but for one day, the world would come to an end."

He was evidently most keen to get to work with Sedgwick, for he wrote to Henslow: "I have not heard from Professor Sedgwick, so I am afraid he will not pay the Severn formations a visit. I hope and trust you did your best to urge him."

My father has given in his Recollections some account of this Tour.

There too we read of the projected excursion to the

© The Complete Work of Charles Darwin Online

Canaries, of which slight mention occurs in letters to Fox and Henslow.

In April 1831 he writes to Fox: "At present I talk, think, and dream of a scheme I have almost hatched of going to the Canary Islands. I have long had a wish of seeing tropical scenery and vegetation, and, according to Humboldt, Teneriffe is a very pretty specimen." And again in May: "As for my Canary scheme, it is rash of you to ask questions; my other friends most sincerely wish me there, I plague them so with talking about tropical scenery, &c. Eyton will go next summer, and I am learning Spanish."

Later on in the summer the scheme took more definite form, and the date seems to have been fixed for June 1832. He got information in London about passage-money, and in July was working at Spanish and calling Fox "un grandísimo lebron," in proof of his knowledge of the language; which, however, he found "intensely stupid." But even then he seems to have had some doubts about his companions' zeal, for he writes to Henslow (July 27, 1831): "I hope you continue to fan your Canary ardour. I read and re-read Humboldt; do you do the same? I am sure nothing will prevent us seeing the Great Dragon Tree."

Geological work and Teneriffe dreams carried him through the summer, till on returning from Barmouth for the sacred 1st of September, he received the offer of appointment as Naturalist to the *Beagle*.

The following extract from the pocket-book will be a help in reading the letters :—

"Returned to Shrewsbury at end of August. Refused offer of voyage.

"*September*.—Went to Maer, returned with Uncle Jos. to Shrewsbury, thence to Cambridge. London.

"11*th*.—Went with Captain Fitz-Roy in steamer to Plymouth to see the *Beagle*.

© The Complete Work of Charles Darwin Online

"*22nd.*—Returned to Shrewsbury, passing through Cambridge.

"*October 2nd.*—Took leave of my home. Stayed in London.

"*24th.*—Reached Plymouth.

"*October and November.*—These months very miserable.

"*December 10th.*—Sailed, but were obliged to put back.

"*21st.*—Put to sea again, and were driven back.

"*27th.*—Sailed from England on our Circumnavigation."]

*George Peacock * to J. S. Henslow.*

7 Suffolk Street, Pall Mall East [1831].

MY DEAR HENSLOW,

Captain Fitz-Roy is going out to survey the southern coast of Tierra del Fuego, and afterwards to visit many of the South Sea Islands, and to return by the Indian Archipelago. The vessel is fitted out expressly for scientific purposes, combined with the survey; it will furnish, therefore, a rare opportunity for a naturalist, and it would be a great misfortune that it should be lost.

An offer has been made to me to recommend a proper person to go out as a naturalist with this expedition; he will be treated with every consideration. The Captain is a young man of very pleasing manners (a nephew of the Duke of Grafton), of great zeal in his profession, and who is very highly spoken of; if Leonard Jenyns could go, what treasures he might bring home with him, as the ship would be placed at his disposal whenever his inquiries made it necessary or desirable. In the absence of so accomplished a naturalist, is there any person whom you could strongly recommend? he must be such a person as would do credit to our recommenda-

* Formerly Dean of Ely, and Lowndean Professor of Astronomy at Cambridge.

© The Complete Work of Charles Darwin Online

tion. Do think of this subject, it would be a serious loss to the cause of natural science if this fine opportunity was lost.

* * * * *

The ship sails about the end of September.

Write immediately, and tell me what can be done.

Believe me,

My dear Henslow,

Most truly yours,

GEORGE PEACOCK.

J. S. Henslow to C. Darwin.

Cambridge, August 24, 1831.

MY DEAR DARWIN,

Before I enter upon the immediate business of this letter, let us condole together upon the loss of our inestimable friend poor Ramsay, of whose death you have undoubtedly heard long before this.

I will not now dwell upon this painful subject, as I shall hope to see you shortly, fully expecting that you will eagerly catch at the offer which is likely to be made you of a trip to Tierra del Fuego, and home by the East Indies. I have been asked by Peacock, who will read and forward this to you from London, to recommend him a Naturalist as companion to Captain Fitz-Roy, employed by Government to survey the southern extremity of America. I have stated that I consider you to be the best qualified person I know of who is likely to undertake such a situation. I state this not in the supposition of your being a *finished* naturalist, but as amply qualified for collecting, observing, and noting, anything worthy to be noted in Natural History. Peacock has the appointment at his disposal, and if he cannot find a man willing to take the office, the opportunity will probably be lost. Captain Fitz-Roy wants a man (I understand) more as a companion than a mere collector, and would not take any one, however good a

© The Complete Work of Charles Darwin Online

naturalist, who was not recommended to him likewise as a *gentleman*. Particulars of salary, &c., I know nothing. The voyage is to last two years, and if you take plenty of books with you, anything you please may be done. You will have ample opportunities at command. In short, I suppose there never was a finer chance for a man of zeal and spirit; Captain Fitz-Roy is a young man. What I wish you to do is instantly to come and consult with Peacock (at No. 7 Suffolk Street, Pall Mall East, or else at the University Club), and learn further particulars. Don't put on any modest doubts or fears about your disqualifications, for I assure you I think you are the very man they are in search of; so conceive yourself to be tapped on the shoulder by your bum-bailiff and affectionate friend,

J. S. HENSLOW.

The expedition is to sail on 25th September (at earliest), so there is no time to be lost.

G. Peacock to C. Darwin.

[1831.]

MY DEAR SIR,

I received Henslow's letter last night too late to forward it to you by the post; a circumstance which I do not regret, as it has given me an opportunity of seeing Captain Beaufort at the Admiralty (the Hydrographer), and of stating to him the offer which I have to make to you. He entirely approves of it, and you may consider the situation as at your absolute disposal. I trust that you will accept it, as it is an opportunity which should not be lost, and I look forward with great interest to the benefit which our collections of Natural History may receive from your labours.

The circumstances are these:—

Captain Fitz-Roy (a nephew of the Duke of Grafton) sails at the end of September, in a ship to survey, in the first

© The Complete Work of Charles Darwin Online

instance, the South Coast of Tierra del Fuego, afterwards to visit the South Sea Islands, and to return by the Indian Archipelago to England. The expedition is entirely for scientific purposes, and the ship will generally wait your leisure for researches in Natural History, &c. Captain Fitz-Roy is a public-spirited and zealous officer, of delightful manners, and greatly beloved by all his brother officers. He went with Captain Beechey,* and spent £1500 in bringing over and educating at his own charge three natives of Patagonia. He engages at his own expense an artist at £200 a year to go with him. You may be sure, therefore, of having a very pleasant companion, who will enter heartily into all your views.

The ship sails about the end of September, and you must lose no time in making known your acceptance to Captain Beaufort, Admiralty Hydrographer. I have had a good deal of correspondence about this matter [with Henslow?], who feels, in common with myself, the greatest anxiety that you should go. I hope that no other arrangements are likely to interfere with it. . . .

The Admiralty are not disposed to give a salary, though they will furnish you with an official appointment, and every accommodation. If a salary should be required, however, I am inclined to think that it would be granted.

Believe me, my dear Sir,

Very truly yours,

GEORGE PEACOCK.

* For "Beechey," read "King." I do not find the name Fitz-Roy in the list of Beechey's officers. The Fuegians were brought back from Captain King's voyage.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

Shrewsbury, Tuesday [August 30, 1831].

MY DEAR SIR,

Mr. Peacock's letter arrived on Saturday, and I received it late yesterday evening. As far as my own mind is concerned, I should, I think *certainly*, most gladly have accepted the opportunity which you so kindly have offered me. But my father, although he does not decidedly refuse me, gives such strong advice against going, that I should not be comfortable if I did not follow it.

My father's objections are these: the unfitting me to settle down as a Clergyman, my little habit of seafaring, *the shortness of the time*, and the chance of my not suiting Captain Fitz-Roy. It is certainly a very serious objection, the very short time for all my preparations, as not only body but mind wants making up for such an undertaking. But if it had not been for my father I would have taken all risks. What was the reason that a Naturalist was not long ago fixed upon? I am very much obliged for the trouble you have had about it; there certainly could not have been a better opportunity.

* * * * *

My trip with Sedgwick answered most perfectly. I did not hear of poor Mr. Ramsay's loss till a few days before your letter. I have been lucky hitherto in never losing any person for whom I had any esteem or affection. My acquaintance, although very short, was sufficient to give me those feelings in a great degree. I can hardly make myself believe he is no more. He was the finest character I ever knew.

Yours most sincerely,
My dear Sir,
CH. DARWIN.

I have written to Mr. Peacock, and I mentioned that I have asked you to send one line in the chance of his not

© The Complete Work of Charles Darwin Online

getting my letter. I have also asked him to communicate with Captain Fitz-Roy. Even if I was to go, my father disliking would take away all energy, and I should want a good stock of that. Again I must thank you, it adds a little to the heavy but pleasant load of gratitude which I owe to you.

C. Darwin to R. W. Darwin.

[Maer] August 31 [1831].

MY DEAR FATHER,

I am afraid I am going to make you again very uncomfortable. But, upon consideration, I think you will excuse me once again, stating my opinions on the offer of the voyage. My excuse and reason is the different way all the Wedgwoods view the subject from what you and my sisters do.

I have given Uncle Jos * what I fervently trust is an accurate and full list of your objections, and he is kind enough to give his opinions on all. The list and his answers will be enclosed. But may I beg of you one favour, it will be doing me the greatest kindness, if you will send me a decided answer, yes or no? If the latter, I should be most ungrateful if I did not implicitly yield to your better judgment, and to the kindest indulgence you have shown me all through my life; and you may rely upon it I will never mention the subject again. If your answer should be yes; I will go directly to Henslow and consult deliberately with him, and then come to Shrewsbury.

The danger appears to me and all the Wedgwoods not great. The expense can not be serious, and the time I do not think, anyhow, would be more thrown away than if I stayed at home. But pray do not consider that I am so bent on going that I would for one *single moment* hesitate, if you thought that after a short period you should continue uncomfortable.

* Josiah Wedgwood.

© The Complete Work of Charles Darwin Online

I must again state I cannot think it would unfit me hereafter for a steady life. I do hope this letter will not give you much uneasiness. I send it by the car to-morrow morning; if you make up your mind directly will you send me an answer on the following day by the same means? If this letter should not find you at home, I hope you will answer as soon as you conveniently can.

I do not know what to say about Uncle Jos' kindness; I never can forget how he interests himself about me.

Believe me, my dear father,

Your affectionate son,

CHARLES DARWIN.

[Here follows the list of objections which are referred to in the following letter:—

(1.) Disreputable to my character as a Clergyman hereafter.

(2.) A wild scheme.

(3.) That they must have offered to many others before me the place of Naturalist.

(4.) And from its not being accepted there must be some serious objection to the vessel or expedition.

(5.) That I should never settle down to a steady life hereafter.

(6.) That my accommodations would be most uncomfortable.

(7.) That you [*i.e.* Dr. Darwin] should consider it as again changing my profession.

(8.) That it would be a useless undertaking.]

© The Complete Work of Charles Darwin Online

Josiah Wedgwood to R. W. Darwin.

Maer, August 31, 1831.
[Read this last.] *

MY DEAR DOCTOR,

I feel the responsibility of your application to me on the offer that has been made to Charles as being weighty, but as you have desired Charles to consult me, I cannot refuse to give the result of such consideration as I have been able to [give ?] it.

Charles has put down what he conceives to be your principal objections, and I think the best course I can take will be to state what occurs to me upon each of them.

1. I should not think that it would be in any degree disreputable to his character as a Clergyman. I should on the contrary think the offer honourable to him; and the pursuit of Natural History, though certainly not professional, is very suitable to a clergyman.

2. I hardly know how to meet this objection, but he would have definite objects upon which to employ himself, and might acquire and strengthen habits of application, and I should think would be as likely to do so as in any way in which he is likely to pass the next two years at home.

3. The notion did not occur to me in reading the letters; and on reading them again with that object in my mind I see no ground for it.

4. I cannot conceive that the Admiralty would send out a bad vessel on such a service. As to objections to the expedition, they will differ in each man's case, and nothing would, I think, be inferred in Charles's case, if it were known that others had objected.

5. You are a much better judge of Charles's character than I can be. If on comparing this mode of spending the next two years with the way in which he will probably spend

* In C. Darwin's writing.

© The Complete Work of Charles Darwin Online

them, if he does not accept this offer, you think him more likely to be rendered unsteady and unable to settle, it is undoubtedly a weighty objection. Is it not the case that sailors are prone to settle in domestic and quiet habits?

6. I can form no opinion on this further than that if appointed by the Admiralty he will have a claim to be as well accommodated as the vessel will allow.

7. If I saw Charles now absorbed in professional studies I should probably think it would not be advisable to interrupt them; but this is not, and, I think, will not be the case with him. His present pursuit of knowledge is in the same track as he would have to follow in the expedition.

8. The undertaking would be useless as regards his profession, but looking upon him as a man of enlarged curiosity, it affords him such an opportunity of seeing men and things as happens to few.

You will bear in mind that I have had very little time for consideration, and that you and Charles are the persons who must decide.

I am,
My dear Doctor,
Affectionately yours,
JOSIAH WEDGWOOD.

C. Darwin to J. S. Henslow.

Cambridge, Red Lion [Sept. 2], 1831.

MY DEAR SIR,

I am just arrived; you will guess the reason. My father has changed his mind. I trust the place is not given away.

I am very much fatigued, and am going to bed.

I dare say you have not yet got my second letter.

How soon shall I come to you in the morning? Send a verbal answer.

Good night,
Yours,
C. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to Miss Susan Darwin.

Cambridge, Sunday Morning [September 4, 1831].

MY DEAR SUSAN,

As a letter would not have gone yesterday, I put off writing till to-day. I had rather a wearisome journey, but got into Cambridge very fresh. The whole of yesterday I spent with Henslow, thinking of what is to be done, and that I find is a great deal. By great good luck I know a man of the name of Wood, nephew of Lord Londonderry. He is a great friend of Captain Fitz-Roy, and has written to him about me. I heard a part of Captain Fitz-Roy's letter, dated some time ago, in which he says: "I have a right good set of officers, and most of my men have been there before." It seems he has been there for the last few years; he was then second in command with the same vessel that he has now chosen. He is only twenty-three years old, but [has] seen a deal of service, and won the gold medal at Portsmouth. The Admiralty say his maps are most perfect. He had choice of two vessels, and he chose the smallest. Henslow will give me letters to all travellers in town whom he thinks may assist me.

Peacock has sole appointment of Naturalist. The first person offered was Leonard Jenyns, who was so near accepting it that he packed up his clothes. But having two livings, he did not think it right to leave them—to the great regret of all his family. Henslow himself was not very far from accepting it, for Mrs. Henslow most generously, and without being asked, gave her consent; but she looked so miserable that Henslow at once settled the point.

* * * * *

I am afraid there will be a good deal of expense at first. Henslow is much against taking many things; it is [the] mistake all young travellers fall into. I write as if it was settled, but Henslow tells me *by no means* to make up my mind till I have had long conversations with Captains

© The Complete Work of Charles Darwin Online

Beaufort and Fitz-Roy. Good-bye. You will hear from me constantly. Direct 17 Spring Gardens. *Tell nobody* in Shropshire yet. Be sure not.

C. DARWIN.

I was so tired that evening I was in Shrewsbury that I thanked none of you for your kindness half so much as I felt.

Love to my father.

The reason I don't want people told in Shropshire: in case I should not go, it will make it more flat.

C. Darwin to Miss S. Darwin.

17 Spring Gardens, Monday
[September 5, 1831].

I have so little time to spare that I have none to waste in re-writing letters, so that you must excuse my bringing up the other with me and altering it. The last letter was written in the morning. In [the] middle of [the] day, Wood received a letter from Captain Fitz-Roy, which I must say was *most* straightforward and *gentlemanlike*, but so much against my going, that I immediately gave up the scheme; and Henslow did the same, saying that he thought Peacock has acted *very wrong* in misrepresenting things so much.

I scarcely thought of going to town, but here I am; and now for more details, and much more promising ones. Captain Fitz-Roy is [in] town, and I have seen him; it is no use attempting to praise him as much as I feel inclined to do, for you would not believe me. One thing I am certain, nothing could be more open and kind than he was to me. It seems he had promised to take a friend with him, who is in office and cannot go, and he only received the letter five minutes before I came in; and this makes things much better for me, as want of room was one of Fitz-Roy's greatest objections. He offers me to go share in everything in his

© The Complete Work of Charles Darwin Online

cabin if I like to come, and every sort of accommodation that I can have, but they will not be numerous. He says nothing would be so miserable for him as having me with him if I was uncomfortable, as in a small vessel we must be thrown together, and thought it his duty to state everything in the worst point of view. I think I shall go on Sunday to Plymouth to see the vessel.

There is something most extremely attractive in his manners and way of coming straight to the point. If I live with him, he says I must live poorly—no wine, and the plainest dinners. The scheme is not certainly so good as Peacock describes. Captain Fitz-Roy advises me not [to] make up my mind quite yet, but that, seriously, he thinks it will have much more pleasure than pain for me. The vessel does not sail till the 10th of October. It contains sixty men, five or six officers, &c., but is a small vessel. It will probably be out nearly three years. I shall pay to mess the same as [the] Captain does himself, £30 per annum; and Fitz-Roy says if I spend, including my outfitting, £500, it will be beyond the extreme. But now for still worse news. The round the world is not *certain*, but the chance most excellent. Till that point is decided, I will not be so. And you may believe, after the many changes I have made, that nothing but my reason shall decide me.

Fitz-Roy says the stormy sea is exaggerated; that if I do not choose to remain with them, I can at any time get home to England, so many vessels sail that way, and that during bad weather (probably two months), if I like, I shall be left in some healthy, safe and nice country; that I shall always have assistance; that he has many books, all instruments, guns, at my service; that the fewer and cheaper clothes I take the better. The manner of proceeding will just suit me. They anchor the ship, and then remain for a fortnight at a place. I have made Captain Beaufort perfectly understand me. He says if I start and do not go round the world,

© The Complete Work of Charles Darwin Online

I shall have good reason to think myself deceived. I am to call the day after to-morrow, and, if possible, to receive more certain instructions. The want of room is decidedly the most serious objection; but Captain Fitz-Roy (probably owing to Wood's letter) seems determined to make me [as] comfortable as he possibly can. I like his manner of proceeding. He asked me at once, "Shall you bear being told that I want the cabin to myself?—when I want to be alone. If we treat each other this way, I hope we shall suit; if not, probably we should wish each other at the devil."

We stop a week at [the] Madeira Islands, and shall see most of [the] big cities in South America. Captain Beaufort is drawing up the track through the South Sea. I am writing in [a] great hurry; I do not know whether you take interest enough to excuse treble postage. I hope I am judging reasonably, and not through prejudice, about Captain Fitz-Roy; if so, I am sure we shall suit. I dine with him to-day. I could write [a] great deal more if I thought you liked it, and I had at present time. There is indeed a tide in the affairs of man, and I have experienced it, and I had *entirely* given it up till one to-day.

Love to my father. Dearest Susan, good-bye.

CH. DARWIN.

C. Darwin to J. S. Henslow.

London, Monday [September 5, 1831].

MY DEAR SIR,

Gloria in excelsis is the most moderate beginning I can think of. Things are more prosperous than I should have thought possible. Captain Fitz-Roy is everything that is delightful. If I was to praise half so much as I feel inclined, you would say it was absurd, only once seeing him. I think he really wishes to have me. He offers me to mess with him, and he will take care I have such room as is possible But about the cases he says I must limit myself; but then he

© The Complete Work of Charles Darwin Online

thinks like a sailor about size. Captain Beaufort says I shall be upon the Boards, and then it will only cost me like other officers. Ship sails 10th of October. Spends a week at Madeira Islands; and then Rio de Janeiro. They all think most extremely probable, home by the Indian archipelago; but till that is decided, I will not be so.

What has induced Captain Fitz-Roy to take a better view of the case is, that Mr. Chester, who was going as a friend, cannot go, so that I shall have his place in every respect.

Captain Fitz-Roy has [a] good stock of books, many of which were in my list, and rifles, &c., so that the outfit will be much less expensive than I supposed.

The vessel will be out three years. I do not object so that my father does not. On Wednesday I have another interview with Captain Beaufort, and on Sunday most likely go with Captain Fitz-Roy to Plymouth. So I hope you will keep on thinking on the subject, and just keep memoranda of what may strike you. I will call most probably on Mr. Burchell and introduce myself. I am in lodgings at 17 Spring Gardens. You cannot imagine anything more pleasant, kind, and open than Captain Fitz-Roy's manners were to me. I am sure it will be my fault if we do not suit.

What changes I have had. Till one to-day I was building castles in the air about hunting foxes in Shropshire, now llamas in South America.

There is indeed a tide in the affairs of men. If you see Mr. Wood, remember me very kindly to him.

Good-bye.

My dear Henslow,

Your most sincere friend,

CHAS. DARWIN.

Excuse this letter in such a hurry.

© The Complete Work of Charles Darwin Online

C. Darwin to W. D. Fox.

17 Spring Gardens, London,
September 6, 1831.

* * * * *

Your letter gave me great pleasure. You cannot imagine how much your former letter annoyed and hurt me.* But, thank heaven, I firmly believe that it was my *own entire* fault in so interpreting your letter. I lost a friend the other day, and I doubt whether the moral death (as I then wickedly supposed) of our friendship did not grieve me as much as the real and sudden death of poor Ramsay. We have known each other too long to need, I trust, any more explanations. But I will mention just one thing—that on my death-bed, I think I could say I never uttered one insincere (which at the time I did not fully feel) expression about my regard for you. One thing more—the sending *immediately* the insects, on my honour, was an unfortunate coincidence. I forgot how you naturally would take them. When you look at them now, I hope no unkindly feelings will rise in your mind, and that you will believe that you have always had in me a sincere, and, I will add, an obliged friend. The very many pleasant minutes that we spent together in Cambridge rose like departed spirits in judgment against me. May we have many more such, will be one of my last wishes in leaving England. God bless you, dear old Fox. May you always be happy.

Yours truly,
CHAS. DARWIN.

I have left your letter behind, so do not know whether I direct right.

* He had misunderstood a letter of Fox's as implying a charge of falsehood.

© The Complete Work of Charles Darwin Online

'*C. Darwin to Miss Susan Darwin.*

17 Spring Gardens, Tuesday.
[September 6, 1831.]

MY DEAR SUSAN,

Again I am going to trouble you. I suspect, if I keep on at this rate, you will sincerely wish me at Tierra del Fuego, or any other Terra, but England. First I will give my commissions. Tell Nancy to make me some twelve instead of eight shirts. Tell Edward to send me up in my carpet-bag (he can slip the key in the bag tied to some string), my slippers, a pair of lightish walking-shoes, my Spanish books, my new microscope (about six inches long and three or four deep), which must have cotton stuffed inside; my geological compass; my father knows that; a little book, if I have got it in my bedroom—'Taxidermy.' Ask my father if he thinks there would be any objection to my taking arsenic for a little time, as my hands are not quite well, and I have always observed that if I once get them well, and change my manner of living about the same time, they will generally remain well. What is the dose? Tell Edward my gun is dirty. What is Erasmus's direction? Tell me if you think there is time to write and to receive an answer before I start, as I should like particularly to know what he thinks about it. I suppose you do not know Sir J. Mackintosh's direction?

I write all this as if it was settled, but it is not more than it was, excepting that from Captain Fitz-Roy wishing me so much to go, and, from his kindness, I feel a predestination I shall start. I spent a very pleasant evening with him yesterday. He must be more than twenty-three years old; he is of a slight figure, and a dark but handsome edition of Mr. Kynaston, and, according to my notions, pre-eminently good manners. He is all for economy, excepting on one point—viz., fire-arms. He recommends me strongly to get a

© The Complete Work of Charles Darwin Online

case of pistols like his, which cost £60!! and never to go on shore anywhere without loaded ones, and he is doubting about a rifle; he says I cannot appreciate the luxury of fresh meat here. Of course I shall buy nothing till everything is settled; but I work all day long at my lists, putting in and striking out articles. This is the first really cheerful day I have spent since I received the letter, and it all is owing to the sort of involuntary confidence I place in my *beau ideal* of a Captain.

We stop at Teneriffe. His object is to stop at as many places as possible. He takes out twenty chronometers, and it will be a "sin" not to settle the longitude. He tells me to get it down in writing at the Admiralty that I have the free choice to leave as soon and whenever I like. I dare say you expect I shall turn back at the Madeira; if I have a morsel of stomach left, I won't give up. Excuse my so often troubling and writing: the one is of great utility, the other a great amusement to me. Most likely I shall write to-morrow. Answer by return of post. Love to my father, dearest Susan.

C. DARWIN.

As my instruments want altering, send my things by the 'Oxonian' the same night.

C. Darwin to Miss Susan Darwin.

London, Friday Morning, September 9, 1831.

MY DEAR SUSAN,

I have just received the parcel. I suppose it was not delivered yesterday owing to the Coronation. I am very much obliged to my father, and everybody else. Everything is done quite right. I suppose by this time you have received my letter written next day, and I hope will send off the things. My affairs remain *in statu quo*. Captain Beaufort says I am on the books for victuals, and he thinks I shall have no difficulty about my collections when I come home. But he is

© The Complete Work of Charles Darwin Online

too deep a fish for me to make him out. The only thing that now prevents me finally making up my mind, is the want of certainty about the South Sea Islands; although morally I have no doubt we should go there whether or no it is put in the instructions. Captain Fitz-Roy says I do good by plaguing Captain Beaufort, it stirs him up with a long pole. Captain Fitz-Roy says he is sure he has interest enough (particularly if this Administration is not everlasting—I shall soon turn Tory!), anyhow, even when out, to get the ship ordered home by whatever track he likes. From what Wood says, I presume the Dukes of Grafton and Richmond interest themselves about him. By the way, Wood has been of the greatest use to me; and I am sure his personal introduction of me inclined Captain Fitz-Roy to have me.

To explain things from the very beginning: Captain Fitz-Roy first wished to have a Naturalist, and then he seems to have taken a sudden horror of the chances of having somebody he should not like on board the vessel. He confesses his letter to Cambridge was to throw cold water on the scheme. I don't think we shall quarrel about politics, although Wood (as might be expected from a Londonderry) solemnly warned Fitz-Roy that I was a Whig. Captain Fitz-Roy was before Uncle Jos., he said, "now your friends will tell you a sea-captain is the greatest brute on the face of the creation. I do not know how to help you in this case, except by hoping you will give me a trial." How one does change! I actually now wish the voyage was longer before we touch land. I feel my blood run cold at the quantity I have to do. Everybody seems ready to assist me. The Zoological want to make me a corresponding member. All this I can construe without crossing the Equator. But one friend is quite invaluable, viz. a Mr. Yarrell, a stationer, and excellent naturalist.* He goes

* William Yarrell, well known for his 'History of British Birds' and 'History of British Fishes,' was born in 1784. He inherited from his father a newsagent's business, to which he steadily adhered up to

© The Complete Work of Charles Darwin Online

to the shops with me and bullies about prices (not that I yet buy): hang me if I give £60 for pistols.

Yesterday all the shops were shut, so that I could do nothing; and I was child enough to give £1 1*s.* for an excellent seat to see the Procession.* And it certainly was very well worth seeing. I was surprised that any quantity of gold could make a long row of people quite glitter. It was like only what one sees in picture-books of Eastern processions. The King looked very well, and seemed popular, but there was very little enthusiasm; so little that I can hardly think there will be a coronation this time fifty years.

The Life Guards pleased me as much as anything—they are quite magnificent; and it is beautiful to see them clear a crowd. You think that they must kill a score at least, and apparently they really hurt nobody, but most deucedly frighten them. Whenever a crowd was so dense that the people were forced off the causeway, one of these six-feet gentlemen, on a black horse, rode straight at the place, making his horse rear very high, and fall on the thickest spot. You would suppose men were made of sponge to see them shrink away.

In the evening there was an illumination, and much grander than the one on the Reform Bill. All the principal streets were crowded just like a race-ground. Carriages generally being six abreast, and I will venture to say not going one mile an hour. The Duke of Northumberland learnt a lesson last time, for his house was very grand; much more so than the other great nobility, and in much better taste; every window in his house was full of straight lines of brilliant lights, and from their extreme regularity and number had a beautiful effect. The paucity of invention was very striking,

---

his death, "in his 73rd year." He was a man of a thoroughly amiable and honourable character, and was a valued office-bearer of several of the learned Societies.

* The Coronation of William IV.

© The Complete Work of Charles Darwin Online

crowns, anchors, and "W. R.'s" were repeated in endless succession. The prettiest were gas-pipes with small holes; they were almost painfully brilliant. I have written so much about the Coronation, that I think you will have no occasion to read the *Morning Herald*.

For about the first time in my life I find London very pleasant; hurry, bustle, and noise are all in unison with my feelings. And I have plenty to do in spare moments. I work at Astronomy, as I suppose it would astound a sailor if one did not know how to find Latitude and Longitude. I am now going to Captain Fitz-Roy, and will keep [this] letter open till evening for anything that may occur. I will give you one proof of Fitz-Roy being a good officer—all the officers are the same as before; two-thirds of his crew and [the] eight marines who went before all offered to come again, so the service cannot be so very bad. The Admiralty have just issued orders for a large stock of canister-meat and lemon-juice, &c. &c. I have just returned from spending a long day with Captain Fitz-Roy, driving about in his gig, and shopping. This letter is too late for to-day's post. You may consider it settled that I go. Yet there is room for change if any untoward accident should happen; this I can see no reason to expect. I feel convinced nothing else will alter my wish of going. I have begun to order things. I have procured a case of good strong pistols and an excellent rifle for £50, there is a saving; a good telescope, with compass, £5, and these are nearly the only expensive instruments I shall want. Captain Fitz-Roy has everything. I never saw so (what I should call, he says not) extravagant a man, as regards himself, but as economical towards me. How he did order things! His fire-arms will cost £400 at least. I found the carpet bag when I arrived all right, and much obliged. I do not think I shall take any arsenic; shall send partridges to Mr. Yarrell; much obliged. Ask Edward to *bargain with* Clemson to make for my gun—*two spare* hammers or cocks,

© The Complete Work of Charles Darwin Online

two main-springs, two sere-springs, four nipples or plugs—I mean one for each barrel, except nipples, of which there must be two for each, all of excellent quality, and set about them immediately; tell Edward to make inquiries about prices. I go on Sunday per packet to Plymouth, shall stay one or two days, then return, and hope to find a letter from you; a few days in London; then Cambridge, Shrewsbury, London, Plymouth, Madeira, is my route. It is a great bore my writing so much about the Coronation; I could fill another sheet. I have just been with Captain King, Fitz-Roy's senior officer last expedition; he thinks that the expedition will suit me. Unasked, he said Fitz-Roy's temper was perfect. He sends his own son with him as midshipman. The key of my microscope was forgotten; it is of no consequence. Love to all.

CHAS. DARWIN.

C. Darwin to W. D. Fox.

17 Spring Gardens (and here I shall remain till I start)
[September 19, 1831].

MY DEAR FOX,

I returned from my expedition to see the *Beagle* at Plymouth on Saturday, and found your most welcome letter on my table. It is quite ridiculous what a very long period these last twenty days have appeared to me, certainly much more than as many weeks on ordinary occasions; this will account for my not recollecting how much I told you of my plans.

* * * * *

But on the whole it is a grand and fortunate opportunity; there will be so many things to interest me—fine scenery and an endless occupation and amusement in the different branches of Natural History; then again navigation and meteorology will amuse me on the voyage, joined to the grand requisite of

© The Complete Work of Charles Darwin Online

there being a pleasant set of officers, and, as far as I can judge, this is certain. On the other hand there is very considerable risk to one's life and health, and the leaving for so very long a time so many people whom I dearly love, is oftentimes a feeling so painful that it requires all my resolution to overcome it. But everything is now settled, and before the 20th of October I trust to be on the broad sea. My objection to the vessel is its smallness, which cramps one so for room for packing my own body and all my cases, &c. &c. As to its safety, I hope the Admiralty are the best judges; to a landsman's eye she looks very small. She is a ten-gun three-masted brig, but, I believe, an excellent vessel. So much for my future plans, and now for my present. I go to-night by the mail to Cambridge, and from thence, after settling my affairs, proceed to Shrewsbury (most likely on Friday 23rd, or perhaps before); there I shall stay a few days, and be in London by the 1st of October, and start for Plymouth on the 9th.

And now for the principal part of my letter. I do not know how to tell you how very kind I feel your offer of coming to see me before I leave England. Indeed I should like it very much; but I must tell you decidedly that I shall have very little time to spare, and that little time will be almost spoilt by my having so much to think about; and secondly, I can hardly think it worth your while to leave your parish for such a cause. But I shall never forget such generous kindness. Now I know you will act just as you think right; but do not come up for my sake. Any time is the same for me. I think from this letter you will know as much of my plans as I do myself, and will judge accordingly the where and when to write to me. Every now and then I have moments of glorious enthusiasm, when I think of the date and cocoa-trees, the palms and ferns so lofty and beautiful, everything new, everything sublime. And if I live to see years in after life, how grand must such recollections be! Do

© The Complete Work of Charles Darwin Online

you know Humboldt? (if you don't, do so directly.) With what intense pleasure he appears always to look back on the days spent in the tropical countries. I hope when you next write to Osmaston, [you will] tell them my scheme, and give them my kindest regards and farewells.

Good-bye, my dear Fox,

Yours ever sincerely,

CHAS. DARWIN.

C. Darwin to R. Fitz-Roy.

17 Spring Gardens [October 17? 1831].

DEAR FITZ-ROY,

Very many thanks for your letter; it has made me most comfortable, for it would have been heart-breaking to have left anything quite behind, and I never should have thought of sending things by some other vessel. This letter will, I trust, accompany some talc. I read your letter without attending to the name. But I have now procured some from Jones, which appears very good, and I will send it this evening by the mail. You will be surprised at not seeing me *propriâ personâ* instead of my handwriting. But I had just found out that the large steam-packet did not intend to sail on Sunday, and I was picturing to myself a small, dirty cabin, with the proportion of 39-40ths of the passengers very sick, when Mr. Earl came in and told me the *Beagle* would not sail till the beginning of November. This, of course, settled the point; so that I remain in London one week more. I shall then send heavy goods by steamer and start myself by the coach on Sunday evening.

Have you a good set of mountain barometers? Several great guns in the scientific world have told me some points in geology to ascertain which entirely depend on their relative height. If you have not a good stock, I will add one more to the list. I ought to be ashamed to trouble you so much,

Q 2

© The Complete Work of Charles Darwin Online

but will you *send one line* to inform me? I am daily becoming more anxious to be off, and, if I am so, you must be in a perfect fever. What a glorious day the 4th of November will be to me! My second life will then commence, and it shall be as a birthday for the rest of my life.

Believe me, dear Fitz-Roy,

Yours most sincerely,

CHAS. DARWIN.

Monday.—I hope I have not put you to much inconvenience by ordering the room in readiness.

C. Darwin to J. S. Henslow.

Devonport, November 15, 1831.

MY DEAR HENSLOW,

The orders are come down from the Admiralty, and everything is finally settled. We positively sail the last day of this month, and I think before that time the vessel will be ready. She looks most beautiful, even a landsman must admire her. *We* all think her the most perfect vessel ever turned out of the Dockyard. One thing is certain, no vessel has been fitted out so expensively, and with so much care. Everything that can be made so is of mahogany, and nothing can exceed the neatness and beauty of all the accommodations. The instructions are very general, and leave a great deal to the Captain's discretion and judgment, paying a substantial as well as a verbal compliment to him.

* * * * *

No vessel ever left England with such a set of Chronometers, viz. twenty-four, all very good ones. In short, everything is well, and I have only now to pray for the sickness to moderate its fierceness, and I shall do very well. Yet I should not call it one of the very best opportunities for natural history that has ever occurred. The absolute want of room is an evil that nothing can surmount. I think L. Jenyns did

© The Complete Work of Charles Darwin Online

very wisely in not coming, that is judging from my own feelings, for I am sure if I had left college some few years, or been those years older, I *never* could have endured it. The officers (excepting the Captain) are like the freshest freshmen, that is in their manners, in everything else widely different. Remember me most kindly to him, and tell him if ever he dreams in the night of palm-trees, he may in the morning comfort himself with the assurance that the voyage would not have suited him.

I am much obliged for your advice, *de Mathematicis*. I suspect when I am struggling with a triangle, I shall often wish myself in your room, and as for those wicked sulky surds, I do not know what I shall do without you to conjure them. My time passes away very pleasantly. I know one or two pleasant people, foremost of whom is Mr. Thunder-and-lightning Harris,* whom I dare say you have heard of. My chiet employment is to go on board the *Beagle*, and try to look as much like a sailor as I can. I have no evidence of having taken in man, woman or child.

I am going to ask you to do one more commission, and I trust it will be the last. When I was in Cambridge, I wrote to Mr. Ash, asking him to send my College account to my father, after having subtracted about £30 for my furniture. This he has forgotten to do, and my father has paid the bill, and I want to have the furniture-money transmitted to my father. Perhaps you would be kind enough to speak to Mr. Ash. I have cost my father so much money, I am quite ashamed of myself.

I will write once again before sailing, and perhaps you will write to me before then.

Remember me to Professor Sedgwick and Mr. Peacock.

Believe me, yours affectionately,

CHAS. DARWIN.

* William Snow Harris, the Electrician.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

Devonport, December 3, 1831.

MY DEAR HENSLOW,

It is now late in the evening, and to-night I am going to sleep on board. On Monday we most certainly sail, so you may guess in what a desperate state of confusion we are all in. If you were to hear the various exclamations of the officers, you would suppose we had scarcely had a week's notice. I am just in the same way taken all *aback*, and in such a bustle I hardly know what to do. The number of things to be done is infinite. I look forward even to sea-sickness with something like satisfaction, anything must be better than this state of anxiety. I am very much obliged for your last kind and affectionate letter. I always like advice from you, and no one whom I have the luck to know is more capable of giving it than yourself. Recollect, when you write, that I am a sort of *protégé* of yours, and that it is your bounden duty to lecture me.

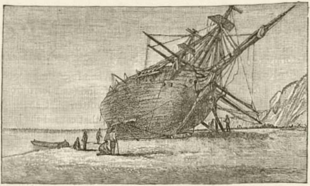
I will now give you my direction: it is at first, Rio; but if you will send me a letter on the first Tuesday (when the packet sails) in February, directed to Monte Video, it will give me very great pleasure; I shall so much enjoy hearing a little Cambridge news. Poor dear old *Alma Mater!* I am a very worthy son in as far as affection goes. I have little more to write about . . . . I cannot end this without telling you how cordially I feel grateful for the kindness you have shown me during my Cambridge life. Much of the pleasure and utility which I may have derived from it is owing to you. I long for the time when we shall again meet, and till then believe me, my dear Henslow,

Your affectionate and obliged friend,

CH. DARWIN.

Remember me most kindly to those who take any interest in me.

© The Complete Work of Charles Darwin Online



THE 'BEAGLE' LAID ASHORE, RIVER SANTA CRUZ.

CHAPTER VI.

THE VOYAGE.

"THERE is a natural good-humoured energy in his letters just like himself."—From a letter of Dr. R. W. Darwin's to Prof. Henslow.

[THE object of the *Beagle* voyage is briefly described in my father's 'Journal of Researches,' p. 1, as being "to complete the Survey of Patagonia and Tierra del Fuego, commenced under Captain King in 1826 to 1830; to survey the shores of Chile, Peru, and some islands in the Pacific; and to carry a chain of chronometrical measurements round the world."

The *Beagle* is described * as a well-built little vessel, of 235 tons, rigged as a barque, and carrying six guns. She belonged to the old class of ten-gun brigs, which were nicknamed "coffins," from their liability to go down in severe weather. They were very "deep-waisted," that is, their bul-

* 'Voyages of the *Adventure* and *Beagle*,' vol. i. introduction xii. The illustration at the head of the chapter is from vol. ii. of the same work.

© The Complete Work of Charles Darwin Online

warks were high in proportion to their size, so that a heavy sea breaking over them might be highly dangerous. Nevertheless, she lived through the five years' work, in the most stormy regions in the world, under Commanders Stokes and Fitz-Roy without a serious accident. When re-commissioned in 1831 for her second voyage, she was found (as I learn from Admiral Sir James Sulivan) to be so rotten that she had practically to be rebuilt, and it was this that caused the long delay in refitting. The upper deck was raised, making her much safer in heavy weather, and giving her far more comfortable accommodation below. By these alterations and by the strong sheathing added to her bottom she was brought up to 242 tons burthen. It is a proof of the splendid seamanship of Captain Fitz-Roy and his officers that she returned without having carried away a spar, and that in only one of the heavy storms that she encountered was she in great danger.

She was fitted out for the expedition with all possible care, being supplied with carefully chosen spars and ropes, six boats, and a "dinghy;" lightning conductors, "invented by Mr. Harris, were fixed in all the masts, the bowsprits, and even in the flying jib-boom." To quote my father's description, written from Devonport, November 17, 1831: "Everybody, who can judge, says it is one of the grandest voyages that has almost ever been sent out. Everything is on a grand scale. . . . In short, everything is as prosperous as human means can make it." The twenty-four chronometers and the mahogany fittings seem to have been especially admired, and are again alluded to.

Owing to the smallness of the vessel, every one on board was cramped for room, and my father's accommodation seems to have been small enough: "I have just room to turn round," he writes to Henslow, "and that is all." Admiral Sir James Sulivan writes to me: "The narrow space at the end of the chart-table was his only accommodation for working,

© The Complete Work of Charles Darwin Online

dressing, and sleeping; the hammock being left hanging over his head by day, when the sea was at all rough, that he might lie on it with a book in his hand when he could not any longer sit at the table. His only stowage for clothes being several small drawers in the corner, reaching from deck to deck; the top one being taken out when the hammock was hung up, without which there was not length for it, so then the foot-clews took the place of the top drawer. For specimens he had a very small cabin under the forecastle."

Yet of this narrow room he wrote enthusiastically, September 17, 1831:—"When I wrote last I was in great alarm about my cabin. The cabins were not then marked out, but when I left they were, and mine is a capital one, certainly next best to the Captain's and remarkably light. My companion most luckily, I think, will turn out to be the officer whom I shall like best. Captain Fitz-Roy says he will take care that one corner is so fitted up that I shall be comfortable in it and shall consider it my home, but that also I shall have the run of his. My cabin is the drawing one; and in the middle is a large table, on which we two sleep in hammocks. But for the first two months there will be no drawing to be done, so that it will be quite a luxurious room, and good deal larger than the Captain's cabin."

My father used to say that it was the absolute necessity of tidiness in the cramped space on the *Beagle* that helped 'to give him his methodical habits of working.' On the *Beagle*, too, he would say, that he learned what he considered the golden rule for saving time; *i.e.*, taking care of the minutes.

Sir James Sulivan tells me that the chief fault in the outfit of the expedition was the want of a second smaller vessel to act as tender. This want was so much felt by Captain Fitz-Roy that he hired two decked boats to survey the coast of Patagonia, at a cost of £1100, a sum which he had to supply, although the boats saved several thousand pounds to the country. He afterwards bought a schooner to act as a

© The Complete Work of Charles Darwin Online

tender, thus saving the country a further large amount. He was ultimately ordered to sell the schooner, and was compelled to bear the loss himself, and it was only after his death that some inadequate compensation was made for all the losses which he suffered through his zeal.

For want of a proper tender, much of the work had to be done in small open whale boats, which were sent away from the ship for weeks together, and this in a climate, where the crews were exposed to severe hardship from the almost constant rains, which sometimes continued for weeks together. The completeness of the equipment was also in other respects largely due to the public spirit of Captain Fitz-Roy. He provided at his own cost an artist, and a skilled instrument-maker, to look after the chronometers.* Captain Fitz-Roy's wish was to take "some well-educated and scientific person" as his private guest, but this generous offer was only accepted by my father on condition of being allowed to pay a fair share of the expense of the Captain's table; he was, moreover, on the ship's books for victuals.

In a letter to his sister (July 1832) he writes contentedly of his manner of life at sea:—"I do not think I have ever given you an account of how the day passes. We breakfast at eight o'clock. The invariable maxim is to throw away all politeness—that is, never to wait for each other, and bolt off the minute one has done eating, &c. At sea, when the weather is calm, I work at marine animals, with which the whole ocean abounds. If there is any sea up I am either sick or contrive to read some voyage or travels. At one we dine. You shore-going people are lamentably mistaken about the manner of living on board. We have never yet (nor shall we) dined off salt meat. Rice and peas and *calavanses* are excellent vegetables, and, with good bread, who could want more? Judge Alderson could not be more temperate, as nothing but water comes on the table. At five we have tea.

* Either one or both were on the books for victuals.

© The Complete Work of Charles Darwin Online

The midshipmen's berth have all their meals an hour before us, and the gun-room an hour afterwards."

The crew of the *Beagle* consisted of Captain Fitz-Roy, "Commander and Surveyor," two lieutenants, one of whom (the first lieutenant) was the late Captain Wickham, Governor of Queensland; the present Admiral Sir James Sulivan, K.C.B., was the second lieutenant. Besides the master and two mates, there was an assistant-surveyor, the present Admiral Lort Stokes. There were also a surgeon, assistant-surgeon, two midshipmen, master's mate, a volunteer (1st class), purser, carpenter, clerk, boatswain, eight marines, thirty-four seamen, and six boys.

There are not, I believe, many survivors of my father's old ship-mates. Admiral Mellersh, Mr. Hamond, and Mr. Philip King, of the Legislative Council of Sydney, and Mr. Usborne, are among the number. Admiral Johnson died almost at the same time as my father.

He retained to the last a most pleasant recollection of the voyage of the *Beagle*, and of the friends he made on board her. To his children their names were familiar, from his many stories of the voyage, and we caught his feeling of friendship for many who were to us nothing more than names.

It is pleasant to know how affectionately his old companions remember him.

Sir James Sulivan remained, throughout my father's lifetime, one of his best and truest friends. He writes:—"I can confidently express my belief that during the five years in the *Beagle*, he was never known to be out of temper, or to say one unkind or hasty word *of* or *to* any one. You will therefore readily understand how this, combined with the admiration of his energy and ability, led to our giving him the name of 'the dear old Philosopher.'" * Admiral Mellersh

* His other nickname was "The Flycatcher." I have heard my father tell how he overheard the boatswain of the *Beagle* showing another boatswain over the ship, and pointing out the officers: "That's our first lieutenant; that's our doctor; that's our flycatcher."

© The Complete Work of Charles Darwin Online

writes to me:—"Your father is as vividly in my mind's eye as if it was only a week ago that I was in the *Beagle* with him; his genial smile and conversation can never be forgotten by any who saw them and heard them. I was sent on two or three occasions away in a boat with him on some of his scientific excursions, and always looked forward to these trips with great pleasure, an anticipation that, unlike many others, was always realised. I think he was the only man I ever knew against whom I never heard a word said; and as people when shut up in a ship for five years are apt to get cross with each other, that is saying a good deal. Certainly we were always so hard at work, we had no time to quarrel, but if we had done so, I feel sure your father would have tried (and have been successful) to throw oil on the troubled waters."

Admiral Stokes, Mr. King, Mr. Usborne, and Mr. Hamond, all speak of their friendship with him in the same warm-hearted way.

Of the life on board and on shore his letters give some idea. Captain Fitz-Roy was a strict officer, and made himself thoroughly respected both by officers and men. The occasional severity of his manner was borne with because every one on board knew that his first thought was his duty, and that he would sacrifice anything to the real welfare of the ship. My father writes, July 1834, "We all jog on very well together, there is no quarrelling on board, which is something to say. The Captain keeps all smooth by rowing every one in turn." The best proof that Fitz-Roy was valued as a commander is given by the fact that many * of the crew had sailed with him in the *Beagle's* former voyage, and there were a few officers as well as seamen and marines, who had served in the *Adventure* or *Beagle* during the whole of that expedition.

My father speaks of the officers as a fine determined set of

* 'Voyage of the *Adventure* and *Beagle*,' vol. ii. p. 21.

© The Complete Work of Charles Darwin Online

men, and especially of Wickham, the first lieutenant, as a "glorious fellow." The latter being responsible for the smartness and appearance of the ship strongly objected to his littering the decks, and spoke of specimens as "d—d beastly devilment," and used to add, "If I were skipper, I would soon have you and all your d—d mess out of the place."

A sort of halo of sanctity was given to my father by the fact of his dining in the Captain's cabin, so that the midshipmen used at first to call him "Sir," a formality, however, which did not prevent his becoming fast friends with the younger officers. He wrote about the year 1861 or 1862 to Mr. P. G. King, M.L.C., Sydney, who, as before stated, was a midshipman on board the *Beagle*:—"The remembrance of old days, when we used to sit and talk on the booms of the *Beagle*, will always, to the day of my death, make me glad to hear of your happiness and prosperity." Mr. King describes the pleasure my father seemed to take "in pointing out to me as a youngster the delights of the tropical nights, with their balmy breezes eddying out of the sails above us, and the sea lighted up by the passage of the ship through the never-ending streams of phosphorescent animalculæ."

It has been assumed that his ill-health in later years was due to his having suffered so much from sea-sickness. This he did not himself believe, but rather ascribed his bad health to the hereditary fault which came out as gout in some of the past generations. I am not quite clear as to how much he actually suffered from sea-sickness; my impression is distinct that, according to his own memory, he was not actually ill after the first three weeks, but constantly uncomfortable when the vessel pitched at all heavily. But, judging from his letters, and from the evidence of some of the officers, it would seem that in later years he forgot the extent of the discomfort from which he suffered. Writing June 3, 1836, from the Cape of Good Hope, he says: "It is a lucky thing for me that the voyage is drawing to its close, for I positively

© The Complete Work of Charles Darwin Online

suffer more from sea-sickness now than three years ago." Admiral Lort Stokes wrote to the *Times*, April 25, 1883:—

"May I beg a corner for my feeble testimony to the marvellous persevering endurance in the cause of science of that great naturalist, my old and lost friend, Mr. Charles Darwin, whose remains are so very justly to be honoured with a resting-place in Westminster Abbey?

"Perhaps no one can better testify to his early and most trying labours than myself. We worked together for several years at the same table in the poop cabin of the *Beagle* during her celebrated voyage, he with his microscope and myself at the charts. It was often a very lively end of the little craft, and distressingly so to my old friend, who suffered greatly from sea-sickness. After, perhaps, an hour's work he would say to me, 'Old fellow, I must take the horizontal for it,' that being the best relief position from ship motion; a stretch out on one side of the table for some time would enable him to resume his labours for a while, when he had again to lie down.

"It was distressing to witness this early sacrifice of Mr. Darwin's health, who ever afterwards seriously felt the ill-effects of the *Beagle's* voyage."

Mr. A. B. Usborne writes, "He was a dreadful sufferer from sea-sickness, and at times, when I have been officer of the watch, and reduced the sails, making the ship more easy, and thus relieving him, I have been pronounced by him to be 'a good officer,' and he would resume his microscopic observations in the poop cabin." The amount of work that he got through on the *Beagle* shows that he was habitually in full vigour; he, had, however, one severe illness in South America, when he was received into the house of an Englishman, Mr. Corfield, who tended him with careful kindness. I have heard him say that in this illness every secretion of the body was affected, and that when he described the

© The Complete Work of Charles Darwin Online

symptoms to his father Dr. Darwin could make no guess as to the nature of the disease. My father was sometimes inclined to think that the breaking up of his health was to some extent due to this attack.

The *Beagle* letters give ample proof of his strong love of home, and all connected with it, from his father down to Nancy, his old nurse, to whom he sometimes sends his love.

His delight in home-letters is shown in such passages as:—"But if you knew the glowing, unspeakable delight, which I felt at being certain that my father and all of you were well, only four months ago, you would not grudge the labour lost in keeping up the regular series of letters."

Or again—his longing to return in words like these:—"It is too delightful to think that I shall see the leaves fall and hear the robin sing next autumn at Shrewsbury. My feelings are those of a schoolboy to the smallest point; I doubt whether ever boy longed for his holidays as much as I do to see you all again. I am at present, although nearly half the world is between me and home, beginning to arrange what I shall do, where I shall go during the first week."

Another feature in his letters is the surprise and delight with which he hears of his collections and observations being of some use. It seems only to have gradually occurred to him that he would ever be more than a collector of specimens and facts, of which the great men were to make use. And even as to the value of his collections he seems to have had much doubt, for he wrote to Henslow in 1834: "I really began to think that my collections were so poor that you were puzzled what to say; the case is now quite on the opposite tack, for you are guilty of exciting all my vain feelings to a most comfortable pitch; if hard work will atone for these thoughts, I vow it shall not be spared."

After his return and settlement in London, he began to realise the value of what he had done, and wrote to Captain Fitz-Roy—"However others may look back to the *Beagle's*

© The Complete Work of Charles Darwin Online

voyage, now that the small disagreeable parts are well-nigh forgotten, I think it far the *most fortunate circumstance in my life* that the chance afforded by your offer of taking a Naturalist fell on me. I often have the most vivid and delightful pictures of what I saw on board the *Beagle* pass before my eyes. These recollections, and what I learnt on Natural History, I would not exchange for twice ten thousand a year."

In selecting the following series of letters, I have been guided by the wish to give as much personal detail as possible. I have given only a few scientific letters, to illustrate the way in which he worked, and how he regarded his own results. In his 'Journal of Researches' he gives incidentally some idea of his personal character; the letters given in the present chapter serve to amplify in fresher and more spontaneous words that impression of his personality which the 'Journal' has given to so many readers.]

C. Darwin to R. W. Darwin.

Bahia, or San Salvador, Brazils
[February 8, 1832].
I find after the first page I have been writing to my sisters.

MY DEAR FATHER,

I am writing this on the 8th of February, one day's sail past St. Jago (Cape de Verd), and intend taking the chance of meeting with a homeward-bound vessel somewhere about the equator. The date, however, will tell this whenever the opportunity occurs. I will now begin from the day of leaving England, and give a short account of our progress. We sailed, as you know, on the 27th of December, and have been fortunate enough to have had from that time to the present a fair and moderate breeze. It afterwards proved that we had escaped a heavy gale in the Channel, another at Madeira, and another on [the] Coast of Africa. But in escaping the gale, we felt its consequence—a heavy sea. In

© The Complete Work of Charles Darwin Online

the Bay of Biscay there was a long and continuous swell, and the misery I endured from sea-sickness is far beyond what I ever guessed at. I believe you are curious about it. I will give you all my dear-bought experience. Nobody who has only been to sea for twenty-four hours has a right to say that sea-sickness is even uncomfortable. The real misery only begins when you are so exhausted that a little exertion makes a feeling of faintness come on. I found nothing but lying in my hammock did me any good. I must especially except your receipt of raisins, which is the only food that the stomach will bear.

On the 4th of January we were not many miles from Madeira, but as there was a heavy sea running, and the island lay to windward, it was not thought worth while to beat up to it. It afterwards has turned out it was lucky we saved ourselves the trouble. I was much too sick even to get up to see the distant outline. On the 6th, in the evening, we sailed into the harbour of Santa Cruz. I now first felt even moderately well, and I was picturing to myself all the delights of fresh fruit growing in beautiful valleys, and reading Humboldt's descriptions of the island's glorious views, when perhaps you may nearly guess at our disappointment, when a small pale man informed us we must perform a strict quarantine of twelve days. There was a death-like stillness in the ship till the Captain cried "up jib," and we left this long-wished for place.

We were becalmed for a day between Teneriffe and the Grand Canary, and here I first experienced any enjoyment. The view was glorious. The Peak of Teneriffe was seen amongst the clouds like another world. Our only drawback was the extreme wish of visiting this glorious island. *Tell Eyton never to forget either the Canary Islands or South America;* that I am sure it will well repay the necessary trouble, but that he must make up his mind to find a good deal of the latter. I feel certain he will regret it if he does

© The Complete Work of Charles Darwin Online

not make the attempt. From Teneriffe to St. Jago the voyage was extremely pleasant. I had a net astern the vessel which caught great numbers of curious animals, and fully occupied my time in my cabin, and on deck the weather was so delightful and clear, that the sky and water together made a picture. On the 16th we arrived at Port Praya, the capital of the Cape de Verds, and there we remained twenty-three days, viz. till yesterday, the 7th of February. The time has flown away most delightfully, indeed nothing can be pleasanter; exceedingly busy, and that business both a duty and a great delight. I do not believe I have spent one half-hour idly since leaving Teneriffe. St. Jago has afforded me an exceedingly rich harvest in several branches of Natural History. I find the descriptions scarcely worth anything of many of the commoner animals that inhabit the Tropics. I allude, of course, to those of the lower classes.

Geologising in a volcanic country is most delightful; besides the interest attached to itself, it leads you into most beautiful and retired spots. Nobody but a person fond of Natural History can imagine the pleasure of strolling under cocoa-nuts in a thicket of bananas and coffee-plants, and an endless number of wild flowers. And this island, that has given me so much instruction and delight, is reckoned the most uninteresting place that we perhaps shall touch at during our voyage. It certainly is generally very barren, but the valleys are more exquisitely beautiful, from the very contrast. It is utterly useless to say anything about the scenery; it would be as profitable to explain to a blind man colours, as to a person who has not been out of Europe, the total dissimilarity of a tropical view. Whenever I enjoy anything, I always either look forward to writing it down, either in my log-book (which increases in bulk), or in a letter; so you must excuse raptures, and those raptures badly expressed. I find my collections are increasing wonderfully, and from Rio I think I shall be obliged to send a cargo home.

© The Complete Work of Charles Darwin Online

All the endless delays which we experienced at Plymouth have been most fortunate, as I verily believe no person ever went out better provided for collecting and observing in the different branches of Natural History. In a multitude of counsellors I certainly found good. I find to my great surprise that a ship is singularly comfortable for all sorts of work. Everything is so close at hand, and being cramped makes one so methodical, that in the end I have been a gainer. I already have got to look at going to sea as a regular quiet place, like going back to home after staying away from it. In short, I find a ship a very comfortable house, with everything you want, and if it was not for sea-sickness the whole world would be sailors. I do not think there is much danger of Erasmus setting the example, but in case there should be, he may rely upon it he does not know one-tenth of the sufferings of sea-sickness.

I like the officers much more than I did at first, especially Wickham, and young King and Stokes, and indeed all of them. The Captain continues steadily very kind, and does everything in his power to assist me. We see very little of each other when in harbour, our pursuits lead us in such different tracks. I never in my life met with a man who could endure nearly so great a share of fatigue. He works incessantly, and when apparently not employed, he is thinking. If he does not kill himself, he will during this voyage do a wonderful quantity of work. I find I am very well, and stand the little heat we have had as yet as well as anybody. We shall soon have it in real earnest. We are now sailing for Fernando Noronha, off the coast of Brazil, where we shall not stay very long, and then examine the shoals between there and Rio, touching perhaps at Bahia. I will finish this letter when an opportunity of sending it occurs.

February 26th.—About 280 miles from Bahia. On the 10th we spoke the packet *Lyra*, on her voyage to Rio. I sent a short letter by her, to be sent to England on [the] first

© The Complete Work of Charles Darwin Online

opportunity. We have been singularly unlucky in not meeting with any homeward-bound vessels, but I suppose [at] Bahia we certainly shall be able to write to England. Since writing the first part of [this] letter nothing has occurred except crossing the Equator, and being shaved. This most disagreeable operation, consists in having your face rubbed with paint and tar, which forms a lather for a saw which represents the razor, and then being half drowned in a sail filled with salt water. About 50 miles north of the line we touched at the rocks of St. Paul; this little speck (about $\frac{1}{4}$ of a mile across) in the Atlantic has seldom been visited. It is totally barren, but is covered by hosts of birds; they were so unused to men that we found we could kill plenty with stones and sticks. After remaining some hours on the island, we returned on board with the boat loaded with our prey. From this we went to Fernando Noronha, a small island where the [Brazilians] send their exiles. The landing there was attended with so much difficulty owing [to] a heavy surf that the Captain determined to sail the next day after arriving. My one day on shore was exceedingly interesting, the whole island is one single wood so matted together by creepers that it is very difficult to move out of the beaten path. I find the Natural History of all these unfrequented spots most exceedingly interesting, especially the geology. I have written this much in order to save time at Bahia.

Decidedly the most striking thing in the Tropics is the novelty of the vegetable forms. Cocoa-nuts could well be imagined from drawings, if you add to them a graceful lightness which no European tree partakes of. Bananas and plantains are exactly the same as those in hothouses, the acacias or tamarinds are striking from the blueness of their foliage; but of the glorious orange trees, no description, no drawings, will give any just idea; instead of the sickly green of our oranges, the native ones exceed the Portugal laurel in the darkness of their tint, and infinitely exceed it in beauty of

© The Complete Work of Charles Darwin Online

form. Cocoa-nuts, papaws, the light green bananas, and oranges, loaded with fruit, generally surround the more luxuriant villages. Whilst viewing such scenes, one feels the impossibility that any description should come near the mark, much less be overdrawn.

March 1st.—Bahia, or San Salvador. I arrived at this place on the 28th of February, and am now writing this letter after having in real earnest strolled in the forests of the new world. No person could imagine anything so beautiful as the ancient town of Bahia, it is fairly embosomed in a luxuriant wood of beautiful trees, and situated on a steep bank, and overlooks the calm waters of the great bay of All Saints. The houses are white and lofty, and, from the windows being narrow and long, have a very light and elegant appearance. Convents, porticos, and public buildings, vary the uniformity of the houses; the bay is scattered over with large ships; in short, and what can be said more, it is one of the finest views in the Brazils. But the exquisite glorious pleasure of walking amongst such flowers, and such trees, cannot be comprehended but by those who have experienced it. Although in so low a latitude the locality is not disagreeably hot, but at present it is very damp, for it is the rainy season. I find the climate as yet agrees admirably with me; it makes me long to live quietly for some time in such a country. If you really want to have [an idea] of tropical countries, study Humboldt. Skip the scientific parts, and commence after leaving Teneriffe. My feelings amount to admiration the more I read him. Tell Eyton (I find I am writing to my sisters!) how exceedingly I enjoy America, and that I am sure it will be a great pity if he does not make a start.

This letter will go on the 5th, and I am afraid will be some time before it reaches you; it must be a warning how in other parts of the world you may be a long time without hearing. A year might by accident thus pass. About the 12th we start for Rio, but we remain some time on the way

© The Complete Work of Charles Darwin Online

in sounding the Albrolhos shoals. Tell Eyton as far as my experience goes let him study Spanish, French, drawing, and Humboldt. I do sincerely hope to hear of (if not to see him) in South America. I look forward to the letters in Rio—till each one is acknowledged, mention its date in the next.

We have beat all the ships in manœuvring, so much so that the commanding officer says, we need not follow his example; because we do everything better than his great ship. I begin to take great interest in naval points, more especially now, as I find they all say we are the No. 1 in South America. I suppose the Captain is a most excellent officer. It was quite glorious to-day how we beat the *Samarang* in furling sails. It is quite a new thing for a "sounding ship" to beat a regular man-of-war; and yet the *Beagle* is not at all a particular ship. Erasmus will clearly perceive it when he hears that in the night I have actually sat down in the sacred precincts of the quarter deck. You must excuse these queer letters, and recollect they are generally written in the evening after my day's work. I take more pains over my log-book, so that eventually you will have a good account of all the places I visit. Hitherto the voyage has answered *admirably* to me, and yet I am now more fully aware of your wisdom in throwing cold water on the whole scheme; the chances are so numerous of turning out quite the reverse; to such an extent do I feel this, that if my advice was asked by any person on a similar occasion, I should be very cautious in encouraging him. I have not time to write to anybody else, so send to Maer to let them know, that in the midst of the glorious tropical scenery, I do not forget how instrumental they were in placing me there. I will not rapturise again, but I give myself great credit in not being crazy out of pure delight.

Give my love to every soul at home, and to the Owens.

I think one's affections, like other good things, flourish and increase in these tropical regions.

The conviction that I am walking in the New World is

© The Complete Work of Charles Darwin Online

even yet marvellous in my own eyes, and I dare say it is little less so to you, the receiving a letter from a son of yours in such a quarter.

Believe me, my dear Father,
Your most affectionate son,
CHARLES DARWIN.

C. Darwin to W. D. Fox.

Botofogo Bay, near Rio de Janeiro,
May, 1832.

MY DEAR FOX,

I have delayed writing to you and all my other friends till I arrived here and had some little spare time. My mind has been, since leaving England, in a perfect *hurricane* of delight and astonishment, and to this hour scarcely a minute has passed in idleness. . . . .

At St. Jago my natural history and most delightful labours commenced. During the three weeks I collected a host of marine animals, and enjoyed many a good geological walk. Touching at some islands, we sailed to Bahia, and from thence to Rio, where I have already been some weeks. My collections go on admirably in almost every branch. As for insects, I trust I shall send a host of undescribed species to England. I believe they have no small ones in the collections, and here this morning I have taken minute Hydropori, Noterus, Colymbetes, Hydrophilus, Hydrobius, Gromius, &c. &c., as specimens of fresh-water beetles. I am entirely occupied with land animals, as the beach is only sand. Spiders and the adjoining tribes have perhaps given me, from their novelty, the most pleasure. I think I have already taken several new genera.

But Geology carries the day: it is like the pleasure of gambling. Speculating, on first arriving, what the rocks may be, I often mentally cry out 3 to 1 tertiary against primitive;

© The Complete Work of Charles Darwin Online

but the latter have hitherto won all the bets. So much for the grand end of my voyage: in other respects things are equally flourishing. My life, when at sea, is so quiet, that to a person who can employ himself, nothing can be pleasanter; the beauty of the sky and brilliancy of the ocean together make a picture. But when on shore, and wandering in the sublime forests, surrounded by views more gorgeous than even Claude ever imagined, I enjoy a delight which none but those who have experienced it can understand. If it is to be done, it must be by studying Humboldt. At our ancient snug breakfasts, at Cambridge, I little thought that the wide Atlantic would ever separate us; but it is a rare privilege that with the body, the feelings and memory are not divided. On the contrary, the pleasantest scenes in my life, many of which have been in Cambridge, rise from the contrast of the present, the more vividly in my imagination. Do you think any diamond beetle will ever give me so much pleasure as our old friend *crux major?* . . . . It is one of my most constant amusements to draw pictures of the past; and in them I often see you and poor little Fan. Oh, Lord, and then old Dash, poor thing! Do you recollect how you all tormented me about his beautiful tail?

. . . . Think when you are picking insects off a hawthorn-hedge on a fine May day (wretchedly cold, I have no doubt), think of me collecting amongst pine-apples and orange-trees; whilst staining your fingers with dirty blackberries, think and be envious of ripe oranges. This is a proper piece of bravado, for I would walk through many a mile of sleet, snow, or rain to shake you by the hand. My dear old Fox, God bless you. Believe me,

Yours very affectionately,

CHAS. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

Rio de Janeiro, May 18, 1832.

MY DEAR HENSLOW,

• • • • •

Till arriving at Teneriffe (we did not touch at Madeira) I was scarcely out of my hammock, and really suffered more than you can well imagine from such a cause. At Santa Cruz, whilst looking amongst the clouds for the Peak, and repeating to myself Humboldt's sublime descriptions, it was announced we must perform twelve days' strict quarantine. We had made a short passage, so "Up jib," and away for St. Jago. You will say all this sounds very bad, and so it was; but from that to the present time it has been nearly one scene of continual enjoyment. A net over the stern kept me at full work till we arrived at St. Jago. Here we spent three most delightful weeks. The geology was pre-eminently interesting, and I believe quite new; there are some facts on a large scale of upraised coast (which is an excellent epoch for all the volcanic rocks to date from), that would interest Mr. Lyell.

One great source of perplexity to me is an utter ignorance whether I note the right facts, and whether they are of sufficient importance to interest others. In the one thing collecting I cannot go wrong. St. Jago is singularly barren, and produces few plants or insects, so that my hammer was my usual companion, and in its company most delightful hours I spent. On the coast I collected many marine animals, chiefly gasteropodous (I think some new). I examined pretty accurately a *Caryophyllea*, and, if my eyes are not bewitched, former descriptions have not the slightest resemblance to the animal. I took several specimens of an Octopus which possessed a most marvellous power of changing its colours, equalling any chameleon, and evidently accommodating the changes to the colour of the ground which it passed over.

© The Complete Work of Charles Darwin Online

Yellowish green, dark brown, and red, were the prevailing colours; this fact appears to be new, as far as I can find out. Geology and the invertebrate animals will be my chief object of pursuit through the whole voyage.

We then sailed for Bahia, and touched at the rock of St. Paul. This is a serpentine formation. Is it not the only island in the Atlantic which is not volcanic? We likewise stayed a few hours at Fernando Noronha; a tremendous surf was running so that a boat was swamped, and the Captain would not wait. I find my life on board when we are on blue water most delightful, so very comfortable and quiet—it is almost impossible to be idle, and that for me is saying a good deal. Nobody could possibly be better fitted in every respect for collecting than I am; many cooks have not spoiled the broth this time. Mr. Brown's little hints about microscopes, &c., have been invaluable. I am well off in books, the 'Dictionnaire Classique' *is most useful*. If you should think of any thing or book that would be useful to me, if you would write one line, E. Darwin, Wyndham Club, St. James's Street, he will procure them, and send them with some other things to Monte Video, which for the next year will be my head-quarters.

Touching at the Abrolhos, we arrived here on April 4th, when amongst others I received your most kind letter. You may rely on it during the evening I thought of the many most happy hours I have spent with you in Cambridge. I am now living at Botofogo, a village about a league from the city, and shall be able to remain a month longer. The *Beagle* has gone back to Bahia, and will pick me up on its return. There is a most important error in the longitude of South America, to settle which this second trip has been undertaken. Our chronometers, at least sixteen of them, are going superbly; none on record have ever gone at all like them.

A few days after arriving I started on an expedition of 150 miles to Rio Macao, which lasted eighteen days. Here I

© The Complete Work of Charles Darwin Online

first saw a tropical forest in all its sublime grandeur—nothing but the reality can give any idea how wonderful, how magnificent the scene is. If I was to specify any one thing I should give the pre-eminence to the host of parasitical plants. Your engraving is exactly true, but underrates rather than exaggerates the luxuriance. I never experienced such intense delight. I formerly admired Humboldt, I now almost adore him; he alone gives any notion of the feelings which are raised in the mind on first entering the Tropics. I am now collecting fresh-water and land animals; if what was told me in London is true, viz. that there are no small insects in the collections from the Tropics, I tell Entomologists to look out and have their pens ready for describing. I have taken as minute (if not more so) as in England, Hydropori, Hygroti, Hydrobii, Pselaphi, Staphylini, Curculio, &c. &c. It is exceedingly interesting observing the difference of genera and species from those which I know; it is however much less than I had expected. I am at present red-hot with spiders; they are very interesting, and if I am not mistaken I have already taken some new genera. I shall have a large box to send very soon to Cambridge, and with that I will mention some more natural history particulars.

The Captain does everything in his power to assist me, and we get on very well, but I thank my better fortune he has not made me a renegade to Whig principles. I would not be a Tory, if it was merely on account of their cold hearts about that scandal to Christian nations—Slavery. I am very good friends with all the officers.

I have just returned from a walk, and as a specimen, how little the insects are known. Noterus, according to the 'Dictionnaire Classique,' contains solely three European species. I in one haul of my net took five distinct species; is this not quite extraordinary? . . . .

Tell Professor Sedgwick he does not know how much I am indebted to him for the Welsh Expedition; it has

© The Complete Work of Charles Darwin Online

given me an interest in Geology which I would not give up for any consideration. I do not think I ever spent a more delightful three weeks than pounding the North-west Mountains. I look forward to the geology about Monte Video as I hear there are slates there, so I presume in that district I shall find the junctions of the Pampas, and the enormous granite formation of Brazils. At Bahia the pegmatite and gneiss in beds had the same direction, as observed by Humboldt, prevailing over Columbia, distant 1300 miles—is it not wonderful? Monte Video will be for a long time my direction. I hope you will write again to me, there is nobody from whom I like receiving advice so much as from you. . . . Excuse this almost unintelligible letter, and believe me, my dear Henslow, with the warmest feelings of respect and friendship,

Yours affectionately,
CHAS. DARWIN.

C. Darwin to J. M. Herbert.

Botofogo Bay, Rio de Janeiro,
June 1832.

MY DEAR OLD HERBERT,

Your letter arrived here when I had given up all hopes of receiving another, it gave me, therefore, an additional degree of pleasure. At such an interval of time and space one does learn to feel truly obliged to those who do not forget one. The memory when recalling scenes past by, affords to us *exiles* one of the greatest pleasures. Often and often whilst wandering amongst these hills do I think of Barmouth, and, I may add, as often wish for such a companion. What a contrast does a walk in these two places afford; here abrupt and stony peaks are to the very summit enclosed by luxuriant woods; the whole surface of the country, excepting where cleared by man, is one impenetrable forest. How different from Wales, with its sloping hills covered with turf, and its

© The Complete Work of Charles Darwin Online

open valleys. I was not previously aware how intimately what may be called the moral part is connected with the enjoyment of scenery. I mean such ideas, as the history of the country, the utility of the produce, and more especially the happiness of the people living with them. Change the English labourer into a poor slave, working for another, and you will hardly recognise the same view. I am sure you will be glad to hear how very well every part (Heaven forefend, except sea-sickness) of the expedition has answered. We have already seen Teneriffe and the Great Canary; St. Jago, where I spent three most delightful weeks, revelling in the delights of first naturalising a tropical volcanic island, and besides other islands, the two celebrated ports in the Brazils, viz. Bahia and Rio.

I was in my hammock till we arrived at the Canaries, and I shall never forget the sublime impression the first view of Teneriffe made on my mind. The first arriving into warm weather was most luxuriously pleasant; the clear blue sky of the Tropics was no common change after those accursed south-west gales at Plymouth. About the Line it became weltering hot. We spent one day at St. Paul's, a little group of rocks about a quarter of a mile in circumference, peeping up in the midst of the Atlantic. There was such a scene here. Wickham (1st Lieutenant) and I were the only two who landed with guns and geological hammers, &c. The birds by myriads were too close to shoot; we then tried stones, but at last, *proh pudor!* my geological hammer was the instrument of death. We soon loaded the boat with birds and eggs. Whilst we were so engaged, the men in the boat were fairly fighting with the sharks for such magnificent fish as you could not see in the London market. Our boat would have made a fine subject for Snyders, such a medley of game it contained. We have been here ten weeks, and shall now start for Monte Video, when I look forward to many a gallop over the Pampas. I am ashamed of sending such a scrambling letter,

© The Complete Work of Charles Darwin Online

but if you were to see the heap of letters on my table, you would understand the reason. . . .

I am glad to hear music flourishes so well in Cambridge; but it [is] as barbarous to talk to me of "celestial concerts" as to a person in Arabia of cold water. In a voyage of this sort, if one gains many new and great pleasures, on the other side the loss is not inconsiderable. How should you like to be suddenly debarred from seeing every person and place, which you have ever known and loved, for five years? I do assure you I am occasionally "taken aback" by this reflection; and then for man or ship it is not so easy to right again. Remember me most sincerely to the remnant of most excellent fellows whom I have the good luck to know in Cambridge—I mean Whitley and Watkins. Tell Lowe I am even beneath his contempt. I can eat salt beef and musty biscuits for dinner. See what a fall man may come to!

My direction for the next year and a half will be Monte Video.

God bless you, my very dear old Herbert. May you always be happy and prosperous is my most cordial wish.

Yours affectionately,

CHAS. DARWIN.

C. Darwin to F. Watkins.

Monte Video, River Plata,
August 18, 1832.

MY DEAR WATKINS,

I do not feel very sure you will think a letter from one so far distant will be worth having; I write therefore on the selfish principle of getting an answer. In the different countries we visit the entire newness and difference from England only serves to make more keen the recollection of its scenes and delights. In consequence the pleasure of thinking of, and hearing from one's former friends, does indeed become great. Recollect this, and some long winter's evening

© The Complete Work of Charles Darwin Online

sit down and send me a long account of yourself and our friends; both what you have, and what [you] intend doing; otherwise in three or four more years when I return you will be all strangers to me. Considering how many months have passed, we have not in the *Beagle* made much way round the world. Hitherto everything has well repaid the necessary trouble and loss of comfort. We stayed three weeks at the Cape de Verds; it was no ordinary pleasure rambling over the plains of lava under a tropical sun, but when I first entered on and beheld the luxuriant vegetation in Brazil it was realizing the visions in the 'Arabian Nights.' The brilliancy of the scenery throws one into a delirium of delight, and a beetle hunter is not likely soon to awaken from it, when whichever way he turns fresh treasures meet his eye. At Rio de Janeiro three months passed away like so many weeks. I made a most delightful excursion during this time of 150 miles into the country. I stayed at an estate which is the last of the cleared ground, behind is one vast impenetrable forest. It is almost impossible to imagine the quietude of such a life. Not a human being within some miles interrupts the solitude. To seat oneself amidst the gloom of such a forest on a decaying trunk, and then think of home, is a pleasure worth taking some trouble for.

We are at present in a much less interesting country. One single walk over the undulatory turf plain shows everything which is to be seen. It is not at all unlike Cambridgeshire, only that every hedge, tree and hill must be levelled, and arable land turned into pasture. All South America is in such an unsettled state that we have not entered one port without some sort of disturbance. At Buenos Ayres a shot came whistling over our heads; it is a noise I had never before heard, but I found I had an instinctive knowledge of what it meant. The other day we landed our men here, and took possession at the request of the inhabitants of the central fort. We philosophers do not bargain for this sort of work,

© The Complete Work of Charles Darwin Online

and I hope there will be no more. We sail in the course of a day or two to survey the coast of Patagonia; as it is entirely unknown, I expect a good deal of interest. But already do I perceive the grievous difference between sailing on these seas and the Equinoctial ocean. In the "Ladies' Gulf," as the Spaniards call it, it is so luxurious to sit on deck and enjoy the coolness of the night, and admire the new constellations of the South. . . . I wonder when we shall ever meet again; but be it when it may, few things will give me greater pleasure than to see you again, and talk over the long time we have passed together.

If you were to meet me at present I certainly should be looked at like a wild beast, a great grizzly beard and flushing jacket would disfigure an angel. Believe me, my dear Watkins, with the warmest feelings of friendship,

Ever yours,

CHARLES DARWIN.

C. Darwin to J. S. Henslow.

April 11, 1833.

MY DEAR HENSLOW,

We are now running up from the Falkland Islands to the Rio Negro (or Colorado). The *Beagle* will proceed to Monte Video; but if it can be managed I intend staying at the former place. It is now some months since we have been at a civilised port; nearly all this time has been spent in the most southern part of Tierra del Fuego. It is a detestable place; gales succeed gales with such short intervals that it is difficult to do anything. We were twenty-three days off Cape Horn, and could by no means get to the westward. The last and final gale before we gave up the attempt was unusually severe. A sea stove one of the boats, and there was so much water on the decks that every place was afloat; nearly all the paper for drying plants is spoiled, and half of this curious collection.

© The Complete Work of Charles Darwin Online

We at last ran into harbour, and in the boats got to the west by the inland channels. As I was one of this party I was very glad of it. With two boats we went about 300 miles, and thus I had an excellent opportunity of geologising and seeing much of the savages. The Fuegians are in a more miserable state of barbarism than I had expected ever to have seen a human being. In this inclement country they are absolutely naked, and their temporary houses are like what children make in summer with boughs of trees. I do not think any spectacle can be more interesting than the first sight of man in his primitive wildness. It is an interest which cannot well be imagined until it is experienced. I shall never forget this when entering Good Success Bay—the yell with which a party received us. They were seated on a rocky point, surrounded by the dark forest of beech; as they threw their arms wildly round their heads, and their long hair streaming, they seemed the troubled spirits of another world. The climate in some respects is a curious mixture of severity and mildness; as far as regards the animal kingdom, the former character prevails; I have in consequence not added much to my collections.

The Geology of this part of Tierra del Fuego was, as indeed every place is, to me very interesting. The country is non-fossiliferous, and a common-place succession of granitic rocks and slates; attempting to make out the relation of cleavage, strata, &c., &c., was my chief amusement. The mineralogy, however, of some of the rocks will, I think, be curious from their resemblance to those of volcanic origin.

* * * * *

After leaving Tierra del Fuego we sailed to the Falklands. I forgot to mention the fate of the Fuegians whom we took back to their country. They had become entirely European in their habits and wishes, so much so that the younger one had forgotten his own language, and their countrymen paid but very little attention to them. We built houses for them

© The Complete Work of Charles Darwin Online

and planted gardens, but by the time we return again on our passage round the Horn, I think it will be very doubtful how much of their property will be left unstolen.

. . . When I am sea-sick and miserable, it is one of my highest consolations to picture the future when we again shall be pacing together the roads round Cambridge. That day is a weary long way off. We have another cruise to make to Tierra del Fuego next summer, and then our voyage round the world will really commence. Captain Fitz-Roy has purchased a large schooner of 170 tons. In many respects it will be a great advantage having a consort—perhaps it may somewhat shorten our cruise, which I most cordially hope it may. I trust, however, that the Coral Reefs and various animals of the Pacific may keep up my resolution. Remember me most kindly to Mrs. Henslow and all other friends; I am a true lover of Alma Mater and all its inhabitants,

Believe me, my dear Henslow,

Your affectionate and most obliged friend,

CHARLES DARWIN.

C. Darwin to Miss C. Darwin.

Maldonado, Rio Plata, May 22, 1833.

. . . The following business piece is to my father. Having a servant of my own would be a really great addition to my comfort. For these two reasons: as at present the Captain has appointed one of the men always to be with me, but I do not think it just thus to take a seaman out of the ship; and, secondly, when at sea I am rather badly off for any one to wait on me. The man is willing to be my servant, and all the expenses would be under £60 per annum. I have taught him to shoot and skin birds, so that in my main object he is very useful. I have now left England nearly a year and a half, and I find my expenses are not above

© The Complete Work of Charles Darwin Online

£200 per annum; so that, it being hopeless (from time) to write for permission, I have come to the conclusion that you would allow me this expense. But I have not yet resolved to ask the Captain, and the chances are even that he would not be willing to have an additional man in the ship. I have mentioned this because for a long time I have been thinking about it.

June.—I have just received a bundle more letters. I do not know how to thank you all sufficiently. One from Catherine, Feb. 8th, another from Susan, March 3rd, together with notes from Caroline and from my father; give my best love to my father. I almost cried for pleasure at receiving it; it was very kind thinking of writing to me. My letters are both few, short, and stupid in return for all yours; but I always ease my conscience by considering the Journal as a long letter. If I can manage it, I will, before doubling the Horn, send the rest. I am quite delighted to find the hide of the Megatherium has given you all some little interest in my employments. These fragments are not, however, by any means the most valuable of the geological relics. I trust and believe that the time spent in this voyage, if thrown away for all other respects, will produce its full worth in Natural History; and it appears to me the doing what *little* we can to increase the general stock of knowledge is as respectable an object of life as one can in any likelihood pursue. It is more the result of such reflections (as I have already said) than much immediate pleasure which now makes me continue the voyage, together with the glorious prospect of the future, when passing the Straits of Magellan, we have in truth the world before us. Think of the Andes, the luxuriant forest of Guayaquil, the islands of the South Sea, and New South Wales. How many magnificent and characteristic views, how many and curious tribes of men we shall see! What fine opportunities for geology and for studying the infinite host of living beings! Is not this a prospect to

S 2

© The Complete Work of Charles Darwin Online

keep up the most flagging spirit? If I was to throw it away, I don't think I should ever rest quiet in my grave. I certainly should be a ghost and haunt the British Museum.

How famously the Ministers appear to be going on. I always much enjoy political gossip and what you at home think will, &c., &c., take place. I steadily read up the weekly paper, but it is not sufficient to guide one's opinion; and I find it a very painful state not to be as obstinate as a pig in politics. I have watched how steadily the general feeling, as shown at elections, has been rising against Slavery. What a proud thing for England if she is the first European nation which utterly abolishes it! I was told before leaving England that after living in slave countries all my opinions would be altered; the only alteration I am aware of is forming a much higher estimate of the negro character. It is impossible to see a negro and not feel kindly towards him; such cheerful, open, honest expressions and such fine muscular bodies. I never saw any of the diminutive Portuguese, with their murderous countenances, without almost wishing for Brazil to follow the example of Hayti; and, considering the enormous healthy-looking black population, it will be wonderful if, at some future day, it does not take place. There is at Rio a man (I know not his title) who has a large salary to prevent (I believe) the landing of slaves; he lives at Botofogo, and yet that was the bay where, during my residence, the greater number of smuggled slaves were landed. Some of the Anti-Slavery people ought to question about his office; it was the subject of conversation at Rio amongst the lower English.....

C. Darwin to J. M. Herbert.

Maldonado, Rio Plata, June 2, 1833.

MY DEAR HERBERT,

I have been confined for the last three days to a miserable dark room, in an old Spanish house, from the torrents

© The Complete Work of Charles Darwin Online

of rain: I am not, therefore, in very good trim for writing; but, defying the blue devils, I will send you a few lines, if it is merely to thank you very sincerely for writing to me. I received your letter, dated December 1st, a short time since. We are now passing part of the winter in the Rio Plata, after having had a hard summer's work to the south. Tierra del Fuego is indeed a miserable place; the ceaseless fury of the gales is quite tremendous. One evening we saw old Cape Horn, and three weeks afterwards we were only thirty miles to windward of it. It is a grand spectacle to see all nature thus raging; but Heaven knows every one in the *Beagle* has seen enough in this one summer to last them their natural lives.

The first place we landed at was Good Success Bay. It was here Banks and Solander met such disasters on ascending one of the mountains. The weather was tolerably fine, and I enjoyed some walks in a wild country, like that behind Barmouth. The valleys are impenetrable from the entangled woods, but the higher parts, near the limits of perpetual snow, are bare. From some of these hills the scenery, from its savage, solitary character, was most sublime. The only inhabitant of these heights is the guanaco, and with its shrill neighing it often breaks the stillness. The consciousness that no European foot had ever trod much of this ground added to the delight of these rambles. How often and how vividly have many of the hours spent at Barmouth come before my mind! I look back to that time with no common pleasure; at this moment I can see you seated on the hill behind the inn, almost as plainly as if you were really there. It is necessary to be separated from all which one has been accustomed to, to know how properly to treasure up such recollections, and at this distance, I may add, how properly to esteem such as yourself, my dear old Herbert. I wonder when I shall ever see you again. I hope it may be, as you say, surrounded with heaps of parchment; but then there must be,

© The Complete Work of Charles Darwin Online

sooner or later, a dear little lady to take care of you and your house. Such a delightful vision makes me quite envious. This is a curious life for a regular shore-going person such as myself; the worst part of it is its extreme length. There is certainly a great deal of high enjoyment, and on the contrary a tolerable share of vexation of spirit. Everything, however, shall bend to the pleasure of grubbing up old bones, and captivating new animals. By the way, you rank my Natural History labours far too high. I am nothing more than a lions' provider: I do not feel at all sure that they will not growl and finally destroy me.

It does one's heart good to hear how things are going on in England. Hurrah for the honest Whigs! I trust they will soon attack that monstrous stain on our boasted liberty, Colonial Slavery. I have seen enough of slavery and the dispositions of the negroes, to be thoroughly disgusted with the lies and nonsense one hears on the subject in England. Thank God, the cold-hearted Tories, who, as J. Mackintosh used to say, have no enthusiasm, except against enthusiasm, have for the present run their race. I am sorry, by your letter, to hear you have not been well, and that you partly attribute it to want of exercise. I wish you were here amongst the green plains; we would take walks which would rival the Dolgelly ones, and you should tell stories, which I would believe, even to a *cubic fathom of pudding*. Instead, I must take my solitary ramble, think of Cambridge days, and pick up snakes, beetles and toads. Excuse this short letter (you know I never studied 'The Complete Letter-writer'), and believe me, my dear Herbert,

Your affectionate friend,
CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

East Falkland Island, March, 1834.

. . . . . I am quite charmed with Geology, but, like the wise animal between two bundles of hay, I do not know which to like the best; the old crystalline group of rocks, or the softer and fossiliferous beds. When puzzling about stratification, &c., I feel inclined to cry "a fig for your big oysters, and your bigger megatheriums." But then when digging out some fine bones, I wonder how any man can tire his arms with hammering granite. By the way I have not one clear idea about cleavage, stratification, lines of upheaval. I have no books which tell me much, and what they do I cannot apply to what I see. In consequence I draw my own conclusions, and most gloriously ridiculous ones they are, I sometimes fancy. . . . Can you throw any light into my mind by telling me what relation cleavage and planes of deposition bear to each other?

And now for my second *section*, Zoology. I have chiefly been employed in preparing myself for the South Sea by examining the polypi of the smaller Corallines in these latitudes. Many in themselves are very curious, and I think are quite undescribed; there was one appalling one, allied to a *Flustra*, which I dare say I mentioned having found to the northward, where the cells have a movable organ (like a vulture's head, with a dilatable beak), fixed on the edge. But what is of more general interest is the unquestionable (as it appears to me) existence of another species of ostrich, besides the *Struthio rhea*. All the Gauchos and Indians state it is the case, and I place the greatest faith in their observations. I have the head, neck, piece of skin, feathers, and legs of one. The differences are chiefly in the colour of the feathers and scales on legs, being feathered below the knees, nidification, and geographical distribution. So much for what I have

© The Complete Work of Charles Darwin Online

lately done; the prospect before me is full of sunshine, fine weather, glorious scenery, the geology of the Andes, plains abounding with organic remains (which perhaps I may have the good luck to catch in the very act of moving), and lastly, an ocean, its shores abounding with life, so that, if nothing unforeseen happens, I will stick to the voyage, although for what I can see this may last till we return a fine set of white-headed old gentlemen. I have to thank you most cordially for sending me the books. I am now reading the Oxford 'Report;' * the whole account of your proceedings is most glorious; you remaining in England cannot well imagine how excessively interesting I find the reports. I am sure from my own thrilling sensations when reading them, that they cannot fail to have an excellent effect upon all those residing in distant colonies, and who have little opportunity of seeing the periodicals. My hammer has flown with redoubled force on the devoted blocks; as I thought over the eloquence of the Cambridge President, I hit harder and harder blows. I hope to give my arms strength for the Cordilleras. You will send me through Capt. Beaufort a copy of the Cambridge 'Report.'

I have forgotten to mention that for some time past, and for the future, I will put a pencil cross on the pill-boxes containing insects, as these alone will require being kept particularly dry; it may perhaps save you some trouble. When this letter will go I do not know, as this little seat of discord has lately been embroiled by a dreadful scene of murder, and at present there are more prisoners than inhabitants. If a merchant vessel is chartered to take them to Rio, I will send some specimens (especially my few plants and seeds). Remember me to all my Cambridge friends. I love and treasure up every recollection of dear old Cambridge. I am much

* The second meeting of the British Association was held at Oxford in 1832, the following year it was at Cambridge.

© The Complete Work of Charles Darwin Online

obliged to you for putting my name down to poor Ramsay's monument; I never think of him without the warmest admiration. Farewell, my dear Henslow.

Believe me your most obliged and affectionate friend,

CHARLES DARWIN.

C. Darwin to Miss C. Darwin.

East Falkland Island, April 6, 1834.

MY DEAR CATHERINE,

When this letter will reach you I know not, but probably some man-of-war will call here before, in the common course of events, I should have another opportunity of writing.

* * * * *

After visiting some of the southern islands, we beat up through the magnificent scenery of the Beagle Channel to Jemmy Button's * country. We could hardly recognise poor Jemmy. Instead of the clean, well-dressed stout lad we left him, we found him a naked, thin, squalid savage. York and Fuegia had moved to their own country some months ago, the former having stolen all Jemmy's clothes. Now he had nothing except a bit of blanket round his waist. Poor Jemmy was very glad to see us, and, with his usual good feeling, brought several presents (otter-skins, which are most valuable to themselves) for his old friends. The Captain offered to take him to England, but this, to our surprise, he at once refused. In the evening his young wife came alongside and showed us the reason. He was quite contented. Last year, in the height of his indignation, he said "his country people no *sabe* nothing—damned fools"—now they were very good people, with *too* much to eat, and all the

* Jemmy Button, York Minster, and Fuegia Basket, were natives of Tierra del Fuego, brought to England by Captain Fitz-Roy in his former voyage, and restored to their country by him in 1832.

© The Complete Work of Charles Darwin Online

luxuries of life. Jemmy and his wife paddled away in their canoe loaded with presents, and very happy. The most curious thing is, that Jemmy, instead of recovering his own language, has taught all his friends a little English. "J. Button's canoe" and "Jemmy's wife come," "Give me knife," &c., was said by several of them.

We then bore away for this island—this little miserable seat of discord. We found that the Gauchos, under pretence of a revolution, had murdered and plundered all the Englishmen whom they could catch, and some of their own countrymen. All the economy at home makes the foreign movements of England most contemptible. How different from old Spain. Here we, dog-in-the-manger fashion, seize an island, and leave to protect it a Union Jack; the possessor has, of course, been murdered; we now send a lieutenant with four sailors, without authority or instructions. A man-of-war, however, ventured to leave a party of marines, and by their assistance, and the treachery of some of the party, the murderers have all been taken, there being now as many prisoners as inhabitants. This island must some day become a very important halting-place in the most turbulent sea in the world. It is mid-way between Australia and the South Sea to England; between Chili, Peru, &c., and the Rio Plata and the Rio de Janeiro. There are fine harbours, plenty of fresh water, and good beef. It would doubtless produce the coarser vegetables. In other respects it is a wretched place. A little time since, I rode across the island, and returned in four days. My excursion would have been longer, but during the whole time it blew a gale of wind, with hail and snow. There is no fire-wood bigger than heath, and the whole country is, more or less, an elastic peat-bog. Sleeping out at night was too miserable work to endure it for all the rocks in South America.

We shall leave this scene of iniquity in two or three days, and go to the Rio de la Sta. Cruz. One of the objects is to

© The Complete Work of Charles Darwin Online

look at the ship's bottom. We struck rather heavily on an unknown rock off Port Desire, and some of her copper is torn off. After this is repaired the Captain has a glorious scheme; it is to go to the very head of this river, that is probably to the Andes. It is quite unknown; the Indians tell us it is two or three hundred yards broad, and horses can nowhere ford it. I cannot imagine anything more interesting. Our plans then are to go to Port Famine, and there we meet the *Adventure*, who is employed in making the Chart of the Falklands. This will be in the middle of winter, so I shall see Tierra del Fuego in her white drapery. We leave the straits to enter the Pacific by the Barbara Channel, one very little known, and which passes close to the foot of Mount Sarmiento (the highest mountain in the south, excepting Mt.!! Darwin!!). We then shall scud away for Concepcion in Chili. I believe the ship must once again steer southward, but if any one catches me there again, I will give him leave to hang me up as a scarecrow for all future naturalists. I long to be at work in the Cordilleras, the geology of this side, which I understand pretty well is so intimately connected with periods of violence in that great chain of mountains. The future is, indeed, to me a brilliant prospect. You say its very brilliancy frightens you; but really I am very careful; I may mention as a proof, in all my rambles I have never had any one accident or scrape. . . . Continue in your good custom of writing plenty of gossip; I much like hearing all about all things. Remember me most kindly to Uncle Jos, and to all the Wedgwoods. Tell Charlotte (their married names sound downright unnatural) I should like to have written to her, to have told her how well everything is going on; but it would only have been a transcript of this letter, and I have a host of animals at this minute surrounding me which all require embalming and numbering. I have not forgotten the comfort I received that day at Maer, when my mind was like a swinging pendulum. Give my best love to my father. I hope he will forgive all my extrava-

© The Complete Work of Charles Darwin Online

gance, but not as a Christian, for then I suppose he would send me no more money.

Good-bye, dear, to you, and all your goodly sisterhood.

Your affectionate brother,

CHAS. DARWIN.

My love to Nancy; * tell her, if she was now to see me with my great beard, she would think I was some worthy Solomon, come to sell the trinkets.

C. Darwin to C. Whitley.

Valparaiso, July 23, 1834.

MY DEAR WHITLEY,

I have long intended writing, just to put you in mind that there is a certain hunter of beetles, and pounder of rocks, still in existence. Why I have not done so before I know not, but it will serve me right if you have quite forgotten me. It is a very long time since I have heard any Cambridge news; I neither know where you are living or what you are doing. I saw your name down as one of the indefatigable guardians of the eighteen hundred philosophers. I was delighted to see this, for when we last left Cambridge you were at sad variance with poor science; you seemed to think her a public prostitute working for popularity. If your opinions are the same as formerly, you would agree most admirably with Captain Fitz-Roy,—the object of his most devout abhorrence is one of the d—d scientific Whigs. As captains of men-of-war are the greatest men going, far greater than kings or schoolmasters, I am obliged to tell him everything in my own favour. I have often said I once had a very good friend, an out-and-out Tory, and we managed to get on very well together. But he is very much inclined to doubt if ever I really was so much honoured; at present we hear scarcely anything about politics; this saves a great deal

* His old nurse.

© The Complete Work of Charles Darwin Online

of trouble, for we all stick to our former opinions rather more obstinately than before, and can give rather fewer reasons for doing so.

I do hope you will write to me: ('H.M.S. *Beagle*, S. American Station' will find me). I should much like to hear in what state you are both in body and mind. *¿ Quién sabe?* as the people say here (and God knows they well may, for they do know little enough), if you are not a married man, and may be nursing, as Miss Austen says, little olive branches, little pledges of mutual affection. Eheu! Eheu! this puts me in mind of former visions of glimpses into futurity, where I fancied I saw retirement, green cottages, and white petticoats. What will become of me hereafter I know not; I feel like a ruined man, who does not see or care how to extricate himself. That this voyage must come to a conclusion my reason tells me, but otherwise I see no end to it. It is impossible not bitterly to regret the friends and other sources of pleasure one leaves behind in England; in place of it there is much solid enjoyment, some present, but more in anticipation, when the ideas gained during the voyage can be compared to fresh ones. I find in Geology a never-failing interest, as it has been remarked, it creates the same grand ideas respecting this world which Astronomy does for the universe. We have seen much fine scenery; that of the Tropics in its glory and luxuriance exceeds even the language of Humboldt to describe. A Persian writer could alone do justice to it, and if he succeeded he would in England be called the 'Grandfather of all liars.'

But I have seen nothing which more completely astonished me than the first sight of a savage. It was a naked Fuegian, his long hair blowing about, his face besmeared with paint. There is in their countenances an expression which I believe, to those who have not seen it, must be inconceivably wild. Standing on a rock he uttered tones and

© The Complete Work of Charles Darwin Online

made gesticulations, than which the cries of domestic animals are far more intelligible.

When I return to England, you must take me in hand with respect to the fine arts. I yet recollect there was a man called Raffaelle Sanctus. How delightful it will be once again to see, in the Fitzwilliam, Titian's Venus. How much more then delightful to go to some good concert or fine opera. These recollections will not do. I shall not be able to-morrow to pick out the entrails of some small animal with half my usual gusto. Pray tell me some news about Cameron, Watkins, Marinden, the two Thompsons of Trinity, Lowe, Heaviside, Matthew. Herbert I have heard from. How is Henslow getting on? and all other good friends of dear Cambridge? Often and often do I think over those past hours, so many of which have been passed in your company. Such can never return, but their recollection can never die away.

God bless you, my dear Whitley,

Believe me, your most sincere friend,

CHAS. DARWIN.

C. Darwin to Miss C. Darwin.

Valparaiso, November 8, 1834.

MY DEAR CATHERINE,

My last letter was rather a gloomy one, for I was not very well when I wrote it. Now everything is as bright as sunshine. I am quite well again after being a second time in bed for a fortnight. Captain Fitz-Roy very generously has delayed the ship ten days on my account, and without at the time telling me for what reason.

We have had some strange proceedings on board the *Beagle*, but which have ended most capitally for all hands. Captain Fitz-Roy has for the last two months been working *extremely* hard, and at the same time constantly annoyed by

© The Complete Work of Charles Darwin Online

interruptions from officers of other ships; the selling the schooner and its consequences were very vexatious; the cold manner the Admiralty (solely I believe because he is a Tory) have treated him, and a thousand other, &c. &c.'s, has made him very thin and unwell. This was accompanied by a morbid depression of spirits, and a loss of all decision and resolution. . . . All that Bynoe (the surgeon) could say, that it was merely the effect of bodily health and exhaustion after such application, would not do; he invalided, and Wickham was appointed to the command. By the instructions Wickham could only finish the survey of the southern part, and would then have been obliged to return direct to England. The grief on board the *Beagle* about the Captain's decision was universal and deeply felt; one great source of his annoyment was the feeling it impossible to fulfil the whole instructions; from his state of mind it never occurred to him that the very instructions order him to do as much of the West coast *as he has time for*, and then proceed across the Pacific.

Wickham (very disinterestedly giving up his own promotion) urged this most strongly, stating that when he took the command nothing should induce him to go to Tierra del Fuego again; and then asked the Captain what would be gained by his resignation? why not do the more useful part, and return as commanded by the Pacific. The Captain at last, to every one's joy, consented, and the resignation was withdrawn.

Hurrah! hurrah! it is fixed the *Beagle* shall not go one mile south of Cape Tres Montes (about 200 miles south of Chiloe), and from that point to Valparaiso will be finished in about five months. We shall examine the Chonos Archipelago, entirely unknown, and the curious inland sea behind Chiloe. For me it is glorious. Cape Tres Montes is the most southern point where there is much geological interest, as there the modern beds end. The Captain then talks of crossing the Pacific; but I think we shall persuade him to finish the Coast of Peru, where the climate is delightful, the country hideously

© The Complete Work of Charles Darwin Online

sterile, but abounding with the highest interest to a geologist. For the first time since leaving England I now see a clear and not so distant prospect of returning to you all: crossing the Pacific, and from Sydney home, will not take much time.

As soon as the Captain invalided I at once determined to leave the *Beagle*, but it was quite absurd what a revolution in five minutes was effected in all my feelings. I have long been grieved and most sorry at the interminable length of the voyage (although I never would have quitted it); but the minute it was all over, I could not make up my mind to return. I could not give up all the geological castles in the air which I had been building up for the last two years. One whole night I tried to think over the pleasure of seeing Shrewsbury again, but the barren plains of Peru gained the day. I made the following scheme (I know you will abuse me, and perhaps if I had put it in execution, my father would have sent a mandamus after me); it was to examine the Cordilleras of Chili during this summer, and in the winter go from port to port on the coast of Peru to Lima, returning this time next year to Valparaiso, cross the Cordilleras to Buenos Ayres, and take ship to England. Would not this have been a fine excursion, and in sixteen months I should have been with you all? To have endured Tierra del Fuego and not seen the Pacific would have been miserable. . . .

I go on board to-morrow; I have been for the last six weeks in Corfield's house. You cannot imagine what a kind friend I have found him. He is universally liked, and respected by the natives and foreigners. Several Chileno Signoritas are very obligingly anxious to become the signoras of this house. Tell my father I have kept my promise of being extravagant in Chili. I have drawn a bill of £100 (had it not better be notified to Messrs. Robarts & Co.); £50 goes to the Captain for the ensuing year, and £30 I take to sea for the small ports; so that *bonâ fide* I have not spent £180 during these last four months. I hope not to draw another bill for six

© The Complete Work of Charles Darwin Online

months. All the foregoing particulars were only settled yesterday. It has done me more good than a pint of medicine and I have not been so happy for the last year. If it had not been for my illness, these four months in Chili would have been very pleasant. I have had ill luck, however, in only one little earthquake having happened. I was lying in bed when there was a party at dinner in the house ; on a sudden I heard such a hubbub in the dining-room ; without a word being spoken, it was devil take the hindmost who should get out first ; at the same moment I felt my bed *slightly* vibrate in a lateral direction. The party were old stagers, and heard the noise which always precedes a shock ; and no old stager looks at an earthquake with philosophical eyes. . . .

Good-bye to you all ; you will not have another letter for some time.

My dear Catherine,
Yours affectionately,
CHAS. DARWIN.

My best love to my father, and all of you. Love to Nancy.

C. Darwin to Miss S. Darwin.

Valparaiso, April 23, 1835.

MY DEAR SUSAN,

I received, a few days since, your letter of November ; the three letters which I before mentioned are yet missing, but I do not doubt they will come to life. I returned a week ago from my excursion across the Andes to Mendoza. Since leaving England I have never made so successful a journey ; it has, however, been very expensive. I am sure my father would not regret it, if he could know how deeply I have enjoyed it : it was something more than enjoyment ; I cannot express the delight which I felt at such a famous winding-up of all my geology in South America. I literally could hardly sleep at nights for thinking over my day's

© The Complete Work of Charles Darwin Online

work. The scenery was so new, and so majestic; everything at an elevation of 12,000 feet bears so different an aspect from that in a lower country. I have seen many views more beautiful, but none with so strongly marked a character. To a geologist, also, there are such manifest proofs of excessive violence; the strata of the highest pinnacles are tossed about like the crust of a broken pie.

I crossed by the Portillo Pass, which at this time of the year is apt to be dangerous, so could not afford to delay there. After staying a day in the stupid town of Mendoza, I began my return by Uspallate, which I did very leisurely. My whole trip only took up twenty-two days. I travelled with, for me, uncommon comfort, as I carried a *bed!* My party consisted of two Peons and ten mules, two of which were with baggage, or rather food, in case of being snowed up. Everything, however, favoured me; not even a speck of this year's snow had fallen on the road. I do not suppose any of you can be much interested in geological details, but I will just mention my principal results:—Besides understanding to a certain extent the description and manner of the force which has elevated this great line of mountains, I can clearly demonstrate that one part of the double line is of an age long posterior to the other. In the more ancient line, which is the true chain of the Andes, I can describe the sort and order of the rocks which compose it. These are chiefly remarkable by containing a bed of gypsum nearly 2000 feet thick—a quantity of this substance I should think unparalleled in the world. What is of much greater consequence, I have procured fossil shells (from an elevation of 12,000 feet). I think an examination of these will give an approximate age to these mountains, as compared to the strata of Europe. In the other line of the Cordilleras there is a strong presumption (in my own mind, conviction) that the enormous mass of mountains, the peaks of which rise to 13,000 and 14,000 feet, are so very modern as to be con-

© The Complete Work of Charles Darwin Online

temporaneous with the plains of Patagonia (or about with the *upper* strata of the Isle of Wight). If this result shall be considered as proved,* it is a very important fact in the theory of the formation of the world; because, if such wonderful changes have taken place so recently in the crust of the globe, there can be no reason for supposing former epochs of excessive violence. These modern strata are very remarkable by being threaded with metallic veins of silver, gold, copper, &c.; hitherto these have been considered as appertaining to older formations. In these same beds, and close to a gold-mine, I found a clump of petrified trees, standing upright, with layers of fine sandstone deposited round them, bearing the impression of their bark. These trees are covered by other sandstones and streams of lava to the thickness of several thousand feet. These rocks have been deposited beneath water; yet it is clear the spot where the trees grew must once have been above the level of the sea, so that it is certain the land must have been depressed by at least as many thousand feet as the superincumbent subaqueous deposits are thick. But I am afraid you will tell me I am prosy with my geological descriptions and theories. . . .

Your account of Erasmus' visit to Cambridge has made me long to be back there. I cannot fancy anything more delightful than his Sunday round of King's, Trinity, and those talking giants, Whewell and Sedgwick; I hope your musical tastes continue in due force. I shall be ravenous for the pianoforte. . . .

I have not quite determined whether I will sleep at the 'Lion' the first night when I arrive per 'Wonder,' or disturb you all in the dead of the night; everything short of that is absolutely planned. Everything about Shrewsbury is growing in my mind bigger and more beautiful; I am certain the

* The importance of these results has been fully recognized by geologists.

© The Complete Work of Charles Darwin Online

acacia and copper beech are two superb trees; I shall know every bush, and I will trouble you young ladies, when each of you cut down your tree, to spare a few. As for the view behind the house, I have seen nothing like it. It is the same with North Wales; Snowdon, to my mind, looks much higher and much more beautiful than any peak in the Cordilleras. So you will say, with my benighted faculties, it is time to return, and so it is, and I long to be with you. Whatever the trees are, I know what I shall find all you. I am writing nonsense, so farewell. My most affectionate love to all, and I pray forgiveness from my father.

Yours most affectionately,
CHARLES DARWIN.

C. Darwin to W. D. Fox.

Lima, July, 1835.

MY DEAR FOX,

I have lately received two of your letters, one dated June and the other November 1834 (they reached me, however, in an inverted order). I was very glad to receive a history of this most important year in your life. Previously I had only heard the plain fact that you were married. You are a true Christian and return good for evil, to send two such letters to so bad a correspondent as I have been. God bless you for writing so kindly and affectionately; if it is a pleasure to have friends in England, it is doubly so to think and know that one is not forgotten, because absent. This voyage is terribly long. I do so earnestly desire to return, yet I dare hardly look forward to the future, for I do not know what will become of me. Your situation is above envy: I do not venture even to frame such happy visions. To a person fit to take the office, the life of a clergyman is a type of all that is respectable and happy. You tempt me by talking of your fireside, whereas it is a sort of scene I never ought to think

© The Complete Work of Charles Darwin Online

about. I saw the other day a vessel sail for England; it was quite dangerous to know how easily I might turn deserter. As for an English lady, I have almost forgotten what she is—something very angelic and good. As for the women in these countries, they wear caps and petticoats, and a very few have pretty faces, and then all is said. But if we are not wrecked on some unlucky reef, I will sit by that same fireside in Vale Cottage and tell some of the wonderful stories, which you seem to anticipate and, I presume, are not very ready to believe. *Gracias a dios*, the prospect of such times is rather shorter than formerly.

From this most wretched 'City of the Kings' we sail in a fortnight, from thence to Guayaquil, Galapagos, Marquesas, Society Islands, &c., &c. I look forward to the Galapagos with more interest than any other part of the voyage. They abound with active volcanoes, and, I should hope, contain Tertiary strata. I am glad to hear you have some thoughts of beginning Geology. I hope you will; there is so much larger a field for thought than in the other branches of Natural History. I am become a zealous disciple of Mr. Lyell's views, as known in his admirable book. Geologising in South America, I am tempted to carry parts to a greater extent even than he does. Geology is a capital science to begin, as it requires nothing but a little reading, thinking, and hammering. I have a considerable body of notes together; but it is a constant subject of perplexity to me, whether they are of sufficient value for all the time I have spent about them, or whether animals would not have been of more certain value.

I shall indeed be glad once again to see you and tell you how grateful I feel for your steady friendship. God bless you, my very dear Fox.

Believe me,

Yours affectionately,

CHAS. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

Sydney, January, 1836.

MY DEAR HENSLOW,

This is the last opportunity of communicating with you before that joyful day when I shall reach Cambridge. I have very little to say: but I must write if it is only to express my joy that the last year is concluded, and that the present one, in which the *Beagle* will return, is gliding onwards. We have all been disappointed here in not finding even a single letter; we are, indeed, rather before our expected time, otherwise I dare say, I should have seen your handwriting. I must feed upon the future, and it is beyond bounds delightful to feel the certainty that within eight months I shall be residing once again most quietly in Cambridge. Certainly, I never was intended for a traveller; my thoughts are always rambling over past or future scenes; I cannot enjoy the present happiness for anticipating the future, which is about as foolish as the dog who dropped the real bone for its shadow.

* * * * *

In our passage across the Pacific we only touched at Tahiti and New Zealand; at neither of these places or at sea had I much opportunity of working. Tahiti is a most charming spot. Everything which former navigators have written is true. 'A new Cytheræa has risen from the ocean.' Delicious scenery, climate, manners of the people are all in harmony. It is, moreover, admirable to behold what the missionaries both here and at New Zealand have effected. I firmly believe they are good men working for the sake of a good cause. I much suspect that those who have abused or sneered at the missionaries, have generally been such as were not very anxious to find the natives moral and intelligent beings. During the remainder of our voyage we shall only visit places generally acknowledged as civilised,

© The Complete Work of Charles Darwin Online

and nearly all under the British flag. These will be a poor field for Natural History, and without it I have lately discovered that the pleasure of seeing new places is as nothing. I must return to my old resource and think of the future, but that I may not become more prosy, I will say farewell till the day arrives, when I shall see my Master in Natural History, and can tell him how grateful I feel for his kindness and friendship.

Believe me, dear Henslow,
Ever yours, most faithfully,
CHAS. DARWIN.

C. Darwin to Miss S. Darwin.

Bahia, Brazil, August 4 [1836].

MY DEAR SUSAN,

I will just write a few lines to explain the cause of this letter being dated on the coast of South America. Some singular disagreements in the longitudes made Captain Fitz-Roy anxious to complete the circle in the southern hemisphere, and then retrace our steps by our first line to England. This zigzag manner of proceeding is very grievous; it has put the finishing stroke to my feelings. I loathe, I abhor the sea and all ships which sail on it. But I yet believe we shall reach England in the latter half of October. At Ascension I received Catherine's letter of October, and yours of November; the letter at the Cape was of a later date, but letters of all sorts are inestimable treasures, and I thank you both for them. The desert, volcanic rocks, and wild sea of Ascension, as soon as I knew there was news from home, suddenly wore a pleasing aspect, and I set to work with a good-will at my old work of Geology. You would be surprised to know how entirely the pleasure in arriving at a new place depends on letters. We only stayed four days at Ascension, and then made a very good passage to Bahia.

© The Complete Work of Charles Darwin Online

I little thought to have put my foot on South American coast again. It has been almost painful to find how much good enthusiasm has been evaporated during the last four years. I can now walk soberly through a Brazilian forest; not but what it is exquisitely beautiful, but now, instead of seeking for splendid contrasts, I compare the stately mango trees with the horse-chestnuts of England. Although this zigzag has lost us at least a fortnight, in some respects I am glad of it. I think I shall be able to carry away one vivid picture of inter-tropical scenery. We go from hence to the Cape de Verds; that is, if the winds or the Equatorial calms will allow us. I have some faint hopes that a steady foul wind might induce the Captain to proceed direct to the Azores. For which most untoward event I heartily pray.

Both your letters were full of good news; especially the expressions which you tell me Professor Sedgwick used about my collections. I confess they are deeply gratifying—I trust one part at least will turn out true, and that I shall act as I now think—as a man who dares to waste one hour of time has not discovered the value of life. Professor Sedgwick mentioning my name at all gives me hopes that he will assist me with his advice, of which, in my geological questions, I stand much in need. It is useless to tell you from the shameful state of this scribble that I am writing against time, having been out all morning, and now there are some strangers on board to whom I must go down and talk civility. Moreover, as this letter goes by a foreign ship, it is doubtful whether it will ever arrive. Farewell, my very dear Susan and all of you. Good-bye,

C. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

St. Helena, July 9, 1836.

MY DEAR HENSLOW,

I am going to ask you to do me a favour. I am very anxious to belong to the Geological Society. I do not know, but I suppose it is necessary to be proposed some time before being ballotted for; if such is the case, would you be good enough to take the proper preparatory steps? Professor Sedgwick very kindly offered to propose me before leaving England, if he should happen to be in London. I dare say he would yet do so.

I have very little to write about. We have neither seen, done, or heard of anything particular for a long time past; and indeed if at present the wonders of another planet could be displayed before us, I believe we should unanimously exclaim, what a consummate plague. No schoolboys ever sung the half sentimental and half jovial strain of 'dulce domum' with more fervour, than we all feel inclined to do. But the whole subject of 'dulce domum,' and the delight of seeing one's friends, is most dangerous, it must infallibly make one very prosy or very boisterous. Oh, the degree to which I long to be once again living quietly with not one single novel object near me! No one can imagine it till he has been whirled round the world during five long years in a ten-gun-brig. I am at present living in a small house (amongst the clouds) in the centre of the island, and within stone's throw of Napoleon's tomb. It is blowing a gale of wind with heavy rain and wretchedly cold; if Napoleon's ghost haunts his dreary place of confinement, this would be a most excellent night for such wandering spirits. If the weather chooses to permit me, I hope to see a little of the Geology (so often partially described) of the island. I suspect that differently from most volcanic islands its structure is rather complicated. It seems strange that this little centre of a

© The Complete Work of Charles Darwin Online

distinct creation should, as is asserted, bear marks of recent elevation.

The *Beagle* proceeds from this place to Ascension, then to the Cape de Verds (what miserable places!) to the Azores to Plymouth, and then to home. That most glorious of all days in my life will not, however, arrive till the middle of October. Some time in that month you will see me at Cambridge, where I must directly come to report myself to you, as my first Lord of the Admiralty. At the Cape of Good Hope we all on board suffered a bitter disappointment in missing nine months' letters, which are chasing us from one side of the globe to the other. I dare say amongst them there was a letter from you; it is long since I have seen your hand-writing, but I shall soon see you yourself, which is far better. As I am your pupil, you are bound to undertake the task of criticising and scolding me for all the things ill done and not done at all, which I fear I shall need much; but I hope for the best, and I am sure I have a good if not too easy taskmaster.

At the Cape Captain Fitz-Roy and myself enjoyed a memorable piece of good fortune in meeting Sir J. Herschel. We dined at his house and saw him a few times besides. He was exceedingly good-natured, but his manners at first appeared to me rather awful. He is living in a very comfortable country house, surrounded by fir and oak trees, which alone in so open a country, give a most charming air of seclusion and comfort. He appears to find time for everything; he showed us a pretty garden full of Cape bulbs of his own collecting, and I afterwards understood that everything was the work of his own hands. . . . I am very stupid, and I have nothing more to say; the wind is whistling so mournfully over the bleak hills, that I shall go to bed and dream of England.

Good night, my dear Henslow,

Yours most truly obliged and affectionately,

CHAS. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

Shrewsbury, Thursday, October 6 [1836].

MY DEAR HENSLOW,

I am sure you will congratulate me on the delight of once again being home. The *Beagle* arrived at Falmouth on Sunday evening, and I reached Shrewsbury yesterday morning. I am exceedingly anxious to see you, and as it will be necessary in four or five days to return to London to get my goods and chattels out of the *Beagle*, it appears to me my best plan to pass through Cambridge. I want your advice on many points; indeed I am in the clouds, and neither know what to do or where to go. My chief puzzle is about the geological specimens—who will have the charity to help me in describing their mineralogical nature? Will you be kind enough to write to me one line by *return of post*, saying whether you are now at Cambridge? I am doubtful till I hear from Captain Fitz-Roy whether I shall not be obliged to start before the answer can arrive, but pray try the chance. My dear Henslow, I do long to see you; you have been the kindest friend to me that ever man possessed. I can write no more, for I am giddy with joy and confusion.

Farewell for the present,

Yours most truly obliged,

CHARLES DARWIN.

C. Darwin to R. Fitz-Roy.

Shrewsbury, Thursday morning, October 6 [1836].

MY DEAR FITZ-ROY,

I arrived here yesterday morning at breakfast-time, and, thank God, found all my dear good sisters and father quite well. My father appears more cheerful and very little older than when I left. My sisters assure me I do not look the least different, and I am able to return the compliment.

© The Complete Work of Charles Darwin Online

Indeed, all England appears changed excepting the good old town of Shrewsbury and its inhabitants, which, for all I can see to the contrary, may go on as they now are to Doomsday. I wish with all my heart I was writing to you amongst your friends instead of at that horrid Plymouth. But the day will soon come, and you will be as happy as I now am. I do assure you I am a very great man at home; the five years' voyage has certainly raised me a hundred per cent. I fear such greatness must experience a fall.

I am thoroughly ashamed of myself in what a dead-and-half-alive state I spent the few last days on board; my only excuse is that certainly I was not quite well. The first day in the mail tired me, but as I drew nearer to Shrewsbury everything looked more beautiful and cheerful. In passing Gloucestershire and Worcestershire I wished much for you to admire the fields, woods, and orchards. The stupid people on the coach did not seem to think the fields one bit greener than usual; but I am sure we should have thoroughly agreed that the wide world does not contain so happy a prospect as the rich cultivated land of England.

I hope you will not forget to send me a note telling me how you go on. I do indeed hope all your vexations and trouble with respect to our voyage, which we now know HAS an end, have come to a close. If you do not receive much satisfaction for all the mental and bodily energy you have expended in His Majesty's service, you will be most hardly treated. I put my radical sisters into an uproar at some of the prudent (if they were not honest Whigs, I would say shabby) proceedings of our Government. By the way, I must tell you for the honour and glory of the family that my father has a large engraving of King George IV. put up in his sitting-room. But I am no renegade, and by the time we meet my politics will be as firmly fixed and as wisely founded as ever they were.

I thought when I began this letter I would convince you

© The Complete Work of Charles Darwin Online

what a steady and sober frame of mind I was in. But I find I am writing most precious nonsense. Two or three of our labourers yesterday immediately set to work, and got most excessively drunk in honour of the arrival of Master Charles. Who then shall gainsay if Master Charles himself chooses to make himself a fool. Good-bye. God bless you! I hope you are as happy, but much wiser, than your most sincere but unworthy philosopher,

CHAS. DARWIN.

© The Complete Work of Charles Darwin Online

CHAPTER VII.

LONDON AND CAMBRIDGE.

1836–1842.

[THE period illustrated by the following letters includes the years between my father's return from the voyage of the *Beagle* and his settling at Down. It is marked by the gradual appearance of that weakness of health which ultimately forced him to leave London and take up his abode for the rest of his life in a quiet country house. In June 1841 he writes to Lyell: "My father scarcely seems to expect that I shall become strong for some years; it has been a bitter mortification for me to digest the conclusion that the 'race is for the strong,' and that I shall probably do little more, but be content to admire the strides others make in science."

There is no evidence of any intention of entering a profession after his return from the voyage, and early in 1840 he wrote to Fitz-Roy: "I have nothing to wish for, excepting stronger health to go on with the subjects to which I have joyfully determined to devote my life."

These two conditions—permanent ill-health and a passionate love of scientific work for its own sake—determined thus early in his career, the character of his whole future life. They impelled him to lead a retired life of constant labour, carried on to the utmost limits of his physical power, a life which signally falsified his melancholy prophecy.

The end of the last chapter saw my father safely arrived at Shrewsbury on October 4, 1836, "after an absence of five

© The Complete Work of Charles Darwin Online

years and two days." He wrote to Fox: "You cannot imagine how gloriously delightful my first visit was at home; it was worth the banishment." But it was a pleasure that he could not long enjoy, for in the last days of October he was at Greenwich unpacking specimens from the *Beagle*. As to the destination of the collections he writes, somewhat despondingly, to Henslow:—

"I have not made much progress with the great men. I find, as you told me, that they are all overwhelmed with their own business. Mr. Lyell has entered, in the *most* good-natured manner, and almost without being asked, into all my plans. He tells me, however, the same story, that I must do all myself. Mr. Owen seems anxious to dissect some of the animals in spirits, and, besides these two, I have scarcely met any one who seems to wish to possess any of my specimens. I must except Dr. Grant, who is willing to examine some of the corallines. I see it is quite unreasonable to hope for a minute that any man will undertake the examination of a whole order. It is clear the collectors so much outnumber the real naturalists that the latter have no time to spare.

"I do not even find that the Collections care for receiving the unnamed specimens. The Zoological Museum * is nearly full, and upwards of a thousand specimens remain unmounted. I dare say the British Museum would receive them, but I cannot feel, from all I hear, any great respect even for the present state of that establishment. Your plan will be not only the best, but the only one, namely, to come down to Cambridge, arrange and group together the different families, and then wait till people, who are already working in different branches, may want specimens. But it appears to me [that] to do this it will be almost necessary to reside in London. As far as I can yet see my best plan will be to spend several

* The Museum of the Zoological Society, then at 33 Bruton Street. The collection was some years later broken up and dispersed.

© The Complete Work of Charles Darwin Online

months in Cambridge, and then when, by your assistance, I know on what ground I stand, to emigrate to London, where I can complete my Geology and try to push on the Zoology. I assure you I grieve to find how many things make me see the necessity of living for some time in this dirty, odious London. For even in Geology I suspect much assistance and communication will be necessary in this quarter, for instance, in fossil bones, of which none excepting the fragments of Megatherium have been looked at, and I clearly see that without my presence they never would be. . . .

"I only wish I had known the Botanists cared so much for specimens * and the Zoologists so little; the proportional number of specimens in the two branches should have had a very different appearance. I am out of patience with the Zoologists, not because they are overworked, but for their mean, quarrelsome spirit. I went the other evening to the Zoological Society, where the speakers were snarling at each other in a manner anything but like that of gentlemen. Thank Heavens! as long as I remain in Cambridge there will not be any danger of falling into any such contemptible quarrels, whilst in London I do not see how it is to be avoided. Of the Naturalists, F. Hope is out of London; Westwood I have not seen, so about my insects I know nothing. I have seen Mr. Yarrell twice, but he is so evidently oppressed with business that it is too selfish to plague him with my concerns. He has asked me to dine with the Linnean on Tuesday, and on Wednesday I dine with the Geological, so that I shall see all the great men. Mr. Bell,

* A passage in a subsequent letter shows that his plants also gave him some anxiety. "I met Mr. Brown a few days after you had called on him; he asked me in rather an ominous manner what I meant to do with my plants. In the course of conversation Mr. Broderip, who was present, remarked to him, 'You forget how long it is since Captain King's expedition.' He answered, 'Indeed, I have something in the shape of Captain King's undescribed plants to make me recollect it.' Could a better reason be given, if I had been asked, by me, for not giving the plants to the British Museum?"

© The Complete Work of Charles Darwin Online

I hear, is so much occupied that there is no chance of his wishing for specimens of reptiles. I have forgotten to mention Mr. Lonsdale,* who gave me a most cordial reception, and with whom I had much most interesting conversation. If I was not much more inclined for geology than the other branches of Natural History, I am sure Mr. Lyell's and Lonsdale's kindness ought to fix me. You cannot conceive anything more thoroughly good-natured than the heart-and-soul manner in which he put himself in my place and thought what would be best to do. At first he was all for London versus Cambridge, but at last I made him confess that, for some time at least, the latter would be for me much the best. There is not another soul whom I could ask, excepting yourself, to wade through and criticise some of those papers which I have left with you. Mr. Lyell owned that, second to London, there was no place in England so good for a Naturalist as Cambridge. Upon my word I am ashamed of writing so many foolish details; no young lady ever described her first ball with more particularity."

A few days later he writes more cheerfully: "I became acquainted with Mr. Bell,† who to my surprise expressed a good deal of interest about my crustacea and reptiles, and seems willing to work at them. I also heard that Mr. Broderip would be glad to look over the South American shells, so that things flourish well with me."

About his plants he writes with characteristic openness as to his own ignorance: "You have made me known amongst the botanists, but I felt very foolish when Mr. Don remarked

* William Lonsdale, b. 1794, d. 1871, was originally in the army, and served at the battles of Salamanca and Waterloo. After the war he left the service and gave himself up to science. He acted as assistant secretary to the Geological Society from 1829–42, when he resigned, owing to ill-health.

† T. Bell, F.R.S., formerly Prof. of Zoology in King's College, London, and sometime secretary to the Royal Society. He afterwards described the reptiles for the zoology of the voyage of the *Beagle*.

© The Complete Work of Charles Darwin Online

on the beautiful appearance of some plant with an astounding long name, and asked me about its habitation. Some one else seemed quite surprised that I knew nothing about a Carex from I do not know where. I was at last forced to plead most entire innocence, and that I knew no more about the plants which I had collected than the man in the moon."

As to part of his Geological Collection he was soon able to write: "I [have] disposed of the most important part [of] my collections, by giving all the fossil bones to the College of Surgeons, casts of them will be distributed, and descriptions published. They are very curious and valuable; one head belonged to some gnawing animal, but of the size of a Hippopotamus! Another to an ant-eater of the size of a horse!"

It is worth noting that at this time the only extinct mammalia from South America, which had been described, were Mastodon (three species) and Megatherium. The remains of the other extinct Edentata from Sir Woodbine Parish's collection had not been described. My father's specimens included (besides the above-mentioned Toxodon and Scelidotherium) the remains of Mylodon, Glossotherium, another gigantic animal allied to the ant-eater, and Macrauchenia. His discovery of these remains is a matter of interest in itself, but it has a special importance as a point in his own life, since it was the vivid impression produced by excavating them with his own hands * that formed one of the chief starting-points of his speculations on the origin of species. This is shown in the following extract from his Pocket Book for this year (1837): "In July opened first note-book on Transmutation of Species. Had been greatly struck from about the month of previous March on character of South American fossils, and species on Galapagos Archipelago. These facts (especially latter), origin of all my views."]

* I have often heard him speak of the despair with which he had to break off the projecting extremity of a huge, partly excavated bone, when the boat waiting for him would wait no longer.

© The Complete Work of Charles Darwin Online

1836–1837.

C. Darwin to W. D. Fox.

43 Great Marlborough Street,
November 6th [1836].

My dear Fox,

I have taken a shamefully long time in answering your letter. But the busiest time of the whole voyage has been tranquillity itself to this last month. After paying Henslow a short but very pleasant visit, I came up to town to wait for the *Beagle's* arrival. At last I have removed all my property from on board, and sent the specimens of Natural History to Cambridge, so that I am now a free man. My London visit has been quite idle as far as Natural History goes, but has been passed in most exciting dissipation amongst the Dons in science. All my affairs, indeed, are most prosperous; I find there are plenty who will undertake the description of whole tribes of animals, of which I know nothing. So that about this day month I hope to set to work tooth and nail at the Geology, which I shall publish by itself.

It is quite ridiculous what an immensely long period it appears to me since landing at Falmouth. The fact is I have talked and laughed enough for years instead of weeks, so [that] my memory is quite confounded with the noise. I am delighted to hear you are turned geologist: when I pay the Isle of Wight a visit, which I am determined shall somehow come to pass, you will be a capital cicerone to the famous line of dislocation. I really suppose there are few parts of the world more interesting to a geologist than your island. Amongst the great scientific men, no one has been nearly so friendly and kind as Lyell. I have seen him several times, and feel inclined to like him much. You cannot imagine how good-naturedly he entered into all my plans. I speak now only of the London men, for Henslow was just like his former self, and therefore a most cordial and affectionate friend.

© The Complete Work of Charles Darwin Online

When you pay London a visit I shall be very proud to take you to the Geological Society, for be it known, I was proposed to be a F.G.S. last Tuesday. It is, however, a great pity that these and the other letters, especially F.R.S. are so very expensive.

I do not scruple to ask you to write to me in a week's time in Shrewsbury, for you are a good letter writer, and if people will have such good characters they must pay the penalty. Good-bye, dear Fox.

Yours,

C. D.

[His affairs being thus so far prosperously managed he was able to put into execution his plan of living at Cambridge, where he settled on December 10th, 1836. He was at first a guest in the comfortable home of the Henslows, but afterwards, for the sake of undisturbed work, he moved into lodgings. He thus writes to Fox, March 13th, 1837, from London:—

"My residence at Cambridge was rather longer than I expected, owing to a job which I determined to finish there, namely, looking over all my geological specimens. Cambridge yet continues a very pleasant, but not half so merry a place as before. To walk through the courts of Christ's College, and not know an inhabitant of a single room, gave one a feeling half melancholy. The only evil I found in Cambridge was its being too pleasant: there was some agreeable party or another every evening, and one cannot say one is engaged with so much impunity there as in this great city."

A trifling record of my father's presence in Cambridge occurs in the book kept in Christ's College combination-room, where fines and bets were recorded, the earlier entries giving a curious impression of the after-dinner frame of mind of the fellows. The bets were not allowed to be made in money, but were, like the fines, paid in wine. The bet which my father made and lost is thus recorded:—

© The Complete Work of Charles Darwin Online

"*Feb.* 23, 1837.—Mr. Darwin *v.* Mr. Baines, that the combination-room measures from the ceiling to the floor more than (x) feet. 1 Bottle paid same day.

"N.B. Mr. Darwin may measure at any part of the room he pleases."

Besides arranging the geological and mineralogical specimens, he had his 'Journal of Researches' to work at, which occupied his evenings at Cambridge. He also read a short paper at the Zoological Society,* and another at the Geological Society,† on the recent elevation of the coast of Chili.

Early in the spring of 1837 (March 6th) he left Cambridge for London, and a week later he was settled in lodgings at 36 Great Marlborough Street; and except for a "short visit to Shrewsbury" in June, he worked on till September, being almost entirely employed on his 'Journal.' He found time, however, for two papers at the Geological Society.‡

He writes of his work to Fox (March, 1837):—

"In your last letter you urge me to get ready *the* book. I am now hard at work and give up everything else for it. Our plan is as follows: Capt. Fitz-Roy writes two volumes out of the materials collected during the last voyage under Capt. King to Tierra del Fuego, and during our circumnavigation. I am to have the third volume, in which I intend giving a kind of journal of a naturalist, not following, however, always the order of time, but rather the order of position. The habits of animals will occupy a large portion, sketches of the geology, the appearance of the country, and personal details will make the hodge-podge complete. Afterwards I shall write an account of the geology in detail, and

* "Notes upon Rhea Americana," 'Zool. Soc. Proc.' v. 1837, pp. 35, 36.

† 'Geol. Soc. Proc.' ii. 1838, pp. 446–449.

‡ "A sketch of the deposits containing extinct mammalia in the neighbourhood of the Plata," 'Geol. Soc. Proc.' ii. 1838, pp. 542–544; and "On certain areas of elevation and subsidence in the Pacific and Indian oceans, as deduced from the study of coral formations," 'Geol. Soc. Proc.' ii. 1838, pp. 552–554.

© The Complete Work of Charles Darwin Online

draw up some zoological papers. So that I have plenty of work for the next year or two, and till that is finished I will have no holidays."

Another letter to Fox (July) gives an account of the progress of his work :—

"I gave myself a holiday and a visit to Shrewsbury [in June], as I had finished my Journal. I shall now be very busy in filling up gaps and getting it quite ready for the press by the first of August. I shall always feel respect for every one who has written a book, let it be what it may, for I had no idea of the trouble which trying to write common English could cost one. And, alas, there yet remains the worst part of all, correcting the press. As soon as ever that is done I must put my shoulder to the wheel and commence at the Geology. I have read some short papers to the Geological Society, and they were favourably received by the great guns, and this gives me much confidence, and I hope not a very great deal of vanity, though I confess I feel too often like a peacock admiring his tail. I never expected that my Geology would ever have been worth the consideration of such men as Lyell, who has been to me, since my return, a most active friend. My life is a very busy one at present, and I hope may ever remain so; though Heaven knows there are many serious drawbacks to such a life, and chief amongst them is the little time it allows one for seeing one's natural friends. For the last three years, I have been longing and longing to be living at Shrewsbury, and after all now in the course of several months, I see my good dear people at Shrewsbury for a week. Susan and Catherine have, however, been staying with my brother here for some weeks, but they had returned home before my visit."

Besides the work already mentioned he had much to busy him in making arrangements for the publication of the 'Zoology of the Voyage of the *Beagle*.' The following letters illustrate this subject.]

© The Complete Work of Charles Darwin Online

*C. Darwin to L. Jenyns.**

36 Great Marlborough Street,
April 10th, 1837.

DEAR JENYNS,

During the last week several of the zoologists of this place have been urging me to consider the possibility of publishing the 'Zoology of the *Beagle's* Voyage' on some uniform plan. Mr. Macleay † has taken a great deal of interest in the subject, and maintains that such a publication is very desirable, because it keeps together a series of observations made respecting animals inhabiting the same part of the world, and allows any future traveller taking them with him. How far this facility of reference is of any consequence I am very doubtful; but if such is the case, it would be more satisfactory to myself to see the gleanings of my hands, after having passed through the brains of other naturalists, collected together in one work. But such considerations ought not to have much weight. The whole scheme is at present merely floating in the air; but I was determined to let you know, as I should much like to know what you think about it, and whether you would object to supply descriptions of the fish to such a work instead of to 'Transactions.' I apprehend the whole will be impracticable, without Government will aid in engraving the plates, and this I fear is a mere chance, only I think I can put in a strong claim, and get myself well backed by the naturalists of this place, who nearly all take a good deal

* Now Rev. L. Blomefield.

† William Sharp Macleay was the son of Alexander Macleay, formerly Colonial Secretary of New South Wales, and for many years Secretary of the Linnean Society. The son, who was a most zealous Naturalist, and had inherited from his father a very large general collection of insects, made Entomology his chief study, and gained great notoriety by his now forgotten *Quinary System*, set forth in the Second Part of his 'Horæ Entomologicæ,' published in 1821.—[I am indebted to Rev. L. Blomefield for the foregoing note.]

© The Complete Work of Charles Darwin Online

of interest in my collections. I mean to-morrow to see Mr. Yarrell; if he approves, I shall begin and take more active steps; for I hear he is most prudent and most wise. It is scarcely any use speculating about any plan, but I thought of getting subscribers and publishing the work in parts (as long as funds would last, for I myself will not lose money by it). In such case, whoever had his own part ready on any order might publish it separately (and ultimately the parts might be sold separately), so that no one should be delayed by the other. The plan would resemble, on a humble scale, Ruppel's 'Atlas,' or Humboldt's 'Zoologie,' where Latreille, Cuvier, &c., wrote different parts. I myself should have little to do with it; excepting in some orders adding habits and ranges, &c., and geographical sketches, and perhaps afterwards some descriptions of invertebrate animals . . . . .

I am working at my Journal; it gets on slowly, though I am not idle. I thought Cambridge a bad place from good dinners and other temptations, but I find London no better, and I fear it may grow worse. I have a capital friend in Lyell, and see a great deal of him, which is very advantageous to me in discussing much South American geology. I miss a walk in the country very much; this London is a vile smoky place, where a man loses a great part of the best enjoyments in life. But I see no chance of escaping, even for a week, from this prison for a long time to come. I fear it will be some time before we shall meet; for I suppose you will not come up here during the spring, and I do not think I shall be able to go down to Cambridge. How I should like to have a good walk along the Newmarket road to-morrow, but Oxford Street must do instead. I do hate the streets of London. Will you tell Henslow to be careful with the *edible* fungi from Tierra del Fuego, for I shall want some specimens for Mr. Brown, who seems *particularly* interested about them. Tell Henslow, I think my silicified wood has unflintified Mr. Brown's heart, for he was very gracious to me, and talked about the Gala-

© The Complete Work of Charles Darwin Online

pagos plants; but before he never would say a word. It is just striking twelve o'clock; so I will wish you a very good night.

My dear Jenyns,
Yours most truly,
C. DARWIN.

[A few weeks later the plan seems to have been matured, and the idea of seeking Government aid to have been adopted.]

C. Darwin to J. S. Henslow.

36 Great Marlborough Street,
[18th May, 1837].

MY DEAR HENSLOW,

I was very glad to receive your letter. I wanted much to hear how you were getting on with your manifold labours. Indeed I do not wonder your head began to ache; it is almost a wonder you have any head left. Your account of the Gamlingay expedition was cruelly tempting, but I cannot anyhow leave London. I wanted to pay my good, dear people at Shrewsbury a visit of a few days, but I found I could not manage it; at present I am waiting for the signatures of the Duke of Somerset, as President of the Linnean, and of Lord Derby and Whewell, to a statement of the value of my collection; the instant I get this I shall apply to Government for assistance in engraving, and so publish the 'Zoology' on some uniform plan. It is quite ridiculous the time any operation requires which depends on many people.

I have been working very steadily, but have only got two-thirds through the Journal part alone. I find, though I remain daily many hours at work, the progress is very slow: it is an awful thing to say to oneself, every fool and every clever man in England, if he chooses, may make as many ill-natured remarks as he likes on this unfortunate sentence.

* * * * *

© The Complete Work of Charles Darwin Online

[In August he writes to Henslow to announce the success of the scheme for the publication of the 'Zoology of the Voyage of the *Beagle*,' through the promise of a grant of £1000 from the Treasury: "I have delayed writing to you, to thank you most sincerely for having so effectually managed my affair. I waited till I had an interview with the Chancellor of the Exchequer.* He appointed to see me this morning, and I had a long conversation with him, Mr. Peacock being present. Nothing could be more thoroughly obliging and kind than his whole manner. He made no sort of restriction, but only told me to make the most of [the] money, which of course I am right willing to do.

"I expected rather an awful interview, but I never found anything less so in my life. It will be my fault if I do not make a good work; but I sometimes take an awful fright that I have not materials enough. It will be excessively satisfactory at the end of some two years to find all materials made the most they were capable of."

Later in the autumn he wrote to Henslow: "I have not been very well of late, with an uncomfortable palpitation of the heart, and my doctors urge me *strongly* to knock off all work, and go and live in the country for a few weeks." He accordingly took a holiday of about a month at Shrewsbury and Maer, and paid Fox a visit in the Isle of Wight. It was, I believe, during this visit, at Mr. Wedgwood's house at Maer, that he made his first observations on the work done by earthworms, and late in the autumn he read a paper on the subject at the Geological Society.† During these two months he was also busy preparing the scheme of the 'Zoology of the Voyage of the *Beagle*,' and in beginning to put together the Geological results of his travels.

The following letter refers to the proposal that he should take the Secretaryship of the Geological Society.]

* T. Spring Rice.

† "On the formation of mould," 'Geol. Soc. Proc.' ii. 1838, pp. 574–576.

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

October 14th [1837].

MY DEAR HENSLOW,

. . . I am much obliged to you for your message about the Secretaryship. I am exceedingly anxious for you to hear my side of the question, and will you be so kind as afterwards to give me your fair judgment. The subject has haunted me all summer. I am unwilling to undertake the office for the following reasons: First, my entire ignorance of English Geology, a knowledge of which would be almost necessary in order to shorten many of the papers before reading them before the Society, or rather to know what parts to skip. Again, my ignorance of all languages, and not knowing how to pronounce even a *single* word of French—a language so perpetually quoted. It would be disgraceful to the Society to have a Secretary who could not read French. Secondly, the loss of time; pray consider that I should have to look after the artists, superintend and furnish materials for the Government work, which will come out in parts, and which must appear regularly. All my Geological notes are in a very rough state; none of my fossil shells worked up; and I have much to read. I have had hopes, by giving up society and not wasting an hour, that I should finish my Geology in a year and a half, by which time the description of the higher animals by others would be completed, and my whole time would then necessarily be required to complete myself the description of the invertebrate ones. If this plan fails, as the Government work must go on, the Geology would necessarily be deferred till probably at least three years from this time. In the present state of the science, a great part of the utility of the little I have done would be lost, and all freshness and pleasure quite taken from me.

I know from experience the time required to make abstracts

© The Complete Work of Charles Darwin Online

even of my own papers for the 'Proceedings.' If I was Secretary, and had to make double abstracts of each paper, studying them before reading, and attendance would *at least* cost me three days (and often more) in the fortnight. There are likewise other accidental and contingent losses of time; I know Dr. Royle found the office consumed much of his time. If by merely giving up any amusement, or by working harder than I have done, I could save time, I would undertake the Secretaryship; but I appeal to you whether, with my slow manner of writing, with two works in hand, and with the certainty, if I cannot complete the Geological part within a fixed period, that its publication must be retarded for a very long time,—whether any Society whatever has any claim on me for three days' disagreeable work every fortnight. I cannot agree that it is a duty on my part, as a follower of science, as long as I devote myself to the completion of the work I have in hand, to delay that, by undertaking what may be done by any person who happens to have more spare time than I have at present. Moreover, so early in my scientific life, with so very much as I have to learn, the office, though no doubt a great honour, &c., for me, would be the more burdensome. Mr. Whewell (I know very well), judging from himself, will think I exaggerate the time the Secretaryship would require; but I absolutely know the time which with me the simplest writing consumes. I do not at all like appearing so selfish as to refuse Mr. Whewell, more especially as he has always shown, in the kindest manner, an interest in my affairs. But I cannot look forward with even tolerable comfort to undertaking an office without entering on it heart and soul, and that would be impossible with the Government work and the Geology in hand.

My last objection is, that I doubt how far my health will stand the confinement of what I have to do, without any additional work. I merely repeat, that you may know I am

© The Complete Work of Charles Darwin Online

not speaking idly, that when I consulted Dr. Clark in town, he at first urged me to give up entirely all writing and even correcting press for some weeks. Of late anything which flurries me completely knocks me up afterwards, and brings on a violent palpitation of the heart. Now the Secretaryship would be a periodical source of more annoying trouble to me than all the rest of the fortnight put together. In fact, till I return to town, and see how I get on, if I wished the office ever so much, I *could* not say I would positively undertake it. I beg of you to excuse this very long prose all about myself, but the point is one of great interest. I can neither bear to think myself very selfish and sulky, nor can I see the possibility of my taking the Secretaryship without making a sacrifice of all my plans and a good deal of comfort.

If you see Whewell, would you tell him the substance of this letter; or, if he will take the trouble, he may read it. My dear Henslow, I appeal to you *in loco parentis*. Pray tell me what you think? But do not judge me by the activity of mind which you and a few others possess, for in that case the more different things in hand the pleasanter the work; but, though I hope I never shall be idle, such is not the case with me.

Ever, dear Henslow,
Yours most truly,
C. DARWIN.

[He ultimately accepted the post, and held it for three years—from February 16, 1838, to February 19, 1841.

After being assured of the Grant for the publication of the 'Zoology of the Voyage of the *Beagle*,' there was much to be done in arranging the scheme of publication, and this occupied him during part of October and November.]

© The Complete Work of Charles Darwin Online

C. Darwin to J. S. Henslow.

[4th November, 1837.]

MY DEAR HENSLOW,

. . . Pray tell Leonard * that my Government work is going on smoothly, and I hope will be prosperous. He will see in the Prospectus his name attached to the fish; I set my shoulders to the work with a good heart. I am very much better than I was during the last month before my Shrewsbury visit. I fear the Geology will take me a great deal of time; I was looking over one set of notes, and the quantity I found I had to read, for that one place was frightful. If I live till I am eighty years old I shall not cease to marvel at finding myself an author; in the summer before I started, if any one had told me that I should have been an angel by this time, I should have thought it an equal impossibility. This marvellous transformation is all owing to you.

I am sorry to find that a good many errata are left in the part of my volume, which is printed. During my absence Mr. Colburn employed some goose to revise, and he has multiplied, instead of diminishing my oversights: but for all that, the smooth paper and clear type has a charming appearance, and I sat the other evening gazing in silent admiration at the first page of my own volume, when I received it from the printers!

Good bye, my dear Henslow,
C. DARWIN.

1838.

[From the beginning of this year to nearly the end of June, he was busily employed on the zoological and geological results of his voyage. This spell of work was interrupted

* Rev. L. Jenyns.

© The Complete Work of Charles Darwin Online

only by a visit of three days to Cambridge, in May; and even this short holiday was taken in consequence of failing health, as we may assume from the entry in his diary: "May 1st, unwell," and from a letter to his sister (May 16, 1838), when he wrote:—

"My trip of three days to Cambridge has done me such wonderful good, and filled my limbs with such elasticity, that I must get a little work out of my body before another holiday." This holiday seems to have been thoroughly enjoyed; he wrote to his sister:—

"Now for Cambridge: I stayed at Henslow's house and enjoyed my visit extremely. My friends gave me a most cordial welcome. Indeed, I was quite a lion there. Mrs. Henslow unfortunately was obliged to go on Friday for a visit in the country. That evening we had at Henslow's a brilliant party of all the geniuses in Cambridge, and a most remarkable set of men they most assuredly are. On Saturday I rode over to L. Jenyns', and spent the morning with him. I found him very cheerful, but bitterly complaining of his solitude. On Saturday evening dined at one of the Colleges, played at bowls on the College Green after dinner, and was deafened with nightingales singing. Sunday, dined in Trinity; capital dinner, and was very glad to sit by Professor Lee* . . . ; I find him a very pleasant chatting man, and in high spirits like a boy, at having lately returned from a living or a curacy, for seven years in Somersetshire, to civilised society and oriental manuscripts. He had exchanged his living to one within fourteen miles of Cambridge, and seemed perfectly happy. In the evening attended Trinity Chapel, and heard 'The Heavens are telling the Glory of God,' in magnificent style; the last chorus seemed to shake the very walls of the College. After chapel a large party in Sedgwick's rooms. So much for my Annals."

* Samuel Lee, of Queens', was Professor of Arabic from 1819 to 1831, and Regius Professor of Hebrew from 1831 to 1848.

© The Complete Work of Charles Darwin Online

He started, towards the end of June, on his expedition to Glen Roy, of which he writes to Fox: "I have not been very well of late, which has suddenly determined me to leave London earlier than I had anticipated. I go by the steam-packet to Edinburgh,—take a solitary walk on Salisbury Craigs, and call up old thoughts of former times, then go on to Glasgow and the great valley of Inverness, near which I intend stopping a week to geologise the parallel roads of Glen Roy, thence to Shrewsbury, Maer for one day, and London for smoke, ill-health and hard work."

He spent "eight good days" over the Parallel Roads. His Essay on this subject was written out during the same summer, and published by the Royal Society.* He wrote in his Pocket Book: "September 6 [1838]. Finished the paper on 'Glen Roy,' one of the most difficult and instructive tasks I was ever engaged on." It will be remembered that in his 'Recollections' he speaks of this paper as a failure, of which he was ashamed.

At the time at which he wrote, the latest theory of the formation of the Parallel Roads was that of Sir Lauder Dick and Dr. Macculloch, who believed that lakes had anciently existed in Glen Roy, caused by dams of rock or alluvium. In arguing against this theory he conceived that he had disproved the admissibility of any lake theory, but in this point he was mistaken. He wrote (Glen Roy paper, p. 49) "the conclusion is inevitable, that no hypothesis founded on the supposed existence of a sheet of water confined by *barriers*, that is a lake, can be admitted as solving the problematical origin of the parallel roads of Lochaber."

Mr. Archibald Geikie has been so good as to allow me to quote a passage from a letter addressed to me (Nov. 19, 1884) in compliance with my request for his opinion on the character of my father's Glen Roy work:—

"Mr. Darwin's 'Glen Roy' paper, I need not say, is marked

* 'Phil. Trans.' 1839, pp. 39–82.

© The Complete Work of Charles Darwin Online

by all his characteristic acuteness of observation and determination to consider all possible objections. It is a curious example, however, of the danger of reasoning by a method of exclusion in Natural Science. Finding that the waters which formed the terraces in the Glen Roy region could not possibly have been dammed back by barriers of rock or of detritus, he saw no alternative but to regard them as the work of the sea. Had the idea of transient barriers of glacier-ice occurred to him, he would have found the difficulties vanish from the lake-theory which he opposed, and he would not have been unconsciously led to minimise the altogether overwhelming objections to the supposition that the terraces are of marine origin."

It may be added that the idea of the barriers being formed by glaciers could hardly have occurred to him, considering what was the state of knowledge at the time, and bearing in mind what his want of opportunities of observing glacial action on a large scale.

The latter half of July was passed at Shrewsbury and Maer. The only entry of any interest is one of being "very idle" at Shrewsbury, and of opening "a note-book connected with metaphysical inquiries." In August he records that he read "a good deal of various amusing books, and paid some attention to metaphysical subjects."

The work done during the remainder of the year comprises the book on coral reefs (begun in October), and some work on the phenomena of elevation in S. America.]

C. Darwin to C. Lyell.

36 Great Marlborough Street,
August 9th [1838].

MY DEAR LYELL,

I did not write to you at Norwich, for I thought I should have more to say, if I waited a few more days. Very many thanks for the present of your 'Elements,' which I

© The Complete Work of Charles Darwin Online

received (and I believe the *very first* copy distributed) together with your note. I have read it through every word, and am full of admiration of it, and, as I now see no geologist, I must talk to you about it. There is no pleasure in reading a book if one cannot have a good talk over it; I repeat, I am full of admiration of it, it is as clear as daylight, in fact I felt in many parts some mortification at thinking how geologists have laboured and struggled at proving what seems, as you have put it, so evidently probable. I read with much interest your sketch of the secondary deposits; you have contrived to make it quite "juicy," as we used to say as children of a good story. There was also much new to me, and I have to copy out some fifty notes and references. It must do good, the heretics against common sense must yield. . . . By the way, do you recollect my telling you how much I disliked the manner ——— referred to his other works, as much as to say, "You must, ought, and shall buy everything I have written." To my mind, you have somehow quite avoided this; your references only seem to say, "I can't tell you all in this work, else I would, so you must go to the 'Principles'; and many a one, I trust, you will send there, and make them, like me, adorers of the good science of rock-breaking. You will see I am in a fit of enthusiasm, and good cause I have to be, when I find you have made such infinitely more use of my Journal than I could have anticipated. I will say no more about the book, for it is all praise. I must, however, admire the elaborate honesty with which you quote the words of all living and dead geologists.

My Scotch expedition answered brilliantly; my trip in the steam-packet was absolutely pleasant, and I enjoyed the spectacle, wretch that I am, of two ladies, and some small children quite sea-sick, I being well. Moreover, on my return from Glasgow to Liverpool, I triumphed in a similar manner over some full-grown men. I stayed one whole day in Edinburgh, or more truly on Salisbury Craigs; I want to hear some day

© The Complete Work of Charles Darwin Online

what you think about that classical ground,—the structure was to me new and rather curious,—that is, if I understand it right. I crossed from Edinburgh in gigs and carts (and carts without springs, as I never shall forget) to Loch Leven. I was disappointed in the scenery, and reached Glen Roy on Saturday evening, one week after leaving Marlborough Street. Here I enjoyed five [?] days of the most beautiful weather with gorgeous sunsets, and all nature looking as happy as I felt. I wandered over the mountains in all directions, and examined that most extraordinary district. I think, without any exceptions, not even the first volcanic island, the first elevated beach, or the passage of the Cordillera, was so interesting to me as this week. It is far the most remarkable area I ever examined. I have fully convinced myself (after some doubting at first) that the shelves are sea-beaches, although I could not find a trace of a shell; and I think I can explain away most, if not all, the difficulties. I found a piece of a road in another valley, not hitherto observed, which is important; and I have some curious facts about erratic blocks, one of which was perched up on a peak 2200 feet above the sea. I am now employed in writing a paper on the subject, which I find very amusing work, excepting that I cannot anyhow condense it into reasonable limits. At some future day I hope to talk over some of the conclusions with you, which the examination of Glen Roy has led me to. Now I have had my talk out, I am much easier, for I can assure you Glen Roy has astonished me.

I am living very quietly, and therefore pleasantly, and am crawling on slowly but steadily with my work. I have come to one conclusion, which you will think proves me to be a very sensible man, namely, that whatever you say proves right; and as a proof of this, I am coming into your way of only working about two hours at a spell; I then go out and do my business in the streets, return and set to work again, and thus make two separate days out of one. The new plan

© The Complete Work of Charles Darwin Online

answers capitally; after the second half day is finished I go and dine at the Athenæum like a gentleman, or rather like a lord, for I am sure the first evening I sat in that great drawing-room, all on a sofa by myself, I felt just like a duke. I am full of admiration at the Athenæum, one meets so many people there that one likes to see. The very first time I dined there (*i.e.* last week) I met Dr. Fitton * at the door, and he got together quite a party—Robert Brown, who is gone to Paris and Auvergne, Macleay [?] and Dr. Boott.† Your helping me into the Athenæum has not been thrown away, and I enjoy it the more because I fully expected to detest it.

I am writing you a most unmerciful letter, but I shall get Owen to take it to Newcastle. If you have a mind to be a very generous man you will write to me from Kinnordy,‡ and tell me some Newcastle news, as well as about the Craig, and about yourself and Mrs. Lyell, and everything else in the world. I will send by Hall the 'Entomological Transactions,' which I have borrowed for you; you will be disappointed in ——'s papers, that is if you suppose my dear friend has a single clear idea upon any one subject. He has so involved recent insects and true fossil insects in one table that I fear you will not make much out of it, though it is a subject which ought I should think to come into the 'Principles.' You will

* W. H. Fitton (b. 1780, d. 1861) was a physician and geologist, and sometime president of the Geological Society. He established the 'Proceedings,' a mode of publication afterwards adopted by other societies.

† Francis Boott (b. 1792, d. 1863) is chiefly known as a botanist through his work on the genus Carex. He was also well known in connection with the Linnean Society of which he was for many years an office-bearer. He is described (in a biographical sketch published in the *Gardeners' Chronicle*, 1864) as having been one of the first physicians in London who gave up the customary black coat, knee-breeches and silk stockings, and adopted the ordinary dress of the period, a blue coat with brass buttons, and a buff waistcoat, a costume which he continued to wear to the last. After giving up practice, which he did early in life, he spent much of his time in acts of unpretending philanthropy.

‡ The house of Lyell's father.

© The Complete Work of Charles Darwin Online

be amused at some of the ridiculo-sublime passages in the papers, and no doubt will feel acutely a sneer there is at yourself. I have heard from more than one quarter that quarrelling is expected at Newcastle *; I am sorry to hear it. I met old —— this evening at the Athenæum, and he muttered something about writing to you or some one on the subject; I am however all in the dark. I suppose, however, I shall be illuminated, for I am going to dine with him in a few days, as my inventive powers failed in making any excuse. A friend of mine dined with him the other day, a party of four, and they finished ten bottles of wine—a pleasant prospect for me; but I am determined not even to taste his wine, partly for the fun of seeing his infinite disgust and surprise. . . .

I pity you the infliction of this most unmerciful letter. Pray remember me most kindly to Mrs. Lyell when you arrive at Kinnordy. I saw her name in the landlord's book of Inverorum. Tell Mrs. Lyell to read the second series of 'Mr. Slick of Slickville's Sayings.' . . . He almost beats "Samivel," that prince of heroes. Good night, my dear Lyell; you will think I have been drinking some strong drink to write so much nonsense, but I did not even taste Minerva's small beer to-day.

Yours most sincerely,
CHAS. DARWIN.

C. Darwin to C. Lyell.

Friday night, September 13th [1838].

MY DEAR LYELL,

I was astonished and delighted at your gloriously long letter, and I am sure I am very much obliged to Mrs. Lyell for having taken the trouble to write so much.† I mean to have a good hour's enjoyment and scribble away

* At the meeting of the British Association.

† Lyell dictated much of his correspondence.

© The Complete Work of Charles Darwin Online

to you, who have so much geological sympathy that I do not care how egotistically I write. . . .

I have got so much to say about all sorts of trifling things that I hardly know what to begin about. I need not say how pleased I am to hear that Mr. Lyell * likes my Journal. To hear such tidings is a kind of resurrection, for I feel towards my first-born child as if it had long since been dead, buried, and forgotten; but the past is nothing and the future everything to us geologists, as you show in your capital motto to the 'Elements.' By the way, have you read the article, in the 'Edinburgh Review,' on M. Comte, 'Cours de la Philosophie' (or some such title)? It is capital; there are some fine sentences about the very essence of science being prediction, which reminded me of "its law being progress."

I will now begin and go through your letter *seriatim*. I dare say your plan of putting the Elie de Beaumont's chapter separately and early will be very good; anyhow, it is showing a bold front in the first edition which is to be translated into French. It will be a curious point to geologists hereafter to note how long a man's name will support a theory so completely exposed as that of De Beaumont's has been by you; you say you "begin to hope that the great principles there insisted on will stand the test of time." *Begin to hope*: why, the *possibility* of a doubt has never crossed my mind for many a day. This may be very unphilosophical, but my geological salvation is staked on it. After having just come back from Glen Roy, and found how difficulties smooth away under your principles, it makes me quite indignant that you should talk of *hoping*. With respect to the question, how far my coral theory bears on De Beaumont's theory, I think it would be prudent to quote me with great caution until my whole account is published, and then you (and others) can judge how far there is foundation for such generalisation. Mind, I do not doubt its truth;

* Father of the geologist.

© The Complete Work of Charles Darwin Online

but the extension of any view over such large spaces, from comparatively few facts, must be received with much caution. I do not myself the least doubt that within the recent (or as you, much to my annoyment, would call it, "New Pliocene") period, tortuous bands—not all the bands parallel to each other—have been elevated and corresponding ones subsided, though within the same period some parts probably remained for a time stationary, or even subsided. I do not believe a more utterly false view could have been invented than great straight lines being suddenly thrown up.

When my book on Volcanoes and Coral Reefs will be published I hardly know; I fear it will be at least four or five months; though, mind, the greater part is written. I find so much time is lost in correcting details and ascertaining their accuracy. The Government Zoological work is a millstone round my neck, and the Glen Roy paper has lost me six weeks. I will not, however, say lost; for, supposing I can prove to others' satisfaction what I have convinced myself is the case, the inference I think you will allow to be important. I cannot doubt that the molten matter beneath the earth's crust possesses a high degree of fluidity, almost like the sea beneath the block ice. By the way, I hope you will give me some Swedish case to quote, of shells being preserved on the surface, but not in contemporaneous beds of gravel. . . .

Remember what I have often heard you say: the country is very bad for the intellects; the Scotch mists will put out some volcanic speculations. You see I am affecting to become very Cockneyfied, and to despise the poor country-folk, who breathe fresh air instead of smoke, and see the goodly fields instead of the brick houses in Marlborough Street, the very sight of which I confess I abhor. I am glad to hear what a favourable report you give of the British Association. I am the more pleased because I have been fighting its battle with Basil Hall, Stokes, and several others, having made up my mind, from the report in the *Athenæum*,

© The Complete Work of Charles Darwin Online

that it must have been an excellent meeting. I have been much amused with an account I have received of the wars of Don Roderick * and Babbage. What a grievous pity it is that the latter should be so implacable . . . This is a most rigmarole letter, for after each sentence I take breath, and you will have need of it in reading it. . . .

I wish with all my heart that my Geological book was out. I have every motive to work hard, and will, following your steps, work just that degree of hardness to keep well. I should like my volume to be out before your new edition of 'Principles' appears. Besides the Coral theory, the volcanic chapters will, I think, contain some new facts. I have lately been sadly tempted to be idle—that is, as far as pure geology is concerned—by the delightful number of new views which have been coming in thickly and steadily,—on the classification and affinities and instincts of animals—bearing on the question of species. Note-book after note-book has been filled with facts which begin to group themselves *clearly* under sub-laws.

Good night, my dear Lyell. I have filled my letter and enjoyed my talk to you as much as I can without having you *in propriâ personâ*. Think of the bad effects of the country—so once more good night.

Ever yours,
CHAS. DARWIN.

Pray again give my best thanks to Mrs. Lyell.

[The record of what he wrote during the year does not give a true index of the most important work that was in progress,—the laying of the foundation-stones of what was to be the achievement of his life. This is shown in the foregoing letter to Lyell, where he speaks of being "idle," and the following extract from a letter to Fox, written in June, is of interest in this point of view:

* Murchison.

© The Complete Work of Charles Darwin Online

"I am delighted to hear you are such a good man as not to have forgotten my questions about the crossing of animals. It is my prime hobby, and I really think some day I shall be able to do something in that most intricate subject, species and varieties."]

1839 to 1841.

[In the winter of 1839 (Jan. 29) my father was married to his cousin, Emma Wedgwood.* The house in which they lived for the first few years of their married life, No. 12 Upper Gower Street, was a small common-place London house, with a drawing-room in front, and a small room behind, in which they lived for the sake of quietness. In later years my father used to laugh over the surpassing ugliness of the furniture, carpets, &c., of the Gower Street house. The only redeeming feature was a better garden than most London houses have, a strip as wide as the house, and thirty yards long. Even this small space of dingy grass made their London house more tolerable to its two country-bred inhabitants.

Of his life in London he writes to Fox (October 1839): "We are living a life of extreme quietness; Delamere itself, which you describe as so secluded a spot, is, I will answer for it, quite dissipated compared with Gower Street. We have given up all parties, for they agree with neither of us; and if one is quiet in London, there is nothing like its quietness—there is a grandeur about its smoky fogs, and the dull distant sounds of cabs and coaches; in fact you may perceive I am becoming a thorough-paced Cockney, and I glory in thoughts that I shall be here for the next six months."

The entries of ill health in the Diary increase in number during these years, and as a consequence the holidays become longer and more frequent. From April 26 to May 13,

* Daughter of Josiah Wedgwood of Maer, and grand-daughter of the founder of the Etruria Works.

© The Complete Work of Charles Darwin Online

1839, he was at Maer and Shrewsbury. Again, from August 23 to October 2 he was away from London at Maer, Shrewsbury, and at Birmingham for the meeting of the British Association.

The entry under August 1839 is: "During my visit to Maer, read a little, was much unwell and scandalously idle. I have derived this much good, that *nothing* is so intolerable as idleness."

At the end of 1839 his eldest child was born, and it was then that he began his observations ultimately published in the 'Expression of the Emotions.' His book on this subject, and the short paper published in 'Mind,' * show how closely he observed his child. He seems to have been surprised at his own feeling for a young baby, for he wrote to Fox (July 1840): "He [*i.e.* the baby] is so charming that I cannot pretend to any modesty. I defy anybody to flatter us on our baby, for I defy any one to say anything in its praise of which we are not fully conscious. . . . I had not the smallest conception there was so much in a five-month baby. You will perceive by this that I have a fine degree of paternal fervour."

During these years he worked intermittently at 'Coral Reefs,' being constantly interrupted by ill health. Thus he speaks of "recommencing" the subject in February 1839, and again in the October of the same year, and once more in July 1841, "after more than thirteen months' interval." His other scientific work consisted of a contribution to the Geological Society,† on the boulders and "till" of South America, as well as a few other minor papers on geological subjects. He also worked busily at the ornithological part of the Zoology of the *Beagle*, *i.e.* the notice of the habits and ranges of the birds which were described by Gould.

* July 1877.
† 'Geol. Soc. Proc.' iii. 1842, and 'Geol. Soc. Trans.' vi.

© The Complete Work of Charles Darwin Online

C. Darwin to C. Lyell.

Wednesday morning [February 1840].

MY DEAR LYELL,

Many thanks for your kind note. I will send for the *Scotsman*. Dr. Holland thinks he has found out what is the matter with me, and now hopes he shall be able to set me going again. Is it not mortifying, it is now nine weeks since I have done a whole day's work, and not more than four half days. But I won't grumble any more, though it is hard work to prevent doing so. Since receiving your note I have read over my chapter on Coral, and find I am prepared to stand by almost everything; it is much more cautiously and accurately written than I thought. I had set my heart upon having my volume completed before your new edition, but not, you may believe me, for you to notice anything new in it (for there is very little besides details), but you are the one man in Europe whose opinion of the general truth of a toughish argument I should be always most anxious to hear. My MS. is in such confusion, otherwise I am sure you should most willingly, if it had been worth your while, have looked at any part you choose.

* * * * *

[In a letter to Fox (January 1841) he shows that his "Species work" was still occupying his mind :—

"If you attend at all to Natural History I send you this P.S. as a memento, that I continue to collect all kinds of facts about 'Varieties and Species,' for my some-day work to be so entitled; the smallest contributions thankfully accepted; descriptions of offspring of all crosses between all domestic birds and animals, dogs, cats, &c., &c., very valuable. Don't forget, if your half-bred African cat should die that I should be very much obliged for its carcase sent up in a little hamper for the skeleton; it, or any cross-bred pigeons, fowl, duck, &c., &c., will be more acceptable than the finest haunch of venison, or the finest turtle."

© The Complete Work of Charles Darwin Online

Later in the year (September) he writes to Fox about his health, and also with reference to his plan of moving into the country :—

"I have steadily been gaining ground, and really believe now I shall some day be quite strong. I write daily for a couple of hours on my Coral volume, and take a little walk or ride every day. I grow very tired in the evenings, and am not able to go out at that time, or hardly to receive my nearest relations ; but my life ceases to be burdensome now that I can do something. We are taking steps to leave London, and live about twenty miles from it on some railway."]

1842.

[The record of work includes his volume on 'Coral Reefs,' * the manuscript of which was at last sent to the printers in January of this year, and the last proof corrected in May. He thus writes of the work in his diary :—

"I commenced this work three years and seven months ago. Out of this period about twenty months (besides work during *Beagle's* voyage) has been spent on it, and besides it, I have only compiled the Bird part of Zoology ; Appendix to Journal, paper on Boulders, and corrected papers on Glen Roy and earthquakes, reading on species, and rest all lost by illness."

In May and June he was at Shrewsbury and Maer, whence he went on to make the little tour in Wales, of which he spoke in his 'Recollections,' and of which the results were published as "Notes on the effects produced by the ancient glaciers of Caernarvonshire, and on the Boulders transported by floating Ice." †

* A notice of the Coral Reef work appeared in the 'Geograph. Soc. Journal,' xii. p. 115.

† 'Philosophical Magazine,' 1842, p. 352.

© The Complete Work of Charles Darwin Online

Mr. Archibald Geikie speaks of this paper as standing "almost at the top of the long list of English contributions to the history of the Ice Age." *

The latter part of this year belongs to the period including the settlement at Down, and is therefore dealt with in another chapter.]

* Charles Darwin, 'Nature' Series, p. 23.

© The Complete Work of Charles Darwin Online

CHAPTER VIII.

RELIGION.

[THE history of this part of my father's life may justly include some mention of his religious views. For although, as he points out, he did not give continuous systematic thought to religious questions, yet we know from his own words that about this time (1836–39) the subject was much before his mind.

In his published works he was reticent on the matter of religion, and what he has left on the subject was not written with a view to publication.*

I believe that his reticence arose from several causes. He felt strongly that a man's religion is an essentially private matter, and one concerning himself alone. This is indicated by the following extract from a letter of 1879:—†

"What my own views may be is a question of no consequence to any one but myself. But, as you ask, I may state that my judgment often fluctuates . . . In my most extreme fluctuations I have never been an Atheist in the sense of denying the existence of a God. I think that generally (and more and more as I grow older), but not always, that an Agnostic would be the more correct description of my state of mind."

* As an exception may be mentioned, a few words of concurrence with Dr. Abbott's 'Truths for the Times,' which my father allowed to be published in the *Index*.

† Addressed to Mr. J. Fordyce, and published by him in his 'Aspects of Scepticism,' 1883.

© The Complete Work of Charles Darwin Online

He naturally shrank from wounding the sensibilities of others in religious matters, and he was also influenced by the consciousness that a man ought not to publish on a subject to which he has not given special and continuous thought. That he felt this caution to apply to himself in the matter of religion is shown in a letter to Dr. F. E. Abbott, of Cambridge, U.S. (Sept. 6, 1871). After explaining that the weakness arising from his bad health prevented him from feeling "equal to deep reflection, on the deepest subject which can fill a man's mind," he goes on to say: "With respect to my former notes to you, I quite forget their contents. I have to write many letters, and can reflect but little on what I write; but I fully believe and hope that I have never written a word, which at the time I did not think; but I think you will agree with me, that anything which is to be given to the public ought to be maturely weighed and cautiously put. It never occurred to me that you would wish to print any extract from my notes: if it had, I would have kept a copy. I put 'private' from habit, only as yet partially acquired, from some hasty notes of mine having been printed, which were not in the least degree worth printing, though otherwise unobjectionable. It is simply ridiculous to suppose that my former note to you would be worth sending to me, with any part marked which you desire to print; but if you like to do so, I will at once say whether I should have any objection. I feel in some degree unwilling to express myself publicly on religious subjects, as I do not feel that I have thought deeply enough to justify any publicity."

I may also quote from another letter to Dr. Abbott (Nov. 16, 1871), in which my father gives more fully his reasons for not feeling competent to write on religious and moral subjects:—

"I can say with entire truth that I feel honoured by your request that I should become a contributor to the *Index*,

© The Complete Work of Charles Darwin Online

and am much obliged for the draft. I fully, also, subscribe to the proposition that it is the duty of every one to spread what he believes to be the truth; and I honour you for doing so, with so much devotion and zeal. But I cannot comply with your request for the following reasons; and excuse me for giving them in some detail, as I should be very sorry to appear in your eyes ungracious. My health is very weak: I *never* pass 24 hours without many hours of discomfort, when I can do nothing whatever. I have thus, also, lost two whole consecutive months this season. Owing to this weakness, and my head being often giddy, I am unable to master new subjects requiring much thought, and can deal only with old materials. At no time am I a quick thinker or writer: whatever I have done in science has solely been by long pondering, patience and industry.

"Now I have never systematically thought much on religion in relation to science, or on morals in relation to society; and without steadily keeping my mind on such subjects for a *long* period, I am really incapable of writing anything worth sending to the *Index*."

He was more than once asked to give his views on religion, and he had, as a rule, no objection to doing so in a private letter. Thus in answer to a Dutch student, he wrote (April 2, 1873):—

"I am sure you will excuse my writing at length, when I tell you that I have long been much out of health, and am now staying away from my home for rest.

"It is impossible to answer your question briefly; and I am not sure that I could do so, even if I wrote at some length. But I may say that the impossibility of conceiving that this grand and wondrous universe, with our conscious selves, arose through chance, seems to me the chief argument for the existence of God; but whether this is an argument of real value, I have never been able to decide. I am aware that if we admit a first cause, the mind still craves to know whence

© The Complete Work of Charles Darwin Online

it came, and how it arose. Nor can I overlook the difficulty from the immense amount of suffering through the world. I am, also, induced to defer to a certain extent to the judgment of the many able men who have fully believed in God; but here again I see how poor an argument this is. The safest conclusion seems to me that the whole subject is beyond the scope of man's intellect; but man can do his duty."

Again in 1879 he was applied to by a German student, in a similar manner. The letter was answered by a member of my father's family, who wrote:—

"Mr. Darwin begs me to say that he receives so many letters, that he cannot answer them all.

"He considers that the theory of Evolution is quite compatible with the belief in a God; but that you must remember that different persons have different definitions of what they mean by God."

This, however, did not satisfy the German youth, who again wrote to my father, and received from him the following reply:—

"I am much engaged, an old man, and out of health, and I cannot spare time to answer your questions fully,—nor indeed can they be answered. Science has nothing to do with Christ, except in so far as the habit of scientific research makes a man cautious in admitting evidence. For myself, I do not believe that there ever has been any revelation. As for a future life, every man must judge for himself between conflicting vague probabilities."

The passages which here follow are extracts, somewhat abbreviated, from a part of the Autobiography, written in 1876, in which my father gives the history of his religious views:—

"During these two years * I was led to think much about religion. Whilst on board the *Beagle* I was quite orthodox,

* Oct. 1836 to Jan. 1839.

© The Complete Work of Charles Darwin Online

and I remember being heartily laughed at by several of the officers (though themselves orthodox) for quoting the Bible as an unanswerable authority on some point of morality. I suppose it was the novelty of the argument that amused them. But I had gradually come by this time, *i.e.* 1836 to 1839, to see that the Old Testament was no more to be trusted than the sacred books of the Hindoos. The question then continually rose before my mind and would not be banished,—is it credible that if God were now to make a revelation to the Hindoos, he would permit it to be connected with the belief in Vishnu, Siva, &c., as Christianity is connected with the Old Testament? This appeared to me utterly incredible.

"By further reflecting that the clearest evidence would be requisite to make any sane man believe in the miracles by which Christianity is supported,—and that the more we know of the fixed laws of nature the more incredible do miracles become,—that the men at that time were ignorant and credulous to a degree almost incomprehensible by us,—that the Gospels cannot be proved to have been written simultaneously with the events,—that they differ in many important details, far too important, as it seemed to me, to be admitted as the usual inaccuracies of eye-witnesses;—by such reflections as these, which I give not as having the least novelty or value, but as they influenced me, I gradually came to disbelieve in Christianity as a divine revelation. The fact that many false religions have spread over large portions of the earth like wild-fire had some weight with me.

"But I was very unwilling to give up my belief; I feel sure of this, for I can well remember often and often inventing day-dreams of old letters between distinguished Romans, and manuscripts being discovered at Pompeii or elsewhere, which confirmed in the most striking manner all that was written in the Gospels. But I found it more and more difficult, with free

© The Complete Work of Charles Darwin Online

scope given to my imagination, to invent evidence which would suffice to convince me. Thus disbelief crept over me at a very slow rate, but was at last complete. The rate was so slow that I felt no distress.

"Although I did not think much about the existence of a personal God until a considerably later period of my life, I will here give the vague conclusions to which I have been driven. The old argument from design in Nature, as given by Paley, which formerly seemed to me so conclusive, fails, now that the law of natural selection has been discovered. We can no longer argue that, for instance, the beautiful hinge of a bivalve shell must have been made by an intelligent being, like the hinge of a door by man. There seems to be no more design in the variability of organic beings, and in the action of natural selection, than in the course which the wind blows. But I have discussed this subject at the end of my book on the 'Variation of Domesticated Animals and Plants,'* and the argument there given has never, as far as I can see, been answered.

"But passing over the endless beautiful adaptations which we everywhere meet with, it may be asked how can the generally beneficent arrangement of the world be accounted for? Some writers indeed are so much impressed with the amount of suffering in the world, that they doubt, if we look to all sentient beings, whether there is more of misery or of happiness; whether the world as a whole is a good or bad one.

* My father asks whether we are to believe that the forms are preordained of the broken fragments of rock tumbled from a precipice which are fitted together by man to build his houses. If not, why should we believe that the variations of domestic animals or plants are preordained for the sake of the breeder? "But if we give up the principle in one case, . . . no shadow of reason can be assigned for the belief that variations, alike in nature and the result of the same general laws, which have been the groundwork through natural selection of the formation of the most perfectly adapted animals in the world, man included, were intentionally and specially guided."—'The Variation of Animals and Plants,' 1st Edit. vol. ii. p. 431.—F. D.

© The Complete Work of Charles Darwin Online

According to my judgment happiness decidedly prevails, though this would be very difficult to prove. If the truth of this conclusion be granted, it harmonizes well with the effects which we might expect from natural selection. If all the individuals of any species were habitually to suffer to an extreme degree, they would neglect to propagate their kind; but we have no reason to believe that this has ever, or at least often occurred. Some other considerations, moreover, lead to the belief that all sentient beings have been formed so as to enjoy, as a general rule, happiness.

"Every one who believes, as I do, that all the corporeal and mental organs (excepting those which are neither advantageous nor disadvantageous to the possessor) of all beings have been developed through natural selection, or the survival of the fittest, together with use or habit, will admit that these organs have been formed so that their possessors may compete successfully with other beings, and thus increase in number. Now an animal may be led to pursue that course of action which is most beneficial to the species by suffering, such as pain, hunger, thirst, and fear; or by pleasure, as in eating and drinking, and in the propagation of the species, &c.; or by both means combined, as in the search for food. But pain or suffering of any kind, if long continued, causes depression and lessens the power of action, yet is well adapted to make a creature guard itself against any great or sudden evil. Pleasurable sensations, on the other hand, may be long continued without any depressing effect; on the contrary, they stimulate the whole system to increased action. Hence it has come to pass that most or all sentient beings have been developed in such a manner, through natural selection, that pleasurable sensations serve as their habitual guides. We see this in the pleasure from exertion, even occasionally from great exertion of the body or mind,—in the pleasure of our daily meals, and especially in the pleasure derived from sociability, and from loving our families. The sum of such pleasures as these,

© The Complete Work of Charles Darwin Online

which are habitual or frequently recurrent, give, as I can hardly doubt, to most sentient beings an excess of happiness over misery, although many occasionally suffer much. Such suffering is quite compatible with the belief in Natural Selection, which is not perfect in its action, but tends only to render each species as successful as possible in the battle for life with other species, in wonderfully complex and changing circumstances.

"That there is much suffering in the world no one disputes. Some have attempted to explain this with reference to man by imagining that it serves for his moral improvement. But the number of men in the world is as nothing compared with that of all other sentient beings, and they often suffer greatly without any moral improvement. This very old argument from the existence of suffering against the existence of an intelligent First Cause seems to me a strong one; whereas, as just remarked, the presence of much suffering agrees well with the view that all organic beings have been developed through variation and natural selection.

"At the present day the most usual argument for the existence of an intelligent God is drawn from the deep inward conviction and feelings which are experienced by most persons.

"Formerly I was led by feelings such as those just referred to (although I do not think that the religious sentiment was ever strongly developed in me), to the firm conviction of the existence of God, and of the immortality of the soul. In my Journal I wrote that whilst standing in the midst of the grandeur of a Brazilian forest, "it is not possible to give an adequate idea of the higher feelings of wonder, admiration, and devotion, which fill and elevate the mind." I well remember my conviction that there is more in man than the mere breath of his body. But now the grandest scenes would not cause any such convictions and feelings to rise in my mind. It may be truly said that I am like a man

© The Complete Work of Charles Darwin Online

who has become colour-blind, and the universal belief by men of the existence of redness makes my present loss of perception of not the least value as evidence. This argument would be a valid one if all men of all races had the same inward conviction of the existence of one God; but we know that this is very far from being the case. Therefore I cannot see that such inward convictions and feelings are of any weight as evidence of what really exists. The state of mind which grand scenes formerly excited in me, and which was intimately connected with a belief in God, did not essentially differ from that which is often called the sense of sublimity; and however difficult it may be to explain the genesis of this sense, it can hardly be advanced as an argument for the existence of God, any more than the powerful though vague and similar feelings excited by music.

"With respect to immortality, nothing shows me [so clearly] how strong and almost instinctive a belief it is, as the consideration of the view now held by most physicists, namely, that the sun with all the planets will in time grow too cold for life, unless indeed some great body dashes into the sun and thus gives it fresh life. Believing as I do that man in the distant future will be a far more perfect creature than he now is, it is an intolerable thought that he and all other sentient beings are doomed to complete annihilation after such long-continued slow progress. To those who fully admit the immortality of the human soul, the destruction of our world will not appear so dreadful.

"Another source of conviction in the existence of God, connected with the reason, and not with the feelings, impresses me as having much more weight. This follows from the extreme difficulty or rather impossibility of conceiving this immense and wonderful universe, including man with his capacity of looking far backwards and far into futurity, as the result of blind chance or necessity. When thus reflecting I feel compelled to look to a First Cause having an intelligent mind in

© The Complete Work of Charles Darwin Online

some degree analogous to that of man; and I deserve to be called a Theist. This conclusion was strong in my mind about the time, as far as I can remember, when I wrote the 'Origin of Species;' and it is since that time that it has very gradually, with many fluctuations, become weaker. But then arises the doubt, can the mind of man, which has, as I fully believe, been developed from a mind as low as that possessed by the lowest animals, be trusted when it draws such grand conclusions?

"I cannot pretend to throw the least light on such abstruse problems. The mystery of the beginning of all things is insoluble by us; and I for one must be content to remain an Agnostic."

[The following letters repeat to some extent what has been given from the Autobiography. The first one refers to 'The Boundaries of Science, a Dialogue,' published in 'Macmillan's Magazine,' for July 1861.]

C. Darwin to Miss Julia Wedgwood.

July 11 [1861].

Some one has sent us 'Macmillan'; and I must tell you how much I admire your Article; though at the same time I must confess that I could not clearly follow you in some parts, which probably is in main part due to my not being at all accustomed to metaphysical trains of thought. I think that you understand my book * perfectly, and that I find a very rare event with my critics. The ideas in the last page have several times vaguely crossed my mind. Owing to several correspondents I have been led lately to think, or rather to try to think over some of the chief points discussed by you. But the result has been with me a maze—something like thinking on the origin of evil, to which you allude. The mind refuses to look at this universe, being what it is,

* The 'Origin of Species.'

© The Complete Work of Charles Darwin Online

without having been designed; yet, where one would most expect design, viz. in the structure of a sentient being, the more I think on the subject, the less I can see proof of design. Asa Gray and some others look at each variation, or at least at each beneficial variation (which A. Gray would compare with the rain drops * which do not fall on the sea, but on to the land to fertilize it) as having been providentially designed. Yet when I ask him whether he looks at each variation in the rock-pigeon, by which man has made by accumulation a pouter or fantail pigeon, as providentially designed for man's amusement, he does not know what to answer; and if he, or any one, admits [that] these variations are accidental, as far as purpose is concerned (of course not accidental as to their cause or origin); then I can see no reason why he should rank the accumulated variations by which the beautifully adapted woodpecker has been formed, as providentially designed. For it would be easy to imagine the enlarged crop of the pouter, or tail of the fantail, as of some use to birds, in a state of nature, having peculiar habits of life. These are the considerations which perplex me about design; but whether you will care to hear them, I know not.

* * * * *

[On the subject of design, he wrote (July 1860) to Dr. Gray:

"One word more on 'designed laws' and 'undesigned results.' I see a bird which I want for food, take my gun and

* Dr. Gray's rain-drop metaphor occurs in the Essay 'Darwin and his Reviewers' ('Darwiniana,' p. 157): "The whole animate life of a country depends absolutely upon the vegetation, the vegetation upon the rain. The moisture is furnished by the ocean, is raised by the sun's heat from the ocean's surface, and is wafted inland by the winds. But what multitudes of rain-drops fall back into the ocean—are as much without a final cause as the incipient varieties which come to nothing! Does it therefore follow that the rains which are bestowed upon the soil with such rule and average regularity were not designed to support vegetable and animal life?"

© The Complete Work of Charles Darwin Online

kill it, I do this *designedly*. An innocent and good man stands under a tree and is killed by a flash of lightning. Do you believe (and I really should like to hear) that God *designedly* killed this man? Many or most persons do believe this; I can't and don't. If you believe so, do you believe that when a swallow snaps up a gnat that God designed that that particular swallow should snap up that particular gnat at that particular instant? I believe that the man and the gnat are in the same predicament. If the death of neither man nor gnat are designed, I see no good reason to believe that their *first* birth or production should be necessarily designed."]

C. Darwin to W. Graham.

Down, July 3rd, 1881.

DEAR SIR,

I hope that you will not think it intrusive on my part to thank you heartily for the pleasure which I have derived from reading your admirably written 'Creed of Science,' though I have not yet quite finished it, as now that I am old I read very slowly. It is a very long time since any other book has interested me so much. The work must have cost you several years and much hard labour with full leisure for work. You would not probably expect any one fully to agree with you on so many abstruse subjects; and there are some points in your book which I cannot digest. The chief one is that the existence of so-called natural laws implies purpose. I cannot see this. Not to mention that many expect that the several great laws will some day be found to follow inevitably from some one single law, yet taking the laws as we now know them, and look at the moon, where the law of gravitation—and no doubt of the conservation of energy—of the atomic theory, &c. &c., hold good, and I cannot see that there is then necessarily any purpose. Would there be purpose if the lowest organisms alone, destitute of con-

© The Complete Work of Charles Darwin Online

sciousness existed in the moon? But I have had no practice in abstract reasoning, and I may be all astray. Nevertheless you have expressed my inward conviction, though far more vividly and clearly than I could have done, that the Universe is not the result of chance.* But then with me the horrid doubt always arises whether the convictions of man's mind, which has been developed from the mind of the lower animals, are of any value or at all trustworthy. Would any one trust in the convictions of a monkey's mind, if there are any convictions in such a mind? Secondly, I think that I could make somewhat of a case against the enormous importance which you attribute to our greatest men; I have been accustomed to think, second, third, and fourth rate men of very high importance, at least in the case of Science. Lastly, I could show fight on natural selection having done and doing more for the progress of civilization than you seem inclined to admit. Remember what risk the nations of Europe ran, not so many centuries ago of being overwhelmed by the Turks, and how ridiculous such an idea now is! The more civilized so-called Caucasian races have beaten the Turkish hollow in the struggle for existence. Looking to the world at no very distant date, what an endless number of the lower races will have been eliminated by the higher civilized races throughout the world. But I will write no more, and not even mention the many points in your work which have

* The Duke of Argyll ('Good Words,' Ap. 1885, p. 244) has recorded a few words on this subject, spoken by my father in the last year of his life. ". . . in the course of that conversation I said to Mr. Darwin, with reference to some of his own remarkable works on the 'Fertilisation of Orchids,' and upon 'The Earthworms,' and various other observations he made of the wonderful contrivances for certain purposes in nature—I said it was impossible to look at these without seeing that they were the effect and the expression of mind. I shall never forget Mr. Darwin's answer. He looked at me very hard and said, 'Well, that often comes over me with overwhelming force; but at other times," and he shook his head vaguely, adding, "it seems to go away.'"

© The Complete Work of Charles Darwin Online

much interested me. I have indeed cause to apologise for troubling you with my impressions, and my sole excuse is the excitement in my mind which your book has aroused.

I beg leave to remain,
Dear Sir,
Yours faithfully and obliged,
CHARLES DARWIN.

[My father spoke little on these subjects, and I can contribute nothing from my own recollection of his conversation which can add to the impression here given of his attitude towards Religion. Some further idea of his views may, however, be gathered from occasional remarks in his letters.]*

* Dr. Aveling has published an account of a conversation with my father. I think that the readers of this pamphlet ('The Religious Views of Charles Darwin,' Free Thought Publishing Company, 1883) may be misled into seeing more resemblance than really existed between the positions of my father and Dr. Aveling: and I say this in spite of my conviction that Dr. Aveling gives quite fairly his impressions of my father's views. Dr. Aveling tried to show that the terms "Agnostic" and "Atheist" were practically equivalent—that an atheist is one who, without denying the existence of God, is without God, inasmuch as he is unconvinced of the existence of a Deity. My father's replies implied his preference for the unaggressive attitude of an Agnostic. Dr. Aveling seems (p. 5) to regard the absence of aggressiveness in my father's views as distinguishing them in an unessential manner from his own. But, in my judgment, it is precisely differences of this kind which distinguish him so completely from the class of thinkers to which Dr. Aveling belongs.

© The Complete Work of Charles Darwin Online

CHAPTER IX.

LIFE AT DOWN.

1842–1854.

"My life goes on like clockwork, and I am fixed on the spot where I shall end it."

Letter to Captain Fitz-Roy, October, 1846.

[WITH the view of giving, in the next volume, a connected account of the growth of the 'Origin of Species,' I have taken the more important letters bearing on that subject out of their proper chronological position here, and placed them with the rest of the correspondence bearing on the same subject; so that in the present group of letters we only get occasional hints of the growth of my father's views, and we may suppose ourselves to be looking at his life, as it might have been looked at by those who had no knowledge of the quiet development of his theory of evolution during this period.

On Sept. 14, 1842, my father left London with his family and settled at Down.* In the Autobiographical chapter, his motives for taking this step in the country are briefly given. He speaks of the attendance at scientific societies, and ordinary social duties, as suiting his health so "badly that

* I must not omit to mention a member of the household who accompanied him. This was his butler, Joseph Parslow, who remained in the family, a valued friend and servant, for forty years, and became, as Sir Joseph Hooker once remarked to me, "an integral part of the family, and felt to be such by all visitors at the house."

© The Complete Work of Charles Darwin Online

we resolved to live in the country, which we both preferred and have never repented of." His intention of keeping up with scientific life in London is expressed in a letter to Fox (Dec., 1842):—

"I hope by going up to town for a night every fortnight or three weeks, to keep up my communication with scientific men and my own zeal, and so not to turn into a complete Kentish hog."

Visits to London of this kind were kept up for some years at the cost of much exertion on his part. I have often heard him speak of the wearisome drives of ten miles to or from Croydon or Sydenham—the nearest stations—with an old gardener acting as coachman, who drove with great caution and slowness up and down the many hills. In later years, all regular scientific intercourse with London became, as before mentioned, an impossibility.

The choice of Down was rather the result of despair than of actual preference; my father and mother were weary of house-hunting, and the attractive points about the place thus seemed to them to counterbalance its somewhat more obvious faults. It had at least one desideratum, namely quietness. Indeed it would have been difficult to find a more retired place so near to London. In 1842 a coach drive of some twenty miles was the only means of access to Down; and even now that railways have crept closer to it, it is singularly out of the world, with nothing to suggest the neighbourhood of London, unless it be the dull haze of smoke that sometimes clouds the sky. The village stands in an angle between two of the larger high-roads of the country, one leading to Tunbridge and the other to Westerham and Edenbridge. It is cut off from the Weald by a line of steep chalk hills on the south, and an abrupt hill, now smoothed down by a cutting and embankment, must formerly have been something of a barrier against encroachments from the side of London. In such a situation, a village, communicating

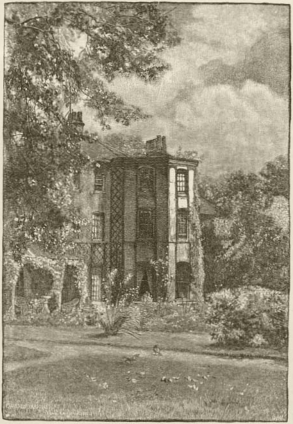
© The Complete Work of Charles Darwin Online

with the main lines of traffic, only by stony tortuous lanes, may well have been enabled to preserve its retired character. Nor is it hard to believe in the smugglers and their strings of pack-horses making their way up from the lawless old villages of the Weald, of which the memory still existed when my father settled in Down. The village stands on solitary upland country, 500 to 600 feet above the sea,—a country with little natural beauty, but possessing a certain charm in the shaws, or straggling strips of wood, capping the chalky banks and looking down upon the quiet ploughed lands of the valleys. The village, of three or four hundred inhabitants, consists of three small streets of cottages meeting in front of the little flint-built church. It is a place where new-comers are seldom seen, and the names occurring far back in the old church registers are still well known in the village. The smock-frock is not yet quite extinct, though chiefly used as a ceremonial dress by the "bearers" at funerals; but as a boy I remember the purple or green smocks of the men at church.

The house stands a quarter of a mile from the village, and is built, like so many houses of the last century, as near as possible to the road—a narrow lane winding away to the Westerham high-road. In 1842, it was dull and unattractive enough: a square brick building of three storeys, covered with shabby whitewash and hanging tiles. The garden had none of the shrubberies or walls that now give shelter; it was overlooked from the lane, and was open, bleak, and desolate. One of my father's first undertakings was to lower the lane by about two feet, and to build a flint wall along that part of it which bordered the garden. The earth thus excavated was used in making banks and mounds round the lawn: these were planted with evergreens, which now give to the garden its retired and sheltered character.

The house was made to look neater by being covered with

© The Complete Work of Charles Darwin Online



THE HOUSE AT DOWN. FROM A DRAWING BY MR. ALFRED PARSONS.
ENGRAVED FOR THE 'CENTURY MAGAZINE,' JANUARY 1883.

To face p. 320, *Vol. I.*

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

stucco, but the chief improvement effected was the building of a large bow extending up through three storeys. This bow became covered with a tangle of creepers, and pleasantly varied the south side of the house. The drawing-room, with its verandah opening into the garden, as well as the study in which my father worked during the later years of his life, were added at subsequent dates.

Eighteen acres of land were sold with the house, of which twelve acres on the south side of the house formed a pleasant field, scattered with fair-sized oaks and ashes. From this field a strip was cut off and converted into a kitchen garden, in which the experimental plot of ground was situated, and where the greenhouses were ultimately put up.

The following letter to Mr. Fox (March 28th, 1843) gives among other things my father's early impressions of Down:—

"I will tell you all the trifling particulars about myself that I can think of. We are now exceedingly busy with the first brick laid down yesterday to an addition to our house; with this, with almost making a new kitchen garden and sundry other projected schemes, my days are very full. I find all this very bad for geology, but I am very slowly progressing with a volume, or rather pamphlet, on the volcanic islands which we visited: I manage only a couple of hours per day, and that not very regularly. It is uphill work writing books, which cost money in publishing, and which are not read even by geologists. I forget whether I ever described this place: it is a good, very ugly house with 18 acres, situated on a chalk flat, 560 feet above sea. There are peeps of far distant country and the scenery is moderately pretty: its chief merit is its extreme rurality. I think I was never in a more perfectly quiet country. Three miles south of us the great chalk escarpment quite cuts us off from the low country of Kent, and between us and the escarpment there is not a village or gentleman's house, but only great woods and arable fields (the latter in sadly preponderant numbers), so that we are abso-

© The Complete Work of Charles Darwin Online

lutely at the extreme verge of the world. The whole country is intersected by foot-paths; but the surface over the chalk is clayey and sticky, which is the worst feature in our purchase. The dingles and banks often remind me of Cambridgeshire and walks with you to Cherry Hinton, and other places, though the general aspect of the country is very different. I was looking over my arranged cabinet (the only remnant I have preserved of all my English insects), and was admiring *Panagæus Crux-major*; it is curious the vivid manner in which this insect calls up in my mind your appearance, with little Fan trotting after, when I was first introduced to you. Those entomological days were very pleasant ones. I am *very* much stronger corporeally, but am little better in being able to stand mental fatigue, or rather excitement, so that I cannot dine out or receive visitors, except relations with whom I can pass some time after dinner in silence."

I could have wished to give here some idea of the position which, at this period of his life, my father occupied among scientific men and the reading public generally. But contemporary notices are few and of no particular value for my purpose,—which therefore must, in spite of a good deal of pains, remain unfulfilled.

His 'Journal of Researches' was then the only one of his books which had any chance of being commonly known. But the fact that it was published with the 'Voyages' of Captains King and Fitz-Roy probably interfered with its general popularity. Thus Lyell wrote to him in 1838 ('Lyell's Life,' ii. p. 43), "I assure you my father is quite enthusiastic about your journal . . . . and he agrees with me that it would have a large sale if published separately. He was disappointed at hearing that it was to be fettered by the other volumes, for, although he should equally buy it, he feared so many of the public would be checked from doing so." In a notice of the three voyages in the 'Edinburgh Review' (July, 1839), there is nothing leading a reader to believe that

© The Complete Work of Charles Darwin Online

he would find it more attractive than its fellow-volumes. And, as a fact, it did not become widely known until it was separately published in 1845. It may be noted, however, that the 'Quarterly Review' (December, 1839) called the attention of its readers to the merits of the 'Journal' as a book of travels. The reviewer speaks of the "charm arising from the freshness of heart which is thrown over these virgin pages of a strong intellectual man and an acute and deep observer."

The German translation (1844) of the 'Journal' received a favourable notice in No. 12 of the 'Heidelberger Jahrbücher der Literatur,' 1847—where the Reviewer speaks of the author's "varied canvas, on which he sketches in lively colours the strange customs of those distant regions with their remarkable fauna, flora and geological peculiarities." Alluding to the translation, my father writes—"Dr. Dieffenbach . . . has translated my 'Journal' into German, and I must, with unpardonable vanity, boast that it was at the instigation of Liebig and Humboldt."

The geological work of which he speaks in the above letter to Mr. Fox occupied him for the whole of 1843, and was published in the spring of the following year. It was entitled 'Geological Observations on the Volcanic Islands, visited during the voyage of H.M.S. *Beagle*, together with some brief notices on the geology of Australia and the Cape of Good Hope': it formed the second part of the 'Geology of the Voyage of the *Beagle*,' published "with the Approval of the Lords Commissioners of Her Majesty's Treasury." The volume on 'Coral Reefs' forms Part I. of the series, and was published, as we have seen, in 1842. For the sake of the non-geological reader, I may here quote Professor Geikie's words* on these two volumes—which were up to this time my father's chief geological works. Speaking of the 'Coral Reefs,' he says :—p. 17, "This well-known treatise, the most original of all its

* Charles Darwin, 'Nature' Series, 1882.

© The Complete Work of Charles Darwin Online

author's geological memoirs, has become one of the classics of geological literature. The origin of those remarkable rings of coral-rock in mid-ocean has given rise to much speculation, but no satisfactory solution of the problem had been proposed. After visiting many of them, and examining also coral reefs that fringe islands and continents, he offered a theory which for simplicity and grandeur strikes every reader with astonishment. It is pleasant, after the lapse of many years, to recall the delight with which one first read the 'Coral Reefs'; how one watched the facts being marshalled into their places, nothing being ignored or passed lightly over; and how, step by step, one was led to the grand conclusion of wide oceanic subsidence. No more admirable example of scientific method was ever given to the world, and even if he had written nothing else, the treatise alone would have placed Darwin in the very front of investigators of nature."

It is interesting to see in the following extract from one of Lyell's letters* how warmly and readily he embraced the theory. The extract also gives incidentally some idea of the theory itself.

"I am very full of Darwin's new theory of Coral Islands, and have urged Whewell to make him read it at our next meeting. I must give up my volcanic crater theory for ever, though it cost me a pang at first, for it accounted for so much, the annular form, the central lagoon, the sudden rising of an isolated mountain in a deep sea; all went so well with the notion of submerged, crateriform, and conical volcanoes, . . . and then the fact that in the South Pacific we had scarcely any rocks in the regions of coral islands, save two kinds, coral limestone and volcanic! Yet spite of all this, the whole theory is knocked on the head, and the annular shape and central lagoon have nothing to do with volcanoes, nor even with a crateriform bottom. Perhaps Darwin told you when at the

* To Sir John Herschel, May 24, 1837. 'Life of Sir Charles Lyell,' vol. ii. p. 12.

© The Complete Work of Charles Darwin Online

Cape what he considers the true cause? Let any mountain be submerged gradually, and coral grow in the sea in which it is sinking, and there will be a ring of coral, and finally only a lagoon in the centre. Why? For the same reason that a barrier reef of coral grows along certain coasts: Australia, &c. Coral islands are the last efforts of drowning continents to lift their heads above water. Regions of elevation and subsidence in the ocean may be traced by the state of the coral reefs." There is little to be said as to published contemporary criticism. The book was not reviewed in the 'Quarterly Review' till 1847, when a favourable notice was given. The reviewer speaks of the "bold and startling" character of the work, but seems to recognize the fact that the views are generally accepted by geologists. By that time the minds of men were becoming more ready to receive geology of this type. Even ten years before, in 1837, Lyell * says, "people are now much better prepared to believe Darwin when he advances proofs of the slow rise of the Andes, than they were in 1830, when I first startled them with that doctrine." This sentence refers to the theory elaborated in my father's geological observations on South America (1846), but the gradual change in receptivity of the geological mind must have been favourable to all his geological work. Nevertheless, Lyell seems at first not to have expected any ready acceptance of the Coral theory; thus he wrote to my father in 1837:—"I could think of nothing for days after your lesson on coral reefs, but of the tops of submerged continents. It is all true, but do not flatter yourself that you will be believed till you are growing bald like me, with hard work and vexation at the incredulity of the world."

The second part of the 'Geology of the Voyage of the *Beagle*,' *i.e.* the volume on Volcanic Islands, which specially concerns us now, cannot be better described than by again quoting from Professor Geikie (p. 18):—

* 'Life of Sir Charles Lyell,' vol. ii. p. 6.

© The Complete Work of Charles Darwin Online

"Full of detailed observations, this work still remains the best authority on the general geological structure of most of the regions it describes. At the time it was written the 'crater of elevation theory,' though opposed by Constant Prévost, Scrope, and Lyell, was generally accepted, at least on the Continent. Darwin, however, could not receive it as a valid explanation of the facts; and though he did not share the view of its chief opponents, but ventured to propose a hypothesis of his own, the observations impartially made and described by him in this volume must be regarded as having contributed towards the final solution of the difficulty." Professor Geikie continues (p. 21): "He is one of the earliest writers to recognize the magnitude of the denudation to which even recent geological accumulations have been subjected. One of the most impressive lessons to be learnt from his account of 'Volcanic Islands' is the prodigious extent to which they have been denuded. . . . He was disposed to attribute more of this work to the sea than most geologists would now admit; but he lived himself to modify his original views, and on this subject his latest utterances are quite abreast of the time."

An extract from a letter of my father's to Lyell shows his estimate of his own work. "You have pleased me much by saying that you intend looking through my 'Volcanic Islands': it cost me eighteen months!!! and I have heard of very few who have read it. Now I shall feel, whatever little (and little it is) there is confirmatory of old work, or new, will work its effect and not be lost."

The third of his geological books, 'Geological Observations on South America,' may be mentioned here, although it was not published until 1846. "In this work the author embodied all the materials collected by him for the illustration of South American Geology, save some which had been published elsewhere. One of the most important features of the book was the evidence which it brought forward to prove the slow

© The Complete Work of Charles Darwin Online

interrupted elevation of the South American Continent during a recent geological period." *

Of this book my father wrote to Lyell :—"My volume will be about 240 pages, dreadfully dull, yet much condensed. I think whenever you have time to look through it, you will think the collection of facts on the elevation of the land and on the formation of terraces pretty good."

Of his special geological work as a whole, Professor Geikie, while pointing out that it was not "of the same epoch-making kind as his biological researches," remarks that he "gave a powerful impulse to" the general reception of Lyell's teaching "by the way in which he gathered from all parts of the world facts in its support."

WORK OF THE PERIOD 1842 TO 1854.

The work of these years may be roughly divided into a period of geology from 1842 to 1846, and one of zoology from 1846 onwards.

I extract from his diary notices of the time spent on his geological books and on his 'Journal.'

'Volcanic Islands.' Summer of 1842 to January, 1844.

'Geology of South America.' July, 1844, to April, 1845.

Second Edition of 'The Journal,' October, 1845, to October, 1846.

The time between October, 1846, and October, 1854, was practically given up to working at the Cirripedia (Barnacles); the results were published in two volumes by the Ray Society in 1851 and 1854. His volumes on the Fossil Cirripedes were published by the Palæontographical Society in 1851 and 1854.

Some account of these volumes will be given later.

The minor works may be placed together, independently, of subject matter.

"Observations on the Structure, &c., of the genus Sagitta," Ann. Nat. Hist. xiii., 1844, pp. 1–6.

* Geikie, *loc. cit.*

© The Complete Work of Charles Darwin Online

"Brief Descriptions of several Terrestrial Planariæ, &c.," Ann. Nat. Hist. xiv., 1844, pp. 241–251.

"An Account [of the Fine Dust * which often Falls on Vessels in the Atlantic Ocean," Geol. Soc. Journ. ii., 1846, pp. 26–30.

"On the Geology of the Falkland Islands," Geol. Soc. Journ. ii., 1846, pp. 267–274.

"On the Transportal of Erratic Boulders, &c.," Geol. Soc. Journ. iv. 1848, pp. 315–323.†

The article "Geology," in the Admiralty Manual of Scientific Enquiry (1849), pp. 156–195. This was written in the spring of 1848.

"On British Fossil Lepadidæ," 'Geol. Soc. Journ.' vi., 1850, pp. 439–440.

"Analogy of the structure of some Volcanic Rocks with that of Glaciers," 'Edin. Roy. Soc. Proc.' ii., 1851, pp. 17–18.

Professor Geikie has been so good as to give me (in a letter dated Nov. 1885) his impressions of my father's article in the 'Admiralty Manual.' He mentions the following points as characteristic of the work :—

* A sentence occurs in this paper of interest, as showing that the author was alive to the importance of all means of distribution :—"The fact that particles of this size having been brought at least 330 miles from the land is interesting as bearing on the distribution of Cryptogamic plants."

† An extract from a letter to Lyell, 1847, is of interest in connection with this essay :—"Would you be so good (if you know it) as to put Maclaren's address on the enclosed letter and post it. It is chiefly to enquire in what paper he has described the Boulders on Arthur's Seat. Mr. D. Milne in the last Edinburgh 'New Phil. Journal' [1847], has a long paper on it. He says : 'Some glacialists have ventured to explain the transportation of boulders even in the situation of those now referred to, by imagining that they were transported on ice floes,' &c. He treats this view, and the scratching of rocks by icebergs, as almost absurd . . . he has finally stirred me up so, that (without you would answer him) I think I will send a paper in opposition to the same Journal. I can thus introduce some old remarks of mine, and some new, and will insist on your capital observations in N. America. It is a bore to stop one's work, but he has made me quite wroth"

© The Complete Work of Charles Darwin Online

"1. Great breadth of view. No one who had not practically studied and profoundly reflected on the questions discussed could have written it.

"2. The insight so remarkable in all that Mr. Darwin ever did. The way in which he points out lines of enquiry that would elucidate geological problems is eminently typical of him. Some of these lines have never yet been adequately followed; so with regard to them he was in advance of his time.

"3. Interesting and sympathetic treatment. The author at once puts his readers into harmony with him. He gives them enough of information to show how delightful the field is to which he invites them, and how much they might accomplish in it. There is a broad sketch of the subject which everybody can follow, and there is enough of detail to instruct and guide a beginner and start him on the right track.

"Of course, geology has made great strides since 1849, and the article, if written now, would need to take notice of other branches of enquiry, and to modify statements which are not now quite accurate; but most of the advice Mr. Darwin gives is as needful and valuable now as when it was given. It is curious to see with what unerring instinct he seems to have fastened on the principles that would stand the test of time."

In a letter to Lyell (1853) my father wrote, "I went up for a paper by the Arctic Dr. Sutherland, on ice action, read only in abstract, but I should think with much good matter. It was very pleasant to hear that it was written owing to the Admiralty Manual."

To give some idea of the retired life which now began for my father at Down, I have noted from his diary the short periods during which he was away from home between the autumn of 1842, when he came to Down, and the end of 1854.

© The Complete Work of Charles Darwin Online

1843, *July.*—Week at Maer and Shrewsbury.
„ *October.*—Twelve days at Shrewsbury.
1844, *April.*—Week at Maer and Shrewsbury.
„ *July.*—Twelve days at Shrewsbury.
1845, *September* 15.—Six weeks, "Shrewsbury, Lincolnshire York, the Dean of Manchester, Waterton, Chatsworth."
1846, *February.*—Eleven days at Shrewsbury.
„ *July.*—Ten days at Shrewsbury.
„ *September.*—Ten days at Southampton, &c., for the British Association.
1847, *February.*—Twelve days at Shrewsbury.
„ *June.*—Ten days at Oxford, &c., for the British Association.
„ *October.*—Fortnight at Shrewsbury.
1848, *May.*—Fortnight at Shrewsbury.
„ *July.*—Week at Swanage.
„ *October.*—Fortnight at Shrewsbury.
„ *November.*—Eleven days at Shrewsbury.
1849, *March to June.*—Sixteen weeks at Malvern.
„ *September.*—Eleven days at Birmingham for the British Association.
1850, *June.*—Week at Malvern.
„ *August.*—Week at Leith Hill, the house of a relative.
„ *October.*—Week at the house of another relative.
1851, *March.*—Week at Malvern.
„ *April.*—Nine days at Malvern.
„ *July.*—Twelve days in London.
1852, *March.*—Week at Rugby and Shrewsbury.
„ *September.*—Six days at the house of a relative.
1853, *July.*—Three weeks at Eastbourne.
„ *August.*—Five days at the military Camp at Chobham.
1854, *March.*—Five days at the house of a relative.
„ *July.*—Three days at the house of a relative.
„ *October.*—Six days at the house of a relative.

© The Complete Work of Charles Darwin Online

It will be seen that he was absent from home sixty weeks in twelve years. But it must be remembered that much of the remaining time spent at Down was lost through ill-health.]

LETTERS.

C. Darwin to R. Fitz-Roy.

Down [March, 31st, 1843].

DEAR FITZ-ROY,—I read yesterday with surprise and the greatest interest, your appointment as Governor of New Zealand. I do not know whether to congratulate you on it, but I am sure I may the Colony, on possessing your zeal and energy. I am most anxious to know whether the report is true, for I cannot bear the thoughts of your leaving the country without seeing you once again; the past is often in my memory, and I feel that I owe to you much bygone enjoyment, and the whole destiny of my life, which (had my health been stronger) would have been one full of satisfaction to me. During the last three months I have never once gone up to London without intending to call in the hopes of seeing Mrs. Fitz-Roy and yourself; but I find, most unfortunately for myself, that the little excitement of breaking out of my most quiet routine so generally knocks me up, that I am able to do scarcely anything when in London, and I have not even been able to attend one evening meeting of the Geological Society. Otherwise, I am very well, as are, thank God, my wife and two children. The extreme retirement of this place suits us all very well, and we enjoy our country life much. But I am writing trifles about myself, when your mind and time must be fully occupied. My object in writing is to beg of you or Mrs. Fitz-Roy to have the kindness to send me one line to say whether it is true, and whether you sail soon. I shall come up next week for one or two days; could you see me for even five minutes, if I called early on Thursday morning,

© The Complete Work of Charles Darwin Online

viz. at nine or ten o'clock, or at whatever hour (if you keep early ship hours) you finish your breakfast. Pray remember me very kindly to Mrs. Fitz-Roy, who I trust is able to look at her long voyage with boldness.

Believe me, dear Fitz-Roy,
Your ever truly obliged,
CHARLES DARWIN.

[A quotation from another letter (1846) to Fitz-Roy may be worth giving, as showing my father's affectionate remembrance of his old Captain.

"Farewell, dear Fitz-Roy, I often think of your many acts of kindness to me, and not seldomest on the time, no doubt quite forgotten by you, when, before making Madeira, you came and arranged my hammock with your own hands, and which, as I afterwards heard, brought tears into my father's eyes."]

C. Darwin to W. D. Fox.

[Down, September 5, 1843.]
Monday morning.

MY DEAR FOX,—When I sent off the glacier paper, I was just going out and so had no time to write. I hope your friend will enjoy (and I wish you were going there with him) his tour as much as I did. It was a kind of geological novel. But your friend must have patience, for he will not get a good *glacial eye* for a few days. Murchison and Count Keyserling *rushed* through North Wales the same autumn and could see nothing except the effects of rain trickling over the rocks! I cross-examined Murchison a little, and evidently saw he had looked carefully at nothing. I feel *certain* about the glacier-effects in North Wales. Get up your steam, if this weather lasts, and have a ramble in Wales; its glorious scenery must do every one's heart and body good. I wish I had energy to come to Delamere and go with you; but as you observe, you might as

© The Complete Work of Charles Darwin Online

well ask St. Paul's. Whenever I give myself a trip, it shall be, I think, to Scotland, to hunt for more parallel roads. My marine theory for these roads was for a time knocked on the head by Agassiz ice-work, but it is now reviving again. . . . .

Farewell,—we are getting nearly finished—almost all the workmen gone, and the gravel laying down on the walks. Ave Maria! how the money does go. There are twice as many temptations to extravagance in the country compared with London. Adios.

Yours,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, [1844?]

. . . . I have also read the 'Vestiges,'* but have been somewhat less amused at it than you appear to have been: the writing and arrangement are certainly admirable, but his geology strikes me as bad, and his zoology far worse. I should be very much obliged, if at any future or leisure time you could tell me on what you ground your doubtful belief in imagination of a mother affecting her offspring.† I have attended to the several statements scattered about, but do not

* 'The Vestiges of the Natural History of Creation,' was published anonymously in 1844, and is confidently believed to have been written by the late Robert Chambers. My father's copy gives signs of having been carefully read, a long list of marked passages being pinned in at the end. One useful lesson he seems to have learned from it. He writes: "The idea of a fish passing into a reptile, monstrous. I will not specify any genealogies—much too little known at present." He refers again to the book in a letter to Fox, February, 1845: "Have you read that strange, unphilosophical, but capitally-written book, the 'Vestiges': it has made more talk than any work of late, and has been by some attributed to me—at which I ought to be much flattered and unflattered."

† This refers to the case of a relative of Sir J. Hooker's, who insisted that a mole, which appeared on one of her children, was the effect of fright upon herself on having, before the birth of the child, blotted with sepia a copy of Turner's 'Liber Studiorum' that had been lent to her with special injunctions to be careful.

© The Complete Work of Charles Darwin Online

believe in more than accidental coincidences. W. Hunter told my father, then in a lying-in hospital, that in many thousand cases, he had asked the mother, *before her confinement*, whether anything had affected her imagination, and recorded the answers; and absolutely not one case came right, though, when the child was anything remarkable, they afterwards made the cap to fit. Reproduction seems governed by such similar laws in the whole animal kingdom, that I am most loth [to believe]. . . .

C. Darwin to J. M. Herbert.

Down, [1844 or 1845].

MY DEAR HERBERT,—I was very glad to see your handwriting and hear a bit of news about you. Though you cannot come here this autumn, I do hope you and Mrs. Herbert will come in the winter, and we will have lots of talk of old times, and lots of Beethoven.

I have little or rather nothing to say about myself; we live like clock-work, and in what most people would consider the dullest possible manner. I have of late been slaving extra hard, to the great discomfiture of wretched digestive organs, at South America, and thank all the fates, I have done three-fourths of it. Writing plain English grows with me more and more difficult, and never attainable. As for your pretending that you will read anything so dull as my pure geological descriptions, lay not such a flattering unction on my soul * for it is incredible. I have long discovered that geologists never read each other's works, and that the only object in writing a book is a proof of earnestness, and that you do not form your opinions without undergoing labour of

* On the same subject he wrote to Fitz-Roy: "I have sent my 'South American Geology' to Dover Street, and you will get it, no doubt, in the course of time. You do not know what you threaten when you propose to read it—it is purely geological. I said to my brother, 'You will of course read it,' and his answer was, 'Upon my life, I would sooner even buy it.'"

© The Complete Work of Charles Darwin Online

some kind. Geology is at present very oral, and what I here say is to a great extent quite true. But I am giving you a discussion as long as a chapter in the odious book itself.

I have lately been to Shrewsbury, and found my father surprisingly well and cheerful.

Believe me, my dear old friend, ever yours,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Monday [February 10th, 1845].

MY DEAR HOOKER,—I am much obliged for your very agreeable letter; it was very good-natured, in the midst of your scientific and theatrical dissipation, to think of writing so long a letter to me. I am astonished at your news, and I must condole with you in your *present* view of the Professorship,* and most heartily deplore it on my own account. There is something so chilling in a separation of so many hundred miles, though we did not see much of each other when nearer. You will hardly believe how deeply I regret for *myself* your present prospects. I had looked forward to [our] seeing much of each other during our lives. It is a heavy disappointment; and in a mere selfish point of view, as aiding me in my work, your loss is indeed irreparable. But, on the other hand, I cannot doubt that you take at present a desponding, instead of bright, view of your prospects: surely there are great advantages, as well as disadvantages. The place is one of eminence; and really it appears to me there are so many indifferent workers, and so few readers, that it is a high advantage, in a purely scientific point of view, for a good worker to hold a position which leads others to attend to his work. I forget whether you attended Edinburgh, as a student, but in my time there was a knot of men who were far from being the indifferent

* Sir J. D. Hooker was a candidate for the Professorship of Botany at Edinburgh University.

© The Complete Work of Charles Darwin Online

and dull listeners which you expect for your audience. Reflect what a satisfaction and honour it would be to *make* a good botanist—with your disposition you will be to many what Henslow was at Cambridge to me and others, a most kind friend and guide. Then what a fine garden, and how good a Public Library! why, Forbes always regrets the advantages of Edinburgh for work: think of the inestimable advantage of getting within a short walk of those noble rocks and hills and sandy shores near Edinburgh! Indeed, I cannot pity you much, though I pity myself exceedingly in your loss. Surely lecturing will, in a year or two, with your *great* capacity for work (whatever you may be pleased to say to the contrary) become easy, and you will have a fair time for your Antarctic Flora and general views of distribution. If I thought your Professorship would stop your work, I should wish it and all the good worldly consequences at *el Diavolo*. I know I shall live to see you the first authority in Europe on that grand subject, that almost keystone of the laws of creation, Geographical Distribution. Well, there is one comfort, you will be at Kew, no doubt, every year, so I shall finish by forcing down your throat my sincere congratulations. Thanks for all your news. I grieve to hear Humboldt is failing; one cannot help feeling, though unrightly, that such an end is humiliating: even when I saw him he talked beyond all reason. If you see him again, pray give him my most respectful and kind compliments, and say that I never forget that my whole course of life is due to having read and re-read as a youth his 'Personal Narrative.' How true and pleasing are all your remarks on his kindness; think how many opportunities you will have, in your new place, of being a Humboldt to others. Ask him about the river in N.E. Europe, with the Flora very different on its opposite banks. I have got and read your Wilkes; what a feeble book in matter and style, and how splendidly got up! Do write me a line from Berlin. Also thanks for the proof-sheets. I did

© The Complete Work of Charles Darwin Online

not, however, mean proof plates; I value them, as saving me copying extracts. Farewell, my dear Hooker, with a heavy heart I wish you joy of your prospects.

Your sincere friend,

C. DARWIN.

[The second edition of the 'Journal,' to which the following letter refers, was completed between April 25th and August 25th. It was published by Mr. Murray in the 'Colonial and Home Library,' and in this more accessible form soon had a large sale.

Up to the time of his first negotiations with Mr. Murray for its publication in this form, he had received payment only in the form of a large number of presentation copies, and he seems to have been glad to sell the copyright of the second edition to Mr. Murray for 150*l.*

The points of difference between it and the first edition are of interest chiefly in connection with the growth of the author's views on evolution, and will be considered later.]

C. Darwin to C. Lyell.

Down [July, 1845].

MY DEAR LYELL,—I send you the first part * of the new edition [of the 'Journal of Researches'], which I so entirely owe to you. You will see that I have ventured to dedicate it to you,† and I trust that this cannot be disagreeable. I have long wished, not so much for your sake, as for my own feelings of honesty, to acknowledge more plainly than by mere reference, how much I geologically owe you. Those authors, however,

* No doubt proof-sheets.

† The dedication of the second edition of the 'Journal of Researches,' is as follows: — "To Charles Lyell, Esq., F.R.S., this second edition is dedicated with grateful pleasure—as an acknowledgment that the chief part of whatever scientific merit this Journal and the other works of the Author may possess, has been derived from studying the well-known and admirable 'Principles of Geology.'"

© The Complete Work of Charles Darwin Online

who like you, educate people's minds as well as teach them special facts, can never, I should think, have full justice done them except by posterity, for the mind thus insensibly improved can hardly perceive its own upward ascent. I had intended putting in the present acknowledgment in the third part of my Geology, but its sale is so exceedingly small that I should not have had the satisfaction of thinking that as far as lay in my power I had owned, though imperfectly, my debt. Pray do not think that I am so silly, as to suppose that my dedication can any ways gratify you, except so far as I trust you will receive it, as a most sincere mark of my gratitude and friendship. I think I have improved this edition, especially the second part, which I have just finished. I have added a good deal about the Fuegians, and cut down into half the mercilessly long discussion on climate and glaciers, &c. I do not recollect anything added to the first part, long enough to call your attention to; there is a page of description of a very curious breed of oxen in Banda Oriental. I should like you to read the few last pages; there is a little discussion on extinction, which will not perhaps strike you as new, though it has so struck me, and has placed in my mind all the difficulties with respect to the causes of extinction, in the same class with other difficulties which are generally quite overlooked and undervalued by naturalists; I ought, however, to have made my discussion longer and shewn by facts, as I easily could, how steadily every species must be checked in its numbers.

I received your Travels * yesterday; and I like exceedingly its external and internal appearance; I read only about a dozen pages last night (for I was tired with hay-making), but I saw quite enough to perceive how *very* much it will interest me, and how many passages will be scored. I am pleased to find a good sprinkling of Natural History; I shall be astonished if it does not sell very largely. . . .

* 'Travels in North America,' 2 vols., 1845.

© The Complete Work of Charles Darwin Online

How sorry I am to think that we shall not see you here again for so long; I wish you may knock yourself a little bit up before you start and require a day's fresh air, before the ocean breezes blow on you. . . .

Ever yours,
C. DARWIN.

C. Darwin to C. Lyell.

Down, Saturday [August 1st, 1845].

MY DEAR LYELL,—I have been wishing to write to you for a week past, but every five minutes' worth of strength has been expended in getting out my second part.* Your note pleased me a good deal more I dare say than my dedication did you, and I thank you much for it. Your work has interested me much, and I will give you my impressions, though, as I never thought you would care to hear what I thought of the non-scientific parts, I made no notes, nor took pains to remember any particular impression of two-thirds of the first volume. The first impression I should say would be with most (though I have literally seen not one soul since reading it) regret at there not being more of the non-scientific [parts]. I am not a good judge, for I have read nothing, *i.e.* non-scientific about North America, but the whole struck me as very new, fresh, and interesting. Your discussions bore to my mind the evident stamp of matured thought, and of conclusions drawn from facts observed by yourself, and not from the opinions of the people whom you met; and this I suspect is comparatively rare.

Your slave discussion disturbed me much; but as you would care no more for my opinion on this head than for the ashes of this letter, I will say nothing except that it gave me some sleepless, most uncomfortable hours. Your account of the religious state of the States particularly interested me; I am surprised throughout at your very proper boldness against

* Of the second edition of the 'Journal of Researches.'

© The Complete Work of Charles Darwin Online

the Clergy. In your University chapter the Clergy, and not the State of Education, are most severely and justly handled, and this I think is very bold, for I conceive you might crush a leaden-headed old Don, as a Don, with more safety, than touch the finger of that Corporate Animal, the Clergy. What a contrast in Education does England shew itself! Your apology (using the term, like the old religionists who meant anything but an apology) for lectures, struck me as very clever; but all the arguments in the world on your side, are not equal to one course of Jamieson's Lectures on the other side, which I formerly for my sins experienced. Although I had read about the 'Coalfields in North America,' I never in the smallest degree really comprehended their area, their thickness and favourable position; nothing hardly astounded me more in your book.

Some few parts struck me as rather heterogenous, but I do not know whether to an extent that at all signified. I missed however, a good deal, some general heading to the chapters, such as the two or three principal places visited. One has no right to expect an author to write down to the zero of geographical ignorance of the reader; but I not knowing a single place, was occasionally rather plagued in tracing your course. Sometimes in the beginning of a chapter, in one paragraph your course was traced through a half dozen places; anyone, as ignorant as myself, if he could be found, would prefer such a disturbing paragraph left out. I cut your map loose, and I found that a great comfort; I could not follow your engraved track. I think in a second edition, interspaces here and there of one line open, would be an improvement. By the way, I take credit to myself in giving my Journal a less scientific air in having printed all names of species and genera in Romans; the printing looks, also, better. All the illustrations strike me as capital, and the map is an admirable volume in itself. If your 'Principles' had not met with such universal admiration, I should have feared there would have been too much geology

© The Complete Work of Charles Darwin Online

in this for the general reader; certainly all that the most clear and light style could do, has been done. To myself the geology was an excellent, well-condensed, well-digested *résumé* of all that has been made out in North America, and every geologist ought to be grateful to you. The summing up of the Niagara chapter appeared to me the grandest part; I was also deeply interested by your discussion on the origin of the Silurian formations. I have made scores of *scores* marking passages hereafter useful to me.

All the coal theory appeared to me very good; but it is no use going on enumerating in this manner. I wish there had been more Natural History; I liked *all* the scattered fragments. I have now given you an exact transcript of my thoughts, but they are hardly worth your reading. . . .

C. Darwin to C. Lyell.

Down, August 25th [1845].

MY DEAR LYELL,—This is literally the first day on which I have had any time to spare; and I will amuse myself by beginning a letter to you. . . .[1]

I was delighted with your letter in which you touch on Slavery; I wish the same feelings had been apparent in your published discussion. But I will not write on this subject, I should perhaps annoy you, and most certainly myself. I have exhaled myself with a paragraph or two in my Journal on the sin of Brazilian slavery; you perhaps will think that it is in answer to you; but such is not the case. I have remarked on nothing which I did not hear on the coast of South America. My few sentences, however, are merely an explosion of feeling. How could you relate so placidly that atrocious sentiment * about separating children from their parents; and in the next page speak of being distressed at the whites not having prospered; I assure you the contrast

* In the passage referred to, Lyell does not give his own views, but those of a planter.

© The Complete Work of Charles Darwin Online

made me exclaim out. But I have broken my intention, and so no more on this odious deadly subject.

There is a favourable, but not strong enough review on you, in the *Gardeners' Chronicle*. I am sorry to see that Lindley abides by the carbonic acid gas theory. By the way, I was much pleased by Lindley picking out my extinction paragraphs and giving them uncurtailed. To my mind, putting the comparative rarity of existing species in the same category with extinction has removed a great weight; though of course it does not explain anything, it shows that until we can explain comparative rarity, we ought not to feel any surprise at not explaining extinction. . . .

I am much pleased to hear of the call for a new edition of the 'Principles': what glorious good that work has done. I fear this time you will not be amongst the old rocks; how I should rejoice to live to see you publish and discover another stage below the Silurian—it would be the grandest step possible, I think. I am very glad to hear what progress Bunbury is making in fossil Botany; there is a fine hiatus for him to fill up in this country. I will certainly call on him this winter. . . . From what little I saw of him, I can quite believe everything which you say of his talents. . . .

C. Darwin to J. D. Hooker.

Shrewsbury, [1845 ?]

MY DEAR HOOKER,—I have just received your note, which has astonished me, and has most truly grieved me. I never for one minute doubted of your success, for I most erroneously imagined, that merit was sure to gain the day. I feel most sure that the day will come soon, when those who have voted against you, if they have any shame or conscience in them, will be ashamed at having allowed politics to blind their eyes to your qualifications, and those qualifications vouched for by Humboldt and Brown! Well, those testimonials must be a

© The Complete Work of Charles Darwin Online

consolation to you. *Proh pudor!* I am vexed and indignant by turns. I cannot even take comfort in thinking that I shall see more of you, and extract more knowledge from your well-arranged stock. I am pleased to think, that after having read a few of your letters, I never once doubted the position you will ultimately hold amongst European Botanists. I can think about nothing else, otherwise I should like [to] discuss 'Cosmos'* with you. I trust you will pay me and my wife a visit this autumn at Down. I shall be at Down on the 24th, and till then moving about.

My dear Hooker, allow me to call myself

Your very true friend,

C. DARWIN.

C. Darwin to C. Lyell.

October 8th [1845] Shrewsbury.

. . . I have lately been taking a little tour to see a farm I have purchased in Lincolnshire,† and then to York, where I visited the Dean of Manchester,‡ the great maker of Hybrids, who gave me much curious information. I also visited Waterton at Walton Hall, and was extremely amused at my visit

* A translation of Humboldt's 'Kosmos.'

† He speaks of his Lincolnshire farm in a letter to Henslow (July 4th):—"I have bought a farm in Lincolnshire, and when I go there this autumn, I mean to see what I can do in providing any cottage on my small estate with gardens. It is a hopeless thing to look to, but I believe few things would do this country more good in future ages than the destruction of primogeniture, so as to lessen the difference in land-wealth, and make more small freeholders. How atrociously unjust are the stamp laws, which render it so expensive for the poor man to buy his quarter of an acre; it makes one's blood burn with indignation."

‡ Hon. and Rev. W. Herbert. The visit is mentioned in a letter to Dr. Hooker:—"I have been taking a little tour, partly on business, and visited the Dean of Manchester, and had very much interesting talk with him on hybrids, sterility, and variation, &c. &c. He is full of self-gained knowledge, but knows surprisingly little what others have done on the same subjects. He is very heterodox on 'species': not much better, as most naturalists would esteem it, than poor Mr. Vestiges."

© The Complete Work of Charles Darwin Online

there. He is an amusing strange fellow; at our early dinner, our party consisted of two Catholic priests and two Mulattresses! He is past sixty years old, and the day before ran down and caught a leveret in a turnip-field. It is a fine old house, and the lake swarms with water-fowl. I then saw Chatsworth, and was in transport with the great hothouse; it is a perfect fragment of a tropical forest, and the sight made me think with delight of old recollections. My little ten-day tour made me feel wonderfully strong at the time, but the good effects did not last. My wife, I am sorry to say, does not get very strong, and the children are the hope of the family, for they are all happy, life, and spirits. I have been much interested with Sedgwick's review; * though I find it is far from popular with our scientific readers. I think some few passages savour of the dogmatism of the pulpit, rather than of the philosophy of the Professor's Chair; and some of the wit strikes me as only worthy of —— in the 'Quarterly.' Nevertheless, it is a grand piece of argument against mutability of species, and I read it with fear and trembling, but was well pleased to find that I had not overlooked any of the arguments, though I had put them to myself as feebly as milk and water. Have you read 'Cosmos' yet? The English translation is wretched, and the semi-metaphysico-politico descriptions in the first part are barely intelligible; but I think the volcanic discussion well worth your attention, it has astonished me by its vigour and information. I grieve to find Humboldt an adorer of Von Buch, with his classification of volcanos, craters of elevation, &c. &c., and carbonic acid gas atmosphere. He is indeed a wonderful man.

I hope to get home in a fortnight and stick to my wearyful South America till I finish it. I shall be very anxious to hear how you get on from the Horners, but you must not think of wasting your time by writing to me. We shall miss, indeed,

* Sedgwick's review of the 'Vestiges of Creation' in the 'Edinburgh Review,' July 1845.

© The Complete Work of Charles Darwin Online

your visits to Down, and I shall feel a lost man in London without my morning "house of call" at Hart Street. . . .

Believe me, my dear Lyell, ever yours,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Farnborough, Kent,
Thursday, September, 1846.

MY DEAR HOOKER,—I hope this letter will catch you at Clifton, but I have been prevented writing by being unwell, and having had the Horners here as visitors, which, with my abominable press-work, has fully occupied my time. It is, indeed, a long time since we wrote to each other; though, I beg to tell you, that I wrote last, but what about I cannot remember, except, I know, it was after reading your last numbers,* and I sent you a uniquely laudatory epistle, considering it was from a man who hardly knows a Daisy from a Dandelion to a professed Botanist. . . .

I cannot remember what papers have given me the impression, but I have that, which you state to be the case, firmly fixed on my mind, namely, the little chemical importance of the soil to its vegetation. What a strong fact it is, as R. Brown once remarked to me, of certain plants being calcareous ones here, which are not so under a more favourable climate on the Continent, or the reverse, for I forget which; but you, no doubt, will know to what I refer. By-the-way, there are some such cases in Herbert's paper in the 'Horticultural Journal.'† Have you read it: it struck me as extremely original, and bears *directly* on your present researches.‡ To a *non-botanist* the chalk has the most peculiar aspect of any flora in England; why will you not come here to make your observations? *We* go to Southampton, if my

* Hooker's Antarctic Botany.

† 'Journal of the Horticultural Society,' 1846.

‡ Sir J. Hooker was at this time attending to polymorphism, variability, &c.

© The Complete Work of Charles Darwin Online

courage and stomach do not fail, for the Brit. Assoc. (Do you not consider it your duty to be there?) And why cannot you come here afterwards and *work?* . . . .

THE MONOGRAPH OF THE CIRRIPEDIA,

October 1846 *to October* 1854.

[Writing to Sir J. D. Hooker in 1845, my father says: "I hope this next summer to finish my South American Geology, then to get out a little Zoology, and hurrah for my species work. . ." This passage serves to show that he had at this time no intention of making an exhaustive study of the Cirripedes. Indeed it would seem that his original intention was, as I learn from Sir J. D. Hooker, merely to work out one special problem. This is quite in keeping with the following passage in the Autobiography: "When on the coast of Chile, I found a most curious form, which burrowed into the shells of Concholepas, and which differed so much from all other Cirripedes that I had to form a new sub-order for its sole reception. . . . To understand the structure of my new Cirripede I had to examine and dissect many of the common forms; and this gradually led me on to take up the whole group." In later years he seems to have felt some doubt as to the value of these eight years of work,—for instance when he wrote in his Autobiography—"My work was of considerable use to me, when I had to discuss in the 'Origin of Species' the principles of a natural classification. Nevertheless I doubt whether the work was worth the consumption of so much time." Yet I learn from Sir J. D. Hooker that he certainly recognised at the time its value to himself as systematic training. Sir Joseph writes to me: "Your father recognised three stages in his career as a biologist: the mere collector at Cambridge; the collector and observer in the *Beagle*, and for some years afterwards; and the trained naturalist after, and only after the Cirripede

© The Complete Work of Charles Darwin Online

work. That he was a thinker all along is true enough, and there is a vast deal in his writings previous to the Cirripedes that a trained naturalist could but emulate. . . . He often alluded to it as a valued discipline, and added that even the 'hateful' work of digging out synonyms, and of describing, not only improved his methods but opened his eyes to the difficulties and merits of the works of the dullest of cataloguers. One result was that he would never allow a depreciatory remark to pass unchallenged on the poorest class of scientific workers, provided that their work was honest, and good of its kind. I have always regarded it as one of the finest traits of his character,—this generous appreciation of the hod-men of science, and of their labours . . . and it was monographing the Barnacles that brought it about."

Professor Huxley allows me to quote his opinion as to the value of the eight years given to the Cirripedes :—

"In my opinion your sagacious father never did a wiser thing than when he devoted himself to the years of patient toil which the Cirripede-book cost him.

"Like the rest of us, he had no proper training in biological science, and it has always struck me as a remarkable instance of his scientific insight, that he saw the necessity of giving himself such training, and of his courage, that he did not shirk the labour of obtaining it.

"The great danger which besets all men of large speculative faculty, is the temptation to deal with the accepted statements of fact in natural science, as if they were not only correct, but exhaustive; as if they might be dealt with deductively, in the same way as propositions in Euclid may be dealt with. In reality, every such statement, however true it may be, is true only relatively to the means of observation and the point of view of those who have enunciated it. So far it may be depended upon. But whether it will bear every speculative conclusion that may be logically deduced from it, is quite another question.

© The Complete Work of Charles Darwin Online

"Your father was building a vast superstructure upon the foundations furnished by the recognised facts of geological and biological science. In Physical Geography, in Geology proper, in Geographical Distribution, and in Palæontology, he had acquired an extensive practical training during the voyage of the *Beagle*. He knew of his own knowledge the way in which the raw materials of these branches of science are acquired, and was therefore a most competent judge of the speculative strain they would bear. That which he needed, after his return to England, was a corresponding acquaintance with Anatomy and Development, and their relation to Taxonomy—and he acquired this by his Cirripede work.

"Thus, in my apprehension, the value of the Cirripede monograph lies not merely in the fact that it is a very admirable piece of work, and constituted a great addition to positive knowledge, but still more in the circumstance that it was a piece of critical self-discipline, the effect of which manifested itself in everything your father wrote afterwards, and saved him from endless errors of detail.

"So far from such work being a loss of time, I believe it would have been well worth his while, had it been practicable, to have supplemented it by a special study of embryology and physiology. His hands would have been greatly strengthened thereby when he came to write out sundry chapters of the 'Origin of Species.' But of course in those days it was almost impossible for him to find facilities for such work."

No one can look at the two volumes on the recent Cirripedes, of 399 and 684 pages respectively (not to speak of the volumes on the fossil species), without being struck by the immense amount of detailed work which they contain. The forty plates, some of them with thirty figures, and the fourteen pages of index in the two volumes together, give some rough idea of the labour spent on the

© The Complete Work of Charles Darwin Online

work.* The state of knowledge, as regards the Cirripedes, was most unsatisfactory at the time that my father began to work at them. As an illustration of this fact, it may be mentioned that he had even to re-organise the nomenclature of the group, or, as he expressed it, he "unwillingly found it indispensable to give names to several valves, and to some few of the softer parts of Cirripedes."† It is interesting to learn from his diary the amount of time which he gave to different genera. Thus the genus Chthamalus, the description of which occupies twenty-two pages, occupied him for thirty-six days; Coronula took nineteen days, and is described in twenty-seven pages. Writing to Fitz-Roy, he speaks of being "for the last half-month daily hard at work in dissecting a little animal about the size of a pin's head, from the Chonos archipelago, and I could spend another month, and daily see more beautiful structure."

Though he became excessively weary of the work before the end of the eight years, he had much keen enjoyment in the course of it. Thus he wrote to Sir J. D. Hooker (1847?):—"As you say, there is an extraordinary pleasure in pure observation; not but what I suspect the pleasure in this case is rather derived from comparisons forming in one's mind with allied structures. After having been so long employed in writing my old geological observations, it is delightful to use one's eyes and fingers again." It was, in fact, a return to the work which occupied so much of his time when at sea during his voyage. His zoological notes of that period give an impression of vigorous work, hampered by ignorance and want of appliances; and his untiring industry in the dissection of marine animals, especially of Crustacea, must have been of value to him as training for his Cirripede work. Most of his work was done with the simple dissecting micro-

* The reader unacquainted with Zoology will find some account of the more interesting results in Mr. Romanes' article on "Charles Darwin" ('Nature' Series, 1882).

† Vol. i. p. 3.

© The Complete Work of Charles Darwin Online

scope—but it was the need which he found for higher powers that induced him, in 1846, to buy a compound microscope. He wrote to Hooker:—"When I was drawing with L., I was so delighted with the appearance of the objects, especially with their perspective, as seen through the weak powers of a good compound microscope, that I am going to order one; indeed, I often have structures in which the $\frac{1}{30}$ is not power enough."

During part of the time covered by the present chapter, my father suffered perhaps more from ill-health than at any other time of his life. He felt severely the depressing influence of these long years of illness; thus as early as 1840 he wrote to Fox: "I am grown a dull, old, spiritless dog to what I used to be. One gets stupider as one grows older I think." It is not wonderful that he should so have written, it is rather to be wondered at that his spirit withstood so great and constant a strain. He wrote to Sir J. Hooker in 1845: "You are very kind in your enquiries about my health; I have nothing to say about it, being always much the same, some days better and some worse. I believe I have not had one whole day, or rather night, without my stomach having been greatly disordered, during the last three years, and most days great prostration of strength: thank you for your kindness; many of my friends, I believe, think me a hypochondriac."

Again, in 1849, he notes in his diary:—"January 1st to March 10th. — Health very bad, with much sickness and failure of power. Worked on all well days." This was written just before his first visit to Dr. Gully's Water-Cure Establishment at Malvern. In April of the same year he wrote:—"I believe I am going on very well, but I am rather weary of my present inactive life, and the water-cure has the most extraordinary effect in producing indolence and stagnation of mind: till experiencing it, I could not have believed it possible. I now increase in weight, have escaped sickness

© The Complete Work of Charles Darwin Online

for thirty days." He returned in June, after sixteen weeks' absence, much improved in health, and, as already described (p. 131), continued the water-cure at home for some time.]

C. Darwin to J. D. Hooker.

Down [October, 1846].

MY DEAR HOOKER,—I have not heard from Sulivan * lately; when he last wrote he named from 8th to 10th as the most likely time. Immediately that I hear, I will fly you a line, for the chance of your being able to come. I forget whether you know him, but I suppose so; he is a real good fellow. Anyhow, if you do not come then, I am very glad that you propose coming soon after. . . .

I am going to begin some papers on the lower marine animals, which will last me some months, perhaps a year, and then I shall begin looking over my ten-year-long accumulation of notes on species and varieties, which, with writing, I dare say will take me five years, and then, when published, I dare say I shall stand infinitely low in the opinion of all sound Naturalists—so this is my prospect for the future.

Are you a good hand at inventing names. I have a quite new and curious genus of Barnacle, which I want to name, and how to invent a name completely puzzles me.

By the way, I have told you nothing about Southampton. We enjoyed (wife and myself) our week beyond measure: the papers were all dull, but I met so many friends and made so many new acquaintances (especially some of the Irish Naturalists), and took so many pleasant excursions. I wish you had been there. On Sunday we had so pleasant an excursion to Winchester with Falconer,† Colonel

* Admiral Sir B. J. Sulivan, formerly an officer of the *Beagle*.

† Hugh Falconer, born 1809, died 1865. Chiefly known as a palæontologist, although employed as a botanist during his whole career in India, where he was also a medical officer in H.E.I.C. Service; he was superintendent of the Company's garden, first at Saha-

© The Complete Work of Charles Darwin Online

Sabine,* and Dr. Robinson,† and others. I never enjoyed a day more in my life. I missed having a look at H. Watson.‡ I suppose you heard that he met Forbes and told him he had a severe article in the Press. I understand that Forbes explained to him that he had no cause to complain, but as the article was printed, he would not withdraw it, but offered it to Forbes for him to append notes to it, which Forbes naturally declined. . . .

C. Darwin to J. D. Hooker.

Down, April 7th, [1847?]

MY DEAR HOOKER,—I should have written before now, had I not been almost continually unwell, and at present I am suffering from four boils and swellings, one of which hardly allows me the use of my right arm, and has stopped all my work, and damped all my spirits. I was much disappointed at missing my trip to Kew, and the more so, as I had forgotten you would be away all this month; but I had no choice, and was in bed nearly all Friday and Saturday. I congratulate you over your improved prospects about India,§ but at the

---

runpore, and then at Calcutta. He was one of the first botanical explorers of Kashmir. Falconer's discoveries of Miocene mammalian remains in the Sewalik Hills, were, at the time, perhaps the greatest "finds" which had been made. His book on the subject, 'Fauna Antiqua Sivalensis,' remained unfinished at the time of his death.

* The late Sir Edward Sabine, formerly President of the Royal Society, and author of a long series of memoirs on Terrestrial Magnetism.

† The late Dr. Thomas Romney Robinson, of the Armagh Observatory.

‡ The late Hewett Cottrell Watson, author of the 'Cybele Britannica,' one of a most valuable series of works on the topography and geographical distribution of the plants of the British Islands.

§ Sir J. Hooker left England on November 11, 1847, for his Himalayan and Tibetan journey. The expedition was supported by a small grant from the Treasury, and thus assumed the character of a Government mission.

© The Complete Work of Charles Darwin Online

same time must sincerely groan over it. I shall feel quite lost without you to discuss many points with, and to point out (ill-luck to you) difficulties and objections to my species hypotheses. It will be a horrid shame if money stops your expedition; but Government will surely help you to some extent. . . . Your present trip, with your new views, amongst the coal-plants, will be very interesting. If you have spare time, *but not without*, I should enjoy having some news of your progress. Your present trip will work well in, if you go to any of the coal districts in India. Would this not be a good object to parade before Government; their utilitarian souls would comprehend this. By the way, I will get some work out of you, about the domestic races of animals in India. . . .

C. Darwin to L. Jenyns (*Blomefield*).

Down [1847].

DEAR JENYNS,—I am very much obliged for the capital little Almanack; * it so happened that I was wishing for one to keep in my portfolio. I had never seen this kind before, and shall certainly get one for the future. I think it is very amusing to have a list before one's eyes of the order of appearance of the plants and animals around one; it gives a fresh interest to each fine day. There is one point I should like to see a little improved, viz. the correction for the clock at

* "This letter relates to a small Almanack first published in 1843, under the name of 'The Naturalists' Pocket Almanack,' by Mr. Van Voorst, and which I edited for him. It was intended especially for those who interest themselves in the periodic phenomena of animals and plants, of which a select list was given under each month of the year.

"The Pocket Almanack contained, moreover, miscellaneous information relating to Zoology and Botany; to Natural History and other scientific societies; to public Museums and Gardens, in addition to the ordinary celestial phenomena found in most other Almanacks. It continued to be issued till 1847, after which year the publication was abandoned."—From a letter from Rev. L. Blomefield to F. Darwin.

© The Complete Work of Charles Darwin Online

shorter intervals. Most people, I suspect, who like myself have dials, will wish to be more precise than with a margin of three minutes. I always buy a shilling almanack for this *sole* end. By the way, *yours*, *i.e.* Van Voorst's Almanack, is very dear; it ought, at least, to be advertised post-free for the shilling. Do you not think a table (not rules) of conversion of French into English measures, and perhaps weights, would be exceedingly useful; also centigrade into Fahrenheit,—magnifying powers according to focal distances?—in fact you might make it the most useful publication of the age. I know what I should like best of all, namely, current meteorological remarks for each month, with statement of average course of winds and prediction of weather, in accordance with movements of barometer. People, I think, are always amused at knowing the extremes and means of temperature for corresponding times in other years.

I hope you will go on with it another year. With many thanks, my dear Jenyns,

Yours very truly,
CHARLES DARWIN.

C. Darwin to J. D. Hooker.

Down, Sunday [April 18th, 1847].

MY DEAR HOOKER,—I return with many thanks Watson's letter, which I have had copied. It is a capital one, and I am extremely obliged to you for obtaining me such valuable information. Surely he is rather in a hurry when he says intermediate varieties must almost be necessarily rare, otherwise they would be taken as the types of the species; for he overlooks numerical frequency as an element. Surely if A, B, C were three varieties, and if A were a good deal the commonest (therefore, also, first known), it would be taken as the type, without regarding whether B was quite intermediate or not, or whether it was rare or not. What capital essays W. would write; but I suppose he has written a good deal in the

© The Complete Work of Charles Darwin Online

'Phytologist.' You ought to encourage him to publish on variation; it is a shame that such facts as those in his letter should remain unpublished. I must get you to introduce me to him; would he be a good and sociable man for Dropmore?* though if he comes, Forbes must not (and I think you talked of inviting Forbes), or we shall have a glorious battle. I should like to see sometime the war correspondence. Have you the 'Phytologist,' and could you sometime spare it; I would go through it quickly. . . . I have read your last five numbers,† and as usual have been much interested in several points, especially with your discussions on the beech and potato. I see you have introduced several sentences against us Transmutationists. I have also been looking through the latter volumes of the 'Annals of Natural History,' and have read two such soulless, pompous papers of ——, quite worthy of the author. . . . The contrast of the papers in the *Annals* with those in the *Annales* is rather humiliating; so many papers in the former, with short descriptions of species, without one word on their affinities, internal structure, range, or habits. I am now reading ——, and I have picked out some things which have interested me; but he strikes me as rather dullish, and with all his Materia Medica smells of the doctor's shop. I shall ever hate the name of the Materia Medica, since hearing Duncan's lectures at eight o'clock on a winter's morning—a whole, cold, breakfastless hour on the properties of rhubarb!

I hope your journey will be very prosperous. Believe me, my dear Hooker,

Ever yours,

C. Darwin.

P.S.—I think I have only made one new acquaintance of late, that is, R. Chambers; and I have just received a

* A much enjoyed expedition made from Oxford — when the British Association met there in 1847.

† Of the Botany of Hooker's 'Antarctic Voyage.'

© The Complete Work of Charles Darwin Online

presentation copy of the sixth edition of the 'Vestiges.' Somehow I now feel perfectly convinced he is the author. He is in France, and has written to me thence.

C. Darwin to J. D. Hooker.

Down, [1847 ?]

. . . I am delighted to hear that Brongniart thought Sigillaria aquatic, and that Binney considers coal a sort of submarine peat. I would bet 5 to 1 that in twenty years this will be generally admitted; * and I do not care for whatever the botanical difficulties or impossibilities may be. If I could but persuade myself that Sigillaria and Co. had a good range of depth, *i.e.* could live from 5 to 100 fathoms under water, all difficulties of nearly all kinds would be removed (for the simple fact of muddy ordinary shallow sea mplies proximity of land). [N.B.—I am chuckling to think how you are sneering all this time.] It is not much of a difficulty, there not being shells with the coal, considering how unfavourable deep mud is for most Mollusca, and that shells would probably decay from the humic acid, as seems to take place in peat and in the *black* moulds (as Lyell tells me) of the Mississippi. So coal question settled—Q. E. D. Sneer away!

Many thanks for your welcome note from Cambridge, and I am glad you like my *alma mater*, which I despise heartily as a place of education, but love from many most pleasant recollections. . . .

Thanks for your offer of the 'Phytologist;' I shall be very much obliged for it, for I do not suppose I should be able to borrow it from any other quarter. I will not be set up too much by your praise, but I do not believe I ever lost a book or forgot to return it during a long lapse of time. Your 'Webb' is well wrapped up, and with your name in large letters *outside*.

* An unfulfilled prophecy.

© The Complete Work of Charles Darwin Online

My new microscope is come home (a "splendid plaything," as old R. Brown called it), and I am delighted with it; it really is a splendid plaything. I have been in London for three days, and saw many of our friends. I was extremely sorry to hear a not very good account of Sir William. Farewell, my dear Hooker, and be a good boy, and make Sigillaria a submarine sea-weed.

Ever yours,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down [May 6th, 1847].

MY DEAR HOOKER,—You have made a savage onslaught, and I must try to defend myself. But, first, let me say that I never write to you except for my own good pleasure; now I fear that you answer me when busy and without inclination (and I am sure I should have none if I was as busy as you). Pray do not do so, and if I thought my writing entailed an answer from you *nolens volens*, it would destroy all my pleasure in writing. Firstly, I did not consider my letter as *reasoning*, or even as *speculation*, but simply as mental rioting; and as I was sending Binney's paper, I poured out to you the result of reading it. Secondly, you are right, indeed, in thinking me mad, if you suppose that I would class any ferns as marine plants; but surely there is a wide distinction between the plants found upright in the coal-beds and those not upright, and which might have been drifted. Is it not possible that the same circumstances which have preserved the vegetation *in situ*, should have preserved drifted plants? I know Calamites is found upright; but I fancied its affinities were very obscure, like Sigillaria. As for Lepidodendron, I forgot its existence, as happens when one goes riot, and now know neither what it is, or whether upright. If these plants, *i.e.* Calamites and Lepidodendron, have *very clear relations* to terrestrial vegetables, like the ferns have, and are found

2 B 2

© The Complete Work of Charles Darwin Online

upright *in situ*, of course I must give up the ghost. But surely Sigillaria is the main upright plant, and on its obscure affinities I have heard you enlarge.

Thirdly, it never entered my head to undervalue botanical relatively to zoological evidence; except in so far as I thought it was admitted that the vegetative structure seldom yielded any evidence of affinity nearer than that of families, and not always so much. And is it not in plants, as certainly it is in animals, dangerous to judge of habits without very near affinity. Could a Botanist tell from structure alone that the Mangrove family, almost or quite alone in Dicotyledons, could live in the sea, and the Zostera family almost alone among the Monocotyledons? Is it a safe argument, that because algæ are almost the only, or the only submerged sea-plants, that formerly other groups had not members with such habits? With animals such an argument would not be conclusive, as I could illustrate by many examples; but I am forgetting myself; I want only to some degree to defend myself, and not burn my fingers by attacking you. The foundation of my letter, and what is my deliberate opinion, though I dare say you will think it absurd, is that I would rather trust, *cæteris paribus*, pure geological evidence than either zoological or botanical evidence. I do not say that I would sooner trust *poor* geological evidence than *good* organic. I think the basis of pure geological reasoning is simpler (consisting chiefly of the action of water on the crust of the earth, and its up and down movements) than a basis drawn from the difficult subject of affinities and of structure in relation to habits. I can hardly analyse the facts on which I have come to this conclusion; but I can illustrate it. Pallas's account would lead any one to suppose that the Siberian strata, with the frozen carcasses, had been quickly deposited, and hence that the embedded animals had lived in the neighbourhood; but our zoological knowledge of thirty years ago led every one falsely to reject this conclusion.

© The Complete Work of Charles Darwin Online

Tell me that an upright fern *in situ* occurs with Sigillaria and Stigmaria, or that the affinities of Calamites and Lepidodendron (supposing that they are found *in situ* with Sigillaria) are so *clear*, that they could not have been marine, like, but in a greater degree, than the mangrove and sea-wrack, and I will humbly apologise to you and all Botanists for having let my mind run riot on a subject on which assuredly I know nothing. But till I hear this, I shall keep privately to my own opinion with the same pertinacity and, as you will think, with the same philosophical spirit with which Koenig maintains that Cheirotherium-footsteps are fuci.

Whether this letter will sink me still lower in your opinion, or put me a little right, I know not, but hope the latter. Anyhow, I have revenged myself with boring you with a very long epistle. Farewell, and be forgiving. Ever yours,

C. DARWIN.

P.S.—When will you return to Kew? I have forgotten one main object of my letter, to thank you *much* for your offer of the 'Hort. Journal,' but I have ordered the two numbers.

[The two following extracts [1847] give the continuation and conclusion of the coal battle.

"By the way, as submarine coal made you so wrath, I thought I would experimentise on Falconer and Bunbury* together, and it made [them] even more savage; 'such infernal nonsense ought to be thrashed out of me.' Bunbury was more polite and contemptuous. So I now know how to stir up and show off any Botanist. I wonder whether Zoologists and Geologists have got their tender points; I wish I could find out."

"I cannot resist thanking you for your most kind note. Pray do not think that I was annoyed by your letter: I perceived that you had been thinking with animation, and accordingly expressed yourself strongly, and so I understood it.

* The late Sir C. Bunbury, well known as a palæobotanist.

© The Complete Work of Charles Darwin Online

Forfend me from a man who weighs every expression with Scotch prudence. I heartily wish you all success in your noble problem, and I shall be very curious to have some talk with you and hear your ultimatum."]

*C. Darwin to J. D. Hooker.**

Down [October, 1847].

I congratulate you heartily on your arrangements being completed, with some prospect for the future. It will be a noble voyage and journey, but I wish it was over, I shall miss you selfishly and all ways to a dreadful extent . . . I am in great perplexity how we are to meet . . . I can well understand how dreadfully busy you must be. If you *cannot* come here, you *must* let me come to you for a night; for I must have one more chat and one more quarrel with you over the coal.

By the way, I endeavoured to stir up Lyell (who has been staying here some days with me) to theorise on the coal: his oolitic *upright* Equisetums are dreadful for my submarine flora. I should die much easier if some one would solve me the coal question. I sometimes think it could not have been formed at all. Old Sir Anthony Carlisle once said to me gravely, that he supposed Megatherium and such cattle were just sent down from heaven to see whether the earth would support them; and I suppose the coal was rained down to puzzle mortals. You must work the coal well in India.

Ever yours,

C. DARWIN.

C. Darwin to J. D. Hooker.

[November 6th, 1847.]

MY DEAR HOOKER,—I have just received your note with sincere grief: there is no help for it. I shall always look at your intention of coming here, under such circumstances, as

* Parts of two letters.

© The Complete Work of Charles Darwin Online

the greatest proof of friendship I ever received from mortal man. My conscience would have upbraided me in not having come to you on Thursday, but, as it turned out, I could not, for I was quite unable to leave Shrewsbury before that day, and I reached home only last night, much knocked up. Without I hear to-morrow (which is hardly possible), and if I am feeling pretty well, I will drive over to Kew on Monday morning, just to say farewell. I will stay only an hour. . . .

C. Darwin to J. D. Hooker.

[November 1847.]

MY DEAR HOOKER,—I am very unwell, and incapable of doing anything. I do hope I have not inconvenienced you. I was so unwell all yesterday, that I was rejoicing you were not here; for it would have been a bitter mortification to me to have had you here and not enjoyed your last day. I shall not now see you. Farewell, and God bless you.

Your affectionate friend,

C. DARWIN.

I will write to you in India.

[In 1847 appeared a paper by Mr. D. Milne,* in which my father's Glen Roy work is criticised, and which is referred to in the following characteristic extract from a letter to Sir J. Hooker: "I have been bad enough for these few last days, having had to think and write too much about Glen Roy. . . . Mr. Milne having attacked my theory, which made me horribly sick." I have not been able to find any published reply to Mr. Milne, so that I imagine the "writing" mentioned was confined to letters. Mr. Milne's paper was not destructive to the Glen Roy paper, and this my father recognises in the following extract from a letter to Lyell (March, 1847). The reference to Chambers is explained by the fact that he ac-

* Now Mr. Milne Home. The essay was published in Transactions of the Edinburgh Royal Society, vol. xvi.

© The Complete Work of Charles Darwin Online

companied Mr. Milne in his visit to Glen Roy. "I got R. Chambers to give me a sketch of Milne's Glen Roy views, and I have re-read my paper, and am, now that I have heard what is to be said, not even staggered. It is provoking and humiliating to find that Chambers not only had not read with any care my paper on this subject, or even looked at the coloured map, so that the new shelf described by me had not been searched for, and my arguments and facts of detail not in the least attended to. I entirely gave up the ghost, and was quite chicken-hearted at the Geological Society, till you reassured and reminded me of the main facts in the whole case."

The two following letters to Lyell, though of later date (June, 1848), bear on the same subject :—

"I was at the evening meeting [of the Geological Society], but did not get within hail of you. What a fool (though I must say a very amusing one) —— did make of himself. Your speech was refreshing after it, and was well characterized by Fox (my cousin) in three words—'What a contrast!' That struck me as a capital speculation about the Wealden Continent going down. I did not hear what you settled at the Council ; I was quite wearied out and bewildered. I find Smith, of Jordan Hill, has a much worse opinion of R. Chambers's book than even I have. Chambers has piqued me a little ; * he says I 'propound' and 'profess my belief' that Glen Roy is marine, and that the idea was accepted because the 'mobility of the land was the ascendant idea of the day.' He adds some very faint *upper* lines in Glen Spean (seen, by the way, by Agassiz), and has shown that Milne and Kemp are right in there being horizontal aqueous markings (*not* at coincident levels with those of Glen Roy) in other parts of Scotland at great heights, and he adds several other cases. This is the whole of his addition to the data. He not only takes my line

* 'Ancient Sea Margins, 1848.' The words quoted by my father should be "the mobility of the land was an ascendant idea."

© The Complete Work of Charles Darwin Online

of argument from the buttresses and terraces below the lower shelf and some other arguments (without acknowledgment), but he sneers at all his predecessors not having perceived the importance of the short portions of lines intermediate between the chief ones in Glen Roy; whereas I commence the description of them with saying, that 'perceiving their importance, I examined them with scrupulous care,' and expatiate at considerable length on them. I have indirectly told him I do not think he has quite claims to consider that he alone (which he pretty directly asserts) has solved the problem of Glen Roy. With respect to the terraces at lower levels coincident in height all round Scotland and England, I am inclined to believe he shows some little probability of there being some leading ones coincident, but much more exact evidence is required. Would you believe it credible? he advances as a probable solution to account for the rise of Great Britain that in some great ocean one-twentieth of the bottom of the whole aqueous surface of the globe has sunk in (he does not say where he puts it) for a thickness of half a mile, and this he has calculated would make an apparent rise of 130 feet."

C. Darwin to C. Lyell.

Down [June 1848].

MY DEAR LYELL,—Out of justice to Chambers I must trouble you with one line to say, as far as I am personally concerned in Glen Roy, he has made the amende honorable, and pleads guilty through inadvertency of taking my two lines of arguments and facts without acknowledgment. He concluded by saying he "came to the same point by an independent course of inquiry, which in a small degree excuses this inadvertency." His letter altogether shows a very good disposition, and says he is "much gratified with the *measured* approbation which you bestow, &c." I am heartily glad I was able to say in truth that I thought he had

© The Complete Work of Charles Darwin Online

done good service in calling more attention to the subject of the terraces. He protests it is unfair to call the sinking of the sea his theory, for that he with care always speaks of mere change of level, and this is quite true; but the one section in which he shows how he conceives the sea might sink is so astonishing, that I believe it will with others, as with me, more than counterbalance his previous caution. I hope that you may think better of the book than I do.

Yours most truly,
C. DARWIN.

C. Darwin to J. D. Hooker.

October 6th, 1848.

. . . I have lately been trying to get up an agitation (but I shall not succeed, and indeed doubt whether I have time and strength to go on with it), against the practice of Naturalists appending for perpetuity the name of the *first* describer to species. I look at this as a direct premium to hasty work, to *naming* instead of *describing*. A species ought to have a name so well known that the addition of the author's name would be superfluous, and a [piece] of empty vanity.*

* His contempt for the self-regarding spirit in a naturalist is illustrated by an anecdote, for which I am indebted to Rev. L. Blomefield. After speaking of my father's love of Entomology at Cambridge, Mr. Blomefield continues:—"He occasionally came over from Cambridge to my Vicarage at Swaffham Bulbeck, and we went out together to collect insects in the woods at Bottisham Hall, close at hand, or made longer excursions in the Fens. On one occasion he captured in a large bag net, with which he used vigorously to sweep the weeds and long grass, a rare coleopterous insect, one of the *Lepturidæ*, which I myself had never taken in Cambridgeshire. He was pleased with his capture, and of course carried it home in triumph. Some years afterwards, the voyage of the *Beagle* having been made in the interim, talking over old times with him, I reverted to this circumstance, and asked if he remembered it. 'Oh yes,' (he said,) 'I remember it well; and I was selfish enough to keep the specimen, when you were collecting materials for a Fauna of Cambridgeshire, and for a local museum in the Philosophical Society.' He followed this up with some remarks on the pettiness of collectors, who aimed at nothing beyond filling their cabinets with rare things."

© The Complete Work of Charles Darwin Online

At present, it would not do to give mere specific names; but I think Zoologists might open the road to the omission, by referring to good systematic writers instead of to first describers. Botany, I fancy, has not suffered so much as Zoology from mere *naming;* the characters, fortunately, are more obscure. Have you ever thought on this point? Why should Naturalists append their own names to new species, when Mineralogists and Chemists do not do so to new substances? When you write to Falconer pray remember me affectionately to him. I grieve most sincerely to hear that he has been ill. My dear Hooker, God bless you, and fare you well.

Your sincere friend,

C. DARWIN.

*C. Darwin to Hugh Strickland.**

Down, Jan. 29th [1849].

. . . . What a labour you have undertaken; I do *honour* your devoted zeal in the good cause of Natural Science. Do

* Hugh Edwin Strickland, M.A., F.R.S., was born 2nd of March, 1811, and educated at Rugby, under Arnold, and at Oriel College, Oxford. In 1835 and 1836 he travelled through Europe to the Levant with W. J. Hamilton, the geologist, wintering in Asia Minor. In 1841 he brought the subject of Natural History Nomenclature before the British Association, and prepared the Code of Rules for Zoological Nomenclature, now known by his name—the principles of which are very generally adopted. In 1843 he was one of the founders (if not the original projector) of the Ray Society. In 1845 he married the second daughter of Sir William Jardine, Bart. In 1850 he was appointed, in consequence of Buckland's illness, Deputy Reader in Geology at Oxford. His promising career was suddenly cut short on September 14, 1853, when, while geologizing in a railway cutting between Retford and Gainsborough, he was run over by a train and instantly killed. A memoir of him and a reprint of his principal contributions to journals was published by Sir William Jardine in 1858; but he was also the author of 'The Dodo and its Kindred' (1848); 'Bibliographia Zoologiæ' (the latter in conjunction with Louis Agassiz, and issued by the Ray Society); 'Ornithological Synonyms' (one volume only published, and that posthumously). A catalogue of his ornithological collection, given by his widow to the University of Cambridge, was compiled by Mr. Salvin, and published in 1882. (I am indebted to Prof. Newton for the above note).

© The Complete Work of Charles Darwin Online

you happen to have a *spare* copy of the Nomenclature rules published in the 'British Association Transactions?' if you have, and would give it me, I should be truly obliged, for I grudge buying the volume for it. I have found the rules very useful, it is quite a comfort to have something to rest on in the turbulent ocean of nomenclature (and am accordingly grateful to you), though I find it very difficult to obey always. Here is a case (and I think it should have been noticed in the rules), Coronula, Cineras and Otion, are names adopted by Cuvier, Lamarck, Owen, and almost *every* well-known writer, but I find that all three names were anticipated by a German: now I believe if I were to follow the strict rule of priority, more harm would be done than good, and more especially as I feel sure that the newly fished-up names would not be adopted. I have almost made up my mind to reject the rule of priority in this case; would you grudge the trouble to send me your opinion? I have been led of late to reflect much on the subject of naming, and I have come to a fixed opinion that the plan of the first describer's name, being appended for perpetuity to a species, has been the greatest curse to Natural History. Some months since, I wrote out the enclosed badly drawn-up paper, thinking that perhaps I would agitate the subject; but the fit has passed, and I do not suppose I ever shall; I send it you for the *chance* of your caring to see my notions. I have been surprised to find in conversation that several naturalists were of nearly my way of thinking. I feel sure as long as species-mongers have their vanity tickled by seeing their own names appended to a species, because they miserably described it in two or three lines, we shall have the same *vast* amount of bad work as at present, and which is enough to dishearten any man who is willing to work out any branch with care and time. I find every genus of Cirripedia has half-a-dozen names, and not one careful description of any one species in any one genus. I do not believe that this would have been the case if each

© The Complete Work of Charles Darwin Online

man knew that the memory of his own name depended on his doing his work well, and not upon merely appending a name with a few wretched lines indicating only a few prominent external characters. But I will not weary you with any longer tirade. Read my paper or *not*, just as you like, and return it whenever you please.

Yours most sincerely,

C. DARWIN.

Hugh Strickland to C. Darwin.

The Lodge, Tewkesbury, Jan. 31st, 1849.

.... I have next to notice your second objection—that retaining the name of the *first* describer in *perpetuum* along with that of the species, is a premium on hasty and careless work. This is quite a different question from that of the law of priority itself, and it never occurred to me before, though it seems highly probable that the general recognition of that law may produce such a result. We must try to counteract this evil in some other way.

The object of appending the name of a man to the name of a species is not to gratify the vanity of the man, but to indicate more precisely the species. Sometimes two men will, by accident, give the same name (independently) to two species of the same genus. More frequently a later author will misapply the specific name of an older one. Thus the *Helix putris* of Montague is not *H. putris* of Linnæus, though Montague supposed it to be so. In such a case we cannot define the species by *Helix putris* alone, but must append the name of the author whom we quote. But when a species has never borne but one name (as *Corvus frugilegus*), and no other species of Corvus has borne the same name, it is, of course, unnecessary to add the author's name. Yet even here I like the form *Corvus frugilegus, Linn.*, as it reminds us that this is one of the old species, long known, and to be found in the 'Systema

© The Complete Work of Charles Darwin Online

Naturæ,' &c. I fear, therefore, that (at least until our nomenclature is more definitely settled) it will be impossible to indicate species with scientific accuracy, without adding the name of their first author. You may, indeed, do it as you propose, by saying *in Lam. An. Invert., &c.*, but then this would be incompatible with the law of priority, for where Lamarck has violated that law, one cannot adopt his name. It is, nevertheless, highly conducive to accurate indication to append to the (oldest) specific name *one* good reference to a standard work, especially to a *figure*, with an accompanying synonym if necessary. This method may be cumbrous, but cumbrousness is a far less evil than uncertainty.

It, moreover, seems hardly possible to carry out the *priority* principle without the historical aid afforded by appending the author's name to the specific one. If I, a *priority man*, called a species *C. D.*, it implies that C. D. is the oldest name that I know of; but in order that you and others may judge of the propriety of that name, you must ascertain when, and by whom, the name was first coined. Now, if to the specific name C. D., I append the name A. B., of its first describer, I at once furnish you with the clue to the dates when, and the book in which, this description was given, and I thus assist you in determining whether C. D. be really the oldest, and therefore the correct, designation.

I do, however, admit that the priority principle (excellent as it is) has a tendency, when the author's name is added, to encourage vanity and slovenly work. I think, however, that much might be done to discourage those obscure and unsatisfactory definitions of which you so justly complain, by *writing down* the practice. Let the better disposed naturalists combine to make a formal protest against all vague, loose, and inadequate definitions of (supposed) new species. Let a committee (say of the British Association) be appointed to prepare a sort of *Class List* of the various modern works in which new species are described, arranged in order of merit.

© The Complete Work of Charles Darwin Online

The lowest class would contain the worst examples of the kind, and their authors would thus be exposed to the obloquy which they deserve, and be gibbeted *in terrorem* for the edification of those who may come after.

I have thus candidly stated my views (I hope intelligibly) of what seems best to be done in the present transitional and dangerous state of systematic zoology. Innumerable labourers, many of them crotchety and half-educated, are rushing into the field, and it depends, I think, on the present generation whether the science is to descend to posterity a chaotic mass, or possessed of some traces of law and organisation. If we could only get a congress of deputies from the chief scientific bodies of Europe and America, something might be done, but, as the case stands, I confess I do not clearly see my way, beyond humbly endeavouring to reform *Number One.*

Yours ever,
H. E. STRICKLAND.

C. Darwin to Hugh Strickland.

Down, Sunday [Feb. 4th, 1849].

MY DEAR STRICKLAND,—I am, in truth, *greatly* obliged to you for your long, most interesting, and clear letter, and the Report. I will consider your arguments, which are of the greatest weight, but I confess I cannot yet bring myself to reject very *well-known* names, not in *one* country, but over the world, for obscure ones,—simply on the ground that I do not believe I should be followed. Pray believe that I should break the law of priority only in rare cases; will you read the enclosed (and return it), and tell me whether it does not stagger you? (N.B. I *promise* that I will not give you any more trouble.) I want simple answers, and not for you to waste your time in reasons; I am curious for your answer in regard to Balanus. I put the case of Otion, &c., to W.

© The Complete Work of Charles Darwin Online

Thompson, who is fierce for the law of priority, and he gave it up in such well-known names. I am in a perfect maze of doubt on nomenclature. In not one large genus of Cirripedia has *any one* species been correctly defined; it is pure guess-work (being guided by range and commonness and habits) to recognise any species: thus I can make out, from plates or descriptions, hardly any of the British sessile cirripedes. I cannot bear to give new names to all the species, and yet I shall perhaps do wrong to attach old names by little better than guess; I cannot at present tell the least which of two species all writers have meant by the common *Anatifera lævis*; I have, therefore, given that name to the one which is rather the commonest. Literally, not one species is properly defined; not one naturalist has ever taken the trouble to open the shell of any species to describe it scientifically, and yet all the genera have half-a-dozen synonyms. For *argument's* sake, suppose I do my work thoroughly well, any one who happens to have the original specimens named, I will say by Chenu, who has figured and named hundreds of species, will be able to upset all my names according to the law of priority (for he may maintain his descriptions are sufficient), do you think it advantageous to science that this should be done: I think not, and that convenience and high merit (here put as mere argument) had better come into some play. The subject is heart-breaking.

I hope you will occasionally turn in your mind my argument of the evil done by the "mihi" attached to specific names; I can most clearly see the *excessive* evil it has caused; in mineralogy I have myself found there is no rage to merely name; a person does not take up the subject without he intends to work it out, as he knows that his *only* claim to merit rests on his work being ably done, and has no relation whatever to *naming*. I give up one point, and grant that reference to first describer's name should be given in all systematic works, but I think something would be gained if a

© The Complete Work of Charles Darwin Online

reference was given without the author's name being actually appended as part of the binomial name, and I think, except in systematic works, a reference, such as I propose, would damp vanity much. I think a very wrong spirit runs through all Natural History, as if some merit was due to a man for merely naming and defining a species; I think scarcely any, or none, is due; if he works out *minutely* and anatomically any one species, or systematically a whole group, credit is due, but I must think the mere defining a species is nothing, and that no *injustice* is done him if it be overlooked, though a great inconvenience to Natural History is thus caused. I do not think more credit is due to a man for defining a species, than to a carpenter for making a box. But I am foolish and rabid against species-mongers, or rather against their vanity; it is useful and necessary work which must be done; but they act as if they had actually made the species, and it was their own property.

I use Agassiz's nomenclator; at least two-thirds of the dates in the Cirripedia are grossly wrong.

I shall do what I can in fossil Cirripedia, and should be very grateful for specimens; but I do not believe that species (and hardly genera) can be defined by single valves; as in every recent species yet examined their forms vary greatly: to describe a species by valves alone, is the same as to describe a crab from *small* portions of its carapace alone, these portions being highly variable, and not, as in Crustacea, modelled over viscera. I sincerely apologise for the trouble which I have given you, but indeed I will give no more.

Yours most sincerely,

C. DARWIN.

P.S.—In conversation I found Owen and Andrew Smith much inclined to throw over the practice of attaching authors' names; I believe if I agitated I could get a large party to join. W. Thompson agreed some way with me, but was not prepared to go nearly as far as I am.

© The Complete Work of Charles Darwin Online

C. Darwin to Hugh Strickland.

Down, Feb. 10th [1849].

MY DEAR STRICKLAND,—I have again to thank you cordially for your letter. Your remarks shall fructify to some extent, and I will try to be more faithful to rigid virtue and priority; but as for calling Balanus "Lepas" (which I did not think of), I cannot do it, my pen won't write it—it is *impossible*. I have great hopes some of my difficulties will disappear, owing to wrong dates in Agassiz, and to my having to run several genera into one, for I have as yet gone, in but few cases, to original sources. With respect to adopting my own notions in my Cirripedia book, I should not like to do so without I found others approved, and in some public way—nor, indeed, is it well adapted, as I can never recognise a species without I have the original specimen, which, fortunately, I have in many cases in the British Museum. Thus far I mean to adopt my notion, as never putting mihi or "Darwin" after my own species, and in the anatomical text giving no authors' names at all, as the systematic Part will serve for those who want to know the History of a species as far as I can imperfectly work it out. . . . .

C. Darwin to J. D. Hooker.

[The Lodge, Malvern,
March 28th, 1849.]

MY DEAR HOOKER,—Your letter of the 13th of October has remained unanswered till this day! What an ungrateful return for a letter which interested me so much, and which contained so much and curious information. But I have had a bad winter.

On the 13th of November, my poor dear father died, and no one who did not know him would believe that a man above eighty-three years old could have retained so tender and affectionate a disposition, with all his sagacity unclouded to the last. I was at the time so unwell, that I was unable to

© The Complete Work of Charles Darwin Online

travel, which added to my misery. Indeed, all this winter I have been bad enough . . . and my nervous system began to be affected, so that my hands trembled, and head was often swimming. I was not able to do anything one day out of three, and was altogether too dispirited to write to you, or to do anything but what I was compelled. I thought I was rapidly going the way of all flesh. Having heard, accidentally, of two persons who had received much benefit from the water-cure, I got Dr. Gully's book, and made further enquiries, and at last started here, with wife, children, and all our servants. We have taken a house for two months, and have been here a fortnight. I am already a little stronger . . . Dr. Gully feels pretty sure he can do me good, which most certainly the regular doctors could not. . . . . . I feel certain that the water-cure is no quackery.

How I shall enjoy getting back to Down with renovated health, if such is to be my good fortune, and resuming the beloved Barnacles. Now I hope that you will forgive me for my negligence in not having sooner answered your letter. I was uncommonly interested by the sketch you give of your intended grand expedition, from which I suppose you will soon be returning. How earnestly I hope that it may prove in every way successful. . . .

[When my father was at the Water-cure Establishment at Malvern he was brought into contact with clairvoyance, of which he writes in the following extract from a letter to Fox, September, 1850.

"You speak about Homœopathy, which is a subject which makes me more wrath, even than does Clairvoyance. Clairvoyance so transcends belief, that one's ordinary faculties are put out of the question, but in homœopathy common sense and common observation come into play, and both these must go to the dogs, if the infinitesimal doses have any effect whatever. How true is a remark I saw the other day by Quetelet,

© The Complete Work of Charles Darwin Online

in respect to evidence of curative processes, viz. that no one knows in disease what is the simple result of nothing being done, as a standard with which to compare homœopathy, and all other such things. It is a sad flaw, I cannot but think, in my beloved Dr. Gully, that he believes in everything. When Miss —— was very ill, he had a clairvoyant girl to report on internal changes, a mesmerist to put her to sleep—an homœopathist, viz. Dr. ——, and himself as hydropathist! and the girl recovered."

A passage out of an earlier letter to Fox (December, 1844) shows that he was equally sceptical on the subject of mesmerism: "With respect to mesmerism, the whole country resounds with wonderful facts or tales . . . I have just heard of a child, three or four years old (whose parents and self I well knew), mesmerised by his father, which is the first fact which has staggered me. I shall not believe fully till I see or hear from good evidence of animals (as has been stated is possible) not drugged, being put to stupor; of course the impossibility would not prove mesmerism false; but it is the only clear *experimentum crucis*, and I am astonished it has not been systematically tried. If mesmerism was investigated, like a science, this could not have been left till the present day to be *done satisfactorily*, as it has been I believe left. Keep some cats yourself, and do get some mesmeriser to attempt it. One man told me he had succeeded, but his experiments were most vague, as was likely from a man who said cats were more easily done than other animals, because they were so electrical!"]

C. Darwin to C. Lyell.

Down, December 4th [1849].

MY DEAR LYELL,—This letter requires no answer, and I write from exuberance of vanity. Dana has sent me the Geology of the United States Expedition, and I have just

© The Complete Work of Charles Darwin Online

read the Coral part. To begin with a modest speech, *I am astonished at my own accuracy!!* If I were to rewrite now my Coral book there is hardly a sentence I should have to alter, except that I ought to have attributed more effect to recent volcanic action in checking growth of coral. When I say all this I ought to add that the *consequences* of the theory on areas of subsidence are treated in a separate chapter to which I have not come, and in this, I suspect, we shall differ more. Dana talks of agreeing with my theory *in most points;* I can find out not one in which he differs. Considering how infinitely more he saw of Coral Reefs than I did, this is wonderfully satisfactory to me. He treats me most courteously. There now, my vanity is pretty well satisfied. . . .

C. Darwin to J. D. Hooker.

Malvern, April 9th, 1849.

MY DEAR HOOKER,—The very next morning after posting my last letter (I think on 23rd of March), I received your two interesting gossipaceous and geological letters; and the latter I have since exchanged with Lyell for his. I will write higglety-pigglety just as subjects occur. I saw the Review in the 'Athenæum,' it was written in an ill-natured spirit; but the whole virus consisted in saying that there was not novelty enough in your remarks for publication. No one, nowadays, cares for reviews. I may just mention that my Journal got some *real good* abuse, "presumption," &c.—ended with saying that the volume appeared "made up of the scraps and rubbish of the author's portfolio." I most truly enter into what you say, and quite believe you that you care only for the review with respect to your father; and that this *alone* would make you like to see extracts from your letters more properly noticed in this same periodical. I have considered to the very best of my judgment whether any portion of your present letters are adapted for the 'Athenæum' (in which I have no

© The Complete Work of Charles Darwin Online

interest; the beasts not having even *noticed* my three geological volumes which I had sent to them), and I have come to the conclusion it is better not to send them. I feel sure, considering all the circumstances, that without you took pains and wrote *with care*, a condensed and finished sketch of some striking feature in your travels, it is better not to send anything. These two letters are, moreover, rather too geological for the 'Athenæum,' and almost require woodcuts. On the other hand, there are hardly enough details for a communication to the Geological Society. I have not the *smallest doubt* that your facts are of the highest interest with regard to glacial action in the Himalaya; but it struck both Lyell and myself that your evidence ought to have been given more distinctly. . . .

I have written so lately that I have nothing to say about myself; my health prevented me going on with a crusade against "mihi" and "nobis," of which you warn me of the dangers. I showed my paper to three or four Naturalists, and they all agreed with me to a certain extent: with health and vigour, I would not have shown a white feather, [and] with aid of half-a-dozen really good Naturalists, I believe something might have been done against the miserable and degrading passion of mere species naming. In your letter you wonder what "Ornamental Poultry" has to do with Barnacles; but do not flatter yourself that I shall not yet live to finish the Barnacles, and then make a fool of myself on the subject of species, under which head ornamental Poultry are very interesting. . . .

C. Darwin to C. Lyell.

The Lodge, Malvern [June, 1849].

. . . I have got your book,* and have read all the first and a small part of the second volume (reading is the hardest work

* 'A Second Visit to the United States.'

© The Complete Work of Charles Darwin Online

allowed here), and greatly I have been interested by it. It makes me long to be a Yankee. E. desires me to say that she quite "gloated" over the truth of your remarks on religious progress . . . . I delight to think how you will disgust some of the bigots and educational dons. As yet there has not been *much* Geology or Natural History, for which I hope you feel a little ashamed. Your remarks on all social subjects strike me as worthy of the author of the 'Principles.' And yet (I know it is prejudice and pride) if I had written the Principles, I never would have written any travels; but I believe I am more jealous about the honour and glory of the Principles than you are yourself. . . .

C. Darwin to C. Lyell.

. . . . September 14th, 1849.

. . . I go on with my aqueous processes, and very steadily but slowly gain health and strength. Against all rules, I dined at Chevening with Lord Mahon, who did me the great honour of calling on me, and how he heard of me I can't guess. I was charmed with Lady Mahon, and any one might have been proud at the pieces of agreeableness which came from her beautiful lips with respect to you. I like old Lord Stanhope very much; though he abused Geology and Zoology heartily. "To suppose that the Omnipotent God made a world, found it a failure, and broke it up, and then made it again, and again broke it up, as the Geologists say, is all fiddle faddle." Describing Species of birds and shells, &c., is all fiddle faddle. . . .

I am heartily glad we shall meet at Birmingham, as I trust we shall, if my health will but keep up. I work now every day at the Cirripedia for 2½ hours, and so get on a little, but very slowly. I sometimes, after being a whole week employed and having described perhaps only two species, agree mentally with Lord Stanhope, that it is all fiddle faddle; however,

© The Complete Work of Charles Darwin Online

the other day I got the curious case of a unisexual, instead of hermaphrodite cirripede, in which the female had the common cirripedial character, and in two valves of her shell had two little pockets, in *each* of which she kept a little husband; I do not know of any other case where a female invariably has two husbands. I have one still odder fact, common to several species, namely, that though they are hermaphrodite, they have small additional, or as I shall call them, complemental males, one specimen itself hermaphrodite had no less than *seven*, of these complemental males attached to it. Truly the schemes and wonders of Nature are illimitable. But I am running on as badly about my cirripedia as about Geology; it makes me groan to think that probably I shall never again have the exquisite pleasure of making out some new district, of evolving geological light out of some troubled dark region. So I must make the best of my Cirripedia. . . .

C. Darwin to J. D. Hooker.

Down, October 12th, 1849.

. . . By the way, one of the pleasantest parts of the British Association was my journey down to Birmingham with Mrs. Sabine, Mrs. Reeve, and the Colonel; also Col. Sykes and Porter. Mrs. Sabine and myself agreed wonderfully on many points, and in none more sincerely than about you. We spoke about your letters from the Erebus; and she quite agreed with me, that you and the *author* * of the description of the cattle hunting in the Falklands, would have made a capital book together! A very nice woman she is, and so is her sharp and sagacious mother. . . . Birmingham was very flat compared to Oxford, though I had my wife with me. We saw a good deal of the Lyells and Horners and Robinsons (the President); but the place was dismal, and

* Sir J. Hooker wrote the spirited description of cattle hunting in Sir J. Ross's 'Voyage of Discovery in the Southern Regions,' 1847, vol. ii. p. 245.

© The Complete Work of Charles Darwin Online

I was prevented, by being unwell, from going to Warwick, though that, *i.e.* the party, by all accounts, was wonderfully inferior to Blenheim, not to say anything of that heavenly day at Dropmore. One gets weary of all the spouting. . . .

You ask about my cold-water cure; I am going on very well, and am certainly a little better every month, my nights mend much slower than my days. I have built a douche, and am to go on through all the winter, frost or no frost. My treatment now is lamp five times per week, and shallow bath for five minutes afterwards; douche daily for five minutes, and dripping sheet daily. The treatment is wonderfully tonic, and I have had more better consecutive days this month than on any previous ones. . . . I am allowed to work now two and a half hours daily, and I find it as much as I can do; for the cold-water cure, together with three short walks, is curiously exhausting; and I am actually *forced* to go to bed at eight o'clock completely tired. I steadily gain in weight, and eat immensely, and am never oppressed with my food. I have lost the involuntary twitching of the muscle, and all the fainting feelings, &c.—black spots before eyes, &c. Dr. Gully thinks he shall quite cure me in six or nine months more.

The greatest bore, which I find in the water-cure, is the having been compelled to give up all reading, except the newspapers; for my daily two and a half hours at the Barnacles is fully as much as I can do of anything which occupies the mind; I am consequently terribly behind in all scientific books. I have of late been at work at mere species describing, which is much more difficult than I expected, and has much the same sort of interest as a puzzle has; but I confess I often feel wearied with the work, and cannot help sometimes asking myself what is the good of spending a week or fortnight in ascertaining that certain just perceptible differences blend together and constitute varieties and not species. As long as I am on anatomy I never feel myself in that disgusting, horrid, *cui bono*, inquiring, humour. What

© The Complete Work of Charles Darwin Online

miserable work, again, it is searching for priority of names. I have just finished two species, which possess seven generic, and twenty-four specific names! My chief comfort is, that the work must be sometime done, and I may as well do it, as any one else.

I have given up my agitation against *mihi* and *nobis;* my paper is too long to send to you, so you must see it, if you care to do so, on your return. By-the-way, you say in your letter that you care more for my species work than for the Barnacles; now this is too bad of you, for I declare your decided approval of my plain Barnacle work over theoretic species work, had very great influence in deciding me to go on with the former, and defer my species paper. . . .

[The following letter refers to the death of his little daughter, which took place at Malvern on April 24, 1851 :]

C. Darwin to W. D. Fox.

Down, April 29th [1851].

MY DEAR FOX,—I do not suppose you will have heard of our bitter and cruel loss. Poor dear little Annie, when going on very well at Malvern, was taken with a vomiting attack, which was at first thought of the smallest importance; but it rapidly assumed the form of a low and dreadful fever, which carried her off in ten days. Thank God, she suffered hardly at all, and expired as tranquilly as a little angel. Our only consolation is that she passed a short, though joyous life. She was my favourite child; her cordiality, openness, buoyant joyousness and strong affections made her most loveable. Poor dear little soul. Well, it is all over. . . .

C. Darwin to W. D. Fox.

Down, March 7th [1852].

MY DEAR FOX,—It is indeed an age since we have had any communication, and very glad I was to receive your note.

© The Complete Work of Charles Darwin Online

Our long silence occurred to me a few weeks since, and I had then thought of writing, but was idle. I congratulate and condole with you on your *tenth* child; but please to observe when I have a tenth, send only condolences to me. We have now seven children, all well, thank God, as well as their mother; of these seven, five are boys; and my father used to say that it was certain that a boy gave as much trouble as three girls; so that *bonâ fide* we have seventeen children. It makes me sick whenever I think of professions; all seem hopelessly bad, and as yet I cannot see a ray of light. I should very much like to talk over this (by the way, my three bugbears are Californian and Australian gold, beggaring me by making my money on mortgage worth nothing; the French coming by the Westerham and Sevenoaks roads, and therefore enclosing Down; and thirdly, professions for my boys), and I should like to talk about education, on which you ask me what we are doing. No one can more truly despise the old stereotyped stupid classical education than I do; but yet I have not had courage to break through the trammels. After many doubts we have just sent our eldest boy to Rugby, where for his age he has been very well placed. . . . I honour, admire, and envy you for educating your boys at home. What on earth shall you do with your boys? Towards the end of this month we go to see W. at Rugby, and thence for five or six days to Susan * at Shrewsbury; I then return home to look after the babies, and E. goes to F. Wedgwood's of Etruria for a week. Very many thanks for your most kind and large invitation to Delamere, but I fear we can hardly compass it. I dread going anywhere, on account of my stomach so easily failing under any excitement. I rarely even now go to London; not that I am at all worse, perhaps rather better, and lead a very comfortable life with my three hours of daily work, but it is the life of a hermit. My nights are *always* bad, and that stops my

* His sister.

© The Complete Work of Charles Darwin Online

becoming vigorous. You ask about water-cure. I take at intervals of two or three months, five or six weeks of *moderately* severe treatment, and always with good effect. Do you come here, I pray and beg whenever you can find time; you cannot tell how much pleasure it would give me and E. I have finished the 1st vol. for the Ray Society of Pedunculated Cirripedes, which, as I think you are a member, you will soon get. Read what I describe on the sexes of Ibla and Scalpellum. I am now at work on the Sessile Cirripedes, and am wonderfully tired of my job: a man to be a systematic naturalist ought to work at least eight hours per day. You saw through me, when you said that I must have wished to have seen the effects of the [word illegible] Debacle, for I was saying a week ago to E., that had I been as I was in old days, I would have been certainly off that hour. You ask after Erasmus; he is much as usual, and constantly more or less unwell. Susan * is much better, and very flourishing and happy. Catherine * is at Rome, and has enjoyed it in a degree that is quite astonishing to my old dry bones. And now I think I have told you enough, and more than enough about the house of Darwin; so my dear old friend, farewell. What pleasant times we had in drinking coffee in your rooms at Christ's College, and think of the glories of Crux major.† Ah, in those days there were no professions for sons, no ill-health to fear for them, no Californian gold, no French invasions. How paramount the future is to the present when one is surrounded by children. My dread is hereditary ill-health. Even death is better for them.

My dear Fox, your sincere friend,

C. DARWIN.

P.S.—Susan has lately been working in a way which I think truly heroic about the scandalous violation of the Act against children climbing chimneys. We have set up a

* His sisters. † The beetle *Panagæus crux major*.

© The Complete Work of Charles Darwin Online

little Society in Shrewsbury to prosecute those who break the law. It is all Susan's doing. She has had very nice letters from Lord Shaftesbury and the Duke of Sutherland, but the brutal Shropshire squires are as hard as stones to move. The Act out of London seems most commonly violated. It makes one shudder to fancy one of one's own children at seven years old being forced up a chimney—to say nothing of the consequent loathsome disease and ulcerated limbs, and utter moral degradation. If you think strongly on this subject, do make some enquiries; add to your many good works, this other one, and try to stir up the magistrates. There are several people making a stir in different parts of England on this subject. It is not very likely that you would wish for such, but I could send you some essays and information if you so liked, either for yourself or to give away.

C. Darwin to W. D. Fox.

Down [October 24th, 1852].

MY DEAR FOX,—I received your long and most welcome letter this morning, and will answer it this evening, as I shall be very busy with an artist, drawing Cirripedia, and much overworked for the next fortnight. But first you deserve to be well abused—and pray consider yourself well abused—for thinking or writing that I could for one minute be bored by any amount of detail about yourself and belongings. It is just what I like hearing; believe me that I often think of old days spent with you, and sometimes can hardly believe what a jolly careless individual one was in those old days. A bright autumn evening often brings to mind some shooting excursion from Osmaston. I do indeed regret that we live so far off each other, and that I am so little locomotive. I have been unusually well of late (no water-cure), but I do not find that I can stand any change better than formerly. . . The other day I went to London and back, and the fatigue, though so trifling,

© The Complete Work of Charles Darwin Online

brought on my bad form of vomiting. I grieve to hear that your chest has been ailing, and most sincerely do I hope that it is only the muscles; how frequently the voice fails with the clergy. I can well understand your reluctance to break up your large and happy party and go abroad; but your life is very valuable, so you ought to be very cautious in good time. You ask about all of us, now five boys (oh! the professions; oh! the gold; and oh! the French—these three oh's all rank as dreadful bugbears) and two girls . . . but another and the worst of my bugbears is hereditary weakness. All my sisters are well except Mrs. Parker, who is much out of health; and so is Erasmus at his poor average: he has lately moved into Queen Anne Street. I had heard of the intended marriage * of your sister Frances. I believe I have seen her since, but my memory takes me back some twenty-five years, when she was lying down. I remember well the delightful expression of her countenance. I most sincerely wish her all happiness.

I see I have not answered half your queries. We like very well all that we have seen and heard of Rugby, and have never repented of sending [W.] there. I feel sure schools have greatly improved since our days; but I hate schools and the whole system of breaking through the affections of the family by separating the boys so early in life; but I see no help, and dare not run the risk of a youth being exposed to the temptations of the world without having undergone the milder ordeal of a great school.

I see you even ask after our pears. We have had lots of Beurrées d'Aremberg, Winter Nelis, Marie Louise, and "Ne plus Ultra," but all off the wall; the standard dwarfs have borne a few, but I have no room for more trees, so their names would be useless to me. You really must make a holiday and pay us a visit sometime; nowhere could you be more heartily welcome. I am at work at the second volume

* To the Rev. J. Hughes.

© The Complete Work of Charles Darwin Online

of the Cirripedia, of which creatures I am wonderfully tired. I hate a Barnacle as no man ever did before, not even a sailor in a slow-sailing ship. My first volume is out; the only part worth looking at is on the sexes of Ibla and Scalpellum. I hope by next summer to have done with my tedious work. Farewell,—do come whenever you can possibly manage it.

I cannot but hope that the carbuncle may possibly do you good; I have heard of all sorts of weaknesses disappearing after a carbuncle. I suppose the pain is dreadful. I agree most entirely, what a blessed discovery is chloroform. When one thinks of one's children, it makes quite a little difference in one's happiness. The other day I had five grinders (two by the elevator) out at a sitting under this wonderful substance, and felt hardly anything.

My dear old friend, yours very affectionately,

CHARLES DARWIN.

C. Darwin to W. D. Fox.

Down, January 29th [1853].

MY DEAR FOX,—Your last account some months ago was so little satisfactory that I have often been thinking of you, and should be really obliged if you would give me a few lines, and tell me how your voice and chest are. I most sincerely hope that your report will be good. . . . Our second lad has a strong mechanical turn, and we think of making him an engineer. I shall try and find out for him some less classical school, perhaps Bruce Castle. I certainly should like to see more diversity in education than there is in any ordinary school—no exercising of the observing or reasoning faculties, no general knowledge acquired—I must think it a wretched system. On the other hand, a boy who has learnt to stick at Latin and conquer its difficulties, ought to be able to stick at any labour. I should always be glad to hear anything about schools or education from you. I am at my old, never-ending subject, but trust I shall really go to

© The Complete Work of Charles Darwin Online

press in a few months with my second volume on Cirripedes. I have been much pleased by finding some odd facts in my first volume believed by Owen and a few others, whose good opinion I regard as final. . . . Do write pretty soon, and tell me all you can about yourself and family; and I trust your report of yourself may be much better than your last.

. . . I have been very little in London of late, and have not seen Lyell since his return from America; how lucky he was to exhume with his own hand parts of three skeletons of reptiles out of the *Carboniferous* strata, and out of the inside of a fossil tree, which had been hollow within.

Farewell, my dear Fox, yours affectionately,

CHARLES DARWIN.

C. Darwin to W. D. Fox.

13 Sea Houses, Eastbourne,
July [15th? 1853].

MY DEAR FOX,—Here we are in a state of profound idleness, which to me is a luxury; and we should all, I believe, have been in a state of high enjoyment, had it not been for the detestable cold gales and much rain, which always gives much *ennui* to children away from their homes. I received your letter of 13th June, when working like a slave with Mr. Sowerby at drawing for my second volume, and so put off answering it till when I knew I should be at leisure. I was extremely glad to get your letter. I had intended a couple of months ago sending you a savage or supplicating jobation to know how you were, when I met Sir P. Egerton, who told me you were well, and, as usual, expressed his admiration of your doings, especially your farming, and the number of animals, including children, which you kept on your land. Eleven children, ave Maria! it is a serious look-out for you. Indeed, I look at my five boys as something awful, and hate the very thoughts of professions, &c. If one could insure moderate

© The Complete Work of Charles Darwin Online

health for them it would not signify so much, for I cannot but hope, with the enormous emigration, professions will somewhat improve. But my bugbear is hereditary weakness. I particularly like to hear all that you can say about education, and you deserve to be scolded for saying "you did not mean to *torment* me with a long yarn." You ask about Rugby. I like it very well, on the same principle as my neighbour, Sir J. Lubbock, likes Eton, viz., that it is not worse than any other school; the expense, *with all, &c., &c.*, including some clothes, travelling expences, &c., is from £110 to £120 per annum. I do not think schools are so wicked as they were, and far more industrious. The boys, I think, live too secluded in their separate studies; and I doubt whether they will get so much knowledge of character as boys used to do; and this, in my opinion, is the *one* good of public schools over small schools. I should think the only superiority of a small school over home was forced regularity in their work, which your boys perhaps get at your home, but which I do not believe my boys would get at my home. Otherwise, it is quite lamentable sending boys so early in life from their home.

. . . To return to schools. My main objection to them, as places of education, is the enormous proportion of time spent over classics. I fancy (though perhaps it is only fancy) that I can perceive the ill and contracting effect on my eldest boy's mind, in checking interest in anything in which reasoning and observation come into play. Mere memory seems to be worked. I shall certainly look out for some school with more diversified studies for my younger boys. I was talking lately to the Dean of Hereford, who takes most strongly this view; and he tells me that there is a school at Hereford commencing on this plan; and that Dr. Kennedy at Shrewsbury is going to begin vigorously to modify that school. . . . .

I am *extremely* glad to hear that you approved of my cirripedial vol ume. I have spent an almost ridiculous amount of labour on the subject, and certainly would never have under-

© The Complete Work of Charles Darwin Online

taken it had I foreseen what a job it was. I hope to have finished by the end of the year. Do write again before a very long time; it is a real pleasure to me to hear from you. Farewell, with my wife's kindest remembrances to yourself and Mrs. Fox.

My dear old friend, yours affectionately,
C. DARWIN.

C. Darwin to W. D. Fox.

Down, August 10th [1853].

MY DEAR FOX,—I thank you sincerely for writing to me so soon after your most heavy misfortunes. Your letter affected me much. We both most truly sympathise with you and Mrs. Fox. We too lost, as you may remember, not so very long ago, a most dear child, of whom I can hardly yet bear to think tranquilly; yet, as you must know from your own most painful experience, time softens and deadens, in a manner truly wonderful, one's feelings and regrets. At first it is indeed bitter. I can only hope that your health and that of poor Mrs. Fox may be preserved, and that time may do its work softly, and bring you all together, once again, as the happy family, which, as I can well believe, you so lately formed.

My dear Fox, your affectionate friend,
CHARLES DARWIN.

[The following letter refers to the Royal Society's Medal, which was awarded to him in November, 1853:]

C. Darwin to J. D. Hooker.

Down, November 5th [1853].

MY DEAR HOOKER,—Amongst my letters received this morning, I opened first one from Colonel Sabine; the contents certainly surprised me very much, but, though the letter was

© The Complete Work of Charles Darwin Online

a *very kind one*, somehow, I cared very little indeed for the announcement it contained. I then opened yours, and such is the effect of warmth, friendship, and kindness from one that is loved, that the very same fact, told as you told it, made me glow with pleasure till my very heart throbbed. Believe me, I shall not soon forget the pleasure of your letter. Such hearty, affectionate sympathy is worth more than all the medals that ever were or will be coined. Again, my dear Hooker, I thank you. I hope Lindley * will never hear that he was a competitor against me; for really it is almost *ridiculous* (of course you would never repeat that I said this, for it would be thought by others, though not, I believe, by you, to be affectation) his not having the medal long before me; I must feel *sure* that you did quite right to propose him; and what a good, dear, kind fellow you are, nevertheless, to rejoice in this honour being bestowed on me.

What *pleasure* I have felt on the occasion, I owe almost entirely to you.

Farewell, my dear Hooker, yours affectionately,

C. DARWIN.

* John Lindley (b. 1799, d. 1865) was the son of a nurseryman near Norwich, through whose failure in business he was thrown at the age of twenty on his own resources. He was befriended by Sir W. Hooker, and employed as assistant librarian by Sir J. Banks. He seems to have had enormous capacity of work, and is said to have translated Richard's 'Analyse du Fruit' at one sitting of two days and three nights. He became Assistant-Secretary to the Horticultural Society, and in 1829 was appointed Professor of Botany at University College, a post which he held for upwards of thirty years. His writings are numerous: the best known being perhaps his 'Vegetable Kingdom,' published in 1846. His influence in helping to introduce the natural system of classification was considerable, and he brought "all the weight of his teaching and all the force of his controversial powers to support it," as against the Linnean system universally taught in the earlier part of his career. Sachs points out (Geschichte der Botanik, 1875, p. 161), that though Lindley adopted in the main a sound classification of plants, he only did so by abandoning his own theoretical principle that the physiological importance of an organ is a measure of its classificatory value.

© The Complete Work of Charles Darwin Online

P.S.—You may believe what a surprise it was, for I had never heard that the medals could be given except for papers in the 'Transactions.' All this will make me work with better heart at finishing the second volume.

C. Darwin to C. Lyell.

Down, February 18th [1854].

MY DEAR LYELL,—I should have written before, had it not seemed doubtful whether you would go on to Teneriffe, but now I am extremely glad to hear your further progress is certain; not that I have much of any sort to say, as you may well believe when you hear that I have only once been in London since you started. I was particularly glad to see, two days since, your letter to Mr. Horner, with its geological news; how fortunate for you that your knees are recovered. I am astonished at what you say of the beauty, though I had fancied it great. It really makes me quite envious to think of your clambering up and down those steep valleys. And what a pleasant party on your return from your expeditions. I often think of the delight which I felt when examining volcanic islands, and I can remember even particular rocks which I struck, and the smell of the hot, black, scoriaceous cliffs; but of those *hot* smells you do not seem to have had much. I do quite envy you. How I should like to be with you, and speculate on the deep and narrow valleys.

How very singular the fact is which you mention about the inclination of the strata being greater round the circumference than in the middle of the island; do you suppose the elevation has had the form of a flat dome. I remember in the Cordillera being *often* struck with the greater abruptness of the strata in the *low extreme* outermost ranges, compared with the great mass of inner mountains. I dare say you will have thought of measuring exactly the width of any dikes at the top and bottom of any great cliff (which was done by

© The Complete Work of Charles Darwin Online

Mr. Searle [?] at St. Helena), for it has often struck me as *very odd* that the cracks did not die out *oftener* upwards. I can think of hardly any news to tell you, as I have seen no one since being in London, when I was delighted to see Forbes looking so well, quite big and burly. I saw at the Museum some of the surprisingly rich gold ore from North Wales. Ramsay also told me that he has lately turned a good deal of New Red Sandstone into Permian, together with the Labyrinthodon. No doubt you see newspapers, and know that E. de Beaumont is perpetual Secretary, and will, I suppose, be more powerful than ever; and Le Verrier has Arago's place in the Observatory. There was a meeting lately at the Geological Society, at which Prestwich (judging from what R. Jones told me) brought forward your exact theory, viz. that the whole red clay and flints over the chalk plateau hereabouts is the residuum from the slow dissolution of the chalk!

As regards ourselves, we have no news, and are all well. The Hookers, sometime ago, stayed a fortnight with us, and, to our extreme delight, Henslow came down, and was most quiet and comfortable here. It does one good to see so composed, benevolent, and intellectual a countenance. There have been great fears that his heart is affected; but, I hope to God, without foundation. Hooker's book * is out, and *most beautifully* got up. He has honoured me beyond measure by dedicating it to me! As for myself, I am got to the page 112 of the Barnacles, and that is the sum total of my history. By-the-way, as you care so much about North America, I may mention that I had a long letter from a ship-mate in Australia, who says the Colony is getting decidedly republican from the influx of Americans, and that all the great and novel schemes for working the gold are planned and executed by these men. What a go-a-head nation it is! Give my kindest remembrances to Lady Lyell, and to Mrs. Bunbury,

* Sir J. Hooker's 'Himalayan Journal.'

© The Complete Work of Charles Darwin Online

and to Bunbury. I most heartily wish that the Canaries may be ten times as interesting as Madeira, and that everything may go on most prosperously with your whole party.

My dear Lyell,

Yours most truly and affectionately,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, March 1st [1854].

MY DEAR HOOKER,—I finished yesterday evening the first volume, and I very sincerely congratulate you on having produced a *first-class* book *—a book which certainly will last. I cannot doubt that it will take its place as a standard, not so much because it contains real solid matter, but that it gives a picture of the whole country. One can feel that one has seen it (and desperately uncomfortable I felt in going over some of the bridges and steep slopes), and one *realises* all the great Physical features. You have in truth reason to be proud; consider how few travellers there have been with a profound knowledge of one subject, and who could in addition make a map (which, by-the-way, is one of the most distinct ones I ever looked at, wherefore blessings alight on your head), and study geology and meteorology! I thought I knew you very well, but I had not the least idea that your Travels were your hobby; but I am heartily glad of it, for I feel sure that the time will never come when you and Mrs. Hooker will not be proud to look back at the labour bestowed on these beautiful volumes.

Your letter, received this morning, has interested me *extremely*, and I thank you sincerely for telling me your old thoughts and aspirations. All that you say makes me even more deeply gratified by the Dedication; but you, bad man, do you remember asking me how I thought Lyell would like the work to be dedicated to him? I remember

* 'Himalayan Journal.'

© The Complete Work of Charles Darwin Online

how strongly I answered, and I presume you wanted to know what I should feel; whoever would have dreamed of your being so crafty? I am glad you have shown a little bit of ambition about your Journal, for you must know that I have often abused you for not caring more about fame, though, at the same time, I must confess, I have envied and honoured you for being so free (too free, as I have always thought) of this "last infirmity of, &c." Do not say, "there never was a past hitherto to me—the phantom was always in view," for you will soon find other phantoms in view. How well I know this feeling, and did formerly still more vividly; but I think my stomach has much deadened my former pure enthusiasm for science and knowledge.

I am writing an unconscionably long letter, but I must return to the Journals, about which I have hardly said anything in detail. Imprimis, the illustrations and maps appear to me the best I have ever seen; the style seems to me everywhere perfectly clear (how rare a virtue), and some passages really eloquent. How excellently you have described the upper valleys, and how detestable their climate; I felt quite anxious on the slopes of Kinchin that dreadful snowy night. Nothing has astonished me more than your physical strength; and all those devilish bridges! Well, thank goodness! it is not *very* likely that I shall ever go to the Himalaya. Much in a scientific point of view has interested me, especially all about those wonderful moraines. I certainly think I quite realise the valleys, more vividly perhaps from having seen the valleys of Tahiti. I cannot doubt that the Himalaya owe almost all their contour to running water, and that they have been subjected to such action longer than any mountains (as yet described) in the world. What a contrast with the Andes!

Perhaps you would like to hear the very little that I can say *per contra*, and this only applied to the beginning, in which (as it struck me) there was not *flow* enough till you get to

© The Complete Work of Charles Darwin Online

Mirzapore on the Ganges (but the Thugs were *most* interesting), where the stream seemed to carry you on more equably with longer sentences and longer facts and discussions, &c. In another edition (and I am delighted to hear that Murray has sold all off), I would consider whether this part could not be condensed. Even if the meteorology was put in foot-notes, I think it would be an improvement. All the world is against me, but it makes me very unhappy to see the Latin names all in Italics, and all mingled with English names in Roman type; but I must bear this burden, for all men of Science seem to think it would corrupt the Latin to dress it up in the same type as poor old English. Well, I am very proud of *my* book; but there is one bore, that I do not much like asking people whether they have seen it, and how they like it, for I feel so much identified with it, that such questions become rather personal. Hence, I cannot tell you the opinion of others. You will have seen a fairly good review in the 'Athenæum.'

What capital news from Tasmania: it really is a very remarkable and creditable fact to the Colony.* I am always building veritable castles in the air about emigrating, and Tasmania has been my head-quarters of late; so that I feel very proud of my adopted country: it is really a very singular and delightful fact, contrasted with the slight appreciation of science in the old country. I thank you heartily for your letter this morning, and for all the gratification your Dedication has given me; I could not help thinking how much —— would despise you for not having dedicated it to some great man, who would have done you and it some good in the eyes of the world. Ah, my dear Hooker, you were very soft on this head, and justify what I say about not caring enough for your own fame. I wish I was in every way more worthy of your good opinion. Farewell. How pleasantly Mrs. Hooker and you must rest from one of your many labours. . . .

* This refers to an unsolicited grant by the Colonial Government towards the expenses of Sir J. Hooker's 'Flora of Tasmania.'

© The Complete Work of Charles Darwin Online

Again farewell: I have written a wonderfully long letter. Adios, and God bless you.

My dear Hooker, ever yours,

C. DARWIN.

P.S.—I have just looked over my rambling letter; I see that I have not at all expressed my strong admiration at the amount of scientific work, in so many branches, which you have effected. It is really grand. You have a right to rest on your oars; or even to say, if it so pleases you, that "your meridian is past;" but well assured do I feel that the day of your reputation and general recognition has only just begun to dawn.

[In September, 1854, his Cirripede work was practically finished, and he wrote to Sir J. Hooker:

"I have been frittering away my time for the last several weeks in a wearisome manner, partly idleness, and odds and ends, and sending ten thousand Barnacles out of the house all over the world. But I shall now in a day or two begin to look over my old notes on species. What a deal I shall have to discuss with you; I shall have to look sharp that I do not 'progress' into one of the greatest bores in life, to the few like you with lots of knowledge."]

END OF VOL. I.

© The Complete Work of Charles Darwin Online

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

69
290

University of Cambridge
DEPARTMENT OF ZOOLOGY
Balfour & Newton Libraries

© The Complete Work of Charles Darwin Online

LIFE

AND

LETTERS

OF

CHARLES

DARWIN.

VOL. II.

LONDON:

JOHN MURRAY.

© The Complete Work of Charles Darwin Online

CAMBRIDGE PHILOSOPHICAL
LIBRARY.

Deposited by Prof. Newton
1901.

D4 (D)~~AR~~ Dar

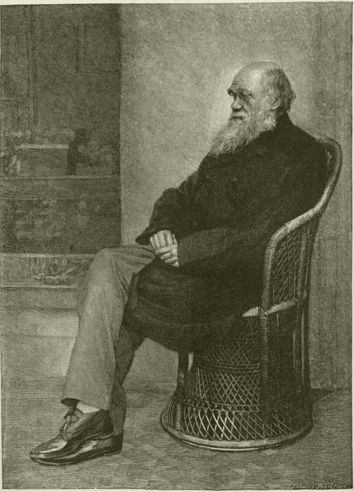
BALFOUR & NEWTON LIBRARY
2L7EO 027929205

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online



FROM A PHOTOGRAPH (1874?) BY CAPTAIN L. DARWIN, R.E. ENGRAVED FOR THE 'CENTURY MAGAZINE,' JANUARY 1883.

Frontispiece, Vol. II.

© The Complete Work of Charles Darwin Online

THE

LIFE AND LETTERS

OF

CHARLES DARWIN,

INCLUDING

AN AUTOBIOGRAPHICAL CHAPTER.

PHILOSOPHICAL LIBRARY
17 OC 1901
CAMBRIDGE

EDITED BY HIS SON,

FRANCIS DARWIN.

IN THREE VOLUMES:—VOL. II.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1887.

All Rights Reserved.

© The Complete Work of Charles Darwin Online

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.

© The Complete Work of Charles Darwin Online

TABLE OF CONTENTS.

VOLUME II.

ILLUSTRATIONS.

© The Complete Work of Charles Darwin Online

ERRATA.

VOLUME II.

P. 239, line 17 : *for* "[?]" *read* "E. R." The surmise given in the footnote is incorrect. It appears from papers in the possession of Mr. J. Estlin Carpenter, that Dr. Carpenter urged on the Editor of the 'Edinburgh Review' a purely scientific treatment of the 'Origin of Species.'

P. 246 note : *for* "Ichthyology" *read* "Ichnology."

P. 289, line 22 : *for* "Crampton" *read* "Crompton."

P. 356, line 6 : *for* "3000" *read* "2000."

P. 380, line 3 from foot : *for* "in the Amazons" *read* "on the Amazons."

P. 390, line 4 : *for* "direct in the" *read* "in the direct."

© The Complete Work of Charles Darwin Online

LIFE AND LETTERS

OF

CHARLES DARWIN.

CHAPTER I.

THE FOUNDATIONS OF THE 'ORIGIN OF SPECIES.'

[In the first volume, p. 82, the growth of the 'Origin of Species' has been briefly described in my father's words. The letters given in the present and following chapters will illustrate and amplify the history thus sketched out.

It is clear that, in the early part of the voyage of the *Beagle* he did not feel it inconsistent with his views to express himself in thoroughly orthodox language as to the genesis of new species. Thus in 1834 he wrote* at Valparaiso: "I have already found beds of recent shells yet retaining their colour at an elevation of 1300 feet, and beneath the level country is strewn with them. It seems not a very improbable conjecture that the want of animals may be owing to none having been created since this country was raised from the sea."

This passage does not occur in the published 'Journal,' the last proof of which was finished in 1837; and this fact harmonizes with the change we know to have been proceeding in his views. But in the published 'Journal' we find passages which show a point of view more in accordance with orthodox

* MS. Journals, p. 468.

© The Complete Work of Charles Darwin Online

theological natural history than with his later views. Thus, in speaking of the birds Synallaxis and Scytalopus (1st edit. p. 353; 2nd edit. p. 289), he says: "When finding, as in this case, any animal which seems to play so insignificant a part in the great scheme of nature, one is apt to wonder why a distinct species should have been created."

A comparison of the two editions of the 'Journal' is instructive, as giving some idea of the development of his views on evolution. It does not give us a true index of the mass of conjecture which was taking shape in his mind, but it shows us that he felt sure enough of the truth of his belief to allow a stronger tinge of evolution to appear in the second edition. He has mentioned in the Autobiography (p. 83), that it was not until he read Malthus that he got a clear view of the potency of natural selection. This was in 1838—a year after he finished the first edition (it was not published until 1839), and seven years before the second edition was written (1845). Thus the turning-point in the formation of his theory took place between the writing of the two editions.

I will first give a few passages which are practically the same in the two editions, and which are, therefore, chiefly of interest as illustrating his frame of mind in 1837.

The case of the two species of Molothrus (1st edit. p. 61; 2nd edit. p. 53) must have been one of the earliest instances noticed by him of the existence of representative species—a phenomenon which we know ('Autobiography,' p. 83) struck him deeply. The discussion on introduced animals (1st edit. p. 139; 2nd edit. p. 120) shows how much he was impressed by the complicated interdependence of the inhabitants of a given area.

An analogous point of view is given in the discussion (1st edit. p. 98; 2nd edit. p. 85) of the mistaken belief that large animals require, for their support, a luxuriant vegetation; the incorrectness of this view is illustrated by the com-

© The Complete Work of Charles Darwin Online

parison of the fauna of South Africa and South America, and the vegetation of the two continents. The interest of the discussion is that it shows clearly our *à priori* ignorance of the conditions of life suitable to any organism.

There is a passage which has been more than once quoted as bearing on the origin of his views. It is where he discusses the striking difference between the species of mice on the east and west of the Andes (1st edit. p. 399): "Unless we suppose the same species to have been created in two different countries, we ought not to expect any closer similarity between the organic beings on the opposite sides of the Andes than on shores separated by a broad strait of the sea." In the 2nd edit. p. 327, the passage is almost verbally identical, and is practically the same.

There are other passages again which are more strongly evolutionary in the 2nd edit., but otherwise are similar to the corresponding passages in the 1st edition. Thus, in describing the blind Tuco-tuco (1st edit. p. 60; 2nd edit. p. 52), in the first edition he makes no allusion to what Lamarck might have thought, nor is the instance used as an example of modification, as in the edition of 1845.

A striking passage occurs in the 2nd edit. (p. 173) on the relationship between the "extinct edentata and the living sloths, ant-eaters, and armadillos."

"This wonderful relationship in the same continent between the dead and the living, will, I do not doubt, hereafter throw more light on the appearance of organic beings on our earth, and their disappearance from it, than any other class of facts."

This sentence does not occur in the 1st edit., but he was evidently profoundly struck by the disappearance of the gigantic forerunners of the present animals. The difference between the discussions in the two editions is most instructive. In both, our ignorance of the conditions of life is insisted on, but in the second edition, the discussion is made to lead up to a strong statement of the intensity of the struggle for life.

B 2

© The Complete Work of Charles Darwin Online

Then follows a comparison between rarity * and extinction, which introduces the idea that the preservation and dominance of existing species depend on the degree in which they are adapted to surrounding conditions. In the first edition, he is merely "tempted to believe in such simple relations as variation of climate and food, or introduction of enemies, or the increased number of other species, as the cause of the succession of races." But finally (1st edit.) he ends the chapter by comparing the extinction of a species to the exhaustion and disappearance of varieties of fruit-trees, as though he thought that a mysterious term of life was impressed on each species at its creation.

The difference of treatment of the Galapagos problem is of some interest. In the earlier book, the American type of the productions of the islands is noticed, as is the fact that the different islands possess forms specially their own, but the importance of the whole problem is not so strongly put forward. Thus, in the first edition, he merely says:—

"This similarity of type between distant islands and continents, while the species are distinct, has scarcely been sufficiently noticed. The circumstance would be explained, according to the views of some authors, by saying that the creative power had acted according to the same law over a wide area."—(1st edit. p. 474.)

This passage is not given in the second edition, and the generalisations on geographical distribution are much wider and fuller. Thus he asks:—

"Why were their aboriginal inhabitants, associated . . . in different proportions both in kind and number from those on the Continent, and therefore acting on each other in a different manner—why were they created on American types of organisation?"—(2nd edit. p. 393.)

* In the second edition, p. 146, the destruction of Niata cattle by droughts is given as a good example of our ignorance of the causes of rarity or extinction. The passage does not occur in the first edition.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

FROM A NOTE-BOOK OF 1837.

led to comprehend true affinities. My theory would give zest to recent & Fossil Comparative Anatomy: it would lead to study of instincts, heredity, & mind heredity, whole metaphysics, it would lead to closest examination of hybridity & generation, causes of change in order to know what we have come from & to what we tend, to what circumstances favour crossing & what prevents it, this and direct examination of direct passages of structure in species, might lead to laws of change, which would then be main object of study, to guide our speculations.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

Facsimile of a page from
a note book of 1837
(See transcript opposite)

—led to comprehend true affinities. My theory would give zest to recent & fossil Comparative Anatomy; & it would lead to study of instincts, heredity, & mind heredity, whole metaphysics.— it would lead to closest examination of hybridity & generation, causes of change in order to know what we have come from & to what we tend.—
to what circumstances favour crossing & what prevents it—
this direct examination of direct passages of ~~species~~ structure in species, might lead to laws of change, which would then be main object of study, to guide our ~~past~~ speculations

© The Complete Work of Charles Darwin Online

The same difference of treatment is shown elsewhere in this chapter. Thus the gradation in the form of beak presented by the thirteen allied species of finch is described in the first edition (p. 461) without comment. Whereas in the second edition (p. 380) he concludes:—

"One might really fancy that from an original paucity of birds in this Archipelago, one species has been taken and modified for different ends."

On the whole it seems to me remarkable that the difference between the two editions is not greater; it is another proof of the author's caution and self-restraint in the treatment of his theory. After reading the second edition of the 'Journal,' we find with a strong sense of surprise how far developed were his views in 1837. We are enabled to form an opinion on this point from the note-books in which he wrote down detached thoughts and queries. I shall quote from the first note-book, completed between July 1837 and February 1838: and this is the more worth doing, as it gives us an insight into the condition of his thoughts before the reading of Malthus. The notes are written in his most hurried style, so many words being omitted, that it is often difficult to arrive at the meaning. With a few exceptions (indicated by square brackets)* I have printed the extracts as written; the punctuation, however, has been altered, and a few obvious slips corrected where it seemed necessary. The extracts are not printed in order, but are roughly classified.†

"Propagation explains why modern animals same type as extinct, which is law, almost proved."

"We can see why structure is common in certain countries

* In the extracts from the note-book ordinary brackets represent my father's parentheses.

† On the first page of the note-book, is written "Zoonomia"; this seems to refer to the first few pages in which reproduction by gemmation is discussed, and where the "Zoonomia" is mentioned. Many pages have been cut out of the note-book, probably for use in writing the Sketch of 1844, and these would have no doubt contained the most interesting extracts.

NEWTON LIBRARY CAMB.

© The Complete Work of Charles Darwin Online

when we can hardly believe necessary, but if it was necessary to one forefather, the result would be as it is. Hence antelopes at Cape of Good Hope; marsupials at Australia."

"Countries longest separated greatest differences—if separated from immersage, possibly two distinct types, but each having its representatives—as in Australia."

"Will this apply to whole organic kingdom when our planet first cooled?"

The two following extracts show that he applied the theory of evolution to the "whole organic kingdom" from plants to man.

"If we choose to let conjecture run wild, then animals, our fellow brethren in pain, disease, death, suffering and famine—our slaves in the most laborious works, our companions in our amusements—they may partake [of?] our origin in one common ancestor—we may be all melted together."

"The different intellects of man and animals not so great as between living things without thought (plants), and living things with thought (animals)."

The following extracts are again concerned with an *à priori* view of the probability of the origin of species by descent—"propagation," as he called it.

"The tree of life should perhaps be called the coral of life, base of branches dead; so that passages cannot be seen."

"There never may have been grade between pig and tapir, yet from some common progenitor. Now if the intermediate ranks had produced infinite species, probably the series would have been more perfect."

At another place, speaking of intermediate forms, he says:—

"Cuvier objects to propagation of species by saying, why have not some intermediate forms been discovered between Palæotherium, Megalonyx, Mastodon, and the species now living? Now according to my view (in S. America) parent of all Armadilloes might be brother to Megatherium—uncle now dead."

© The Complete Work of Charles Darwin Online

Speaking elsewhere of intermediate forms, he remarks :—

"Opponents will say—*show them me.* I will answer yes, if you will show me every step between bulldog and greyhound."

Here we see that the case of domestic animals was already present in his mind as bearing on the production of natural species. The disappearance of intermediate forms naturally leads up to the subject of extinction, with which the next extract begins.

"It is a wonderful fact, horse, elephant, and mastodon, dying out about same time in such different quarters.

"Will Mr. Lyell say that some [same ?] circumstance killed it over a tract from Spain to South America ?—(Never.)

"They die, without they change, like golden pippins ; it is a *generation of species* like generation *of individuals.*

"Why does individual die ? To perpetuate certain peculiarities (therefore adaptation), and obliterate accidental varieties, and to accommodate itself to change (for, of course, change, even in varieties, is accommodation). Now this argument applies to species.

"If individual cannot propagate he has no issue—so with species. . . . .

"If *species* generate other *species*, their race is not utterly cut off :—like golden pippins, if produced by seed, go on—otherwise all die.

"The fossil horse generated, in South Africa, zebra—and continued—perished in America.

"All animals of same species are bound together just like buds of plants, which die at one time, though produced either sooner or later. Prove animals like plants—trace gradation between associated and non-associated animals—and the story will be complete."

Here we have the view already alluded to of a term of life impressed on a species.

But in the following note we get extinction connected with

© The Complete Work of Charles Darwin Online

unfavourable variation, and thus a hint is given of natural selection:—

"With respect to extinction, we can easily see that [a] variety of [the] ostrich (Petise), may not be well adapted, and thus perish out; or, on the other hand, like Orpheus [a Galapagos bird], being favourable, many might be produced. This requires [the] principle that the permanent variations produced by confined breeding and changing circumstances are continued and produce[d] according to the adaptation of such circumstances, and therefore that death of species is a consequence (contrary to what would appear from America) of non-adaptation of circumstances."

The first part of the next extract has a similar bearing. The end of the passage is of much interest, as showing that he had at this early date visions of the far-reaching character of his speculations:—

"With belief of transmutation and geographical grouping, we are led to endeavour to discover *causes* of change; the manner of adaptation (wish of parents??), instinct and structure becomes full of speculation and lines of observation. View of generation being condensation,* test of highest organisation intelligible . . . . My theory would give zest to recent and fossil comparative anatomy; it would lead to the study of instincts, heredity, and mind-heredity, whole [of] metaphysics.

"It would lead to closest examination of hybridity, regeneration, causes of change in order to know what we have come from and to what we tend—to what circumstances favour crossing and what prevents it—this, and direct examination of direct passages of structure in species, might lead to laws of change, which would then be the main object of study, to guide our speculations."

The following two extracts have a similar interest; the

* I imagine him to mean that each generation is "condensed" to a small number of the best organized individuals.

© The Complete Work of Charles Darwin Online

second is especially interesting, as it contains the germ of the concluding sentence of the 'Origin of Species':*—

"Before the attraction of gravity discovered it might have been said it was as great a difficulty to account for the movement of all [planets] by one law, as to account for each separate one; so to say that all mammalia were born from one stock, and since distributed by such means as we can recognise, may be thought to explain nothing.

"Astronomers might formerly have said that God fore-ordered each planet to move in its particular destiny. In the same manner God orders each animal created with certain forms in certain countries; but how much more simple and sublime [a] power—let attraction act according to certain law, such are inevitable consequences—let animals be created, then by the fixed laws of generation, such will be their successors.

"Let the powers of transportal be such, and so will be the forms of one country to another—let geological changes go at such a rate, so will be the number and distribution of the species!!"

The three next extracts are of miscellaneous interest:—

"When one sees nipple on man's breast, one does not say some use, but sex not having been determined—so with useless wings under elytra of beetles—born from beetles with wings, and modified—if simple creation merely, would have been born without them."

"In a decreasing population at any one moment fewer closely related (few species of genera); ultimately few genera (for otherwise the relationship would converge sooner), and lastly, perhaps, some one single one. Will not this account

* 'Origin of Species' (edit. i.), p. 490:—"There is a grandeur in this view of life, with its several powers, having been originally breathed into a few forms or into one; and that whilst this planet has gone cycling on according to the fixed law of gravity, from so simple a beginning endless forms most beautiful and most wonderful have been, and are being evolved."

© The Complete Work of Charles Darwin Online

for the odd genera with few species which stand between great groups, which we are bound to consider the increasing ones?"

The last extract which I shall quote gives the germ of his theory of the relation between alpine plants in various parts of the world, in the publication of which he was forestalled by E. Forbes (see Vol. I. p. 88). He says, in the 1837 note-book, that alpine plants, "formerly descended lower, therefore [they are] species of lower genera altered, or northern plants."

When we turn to the Sketch of his theory, written in 1844 (still therefore before the second edition of the 'Journal' was completed), we find an enormous advance made on the note-book of 1837. The Sketch is in fact a surprisingly complete presentation of the argument afterwards familiar to us in the 'Origin of Species.' There is some obscurity as to the date of the short Sketch which formed the basis of the 1844 Essay. We know from his own words (Vol. I. p. 184), that it was in June 1842 that he first wrote out a short sketch of his views.* This statement is given with so much circumstance that it is almost impossible to suppose that it contains an error of date. It agrees also with the following extract from his Diary.

"1842. May 18th. Went to Maer.

"June 15th to Shrewsbury, and on 18th to Capel Curig. During my stay at Maer and Shrewsbury (five years after commencement) wrote pencil-sketch of species theory."

Again in the introduction to the 'Origin,' p. 1, he writes, "after an interval of five years' work," [from 1837, *i.e.* in 1842,] "I allowed myself to speculate on the subject, and drew up some short notes."

Nevertheless in the letter signed by Sir C. Lyell and Sir J. D. Hooker, which serves as an introduction to the joint paper of Messrs. C. Darwin and A. Wallace on the 'Tendency

* This version I cannot find, and it was probably destroyed, like so much of his MS., after it had been enlarged and re-copied in 1844.

© The Complete Work of Charles Darwin Online

of Species to form Varieties,' * the essay of 1844 (extracts from which form part of the paper) is said to have been "sketched in 1839, and copied in 1844." This statement is obviously made on the authority of a note written in my father's hand across the Table of Contents of the 1844 Essay. It is to the following effect: "This was sketched in 1839, and copied out in full, as here written and read by you in 1844." I conclude that this note was added in 1858, when the MS. was sent to Sir J. D. Hooker (see Letter of June 29, 1858, Vol. II. p. 119). There is also some further evidence on this side of the question. Writing to Mr. Wallace (Jan. 25, 1859) my father says:—"Every one whom I have seen has thought your paper very well written and interesting. It puts my extracts (written in 1839, now just twenty years ago!), which I must say in apology were never for an instant intended for publication, into the shade." The statement that the earliest sketch was written in 1839 has been frequently made in biographical notices of my father, no doubt on the authority of the 'Linnean Journal,' but it must, I think, be considered as erroneous. The error may possibly have arisen in this way. In writing on the Table of Contents of the 1844 MS. that it was sketched in 1839, I think my father may have intended to imply that the framework of the theory was clearly thought out by him at that date. In the Autobiography (p. 88) he speaks of the time, "about 1839, when the theory was clearly conceived," meaning, no doubt, the end of 1838 and beginning of 1839, when the reading of Malthus had given him the key to the idea of natural selection. But this explanation does not apply to the letter to Mr. Wallace; and with regard to the passage † in the 'Linnean Journal' it is difficult to understand how it should have been allowed to

* 'Linn. Soc. Journal,' 1858, p. 45.

† My father certainly saw the proofs of the paper, for he added a footnote apologising for the style of the extracts, on the ground that the "work was never intended for publication."

© The Complete Work of Charles Darwin Online

remain as it now stands, conveying, as it clearly does, the impression that 1839 was the date of his earliest written sketch.

The sketch of 1844 is written in a clerk's hand, in two hundred and thirty-one pages folio, blank leaves being alternated with the MS. with a view to amplification. The text has been revised and corrected, criticisms being pencilled by himself on the margin. It is divided into two parts: I. "On the variation of Organic Beings under Domestication and in their Natural State." II. "On the Evidence favourable and opposed to the view that Species are naturally formed races descended from common Stocks." The first part contains the main argument of the 'Origin of Species.' It is founded, as is the argument of that work, on the study of domestic animals, and both the Sketch and the 'Origin' open with a chapter on variation under domestication and on artificial selection. This is followed, in both essays, by discussions on variation under nature, on natural selection, and on the struggle for life. Here, any close resemblance between the two essays with regard to arrangement ceases. Chapter III. of the Sketch, which concludes the first part, treats of the variations which occur in the instincts and habits of animals, and thus corresponds to some extent with Chapter VII. of the 'Origin' (1st edit.). It thus forms a complement to the chapters which deal with variation in structure. It seems to have been placed thus early in the Essay to prevent the hasty rejection of the whole theory by a reader to whom the idea of natural selection acting on instincts might seem impossible. This is the more probable, as the Chapter on Instinct in the 'Origin' is specially mentioned (Introduction, p. 5) as one of the "most apparent and gravest difficulties on the theory." Moreover the chapter in the Sketch ends with a discussion, "whether any particular corporeal structures . . . . . are so wonderful as to justify the rejection *primâ facie* of our theory." Under this heading comes the discussion of the eye, which in the 'Origin' finds its place in Chapter VI.

© The Complete Work of Charles Darwin Online

under "Difficulties on Theory." The second part seems to have been planned in accordance with his favourite point of view with regard to his theory. This is briefly given in a letter to Dr. Asa Gray, November 11th, 1859: "I cannot possibly believe that a false theory would explain so many classes of facts, as I think it certainly does explain. On these grounds I drop my anchor, and believe that the difficulties will slowly disappear." On this principle, having stated the theory in the first part, he proceeds to show to what extent various wide series of facts can be explained by its means.

Thus the second part of the Sketch corresponds roughly to the nine concluding Chapters of the First Edition of the 'Origin.' But we must exclude Chapter VII. ('Origin') on Instinct, which forms a chapter in the first part of the Sketch, and Chapter VIII. ('Origin') on Hybridism, a subject treated in the Sketch with 'Variation under Nature' in the first part.

The following list of the chapters of the second part of the Sketch will illustrate their correspondence with the final chapters of the 'Origin.'

Chapter I. "On the kind of intermediateness necessary, and the number of such intermediate forms."

This includes a geological discussion, and corresponds to parts of Chapters VI. and IX. of the 'Origin.'

Chapter II. "The gradual appearance and disappearance of organic beings." Corresponds to Chapter X. of the 'Origin.'

Chapter III. "Geographical Distribution." Corresponds to Chapters XI. and XII. of the 'Origin.'

Chapter IV. "Affinities and Classification of Organic beings."

Chapter V. "Unity of Type," Morphology, Embryology.

Chapter VI. Rudimentary Organs.

These three chapters correspond to Chapter XII. of the 'Origin.'

Chapter VII. Recapitulation and Conclusion. The final

© The Complete Work of Charles Darwin Online

sentence of the Sketch, which we saw in its first rough form in the Note Book of 1837, closely resembles the final sentence of the 'Origin,' much of it being identical. The 'Origin' is not divided into two "Parts," but we see traces of such a division having been present in the writer's mind, in this resemblance between the second part of the Sketch and the final chapters of the 'Origin.' That he should speak * of the chapters on transition, on instinct, on hybridism, and on the geological record, as forming a group, may be due to the division of his early MS. into two parts.

Mr. Huxley, who was good enough to read the Sketch at my request, while remarking that the "main lines of argument" and the illustrations employed are the same, points out that in the 1844 Essay, "much more weight is attached to the influence of external conditions in producing variation, and to the inheritance of acquired habits than in the 'Origin.'"

It is extremely interesting to find in the Sketch the first mention of principles familiar to us in the 'Origin of Species.' Foremost among these may be mentioned the principle of Sexual Selection, which is clearly enunciated. The important form of selection known as "unconscious," is also given. Here also occurs a statement of the law that peculiarities tend to appear in the offspring at an age corresponding to that at which they occurred in the parent.

Professor Newton, who was so kind as to look through the 1844 Sketch, tells me that my father's remarks on the migration of birds, incidentally given in more than one passage, show that he had anticipated the views of some later writers.

With regard to the general style of the Sketch, it is not to be expected that it should have all the characteristics of the 'Origin,' and we do not, in fact, find that balance and control, that concentration and grasp, which are so striking in the work of 1859.

* 'Origin,' Introduction, p. 5.

© The Complete Work of Charles Darwin Online

In the Autobiography (Vol. I. p. 84) my father has stated what seemed to him the chief flaw of the 1844 Sketch; he had overlooked "one problem of great importance," the problem of the divergence of character. This point is discussed in the 'Origin of Species,' but, as it may not be familiar to all readers, I will give a short account of the difficulty and its solution. The author begins by stating that varieties differ from each other less than species, and then goes on: "Nevertheless, according to my view, varieties are species in process of formation. . . . . How then does the lesser difference between varieties become augmented into the greater difference between species." * He shows how an analogous divergence takes place under domestication where an originally uniform stock of horses has been split up into race-horses, dray-horses, &c., and then goes on to explain how the same principle applies to natural species. "From the simple circumstance that the more diversified the descendants from any one species become in structure, constitution, and habits, by so much will they be better enabled to seize on many and widely diversified places in the polity of nature, and so be enabled to increase in numbers."

The principle is exemplified by the fact that if on one plot of ground a single variety of wheat be sown, and on to another a mixture of varieties, in the latter case the produce is greater. More individuals have been able to exist because they were not all of the same variety. An organism becomes more perfect and more fitted to survive when by division of labour the different functions of life are performed by different organs. In the same way a species becomes more efficient and more able to survive when different sections of the species become differentiated so as to fill different stations.

In reading the Sketch of 1844, I have found it difficult to recognise, as a flaw in the Essay, the absence of any definite statement of the principle of divergence. Descent with

* 'Origin,' 1st edit. p. 111.

© The Complete Work of Charles Darwin Online

modification implies divergence, and we become so habituated to a belief in descent, and therefore in divergence, that we do not notice the absence of proof that divergence is in itself an advantage. As shown in the Autobiography, my father in 1876 found it hardly credible that he should have overlooked the problem and its solution.

The following letter will be more in place here than its chronological position, since it shows what was my father's feeling as to the value of the Sketch at the time of its completion.] . . . .

C. Darwin to Mrs. Darwin.

Down, July 5, 1844.

. . . I have just finished my sketch of my species theory. If, as I believe, my theory in time be accepted even by one competent judge, it will be a considerable step in science.

I therefore write this in case of my sudden death, as my most solemn and last request, which I am sure you will consider the same as if legally entered in my will, that you will devote £400 to its publication, and further, will yourself, or through Hensleigh,* take trouble in promoting it. I wish that my sketch be given to some competent person, with this sum to induce him to take trouble in its improvement and enlargement. I give to him all my books on Natural History, which are either scored or have references at the end to the pages, begging him carefully to look over and consider such passages as actually bearing, or by possibility bearing, on this subject. I wish you to make a list of all such books as some temptation to an editor. I also request that you will hand over [to] him all those scraps roughly divided in eight or ten brown paper portfolios. The scraps, with copied quotations from various works, are those which may aid my editor. I also request that you, or some amanuensis, will aid

* Mr. H. Wedgwood.

© The Complete Work of Charles Darwin Online

PHILOSOPHICAL ... 1901

in deciphering any of the scraps which the editor may think possibly of use. I leave to the editor's judgment whether to interpolate these facts in the text, or as notes, or under appendices. As the looking over the references and scraps will be a long labour, and as the *correcting* and enlarging and altering my sketch will also take considerable time, I leave this sum of £400 as some remuneration, and any profits from the work. I consider that for this the editor is bound to get the sketch published either at a publisher's or his own risk. Many of the scraps in the portfolios contain mere rude suggestions and early views, now useless, and many of the facts will probably turn out as having no bearing on my theory.

With respect to editors, Mr. Lyell would be the best if he would undertake it; I believe he would find the work pleasant, and he would learn some facts new to him. As the editor must be a geologist as well as a naturalist, the next best editor would be Professor Forbes of London. The next best (and quite best in many respects) would be Professor Henslow. Dr. Hooker would be *very* good. The next, Mr. Strickland.* If none of these would undertake it, I would request you to consult with Mr. Lyell, or some other capable man for some editor, a geologist and naturalist. Should one other hundred pounds make the difference of procuring a good editor, I request earnestly that you will raise £500.

My remaining collections in Natural History may be given to any one or any museum where [they] would be accepted....

[The following note seems to have formed part of the original letter, but may have been of later date:

"Lyell, especially with the aid of Hooker (and if any good zoological aid), would be best of all. Without an editor will pledge himself to give up time to it, it would be of no use paying such a sum.

* After Mr. Strickland's name comes the following sentence, which has been erased, but remains legible. "Professor Owen would be very good; but I presume he would not undertake such a work."

© The Complete Work of Charles Darwin Online

"If there should be any difficulty in getting an editor who would go thoroughly into the subject, and think of the bearing of the passages marked in the books and copied out of scraps of paper, then let my sketch be published as it is, stating that it was done several years ago * and from memory without consulting any works, and with no intention of publication in its present form."

The idea that the Sketch of 1844 might remain, in the event of his death, as the only record of his work, seems to have been long in his mind, for in August 1854, when he had finished with the Cirripedes, and was thinking of beginning his "species work," he added on the back of the above letter, "Hooker by far best man to edit my species volume. August 1854."]

* The words "several years ago and," seem to have been added at a later date.

© The Complete Work of Charles Darwin Online

CHAPTER II.

THE GROWTH OF THE 'ORIGIN OF SPECIES.

LETTERS, 1843–1856.

[THE history of my father's life is told more completely in his correspondence with Sir J. D. Hooker than in any other series of letters; and this is especially true of the history of the growth of the 'Origin of Species.' This, therefore, seems an appropriate place for the following notes, which Sir Joseph Hooker has kindly given me. They give, moreover, an interesting picture of his early friendship with my father:—

"My first meeting with Mr. Darwin was in 1839, in Trafalgar Square. I was walking with an officer who had been his shipmate for a short time in the *Beagle* seven years before, but who had not, I believe, since met him. I was introduced; the interview was of course brief, and the memory of him that I carried away and still retain was that of a rather tall and rather broad-shouldered man, with a slight stoop, an agreeable and animated expression when talking, beetle brows, and a hollow but mellow voice; and that his greeting of his old acquaintance was sailor-like—that is, delightfully frank and cordial. I observed him well, for I was already aware of his attainments and labours, derived from having read various proof-sheets of his then unpublished 'Journal.' These had been submitted to Mr. (afterwards Sir Charles) Lyell by Mr. Darwin, and by him sent to his father, Ch. Lyell, Esq., of Kinnordy, who (being a very old friend of my father, and taking a kind interest in my projected career as a naturalist) had allowed me to peruse them. At this time

© The Complete Work of Charles Darwin Online

I was hurrying on my studies, so as to take my degree before volunteering to accompany Sir James Ross in the Antarctic Expedition, which had just been determined on by the Admiralty; and so pressed for time was I, that I used to sleep with the sheets of the 'Journal' under my pillow, that I might read them between waking and rising. They impressed me profoundly, I might say despairingly, with the variety of acquirements, mental and physical, required in a naturalist who should follow in Darwin's footsteps, whilst they stimulated me to enthusiasm in the desire to travel and observe.

"It has been a permanent source of happiness to me that I knew so much of Mr. Darwin's scientific work so many years before that intimacy began which ripened into feelings as near to those of reverence for his life, works, and character as is reasonable and proper. It only remains to add to this little episode that I received a copy of the 'Journal' complete,—a gift from Mr. Lyell,—a few days before leaving England.

"Very soon after the return of the Antarctic Expedition my correspondence with Mr. Darwin began (December, 1843) by his sending me a long letter, warmly congratulating me on my return to my family and friends, and expressing a wish to hear more of the results of the expedition, of which he had derived some knowledge from private letters of my own (written to or communicated through Mr. Lyell). Then, plunging at once into scientific matters, he directed my attention to the importance of correlating the Fuegian Flora with that of the Cordillera and of Europe, and invited me to study the botanical collections which he had made in the Galapagos Islands, as well as his Patagonian and Fuegian plants.

"This led to me sending him an outline of the conclusions I had formed regarding the distribution of plants in the southern regions, and the necessity of assuming the destruction of considerable areas of land to account for the relations

© The Complete Work of Charles Darwin Online

of the flora of the so-called Antarctic Islands. I do not suppose that any of these ideas were new to him, but they led to an animated and lengthy correspondence full of instruction."

Here follows the letter (1843) to Sir J. D. Hooker above referred to.]

MY DEAR SIR,—I had hoped before this time to have had the pleasure of seeing you and congratulating you on your safe return from your long and glorious voyage. But as I seldom go to London, we may not yet meet for some time—without you are led to attend the Geological meetings.

I am anxious to know what you intend doing with all your materials—I had so much pleasure in reading parts of some of your letters, that I shall be very sorry if I, as one of the public, have no opportunity of reading a good deal more. I suppose you are very busy now and full of enjoyment: how well I remember the happiness of my first few months of England—it was worth all the discomforts of many a gale! But I have run from the subject, which made me write, of expressing my pleasure that Henslow (as he informed me a few days since by letter) has sent to you my small collection of plants. You cannot think how much pleased I am, as I feared they would have been all lost, and few as they are, they cost me a good deal of trouble. There are a very few notes, which I believe Henslow has got, describing the habitats, &c., of some few of the more remarkable plants. I paid particular attention to the Alpine flowers of Tierra del Fuego, and I am sure I got every plant which was in flower in Patagonia at the seasons when we were there. I have long thought that some general sketch of the Flora of the point of land, stretching so far into the southern seas, would be very curious. Do make comparative remarks on the species allied to the European species, for the advantage of botanical ignoramuses like myself. It has often struck me as a curious

© The Complete Work of Charles Darwin Online

point to find out, whether there are many European genera in T. del Fuego which are not found along the ridge of the Cordillera; the separation in such case would be so enormous. Do point out in any sketch you draw up, what genera are American and what European, and how great the differences of the species are, when the genera are European, for the sake of the ignoramuses.

I hope Henslow will send you my Galapagos plants (about which Humboldt even expressed to me considerable curiosity) —I took much pains in collecting all I could. A Flora of this archipelago would, I suspect, offer a nearly parallel case to that of St. Helena, which has so long excited interest. Pray excuse this long rambling note, and believe me, my dear sir, yours very sincerely,

C. DARWIN.

Will you be so good as to present my respectful compliments to Sir W. Hooker.

[Referring to Sir J. D. Hooker's work on the Galapagos Flora, my father wrote in 1846:

"I cannot tell you how delighted and astonished I am at the results of your examination; how wonderfully they support my assertion on the differences in the animals of the different islands, about which I have always been fearful."

Again he wrote (1849):—

"I received a few weeks ago your Galapagos papers,* and I have read them since being here. I really cannot express too strongly my admiration of the geographical discussion: to my judgment it is a perfect model of what such a paper should be; it took me four days to read and think over. How interesting the Flora of the Sandwich Islands appears to be, how I wish there were materials for you to treat its

* These papers include the results of Sir J. D. Hooker's examination of my father's Galapagos plants, and were published by the Linnean Society in 1849.

© The Complete Work of Charles Darwin Online

flora as you have done the Galapagos. In the Systematic paper I was rather disappointed in not finding general remarks on affinities, structures, &c., such as you often give in conversation, and such as De Candolle and St. Hilaire introduced in almost all their papers, and which make them interesting even to a non-Botanist."

"Very soon afterwards [continues Sir J. D. Hooker] in a letter dated January 1844, the subject of the 'Origin of Species' was brought forward by him, and I believe that I was the first to whom he communicated his then new ideas on the subject, and which being of interest as a contribution to the history of Evolution, I here copy from his letter":—]

C. Darwin to J. D. Hooker.

[January 11th, 1844.]

. . . Besides a general interest about the southern lands, I have been now ever since my return engaged in a very presumptuous work, and I know no one individual who would not say a very foolish one. I was so struck with the distribution of the Galapagos organisms, &c. &c., and with the character of the American fossil mammifers, &c. &c., that I determined to collect blindly every sort of fact, which could bear any way on what are species. I have read heaps of agricultural and horticultural books, and have never ceased collecting facts. At last gleams of light have come, and I am almost convinced (quite contrary to the opinion I started with) that species are not (it is like confessing a murder) immutable. Heaven forfend me from Lamarck nonsense of a "tendency to progression," "adaptations from the slow willing of animals," &c.! But the conclusions I am led to are not widely different from his; though the means of change are wholly so. I think I have found out (here's presumption!) the simple way by which species become exquisitely adapted to various ends. You will now groan, and think to

© The Complete Work of Charles Darwin Online

yourself, "on what a man have I been wasting my time and writing to." I should, five years ago, have thought so. . . .

[The following letter written on February 23, 1844, shows that the acquaintanceship with Sir J. D. Hooker was then fast ripening into friendship. The letter is chiefly of interest as showing the sort of problems then occupying my father's mind :]

DEAR HOOKER,—I hope you will excuse the freedom of my address, but I feel that as co-circum-wanderers and as fellow labourers (though myself a very weak one) we may throw aside some of the old-world formality. . . . I have just finished a little volume on the volcanic islands which we visited. I do not know how far you care for dry simple geology, but I hope you will let me send you a copy. I suppose I can send it from London by common coach conveyance. . . .

. . . I am going to ask you some *more* questions, though I dare say, without asking them, I shall see answers in your work, when published, which will be quite time enough for my purposes. First for the Galapagos, you will see in my Journal, that the Birds, though peculiar species, have a most obvious S. American aspect: I have just ascertained the same thing holds good with the sea-shells. Is it so with those plants which are peculiar to this archipelago; you state that their numerical proportions are continental (is not this a very curious fact?) but are they related in forms to S. America. Do you know of any other case of an archipelago, with the separate islands possessing distinct representative species? I have always intended (but have not yet done so) to examine Webb and Berthelot on the Canary Islands for this object. Talking with Mr. Bentham, he told me that the separate islands of the Sandwich Archipelago possessed distinct representative species of the same genera of Labiatæ: would not this be worth your enquiry? How is it with the

© The Complete Work of Charles Darwin Online

Azores; to be sure the heavy western gales would tend to diffuse the same species over that group.

I hope you will (I dare say my hope is quite superfluous) attend to this general kind of affinity in isolated islands, though I suppose it is more difficult to perceive this sort of relation in plants, than in birds or quadrupeds, the groups of which are, I fancy, rather more confined. Can St. Helena be classed, though remotely, either with Africa or S. America? From some facts, which I have collected, I have been led to conclude that the fauna of mountains are *either* remarkably similar (sometimes in the presence of the same species and at other times of same genera), *or* that they are remarkably dissimilar; and it has occurred to me that possibly part of this peculiarity of the St. Helena and Galapagos floras may be attributed to a great part of these two Floras being mountain Floras. I fear my notes will hardly serve to distinguish much of the habitats of the Galapagos plants, but they may in some cases; most, if not all, of the green, leafy plants come from the summits of the islands, and the thin brown leafless plants come from the lower arid parts: would you be so kind as to bear this remark in mind, when examining my collection.

I will trouble you with only one other question. In discussion with Mr. Gould, I found that in most of the genera of birds which range over the whole or greater part of the world, the individual species have wider ranges, thus the Owl is mundane, and many of the species have very wide ranges. So I believe it is with land and fresh-water shells—and I might adduce other cases. Is it not so with Cryptogamic plants; have not most of the species wide ranges, in those genera which are mundane? I do not suppose that the converse holds, viz.—that when a species has a wide range, its genus also ranges wide. Will you so far oblige me by occasionally thinking over this? It would cost me vast trouble to get a list of mundane phanerogamic genera and

© The Complete Work of Charles Darwin Online

then search how far the species of these genera are apt to range wide in their several countries; but you might occasionally, in the course of your pursuits, just bear this in mind, though perhaps the point may long since have occurred to you or other Botanists. Geology is bringing to light interesting facts, concerning the ranges of shells; I think it is pretty well established, that according as the geographical range of a species is wide, so is its persistence and duration in time. I hope you will try to grudge as little as you can the trouble of my letters, and pray believe me very truly yours,

C. DARWIN.

P.S. I should feel extremely obliged for your kind offer of the sketch of Humboldt; I venerate him, and after having had the pleasure of conversing with him in London, I shall still more like to have any portrait of him.

[What follows is quoted from Sir J. D. Hooker's notes.

"The next act in the drama of our lives opens with personal intercourse. This began with an invitation to breakfast with him at his brother's (Erasmus Darwin's) house in Park Street; which was shortly afterwards followed by an invitation to Down to meet a few brother Naturalists. In the short intervals of good health that followed the long illnesses which oftentimes rendered life a burthen to him, between 1844 and 1847, I had many such invitations, and delightful they were. A more hospitable and more attractive home under every point of view could not be imagined—of Society there were most often Dr. Falconer, Edward Forbes, Professor Bell, and Mr. Waterhouse—there were long walks, romps with the children on hands and knees, music that haunts me still. Darwin's own hearty manner, hollow laugh, and thorough enjoyment of home life with friends; strolls with him all together, and interviews with us one by one in his study, to discuss questions in any branch of biological or physical knowledge that we had followed; and which I at any rate

© The Complete Work of Charles Darwin Online

always left with the feeling that I had imparted nothing and carried away more than I could stagger under. Latterly, as his health became more seriously affected, I was for days and weeks the only visitor, bringing my work with me and enjoying his society as opportunity offered. It was an established rule that he every day pumped me, as he called it, for half an hour or so after breakfast in his study, when he first brought out a heap of slips with questions botanical, geographical, &c., for me to answer, and concluded by telling me of the progress he had made in his own work, asking my opinion on various points. I saw no more of him till about noon, when I heard his mellow ringing voice calling my name under my window—this was to join him in his daily forenoon walk round the sand-walk.* On joining him I found him in a rough grey shooting-coat in summer, and thick cape over his shoulders in winter, and a stout staff in his hand; away we trudged through the garden, where there was always some experiment to visit, and on to the sand-walk, round which a fixed number of turns were taken, during which our conversation usually ran on foreign lands and seas, old friends, old books, and things far off to both mind and eye.

"In the afternoon there was another such walk, after which he again retired till dinner if well enough to join the family; if not, he generally managed to appear in the drawing-room, where seated in his high chair, with his feet in enormous carpet shoes, supported on a high stool—he enjoyed the music or conversation of his family."

Here follows a series of letters illustrating the growth of my father's views, and the nature of his work during this period.]

* See Vol. I. p. 115.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down [1844].

. . . The conclusion, which I have come at is, that those areas, in which species are most numerous, have oftenest been divided and isolated from other areas, united and again divided; a process implying antiquity and some changes in the external conditions. This will justly sound very hypothetical. I cannot give my reasons in detail; but the most general conclusion, which the geographical distribution of all organic beings, appears to me to indicate, is that isolation is the chief concomitant or cause of the appearance of *new* forms (I well know there are some staring exceptions). Secondly, from seeing how often the plants and animals swarm in a country, when introduced into it, and from seeing what a vast number of plants will live, for instance in England, if kept *free from weeds, and native plants*, I have been led to consider that the spreading and number of the organic beings of any country depend less on its external features, than on the number of forms, which have been there originally created or produced. I much doubt whether you will find it possible to explain the number of forms by proportional differences of exposure; and I cannot doubt if half the species in any country were destroyed or had not been created, yet that country would appear to us fully peopled. With respect to original creation or production of new forms, I have said that isolation appears the chief element. Hence, with respect to terrestrial productions, a tract of country, which had oftenest within the late geological periods subsided and been converted into islands, and reunited, I should expect to contain most forms.

But such speculations are amusing only to one's self, and in this case useless, as they do not show any direct line of observation: if I had seen how hypothetical [is] the little, which I

© The Complete Work of Charles Darwin Online

have unclearly written, I would not have troubled you with the reading of it. Believe me,—at last not hypothetically,

Yours very sincerely,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, 1844.

. . . I forget my last letter, but it must have been a very silly one, as it seems I gave my notion of the number of species being in great degree governed by the degree to which the area had been often isolated and divided; I must have been cracked to have written it, for I have no evidence, without a person be willing to admit all my views, and then it does follow; but in my most sanguine moments, all I expect, is that I shall be able to show even to sound Naturalists, that there are two sides to the question of the immutability of species;—that facts can be viewed and grouped under the notion of allied species having descended from common stocks. With respect to books on this subject, I do not know of any systematical ones, except Lamarck's, which is veritable rubbish; but there are plenty, as Lyell, Pritchard, &c., on the view of the immutability. Agassiz lately has brought the strongest argument in favour of immutability. Isidore G. St. Hilaire has written some good Essays, tending towards the mutability-side, in the 'Suites à Buffon,' entitled "Zoolog. Générale." Is it not strange that the author of such a book as the 'Animaux sans Vertèbres' should have written that insects, which never see their eggs, should *will* (and plants, their seeds) to be of particular forms, so as to become attached to particular objects. The other common (specially Germanic) notion is hardly less absurd, viz. that climate, food, &c., should make a Pediculus formed to climb hair, or wood-pecker to climb trees. I believe all these absurd views arise from no one having, as far as I know, approached the subject on the side of variation under domest-

© The Complete Work of Charles Darwin Online

ication, and having studied all that is known about domestication. I was very glad to hear your criticism on island-floras and on non-diffusion of plants: the subject is too long for a letter: I could defend myself to some considerable extent, but I doubt whether successfully in your eyes, or indeed in my own. . . .

C. Darwin to J. D. Hooker.

Down, [July, 1844.]

. . . I am now reading a wonderful book for facts on variation—Bronn, 'Geschichte der Natur.' It is stiff German: it forestalls me, sometimes I think delightfully, and sometimes cruelly. You will be ten times hereafter more horrified at me than at H. Watson. I hate arguments from results, but on my views of descent, really Natural History becomes a sublimely grand result-giving subject (now you may quiz me for so foolish an escape of mouth). . . . I must leave this letter till to-morrow, for I am tired; but I so enjoy writing to you, that I must inflict a little more on you.

Have you any good evidence for absence of insects in small islands? I found thirteen species in Keeling Atoll. Flies are good fertilizers, and I have seen a microscopic Thrips and a Cecidomya take flight from a flower in the direction of another with pollen adhering to them. In Arctic countries a bee seems to go as far N. as any flower. . . .

C. Darwin to J. D. Hooker.

Shrewsbury [September, 1845].

MY DEAR HOOKER,—I write a line to say that Cosmos * arrived quite safely (N.B. One sheet came loose in Pt. I.), and to thank you for your nice note. I have just begun the introduction, and groan over the style, which in such parts is full half the battle. How true many of the remarks are (*i.e.* as far as I can understand the wretched English) on the scenery; it is an exact expression of one's own thoughts.

* A translation of Humboldt's 'Kosmos.'

© The Complete Work of Charles Darwin Online

I wish I ever had any books to lend you in return for the many you have lent me. . . .

All of what you kindly say about my species work does not alter one iota my long self-acknowledged presumption in accumulating facts and speculating on the subject of variation, without having worked out my due share of species. But now for nine years it has been anyhow the greatest amusement to me.

Farewell, my dear Hooker, I grieve more than you can well believe, over our prospect of so seldom meeting.

I have never perceived but one fault in you, and that you have grievously, viz. modesty; you form an exception to Sydney Smith's aphorism, that merit and modesty have no other connection, except in their first letter. Farewell,

C. DARWIN.

C. Darwin to L. Jenyns (*Blomefield*).

Down, Oct. 12th [1845].

MY DEAR JENYNS,—Thanks for your note. I am sorry to say I have not even the tail-end of a fact in English Zoology to communicate. I have found that even trifling observations require, in my case, some leisure and energy, both of which ingredients I have had none to spare, as writing my Geology thoroughly expends both. I had always thought that I would keep a journal and record everything, but in the way I now live I find I observe nothing to record. Looking after my garden and trees, and occasionally a very little walk in an idle frame of my mind, fills up every afternoon in the same manner. I am surprised that with all your parish affairs, you have had time to do all that which you have done. I shall be very glad to see your little work * (and

* Mr. Jenyns' 'Observations in Natural History.' It is prefaced by an Introduction on "Habits of observing as connected with the study of Natural History," and followed by a "Calendar of Periodic Phenomena in Natural History," with "Remarks on the importance of such Registers."

© The Complete Work of Charles Darwin Online

proud should I have been if I could have added a single fact to it). My work on the species question has impressed me very forcibly with the importance of all such works as your intended one, containing what people are pleased generally to call trifling facts. These are the facts which make one understand the working or economy of nature. There is one subject, on which I am very curious, and which perhaps you may throw some light on, if you have ever thought on it; namely, what are the checks and what the periods of life,— by which the increase of any given species is limited. Just calculate the increase of any bird, if you assume that only half the young are reared, and these breed: within the *natural* (*i.e.* if free from accidents) life of the parents the number of individuals will become enormous, and I have been much surprised to think how great destruction *must* annually or occasionally be falling on every species, yet the means and period of such destruction is scarcely perceived by us.

I have continued steadily reading and collecting facts on variation of domestic animals and plants, and on the question of what are species. I have a grand body of facts, and I think I can draw some sound conclusions. The general conclusions at which I have slowly been driven from a directly opposite conviction, is that species are mutable, and that allied species are co-descendants from common stocks. I know how much I open myself to reproach for such a conclusion, but I have at least honestly and deliberately come to it. I shall not publish on this subject for several years. At present I am on the Geology of South America. I hope to pick up from your book some facts on slight variations in structure or instincts in the animals of your acquaintance.

Believe me, ever yours,

C. DARWIN.

© The Complete Work of Charles Darwin Online

*C. Darwin to L. Jenyns.**

Down, [1845?].

MY DEAR JENYNS,—I am very much obliged to you for the trouble you have taken in having written me so long a note. The question of where, when, and how the check to the increase of a given species falls appears to me particularly interesting, and our difficulty in answering it shows how really ignorant we are of the lives and habits of our most familiar species. I was aware of the bare fact of old birds driving away their young, but had never thought of the effect you so clearly point out, of local gaps in number being thus immediately filled up. But the original difficulty remains; for if your farmers had not killed your sparrows and rooks, what would have become of those which now immigrate into your parish? in the middle of England one is too far distant from the natural limits of the rook and sparrow to suppose that the young are thus far expelled from Cambridgeshire. The check must fall heavily at some time of each species' life; for, if one calculates that only half the progeny are reared and bred, how enormous is the increase! One has, however, no business to feel so much surprise at one's ignorance, when one knows how impossible it is without statistics to conjecture the duration of life and percentage of deaths to births in mankind. If it could be shown that apparently the birds of passage *which breed here* and increase, return in the succeeding years in about the same number, whereas those that come here for their winter and non-breeding season annually, come here with the same numbers, but return with greatly decreased numbers, one would know (as indeed seems probable) that the check fell chiefly on full-grown birds in the winter season, and not on the eggs and very young birds, which has appeared to me often the most probable period. If at any time any remarks on this subject should

* Rev. L. Blomefield.

© The Complete Work of Charles Darwin Online

occur to you, I should be most grateful for the benefit of them.

With respect to my far distant work on species, I must have expressed myself with singular inaccuracy if I led you to suppose that I meant to say that my conclusions were inevitable. They have become so, after years of weighing puzzles, to myself *alone*; but in my wildest day-dream, I never expect more than to be able to show that there are two sides to the question of the immutability of species, *i.e.* whether species are *directly* created or by intermediate laws (as with the life and death of individuals). I did not approach the subject on the side of the difficulty in determining what are species and what are varieties, but (though why I should give you such a history of my doings it would be hard to say) from such facts as the relationship between the living and extinct mammifers in South America, and between those living on the Continent and on adjoining islands, such as the Galapagos. It occurred to me that a collection of all such analogous facts would throw light either for or against the view of related species being co-descendants from a common stock. A long searching amongst agricultural and horticultural books and people makes me believe (I well know how absurdly presumptuous this must appear) that I see the way in which new varieties become exquisitely adapted to the external conditions of life and to other surrounding beings. I am a bold man to lay myself open to being thought a complete fool, and a most deliberate one. From the nature of the grounds which make me believe that species are mutable in form, these grounds cannot be restricted to the closest-allied species; but how far they extend I cannot tell, as my reasons fall away by degrees, when applied to species more and more remote from each other. Pray do not think that I am so blind as not to see that there are numerous immense difficulties in my notions, but they appear to me less than on the common view. I have

© The Complete Work of Charles Darwin Online

drawn up a sketch and had it copied (in 200 pages) of my conclusions; and if I thought at some future time that you would think it worth reading, I should, of course, be most thankful to have the criticism of so competent a critic. Excuse this very long and egotistical and ill-written letter, which by your remarks you have led me into, and believe me,

Yours very truly,

C. DARWIN.

C. Darwin to L. Jenyns.

Down, Oct. 17th, 1846.

DEAR JENYNS,—I have taken a most ungrateful length of time in thanking you for your very kind present of your 'Observations.' But I happened to have had in hand several other books, and have finished yours only a few days ago. I found it very pleasant reading, and many of your facts interested me much. I think I was more interested, which is odd, with your notes on some of the lower animals than on the higher ones. The introduction struck me as very good; but this is what I expected, for I well remember being quite delighted with a preliminary essay to the first number of the 'Annals of Natural History.' I missed one discussion, and think myself ill-used, for I remember your saying you would make some remarks on the weather and barometer, as a guide for the ignorant in prediction. I had also hoped to have perhaps met with some remarks on the amount of variation in our common species. Andrew Smith once declared he would get some hundreds of specimens of larks and sparrows from all parts of Great Britain, and see whether, with finest measurements, he could detect any proportional variations in beaks or limbs, &c. This point interests me from having lately been skimming over the absurdly opposite conclusions of Gloger and Brehm; the one making half-a-dozen species out of every common bird, and the other

© The Complete Work of Charles Darwin Online

turning so many reputed species into one. Have you ever done anything of this kind, or have you ever studied Gloger's or Brehm's works? I was interested in your account of the martins, for I had just before been utterly perplexed by noticing just such a proceeding as you describe: I counted seven, one day lately, visiting a single nest and sticking dirt on the adjoining wall. I may mention that I once saw some squirrels eagerly splitting those little semi-transparent spherical galls on the back of oak-leaves for the maggot within; so that they are insectivorous. A *Cychrus rostratus* once squirted into my eyes and gave me extreme pain; and I must tell you what happened to me on the banks of the Cam, in my early entomological days: under a piece of bark I found two *Carabi* (I forget which), and caught one in each hand, when lo and behold I saw a sacred *Panagæus crux major!* I could not bear to give up either of my *Carabi*, and to lose *Panagæus* was out of the question; so that in despair I gently seized one of the *Carabi* between my teeth, when to my unspeakable disgust and pain the little inconsiderate beast squirted his acid down my throat, and I lost both *Carabi* and *Panagæus*! I was quite astonished to hear of a terrestrial *Planaria*; for about a year or two ago I described in the 'Annals of Natural History' several beautifully coloured terrestrial species of the Southern Hemisphere, and thought it quite a new fact. By the way, you speak of a sheep with a broken leg not having flukes: I have heard my father aver that a fever, or any *serious accident*, as a broken limb, will cause in a man all the intestinal worms to be evacuated. Might not this possibly have been the case with the flukes in their early state?

I hope you were none the worse for Southampton;* I wish I had seen you looking rather fatter. I enjoyed my week extremely, and it did me good. I missed you the last few days, and we never managed to see much of each other; but

* The meeting of the British Association.

© The Complete Work of Charles Darwin Online

there were so many people there, that I for one hardly saw anything of any one. Once again I thank you very cordially for your kind present, and the pleasure it has given me, and believe me,

Ever most truly yours,
C. DARWIN.

P.S.—I have quite forgotten to say how greatly interested I was with your discussion on the statistics of animals: when will Natural History be so perfect that such points as you discuss will be perfectly known about any one animal?

C. Darwin to J. D. Hooker.

Malvern, June 13 [1849].

. . . At last I am going to press with a small poor first-fruit of my confounded Cirripedia, viz. the fossil pedunculate cirripedia. You ask what effect studying species has had on my variation theories; I do not think much—I have felt some difficulties more. On the other hand, I have been struck (and probably unfairly from the class) with the variability of every part in some slight degree of every species. When the same organ is *rigorously* compared in many individuals, I always find some slight variability, and consequently that the diagnosis of species from minute differences is always dangerous. I had thought the same parts of the same species more resemble (than they do anyhow in Cirripedia) objects cast in the same mould. Systematic work would be easy were it not for this confounded variation, which, however, is pleasant to me as a speculatist, though odious to me as a systematist. Your remarks on the distinctness (so unpleasant to me) of the Himalayan Rubi, willows, &c., compared with those of northern [Europe ?], &c., are very interesting; if my rude species-sketch had any *small* share in leading you to these

© The Complete Work of Charles Darwin Online

observations, it has already done good and ample service, and may lay its bones in the earth in peace. I never heard anything so strange as Falconer's neglect of your letters; I am extremely glad you are cordial with him again, though it must have cost you an effort. Falconer is a man one must love. . . . May you prosper in every way, my dear Hooker.

Your affectionate friend,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Wednesday, [September, n. d.]

. . . Many thanks for your letter received yesterday, which, as always, set me thinking: I laughed at your attack at my stinginess in changes of level towards Forbes,* being so liberal towards myself; but I must maintain, that I have never let down or upheaved our mother-earth's surface, for the sake of explaining any one phenomenon, and I trust I have very seldom done so without some distinct evidence. So I must still think it a bold step (perhaps a very true one) to sink into the depths of ocean, within the period of existing species, so large a tract of surface. But there is no amount or extent of change of level, which I am not fully prepared to admit, but I must say I should like better evidence, than the identity of a few plants, which *possibly* (I do not say probably) might have been otherwise transported. Particular

* Edward Forbes, born in the Isle of Man 1815, died 1854. His best known work was his Report on the distribution of marine animals at different depths in the Mediterranean. An important memoir of his is referred to in my father's 'Autobiography,' p. 88. He held successively the posts of Curator to the Geological Society's Museum, and Professor of Natural History in the Museum of Practical Geology; shortly before he died he was appointed Professor of Natural History in the University of Edinburgh. He seems to have impressed his contemporaries as a man of strikingly versatile and vigorous mind. The above allusion to changes of level refers to Forbes's tendency to explain the facts of geographical distribution by means of an active geological imagination.

© The Complete Work of Charles Darwin Online

thanks for your attempt to get me a copy of 'L'Espèce,' * and almost equal thanks for your criticisms on him: I rather misdoubted him, and felt not much inclined to take as gospel his facts. I find this one of my greatest difficulties with foreign authors, viz. judging of their credibility. How painfully (to me) true is your remark, that no one has hardly a right to examine the question of species who has not minutely described many. I was, however, pleased to hear from Owen (who is vehemently opposed to any mutability in species), that he thought it was a very fair subject, and that there was a mass of facts to be brought to bear on the question, not hitherto collected. My only comfort is (as I mean to attempt the subject), that I have dabbled in several branches of Natural History, and seen good specific men work out my species, and know something of geology (an indispensable union); and though I shall get more kicks than half-pennies, I will, life serving, attempt my work. Lamarck is the only exception, that I can think of, of an accurate describer of species, at least in the Invertebrate Kingdom, who has disbelieved in permanent species, but he in his absurd though clever work has done the subject harm, as has Mr. Vestiges, and, as (some future loose naturalist attempting the same speculations will perhaps say) has Mr. D. . . .

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, September 25th [1853].

MY DEAR HOOKER,—I have read your paper with great interest; it seems all very clear, and will form an admirable introduction to the New Zealand Flora, or to any Flora in the world. How few generalizers there are among systematists;

* Probably Godron's essay, published by the Academy of Nancy in 1848–49, and afterwards as a separate book in 1859.

© The Complete Work of Charles Darwin Online

I really suspect there is something absolutely opposed to each other and hostile in the two frames of mind required for systematising and reasoning on large collections of facts. Many of your arguments appear to me very well put, and, as far as my experience goes, the candid way in which you discuss the subject is unique. The whole will be very useful to me whenever I undertake my volume, though parts take the wind very completely out of my sails; it will be all nuts to me . . . for I have for some time determined to give the arguments on *both* sides (as far as I could), instead of arguing on the mutability side alone.

In my own Cirripedial work (by the way, thank you for the dose of soft solder; it does one—or at least me—a great deal of good)—in my own work I have not felt conscious that disbelieving in the mere *permanence* of species has made much difference one way or the other; in some few cases (if publishing avowedly on the doctrine of non-permanence), I should *not* have affixed names, and in some few cases should have affixed names to remarkable varieties. Certainly I have felt it humiliating, discussing and doubting, and examining over and over again, when in my own mind the only doubt has been whether the form varied *to-day or yesterday* (not to put too fine a point on it, as Snagsby * would say). After describing a set of forms as distinct species, tearing up my MS., and making them one species, tearing that up and making them separate, and then making them one again (which has happened to me), I have gnashed my teeth, cursed species, and asked what sin I had committed to be so punished. But I must confess that perhaps nearly the same thing would have happened to me on any scheme of work.

I am heartily glad to hear your Journal † is so much advanced; how magnificently it seems to be illustrated!

* In 'Bleak House.' † Sir J. D. Hooker's 'Himalayan Journal.'

© The Complete Work of Charles Darwin Online

An '*Oriental Naturalist*,' with lots of imagination and not too much regard to facts, is just the man to discuss species! I think your title of 'A Journal of a Naturalist in the East' very good; but whether "in the Himalaya" would not be better, I have doubted, for the East sounds rather vague. . . .

C. Darwin to J. D. Hooker.

[1853.]

My dear Hooker,—I have no remarks at all worth sending you, nor, indeed, was it likely that I should, considering how perfect and elaborated an essay it is.* As far as my judgment goes, it is the most important discussion on the points in question ever published. I can say no more. I agree with almost everything you say; but I require much time to digest an essay of such quality. It almost made me gloomy, partly from feeling I could not answer some points which theoretically I should have liked to have been different, and partly from seeing *so far better done* than I could have done, discussions on some points which I had intended to have taken up. . . .

I much enjoyed the slaps you have given to the provincial species-mongers. I wish I could have been of the slightest use: I have been deeply interested by the whole essay, and congratulate you on having produced a memoir which I believe will be memorable. I was deep in it when your most considerate note arrived, begging me not to hurry. I thank Mrs. Hooker and yourself most sincerely for your wish to see me. I will not let another summer pass without seeing you at Kew, for indeed I should enjoy it much. . . .

You do me really more honour than I have any claim to, putting me in after Lyell on ups and downs. In a year or two's time, when I shall be at my species book (if I do

* 'New Zealand Flora,' 1853.

© The Complete Work of Charles Darwin Online

not break down), I shall gnash my teeth and abuse you for having put so many hostile facts so confoundedly well.

Ever yours affectionately,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, March 26th [1854].

MY DEAR HOOKER,—I had hoped that you would have had a little breathing-time after your Journal, but this seems to be very far from the case; and I am the more obliged (and somewhat contrite) for the long letter received this morning, *most* juicy with news and *most* interesting to me in many ways. I am very glad indeed to hear of the reforms, &c., in the Royal Society. With respect to the Club,* I am deeply interested; only two or three days ago, I was regretting to my wife, how I was letting drop and being dropped by nearly all my acquaintances, and that I would endeavour to go oftener to London; I was not then thinking of the Club, which, as far as any one thing goes, would answer my exact object in keeping up old and making some new acquaintances. I will therefore come up to London for every (with rare exceptions) Club-day, and then my head, I think, will allow me on an average to go to every other meeting. But it is

* The Philosophical Club, to which my father was elected (as Professor Bonney is good enough to inform me) on April 24, 1854. He resigned his membership in 1864. The Club was founded in 1847. The number of members being limited to 47, it was proposed to christen it "the Club of 47," but the name was never adopted. The nature of the Club may be gathered from its first rule: "The purpose of the Club is to promote as much as possible the scientific objects of the Royal Society; to facilitate intercourse between those Fellows who are actively engaged in cultivating the various branches of Natural Science, and who have contributed to its progress; to increase the attendance at the evening meetings, and to encourage the contribution and discussion of papers." The Club met for dinner at 6, and the chair was to be quitted at 8.15, it being expected that members would go to the Royal Society. Of late years the dinner has been at 6.30, the Society meeting in the afternoon.

© The Complete Work of Charles Darwin Online

grievous how often any change knocks me up. I will further pledge myself, as I told Lyell, to resign after a year, if I did not attend pretty often, so that I should *at worst* encumber the Club temporarily. If you can get me elected, I certainly shall be very much pleased. Very many thanks for answers about Glaciers. I am very glad to hear of the second Edit.* so very soon; but am not surprised, for I have heard of several, in our small circle, reading it with very much pleasure. I shall be curious to hear what Humboldt will say: it will, I should think, delight him, and meet with more praise from him than any other book of Travels, for I cannot remember one, which has so many subjects in common with him. What a wonderful old fellow he is. . . . . By the way, I hope, when you go to Hitcham,† towards the end of May, you will be forced to have some rest. I am grieved to hear that all the bad symptoms have not left Henslow; it is so strange and new to feel any uneasiness about his health. I am particularly obliged to you for sending me Asa Gray's letter; how very pleasantly he writes. To see his and your caution on the species-question ought to overwhelm me in confusion and shame; it does make me feel deuced uncomfortable. . . . It is delightful to hear all that he says on Agassiz: how very singular it is that so *eminently* clever a man, with such *immense* knowledge on many branches of Natural History, should write as he does. Lyell told me that he was so delighted with one of his (Agassiz') lectures on progressive development, &c. &c., that he went to him afterwards and told him, "that it was so delightful, that he could not help all the time wishing it was true." I seldom see a Zoological paper from North America, without observing the impress of Agassiz' doctrines,—another proof, by the way, of how great a man he is. I was pleased and surprised to see A. Gray's remarks on crossing, obliterating varieties, on which, as you know, I have been collecting facts for these dozen years.

* Of the Himalayan Journal.

† Henslow's living.

© The Complete Work of Charles Darwin Online

How awfully flat I shall feel, if, when I get my notes together on species, &c. &c., the whole thing explodes like an empty puff-ball. Do not work yourself to death.

Ever yours most truly,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Nov. 5th [1854].

MY DEAR HOOKER,—I was delighted to get your note yesterday. I congratulate you very heartily,* and whether you care much or little, I rejoice to see the highest scientific judgment-court in Great Britain recognise your claims. I do hope Mrs. Hooker is pleased, and E. desires me particularly to send her cordial congratulations. . . . I pity you from the very bottom of my heart about your after-dinner speech, which I fear I shall not hear. Without you have a very much greater soul than I have (and I believe that you have), you will find the medal a pleasant little stimulus; when work goes badly, and one ruminates that all is vanity, it is pleasant to have some tangible proof, that others have thought something of one's labours.

Good-bye, my dear Hooker, I can assure [you] that we both most truly enjoyed your and Mrs. Hooker's visit here. Farewell.

My dear Hooker, your sincere friend,
C. DARWIN.

C. Darwin to J. D. Hooker.

March 7 [1855].

. . . I have just finished working well at Wollaston's † 'Insecta Maderensia': it is an *admirable* work. There is a

* On the award to him of the Royal Society's Medal.

† Thomas Vernon Wollaston, born March 9, 1821; died Jan. 4, 1878. His health forcing him in early manhood to winter in the south, he devoted himself to a study of the Coleoptera of

© The Complete Work of Charles Darwin Online

very curious point in the astounding proportion of Coleoptera that are apterous; and I think I have guessed the reason, viz. that powers of flight would be injurious to insects inhabiting a confined locality, and expose them to be blown to the sea: to test this, I find that the insects inhabiting the Dezerte Grande, a quite small islet, would be still more exposed to this danger, and here the proportion of apterous insects is even considerably greater than on Madeira proper. Wollaston speaks of Madeira and the other Archipelagoes as being "sure and certain witnesses of Forbes' old continent," and of course the Entomological world implicitly follows this view. But to my eyes it would be difficult to imagine facts more opposed to such a view. It is really disgusting and humiliating to see directly opposite conclusions drawn from the same facts.

I have had some correspondence with Wollaston on this and other subjects, and I find that he coolly assumes, (1) that formerly insects possessed greater migratory powers than now, (2) that the old land was *specially* rich in centres of creation, (3) that the uniting land was destroyed before the special creations had time to diffuse, and (4) that the land was broken down before certain families and genera had time to reach from Europe or Africa the points of land in question. Are not these a jolly lot of assumptions? and yet I shall see for the next dozen or score of years Wollaston

Madeira, the Cape de Verdes, and St. Helena, whence he deduced evidence in support of the belief in the submerged continent of 'Atlantis.' In an obituary notice by Mr. Rye ('Nature,' 1878) he is described as working persistently "upon a broad conception of the science to which he was devoted," while being at the same time "accurate, elaborate, and precise *ad punctum*, and naturally of a minutely critical habit." His first scientific paper was written when he was an undergraduate at Jesus College, Cambridge. While at the University, he was an Associate and afterwards a Member of the Ray Club: this is a small society which still meets once a week, and where the undergraduate members, or Associates, receive much kindly encouragement from their elders.

© The Complete Work of Charles Darwin Online

quoted as proving the former existence of poor Forbes' Atlantis.

I hope I have not wearied you, but I thought you would like to hear about this book, which strikes me as *excellent* in its facts, and the author a most nice and modest man.

Most truly yours,

C. DARWIN.

C. Darwin to W. D. Fox.

Down, March 19th [1855].

MY DEAR FOX,—How long it is since we have had any communication, and I really want to hear how the world goes with you; but my immediate object is to ask you to observe a point for me, and as I know now you are a very busy man with too much to do, I shall have a good chance of your doing what I want, as it would be hopeless to ask a quite idle man. As you have a Noah's Ark, I do not doubt that you have pigeons. (How I wish by any chance they were fantails!) Now what I want to know is, at what age nestling pigeons have their tail feathers sufficiently developed to be counted. I do not think I ever saw a young pigeon. I am hard at work at my notes collecting and comparing them, in order in some two or three years to write a book with all the facts and arguments, which I can collect, *for and versus* the immutability of species. I want to get the young of our domestic breeds, to see how young, and to what degree the differences appear. I must either breed myself (which is no amusement but a horrid bore to me) the pigeons or buy their young; and before I go to a seller, whom I have heard of from Yarrell, I am really anxious to know something about their development, not to expose my excessive ignorance, and therefore be excessively liable to be cheated and gulled. With respect to the *one* point of the tail feathers, it is of course in relation to the wonderful development of tail feathers in the adult fantail. If you had any breed of poultry pure, I

© The Complete Work of Charles Darwin Online

would beg a chicken with exact age stated, about a week or fortnight old! to be sent in a box by post, if you could have the heart to kill one; and secondly, would let me pay postage . . . Indeed, I should be very glad to have a nestling common pigeon sent, for I mean to make skeletons, and have already just begun comparing wild and tame ducks. And I think the results rather curious,* for on weighing the several bones very carefully, when perfectly cleaned the proportional weights of the two have greatly varied, the foot of the tame having largely increased. How I wish I could get a little wild duck of a week old, but that I know is almost impossible.

With respect to ourselves, I have not much to say; we have now a terribly noisy house with the whooping cough, but otherwise are all well. Far the greatest fact about myself is that I have at last quite done with the everlasting barnacles. At the end of the year we had two of our little boys very ill with fever and bronchitis, and all sorts of ailments. Partly for amusement, and partly for change of air, we went to London and took a house for a month, but it turned out a great failure, for that dreadful frost just set in when we went, and all our children got unwell, and E. and I had coughs and colds and rheumatism nearly all the time. We had put down first on our list of things to do, to go and see Mrs. Fox, but literally after waiting some time to see whether the weather would not improve, we had not a day when we both could go out.

I do hope before very long you will be able to manage to pay us a visit. Time is slipping away, and we are getting oldish. Do tell us about yourself and all your large family.

I know you will help me *if you can* with information

* "I have just been testing practically what disuse does in reducing parts; I have made skeleton of wild and tame duck (oh, the smell of well-boiled, high duck!!) and I find the tame-duck wing ought, according to scale of wild prototype, to have its two wings 360 grains in weight, but it has it only 317."—A letter to Sir J. D. Hooker, 1855.

© The Complete Work of Charles Darwin Online

about the young pigeons; and anyhow do write before very long.

My dear Fox, your sincere old friend,
C. DARWIN.

P.S.—Amongst all sorts of odds and ends, with which I am amusing myself, I am comparing the seeds of the variations of plants. I had formerly some wild cabbage seeds, which I gave to some one, was it to you? It is a *thousand* to one it was thrown away, if not I should be very glad of a pinch of it.

[The following extract from a letter to Mr. Fox (March 27th, 1855) refers to the same subject as the last letter, and gives some account of the "species work:" "The way I shall kill young things will be to put them under a tumbler glass with a teaspoon of ether or chloroform, the glass being pressed down on some yielding surface, and leave them for an hour or two, young have such power of revivification. (I have thus killed moths and butterflies.) The best way would be to send them as you procure them, in pasteboard chip-boxes by post, on which you could write and just tie up with string; and you will *really* make me happier by allowing me to keep an account of postage, &c. Upon my word I can hardly believe that *any one* could be so good-natured as to take such trouble and do such a very disagreeable thing as kill babies; and I am very sure I do not know one soul who, except yourself, would do so. I am going to ask one thing more; should old hens of any above poultry (not duck) die or become so old as to be *useless*, I wish you would send her to me per rail, addressed to 'C. Darwin, care of Mr. Acton, Post-office, Bromley, Kent.' Will you keep this address? as shortest way for parcels. But I do not care so much for this, as I could buy the old birds dead at Baily's to make skeletons. I should have written at once even if I had not heard from you, to beg you not to take trouble about pigeons, for Yarrell has persuaded me to attempt it, and I am now fitting up a

© The Complete Work of Charles Darwin Online

place, and have written to Baily about prices, &c. &c. *Sometime* (when you are better) I should like very much to hear a little about your "Little Call Duck"; why so called? And where you got it? and what it is like? . . . I was so ignorant I did not even know there were three varieties of Dorking fowl: how do they differ? . . .

I forget whether I ever told you what the object of my present work is,—it is to view all facts that I can master (eheu, eheu, how ignorant I find I am) in Natural History (as on geographical distribution, palæontology, classification, hybridism, domestic animals and plants, &c. &c. &c.) to see how far they favour or are opposed to the notion that wild species are mutable or immutable: I mean with my utmost power to give all arguments and facts on both sides. I have a *number* of people helping me in every way, and giving me most valuable assistance; but I often doubt whether the subject will not quite overpower me.

So much for the quasi-business part of my letter. I am very very sorry to hear so indifferent an account of your health: with your large family your life is very precious, and I am sure with all your activity and goodness it ought to be a happy one, or as happy as can reasonably be expected with all the cares of futurity on one.

One cannot expect the present to be like the old Crux-major days at the foot of those noble willow stumps, the memory of which I revere. I now find my little entomology, which I wholly owe to you, comes in very useful. I am very glad to hear that you have given yourself a rest from Sunday duties. How much illness you have had in your life! Farewell, my dear Fox. I assure you I thank you heartily for your proffered assistance."]

C. Darwin to W. D. Fox.

Down, May 7th [1855].

MY DEAR FOX,—My correspondence has cost you a deal of trouble, though this note will not. I found yours on my return

© The Complete Work of Charles Darwin Online

home on Saturday after a week's work in London. Whilst there I saw Yarrell, who told me he had carefully examined all points in the Call Duck, and did not feel any doubt about it being specifically identical, and that it had crossed freely with common varieties in St. James's Park. I should therefore be very glad for a seven-days' duckling and for one of the old birds, should one ever die a natural death. Yarrell told me that Sabine had collected forty varieties of the common duck! . . . Well, to return to business; nobody, I am sure, could fix better for me than you the characteristic age of little chickens; with respect to skeletons, I have feared it would be impossible to make them, but I suppose I shall be able to measure limbs, &c., by feeling the joints. What you say about old cocks just confirms what I thought, and I will make my skeletons of old cocks. Should an old wild turkey ever die, please remember me; I do not care for a baby turkey, nor for a mastiff. Very many thanks for your offer. I have puppies of bull-dogs and greyhound in salt, and I have had cart-horse and race-horse young colts carefully measured. Whether I shall do any good I doubt. I am getting out of my depth. Most truly yours,

C. DARWIN.

[An extract from a letter to Mr. Fox may find a place here, though of a later date, viz. July, 1855:

"Many thanks for the seven days old white Dorking, and for the other promised ones. I am getting quite 'a chamber of horrors;' I appreciate your kindness even more than before, for I have done the black deed and murdered an angelic little fantail, and a pouter at ten days old. I tried chloroform and ether for the first, and though evidently a perfectly easy death, it was prolonged; and for the second I tried putting lumps of cyanide of potassium in a very large damp bottle, half an hour before putting in the pigeon,

© The Complete Work of Charles Darwin Online

and the prussic acid gas thus generated was very quickly fatal."

A letter to Mr. Fox (May 23rd, 1855) gives the first mention of my father's laborious piece of work on the breeding of pigeons :

"I write now to say that I have been looking at some of our mongrel chickens, and I should say *one week old* would do very well. The chief points which I am, and have been for years, very curious about, is to ascertain whether the *young* of our domestic breeds differ as much from each other as do their parents, and I have no faith in anything short of actual measurement and the Rule of Three. I hope and believe I am not giving so much trouble without a motive of sufficient worth. I have got my fantails and pouters (choice birds, I hope, as I paid 20*s*. for each pair from Baily) in a grand cage and pigeon-house, and they are a decided amusement to me, and delight to H."

In the course of my father's pigeon-fancying enterprise he necessarily became acquainted with breeders, and was fond of relating his experiences as a member of the Columbarian and Philoperistera Clubs, where he met the purest enthusiasts of the "fancy," and learnt much of the mysteries of their art. In writing to Mr. Huxley some years afterwards, he quotes from a book on Pigeons by Mr. J. Eaton, in illustration of the "extreme attention and close observation" necessary to be a good fancier.

"In his [Mr. Eaton's] treatise, devoted to the Almond Tumbler *alone*, which is a sub-variety of the short-faced variety, which is a variety of the Tumbler, as that is of the Rock-pigeon, Mr. Eaton says : 'There are some of the young fanciers who are over-covetous, who go for all the five properties at once (*i.e.* the five characteristic points which are mainly attended to,—C. D.), they have their reward

© The Complete Work of Charles Darwin Online

by getting nothing.' In short, it is almost beyond the human intellect to attend to *all* the excellencies of the Almond Tumbler!

"To be a good breeder, and to succeed in improving any breed, beyond everything enthusiasm is required. Mr. Eaton has gained lots of prizes, listen to him.

"'If it was possible for noblemen and gentlemen to know the amazing amount of solace and pleasure derived from the Almond Tumbler, when they begin to understand their (*i.e.* the tumbler's) properties, I should think that scarce any nobleman or gentleman would be without their aviaries of Almond Tumblers.'"

My father was fond of quoting this passage, and always with a tone of fellow-feeling for the author, though, no doubt, he had forgotten his own wonderings as a child that "every gentleman did not become an ornithologist."—('Autobiography,' p. 35.)

To Mr. W. B. Tegetmeier, the well-known writer on poultry, &c., he was indebted for constant advice and co-operation. Their correspondence began in 1855, and lasted to 1881, when my father wrote: "I can assure you that I often look back with pleasure to the old days when I attended to pigeons, fowls, &c., and when you gave me such valuable assistance. I not rarely regret that I have had so little strength that I have not been able to keep up old acquaintances and friendships." My father's letters to Mr. Tegetmeier consist almost entirely of series of questions relating to the different breeds of fowls, pigeons, &c., and are not, therefore, interesting. In reading through the pile of letters, one is much struck by the diligence of the writer's search for facts, and it is made clear that Mr. Tegetmeier's knowledge and judgment were completely trusted and highly valued by him. Numerous phrases, such as "your note is a mine of wealth to me," occur, expressing his sense of the value of Mr. Tegetmeier's help, as well as words expressing his warm

© The Complete Work of Charles Darwin Online

appreciation of Mr. Tegetmeier's unstinting zeal and kindness, or his "pure and disinterested love of science." On the subject of hive-bees and their combs, Mr. Tegetmeier's help was also valued by my father, who wrote, "your paper on 'Bees-cells,' read before the British Association, was highly useful and suggestive to me."

To work out the problems on the Geographical Distributions of animals and plants on evolutionary principles, he had to study the means by which seeds, eggs, &c., can be transported across wide spaces of ocean. It was this need which gave an interest to the class of experiment to which the following letters allude.]

C. Darwin to W. D. Fox.

Down, May 17th [1855].

MY DEAR FOX,—You will hate the very sight of my handwriting; but after this time I promise I will ask for nothing more, at least for a long time. As you live on sandy soil, have you lizards at all common? If you have, should you think it too ridiculous to offer a reward for me for lizard's eggs to the boys in your school; a shilling for every half-dozen, or more if rare, till you got two or three dozen and send them to me? If snake's eggs were brought in mistake it would be very well, for I want such also; and we have neither lizards nor snakes about here. My object is to see whether such eggs will float on sea water, and whether they will keep alive thus floating for a month or two in my cellar. I am trying experiments on transportation of all organic beings that I can; and lizards are found on every island, and therefore I am very anxious to see whether their eggs stand sea water. Of course this note need not be answered, without, by a strange and favourable chance, you can some day answer it with the eggs. Your most troublesome friend,

C. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

April 13th [1855].

. . . I have had one experiment some little time in progress which will, I think, be interesting, namely, seeds in salt water, immersed in water of 32°–33°, which I have and shall long have, as I filled a great tank with snow. When I wrote last I was going to triumph over you, for my experiment had in a slight degree succeeded; but this, with infinite baseness, I did not tell, in hopes that you would say that you would eat all the plants which I could raise after immersion. It is very aggravating that I cannot in the least remember what you did formerly say that made me think you scoffed at the experiments vastly; for you now seem to view the experiment like a good Christian. I have in small bottles out of doors, exposed to variation of temperature, cress, radish, cabbages, lettuces, carrots, and celery, and onion seed—four great families. These, after immersion for exactly one week, have all germinated, which I did not in the least expect (and thought how you would sneer at me); for the water of nearly all, and of the cress especially, smelt very badly, and the cress seed emitted a wonderful quantity of mucus (the 'Vestiges' would have expected them to turn into tadpoles), so as to adhere in a mass; but these seeds germinated and grew splendidly. The germination of all (especially cress and lettuces) has been accelerated, except the cabbages, which have come up very irregularly, and a good many, I think, dead. One would have thought, from their native habitat, that the cabbage would have stood well. The Umbelliferæ and onions seem to stand the salt well. I wash the seed before planting them. I have written to the *Gardeners' Chronicle*,* though I doubt whether it was worth

* A few words asking for information. The results were published in the 'Gardeners' Chronicle,' May 26, Nov. 24, 1855. In the same year (p. 789) he sent a P.S. to his former paper, correcting a misprint and adding a few words on the seeds of the Leguminosæ. A fuller paper

© The Complete Work of Charles Darwin Online

while. If my success seems to make it worth while, I will send a seed list, to get you to mark some different classes of seeds. To-day I replant the same seeds as above after fourteen days' immersion. As many sea-currents go a mile an hour, even in a week they might be transported 168 miles; the Gulf Stream is said to go fifty and sixty miles a day. So much and too much on this head; but my geese are always swans. . . .

C. Darwin to J. D. Hooker.

[April 14th, 1855.]

. . . You are a good man to confess that you expected the cress would be killed in a week, for this gives me a nice little triumph. The children at first were tremendously eager, and asked me often, "whether I should beat Dr. Hooker!" The cress and lettuce have just vegetated well after twenty-one days' immersion. But I will write no more, which is a great virtue in me; for it is to me a very great pleasure telling you everything I do.

. . . If you knew some of the experiments (if they may be so called) which I am trying, you would have a good right to sneer, for they are so *absurd* even in *my* opinion that I dare not tell you.

Have not some men a nice notion of experimentising? I have had a letter telling me that seeds *must* have *great* power of resisting salt water, for otherwise how could they get to islands? This is the true way to solve a problem!

C. Darwin to J. D. Hooker.

Down, [1855.]

MY DEAR HOOKER,—You have been a very good man to exhale some of your satisfaction in writing two notes to me;

on the germination of seeds after treatment in salt water, appeared in the 'Linnean Soc. Journal,' 1857, p. 130.

© The Complete Work of Charles Darwin Online

you could not have taken a better line, in my opinion; but as for showing your satisfaction in confounding my experiments, I assure you I am quite enough confounded—those horrid seeds, which, as you truly observe, if they sink they won't float.

I have written to Scoresby and have had a rather dry answer, but very much to the purpose, and giving me no hopes of any law unknown to me which might arrest their everlasting descent into the deepest depths of the ocean. By the way it was very odd, but I talked to Col. Sabine for half an hour on the subject, and could not make him see with respect to transportal the difficulty of the sinking question! The bore is, if the confounded seeds will sink, I have been taking all this trouble in salting the ungrateful rascals for nothing.

Everything has been going wrong with me lately; the fish at the Zoolog. Soc. ate up lots of soaked seeds, and in imagination they had in my mind been swallowed, fish and all, by a heron, had been carried a hundred miles, been voided on the banks of some other lake and germinated splendidly, when lo and behold, the fish ejected vehemently, and with disgust equal to my own, *all* the seeds from their mouths.*

But I am not going to give up the floating yet: in first place I must try fresh seeds, though of course it seems far more probable that they will sink; and secondly, as a last resource, I must believe in the pod or even whole plant or branch being washed into the sea; with floods and slips and

* In describing these troubles to Mr. Fox, my father wrote:—"All nature is perverse and will not do as I wish it; and just at present I wish I had my old barnacles to work at, and nothing new." The experiment ultimately succeeded, and he wrote to Sir J. Hooker:—"I find fish will greedily eat seeds of aquatic grasses, and that millet-seed put into fish and given to a stork, and then voided, will germinate. So this is the nursery rhyme of 'this is the stick that beats the pig,' &c. &c."

© The Complete Work of Charles Darwin Online

earthquakes; this must continually be happening, and if kept wet, I fancy the pods, &c. &c., would not open and shed their seeds. Do try your Mimosa seed at Kew.

I had intended to have asked you whether the *Mimosa scandens* and *Guilandina bonduc* grows at Kew, to try fresh seeds. R. Brown tells me he believes four W. Indian seeds have been washed on shores of Europe. I was assured at Keeling Island that seeds were not rarely washed on shore: so float they must and shall! What a long yarn I have been spinning.

If you have several of the Loffoden seeds, do soak some in tepid water, and get planted with the utmost care: this is an experiment after my own heart, with chances 1000 to 1 against its success.

C. Darwin to J. D. Hooker.

Down, May 11th [1855].

MY DEAR HOOKER,—I have just received your note. I am most sincerely and heartily glad at the news * it contains, and so is my wife. Though the income is but a poor one, yet the certainty, I hope, is satisfactory to yourself and Mrs. Hooker. As it must lead in future years to the Directorship, I do hope you look at it as a piece of good fortune. For my own taste I cannot fancy a pleasanter position, than the Head of such a noble and splendid place; far better, I should think, than a Professorship in a great town. The more I think of it, the gladder I am. But I will say no more; except that I hope Mrs. Hooker is pretty well pleased. . . .

As the *Gardeners' Chronicle* put in my question, and took notice of it, I think I am bound to send, which I had thought of doing next week, my first report to Lindley to give him the option of inserting it; but I think it likely that he may not think it fit for a Gardening periodical. When

* The appointment of Sir J. D. Hooker as Assistant Director of the Royal Gardens at Kew.

© The Complete Work of Charles Darwin Online

my experiments are ended (should the results appear worthy) and should the 'Linnean Journal' not object to the previous publication of imperfect and provisional reports, I should be *delighted* to insert the final report there; for it has cost me so much trouble, that I should think that probably the result was worthy of more permanent record than a newspaper; but I think I am bound to send it first to Lindley.

I begin to think the floating question more serious than the germinating one; and am making all the enquiries which I can on the subject, and hope to get some little light on it . . .

I hope you managed a good meeting at the Club. The Treasurership must be a plague to you, and I hope you will not be Treasurer for long: I know I would much sooner give up the Club than be its Treasurer.

Farewell, Mr. Assistant Director and dear friend,

C. DARWIN.

C. Darwin to J. D. Hooker.

June 5th, 1855.

. . . . Miss Thorley * and I are doing *a little Botanical work!* for our amusement, and it does amuse me very much, viz. making a collection of all the plants, which grow in a field, which has been allowed to run waste for fifteen years, but which before was cultivated from time immemorial; and we are also collecting all the plants in an adjoining and *similar* but cultivated field; just for the fun of seeing what plants have arrived or died out. Hereafter we shall want a bit of help in naming puzzlers. How dreadfully difficult it is to name plants.

What a *remarkably* nice and kind letter Dr. A. Gray has sent me in answer to my troublesome queries; I retained your copy of his 'Manual' till I heard from him, and when I have answered his letter, I will return it to you.

I thank you much for Hedysarum: I do hope it is not very

* A lady who was for many years a governess in the family.

© The Complete Work of Charles Darwin Online

precious, for as I told you it is for probably a *most* foolish purpose. I read somewhere that no plant closes its leaves so promptly in darkness, and I want to cover it up daily for half an hour, and see if I can teach it to close by itself, or more easily than at first in darkness. . . . . I cannot make out why you would prefer a continental transmission, as I think you do, to carriage by sea. I should have thought you would have been pleased at as many means of transmission as possible. For my own pet theoretic notions, it is quite indifferent whether they are transmitted by sea or land, as long as some tolerably probable way is shown. But it shocks my philosophy to create land, without some other and independent evidence. Whenever we meet, by a very few words I should, I think, more clearly understand your views. . . .

I have just made out my first grass, hurrah! hurrah! I must confess that fortune favours the bold, for, as good luck would have it, it was the easy *Anthoxanthum odoratum:* nevertheless it is a great discovery; I never expected to make out a grass in all my life, so hurrah! It has done my stomach surprising good. . . .

C. Darwin to J. D. Hooker.

Down, [June?] 15th, [1855].

MY DEAR HOOKER,—I just write one line to say that the Hedysarum is come *quite safely*, and thank you for it.

You cannot imagine what amusement you have given me by naming those three grasses: I have just got paper to dry and collect all grasses. If ever you catch quite a beginner, and want to give him a taste for Botany, tell him to make a perfect list of some little field or wood. Both Miss Thorley and I agree that it gives a really uncommon interest to the work, having a nice little definite world to work on, instead of the awful abyss and immensity of all British Plants.

Adios. I was really consummately impudent to express

© The Complete Work of Charles Darwin Online

my opinion "on the retrograde step," * and I deserved a good snub, and upon reflection I am very glad you did not answer me in the *Gardeners' Chronicle.*

I have been *very much* interested with the Florula. †

[Writing on June 5th to Sir J. D. Hooker, my father mentions a letter from Dr. Asa Gray. The letter referred to was an answer to the following :]

C. Darwin to Asa Gray. ‡

Down, April 25th [1855].

MY DEAR SIR,—I hope that you will remember that I had the pleasure of being introduced to you at Kew. I want to beg a great favour of you, for which I well know I can offer no apology. But the favour will not, I think, cause you much trouble, and will greatly oblige me. As I am no botanist, it will seem so absurd to you my asking botanical questions; that I may premise that I have for several years been collecting facts on "variation," and when I find that any general remark seems to hold good amongst animals, I try to test it in Plants. [Here follows a request for information on American Alpine plants, and a suggestion as to publishing on the subject.] I can assure you that I perceive how presumptuous it is in me, not a botanist, to make even the most

* "To imagine such enormous geological changes within the period of the existence of now living beings, on no other ground but to account for their distribution, seems to me, in our present state of ignorance on the means of transportal, an almost retrograde step in science."—Extract from the paper on 'Salt Water and Seeds' in the *Gardeners' Chronicle,* May 26, 1855.

† Godron's 'Florula Juvenalis,' which gives an interesting account of plants introduced in imported wool.

‡ The well-known American Botanist. My father's friendship with Dr. Gray began with the correspondence of which the present is the first letter. An extract from a letter to Sir J. Hooker, 1857, shows that my father's strong personal regard for Dr. Gray had an early origin: "I have been glad to see A. Gray's letters; there is always something in them that shows that he is a very lovable man."

© The Complete Work of Charles Darwin Online

trifling suggestion to such a botanist as yourself; but from what I saw and have heard of you from our dear and kind friend Hooker, I hope and think that you will forgive me, and believe me, with much respect,

Dear sir, yours very faithfully,

CHARLES DARWIN.

C. Darwin to Asa Gray.

Down, June 8th [1855].

MY DEAR SIR,—I thank you cordially for your remarkably kind letter of the 22nd ult., and for the extremely pleasant and obliging manner in which you have taken my rather troublesome questions. I can hardly tell you how much your list of Alpine plants has interested me, and I can now in some degree picture to myself the plants of your Alpine summits. The new edit. of your Manual is *capital* news for me. I know from your preface how pressed you are for room, but it would take no space to append (Eu) in brackets to any European plant, and, as far as I am concerned, this would answer every purpose.* From my own experience, whilst making out English plants in our manuals, it has often struck me how much interest it would give if some notion of their range had been given; and so, I cannot doubt, your American inquirers and beginners would much like to know which of their plants were indigenous and which European. Would it not be well in the Alpine plants to append the very same addition which you have now sent me in MS.? though here, owing to your kindness, I do not speak selfishly, but merely *pro bono Americano publico.* I presume it would be too troublesome to give in your manual the habitats of those plants found west of the Rocky Mountains, and likewise those found in Eastern Asia, taking the Yeneseï (?),—which, if I remember right, according to Gmelin, is the main partition

* This suggestion Dr. Gray adopted in subsequent editions.

© The Complete Work of Charles Darwin Online

line of Siberia. Perhaps Siberia more concerns the northern Flora of North America. The ranges of the plants to the east and west, viz. whether most found are in Greenland and Western Europe, or in E. Asia, appears to me a very interesting point as tending to show whether the migration has been eastward or westward. Pray believe me that I am most entirely conscious that the *only use* of these remarks is to show a botanist what points a non-botanist is curious to learn; for I think every one who studies profoundly a subject often becomes unaware [on] what points the ignorant require information. I am so very glad that you think of drawing up some notice on your geographical distribution, for the area of the Manual strikes me as in some points better adapted for comparison with Europe than that of the whole of North America. You ask me to state definitely some of the points on which I much wish for information; but I really hardly can, for they are so vague; and I rather wish to see what results will come out from comparisons, than have as yet defined objects. I presume that, like other botanists, you would give, for your area, the proportion (leaving out introduced plants) to the whole of the great leading families: this is one point I had intended (and, indeed, have done roughly) to tabulate from your book, but of course I could have done it only *very imperfectly*. I should also, of course, have ascertained the proportion, to the whole Flora, of the European plants (leaving out introduced) *and of the separate great families*, in order to speculate on means of transportal. By the way, I ventured to send a few days ago a copy of the *Gardeners' Chronicle* with a short report by me of some trifling experiments which I have been trying on the power of seeds to withstand sea water. I do not know whether it has struck you, but it has me, that it would be advisable for botanists to give in *whole numbers*, as well as in the lowest fraction, the proportional numbers of the families, thus I make out from your Manual that of the *indigenous* plants

© The Complete Work of Charles Darwin Online

the proportion of the Umbelliferæ are $\frac{36}{1798}=\frac{1}{49}$; for, without one knows the *whole* numbers, one cannot judge how really close the numbers of the plants of the same family are in two distant countries; but very likely you may think this superfluous. Mentioning these proportional numbers, I may give you an instance of the sort of points, and how vague and futile they often are, which I *attempt* to work out . . . ; reflecting on R. Brown's and Hooker's remark, that near identity of proportional numbers of the great families in two countries, shows probably that they were once continuously united, I thought I would calculate the proportions of, for instance, the *introduced* Compositæ in Great Britain to all the introduced plants, and the result was $\frac{10}{92}=\frac{1}{9 \cdot 2}$. In our *aboriginal* or indigenous flora the proportion is $\frac{1}{10}$; and in many other cases I found an equally striking correspondence. I then took your Manual, and worked out the same question; here I find in the Compositæ an almost equally striking correspondence, viz. $\frac{24}{206}=\frac{1}{8}$ in the introduced plants, and $\frac{223}{1798}=\frac{1}{8}$ in the indigenous; but when I came to the other families I found the proportion entirely different, showing that the coincidences in the British Flora were probably accidental!

You will, I presume, give the proportion of the species to the genera, *i.e.* show on an average how many species each genus contains; though I have done this for myself.

If it would not be too troublesome, do you not think it would be very interesting, and give a very good idea of your Flora, to divide the species into three groups, viz. (*a*) species common to the old world, stating numbers common to Europe and Asia; (*b*) indigenous species, but belonging to genera found in the Old World; and (*c*) species belonging to genera confined to America or the New World? To make (according to my ideas) perfection perfect, one ought to be told whether there are other cases, like Erica, of genera common in Europe or in Old World not found in your area. But honestly I feel

© The Complete Work of Charles Darwin Online

that it is quite ridiculous my writing to you at such length on the subject; but, as you have asked me, I do it gratefully, and write to you as I should to Hooker, who often laughs at me unmercifully, and I am sure you have better reason to do so.

There is one point on which I am *most* anxious for information, and I mention it with the greatest hesitation, and only in the *full belief* that you will believe me that I have not the folly and presumption to hope for a second that you will give it, without you can with very little trouble. The point can at present interest no one but myself, which makes the case wholly different from geographical distribution. The only way in which, I think, you possibly could do it with little trouble would be to bear in mind, whilst correcting your proof-sheets of the Manual, my question and put a cross or mark to the species, and whenever sending a parcel to Hooker to let me have such old sheets. But this would give you the trouble of remembering my question, and I can hardly hope or expect that you will do it. But I will just mention what I want; it is to have marked the "close species" in a Flora, so as to compare in *different* Floras whether the same genera have "close species," and for other purposes too vague to enumerate. I have attempted, by Hooker's help, to ascertain in a similar way whether the different species of the same genera in distant quarters of the globe are variable or present varieties. The definition I should give of a "*close species*" was one that *you* thought specifically distinct, but which you could conceive some other *good* botanist might think only a race or variety; or, again, a species that you had trouble, though having opportunities of knowing it well, in discriminating from some other species. Supposing that you were inclined to be so very kind as to do this, and could (which I do not expect) spare the time, as I have said, a mere cross to each such species in any useless proof-sheets would give me the information desired, which, I may add, I know must be vague.

© The Complete Work of Charles Darwin Online

How can I apologise enough for all my presumption and the extreme length of this letter? The great good nature of your letter to me has been partly the cause, so that, as is too often the case in this world, you are punished for your good deeds. With hearty thanks, believe me,

Yours very truly and gratefully,

CH. DARWIN.

C. Darwin to J. D. Hooker.

Down, 18th [July, 1855].

. . . I think I am getting a *mild* case about Charlock seed;* but just as about salting, ill luck to it, I cannot remember how many years you would allow that Charlock seed might live in the ground. Next time you write, show a bold face, and say in how many years, you think, Charlock seed would probably all be dead. A man told me the other day of, as I thought, a splendid instance,—and *splendid* it was, for according to his evidence the seed came up alive out of the *lower part* of the *London Clay!!!* I disgusted him by telling him that Palms ought to have come up.

You ask how far I go in attributing organisms to a common descent: I answer I know not; the way in which I intend treating the subject, is to show (*as far as I can*) the facts and arguments for and against the common descent of the species of the same genus; and then show how far the same arguments tell for or against forms, more and more widely different: and when we come to forms of different orders and

* In the *Gardeners' Chronicle*, 1855, p. 758, appeared a notice (half a column in length) by my father on the "Vitality of Seeds." The facts related refer to the "Sandwalk"; the wood was planted in 1846 on a piece of pasture land laid down as grass in 1840. In 1855, on the soil being dug in several places, Charlock (*Brassica sinapistrum*) sprang up freely. The subject continued to interest him, and I find a note dated July 2nd, 1874, in which my father recorded that forty-six plants of Charlock sprang up in that year over a space (14 × 7 feet) which had been dug to a considerable depth.

© The Complete Work of Charles Darwin Online

classes, there remain only some such arguments as those which can perhaps be deduced from similar rudimentary structures, and very soon not an argument is left.

[The following extract from a letter to Mr. Fox [Oct. 1855* gives a brief mention of the last meeting of the British Association which he attended:] "I really have no news: the only thing we have done for a long time, was to go to Glasgow; but the fatigue was to me more than it was worth, and E. caught a bad cold. On our return we stayed a single day at Shrewsbury, and enjoyed seeing the old place. I saw a little of Sir Philip † (whom I liked much), and he asked me 'why on earth I instigated you to rob his poultry-yard?' The meeting was a good one, and the Duke of Argyll spoke excellently."]

* In this year he published ('Phil. Mag.' x.) a paper "On the power of icebergs to make rectilinear uniformly-directed grooves across a submarine undulatory surface."

† Sir P. Egerton was a neighbour of Mr. Fox.

© The Complete Work of Charles Darwin Online

CHAPTER III.

THE UNFINISHED BOOK.

MAY 1856 TO JUNE 1858.

[IN the Autobiographical chapter (Vol. I. p. 84) my father wrote:—"Early in 1856 Lyell advised me to write out my views pretty fully, and I began at once to do so on a scale three or four times as extensive as that which was afterwards followed in my 'Origin of Species;' yet it was only an abstract of the materials which I had collected." The letters in the present chapter are chiefly concerned with the preparation of this unfinished book.

The work was begun on May 14th, and steadily continued up to June 1858, when it was interrupted by the arrival of Mr. Wallace's MS. During the two years which we are now considering, he wrote ten chapters (that is about one-half) of the projected book. He remained for the most part at home, but paid several visits to Dr. Lane's Water-Cure Establishment at Moor Park, during one of which he made a pilgrimage to the shrine of Gilbert White at Selborne.]

LETTERS.

C. Darwin to C. Lyell.

May 3 [1856].

. . . With respect to your suggestion of a sketch of my views, I hardly know what to think, but will reflect on it, but

© The Complete Work of Charles Darwin Online

it goes against my prejudices. To give a fair sketch would be absolutely impossible, for every proposition requires such an array of facts. If I were to do anything, it could only refer to the main agency of change—selection—and perhaps point out a very few of the leading features, which countenance such a view, and some few of the main difficulties. But I do not know what to think; I rather hate the idea of writing for priority, yet I certainly should be vexed if any one were to publish my doctrines before me. Anyhow, I thank you heartily for your sympathy. I shall be in London next week, and I will call on you on Thursday morning for one hour precisely, so as not to lose much of your time and my own; but will you let me this time come as early as 9 o'clock, for I have much which I must do in the morning in my strongest time? Farewell, my dear old patron.

Yours,

C. DARWIN.

By the way, *three* plants have come up out of the earth, perfectly enclosed in the roots of the trees. And twenty-nine plants in the table-spoonful of mud, out of the little pond; Hooker was surprised at this, and struck with it, when I showed him how much mud I had scraped off one duck's feet.

If I did publish a short sketch, where on earth should I publish it?

If I do *not* hear, I shall understand that I may come from 9 to 10 on Thursday.

C. Darwin to J. D. Hooker.

May 9th [1856].

. . . I very much want advice and *truthful* consolation if you can give it. I had a good talk with Lyell about my species work, and he urges me strongly to publish something. I am fixed against any periodical or Journal, as I positively will *not* expose myself to an Editor or a Council, allowing a publication for which they might be abused. If I publish

© The Complete Work of Charles Darwin Online

anything it must be a *very thin* and little volume, giving a sketch of my views and difficulties; but it is really dreadfully unphilosophical to give a *résumé*, without exact references, of an unpublished work. But Lyell seemed to think I might do this, at the suggestion of friends, and on the ground, which I might state, that I had been at work for eighteen * years, and yet could not publish for several years, and especially as I could point out difficulties which seemed to me to require especial investigation. Now what think you? I should be really grateful for advice. I thought of giving up a couple of months and writing such a sketch, and trying to keep my judgment open whether or no to publish it when completed. It will be simply impossible for me to give exact references; anything important I should state on the authority of the author generally; and instead of giving all the facts on which I ground my opinion, I could give by memory only one or two. In the Preface I would state that the work could not be considered strictly scientific, but a mere sketch or outline of a future work in which full references, &c., should be given. Eheu, eheu, I believe I should sneer at any one else doing this, and my only comfort is, that I *truly* never dreamed of it, till Lyell suggested it, and seems deliberately to think it advisable.

I am in a peck of troubles, and do pray forgive me for troubling you.

Yours affectionately,

C. Darwin.

C. Darwin to J. D. Hooker.

May 11th [1856].

. . . Now for a *more important!* subject, viz. my own self: I am extremely glad you think well of a separate "Pre-

* The interval of eighteen years, from 1837 when he began to collect facts, would bring the date of this letter to 1855, not 1856, nevertheless the latter seems the more probable date.

© The Complete Work of Charles Darwin Online

liminary Essay" (*i.e.* if anything whatever is published; for Lyell seemed rather to doubt on this head)*; but I cannot bear the idea of *begging* some Editor and Council to publish, and then perhaps to have to *apologise* humbly for having led them into a scrape. In this one respect I am in the state which, according to a very wise saying of my father's, is the only fit state for asking advice, viz. with my mind firmly made up, and then, as my father used to say, *good* advice was very comfortable, and it was easy to reject *bad* advice. But Heaven knows I am not in this state with respect to publishing at all any preliminary essay. It yet strikes me as quite unphilosophical to publish results without the full details which have led to such results.

It is a melancholy, and I hope not quite true view of yours that facts will prove anything, and are therefore superfluous! But I have rather exaggerated, I see, your doctrine. I do not fear being tied down to error, *i.e.* I feel pretty sure I should give up anything false published in the preliminary essay, in my larger work; but I may thus, it is very true, do mischief by spreading error, which as I have often heard you say is much easier spread than corrected. I confess I lean more and more to at least making the attempt and drawing up a sketch and trying to keep my judgment, whether to publish, open. But I always return to my fixed idea that it is dreadfully unphilosophical to publish without full details. I certainly think my future work in full would profit by hearing what my friends or critics (if reviewed) thought of the outline.

To any one but you I should apologise for such long discussion on so personal an affair; but I believe, and indeed you have proved it by the trouble you have taken, that this would be superfluous.

Yours truly obliged,

CH. DARWIN.

* The meaning of the sentence in parentheses is obscure.

© The Complete Work of Charles Darwin Online

P.S.—What you say (for I have just re-read your letter) that the Essay might supersede and take away all novelty and value from any future larger Book, is very true; and that would grieve me beyond everything. On the other hand (again from Lyell's urgent advice), I published a preliminary sketch of the Coral Theory, and this did neither good nor harm. I begin *most heartily* to wish that Lyell had never put this idea of an Essay into my head.

From a Letter to Sir C. Lyell [*July*, 1856].

"I am delighted that I may say (with absolute truth) that my essay is published at your suggestion, but I hope it will not need so much apology as I at first thought; for I have resolved to make it nearly as complete as my present materials allow. I cannot put in all which you suggest, for it would appear too conceited."

From a Letter to W. D. Fox.

Down, June 14th [1856].

"... What you say about my Essay, I dare say is very true; and it gave me another fit of the wibber-gibbers: I hope that I shall succeed in making it modest. One great motive is to get information on the many points on which I want it. But I tremble about it, which I should not do, if I allowed some three or four more years to elapse before publishing anything. . . ."

[The following extracts from letters to Mr. Fox are worth giving, as showing how great was the accumulation of material which now had to be dealt with.

June 14th [1856].

"Very many thanks for the capital information on cats; I see I had blundered greatly, but I know I have somewhere your original notes; but my notes are so numerous during

© The Complete Work of Charles Darwin Online

nineteen years' collection, that it would take me at least a year to go over and classify them."

Nov. 1856. "Sometimes I fear I shall break down, for my subject gets bigger and bigger with each month's work."]

C. Darwin to C. Lyell.

Down, 16th [June, 1856].

MY DEAR LYELL,—I am going to do the most impudent thing in the world. But my blood gets hot with passion and turns cold alternately at the geological strides, which many of your disciples are taking.

Here, poor Forbes made a continent to [*i.e.* extending to] North America and another (or the same) to the Gulf weed; Hooker makes one from New Zealand to South America and round the World to Kerguelen Land. Here is Wollaston speaking of Madeira and P. Santo "as the sure and certain witnesses of a former continent." Here is Woodward writes to me, if you grant a continent over 200 or 300 miles of ocean depths (as if that was nothing), why not extend a continent to every island in the Pacific and Atlantic Oceans? And all this within the existence of recent species! If you do not stop this, if there be a lower region for the punishment of geologists, I believe, my great master, you will go there. Why, your disciples in a slow and creeping manner beat all the old Catastrophists who ever lived. You will live to be the great chief of the Catastrophists.

There, I have done myself a great deal of good, and have exploded my passion.

So my master, forgive me, and believe me, ever yours,

C. DARWIN.

P.S.—Don't answer this, I did it to ease myself.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down [June] 17th, 1856.

. . . I have been very deeply interested by Wollaston's book,* though I differ *greatly* from many of his doctrines. Did you ever read anything so rich, considering how very far he goes, as his denunciations against those who go further: "most mischievous," "absurd," "unsound." Theology is at the bottom of some of this. I told him he was like Calvin burning a heretic. It is a very valuable and clever book in my opinion. He has evidently read very little out of his own line. I urged him to read the New Zealand essay. His Geology also is rather *eocene*, as I told him. In fact I wrote most frankly; I fear too frankly; he says he is sure that ultra-honesty is my characteristic: I do not know whether he meant it as a sneer; I hope not. Talking of eocene geology, I got so wroth about the Atlantic continent, more especially from a note from Woodward (who has published a capital book on shells), who does not seem to doubt that every island in the Pacific and Atlantic are the remains of continents, submerged within period of existing species, that I fairly exploded, and wrote to Lyell to protest, and summed up all the continents created of late years by Forbes (the head sinner!) *yourself*, Wollaston, and Woodward, and a pretty nice little extension of land they make altogether! I am fairly rabid on the question and therefore, if not wrong already, am pretty sure to become so . . .

I have enjoyed your note much. Adios,

C. Darwin.

P.S. [June] 18th.—Lyell has written me a *capital* letter on your side, which ought to upset me entirely, but I cannot say it does quite.

Though I must try and cease being rabid and try to feel

* 'The Variation of Species,' 1856.

© The Complete Work of Charles Darwin Online

humble, and allow you all to make continents, as easily as a cook does pancakes.

C. Darwin to C. Lyell.

Down, June 25th [1856].

My dear Lyell,—I will have the following tremendous letter copied to make the reading easier, and as I want to keep a copy.

As you say you would like to hear my reasons for being most unwilling to believe in the continental extensions of late authors, I gladly write them, as, without I am convinced of my error, I shall have to give them condensed in my essay, when I discuss single and multiple creation; I shall therefore be particularly glad to have your general opinion on them. I may *quite likely* have persuaded myself in my wrath that there is more in them than there is. If there was much more reason to admit a continental extension in any one or two instances (as in Madeira) than in other cases, I should feel no difficulty whatever. But if on account of European plants, and littoral sea shells, it is thought necessary to join Madeira to the mainland, Hooker is quite right to join New Holland to New Zealand, and Auckland Island (and Raoul Island to N.E.), and these to S. America and the Falklands, and these to Tristan d'Acunha, and these to Kerguelen Land; thus making, either strictly at the same time, or at different periods, but all within the life of recent beings, an almost circumpolar belt of land. So again Galapagos and Juan Fernandez must be joined to America; and if we trust to littoral sea shells, the Galapagos must have been joined to the Pacific Islands (2400 miles distant) as well as to America, and as Woodward seems to think all the islands in the Pacific into a magnificent continent; also the islands in the Southern Indian Ocean into another continent, with Madagascar and Africa, and perhaps India. In the North Atlantic, Europe will stretch half-way

© The Complete Work of Charles Darwin Online

across the ocean to the Azores, and further north right across. In short, we must suppose probably, half the present ocean was land within the period of living organisms. The Globe within this period must have had a quite different aspect. Now the only way to test this, that I can see, is to consider whether the continents have undergone within this same period such wonderful permutations. In all North and South and Central America, we have both recent and miocene (or eocene) shells, quite distinct on the opposite sides, and hence I cannot doubt that *fundamentally* America has held its place since at least, the miocene period. In Africa almost all the living shells are distinct on the opposite sides of the intertropical regions, short as the distance is compared to the range of marine mollusca, in uninterrupted seas; hence I infer that Africa has existed since our present species were created. Even the isthmus of Suez and the Aralo-Caspian basin have had a great antiquity. So I imagine, from the tertiary deposits, has India. In Australia the great fauna of extinct marsupials shows that before the present mammals appeared, Australia was a separate continent. I do not for one second doubt that very large portions of all these continents have undergone *great* changes of level within this period, but yet I conclude that fundamentally they stood as barriers in the sea, where they now stand; and therefore I should require the weightiest evidence to make me believe in such immense changes within the period of living organisms in our oceans, where, moreover, from the great depths, the changes must have been vaster in a vertical sense.

Secondly. Submerge our present continents, leaving a few mountain peaks as islands, and what will the character of the islands be?—Consider that the Pyrenees, Sierra Nevada, Apennines, Alps, Carpathians, are non-volcanic, Etna and Caucasus, volcanic. In Asia, Altai and Himalaya, I believe non-volcanic. In North Africa the non-volcanic, as I imagine, Alps of Abyssinia and of the Atlas. In South Africa, the

© The Complete Work of Charles Darwin Online

Snow Mountains. In Australia, the non-volcanic Alps. In North America, the White Mountains, Alleghanies and Rocky Mountains—some of the latter alone, I believe, volcanic. In South America to the east, the non-volcanic [Silla] of Caracas, and Itacolumi of Brazil, further south the Sierra Ventanas, and in the Cordilleras, many volcanic but not all. Now compare these peaks with the oceanic islands; as far as known all are volcanic, except St. Paul's (a strange bedevilled rock), and the Seychelles, if this latter can be called oceanic, in the line of Madagascar; the Falklands, only 500 miles off, are only a shallow bank; New Caledonia, hardly oceanic, is another exception. This argument has to me great weight. Compare on a Geographical Map, islands which, we have *several* reasons to suppose, were connected with mainland, as Sardinia, and how different it appears. Believing, as I am inclined, that continents as continents, and oceans as oceans, are of immense antiquity—I should say that if any of the existing oceanic islands have any relation of any kind to continents, they are forming continents; and that by the time they could form a continent, the volcanoes would be denuded to their cores, leaving peaks of syenite, diorite, or porphyry. But have we nowhere any last wreck of a continent, in the midst of the ocean? St. Paul's Rock, and such old battered volcanic islands, as St. Helena, may be; but I think we can see some reason why we should have less evidence of sinking than of rising continents (if my view in my Coral volume has any truth in it, viz.: that volcanic outbursts accompany rising areas), for during subsidence there will be no compensating agent at work, in rising areas there will be the *additional* element of outpoured volcanic matter.

Thirdly. Considering the depth of the ocean, I was, before I got your letter, inclined vehemently to dispute the vast amount of subsidence, but I must strike my colours. With respect to coral reefs, I carefully guarded against its being

© The Complete Work of Charles Darwin Online

supposed that a continent was indicated by the groups of atolls. It is difficult to guess, as it seems to me, the amount of subsidence indicated by coral reefs; but in such large areas as the Lowe Archipelago, the Marshall Archipelago, and Laccadive group, it would, judging from the heights of existing oceanic archipelagoes, be odd, if some peaks of from 8000 to 10,000 feet had not been buried. Even after your letter a suspicion crossed me whether it would be fair to argue from subsidences in the middle of the greatest oceans to continents; but refreshing my memory by talking with Ramsay in regard to the probable thickness in one vertical line of the Silurian and carboniferous formation, it seems there must have been *at least* 10,000 feet of subsidence during these formations in Europe and North America, and therefore during the continuance of nearly the same set of organic beings. But even 12,000 feet would not be enough for the Azores, or for Hooker's continent; I believe Hooker does not infer a continuous continent, but approximate groups of islands, with, if we may judge from existing continents, not *profoundly* deep sea between them; but the argument from the volcanic nature of nearly every existing oceanic island tells against such supposed groups of islands,—for I presume he does not suppose a mere chain of volcanic islands belting the southern hemisphere.

Fourthly. The supposed continental extensions do not seem to me, perfectly to account for all the phenomena of distribution on islands; as the absence of mammals and Batrachians; the absence of certain great groups of insects on Madeira, and of Acaciæ and Banksias, &c., in New Zealand; the paucity of plants in some cases, &c. Not that those who believe in various accidental means of dispersal, can explain most of these cases; but they may at least say that these facts seem hardly compatible with former continuous land.

Finally. For these several reasons, and especially considering it certain (in which you will agree) that we are ex-

© The Complete Work of Charles Darwin Online

tremely ignorant of means of dispersal, I cannot avoid thinking that Forbes' 'Atlantis' was an ill-service to science, as checking a close study of means of dissemination. I shall be really grateful to hear, as briefly as you like, whether these arguments have any weight with you, putting yourself in the position of an honest judge. I told Hooker I was going to write to you on this subject; and I should like him to read this; but whether he or you will think it worth time and postage remains to be proved.

Yours most truly,
CHARLES DARWIN.

[On July 8th he wrote to Sir Charles Lyell.

"I am sorry you cannot give any verdict on Continental extensions; and I infer that you think my argument of not much weight against such extensions. I know I wish I could believe so."]

C. Darwin to Asa Gray.

Down, July 20th [1856].

. . . It is not a little egotistical, but I should like to tell you (and I do not *think* I have) how I view my work. Nineteen years (!) ago it occurred to me that whilst otherwise employed on Nat. Hist., I might perhaps do good if I noted any sort of facts bearing on the question of the origin of species, and this I have since been doing. Either species have been independently created, or they have descended from other species, like varieties from one species. I think it can be shown to be probable that man gets his most distinct varieties by preserving such as arise best worth keeping and destroying the others, but I should fill a quire if I were to go on. To be brief, I *assume* that species arise like our domestic varieties with *much* extinction; and then test this hypothesis by comparison with as many general and pretty well-established propositions as I can find made out,—in geographical

© The Complete Work of Charles Darwin Online

distribution, geological history, affinities, &c. &c. And it seems to me that, *supposing* that such hypothesis were to explain such general propositions, we ought, in accordance with the common way of following all sciences, to admit it till some better hypothesis be found out. For to my mind to say that species were created so and so is no scientific explanation, only a reverent way of saying it is so and so. But it is nonsensical trying to show how I try to proceed, in the compass of a note. But as an honest man, I must tell you that I have come to the heterodox conclusion, that there are no such things as independently created species—that species are only strongly defined varieties. I know that this will make you despise me. I do not much underrate the many *huge* difficulties on this view, but yet it seems to me to explain too much, otherwise inexplicable, to be false. Just to allude to one point in your last note, viz. about species of the same genus *generally* having a common or continuous area; if they are actual lineal descendants of one species, this of course would be the case; and the sadly too many exceptions (for me) have to be explained by climatal and geological changes. *A fortiori* on this view (but on exactly same grounds), all the individuals of the same species should have a continuous distribution. On this latter branch of the subject I have put a chapter together, and Hooker kindly read it over. I thought the exceptions and difficulties were so great that on the whole the balance weighed against my notions, but I was much pleased to find that it seemed to have considerable weight with Hooker, who said he had never been so much staggered about the permanence of species.

I must say one word more in justification (for I feel sure that your tendency will be to despise me and my crotchets), that all my notions about *how* species change are derived from long-continued study of the works of (and converse with) agriculturists and horticulturists; and I believe I see my way pretty clearly on the means used by nature to

© The Complete Work of Charles Darwin Online

change her species and *adapt* them to the wondrous and exquisitely beautiful contingencies to which every living being is exposed. . . .

C. Darwin to J. D. Hooker.

Down, July 30th, 1856.

MY DEAR HOOKER,—Your letter is of *much* value to me. I was not able to get a definite answer from Lyell,* as you will see in the enclosed letters, though I inferred that he thought nothing of my arguments. Had it not been for this correspondence, I should have written sadly too strongly. You may rely on it I shall put my doubts moderately. There never was such a predicament as mine: here you continental extensionists would remove enormous difficulties opposed to me, and yet I cannot honestly admit the doctrine, and must therefore say so. I cannot get over the fact that not a fragment of secondary or palæozoic rock has been found on any island above 500 or 600 miles from a mainland. You rather misunderstand me when you think I doubt the *possibility* of subsidence of 20,000 or 30,000 feet; it is only probability, considering such evidence as we have independently of distribution. I have not yet worked out in full detail the distribution of mammalia, both *identical* and allied, with respect to the *one element of depth of the sea*; but as far as I have gone, the results are to me surprisingly accordant with my very most troublesome belief in not such great geographical changes as you believe; and in mammalia we certainly know more of *means* of distribution than in any other class. Nothing is so vexatious to me, as so constantly finding myself drawing different conclusions from better judges than myself, from the same facts.

I fancy I have lately removed many (not geographical) great difficulties opposed to my notions, but God knows it may be all hallucination.

* On the continental extensions of Forbes and others.

© The Complete Work of Charles Darwin Online

Please return Lyell's letters.

What a capital letter of Lyell's that to you is, and what a wonderful man he is. I differ from him greatly in thinking that those who believe that species are *not* fixed will multiply specific names: I know in my own case my most frequent source of doubt was whether others would not think this or that was a God-created Barnacle, and surely deserved a name. Otherwise I should only have thought whether the amount of difference and permanence was sufficient to justify a name: I am, also, surprised at his thinking it immaterial whether species are absolute or not: whenever it is proved that all species are produced by generation, by laws of change, what good evidence we shall have of the gaps in formations. And what a science Natural History will be, when we are in our graves, when all the laws of change are thought one of the most important parts of Natural History.

I cannot conceive why Lyell thinks such notions as mine or of 'Vestiges,' will invalidate specific centres. But I must not run on and take up your time. My MS. will not, I fear, be copied before you go abroad. With hearty thanks.

Ever yours,

C. Darwin.

P.S.—After giving much condensed, my argument versus continental extensions, I shall append some such sentence, as that two better judges than myself have considered these arguments, and attach no weight to them.

C. Darwin to J. D. Hooker.

Down, August 5th [1856].

. . . I quite agree about Lyell's letters to me, which, though to me interesting, have afforded me no new light. Your letters, under the *geological* point of view, have been more valuable to me. You cannot imagine how earnestly I wish I could swallow continental extension, but I cannot;

© The Complete Work of Charles Darwin Online

the more I think (and I cannot get the subject out of my head), the more difficult I find it. If there were only some half-dozen cases, I should not feel the least difficulty; but the generality of the facts of all islands (except one or two) having a considerable part of their productions in common with one or more mainlands utterly staggers me. What a wonderful case of the Epacridæ! It is most vexatious, also humiliating, to me that I cannot follow and subscribe to the way in which you strikingly put your view of the case. I look at your facts (about Eucalyptus, &c.) as *damning* against continental extension, and if you like also damning against migration, or at least of *enormous* difficulty. I see the ground of our difference (in a letter I must put myself on an equality in arguing) lies, in my opinion, that scarcely anything is known of means of distribution. I quite agree with A. De Candolle's (and I dare say your) opinion that it is poor work putting together the merely *possible* means of distribution; but I see no other way in which the subject can be attacked, for I think that A. De Candolle's argument, that no plants have been introduced into England except by man's agency, of no weight. I cannot but think that the theory of continental extension does do some little harm as stopping investigation of the means of dispersal, which, whether *negative* or positive, seems to me of value; when negatived, then every one who believes in single centres will have to admit continental extensions.

. . . I see from your remarks that you do not understand my notions (whether or no worth anything) about modification; I attribute very little to the direct action of climate, &c. I suppose, in regard to specific centres, we are at cross purposes; I should call the kitchen garden in which the red cabbage was produced, or the farm in which Bakewell made the Shorthorn cattle, the specific centre of these *species*! And surely this is centralisation enough!

I thank you most sincerely for all your assistance; and

© The Complete Work of Charles Darwin Online

whether or no my book may be wretched, you have done your best to make it less wretched. Sometimes I am in very good spirits and sometimes very low about it. My own mind is decided on the question of the origin of species; but, good heavens, how little that is worth! . . .

[With regard to "specific centres," a passage from a letter dated July 25, 1856, from Sir Charles Lyell to Sir J. D. Hooker ('Life,' vol. ii. p. 216) is of interest:

"I fear much that if Darwin argues that species are phantoms, he will also have to admit that single centres of dispersion are phantoms also, and that would deprive me of much of the value which I ascribe to the present provinces of animals and plants, as illustrating modern and tertiary changes in physical geography."

He seems to have recognised, however, that the phantom doctrine would soon have to be faced, for he wrote in the same letter: "Whether Darwin persuades you and me to renounce our faith in species (when geological epochs are considered) or not, I foresee that many will go over to the indefinite modifiability doctrine."

In the autumn my father was still working at geographical distribution, and again sought aid from Sir J. D. Hooker.

"In the course of some weeks, you unfortunate wretch, you will have my MS. on one point of Geographical Distribution. I will, however, never ask such a favour again; but in regard to this one piece of MS., it is of infinite importance to me for you to see it; for never in my life have I felt such difficulty what to do, and I heartily wish I could slur the whole subject over."

In a letter to Sir J. D. Hooker (June, 1856), the following characteristic passage occurs, suggested, no doubt, by the

© The Complete Work of Charles Darwin Online

kind of work which his chapter on Geographical Distribution entailed:

"There is wonderful ill logic in his [E. Forbes'] famous and admirable memoir on distribution, as it appears to me, now that I have got it up so as to give the heads in a page. Depend on it, my saying is a true one, viz. that a compiler is a *great* man, and an original man a commonplace man. Any fool can generalise and speculate; but, oh, my heavens! to get up *at second hand* a New Zealand Flora, that is work."]

C. Darwin to W. D. Fox.

Oct. 3 [1856].

. . . I remember you protested against Lyell's advice of writing a *sketch* of my species doctrines. Well, when I began I found it such unsatisfactory work that I have desisted, and am now drawing up my work as perfect as my materials of nineteen years' collecting suffice, but do not intend to stop to perfect any line of investigation beyond current work. Thus far and no farther I shall follow Lyell's urgent advice. Your remarks weighed with me considerably. I find to my sorrow it will run to quite a big book. I have found my careful work at pigeons really invaluable, as enlightening me on many points on variation under domestication. The copious old literature, by which I can trace the gradual changes in the breeds of pigeons has been extraordinarily useful to me. I have just had pigeons and fowls *alive* from the Gambia! Rabbits and ducks I am attending to pretty carefully, but less so than pigeons. I find most remarkable differences in the skeletons of rabbits. Have you ever kept any odd breeds of rabbits, and can you give me any details? One other question. You used to keep hawks; do you at all know, after eating a bird, how soon after they throw up the pellet?

© The Complete Work of Charles Darwin Online

No subject gives me so much trouble and doubt and difficulty as the means of dispersal of the same species of terrestrial productions on the oceanic islands. Land mollusca drive me mad, and I cannot anyhow get their eggs to experimentise their power of floating and resistance to the injurious action of salt water. I will not apologise for writing so much about my own doings, as I believe you will like to hear. Do sometime, I beg you, let me hear how you get on in health; and *if so inclined*, let me have some words on call-ducks.

My dear Fox, yours affectionately,

CH. DARWIN.

[With regard to his book he wrote (Nov. 10th) to Sir Charles Lyell:

"I am working very steadily at my big book; I have found it quite impossible to publish any preliminary essay or sketch; but am doing my work as completely as my present materials allow without waiting to perfect them. And this much acceleration I owe to you."]

C. Darwin to J. D. Hooker.

Down, Sunday [Oct. 1856].

MY DEAR HOOKER,—The seeds are come all safe, many thanks for them. I was very sorry to run away so soon and miss any part of my *most* pleasant evening; and I ran away like a Goth and Vandal without wishing Mrs. Hooker good-bye; but I was only just in time, as I got on the platform the train had arrived.

I was particularly glad of our discussion after dinner; fighting a battle with you always clears my mind wonderfully. I groan to hear that A. Gray agrees with you about the condition of Botanical Geography. All I know is that if you had had to search for light in Zoological Geography you would by contrast, respect your own subject a vast deal

© The Complete Work of Charles Darwin Online

more than you now do. The hawks have behaved like gentlemen, and have cast up pellets with lots of seeds in them; and I have just had a parcel of partridge's feet well caked with mud!!! * Adios.

Your insane and perverse friend,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Nov. 4th [1856].

MY DEAR HOOKER,—I thank you more *cordially* than you will think probable, for your note. Your verdict † has been a great relief. On my honour I had no idea whether or not you would say it was (and I knew you would say it very kindly) so bad, that you would have begged me to have burnt the whole. To my own mind my MS. relieved me of some few difficulties, and the difficulties seemed to me pretty fairly stated, but I had become so bewildered with conflicting facts, evidence, reasoning and opinions, that I felt to myself that I had lost all judgment. Your general verdict is *incomparably* more favourable than I had anticipated . . .

C. Darwin to J. D. Hooker.

Down, Nov. 23rd [1856].

MY DEAR HOOKER,—I fear I shall weary you with letters, but do not answer this, for in truth and without flattery, I so value your letters, that after a heavy batch, as of late, I feel that I have been extravagant and have drawn too much money, and shall therefore have to stint myself on another occasion.

When I sent my MS. I felt strongly that some preliminary questions on the causes of variation ought to have been sent you. Whether I am right or wrong in these points is quite a

* The mud in such cases often contains seeds, so that plants are thus transported.

† On the MS. relating to geographical distribution.

© The Complete Work of Charles Darwin Online

separate question, but the conclusion which I have come to, quite independently of geographical distribution, is that external conditions (to which naturalists so often appeal) do by themselves *very little*. How much they do is the point of all others on which I feel myself very weak. I judge from the facts of variation under domestication, and I may yet get more light. But at present, after drawing up a rough copy on this subject, my conclusion is that external conditions do *extremely* little, except in causing mere variability. This mere variability (causing the child *not* closely to resemble its parent) I look at as *very* different from the formation of a marked variety or new species. (No doubt the variability is governed by laws, some of which I am endeavouring very obscurely to trace.) The formation of a strong variety or species I look at as almost wholly due to the selection of what may be incorrectly called *chance* variations or variability. This power of selection stands in the most direct relation to time, and in the state of nature can be only excessively slow. Again, the slight differences selected, by which a race or species is at last formed, stands, as I think can be shown (even with plants, and obviously with animals), in a far more important relation to its associates than to external conditions. Therefore, according to my principles, whether right or wrong, I cannot agree with your proposition that time, and altered conditions, and altered associates, are "convertible terms." I look at the first and the last as *far* more important: time being important only so far as giving scope to selection. God knows whether you will perceive at what I am driving. I shall have to discuss and think more about your difficulty of the temperate and sub-arctic forms in the S. hemisphere than I have yet done. But I am inclined to think that I am right (if my general principles are right), that there would be little tendency to the formation of a new species, during the period of migration, whether shorter or longer, though considerable variability may have supervened. . . .

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Dec. 24th [1856].

. . . How I do wish I lived near you to discuss matters with. I have just been comparing definitions of species, and stating briefly how systematic naturalists work out their subjects. Aquilegia in the Flora Indica was a capital example for me. It is really laughable to see what different ideas are prominent in various naturalists' minds, when they speak of "species;" in some, resemblance is everything and descent of little weight—in some, resemblance seems to go for nothing, and Creation the reigning idea—in some, descent is the key,—in some, sterility an unfailing test, with others it is not worth a farthing. It all comes, I believe, from trying to define the undefinable. I suppose you have lost the odd black seed from the birds' dung, which germinated,—anyhow, it is not worth taking trouble over. I have now got about a dozen seeds out of small birds' dung. Adios,

My dear Hooker, ever yours,

C. DARWIN.

C. Darwin to Asa Gray.

Down, Jan. 1st [1857?]

MY DEAR DR. GRAY,—I have received the second part of your paper,* and though I have nothing particular to say, I must send you my thanks and hearty admiration. The whole paper strikes me as quite exhausting the subject, and I quite fancy and flatter myself I now appreciate the character of your Flora. What a difference in regard to Europe your remark in relation to the genera makes! I have been eminently glad to see your conclusion in regard to the species of large genera widely ranging; it is in strict conformity with

* 'Statistics of the Flora of the Northern United States.'—*Silliman's Journal*, 1857.

© The Complete Work of Charles Darwin Online

the results I have worked out in several ways. It is of great importance to my notions. By the way you have paid me a *great* compliment:* to be *simply* mentioned even in such a paper I consider a very great honour. One of your conclusions makes me groan, viz. that the line of connection of the strictly Alpine plants is through Greenland. I should *extremely* like to see your reasons published in detail, for it "riles" me (this is a proper expression, is it not?) dreadfully. Lyell told me, that Agassiz having a theory about when Saurians were first created, on hearing some careful observations opposed to this, said he did not believe it, "for Nature never lied." I am just in this predicament, and repeat to you that, "Nature never lies," ergo, theorisers are always right. . . .

Overworked as you are, I dare say you will say that I am an odious plague; but here is another suggestion! I was led by one of my wild speculations to conclude (though it has nothing to do with geographical distribution, yet it has with your statistics) that trees would have a strong tendency to have flowers with diœcious, monœcious or polygamous structure. Seeing that this seemed so in Persoon, I took one little British Flora, and discriminating trees from bushes according to Loudon, I have found that the result was in species, genera and families, as I anticipated. So I sent my notions to Hooker to ask him to tabulate the New Zealand Flora for this end, and he thought my result sufficiently curious, to do so; and the accordance with Britain is very striking, and the more so, as he made three classes of trees, bushes, and herbaceous plants. (He says further he shall work the Tasmanian Flora on the same principle.) The bushes hold an intermediate position between the other two classes. It seems to me a

* "From some investigations of his own, this sagacious naturalist inclines to think that large genera range over a larger area than the species of small genera do."—Asa Gray, *loc. cit.*

© The Complete Work of Charles Darwin Online

curious relation in itself, and is very much so, if my theory and explanation are correct.*

With hearty thanks, your most troublesome friend,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, April 12th [1857].

MY DEAR HOOKER,—Your letter has pleased me much, for I never can get it out of my head, that I take unfair advantage of your kindness, as I receive all and give nothing. What a splendid discussion you could write on the whole subject of variation! The cases discussed in your last note are valuable to me (though odious and damnable), as showing how profoundly ignorant we are on the causes of variation. I shall just allude to these cases, as a sort of sub-division of polymorphism a little more definite, I fancy, than the variation of, for instance, the Rubi, and equally or more perplexing.

I have just been putting my notes together on variations *apparently* due to the immediate and direct action of external causes; and I have been struck with one result. The most firm sticklers for independent creation admit, that the fur of the *same* species is thinner towards the south of the range of the same species than to the north—that the *same* shells are brighter-coloured to the south than north; that the same [shell] is paler-coloured in deep water—that insects are smaller and darker on mountains—more livid and testaceous near the sea—that plants are smaller and more hairy and with brighter flowers on mountains: now in all such, and other cases, distinct species in the two zones follow the same rule, which seems to me to be most simply explained by species, being only strongly marked varieties, and therefore following

* See 'Origin,' ed. i. p. 100.

© The Complete Work of Charles Darwin Online

the same laws as recognised and admitted varieties. I mention all this on account of the variation of plants in ascending mountains; I have quoted the foregoing remark only generally with no examples, for I add, there is so much doubt and dispute what to call varieties; but yet I have stumbled on so many casual remarks on *varieties* of plants on mountains being so characterised, that I presume there is some truth in it. What think you? Do you believe there is *any* tendency in *varieties*, as *generally* so called, of plants to become more hairy, and with proportionally larger and brighter-coloured flowers in ascending a mountain?

I have been interested in my "weed garden," of 3 × 2 feet square: I mark each seedling as it appears, and I am astonished at the number that come up, and still more at the number killed by slugs, &c. Already 59 have been so killed; I expected a good many, but I had fancied that this was a less potent check than it seems to be, and I attributed almost exclusively to mere choking, the destruction of the seedlings. Grass-seedlings seem to suffer much less than exogens. . . .

C. Darwin to J. D. Hooker.

Moor Park, Farnham, [April (?) 1857.]

MY DEAR HOOKER,—Your letter has been forwarded to me here, where I am undergoing hydropathy for a fortnight, having been here a week, and having already received an amount of good which is quite incredible to myself and quite unaccountable. I can walk and eat like a hearty Christian, and even my nights are good. I cannot in the least understand how hydropathy can act as it certainly does on me. It dulls one's brain splendidly; I have not thought about a single species of any kind since leaving home. Your note has taken me aback; I thought the hairiness, &c., of Alpine *species* was generally admitted; I am sure I have seen it

© The Complete Work of Charles Darwin Online

alluded to a score of times. Falconer was haranguing on it the other day to me. Meyen or Gay, or some such fellow (whom you would despise), I remember, makes some remark on Chilian Cordillera plants. Wimmer has written a little book on the same lines, and on *varieties* being so characterized in the Alps. But after writing to you, I confess I was staggered by finding one man (Moquin-Tandon, I think) saying that Alpine flowers are strongly inclined to be white, and Linnæus saying that cold makes plants *apetalous*, even the same species! Are Arctic plants often apetalous? My general belief from my compiling work is quite to agree with what you say about the little direct influence of climate; and I have just alluded to the hairiness of Alpine plants as an *exception*. The odoriferousness would be a good case for me if I knew of *varieties* being more odoriferous in dry habitats.

I fear that I have looked at the hairiness of Alpine plants as so generally acknowledged that I have not marked passages, so as at all to see what kind of evidence authors advance. I must confess, the other day, when I asked Falconer, whether he knew of *individual* plants losing or acquiring hairiness when transported, he did not. But now *this second*, my memory flashes on me, and I am certain I have somewhere got marked a case of hairy plants from the Pyrenees losing hairs when cultivated at Montpellier. Shall you think me very impudent if I tell you that I have sometimes thought that (quite independently of the present case), you are a little too hard on bad observers; that a remark made by a bad observer *cannot* be right; an observer who deserves to be damned, you would utterly damn. I feel entire deference to any remark you make out of your own head; but when in opposition to some poor devil, I somehow involuntarily feel not quite so much, but yet much deference for your opinion. I do not know in the least whether there is any truth in this my criticism against you, but I have often thought I would tell you it.

© The Complete Work of Charles Darwin Online

I am really very much obliged for your letter, for, though I intended to put only one sentence and that vaguely, I should probably have put that much too strongly.

Ever, my dear Hooker, yours most truly,

C. DARWIN.

P.S.—This note, as you see, has not anything requiring an answer.

The distribution of fresh-water molluscs has been a horrid incubus to me, but I think I know my way now; when first hatched they are very active, and I have had thirty or forty crawl on a dead duck's foot; and they cannot be jerked off, and will live fifteen and even twenty-four hours out of water.

[The following letter refers to the expedition of the Austrian frigate *Novara;* Lyell had asked my father for suggestions.]

C. Darwin to C. Lyell.

Down, Feb. 11th [1857].

MY DEAR LYELL,—I was glad to see in the newspapers about the Austrian Expedition. I have nothing to add geologically to my notes in the Manual.* I do not know whether the Expedition is tied down to call at only fixed spots. But if there be any choice or power in the scientific men to influence the places—this would be most desirable. It is my most deliberate conviction that nothing would aid more, Natural History, than careful collecting and investigating *all the productions* of the most isolated islands, especially of the southern hemisphere. Except Tristan d'Acunha and Kerguelen Land, they are very imperfectly known; and even at Kerguelen Land, how much there is to make out about the lignite beds, and whether there are signs of old Glacial action. Every sea-shell and insect and plant is of value from such spots. Some one in the Expedition especially ought to have

* The article "Geology" in the 'Admiralty 'Manual of Scientific Enquiry.'

© The Complete Work of Charles Darwin Online

Hooker's New Zealand Essay. What grand work to explore Rodriguez, with its fossil birds, and little known productions of every kind. Again the Seychelles, which, with the Cocos so near, must be a remnant of some older land. The outer island of Juan Fernandez is little known. The investigation of these little spots by a band of naturalists would be grand; St. Paul's and Amsterdam would be glorious, botanically, and geologically. Can you not recommend them to get my 'Journal' and 'Volcanic Islands' on account of the Galapagos. If they come from the north it will be a shame and a sin if they do not call at Cocos Islet, one of the Galapagos. I always regretted that I was not able to examine the great craters on Albemarle Island, one of the Galapagos. In New Zealand urge on them to look out for erratic boulders and marks of old glaciers.

Urge the use of the dredge in the Tropics; how little or nothing we know of the limit of life downward in the hot seas?

My present work leads me to perceive how much the domestic animals have been neglected in out of the way countries.

The Revillagigedo Island off Mexico, I believe, has never been trodden by foot of naturalist.

If the expedition sticks to such places as Rio, Cape of Good Hope, Ceylon and Australia, &c., it will not do much.

Ever yours most truly,

C. DARWIN.

[The following passage occurs in a letter to Mr. Fox, February 22, 1857, and has reference to the book on Evolution on which he was still at work:

"I am got most deeply interested in my subject; though I wish I could set less value on the bauble fame, either present or posthumous, than I do, but not I think, to any extreme

© The Complete Work of Charles Darwin Online

degree: yet, if I know myself, I would work just as hard, though with less gusto, if I knew that my book would be published for ever anonymously."]

C. Darwin to A. R. Wallace.

Moor Park, May 1st, 1857.

MY DEAR SIR,—I am much obliged for your letter of October 10th, from Celebes, received a few days ago; in a laborious undertaking, sympathy is a valuable and real encouragement. By your letter and even still more by your paper * in the Annals, a year or more ago, I can plainly see that we have thought much alike and to a certain extent have come to similar conclusions. In regard to the Paper in the Annals, I agree to the truth of almost every word of your paper; and I dare say that you will agree with me that it is very rare to find oneself agreeing pretty closely with any theoretical paper; for it is lamentable how each man draws his own different conclusions from the very same facts. This summer will make the 20th year (!) since I opened my first note-book, on the question how and in what way do species and varieties differ from each other. I am now preparing my work for publication, but I find the subject so very large, that though I have written many chapters, I do not suppose I shall go to press for two years. I have never heard how long you intend staying in the Malay Archipelago; I wish I might profit by the publication of your Travels there before my work appears, for no doubt you will reap a large harvest of facts. I have acted already in accordance with your advice of keeping domestic varieties, and those appearing in a state of nature, distinct; but I have sometimes doubted of the wisdom of this, and therefore I am glad to be backed by your opinion. I must confess, however, I rather doubt the truth

* "On the Law that has regulated the Introduction of New Species." —Ann. Nat. Hist., 1855.

© The Complete Work of Charles Darwin Online

of the now very prevalent doctrine of all our domestic animals having descended from several wild stocks; though I do not doubt that it is so in some cases. I think there is rather better evidence on the sterility of hybrid animals than you seem to admit: and in regard to plants the collection of carefully recorded facts by Kölreuter and Gaertner (and Herbert) is *enormous*. I most entirely agree with you on the little effects of "climatal conditions," which one sees referred to *ad nauseam* in all books: I suppose some very little effect must be attributed to such influences, but I fully believe that they are very slight. It is really *impossible* to explain my views (in the compass of a letter), on the causes and means of variation in a state of nature; but I have slowly adopted a distinct and tangible idea,—whether true or false others must judge; for the firmest conviction of the truth of a doctrine by its author, seems, alas, not to be the slightest guarantee of truth! . . .

C. Darwin to J. D. Hooker.

Moor Park, Saturday [May 2nd, 1857].

MY DEAR HOOKER,—You have shaved the hair off the Alpine plants pretty effectually. The case of the Anthyllis will make a "tie" with the believed case of Pyrenees plants becoming glabrous at low levels. If I *do* find that I have marked such facts, I will lay the evidence before you. I wonder how the belief could have originated! Was it through final causes to keep the plants warm? Falconer in talk coupled the two facts of woolly Alpine plants and mammals. How candidly and meekly you took my Jeremiad on your severity to second-class men. After I had sent it off, an ugly little voice asked me, once or twice, how much of my noble defence of the poor in spirit and in fact, was owing to your having not seldom smashed favourite notions of my own. I silenced the ugly little voice with contempt, but it would whisper again and again. I sometimes despise

© The Complete Work of Charles Darwin Online

myself as a poor compiler as heartily as you could do, though I do *not* despise my whole work, as I think there is enough known to lay a foundation for the discussion on the origin of species. I have been led to despise and laugh at myself as a compiler, for having put down that "Alpine plants have large flowers," and now perhaps I may write over these very words, "Alpine plants have small or apetalous flowers!" . . .

C. Darwin to J. D. Hooker.

Down [May] 16th [1857].

MY DEAR HOOKER,—You said—I hope honestly—that you did not dislike my asking questions on general points, you of course answering or not as time and inclination might serve. I find in the animal kingdom that . . . . any part or organ developed normally, (*i.e.* not a monstrosity) in a species in any *high* or *unusual* degree, compared with the same part or organ in allied species, tends to be *highly variable.* I cannot doubt this from my mass of collected facts. To give an instance, the Cross-bill is very abnormal in the structure of its bill compared with other allied Fringillidæ, and the beak is *eminently variable.* The Himantopus, remarkable from the wonderful length of its legs, is *very* variable in the length of its legs. I could give *many* most striking and curious illustrations in all classes; so many that I think it cannot be chance. But I have *none* in the vegetable kingdom, owing, as I believe, to my ignorance. If Nepenthes consisted of *one* or two species in a group with a pitcher developed, then I should have expected it to have been very variable; but I do not consider Nepenthes a case in point, for when a whole genus or group has an organ, however anomalous, I do not expect it to be variable,—it is only when one or few species differ greatly in some one part or organ from the forms *closely allied* to it in all other respects, that I believe such part or organ to be highly vari-

© The Complete Work of Charles Darwin Online

able. Will you turn this in your mind? it is an important apparent *law* (!) for me.

Ever yours,

C. DARWIN.

P.S.—I do not know how far you will care to hear, but I find Moquin-Tandon treats in his 'Tératologie' on villosity of plants, and seems to attribute more to dryness than altitude; but seems to think that it must be admitted that mountain plants are villose, and that this villosity is only in part explained by De Candolle's remark that the dwarfed condition of mountain plants would condense the hairs, and so give them the *appearance* of being more hairy. He quotes Senebier, 'Physiologie Végétale,' as authority—I suppose the first authority, for mountain plants being hairy.

If I could show positively that the endemic species were more hairy in dry districts, then the case of the varieties becoming more hairy in dry ground would be a fact for me.

C. Darwin to J. D. Hooker.

Down, June 3rd [1857].

MY DEAR HOOKER,—I am going to enjoy myself by having a prose on my own subjects to you, and this is a greater enjoyment to me than you will readily understand, as I for months together do not open my mouth on Natural History. Your letter is of great value to me, and staggers me in regard to my proposition. I dare say the absence of botanical facts may in part be accounted for by the difficulty of measuring slight variations. Indeed, after writing, this occurred to me; for I have *Crucianella stylosa* coming into flower, and the pistil ought to be very variable in length, and thinking of this I at once felt how could one judge whether it was variable in any high degree. How different, for instance, from the beak of a bird! But I am not satisfied with this explanation, and am staggered. Yet I think there is something

© The Complete Work of Charles Darwin Online

in the law; I have had so many instances, as the following: I wrote to Wollaston to ask him to run through the Madeira Beetles and tell me whether any one presented anything very anomalous in relation to its allies. He gave me a unique case of an enormous head in a female, and then I found in his book, already stated, that the size of the head was *astonishingly* variable. Part of the difference with plants may be accounted for by many of my cases being secondary male or *female* characters but then I have striking cases with hermaphrodite Cirripedes. The cases seem to me far too numerous for accidental coincidences of great variability and abnormal development. I presume that you will not object to my putting a note saying that you had reflected over the case, and though one or two cases seemed to support, quite as many or more seemed wholly contradictory. This want of evidence is the more surprising to me, as generally I find any proposition more easily tested by observations in botanical works, which I have picked up, than in zoological works. I never dreamed that you had kept the subject at all before your mind. Altogether the case is one more of my *many* horrid puzzles. My observations, though on so infinitely a small scale, on the struggle for existence, begin to make me see a little clearer how the fight goes on. Out of sixteen kinds of seed sown on my meadow, fifteen have germinated, but now they are perishing at such a rate that I doubt whether more than one will flower. Here we have choking which has taken place likewise on a great scale, with plants not seedlings, in a bit of my lawn allowed to grow up. On the other hand, in a bit of ground, 2 by 3 feet, I have daily marked each seedling weed as it has appeared during March, April and May, and 357 have come up, and of these 277 have *already* been killed, chiefly by slugs. By the way, at Moor Park, I saw rather a pretty case of the effects of animals on vegetation: there are enormous commons with clumps of old Scotch firs on the hills, and about eight or ten years ago some of these commons were

H 2

© The Complete Work of Charles Darwin Online

enclosed, and all round the clumps nice young trees are springing up by the million, looking exactly as if planted, so many are of the same age. In other parts of the common, not yet enclosed, I looked for miles and not *one* young tree could be seen. I then went near (within quarter of a mile of the clumps) and looked closely in the heather, and there I found tens of thousands of young Scotch firs (thirty in one square yard) with their tops nibbled off by the few cattle which occasionally roam over these wretched heaths. One little tree, three inches high, by the rings appeared to be twenty-six years old, with a short stem about as thick as a stick of sealing-wax. What a wondrous problem it is, what a play of forces, determining the kind and proportion of each plant in a square yard of turf! It is to my mind truly wonderful. And yet we are pleased to wonder when some animal or plant becomes extinct.

I am so sorry that you will not be at the Club. I see Mrs. Hooker is going to Yarmouth; I trust that the health of your children is not the motive. Good-bye.

My dear Hooker, ever yours,

C. DARWIN.

P.S.—I believe you are afraid to send me a ripe Edwardsia pod, for fear I should float it from New Zealand to Chile!!!

C. Darwin to J. D. Hooker.

Down, June 5 [1857].

MY DEAR HOOKER,—I honour your conscientious care about the medals.* Thank God! I am only an amateur (but a much interested one) on the subject.

It is an old notion of mine that more good is done by giving medals to younger men in the early part of their career, than as a mere reward to men whose scientific career is nearly finished. Whether medals ever do any good is a question which does

* The Royal Society's medals.

© The Complete Work of Charles Darwin Online

not concern us, as there the medals are. I am almost inclined to think that I would rather lower the standard, and give medals to young workers than to old ones with no *especial* claims. With regard to especial claims, I think it just deserving your attention, that if general claims are once admitted, it opens the door to great laxity in giving them. Think of the case of a very rich man, who aided *solely* with his money, but to a grand extent—or such an inconceivable prodigy as a minister of the Crown who really cared for science. Would you give such men medals? Perhaps medals could not be better applied than *exclusively* to such men. I confess at present I incline to stick to especial claims which can be put down on paper. . . .

I am much confounded by your showing that there are not obvious instances of my (or rather Waterhouse's) law of abnormal developments being highly variable. I have been thinking more of your remark about the difficulty of judging or comparing variability in plants from the great general variability of parts. I should look at the law as more completely smashed if you would turn in your mind for a little while for cases of great variability of an organ, and tell me whether it is moderately easy to pick out such cases; *for if they can be picked out*, and, notwithstanding, do not coincide with great or abnormal development, it would be a complete smasher. It is only beginning in your mind at the variability end of the question instead of at the abnormality end. *Perhaps* cases in which a part is highly variable in all the species of a group should be excluded, as possibly being something distinct, and connected with the perplexing subject of polymorphism. Will you perfect your assistance by further considering, for a little, the subject this way?

I have been so much interested this morning in comparing all my notes on the variation of the several species of the genus Equus and the results of their crossing. Taking most strictly analogous facts amongst the blessed pigeons for my guide,

© The Complete Work of Charles Darwin Online

I believe I can plainly see the colouring and marks of the grandfather of the Ass, Horse, Quagga, Hemionus and Zebra, some millions of generations ago! Should not I [have] sneer[ed] at any one who made such a remark to me a few years ago; but my evidence seems to me so good that I shall publish my vision at the end of my little discussion on this genus.

I have of late inundated you with my notions, you best of friends and philosophers.

Adios,

C. DARWIN.

C. Darwin to J. D. Hooker.

Moor Park, Farnham, June 25th [1857].

MY DEAR HOOKER,—This requires no answer, but I will ask you whenever we meet. Look at enclosed seedling gorses, especially one with the top knocked off. The leaves succeeding the cotyledons being almost clover-like in shape, seems to me feebly analogous to embryonic resemblances in young animals, as, for instance, the young lion being striped. I shall ask you whether this is so.* . . .

Dr. Lane † and wife, and mother-in-law, Lady Drysdale, are some of the nicest people I have ever met.

I return home on the 30th. Good-bye, my dear Hooker.

Ever yours,

C. DARWIN.

[Here follows a group of letters, of various dates, bearing on the question of large genera varying.]

C. Darwin to J. D. Hooker.

March 11th [1858].

. . . I was led to all this work by a remark of Fries, that the species in large genera were more closely related to each

* See 'Power of Movements in Plants,' p. 414.

† The physician at Moor Park.

© The Complete Work of Charles Darwin Online

other than in small genera; and if this were so, seeing that varieties and species are so hardly distinguishable, I concluded that I should find more varieties in the large genera than in the small. . . . Some day I hope you will read my short discussion on the whole subject. You have done me infinite service, whatever opinion I come to, in drawing my attention to at least the possibility or the probability of botanists recording more varieties in the large than in the small genera. It will be hard work for me to be candid in coming to my conclusion.

Ever yours, most truly,
C. DARWIN.

P.S.—I shall be several weeks at my present job. The work has been turning out badly for me this morning, and I am sick at heart; and, oh! how I do hate species and varieties.

C. Darwin to J. D. Hooker.

July 14th [1857?]

. . . I write now to supplicate most earnestly a favour, viz. the loan of *Boreau, Flore du centre de la France, either 1st or 2nd edition,* last best; also "Flora Ratisbonensis," by Dr. Fürnrohr, in 'Naturhist. Topographie von Regensburg, 1839.' If you can *possibly* spare them, will you send them at once to the enclosed address. If you have not them, will you send one line by return of post: as I must try whether Kippist* can anyhow find them, which I fear will be nearly impossible in the Linnean Library, in which I know they are.

I have been making some calculations about varieties, &c., and talking yesterday with Lubbock, he has pointed out to me the grossest blunder which I have made in principle, and which entails two or three weeks' lost work; and I am at a dead-lock till I have these books to go over again, and see

* The late Mr. Kippist was at this time in charge of the Linnean Society's Library.

© The Complete Work of Charles Darwin Online

what the result of calculation on the right principle is. I am the most miserable, bemuddled, stupid dog in all England, and am ready to cry with vexation at my blindness and presumption.

Ever yours, most miserably,
C. DARWIN.

C. Darwin to John Lubbock.

Down, [July] 14th [1857].

MY DEAR LUBBOCK,—You have done me the greatest possible service in helping me to clarify my brains. If I am as muzzy on all subjects as I am on proportion and chance, —what a book I shall produce!

I have divided the New Zealand Flora as you suggested. There are 339 species in genera of 4 and upwards, and 323 in genera of 3 and less.

The 339 species have 51 species presenting one or more varieties. The 323 species have only 37. Proportionately (339 : 323 :: 51 : 48·5) they ought to have had 48½ species presenting vars. So that the case goes as I want it, but not strong enough, without it be general, for me to have much confidence in. I am quite convinced yours is the right way: I had thought of it, but should never have done it had it not been for my most fortunate conversation with you.

I am quite shocked to find how easily I am muddled, for I had before thought over the subject much, and concluded my way was fair. It is dreadfully erroneous.

What a disgraceful blunder you have saved me from. I heartily thank you.

Ever yours,
C. DARWIN.

P.S.—It is enough to make me tear up all my MS. and give up in despair.

It will take me several weeks to go over all my materials. But oh, if you knew how thankful I am to you!

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, Aug. [1857].

MY DEAR HOOKER,—It is a horrid bore you cannot come soon, and I reproach myself that I did not write sooner. How busy you must be! with such a heap of botanists at Kew. Only think, I have just had a letter from Henslow, saying he will come here between 11th and 15th! Is not that grand? Many thanks about Fürnrohr. I must humbly supplicate Kippist to search for it: he most kindly got Boreau for me.

I am got extremely interested in tabulating, according to mere size of genera, the species having any varieties marked by Greek letters or otherwise: the result (as far as I have yet gone) seems to me one of the most important arguments I have yet met with, that varieties are only small species—or species only strongly marked varieties. The subject is in many ways so very important for me; I wish much you would think of any well-worked Floras with from 1000–2000 species, with the varieties marked. It is good to have hair-splitters and lumpers.* I have done, or am doing:—

Babington	British Flora.
Henslow	British Flora.
London Catalogue. H. C. Watson	British Flora.
Boreau	France.
Miquel	Holland.
Asa Gray	U. States.
Hooker	N. Zealand. Fragment of Indian Flora.
Wollaston	Madeira insects.

Has not Koch published a good German Flora? Does he mark varieties? Could you send it me? Is there not some grand Russian Flora, which perhaps has varieties marked? The Floras ought to be well known.

* Those who make many species are the "splitters," and those who make few are the "lumpers."

© The Complete Work of Charles Darwin Online

I am in no hurry for a few weeks. Will you turn this in your head, when, if ever, you have leisure? The subject is very important for my work, though I clearly see *many* causes of error. . . .

C. Darwin to Asa Gray.

Down, Feb. 21st [1859].

MY DEAR GRAY,—My last letter begged no favour, this one does: but it will really cost you very little trouble to answer me, and it will be of very *great* service to me, owing to a remark made to me by Hooker, which I cannot credit, and which was suggested to him by one of my letters. He suggested my asking you, and I told him I would not give the least hint what he thought. I generally believe Hooker implicitly, but he is sometimes, I think, and he confesses it, rather over-critical, and his ingenuity in discovering flaws seems to me admirable. Here is my question:—"Do you think that good botanists in drawing up a local Flora, whether small or large, or in making a Prodromus like De Candolle's, would almost universally, but unintentionally and unconsciously, tend to record (*i.e.* marking with Greek letters and giving short characters) varieties in the large or in the small genera? Or would the tendency be to record the varieties about equally in genera of all sizes? Are you yourself conscious on reflection that you have attended to, and recorded more carefully the varieties in large or small, or very small genera?"

I know what fleeting and trifling things varieties very often are; but my query applies to such as have been thought worth marking and recording. If you could screw time to send me ever so brief an answer to this, pretty soon, it would be a great service to me.

Yours most truly obliged,

CH. DARWIN.

© The Complete Work of Charles Darwin Online

P.S.—Do you know whether any one has ever published any remarks on the geographical range of varieties of plants in comparison with the species to which they are supposed to belong? I have in vain tried to get some vague idea, and with the exception of a little information on this head given me by Mr. Watson in a paper on Land Shells in U. States, I have quite failed; but perhaps it would be difficult for you to give me even a brief answer on this head, and if so I am not so unreasonable, *I assure you*, as to expect it.

If you are writing to England soon, you could enclose other letters [for] me to forward.

Please observe, the question is not whether there are more or fewer varieties in larger or smaller genera, but whether there is a stronger or weaker tendency in the minds of botanists to *record* such in large or small genera.

C. Darwin to J. D. Hooker.

Down, May 6th [1858].

. . . I send by this post my MS. on the "commonness," "range," and "variation" of species in large and small genera. You have undertaken a horrid job in so very kindly offering to read it, and I thank you warmly. I have just corrected the copy, and am disappointed in finding how tough and obscure it is; but I cannot make it clearer, and at present I loathe the very sight of it. The style of course requires further correction, and if published I must try, but as yet see not how, to make it clearer.

If you have much to say and can have patience to consider the whole subject, I would meet you in London on the Phil. Club day, so as to save you the trouble of writing. For Heaven's sake, you stern and awful judge and sceptic, remember that my conclusions may be true, notwithstanding that Botanists may have recorded more varieties in large than in small

© The Complete Work of Charles Darwin Online

genera. It seems to me a mere balancing of probabilities. Again I thank you most sincerely, but I fear you will find it a horrid job.

Ever yours,
C. DARWIN.

[The letters now continue the history of the years 1857 and 1858.]

C. Darwin to A. R. Wallace.

Down, Dec. 22nd, 1857.

MY DEAR SIR,—I thank you for your letter of Sept. 27th. I am extremely glad to hear that you are attending to distribution in accordance with theoretical ideas. I am a firm believer that without speculation there is no good and original observation. Few travellers have attended to such points as you are now at work on; and, indeed, the whole subject of distribution of animals is dreadfully behind that of plants. You say that you have been somewhat surprised at no notice having been taken of your paper in the Annals.* I cannot say that I am, for so very few naturalists care for anything beyond the mere description of species. But you must not suppose that your paper has not been attended to: two very good men, Sir C. Lyell, and Mr. E. Blyth at Calcutta, specially called my attention to it. Though agreeing with you on your conclusions in that paper, I believe I go much further than you; but it is too long a subject to enter on my speculative notions. I have not yet seen your paper on the distribution of animals in the Aru Islands. I shall read it with the utmost interest; for I think that the most interesting quarter of the whole globe in respect to distribution, and I have long been very imperfectly trying to collect data for the Malay Archipelago. I shall be quite prepared to subscribe to your

* "On the Law that has regulated the Introduction of New Species." —Ann. Nat. Hist., 1855.

© The Complete Work of Charles Darwin Online

doctrine of subsidence; indeed, from the quite independent evidence of the Coral Reefs I coloured my original map (in my Coral volume) of the Aru Islands as one of subsidence, but got frightened and left it uncoloured. But I can see that you are inclined to go much further than I am in regard to the former connection of oceanic islands with continents. Ever since poor E. Forbes propounded this doctrine, it has been eagerly followed; and Hooker elaborately discusses the former connection of all the Antarctic Islands and New Zealand and South America. About a year ago I discussed this subject much with Lyell and Hooker (for I shall have to treat of it), and wrote out my arguments in opposition; but you will be glad to hear that neither Lyell nor Hooker thought much of my arguments. Nevertheless, for once in my life, I dare withstand the almost preternatural sagacity of Lyell.

You ask about land-shells on islands far distant from continents: Madeira has a few identical with those of Europe, and here the evidence is really good, as some of them are subfossil. In the Pacific Islands there are cases of identity, which I cannot at present persuade myself to account for by introduction through man's agency; although Dr. Aug. Gould has conclusively shown that many land-shells have thus been distributed over the Pacific by man's agency. These cases of introduction are most plaguing. Have you not found it so in the Malay Archipelago? It has seemed to me in the lists of mammals of Timor and other islands, that *several* in all probability have been naturalised. . . .

You ask whether I shall discuss "man." I think I shall avoid the whole subject, as so surrounded with prejudices; though I fully admit that it is the highest and most interesting problem for the naturalist. My work, on which I have now been at work more or less for twenty years, will not fix or settle anything; but I hope it will aid by giving a large collection of facts, with one definite end. I get on very slowly, partly from ill-health, partly from being a very slow worker.

© The Complete Work of Charles Darwin Online

I have got about half written; but I do not suppose I shall publish under a couple of years. I have now been three whole months on one chapter on Hybridism!

I am astonished to see that you expect to remain out three or four years more. What a wonderful deal you will have seen, and what interesting areas—the grand Malay Archipelago and the richest parts of South America! I infinitely admire and honour your zeal and courage in the good cause of Natural Science; and you have my very sincere and cordial good wishes for success of all kinds, and may all your theories succeed, except that on Oceanic Islands, on which subject I will do battle to the death.

Pray believe me, my dear sir, yours very sincerely,

C. DARWIN.

C. Darwin to W. D. Fox.

Feb. 8th [1858].

. . . I am working very hard at my book, perhaps too hard. It will be very big, and I am become most deeply interested in the way facts fall into groups. I am like Crœsus overwhelmed with my riches in facts, and I mean to make my book as perfect as ever I can. I shall not go to press at soonest for a couple of years. . . .

C. Darwin to J. D. Hooker.

Feb. 23rd [1858].

. . . I was not much struck with the great Buckle, and I admired the way you stuck up about deduction and induction. I am reading his book,* which, with much sophistry, as it seems to me, is *wonderfully* clever and original, and with astounding knowledge.

I saw that you admired Mrs. Farrer's 'Questa tomba' of

* 'The History of Civilisation.'

© The Complete Work of Charles Darwin Online

Beethoven thoroughly; there is something grand in her sweet tones.

Farewell. I have partly written this note to drive bee's-cells out of my head; for I am half-mad on the subject to try to make out some simple steps from which all the wondrous angles may result.*

I was very glad to see Mrs. Hooker on Friday; how well she appears to be and looks.

Forgive your intolerable but affectionate friend,

C. DARWIN.

C. Darwin to W. D. Fox.

Down, April 16th [1858].

MY DEAR FOX,—I want you to observe one point for me, on which I am extremely much interested, and which will give you no trouble beyond keeping your eyes open, and that is a habit I know full well that you have.

I find horses of various colours often have a spinal band or stripe of different and darker tint than the rest of the body; rarely transverse bars on the legs, generally on the under-side of the front legs, still more rarely a very faint transverse shoulder-stripe like an ass.

Is there any breed of Delamere forest ponies? I have found out little about ponies in these respects. Sir P. Egerton has, I believe, some quite thoroughbred chestnut horses; have any of them the spinal stripe? Mouse-coloured ponies, or rather small horses, often have spinal and leg bars. So have dun horses (by dun I mean real colour of cream mixed with brown, bay, or chestnut). So have sometimes chestnuts, but I have not yet got a case of spinal stripe in chestnut, race horse, or in quite heavy cart-horse. Any fact of this nature of such stripes in horses would be *most* useful to me. There is a

* He had much correspondence on this subject with the late Professor Miller of Cambridge.

© The Complete Work of Charles Darwin Online

parallel case in the legs of the donkey, and I have collected some most curious cases of stripes appearing in various crossed equine animals. I have also a large mass of parallel facts in the breeds of pigeons about the wing bars. I *suspect* it will throw light on the colour of the primeval horse. So do help me if occasion turns up. . . . My health has been lately very bad from overwork, and on Tuesday I go for a fortnight's hydropathy. My work is everlasting. Farewell.

My dear Fox, I trust you are well. Farewell,

C. DARWIN.

C. Darwin to J. D. Hooker.

Moor Park, Farnham [April 26th, 1858].

. . . I have just had the innermost cockles of my heart rejoiced by a letter from Lyell. I said to him (or he to me) that I believed from the character of the flora of the Azores, that icebergs must have been stranded there; and that I expected erratic boulders would be detected embedded between the upheaved lava-beds; and I got Lyell to write to Hartung to ask, and now H. says my question explains what had astounded him, viz. large boulders (and some polished) of mica-schist, quartz, sandstone, &c., some embedded, and some 40 and 50 feet above the level of the sea, so that he had inferred that they had not been brought as ballast. Is this not beautiful?

The water-cure has done me some good, but I [am] nothing to boast of to-day, so good-bye.

My dear friend, yours,

C. D.

C. Darwin to C. Lyell.

Moor Park, Farnham, April 26th [1858].

MY DEAR LYELL,—I have come here for a fortnight's hydropathy, as my stomach had got, from steady work, into a

© The Complete Work of Charles Darwin Online

horrid state. I am extremely much obliged to you for sending me Hartung's interesting letter. The erratic boulders are splendid. It is a grand case of floating ice versus glaciers. He ought to have compared the northern and southern shores of the islands. It is eminently interesting to me, for I have written a very long chapter on the subject, collecting briefly all the geological evidence of glacial action in different parts of the world, and then at great length (on the theory of species changing) I have discussed the migration and modification of plants and animals, in sea and land, over a large part of the world. To my mind, it throws a flood of light on the whole subject of distribution, if combined with the modification of species. Indeed, I venture to speak with some little confidence on this, for Hooker, about a year ago, kindly read over my chapter, and though he then demurred gravely to the general conclusion, I was delighted to hear a week or two ago that he was inclined to come round pretty strongly to my views of distribution and change during the glacial period. I had a letter from Thompson, of Calcutta, the other day, which helps me much, as he is making out for me what heat our temperate plants can endure. But it is too long a subject for a note; and I have written thus only because Hartung's note has set the whole subject afloat in my mind again. But I will write no more, for my object here is to think about nothing, bathe much, walk much, eat much, and read much novels. Farewell, with many thanks, and very kind remembrance to Lady Lyell.

Ever yours,
C. DARWIN.

C. Darwin to Mrs. Darwin.

Moor Park, Wednesday, April [1858].

The weather is quite delicious. Yesterday, after writing to you, I strolled a little beyond the glade for an hour and a half,

© The Complete Work of Charles Darwin Online

and enjoyed myself—the fresh yet dark-green of the grand Scotch firs, the brown of the catkins of the old birches, with their white stems, and a fringe of distant green from the larches, made an excessively pretty view. At last I fell fast asleep on the grass, and awoke with a chorus of birds singing around me, and squirrels running up the trees, and some woodpeckers laughing, and it was as pleasant and rural a scene as ever I saw, and I did not care one penny how any of the beasts or birds had been formed. I sat in the drawing-room till after eight, and then went and read the Chief Justice's summing up, and thought Bernard * guilty, and then read a bit of my novel, which is feminine, virtuous, clerical, philanthropical, and all that sort of thing, but very decidedly flat. I say feminine, for the author is ignorant about money matters, and not much of a lady—for she makes her men say, "My Lady." I like Miss Craik very much, though we have some battles, and differ on every subject. I like also the Hungarian; a thorough gentleman, formerly attaché at Paris, and then in the Austrian cavalry, and now a pardoned exile, with broken health. He does not seem to like Kossuth, but says, he is certain [he is] a sincere patriot, most clever and eloquent, but weak, with no determination of character. . . .

* Simon Bernard was tried in April 1858 as an accessory to Orsini's attempt on the life of the Emperor of the French. The verdict was "not guilty."

© The Complete Work of Charles Darwin Online

CHAPTER IV.

THE WRITING OF THE 'ORIGIN OF SPECIES.

JUNE 18, 1858, TO NOVEMBER 1859.

[The letters given in the present chapter tell their story with sufficient clearness, and need but a few words of explanation. Mr. Wallace's Essay, referred to in the first letter, bore the title, 'On the Tendency of Varieties to depart indefinitely from the Original Type,' and was published in the Linnean Society's 'Journal' (1858, vol. iii. p. 53) as part of the joint paper of "Messrs. C. Darwin and A. Wallace," of which the full title was 'On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection.'

My father's contribution of the paper consisted of (1) Extracts from the sketch of 1844; (2) part of a letter addressed to Dr. Asa Gray, dated September 5, 1857, and which is given at p. 120. The paper was "communicated" to the Society by Sir Charles Lyell and Sir Joseph Hooker, in whose prefatory letter, a clear account of the circumstances of the case is given.

Referring to Mr. Wallace's Essay, they wrote:—

"So highly did Mr. Darwin appreciate the value of the views therein set forth, that he proposed, in a letter to Sir Charles Lyell, to obtain Mr. Wallace's consent to allow the Essay to be published as soon as possible. Of this step we highly approved, provided Mr. Darwin did not withhold from the public, as he was strongly inclined to do (in favour of

I 2

© The Complete Work of Charles Darwin Online

Mr. Wallace), the memoir which he had himself written on the same subject, and which, as before stated, one of us had perused in 1844, and the contents of which we had both of us been privy to for many years. On representing this to Mr. Darwin, he gave us permission to make what use we thought proper of his memoir, &c.; and in adopting our present course, of presenting it to the Linnean Society, we have explained to him that we are not solely considering the relative claims to priority of himself and his friend, but the interests of science generally."]

LETTERS.

C. Darwin to C. Lyell.

Down, 18th [June 1858].

MY DEAR LYELL,—Some year or so ago you recommended me to read a paper by Wallace in the 'Annals,'* which had interested you, and, as I was writing to him, I knew this would please him much, so I told him. He has to-day sent me the enclosed, and asked me to forward it to you. It seems to me well worth reading. Your words have come true with a vengeance—that I should be forestalled. You said this, when I explained to you here very briefly my views of 'Natural Selection' depending on the struggle for existence. I never saw a more striking coincidence; if Wallace had my MS. sketch written out in 1842, he could not have made a better short abstract! Even his terms now stand as heads of my chapters. Please return me the MS., which he does not say he wishes me to publish, but I shall, of course, at once write and offer to send to any journal. So all my originality, whatever it may amount to, will be smashed, though my book,

* Annals and Mag. of Nat. Hist., 1855.

© The Complete Work of Charles Darwin Online

if it will ever have any value, will not be deteriorated; as all the labour consists in the application of the theory.

I hope you will approve of Wallace's sketch, that I may tell him what you say.

My dear Lyell, yours most truly,

C. DARWIN.

C. Darwin to C. Lyell.

Down, Friday [June 25, 1858].

MY DEAR LYELL,—I am very sorry to trouble you, busy as you are, in so merely personal an affair; but if you will give me your deliberate opinion, you will do me as great a service as ever man did, for I have entire confidence in your judgment and honour. . . .

There is nothing in Wallace's sketch which is not written out much fuller in my sketch, copied out in 1844, and read by Hooker some dozen years ago. About a year ago I sent a short sketch, of which I have a copy, of my views (owing to correspondence on several points) to Asa Gray, so that I could most truly say and prove that I take nothing from Wallace. I should be extremely glad now to publish a sketch of my general views in about a dozen pages or so; but I cannot persuade myself that I can do so honourably. Wallace says nothing about publication, and I enclose his letter. But as I had not intended to publish any sketch, can I do so honourably, because Wallace has sent me an outline of his doctrine? I would far rather burn my whole book, than that he or any other man should think that I had behaved in a paltry spirit. Do you not think his having sent me this sketch ties my hands? . . . . If I could honourably publish, I would state that I was induced now to publish a sketch (and I should be very glad to be permitted to say, to follow your advice long ago given) from Wallace having sent me an outline of my general conclusions. We differ only, [in] that I was led to my

© The Complete Work of Charles Darwin Online

views from what artificial selection has done for domestic animals. I would send Wallace a copy of my letter to Asa Gray, to show him that I had not stolen his doctrine. But I cannot tell whether to publish now would not be base and paltry. This was my first impression, and I should have certainly acted on it had it not been for your letter.

This is a trumpery affair to trouble you with, but you cannot tell how much obliged I should be for your advice.

By the way, would you object to send this and your answer to Hooker to be forwarded to me, for then I shall have the opinion of my two best and kindest friends. This letter is miserably written, and I write it now, that I may for a time banish the whole subject; and I am worn out with musing . . .

My good dear friend, forgive me. This is a trumpery letter, influenced by trumpery feelings.

Yours most truly,

C. DARWIN.

I will never trouble you or Hooker on the subject again.

C. Darwin to C. Lyell.

Down, 26th [June 1858].

MY DEAR LYELL,—Forgive me for adding a P.S. to make the case as strong as possible against myself.

Wallace might say, "You did not intend publishing an abstract of your views till you received my communication. Is it fair to take advantage of my having freely, though unasked, communicated to you my ideas, and thus prevent me forestalling you?" The advantage which I should take being that I am induced to publish from privately knowing that Wallace is in the field. It seems hard on me that I should be thus compelled to lose my priority of many years' standing, but I cannot feel at all sure that this alters the

© The Complete Work of Charles Darwin Online

justice of the case. First impressions are generally right, and I at first thought it would be dishonourable in me now to publish.

Yours most truly,
C. DARWIN.

P.S.—I have always thought you would make a first-rate Lord Chancellor; and I now appeal to you as a Lord Chancellor.

C. Darwin to J. D. Hooker.

Down, Tuesday [June 29, 1858].

. . . . I have received your letters. I cannot think now * on the subject, but soon will. But I can see that you have acted with more kindness, and so has Lyell, even than I could have expected from you both, most kind as you are.

I can easily get my letter to Asa Gray copied, but it is too short.

. . . . God bless you. You shall hear soon, as soon as I can think.

Yours affectionately,
C. DARWIN.

C. Darwin to J. D. Hooker.

Tuesday night [June 29, 1858].

MY DEAR HOOKER,—I have just read your letter, and see you want the papers at once. I am quite prostrated, and can do nothing, but I send Wallace, and the abstract † of my letter to Asa Gray, which gives most imperfectly only the means of change, and does not touch on reasons for believing that species do change. I dare say all is too late. I hardly

* So soon after the death, from scarlet fever, of his infant child.

† "Abstract" is here used in the sense of "extract;" in this sense also it occurs in the 'Linnean Journal,' where the sources o my father's paper are described.

© The Complete Work of Charles Darwin Online

care about it. But you are too generous to sacrifice so much time and kindness. It is most generous, most kind. I send my sketch of 1844 solely that you may see by your own handwriting that you did read it. I really cannot bear to look at it. Do not waste much time. It is miserable in me to care at all about priority.

The table of contents will show what it is.

I would make a similar, but shorter and more accurate sketch for the 'Linnean Journal.'

I will do anything. God bless you, my dear kind friend.

I can write no more. I send this by my servant to Kew.

Yours,
C. DARWIN.

[The following letter is that already referred to as forming part of the joint paper published in the Linnean Society's 'Journal,' 1858] :—

C. Darwin to Asa Gray.

Down, Sept.* 5th [1857].

MY DEAR GRAY,—I forget the exact words which I used in my former letter, but I dare say I said that I thought you would utterly despise me when I told you what views I had arrived at, which I did because I thought I was bound as an honest man to do so. I should have been a strange mortal, seeing how much I owe to your quite extraordinary kindness, if in saying this I had meant to attribute the least bad feeling to you. Permit me to tell you that, before I had ever corresponded with you, Hooker had shown me several of your letters (not of a private nature), and these gave me the warmest feeling of respect to you; and I should indeed be

* The date is given as October in the 'Linnean Journal.' The extracts were printed from a duplicate undated copy in my father's possession, on which he had written, "This was sent to Asa Gray 8 or 9 months ago, I think October 1857."

© The Complete Work of Charles Darwin Online

ungrateful if your letters to me, and all I have heard of you, had not strongly enhanced this feeling. But I did not feel in the least sure that when you knew whither I was tending, you might not think me so wild and foolish in my views (God knows, arrived at slowly enough, and I hope conscientiously), that you would think me worth no more notice or assistance. To give one example: the last time I saw my dear old friend Falconer, he attacked me most vigorously, but quite kindly, and told me, "You will do more harm than any ten Naturalists will do good. I can see that you have already *corrupted* and half-spoiled Hooker!!" Now when I see such strong feeling in my oldest friends, you need not wonder that I always expect my views to be received with contempt. But enough and too much of this.

I thank you most truly for the kind spirit of your last letter. I agree to every word in it, and think I go as far as almost any one in seeing the grave difficulties against my doctrine. With respect to the extent to which I go, all the arguments in favour of my notions fall *rapidly* away, the greater the scope of forms considered. But in animals, embryology leads me to an enormous and frightful range. The facts which kept me longest scientifically orthodox are those of adaptation—the pollen-masses in asclepias—the mistletoe, with its pollen carried by insects, and seed by birds—the woodpecker, with its feet and tail, beak and tongue, to climb the tree and secure insects. To talk of climate or Lamarckian habit producing such adaptations to other organic beings is futile. This difficulty I believe I have surmounted. As you seem interested in the subject, and as it is an *immense* advantage to me to write to you and to hear, ever so briefly, what you think, I will enclose (copied, so as to save you trouble in reading) the briefest abstract of my notions on the means by which Nature makes her species. Why I think that species have really changed, depends on general facts in the affinities, embryology, rudimentary organs, geological history, and geo-

© The Complete Work of Charles Darwin Online

graphical distribution of organic beings. In regard to my Abstract, you must take immensely on trust, each paragraph occupying one or two chapters in my book. You will, perhaps, think it paltry in me, when I ask you not to mention my doctrine; the reason is, if any one, like the author of the 'Vestiges,' were to hear of them, he might easily work them in, and then I should have to quote from a work perhaps despised by naturalists, and this would greatly injure any chance of my views being received by those alone whose opinions I value. [Here follows a discussion on "large genera varying," which has no direct connection with the remainder of the letter.]

I. It is wonderful what the principle of Selection by Man, that is the picking out of individuals with any desired quality, and breeding from them, and again picking out, can do. Even breeders have been astonished at their own results. They can act on differences inappreciable to an uneducated eye. Selection has been *methodically* followed in Europe for only the last half century. But it has occasionally, and even in some degree methodically, been followed in the most ancient times. There must have been also a kind of unconscious selection from the most ancient times, namely, in the preservation of the individual animals (without any thought of their offspring) most useful to each race of man in his particular circumstances. The "roguing," as nursery-men call the destroying of varieties, which depart from their type, is a kind of selection. I am convinced that intentional and occasional selection has been the main agent in making our domestic races. But, however this may be, its great power of modification has been indisputably shown in late times. Selection acts only by the accumulation of very slight or greater variations, caused by external conditions, or by the mere fact that in generation the child is not absolutely similar to its parent. Man, by this power of accumulating variations, adapts living beings to his wants—he *may be said* to make

© The Complete Work of Charles Darwin Online

the wool of one sheep good for carpets, and another for cloth, &c.

II. Now, suppose there was a being, who did not judge by mere external appearance, but could study the whole internal organisation—who never was capricious—who should go on selecting for one end during millions of generations, who will say what he might not effect! In nature we have some *slight* variations, occasionally in all parts: and I think it can be shown that a change in the conditions of existence is the main cause of the child not exactly resembling its parents; and in nature, geology shows us what changes have taken place, and are taking place. We have almost unlimited time: no one but a practical geologist can fully appreciate this: think of the Glacial period, during the whole of which the same species of shells at least have existed; there must have been during this period, millions on millions of generations.

III. I think it can be shown that there is such an unerring power at work, or *Natural Selection* (the title of my book), which selects exclusively for the good of each organic being. The elder De Candolle, W. Herbert, and Lyell, have written strongly on the struggle for life; but even they have not written strongly enough. Reflect that every being (even the elephant) breeds at such a rate that, in a few years, or at most a few centuries or thousands of years, the surface of the earth would not hold the progeny of any one species. I have found it hard constantly to bear in mind that the increase of every single species is checked during some part of its life, or during some shortly recurrent generation. Only a few of those annually born can live to propagate their kind. What a trifling difference must often determine which shall survive and which perish!

IV. Now take the case of a country undergoing some change; this will tend to cause some of its inhabitants to vary slightly; not but what I believe most beings vary at all times

© The Complete Work of Charles Darwin Online

enough for selection to act on. Some of its inhabitants will be exterminated, and the remainder will be exposed to the mutual action of a different set of inhabitants, which I believe to be more important to the life of each being than mere climate. Considering the infinitely various ways beings have to obtain food by struggling with other beings, to escape danger at various times of life, to have their eggs or seeds disseminated, &c. &c., I cannot doubt that during millions of generations individuals of a species will be born with some slight variation profitable to some part of its economy; such will have a better chance of surviving, propagating this variation, which again will be slowly increased by the accumulative action of natural selection; and the variety thus formed will either coexist with, or more commonly will exterminate its parent form. An organic being like the woodpecker, or the mistletoe, may thus come to be adapted to a score of contingencies; natural selection, accumulating those slight variations in all parts of its structure which are in any way useful to it, during any part of its life.

V. Multiform difficulties will occur to every one on this theory. Most can, I think, be satisfactorily answered.—"Natura non facit saltum" answer some of the most obvious. The slowness of the change, and only a very few undergoing change at any one time answers others. The extreme imperfections of our geological records answer others.

VI. One other principle, which may be called the principle of divergence, plays, I believe, an important part in the origin of species. The same spot will support more life if occupied by very diverse forms: we see this in the many generic forms in a square yard of turf (I have counted twenty species belonging to eighteen genera), or in the plants and insects, on any little uniform islet, belonging to almost as many genera and families as to species. We can understand this with the higher animals, whose habits we best understand. We know that it has been experimentally shown that a plot

© The Complete Work of Charles Darwin Online

of land will yield a greater weight, if cropped with several species of grasses, than with two or three species. Now every single organic being, by propagating rapidly, may be said to be striving its utmost to increase in numbers. So it will be with the offspring of any species after it has broken into varieties, or sub-species, or true species. And it follows, I think, from the foregoing facts, that the varying offspring of each species will try (only few will succeed) to seize on as many and as diverse places in the economy of nature as possible. Each new variety or species when formed will generally take the place of, and so exterminate its less well-fitted parent. This, I believe, to be the origin of the classification or arrangement of all organic beings at all times. These always *seem* to branch and sub-branch like a tree from a common trunk; the flourishing twigs destroying the less vigorous—the dead and lost branches rudely representing extinct genera and families.

This sketch is *most* imperfect; but in so short a space I cannot make it better. Your imagination must fill up many wide blanks. Without some reflection, it will appear all rubbish; perhaps it will appear so after reflection.

C. D.

P.S.—This little abstract touches only the accumulative power of natural selection, which I look at as by far the most important element in the production of new forms. The laws governing the incipient or primordial variation (unimportant except as the groundwork for selection to act on, in which respect it is all important), I shall discuss under several heads, but I can come, as you may well believe, only to very partial and imperfect conclusions.

[The joint paper of Mr. Wallace and my father was read at the Linnean Society on the evening of July 1st. Sir Charles Lyell and Sir J. D. Hooker were present, and both, I believe, made a few remarks, chiefly with a view of impressing on those

© The Complete Work of Charles Darwin Online

present the necessity of giving the most careful consideration to what they had heard. There was, however, no semblance of a discussion. Sir Joseph Hooker writes to me: "The interest excited was intense, but the subject was too novel and too ominous for the old school to enter the lists, before armouring. After the meeting it was talked over with bated breath: Lyell's approval, and perhaps in a small way mine, as his lieutenant in the affair, rather overawed the Fellows, who would otherwise have flown out against the doctrine. We had, too, the vantage ground of being familiar with the authors and their theme."]

C. Darwin to J. D. Hooker.

Down, July 5th [1858].

MY DEAR HOOKER,—We are become more happy and less panic-struck, now that we have sent out of the house every child, and shall remove H., as soon as she can move. The first nurse became ill with ulcerated throat and quinsy, and the second is now ill with the scarlet fever, but, thank God, is recovering. You may imagine how frightened we have been. It has been a most miserable fortnight. Thank you much for your note, telling me that all had gone on prosperously at the Linnean Society. You must let me once again tell you how deeply I feel your generous kindness and Lyell's on this occasion. But in truth it shames me that you should have lost time on a mere point of priority. I shall be curious to see the proofs. I do not in the least understand whether my letter to A. Gray is to be printed; I suppose not, only your note; but I am quite indifferent, and place myself absolutely in your and Lyell's hands.

I can easily prepare an abstract of my whole work, but I can hardly see how it can be made scientific for a Journal, without giving facts, which would be impossible. Indeed, a mere abstract cannot be very short. Could you give me any

© The Complete Work of Charles Darwin Online

idea how many pages of the Journal could probably be spared me?

Directly after my return home, I would begin and cut my cloth to my measure. If the Referees were to reject it as not strictly scientific, I could, perhaps, publish it as a pamphlet.

With respect to my big interleaved abstract,* would you send it any time before you leave England, to the enclosed address? If you do not go till August 7th–10th, I should prefer it left with you. I hope you have jotted criticisms on my MS. on big Genera, &c., sufficient to make you remember your remarks, as I should be infinitely sorry to lose them. And I see no chance of our meeting if you go soon abroad. We thank you heartily for your invitation to join you: I can fancy nothing which I should enjoy more; but our children are too delicate for us to leave; I should be mere living lumber.

Lastly, you said you would write to Wallace; I certainly should much like this, as it would quite exonerate me: if you would send me your note, sealed up, I would forward it with my own, as I know the address, &c.

Will you answer me some time about your notions of the length of my abstract.

If you see Lyell, will you tell him how truly grateful I feel for his kind interest in this affair of mine. You must know that I look at it, as very important, for the reception of the view of species not being immutable, the fact of the greatest Geologist and Botanist in England taking *any sort of interest* in the subject: I am sure it will do much to break down prejudices.

Yours affectionately,

C. DARWIN.

* The Sketch of 1844.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Miss Wedgwood's, Hartfield, Tunbridge Wells,
[July 13th, 1858].

MY DEAR HOOKER,—Your letter to Wallace seems to me perfect, quite clear and most courteous. I do not think it could possibly be improved, and I have to-day forwarded it with a letter of my own. I always thought it very possible that I might be forestalled, but I fancied that I had a grand enough soul not to care; but I found myself mistaken and punished; I had, however, quite resigned myself, and had written half a letter to Wallace to give up all priority to him, and should certainly not have changed had it not been for Lyell's and your quite extraordinary kindness. I assure you I feel it, and shall not forget it. I am *more* than satisfied at what took place at the Linnean Society. I had thought that your letter and mine to Asa Gray were to be only an appendix to Wallace's paper.

We go from here in a few days to the sea-side, probably to the Isle of Wight, and on my return (after a battle with pigeon skeletons) I will set to work at the abstract, though how on earth I shall make anything of an abstract in thirty pages of the Journal, I know not, but will try my best. I shall order Bentham; is it not a pity that you should waste time in tabulating varieties? for I can get the Down schoolmaster to do it on my return, and can tell you all the results.

I must try and see you before your journey; but do not think I am fishing to ask you to come to Down, for you will have no time for that.

You cannot imagine how pleased I am that the notion of Natural Selection has acted as a purgative on your bowels of immutability. Whenever naturalists can look at species changing as certain, what a magnificent field will be open,—on all the laws of variation,—on the genealogy of all living beings,—on their lines of migration, &c. &c. Pray thank

© The Complete Work of Charles Darwin Online

Mrs. Hooker for her very kind little note, and pray say how truly obliged I am, and in truth ashamed to think that she should have had the trouble of copying my ugly MS. It was extraordinarily kind in her. Farewell, my dear kind friend.

Yours affectionately,

C. DARWIN.

P.S.—I have had some fun here in watching a slave-making ant; for I could not help rather doubting the wonderful stories, but I have now seen a defeated marauding party, and I have seen a migration from one nest to another of the slave-makers, carrying their slaves (who are *house*, and not field niggers) in their mouths!

I am inclined to think that it is a true generalisation that, when honey is secreted at one point of the circle of the corolla, if the pistil bends, it always bends into the line of the gangway to the honey. The Larkspur is a good instance, in contrast to Columbine,—if you think of it, just attend to this little point.

C. Darwin to C. Lyell.

King's Head Hotel, Sandown, Isle of Wight.
July 18th [1858].

. . . We are established here for ten days, and then go on to Shanklin, which seems more amusing to one, like myself, who cannot walk. We hope much that the sea may do H. and L. good. And if it does, our expedition will answer, but not otherwise.

I have never half thanked you for all the extraordinary trouble and kindness you showed me about Wallace's affair. Hooker told me what was done at the Linnean Society, and I am far more than satisfied, and I do not think that Wallace can think my conduct unfair in allowing you and Hooker to do whatever you thought fair. I certainly was a little annoyed to lose all priority, but had resigned myself to my fate. I am

© The Complete Work of Charles Darwin Online

going to prepare a longer abstract; but it is really impossible to do justice to the subject, except by giving the facts on which each conclusion is grounded, and that will, of course, be absolutely impossible. Your name and Hooker's name appearing as in any way the least interested in my work will, I am certain, have the most important bearing in leading people to consider the subject without prejudice. I look at this as so very important, that I am almost glad of Wallace's paper for having led to this.

My dear Lyell, yours most gratefully,

CH. DARWIN.

[The following letter refers to the proof-sheets of the Linnean paper. The 'introduction' means the prefatory letter signed by Sir C. Lyell and Sir J. D. Hooker.]

C. Darwin to J. D. Hooker.

King's Head Hotel, Sandown, Isle of Wight.
July 21st [1858].

MY DEAR HOOKER,—I received only yesterday the proof-sheets, which I now return. I think your introduction cannot be improved.

I am disgusted with my bad writing. I could not improve it, without rewriting all, which would not be fair or worth while, as I have begun on a better abstract for the Linnean Society. My excuse is that it *never* was intended for publication. I have made only a few corrections in the style; but I cannot make it decent, but I hope moderately intelligible. I suppose some one will correct the revise. (Shall I?)

Could I have a clean proof to send to Wallace?

I have not yet fully considered your remarks on big genera (but your general concurrence is of the *highest possible* interest to me); nor shall I be able till I re-read my MS.; but you may rely on it that you never make a remark to me which is

© The Complete Work of Charles Darwin Online

lost from *inattention*. I am particularly glad you do not object to my stating your objections in a modified form, for they always struck me as very important, and as having much inherent value, whether or no they were fatal to my notions. I will consider and reconsider all your remarks. . . .

I have ordered Bentham, for, as —— says, it will be very curious to see a Flora written by a man who knows nothing of British plants!!

I am very glad at what you say about my Abstract, but you may rely on it that I will condense to the utmost. I would aid in money if it is too long.* In how many ways you have aided me!

Yours affectionately,
C. DARWIN.

[The 'Abstract' mentioned in the last sentence of the preceding letter was in fact the 'Origin of Species,' on which he now set to work. In his 'Autobiography' (p. 85) he speaks of beginning to write in September, but in his Diary he wrote, "July 20 to Aug. 12, at Sandown, began Abstract of Species book." "Sep. 16, Recommenced Abstract." The book was begun with the idea that it would be published as a paper, or series of papers, by the Linnean Society, and it was only in the late autumn that it became clear that it must take the form of an independent volume.]

C. Darwin to J. D. Hooker.

Norfolk House, Shanklin, Isle of Wight.
Friday [July] 30th [1858].

MY DEAR HOOKER,—Will you give the enclosed scrap to Sir William to thank him for his kindness; and this gives me an excuse to amuse myself by writing to you a note, which requires no answer.

* That is to say, he would help to pay for the printing, if it should prove too long for the Linnean Society.

© The Complete Work of Charles Darwin Online

This is a very charming place, and we have got a very comfortable house. But, alas, I cannot say that the sea has done H. or L. much good. Nor has my stomach recovered from all our troubles. I am very glad we left home, for six children have now died of scarlet fever in Down. We return on the 14th of August.

I have got Bentham,* and am charmed with it, and William (who has just started for a tour abroad) has been making out all sorts of new (to me) plants capitally. The little scraps of information are so capital . . . The English names in the analytical keys drive us mad: give them by all means, but why on earth [not] make them subordinate to the Latin; it puts me in a passion. W. charged into the Compositæ and Umbelliferæ like a hero, and demolished ever so many in grand style.

I pass my time by doing daily a couple of hours of my Abstract, and I find it amusing and improving work. I am now most heartily obliged to you and Lyell for having set me on this; for I shall, when it is done, be able to finish my work with greater ease and leisure. I confess I hated the thought of the job; and now I find it very unsatisfactory in not being able to give my reasons for each conclusion.

It will be longer than I expected; it will take thirty-five of my MS. folio pages to give an abstract on variation under domestication alone; but I will try to put in nothing which does not seem to me of some interest, and which was once new to me. It seems a queer plan to give an abstract of an unpublished work; nevertheless, I repeat, I am extremely glad I have begun in earnest on it.

I hope you and Mrs. Hooker will have a very very pleasant tour. Farewell, my dear Hooker.

Yours affectionately,

C. DARWIN.

* 'British Flora.'

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Norfolk House, Shanklin, Isle of Wight.
Thursday [Aug. 5, 1858].

MY DEAR HOOKER,—I should think the note apologetical about the style of the Abstract was best as a note . . . . But I write now to ask you to send me by return of post the MS. on big genera, that I may make an abstract of a couple of pages in length. I presume that you have quite done with it, otherwise I would not for anything have it back. If you tie it with string, and mark it MS. for printing, it will not cost, I should think, more than *4d.* I shall wish much to say that you have read this MS. and concur; but you shall, before I read it to the Society, hear the sentence.

What you tell me after speaking with Busk about the length of the Abstract is an *immense* relief to me; it will make the labour far less, not having to shorten so much every single subject; but I will try not to be too diffusive. I fear it will spoil all interest in my book,* whenever published. The Abstract will do very well to divide into several parts: thus I have just finished "Variation under Domestication," in forty-four MS. pages, and that would do for one evening; but I should be extremely sorry if all could not be published together.

What else you say about my Abstract pleases me highly, but frightens me, for I fear I shall never be able to make it good enough. But how I do run on about my own affairs to you!

I was astonished to see Sir W. Hooker's card here two or three days ago: I was unfortunately out walking. Henslow, also, has written to me, proposing to come to Down on the 9th, but alas, I do not return till the 13th, and my wife not till a week later; so that I am also most sorry to think I shall

* The larger book begun in 1856.

© The Complete Work of Charles Darwin Online

not see you, for I should not like to leave home so soon. I had thought of going to London and running down for an hour or two to Kew. . . .

C. Darwin to J. D. Hooker.

Norfolk House, Shanklin, Isle of Wight.
[August 1858.]

MY DEAR HOOKER,—I write merely to say that the MS. came safely two or three days ago. I am much obliged for the correction of style: I find it unutterably difficult to write clearly. When we meet I must talk over a few points on the subject.

You speak of going to the sea-side somewhere; we think this the nicest sea-side place which we have ever seen, and we like Shanklin better than other spots on the south coast of the island, though many are charming and prettier, so that I would suggest your thinking of this place. We are on the actual coast; but tastes differ so much about places.

If you go to Broadstairs, when there is a strong wind from the coast of France and in fine, dry, warm weather, look out and you will *probably* (!) see thistle-seeds blown across the Channel. The other day I saw one blown right inland, and then in a few minutes a second one and then a third; and I said to myself, God bless me, how many thistles there must be in France; and I wrote a letter in imagination to you. But I then looked at the *low* clouds, and noticed that they were not coming inland, so I feared a screw was loose, I then walked beyond a headland and found the wind parallel to the coast, and on this very headland a noble bed of thistles, which by every wide eddy were blown far out to sea, and then came right in at right angles to the shore! One day such a number of insects were washed up by the tide, and I brought to life thirteen species of Coleoptera; not that I suppose these came from France. But do you watch for thistle-seed as you saunter along the coast. . . .

© The Complete Work of Charles Darwin Online

C. Darwin to Asa Gray.

Aug. 11th [1858].

MY DEAR GRAY,—Your note of July 27th has just reached me in the Isle of Wight. It is a real and great pleasure to me to write to you about my notions; and even if it were not so, I should be a most ungrateful dog, after all the invaluable assistance which you have rendered me, if I did not do anything which you asked.

I have discussed in my long MS. the later changes of climate and the effect on migration, and I will here give you an *abstract* of an *abstract* (which latter I am preparing of my whole work for the Linnean Society). I cannot give you facts, and I must write dogmatically, though I do not feel so on any point. I may just mention, in order that you may believe that I have *some* foundation for my views, that Hooker has read my MS., and though he at first demurred to my main point, he has since told me that further reflection and new facts have made him a convert.

In the older, or perhaps newer, Pliocene age (a little *before* the Glacial epoch) the temperature was higher; of this there can be little doubt; the land, on a *large scale*, held much its present disposition: the species were mainly, judging from shells, what they are now. At this period when all animals and plants ranged 10° or 15° nearer the poles, I believe the northern part of Siberia and of North America, being almost *continuous*, were peopled (it is quite possible, considering the shallow water, that Behring Straits were united, perhaps a little southward) by a nearly uniform fauna and flora, just as the Arctic regions now are. The climate then became gradually colder till it became what it now is; and then the temperate parts of Europe and America would be separated, as far as migration is concerned, just as they now are. Then came on the Glacial period, driving far south all living things; middle or even southern

© The Complete Work of Charles Darwin Online

Europe being peopled with Arctic productions; as the warmth returned, the Arctic productions slowly crawled up the mountains as they became denuded of snow; and we now see on their summits the remnants of a once continuous flora and fauna. This is E. Forbes's theory, which, however, I may add, I had written out four years before he published.

Some facts have made me vaguely *suspect* that between the glacial and the present temperature there was a period of *slightly* greater warmth. According to my modification-doctrines, I look at many of the species of North America which *closely* represent those of Europe, as having become modified since the Pliocene period, when in the northern part of the world there was nearly free communication between the old and new worlds. But now comes a more important consideration; there is a considerable body of geological evidence that during the Glacial epoch the whole world was colder; I inferred that, many years ago, from erratic boulder phenomena carefully observed by me on both the east and west coast of South America. Now I am so bold as to believe that at the height of the Glacial epoch, *and when all Tropical productions must have been considerably distressed*, several temperate forms slowly travelled into the heart of the Tropics, and even reached the southern hemisphere; and some few southern forms penetrated in a reverse direction northward. (Heights of Borneo with Australian forms, Abyssinia with Cape forms.) Wherever there was nearly continuous *high* land, this migration would have been immensely facilitated; hence the European character of the plants of Tierra del Fuego and summits of Cordilleras; hence ditto on Himalaya. As the temperature rose, all the temperate intruders would crawl up the mountains. Hence the European forms on Nilgherries, Ceylon, summit of Java, Organ Mountains of Brazil. But these intruders being surrounded with new forms would be very liable to be improved or modified by natural selection, to adapt them to the new forms with which they had to

© The Complete Work of Charles Darwin Online

compete; hence most of the forms on the mountains of the Tropics are not identical, but *representative* forms of North temperate plants.

There are similar classes of facts in marine productions. All this will appear very rash to you, and rash it may be; but I am sure not so rash as it will at first appear to you: Hooker could not stomach it at all at first, but has become largely a convert. From mammalia and shallow sea, I believe Japan to have been joined to main land of China within no remote period; and then the migration north and south before, during, and after the Glacial epoch would act on Japan, as on the corresponding latitude of China and the United States.

I should beyond anything like to know whether you have any Alpine collections from Japan, and what is their character. This letter is miserably expressed, but perhaps it will suffice to show what I believe have been the later main migrations and changes of temperature. . . .

C. Darwin to J. D. Hooker.

[Down,] Oct. 6th, 1858.

. . . If you have or can make leisure, I should very much like to hear news of Mrs. Hooker, yourself, and the children. Where did you go, and what did you do and are doing? There is a comprehensive text.

You cannot tell how I enjoyed your little visit here. It did me much good. If Harvey is still with you, pray remember me very kindly to him.

. . . I am working most steadily at my Abstract, but it grows to an inordinate length; yet fully to make my view clear (and never giving briefly more than a fact or two, and slurring over difficulties), I cannot make it shorter. It will yet take me three or four months; so slow do I work, though never idle. You cannot imagine what a service you have

© The Complete Work of Charles Darwin Online

done me in making me make this Abstract; for though I thought I had got all clear, it has clarified my brains very much, by making me weigh the relative importance of the several elements.

I have been reading with much interest your (as I believe it to be) capital memoir of R. Brown in the *Gardeners' Chronicle*. . . .

C. Darwin to J. D. Hooker.

Down, Oct. 12th, 1858.

. . . I have sent eight copies* by post to Wallace, and will keep the others for him, for I could not think of any one to send any to.

I pray you not to pronounce too strongly against Natural Selection, till you have read my Abstract, for though I dare say you will strike out *many* difficulties, which have never occurred to me; yet you cannot have thought so fully on the subject as I have.

I expect my Abstract will run into a small volume, which will have to be published separately. . . .

What a splendid lot of work you have in hand.

Ever yours,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Oct. 13th, 1858.

. . . I have been a little vexed at myself at having asked you not "to pronounce too strongly against Natural Selection." I am sorry to have bothered you, though I have been much interested by your note in answer. I wrote the sentence without reflection. But the truth is, that I have so accustomed myself, partly from being quizzed by my non-naturalist relations, to expect opposition and even contempt, that I forgot for

* Of the joint paper by C. Darwin and A. R. Wallace.

© The Complete Work of Charles Darwin Online

the moment that you are the one living soul from whom I have constantly received sympathy. Believe [me] that I never forget for even a minute how much assistance I have received from you. You are quite correct that I never even suspected that my speculations were a "jam-pot" to you; indeed, I thought, until quite lately, that my MS. had produced no effect on you, and this has often staggered me. Nor did I know that you had spoken in general terms about my work to our friends, excepting to dear old Falconer, who some few years ago once told me that I should do more mischief than any ten other naturalists would do good, [and] that I had half-spoiled you already! All this is stupid egotistical stuff, and I write it only because you may think me ungrateful for not having valued and understood your sympathy; which God knows is not the case. It is an accursed evil to a man to become so absorbed in any subject as I am in mine.

I was in London yesterday for a few hours with Falconer, and he gave me a magnificent lecture on the age of man. We are not upstarts; we can boast of a pedigree going far back in time coeval with extinct species. He has a grand fact of some large molar tooth in the Trias.

I am quite knocked up, and am going next Monday to revive under Water-cure at Moor Park.

My dear Hooker, yours affectionately,

C. DARWIN.

C. Darwin to J. D. Hooker.

Nov. 1858.

. . . . I had vowed not to mention my everlasting Abstract to you again, for I am sure I have bothered you far more than enough about it; but, as you allude to its publication, I may say that I have the chapters on Instinct and Hybridism to abstract, which may take a fortnight each; and my materials for Palæontology, Geographical Distribution,

© The Complete Work of Charles Darwin Online

and Affinities, being less worked up, I dare say each of these will take me three weeks, so that I shall not have done at soonest till April, and then my Abstract will in bulk make a small volume. I never give more than one or two instances, and I pass over briefly all difficulties, and yet I cannot make my Abstract shorter, to be satisfactory, than I am now doing, and yet it will expand to a small volume. . . .

[About this time my father revived his old knowledge of beetles in helping his boys in their collecting. He sent a short notice to the 'Entomologist's Weekly Intelligencer,' June 25th, 1859, recording the capture of *Licinus silphoides*, *Clytus mysticus*, *Panagæus 4-pustulatus*. The notice begins with the words, "We three very young collectors having lately taken in the parish of Down," &c., and is signed by three of his boys, but was clearly not written by them. I have a vivid recollection of the pleasure of turning out my bottle of dead beetles for my father to name, and the excitement, in which he fully shared, when any of them proved to be uncommon ones. The following letters to Mr. Fox (November 13, 1858), and to Sir John Lubbock, illustrate this point:]

C. Darwin to W. D. Fox.

Down, Nov. 13th [1858].

. . . W., my son, is now at Christ's College, in the rooms above yours. My old Gyp, Impey, was astounded to hear that he was my son, and very simply asked, "Why, has he been long married?" What pleasant hours those were when I used to come and drink coffee with you daily! I am reminded of old days by my third boy having just begun collecting beetles, and he caught the other day *Brachinus crepitans*, of immortal Whittlesea Mere memory. My blood boiled with old ardour when he caught a Licinus—a prize unknown to me . . .

© The Complete Work of Charles Darwin Online

C. Darwin to John Lubbock.

Thursday [before 1857].

DEAR LUBBOCK,—I do not know whether you care about beetles, but for the chance I send this in a bottle, which I never remember having seen; though it is excessively rash to speak from a twenty-five-year old remembrance. Whenever we meet you can tell me whether you know it. . . .

I feel like an old war-horse at the sound of the trumpet, when I read about the capturing of rare beetles—is not this a magnanimous simile for a decayed entomologist?—It really almost makes me long to begin collecting again. Adios.

"Floreat Entomologia"!—to which toast at Cambridge I have drunk many a glass of wine. So again, "Floreat Entomologia." N.B. I have *not* now been drinking any glasses full of wine.

Yours,

C. D.

C. Darwin to Herbert Spencer.

Down, Nov. 25th [1858].

DEAR SIR,—I beg permission to thank you sincerely for your very kind present of your Essays.* I have already read several of them with much interest. Your remarks on the general argument of the so-called development theory seems to me admirable. I am at present preparing an Abstract of a larger work on the changes of species; but I treat the subject simply as a naturalist, and not from a general point of view, otherwise, in my opinion, your argument could not have been improved on, and might have been quoted by me with great advantage. Your article on Music has also interested me much, for I had often thought on the subject, and had come

* 'Essays, Scientific, Political, and Speculative,' by Herbert Spencer, 1858–74.

© The Complete Work of Charles Darwin Online

to nearly the same conclusion with you, though unable to support the notion in any detail. Furthermore, by a curious coincidence, expression has been for years a persistent subject with me for *loose* speculation, and I must entirely agree with you that all expression has some biological meaning. I hope to profit by your criticism on style, and with very best thanks, I beg leave to remain, dear Sir,

Yours truly obliged,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Dec. 24th [1858].

MY DEAR HOOKER,—Your news about your unsolicited salary and house is jolly, and creditable to the Government. My room (28 x 19), with divided room above, with *all fixtures* (and painted), not furniture, and plastered outside, cost about £500. I am heartily glad of this news.

Your facts about distribution are, indeed, very striking. I remember well that none of your many wonderful facts in your several works, perplexed me, for years, more than the migration having been mainly from north to south, and not in the reverse direction. I have now at last satisfied *myself* (but that is very different from satisfying others) on this head; but it would take a little volume to fully explain myself. I did not for long see the bearing of a conclusion, at which I had arrived, with respect to this subject. It is, that species inhabiting a very large area, and therefore existing in large numbers, and which have been subjected to the severest competition with many other forms, will have arrived, through natural selection, at a higher stage of perfection than the inhabitants of a small area. Thus I explain the fact of so many anomalies, or what may be called "living fossils," inhabiting now only fresh water, having been beaten out, and exterminated in the sea, by more im-

© The Complete Work of Charles Darwin Online

proved forms; thus all existing Ganoid fishes are fresh water, as [are] Lepidosiren and Ornithorhynchus, &c. The plants of Europe with Asia, as being the largest territory, I look at as the most "improved," and therefore as being able to withstand the less-perfected Australian plants; though these could not resist the Indian. See how all the productions of New Zealand yield to those of Europe. I dare say you will think all this utter bosh, but I believe it to be solid truth.

You will, I think, admit that Australian plants, flourishing so in India, is no argument that they could hold their own against the ten thousand natural contingencies of other plants, insects, animals, &c. &c. With respect to South-West Australia and the Cape, I am shut up, and can only d—n the whole case.

. . . You say you should like to see my MS., but you did read and approved of my long Glacial chapter, and I have not yet written my Abstract on the whole of the Geographical Distribution, nor shall I begin it for two or three weeks. But either Abstract or the old MS. I should be *delighted* to send you, especially the Abstract chapter. . . .

I have now written 330 folio pages of my Abstract, and it will require 150–200; so that it will make a printed volume of 400 pages, and must be printed separately, which I think will be better in many respects. The subject really seems to me too large for discussion at any Society, and I believe religion would be brought in by men whom I know.

I am thinking of a 12mo. volume, like Lyell's fourth or fifth edition of the 'Principles.' . . .

I have written you a scandalously long note. So now good bye, my dear Hooker,

Ever yours,
C. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, Jan. 20th, 1859.

MY DEAR HOOKER,—I should very much like to borrow Heer at some future time, for I want to read nothing perplexing at present till my Abstract is done. Your last very instructive letter shall make me very cautious on the hyper-speculative points we have been discussing.

When you say you cannot master the train of thoughts, I know well enough that they are too doubtful and obscure to be mastered. I have often experienced what you call the humiliating feeling of getting more and more involved in doubt, the more one thinks of the facts and reasoning on doubtful points. But I always comfort myself with thinking of the future, and in the full belief that the problems which we are just entering on, will some day be solved ; and if we just break the ground we shall have done some service, even if we reap no harvest.

I quite agree that we only differ in *degree* about the means of dispersal, and that I think a satisfactory amount of accordance. You put in a very striking manner the mutation of our continents, and I quite agree ; I doubt only about our oceans.

I also agree (I am in a very agreeing frame of mind) with your *argumentum ad hominem*, about the highness of the Australian Flora from the number of species and genera ; but here comes in a superlative bothering element of doubt, viz. the effects of isolation.

The only point in which I *presumptuously* rather demur is about the status of the naturalised plants in Australia. I think Müller speaks of their having spread largely beyond cultivated ground ; and I can hardly believe that our European plants would occupy stations so barren that the native plants could not live there. I should require much evidence to make me believe this. I have written this note merely to thank you, as you will see it requires no answer.

© The Complete Work of Charles Darwin Online

I have heard to my amazement this morning from Phillips that the Geological Council have given me the Wollaston Medal ! ! !

Ever yours,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Jan. 23rd, 1859.

. . . I enclose letters to you and me from Wallace. I admire extremely the spirit in which they are written. I never felt very sure what he would say. He must be an amiable man. Please return that to me, and Lyell ought to be told how well satisfied he is. These letters have vividly brought before me how much I owe to your and Lyell's most kind and generous conduct in all this affair.

. . . How glad I shall be when the Abstract is finished, and I can rest ! . . .

C. Darwin to A. R. Wallace.

Down, Jan. 25th [1859].

MY DEAR SIR,—I was extremely much pleased at receiving three days ago your letter to me and that to Dr. Hooker. Permit me to say how heartily I admire the spirit in which they are written. Though I had absolutely nothing whatever to do in leading Lyell and Hooker to what they thought a fair course of action, yet I naturally could not but feel anxious to hear what your impression would be. I owe indirectly much to you and them ; for I almost think that Lyell would have proved right, and I should never have completed my larger work, for I have found my Abstract hard enough with my poor health, but now, thank God, I am in my last chapter but one. My Abstract will make a small volume of 400 or 500 pages. Whenever published, I will, of course, send you a copy, and then you will see what I mean about the part which I believe selection has played with domestic produc-

© The Complete Work of Charles Darwin Online

tions. It is a very different part, as you suppose, from that played by "Natural Selection." I sent off, by the same address as this note, a copy of the 'Journal of the Linnean Society,' and subsequently I have sent some half-dozen copies of the paper. I have many other copies at your disposal. . . .

I am glad to hear that you have been attending to birds' nests. I have done so, though almost exclusively under one point of view, viz. to show that instincts vary, so that selection could work on and improve them. Few other instincts, so to speak, can be preserved in a Museum.

Many thanks for your offer to look after horses' stripes; if there are any donkeys, pray add them. I am delighted to hear that you have collected bees' combs. . . . . This is an especial hobby of mine, and I think I can throw a light on the subject. If you can collect duplicates, at no very great expense, I should be glad of some specimens for myself with some bees of each kind. Young, growing, and irregular combs, and those which have not had pupæ, are most valuable for measurements and examination. Their edges should be well protected against abrasion.

Every one whom I have seen has thought your paper very well written and interesting. It puts my extracts (written in 1839, now just twenty years ago!), which I must say in apology were never for an instant intended for publication, into the shade.

You ask about Lyell's frame of mind. I think he is somewhat staggered, but does not give in, and speaks with horror, often to me, of what a thing it would be, and what a job it would be for the next edition of 'The Principles,' if he were "perverted." But he is most candid and honest, and I think will end by being perverted. Dr. Hooker has become almost as heterodox as you or I, and I look at Hooker as *by far* the most capable judge in Europe.

Most cordially do I wish you health and entire success in all your pursuits, and, God knows, if admirable zeal and energy deserve success, most amply do you deserve it. I look

© The Complete Work of Charles Darwin Online

at my own career as nearly run out. If I can publish my Abstract and perhaps my greater work on the same subject, I shall look at my course as done.

Believe me, my dear sir, yours very sincerely,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, March 2nd [1859].

MY DEAR HOOKER,—Here is an odd, though very little, fact. I think it would be hardly possible to name a bird which apparently could have less to do with distribution than a Petrel. Sir W. Milner, at St. Kilda, cut open some young nestling Petrels, and he found large, curious nuts in their crops; I suspect picked up by parent birds from the Gulf stream. He seems to value these nuts excessively. I have asked him (but I doubt whether he will) to send a nut to Sir William Hooker (I gave this address for grandeur's sake) to see if any of you can name it and its native country. Will you *please mention* this to Sir William Hooker, and if the nut does arrive, will you oblige me by returning it to "Sir W. Milner, Bart., Nunappleton, Tadcaster," in a registered letter, and I will repay you postage. Enclose slip of paper with the name and country if you can, and let me hereafter know. Forgive me asking you to take this much trouble; for it is a funny little fact after my own heart.

Now for another subject. I have finished my Abstract of the chapter on Geographical Distribution, as bearing on my subject. I should like you much to read it; but I say this, believing that you will not do so, if, as I believe to be the case, you are extra busy. On my honour, I shall not be mortified, and I earnestly beg you not to do it, if it will bother you. I want it, because I here feel especially unsafe, and errors may have crept in. Also, I should much like to know what parts you will *most vehemently* object to. I know

© The Complete Work of Charles Darwin Online

we do, and must, differ widely on several heads. Lastly, I should like particularly to know whether I have taken anything from you, which you would like to retain for first publication; but I think I have chiefly taken from your published works, and, though I have several times, in this chapter and elsewhere, acknowledged your assistance, I am aware that it is not possible for me in the Abstract to do it sufficiently.* But again let me say that you must not offer to read it if very irksome. It is long—about ninety pages, I expect, when fully copied out.

I hope you are all well. Moor Park has done me some good.

Yours affectionately,

C. DARWIN.

P.S.—Heaven forgive me, here is another question: How far am I right in supposing that with plants, the most important characters for main divisions are embryological? The seed itself cannot be considered as such, I suppose, nor the albumen, &c. But I suppose the cotyledons and their position, and the position of the plumule and the radicle, and the position and form of the whole embryo in the seed are embryological, and how far are these very important? I wish to instance plants as a case of high importance of embryological characters in classification. In the Animal Kingdom there is, of course, no doubt of this.

C. Darwin to J. D. Hooker.

Down, March 5th [1859].

MY DEAR HOOKER,—Many thanks about the seed . . . it is curious. Petrels at St. Kilda apparently being fed by

* "I never did pick any one's pocket, but whilst writing my present chapter I keep on feeling (even when differing most from you) just as if I were stealing from you, so much do I owe to your writings and conversation, so much more than mere acknowledgments show."—Letter to Sir J. D. Hooker, 1859.

© The Complete Work of Charles Darwin Online

seeds raised in the West Indies. It should be noted whether it is a nut ever imported into England. I am *very* glad you will read my Geographical MS.; it is now copying, and it will (I presume) take ten days or so in being finished; it shall be sent as soon as done. . . .

I shall be very glad to see your embryological ideas on plants; by the sentence which I sent you, you will see that I only want one sentence; if facts are at all, as I suppose, and I shall see this from your note, for sending which very many thanks.

I have been so poorly, the last three days, that I sometimes doubt whether I shall ever get my little volume done, though so nearly completed. . . .

C. Darwin to J. D. Hooker.

Down, March 15th [1859].

MY DEAR HOOKER,—I am *pleased* at what you say of my chapter. You have not attacked it nearly so much as I feared you would. You do not seem to have detected *many* errors. It was nearly all written from memory, and hence I was particularly fearful; it would have been better if the whole had first been carefully written out, and abstracted afterwards. I look at it as morally certain that it must include much error in some of its general views. I will just run over a few points in your note, but do not trouble yourself to reply without you have something important to say. . . .

. . . I should like to know whether the case of endemic bats in islands struck you; it has me especially; perhaps too strongly.

With hearty thanks, ever yours,

C. DARWIN.

P.S.—You cannot tell what a relief it has been to me your looking over this chapter, as I felt very shaky on it.

I shall to-morrow finish my last chapter (except a re-

© The Complete Work of Charles Darwin Online

capitulation) on Affinities, Homologies, Embryology, &c., and the facts seem to me to come out *very* strong for mutability of species.

I have been much interested in working out the chapter.

I shall now, thank God, begin looking over old first chapters for press.

But my health is now so very poor, that even this will take me long.

C. Darwin to W. D. Fox.

Down, [March] 24th [1859].

MY DEAR FOX,—It was very good of you to write to me in the midst of all your troubles, though you seem to have got over some of them, in the recovery of your wife's and your own health. I had not heard lately of your mother's health, and am sorry to hear so poor an account. But as she does not suffer much, that is the great thing; for mere life I do not think is much valued by the old. What a time you must have had of it, when you had to go backwards and forwards.

We are all pretty well, and our eldest daughter is improving. I can see daylight through my work, and am now finally correcting my chapters for the press; and I hope in a month or six weeks to have proof-sheets. I am weary of my work. It is a very odd thing that I have no sensation that I overwork my brain; but facts compel me to conclude that my brain was never formed for much thinking. We are resolved to go for two or three months, when I have finished, to Ilkley, or some such place, to see if I can anyhow give my health a good start, for it certainly has been wretched of late, and has incapacitated me for everything. You do me injustice when you think that I work for fame; I value it to a certain extent; but, if I know myself, I work from a sort of instinct to try to make out truth. How glad I should be if you could sometime come to Down; especially when I get a little better,

© The Complete Work of Charles Darwin Online

as I still hope to be. We have set up a billiard table, and I find it does me a deal of good, and drives the horrid species out of my head. Farewell, my dear old friend.

Yours affectionately,

C. DARWIN.

C. Darwin to C. Lyell.

Down, March 28th [1859].

MY DEAR LYELL,—If I keep decently well, I hope to be able to go to press with my volume early in May. This being so, I want much to beg a little advice from you. From an expression in Lady Lyell's note, I fancy that you have spoken to Murray. Is it so? And is he willing to publish my Abstract? If you will tell me whether anything, and what has passed, I will then write to him. Does he know at all of the subject of the book? Secondly, can you advise me, whether I had better state what terms of publication I should prefer, or first ask him to propose terms? And what do you think would be fair terms for an edition? Share profits, or what?

Lastly, will you be so very kind as to look at the enclosed title and give me your opinion and any criticisms; you must remember that, if I have health and it appears worth doing, I have a much larger and full book on the same subject nearly ready.

My Abstract will be about five hundred pages of the size of your first edition of the 'Elements of Geology.'

Pray forgive me troubling you with the above queries; and you shall have no more trouble on the subject. I hope the world goes well with you, and that you are getting on with your various works.

I am working very hard for me, and long to finish and be free and try to recover some health.

My dear Lyell, ever yours,

C. DARWIN.

© The Complete Work of Charles Darwin Online

Very sincere thanks to you for standing my proxy for the Wollaston Medal.

P.S.—Would you advise me to tell Murray that my book is not more *un*-orthodox than the subject makes inevitable. That I do not discuss the origin of man. That I do not bring in any discussion about Genesis, &c. &c., and only give facts, and such conclusions from them as seem to me fair.

Or had I better say *nothing* to Murray, and assume that he cannot object to this much unorthodoxy, which in fact is not more than any Geological Treatise which runs slap counter to Genesis.

Enclosure.

AN ABSTRACT OF AN ESSAY

ON THE

ORIGIN

OF

SPECIES AND VARIETIES

THROUGH NATURAL SELECTION

BY

CHARLES DARWIN, M.A.

FELLOW OF THE ROYAL, GEOLOGICAL, AND LINNEAN SOCIETIES

LONDON:

&c. &c. &c. &c.

1859.

C. Darwin to C. Lyell.

Down, March 30th [1859].

MY DEAR LYELL,—You have been uncommonly kind in all you have done. You not only have saved me much trouble and some anxiety, but have done all incomparably better than I could have done it. I am much pleased at all you say about Murray. I will write either to-day or to-morrow to him, and will send shortly a large bundle of

© The Complete Work of Charles Darwin Online

MS., but unfortunately I cannot for a week, as the first three chapters are in the copyists' hands.

I am sorry about Murray objecting to the term Abstract, as I look at it as the only possible apology for *not* giving references and facts in full, but I will defer to him and you. I am also sorry about the term "natural selection." I hope to retain it with explanation somewhat as thus :—

"Through natural selection, or the preservation of favoured races."

Why I like the term is that it is constantly used in all works on breeding, and I am surprised that it is not familiar to Murray; but I have so long studied such works that I have ceased to be a competent judge.

I again most truly and cordially thank you for your really valuable assistance. . . . .

Yours most truly,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, April 2nd [1859].

. . . . I wrote to him [Mr. Murray] and gave him the headings of the chapters, and told him he could not have the MS. for ten days or so; and this morning I received a letter, offering me handsome terms, and agreeing to publish without seeing the MS.! So he is eager enough; I think I should have been cautious, anyhow, but, owing to your letter, I told him most *explicitly* that I accept his offer solely on condition that, after he has seen part or all the MS., he has full power of retracting. You will think me presumptuous, but I think my book will be popular to a certain extent (enough to ensure [against] heavy loss) amongst scientific and semi-scientific men; why I think so is, because I have found in conversation so great and surprising an interest amongst such men, and some 0-scientific [non-scientific] men on this subject,

© The Complete Work of Charles Darwin Online

and all my chapters are not *nearly* so dry and dull as that which you have read on geographical distribution. Anyhow, Murray ought to be the best judge, and if he chooses to publish it, I think I may wash my hands of all responsibility. I am sure my friends, *i.e.* Lyell and you, have been *extraordinarily* kind in troubling yourselves on the matter.

I shall be delighted to see you the day before Good Friday; there would be one advantage for you in any other day—as I believe both my boys come home on that day—and it would be almost impossible that I could send the carriage for you. There will, I believe, be some relations in the house—but I hope you will not care for that, as we shall easily get as much talking as my *imbecile state* allows. I shall deeply enjoy seeing you.

. . . . I am tired, so no more.

My dear Hooker, your affectionate,

C. DARWIN.

P.S.—Please to send, well *tied up* with strong string, my Geographical MS., towards the latter half of next week—*i.e.* 7th or 8th—that I may send it with more to Murray; and God help him if he tries to read it.

. . . . I cannot help a little doubting whether Lyell would take much pains to induce Murray to publish my book; this was not done at my request, and it rather grates against my pride.

I know that Lyell has been *infinitely* kind about my affair, but your dashed [*i.e.* underlined] "*induce*" gives the idea that Lyell had unfairly urged Murray.

C. Darwin to Asa Gray.

April 4th [1859].

. . . . You ask to see my sheets as printed off; I assure you that it will be the *highest* satisfaction to me to do so: I look at the request as a high compliment. I shall not, you

© The Complete Work of Charles Darwin Online

may depend, forget a request which I look at as a favour. But (and it is a heavy "but" to me) it will be long before I go to press; I can truly say I am *never* idle; indeed, I work too hard for my much weakened health; yet I can do only three hours of work daily, and I cannot at all see when I shall have finished: I have done eleven long chapters, but I have got some other very difficult ones: as palæontology, classifications, and embryology, &c., and I have to correct and add largely to all those done. I find, alas! each chapter takes me on an average three months, so slow I am. There is no end to the necessary digressions. I have just finished a chapter on instinct, and here I found grappling with such a subject as bees' cells, and comparing all my notes made during twenty years, took up a despairing length of time.

But I am running on about myself in a most egotistical style. Yet I must just say how useful I have again and again found your letters, which I have lately been looking over and quoting! but you need not fear that I shall quote anything you would dislike, for I try to be very cautious on this head. I most heartily hope you may succeed in getting your "incubus" of old work off your hands, and be in some degree a free man. . . . .

Again let me say that I do indeed feel grateful to you . . .

C. Darwin to J. Murray.

Down, April 5th [1859].

MY DEAR SIR,—I send by this post, the Title (with some remarks on a separate page), and the first three chapters. If you have patience to read all Chapter I., I honestly think you will have a fair notion of the interest of the whole book. It may be conceit, but I believe the subject will interest the public, and I am sure that the views are original. If you think otherwise, I must repeat my request that you will freely

© The Complete Work of Charles Darwin Online

reject my work; and though I shall be a little disappointed, I shall be in no way injured.

If you choose to read Chapters II. and III., you will have a dull and rather abstruse chapter, and a plain and interesting one, in my opinion.

As soon as you have done with the MS., please to send it by *careful messenger, and plainly directed*, to Miss G. Tollett, 14, Queen Anne Street, Cavendish Square.

This lady, being an excellent judge of style, is going to look out for errors for me.

You must take your own time, but the sooner you finish, the sooner she will, and the sooner I shall get to press, which I so earnestly wish.

I presume you will wish to see Chapter IV., the key-stone of my arch, and Chapters X. and XI., but please to inform me on this head.

My dear Sir, yours sincerely,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, April 11th [1859].

. . . I write one line to say that I heard from Murray yesterday, and he says he has read the first three chapters of one MS. (and this includes a very dull one, and he abides by his offer). Hence he does not want more MS., and you can send my Geographical chapter when it pleases you. . . .

[Part of the MS. seems to have been lost on its way back to my father, he wrote (April 14) to Sir J. D. Hooker:

"I have the old MS., otherwise, the loss would have killed me! The worst is now that it will cause delay in getting to press, and *far worst* of all, I lose all advantage of your having looked over my chapter, except the third part returned. I am very sorry Mrs. Hooker took the trouble of copying the two pages."]

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

[April or May, 1859.]

. . . Please do not say to any one that I thought my book on Species would be fairly popular, and have a fairly remunerative sale (which was the height of my ambition), for if it prove a dead failure, it would make me the more ridiculous.

I enclose a criticism, a taste of the future—

*Rev. S. Haughton's Address to the Geological Society, Dublin.**

"This speculation of Messrs. Darwin and Wallace would not be worthy of notice were it not for the weight of authority of the names (*i.e.* Lyell's and yours), under whose auspices it has been brought forward. If it means what it says, it is a truism; if it means anything more, it is contrary to fact."

Q. E. D.

C. Darwin to J. D. Hooker.

Down, May 11th [1859].

MY DEAR HOOKER,—Thank you for telling me about obscurity of style. But on my life no nigger with lash over him could have worked harder at clearness than I have done. But the very difficulty to me, of itself leads to the probability that I fail. Yet one lady who has read all my MS. has found only two or three obscure sentences, but Mrs. Hooker having so found it, makes me tremble. I will do my best in proofs. You are a good man to take the trouble to write about it.

With respect to our mutual muddle,† I never for a moment

* Feb. 9, 1858.

† "When I go over the chapter I will see what I can do, but I hardly know how I am obscure, and I think we are somehow in a mutual muddle with respect to each other, from starting from some fundamentally different notions."—Letter of May 6, 1859.

© The Complete Work of Charles Darwin Online

thought we could not make our ideas clear to each other by talk, or if either of us had time to write in extenso.

I imagine from some expressions (but if you ask me what, I could not answer) that you look at variability as some necessary contingency with organisms, and further that there is some necessary tendency in the variability to go on diverging in character or degree. *If you do*, I do not agree. "Reversion" again (a form of inheritance), I look at as in no way directly connected with Variation, though of course inheritance is of fundamental importance to us, for if a variation be not inherited, it is of no signification to us. It was on such points as these *I fancied* that we perhaps started differently.

I fear that my book will not deserve at all the pleasant things you say about it; and Good Lord, how I do long to have done with it!

Since the above was written, I have received and have been *much interested* by A. Gray. I am delighted at his note about my and Wallace's paper. He will go round, for it is futile to give up very many species, and stop at an arbitrary line at others. It is what my grandfather called Unitarianism, "a feather bed to catch a falling Christian."...

C. Darwin to J. D. Hooker.

Down, May 18th [1859].

MY DEAR HOOKER,—My health has quite failed. I am off to-morrow for a week of Hydropathy. I am very very sorry to say that I cannot look over any proofs * in the week, as my object is to drive the subject out of my head. I shall return to-morrow week. If it be worth while, which probably it is not, you could keep back any proofs till my return home.

In haste, ever yours,

C. DARWIN.

* Of Sir J. D. Hooker's Introduction to the 'Flora of Australia.'

© The Complete Work of Charles Darwin Online

[Ten days later he wrote to Sir J. D. Hooker:

"... I write one word to say that I shall return on Saturday, and if you have any proof-sheets to send, I shall be glad to do my best in any criticisms.

I had ... great prostration of mind and body, but entire rest, and the douche, and 'Adam Bede,' have together done me a world of good."]

C. Darwin to J. Murray.

Down, June 14th [1859].

MY DEAR SIR,—The diagram will do very well, and I will send it shortly to Mr. West to have a few trifling corrections made.

I get on very slowly with proofs. I remember writing to you that I thought there would be not much correction. I honestly wrote what I thought, but was most grievously mistaken. I find the style incredibly bad, and most difficult to make clear and smooth. I am extremely sorry to say, on account of expense, and loss of time for me, that the corrections are very heavy, as heavy as possible. But from casual glances, I still hope that later chapters are not so badly written. How I could have written so badly is quite inconceivable, but I suppose it was owing to my whole attention being fixed on the general line of argument, and not on details. All I can say is, that I am very sorry.

Yours very sincerely,

C. DARWIN.

P.S.—I have been looking at the corrections, and considering them. It seems to me that I shall put you to a quite unfair expense. If you please I should like to enter into some such arrangement as the following: When work completed, you to allow in the account a fairly moderately heavy charge for corrections, and all excess over that to be deducted from my profits, or paid by me individually.

© The Complete Work of Charles Darwin Online

C. Darwin to C. Lyell.

Down, June 21st [1859].

. . . I am working very hard, but get on slowly, for I find that my corrections are terrifically heavy, and the work most difficult to me. I have corrected 130 pages, and the volume will be about 500. I have tried my best to make it clear and striking, but very much fear that I have failed—so many discussions are and must be very perplexing. I have done my best. If you had all my materials, I am sure you would have made a splendid book. I long to finish, for I am nearly worn out.

My dear Lyell, ever yours most truly,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down, 22nd [June, 1859].

MY DEAR HOOKER,—I did not answer your pleasant note, with a good deal of news to me, of May 30th, as I have been expecting proofs from you. But now, having nothing particular to do, I will fly a note, though I have nothing particular to say or ask. Indeed, how can a man have anything to say, who spends every day in correcting accursed proofs; and such proofs! I have fairly to blacken them, and fasten slips of paper on, so miserable have I found the style. You say that you dreamt that my book was *entertaining;* that dream is pretty well over with me, and I begin to fear that the public will find it intolerably dry and perplexing. But I will never give up that a better man could have made a splendid book out of the materials. I was glad to hear about Prestwich's paper.* My doubt has been (and I see Wright

* Mr. Prestwich wrote on the occurrence of flint instruments associated with the remains of extinct animals in France.—Proc. R. Soc., 1859.

© The Complete Work of Charles Darwin Online

has inserted the same in the 'Athenæum') whether the pieces of flint are really tools; their numbers make me doubt, and when I formerly looked at Boucher de Perthe's drawings, I came to the conclusion that they were angular fragments broken by ice action.

Did crossing the Acacia do any good? I am so hard worked, that I can make no experiments. I have got only to 150 pages in first proof.

Adios, my dear Hooker, ever yours,

C. DARWIN.

C. Darwin to J. Murray.

Down, July 25th [1859].

MY DEAR SIR,—I write to say that five sheets are returned to the printers ready to strike off, and two more sheets require only a revise; so that I presume you will soon have to decide what number of copies to print off.

I am quite incapable of forming any opinion. I think I have got the style *fairly* good and clear, with infinite trouble. But whether the book will be successful to a degree to satisfy you, I really cannot conjecture. I heartily hope it may.

My dear Sir, yours very sincerely,

C. DARWIN.

C. Darwin to A. R. Wallace.

Down, Aug. 9th, 1859.

MY DEAR MR. WALLACE,—I received your letter and memoir* on the 7th, and will forward it to-morrow to the Linnean Society. But you will be aware that there is no meeting till the beginning of November. Your paper seems to me *admirable* in matter, style, and reasoning; and I thank

* This seems to refer to Mr. Wallace's paper, "On the Zoological Geography of the Malay Archipelago," 'Linn. Soc. Journ.,' 1860.

© The Complete Work of Charles Darwin Online

you for allowing me to read it. Had I read it some months ago, I should have profited by it for my forthcoming volume. But my two chapters on this subject are in type, and, though not yet corrected, I am so wearied out and weak in health, that I am fully resolved not to add one word, and merely improve the style. So you will see that my views are nearly the same with yours, and you may rely on it that not one word shall be altered owing to my having read your ideas. Are you aware that Mr. W. Earl* [*sic*] published several years ago the view of distribution of animals in the Malay Archipelago, in relation to the depth of the sea between the islands? I was much struck with this, and have been in the habit of noting all facts in distribution in that archipelago, and elsewhere, in this relation. I have been led to conclude that there has been a good deal of naturalisation in the different Malay islands, and which I have thought, to a certain extent, would account for anomalies. Timor has been my greatest puzzle. What do you say to the peculiar Felis there? I wish that you had visited Timor; it has been asserted that a fossil mastodon's or elephant's tooth (I forget which) has been found there, which would be a grand fact. I was aware that Celebes was very peculiar; but the relation to Africa is quite new to me, and marvellous, and almost passes belief. It is as anomalous as the relation of *plants* in S.W. Australia to the Cape of Good Hope. I differ *wholly* from you on the colonisation of oceanic islands, but you will have *every one* else on your side. I quite agree with respect to all islands not situated far in the ocean. I quite agree on the little occasional intermigration between lands [islands?] when once pretty well stocked with inhabitants, but think this does not apply to rising and ill-stocked islands. Are you aware that *annually* birds are blown to Madeira, the Azores (and to Bermuda from America)? I wish I had given a fuller abstract of my reasons for not believing in Forbes's great continental

* Probably Mr. W. Earle's paper, Geographical Soc. Journal, 1845.

© The Complete Work of Charles Darwin Online

extensions; but it is too late, for I will alter nothing—I am worn out, and must have rest. Owen, I do not doubt, will bitterly oppose us. . . . Hooker is publishing a grand Introduction to the Flora of Australia, and goes the whole length. I have seen proofs of about half. With every good wish.

Believe me, yours very sincerely,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Sept. 1st [1859].

. . . I am not surprised at your finding your Introduction very difficult. But do not grudge the labour, and do not say you "have burnt your fingers," and are "deep in the mud"; for I feel sure that the result will be well worth the labour. Unless I am a fool, I must be a judge to some extent of the value of such general essays, and I am fully convinced that yours are the most valuable ever published.

I have corrected all but the last two chapters of my book, and hope to have done revises and all in about three weeks, and then I (or we all) shall start for some months' hydropathy; my health has been very bad, and I am becoming as weak as a child, and incapable of doing anything whatever, except my three hours daily work at proof-sheets. God knows whether I shall ever be good for anything again, perhaps a long rest and hydropathy may do something.

I have not had A. Gray's Essay, and should not feel up to criticise it, even if I had the impertinence and courage. You will believe me that I speak strictly the truth when I say that your Australian Essay is *extremely* interesting to me, rather too much so. I enjoy reading it over, and if you think my criticisms are worth anything to you, I beg you to send the sheets (if you can give me time for good days); but unless I can render you any little, however little assistance,

© The Complete Work of Charles Darwin Online

I would rather read the essay when published. Pray understand that I should be *truly* vexed not to read them, if you wish it for your own sake.

I had a terribly long fit of sickness yesterday, which makes the world rather extra gloomy to-day, and I have an insanely strong wish to finish my accursed book, such corrections every page has required as I never saw before. It is so weariful, killing the whole afternoon, after 12 o'clock doing nothing whatever. But I will grumble no more. So farewell, we shall meet in the winter I trust.

Farewell, my dear Hooker, your affectionate friend,

C. DARWIN.

C. Darwin to C. Lyell.

Down, Sept. 2nd [1859].

. . . I am very glad you wish to see my clean sheets: I should have offered them, but did not know whether it would bore you; I wrote by this morning's post to Murray to send them. Unfortunately I have not got to the part which will interest you, I think most, and which tells most in favour of the view, viz. Geological Succession, Geographical Distribution, and especially Morphology, Embryology and Rudimentary Organs. I will see that the remaining sheets, when printed off, are sent to you. But would you like for me to send the last and perfect revises of the sheets as I correct them? if so, send me your address in a blank envelope. I hope that you will read all, whether dull (especially latter part of Chapter II.) or not, for I am convinced there is not a sentence which has not a bearing on the whole argument. You will find Chapter IV. perplexing and unintelligible, without the aid of the enclosed queer diagram,* of which I send an old and useless proof. I have, as Murray says, corrected so heavily, as almost to have re-written it; but yet I fear it is poorly written. Parts are

* The diagram illustrates descent with divergence.

© The Complete Work of Charles Darwin Online

intricate; and I do not think that even you could make them quite clear. Do not, I beg, be in a hurry in committing yourself (like so many naturalists) to go a certain length and no further; for I am deeply convinced that it is absolutely necessary to go the whole vast length, or stick to the creation of each separate species; I argue this point briefly in the last chapter. Remember that your verdict will probably have more influence than my book in deciding whether such views as I hold will be admitted or rejected at present; in the future I cannot doubt about their admittance, and our posterity will marvel as much about the current belief as we do about fossil shells having been thought to have been created as we now see them. But forgive me for running on about my hobby-horse. . . .

C. Darwin to J. D. Hooker.

Down, [Sept.] 11th [1859].

MY DEAR HOOKER,—I corrected the last proof yesterday, and I have now my revises, index, &c., which will take me near to the end of the month. So that the neck of my work, thank God, is broken.

I write now to say that I am uneasy in my conscience about hesitating to look over your proofs, but I was feeling miserably unwell and shattered when I wrote. I do not suppose I could be of hardly any use, but if I could, pray send me any proofs. I should be (and fear I was) the most ungrateful man to hesitate to do anything for you after some fifteen or more years' help from you.

As soon as ever I have fairly finished I shall be off to Ilkley, or some other Hydropathic establishment. But I shall be some time yet, as my proofs have been so utterly obscured with corrections, that I have to correct heavily on revises.

Murray proposes to publish the first week in November. Oh, good heavens, the relief to my head and body to banish the whole subject from my mind!

© The Complete Work of Charles Darwin Online

I hope to God, you do not think me a brute about your proof-sheets.

Farewell, yours affectionately,
C. DARWIN.

C. Darwin to C. Lyell.

Down, Sept. 20th [1859].

MY DEAR LYELL.—You once gave me intense pleasure, or rather delight, by the way you were interested, in a manner I never expected, in my Coral Reef notions, and now you have again given me similar pleasure by the manner you have noticed my species work.* Nothing could be more satisfactory to me, and I thank you for myself, and even more for the subject's sake, as I know well that the sentence will make many fairly consider the subject, instead of ridiculing it. Although your previously felt doubts on the immutability of species, may have more influence in converting you (if you be converted) than my book; yet as I regard your verdict as far more important in my own eyes, and I believe in the eyes of the world than of any other dozen men, I am naturally very anxious about it. Therefore let me beg you to keep your mind open till you receive (in perhaps a fortnight's time) my latter chapters, which are the most

* Sir Charles was President of the Geological section at the meeting of the British Association at Aberdeen in 1859. The following passage occurs in the address: "On this difficult and mysterious subject a work will very shortly appear by Mr. Charles Darwin, the result of twenty years of observations and experiments in Zoology, Botany, and Geology, by which he has been led to the conclusion that those powers of nature which give rise to races and permanent varieties in animals and plants, are the same as those which in much longer periods produce species, and in a still longer series of ages give rise to differences of generic rank. He appears to me to have succeeded by his investigations and reasonings in throwing a flood of light on many classes of phenomena connected with the affinities, geographical distribution, and geological succession of organic beings, for which no other hypothesis has been able, or has even attempted to account."

© The Complete Work of Charles Darwin Online

important of all on the favourable side. The last chapter, which sums up, and balances in a mass, all the arguments contra and pro, will, I think, be useful to you. I cannot too strongly express my conviction of the general truth of my doctrines, and God knows I have never shirked a difficulty. I am foolishly anxious for your verdict, not that I shall be disappointed if you are not converted; for I remember the long years it took me to come round; but I shall be most deeply delighted if you do come round, especially if I have a fair share in the conversion, I shall then feel that my career is run, and care little whether I ever am good for anything again in this life.

Thank you much for allowing me to put in the sentence about your grave doubt.* So much and too much about myself.

I have read with extreme interest in the Aberdeen paper about the flint tools; you have made the whole case far clearer to me; I suppose that you did not think the evidence sufficient about the Glacial period.

With cordial thanks for your splendid notice of my book.

Believe me, my dear Lyell, your affectionate disciple,

CHARLES DARWIN.

C. Darwin to W. D. Fox.

Down, Sept. 23rd [1859].

MY DEAR FOX,—I was very glad to get your letter a few days ago. I was wishing to hear about you, but have been in such an absorbed, slavish, overworked state, that I had not heart without compulsion to write to any one or do anything beyond my daily work. Though your account of yourself is better, I cannot think it at all satisfactory, and I wish you would soon go to Malvern again. My father used to believe largely in an old saying that, if a man grew thinner between

* As to the immutability of species, 'Origin,' ed. i., p. 310.

© The Complete Work of Charles Darwin Online

fifty and sixty years of age, his chance of long life was poor, and that on the contrary it was à very good sign if he grew fatter; so that your stoutness, I look at as a very good omen. My health has been as bad as it well could be all this summer; and I have kept on my legs, only by going at short intervals to Moor Park; but I have been better lately, and, thank Heaven, I have at last as good as done my book, having only the index and two or three revises to do. It will be published in the first week in November, and a copy shall be sent you. Remember it is only an Abstract (but has cost me above thirteen months to write!!), and facts and authorities are far from given in full. I shall be curious to hear what you think of it, but I am not so silly as to expect to convert you. Lyell has read about half of the volume in clean sheets, and gives me very great *kudos*. He is wavering so much about the immutability of species, that I expect he will come round. Hooker has come round, and will publish his belief soon. So much for my abominable volume, which has cost me so much labour that I almost hate it. On October 3rd I start for Ilkley, but shall take three days for the journey! It is so late that we shall not take a house; but I go there alone for three or four weeks; then return home for a week and go to Moor Park for three or four weeks, and then I shall get a moderate spell of hydropathy; and I intend, if I can keep to my resolution, of being idle this winter. But I fear *ennui* will be as bad as a bad stomach. . . .

C. Darwin to C. Lyell.

Down, Sept. 25th [1859].

MY DEAR LYELL,—I send by this post four corrected sheets. I have altered the sentence about the Eocene fauna being beaten by recent, thanks to your remark. But I imagined that it would have been clear that I supposed the climate to be nearly similar; you do not doubt, I imagine, that the climate

© The Complete Work of Charles Darwin Online

of the Eocene and recent periods in *different* parts of the world could be matched. Not that I think climate nearly so important as most naturalists seem to think. In my opinion no error is more mischievous than this.

I was very glad to find that Hooker, who read over, in MS., my Geographical chapters, quite agreed in the view of the greater importance of organic relations. I should like you to consider p. 77 and reflect on the case of any organism in the midst of its range.

I shall be curious hereafter to hear what you think of distribution during the glacial and preceding warmer periods. I am so glad you do not think the Chapter on the Imperfection of the Geological Record exaggerated; I was more fearful about this chapter than about any part.

Embryology in Chapter VIII. is one of my strongest points I think. But I must not bore you by running on. My mind is so wearisomely full of the subject.

I do thank you for your eulogy at Aberdeen. I have been so wearied and exhausted of late that I have for months doubted whether I have not been throwing away time and labour for nothing. But now I care not what the universal world says; I have always found you right, and certainly on this occasion I am not going to doubt for the first time. Whether you go far, or but a very short way with me and others who believe as I do, I am contented, for my work cannot be in vain. You would laugh if you knew how often I have read your paragraph, and it has acted like a little dram. . . .

Farewell,

C. DARWIN.

C. Darwin to C. Lyell.

Down, Sept. 30th [1859].

MY DEAR LYELL,—I sent off this morning the last sheets, but without index, which is not in type. I look at you as my Lord High Chancellor in Natural Science, and therefore

© The Complete Work of Charles Darwin Online

I request you, after you have finished, just to *re-run* over the heads in the recapitulation-part of last chapter. I shall be deeply anxious to hear what you decide (if you are able to decide) on the balance of the pros and contras given in my volume, and of such other pros and contras as may occur to you. I hope that you will think that I have given the difficulties fairly. I feel an entire conviction that if you are now staggered to any moderate extent, you will come more and more round, the longer you keep the subject at all before your mind. I remember well how many long years it was before I could look into the face of some of the difficulties and not feel quite abashed. I fairly struck my colours before the case of neuter insects.

I suppose that I am a very slow thinker, for you would be surprised at the number of years it took me to see clearly what some of the problems were which had to be solved, such as the necessity of the principle of divergence of character, the extinction of intermediate varieties, on a continuous area, with graduated conditions; the double problem of sterile first crosses and sterile hybrids, &c. &c.

Looking back, I think it was more difficult to see what the problems were than to solve them, so far as I have succeeded in doing, and this seems to me rather curious. Well, good or bad, my work, thank God, is over; and hard work, I can assure you, I have had, and much work which has never borne fruit. You can see, by the way I am scribbling, that I have an idle and rainy afternoon. I was not able to start for Ilkley yesterday as I was too unwell; but I hope to get there on Tuesday or Wednesday. Do, I beg you, when you have finished my book and thought a little over it, let me hear from you. Never mind and pitch into me, if you think it requisite; some future day, in London possibly, you may give me a few criticisms in detail, that is, if you have scribbled any remarks on the margin, for the chance of a second edition.

© The Complete Work of Charles Darwin Online

Murray has printed 1250 copies, which seems to me rather too large an edition, but I hope he will not lose.

I make as much fuss about my book as if it were my first. Forgive me, and believe me, my dear Lyell,

Yours most sincerely,

C. DARWIN.

C. Darwin to J. D. Hooker.

Ilkley, Yorkshire, Oct. 15th [1859].

MY DEAR HOOKER,—Be a good man and screw out time enough to write me a note and tell me a little about yourself, your doings, and belongings.

Is your Introduction fairly finished? I know you will abuse it, and I know well how much I shall like it. I have been here nearly a fortnight, and it has done me very much good, though I sprained my ankle last Sunday, which has quite stopped walking. All my family come here on Monday to stop three or four weeks, and then I shall go back to the great establishment, and stay a fortnight; so that if I can keep my spirits, I shall stay eight weeks here, and thus give hydropathy a fair chance. Before starting here I was in an awful state of stomach, strength, temper, and spirits. My book has been completely finished some little time; as soon as copies are ready, of course one will be sent you. I hope you will mark your copy with scores, so that I may profit by any criticisms. I should like to hear your general impression. From Lyell's letters, he thinks favourably of it, but seems staggered by the lengths to which I go. But if you go any considerable length in the admission of modification, I can see no possible means of drawing the line, and saying here you must stop. Lyell is going to reread my book, and I yet entertain hopes that he will be converted, or perverted, as he calls it. Lyell has been *extremely* kind in writing me three volume-like letters; but he says nothing about dispersal during the

© The Complete Work of Charles Darwin Online

Glacial period. I should like to know what he thinks on this head. I have one question to ask: Would it be any good to send a copy of my book to Decaisne? and do you know any philosophical botanists on the Continent, who read English and care for such subjects? if so, give me their addresses. How about Andersson in Sweden? You cannot think how refreshing it is to idle away the whole day, and hardly ever think in the least about my confounded book which half-killed me. I much wish I could hear of your taking a real rest. I know how very strong you are mentally, but I never will believe you can go on working as you have worked of late with impunity. You will some day stretch the string too tight. Farewell, my good, and kind, and dear friend,

Yours affectionately,

C. DARWIN.

C. Darwin to T. H. Huxley.

Ilkley, Yorkshire, Oct. 15th [1859].

MY DEAR HUXLEY, — I am here hydropathising and coming to life again, after having finished my accursed book, which would have been easy work to any one else, but half-killed me. I have thought you would give me one bit of information, and I know not to whom else to apply; viz., the addresses of Barrande, Von Siebold, Keyserling (I dare say Sir Roderick would know the latter).

Can you tell me of any good and *speculative* foreigners to whom it would be worth while to send copies of my book, on the 'Origin of Species'? I doubt whether it is worth sending to Siebold. I should like to send a few copies about, but how many I can afford I know not yet till I hear what price Murray affixes.

I need not say that I will send, of course, one to you, in the first week of November. I hope to send copies abroad immediately. I shall be *intensely* curious to hear what effect

© The Complete Work of Charles Darwin Online

the book produces on you. I know that there will be much in it which you will object to, and I do not doubt many errors. I am very far from expecting to convert you to many of my heresies; but if, on the whole, you and two or three others think I am on the right road, I shall not care what the mob of naturalists think. The penultimate chapter,* though I believe it includes the truth, will, I much fear, make you savage. Do not act and say, like Macleay versus Fleming, "I write with aqua fortis to bite into brass."

Ever yours,

C. DARWIN.

C. Darwin to C. Lyell.

Ilkley, Yorkshire.
Oct. 20th [1859].

MY DEAR LYELL,—I have been reading over all your letters consecutively, and I do not feel that I have thanked you half enough for the extreme pleasure which they have given me, and for their utility. I see in them evidence of fluctuation in the degree of credence you give to the theory; nor am I at all surprised at this, for many and many fluctuations I have undergone.

There is one point in your letter which I did not notice, about the animals (and many plants) naturalised in Australia, which you think could not endure without man's aid. I cannot see how man does aid the feral cattle. But, letting that pass, you seem to think, that because they suffer prodigious destruction during droughts, they would all be destroyed. In the "grandes secos" of La Plata, the indigenous animals, such as the American deer, die by thousands, and suffer apparently as much as the cattle. In parts of India, after a drought, it takes ten or more years before the indigenous mammals get

* Chapter XIII. is on Classification, Morphology, Embryology, and Rudimentary Organs.

© The Complete Work of Charles Darwin Online

up to their full number again. Your argument would, I think, apply to the aborigines as well as to the feral.

An animal or plant which becomes feral in one small territory might be destroyed by climate, but I can hardly believe so, when once feral over several large territories. Again, I feel inclined to swear at climate: do not think me impudent for attacking you about climate. You say you doubt whether man could have existed under the Eocene climate, but man can now withstand the climate of Esquimaux-land and West Equatorial Africa; and surely you do not think the Eocene climate differed from the present throughout all Europe, as much as the Arctic regions differ from Equatorial Africa?

With respect to organisms being created on the American type in America, it might, I think, be said that they were so created to prevent them being too well created, so as to beat the aborigines; but this seems to me, somehow, a monstrous doctrine.

I have reflected a good deal on what you say on the necessity of continued intervention of creative power. I cannot see this necessity; and its admission, I think, would make the theory of Natural Selection valueless. Grant a simple Archetypal creature, like the Mud-fish or Lepidosiren, with the five senses and some vestige of mind, and I believe natural selection will account for the production of every vertebrate animal.

Farewell; forgive me for indulging in this prose, and believe me, with cordial thanks,

Your ever attached disciple,

C. DARWIN.

P.S.—When, and if, you reread, I supplicate you to write on the margin the word "expand," when too condensed, or "not clear," or "?". Such marks would cost you little trouble, and I could copy them and reflect on them, and their value would be infinite to me.

© The Complete Work of Charles Darwin Online

My larger book will have to be wholly re-written, and not merely the present volume expanded; so that I want to waste as little time over this volume as possible, if another edition be called for; but I fear the subject will be too perplexing, as I have treated it, for general public.

C. Darwin to J. D. Hooker.

Ilkley, Yorkshire.
Sunday [Oct. 23rd, 1859].

MY DEAR HOOKER,—I congratulate you on your 'Introduction' * being in fact finished. I am sure from what I read of it (and deeply I shall be interested in reading it straight through), that it must have cost you a prodigious amount of labour and thought. I shall like very much to see the sheet, which you wish me to look at. Now I am so completely a gentleman, that I have sometimes a little difficulty to pass the day; but it is astonishing how idle a three weeks I have passed. If it is any comfort to you, pray delude yourself by saying that you intend "sticking to humdrum science." But I believe it just as much as if a plant were to say that, "I have been growing all my life, and, by Jove, I will stop growing." You cannot help yourself; you are not clever enough for that. You could not even remain idle, as I have done, for three weeks! What you say about Lyell pleases me exceedingly; I had not at all inferred from his letters that he had come so much round. I remember thinking, above a year ago, that if ever I lived to see Lyell, yourself, and Huxley come round, partly by my book, and partly by their own reflections, I should feel that the subject is safe, and all the world might rail, but that ultimately the theory of Natural Selection (though, no doubt, imperfect in its present condition, and embracing many errors) would prevail. Nothing will ever convince me that three such men, with so much diversified

* 'Australian Flora.'

© The Complete Work of Charles Darwin Online

knowledge, and so well accustomed to search for truth, could err greatly. I have spoken of you here as a convert made by me; but I know well how much larger the share has been of your own self-thought. I am intensely curious to hear Huxley's opinion of my book. I fear my long discussion on Classification will disgust him; for it is much opposed to what he once said to me.

But, how I am running on! You see how idle I am; but I have so enjoyed your letter that you must forgive me. With respect to migration during the Glacial period: I think Lyell quite comprehends, for he has given me a supporting fact. But, perhaps, he unconsciously hates (do not say so to him) the view, as slightly staggering him on his favourite theory of all changes of climate being due to changes in the relative position of land and water.

I will send copies of my book to all the men specified by you; . . . would you be so kind as to add title, as Doctor, or Professor, or Monsieur, or Von, and initials (when wanted), and addresses to the names on the enclosed list, and let me have it pretty *soon*, as towards the close of this week Murray says the copies to go abroad will be ready. I am anxious to get my view generally known, and not, I hope and think, for mere personal conceit. . . . .

C. Darwin to C. Lyell.

Ilkley, Yorkshire, Oct. 25th [1859].

. . . Our difference on "principle of improvement" and "power of adaptation" is too profound for discussion by letter. If I am wrong, I am quite blind to my error. If I am right, our difference will be got over only by your re-reading carefully and reflecting on my first four chapters. I supplicate you to read these again carefully. The so-called improvement of our Shorthorn cattle, pigeons, &c., does not presuppose or require any aboriginal "power of adaptation," or "principle of improvement;" it requires only diversified

© The Complete Work of Charles Darwin Online

variability, and man to select or take advantage of those modifications which are useful to him; so under nature any slight modification which *chances* to arise, and is useful to any creature, is selected or preserved in the struggle for life; any modification which is injurious is destroyed or rejected; any which is neither useful nor injurious will be left a fluctuating element. When you contrast natural selection and "improvement," you seem always to overlook (for I do not see how you can deny) that every step in the natural selection of each species implies improvement in that species in relation to its conditions of life. No modification can be selected without it be an improvement or advantage. Improvement implies, I suppose, each form obtaining many parts or organs, all excellently adapted for their functions. As each species is improved, and as the number of forms will have increased, if we look to the whole course of time, the organic condition of life for other forms will become more complex, and there will be a necessity for other forms to become improved, or they will be exterminated; and I can see no limit to this process of improvement, without the intervention of any other and direct principle of improvement. All this seems to me quite compatible with certain forms fitted for simple conditions, remaining unaltered, or being degraded.

If I have a second edition, I will reiterate "Natural Selection, and as a general consequence, Natural Improvement."

As you go, as far as you do, I begin strongly to think, judging from myself, that you will go much further. How slowly the older geologists admitted your grand views on existing geological causes of change!

If at any time you think I can answer any question, it is a real pleasure to me to write.

Yours affectionately,
C. DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. Murray.

Ilkley, Yorkshire [1859].

MY DEAR SIR,—I have received your kind note and the copy; I am infinitely pleased and proud at the appearance of my child.

I quite agree to all you propose about price. But you are really too generous about the, to me, scandalously heavy corrections. Are you not acting unfairly towards yourself? Would it not be better at least to share the £72 8*s.*? I shall be fully satisfied, for I had no business to send, though quite unintentionally and unexpectedly, such badly composed MS. to the printers.

Thank you for your kind offer to distribute the copies to my friends and assisters as soon as possible. Do not trouble yourself much about the foreigners, as Messrs. Williams and Norgate have most kindly offered to do their best, and they are accustomed to send to all parts of the world.

I will pay for my copies whenever you like. I am so glad that you were so good as to undertake the publication of my book.

My dear Sir, yours very sincerely,

CHARLES DARWIN.

P.S.—Please do not forget to let me hear about two days before the copies are distributed.

I do not know when I shall leave this place, certainly not for several weeks. Whenever I am in London I will call on you.

© The Complete Work of Charles Darwin Online

CHAPTER V.

BY PROFESSOR HUXLEY.

ON THE RECEPTION OF THE 'ORIGIN OF SPECIES.'

TO the present generation, that is to say, the people a few years on the hither and thither side of thirty, the name of Charles Darwin stands alongside of those of Isaac Newton and Michael Faraday; and, like them, calls up the grand ideal of a searcher after truth and interpreter of Nature. They think of him who bore it as a rare combination of genius, industry, and unswerving veracity, who earned his place among the most famous men of the age by sheer native power, in the teeth of a gale of popular prejudice, and uncheered by a sign of favour or appreciation from the official fountains of honour; as one who, in spite of an acute sensitiveness to praise and blame, and notwithstanding provocations which might have excused any outbreak, kept himself clear of all envy, hatred, and malice, nor dealt otherwise than fairly and justly with the unfairness and injustice which was showered upon him; while, to the end of his days, he was ready to listen with patience and respect to the most insignificant of reasonable objectors.

And with respect to that theory of the origin of the forms of life peopling our globe, with which Darwin's name is bound up as closely as that of Newton with the theory of gravitation, nothing seems to be further from the mind of the present generation than any attempt to smother it with ridicule or to crush it by vehemence of denunciation. "The struggle for

© The Complete Work of Charles Darwin Online

existence," and "Natural selection," have become household words and every-day conceptions. The reality and the importance of the natural processes on which Darwin founds his deductions are no more doubted than those of growth and multiplication; and, whether the full potency attributed to them is admitted or not, no one doubts their vast and far-reaching significance. Wherever the biological sciences are studied, the 'Origin of Species' lights the path of the investigator; wherever they are taught it permeates the course of instruction. Nor has the influence of Darwinian ideas been less profound, beyond the realms of Biology. The oldest of all philosophies, that of Evolution, was bound hand and foot and cast into utter darkness during the millennium of theological scholasticism. But Darwin poured new life-blood into the ancient frame; the bonds burst, and the revivified thought of ancient Greece has proved itself to be a more adequate expression of the universal order of things than any of the schemes which have been accepted by the credulity and welcomed by the superstition of seventy later generations of men.

To any one who studies the signs of the times, the emergence of the philosophy of Evolution, in the attitude of claimant to the throne of the world of thought, from the limbo of hated and, as many hoped, forgotten things, is the most portentous event of the nineteenth century. But the most effective weapons of the modern champions of Evolution were fabricated by Darwin; and the 'Origin of Species' has enlisted a formidable body of combatants, trained in the severe school of Physical Science, whose ears might have long remained deaf to the speculations of *a priori* philosophers.

I do not think that any candid or instructed person will deny the truth of that which has just been asserted. He may hate the very name of Evolution, and may deny its pretensions as vehemently as a Jacobite denied those of George the Second. But there it is—not only as solidly seated as the Hanoverian

© The Complete Work of Charles Darwin Online

dynasty, but happily independent of Parliamentary sanction—and the dullest antagonists have come to see that they have to deal with an adversary whose bones are to be broken by no amount of bad words.

Even the theologians have almost ceased to pit the plain meaning of Genesis against the no less plain meaning of Nature. Their more candid, or more cautious, representatives have given up dealing with Evolution as if it were a damnable heresy, and have taken refuge in one of two courses. Either they deny that Genesis was meant to teach scientific truth, and thus save the veracity of the record at the expense of its authority; or they expend their energies in devising the cruel ingenuities of the reconciler, and torture texts in the vain hope of making them confess the creed of Science. But when the *peine forte et dure* is over, the antique sincerity of the venerable sufferer always reasserts itself. Genesis is honest to the core, and professes to be no more than it is, a repository of venerable traditions of unknown origin, claiming no scientific authority and possessing none.

As my pen finishes these passages, I can but be amused to think what a terrible hubbub would have been made (in truth was made) about any similar expressions of opinion a quarter of a century ago. In fact, the contrast between the present condition of public opinion upon the Darwinian question; between the estimation in which Darwin's views are now held in the scientific world; between the acquiescence, or at least quiescence, of the theologians of the self-respecting order at the present day and the outburst of antagonism on all sides in 1858–9, when the new theory respecting the origin of species first became known to the older generation to which I belong, is so startling that, except for documentary evidence, I should be sometimes inclined to think my memories dreams. I have a great respect for the younger generation myself (they can write our lives, and ravel out all our follies, if they choose to take the

© The Complete Work of Charles Darwin Online

trouble, by and by), and I should be glad to be assured that the feeling is reciprocal; but I am afraid that the story of our dealings with Darwin may prove a great hindrance to that veneration for our wisdom which I should like them to display. We have not even the excuse that, thirty years ago, Mr. Darwin was an obscure novice, who had no claims on our attention. On the contrary, his remarkable zoological and geological investigations had long given him an assured position among the most eminent and original investigators of the day; while his charming 'Voyage of a Naturalist' had justly earned him a wide-spread reputation among the general public. I doubt if there was any man then living who had a better right to expect that anything he might choose to say on such a question as the Origin of Species would be listened to with profound attention, and discussed with respect; and there was certainly no man whose personal character should have afforded a better safeguard against attacks, instinct with malignity and spiced with shameless impertinences.

Yet such was the portion of one of the kindest and truest men that it was ever my good fortune to know; and years had to pass away before misrepresentation, ridicule, and denunciation, ceased to be the most notable constituents of the majority of the multitudinous criticisms of his work which poured from the press. I am loth to rake any of these ancient scandals from their well-deserved oblivion; but I must make good a statement which may seem overcharged to the present generation, and there is no *pièce justificative* more apt for the purpose, or more worthy of such dishonour, than the article in the 'Quarterly Review' for July 1860.* Since Lord Brougham

* I was not aware when I wrote these passages that the authorship of the article had been publicly acknowledged. Confession unaccompanied by penitence, however, affords no ground for mitigation of judgment; and the kindliness with which Mr. Darwin speaks of his assailant, Bishop Wilberforce (Vol. II. pp. 325, 329, 332), is so striking an exemplification of his singular gentleness and modesty, that it rather increases one's indignation against the presumption of his critic.

© The Complete Work of Charles Darwin Online

assailed Dr. Young, the world has seen no such specimen of the insolence of a shallow pretender to a Master in Science as this remarkable production, in which one of the most exact of observers, most cautious of reasoners, and most candid of expositors, of this or any other age, is held up to scorn as a "flighty" person, who endeavours "to prop up his utterly rotten fabric of guess and speculation," and whose "mode of dealing with nature" is reprobated as "utterly dishonourable to Natural Science." And all this high and mighty talk, which would have been indecent in one of Mr. Darwin's equals, proceeds from a writer whose want of intelligence, or of conscience, or of both, is so great, that, by way of an objection to Mr. Darwin's views, he can ask, "Is it credible that all favourable varieties of turnips are tending to become men;" who is so ignorant of paleontology, that he can talk of the "flowers and fruits" of the plants of the carboniferous epoch; of comparative anatomy, that he can gravely affirm the poison apparatus of the venomous snakes to be "entirely separate from the ordinary laws of animal life, and peculiar to themselves;" of the rudiments of physiology, that he can ask, "what advantage of life could alter the shape of the corpuscles into which the blood can be evaporated?" Nor does the reviewer fail to flavour this outpouring of preposterous incapacity with a little stimulation of the *odium theologicum*. Some inkling of the history of the conflicts between Astronomy, Geology, and Theology, leads him to keep a retreat open by the proviso that he cannot "consent to test the truth of Natural Science by the word of Revelation;" but, for all that, he devotes pages to the exposition of his conviction that Mr. Darwin's theory "contradicts the revealed relation of the creation to its Creator," and is "inconsistent with the fulness of his glory."

If I confine my retrospect of the reception of the 'Origin of Species' to a twelvemonth, or thereabouts, from the time

© The Complete Work of Charles Darwin Online

of its publication, I do not recollect anything quite so foolish and unmannerly as the 'Quarterly Review' article, unless, perhaps, the address of a Reverend Professor to the Dublin Geological Society might enter into competition with it. But a large proportion of Mr. Darwin's critics had a lamentable resemblance to the 'Quarterly' reviewer, in so far as they lacked either the will, or the wit, to make themselves masters of his doctrine; hardly any possessed the knowledge required to follow him through the immense range of biological and geological science which the 'Origin' covered; while, too commonly, they had prejudged the case on theological grounds, and, as seems to be inevitable when this happens, eked out lack of reason by superfluity of railing.

But it will be more pleasant and more profitable to consider those criticisms, which were acknowledged by writers of scientific authority, or which bore internal evidence of the greater or less competency and, often, of the good faith, of their authors. Restricting my survey to a twelvemonth, or thereabouts, after the publication of the 'Origin,' I find among such critics Louis Agassiz;* Murray, an excellent entomologist; Harvey, a botanist of considerable repute; and the author of an article in the 'Edinburgh Review,' all strongly adverse to Darwin. Pictet, the distinguished and widely learned paleontologist of Geneva, treats Mr. Darwin with a respect which forms a grateful contrast to the tone of some of the preceding writers, but consents to go with him

* "The arguments presented by Darwin in favor of a universal derivation from one primary form of all the peculiarities existing now among living beings have not made the slightest impression on my mind.

"Until the facts of Nature are shown to have been mistaken by those who have collected them, and that they have a different meaning from that now generally assigned to them, I shall therefore consider the transmutation theory as a scientific mistake, untrue in its facts, unscientific in its method, and mischievous in its tendency."—Silliman's 'Journal,' July 1860, pp. 143, 154. Extract from the 3rd vol. of 'Contributions to the Natural History of the United States.'

© The Complete Work of Charles Darwin Online

only a very little way.* On the other hand, Lyell, up to that time a pillar of the anti-transmutationists (who regarded him, ever afterwards, as Pallas Athene may have looked at Dian, after the Endymion affair), declared himself a Darwinian, though not without putting in a serious *caveat.* Nevertheless, he was a tower of strength, and his courageous stand for truth as against consistency, did him infinite honour. As evolutionists, *sans phrase*, I do not call to mind among the biologists more than Asa Gray, who fought the battle splendidly in the United States; Hooker, who was no less vigorous here; the present Sir John Lubbock and myself. Wallace was far away in the Malay Archipelago; but, apart from his direct share in the promulgation of the theory of natural selection, no enumeration of the influences at work, at the time I am speaking of, would be complete without the mention of his powerful essay 'On the Law which has regulated the Introduction of New Species,' which was published in 1855. On reading it afresh, I have been astonished to recollect how small was the impression it made.

In France, the influence of Elie de Beaumont and of Flourens, —the former of whom is said to have "damned himself to everlasting fame" by inventing the nickname of "*la science moussante*" for Evolutionism,†—to say nothing of the ill-will of other powerful members of the Institut, produced for a

* "I see no serious objections to the formation of varieties by natural selection in the existing world, and that, so far as earlier epochs are concerned, this law may be assumed to explain the origin of closely allied species, supposing for this purpose a very long period of time.

"With regard to simple varieties and closely allied species, I believe that Mr. Darwin's theory may explain many things, and throw a great light upon numerous questions."—'Sur l'Origine de l'Espèce. Par Charles Darwin.' 'Archives des Sc. de la Bibliothèque Universelle de Genève,' pp. 242, 243, Mars 1860.

† One is reminded of the effect of another small academic epigram. The so-called vertebral theory of the skull is said to have been nipped in the bud in France by the whisper of an academician to his neighbour, that, in that case, one's head was a "*vertèbre pensante*."

© The Complete Work of Charles Darwin Online

long time the effect of a conspiracy of silence; and many years passed before the Academy redeemed itself from the reproach that the name of Darwin was not to be found on the list of its members. However, an accomplished writer, out of the range of academical influences, M. Laugel, gave an excellent and appreciative notice of the 'Origin' in the 'Revue des Deux Mondes.' Germany took time to consider; Bronn produced a slightly Bowdlerized translation of the 'Origin'; and 'Kladderadatsch' cut his jokes upon the ape origin of man; but I do not call to mind that any scientific notability declared himself publicly in 1860.* None of us dreamed that, in the course of a few years, the strength (and perhaps I may add the weakness) of "Darwinismus" would have its most extensive and most brilliant illustrations in the land of learning. If a foreigner may presume to speculate on the cause of this curious interval of silence, I fancy it was that one moiety of the German biologists were orthodox at any price, and the other moiety as distinctly heterodox. The latter were evolutionists, *a priori*, already, and they must have felt the disgust natural to deductive philosophers at being offered an inductive and experimental foundation for a conviction which they had reached by a shorter cut. It is undoubtedly trying to learn that, though your conclusions may be all right, your reasons for them are all wrong, or, at any rate, insufficient.

On the whole, then, the supporters of Mr. Darwin's views in 1860 were numerically extremely insignificant. There is not the slightest doubt that, if a general council of the Church scientific had been held at that time, we should have been condemned by an overwhelming majority. And there is as little doubt that, if such a council gathered now, the decree would be of an exactly contrary nature. It would indicate a lack

* However, the man who stands next to Darwin in his influence on modern biologists, K. E. von Bär, wrote to me, in August 1860, expressing his general assent to evolutionist views. His phrase, "J'ai énoncé les mêmes idées . . . que M. Darwin" (vol. ii. p. 329), is shown by his subsequent writings to mean no more than this.

© The Complete Work of Charles Darwin Online

of sense, as well as of modesty, to ascribe to the men of that generation less capacity or less honesty than their successors possess. What, then, are the causes which led instructed and fair-judging men of that day to arrive at a judgment so different from that which seems just and fair to those who follow them? That is really one of the most interesting of all questions connected with the history of science, and I shall try to answer it. I am afraid that in order to do so I must run the risk of appearing egotistical. However, if I tell my own story it is only because I know it better than that of other people.

I think I must have read the 'Vestiges' before I left England in 1846; but, if I did, the book made very little impression upon me, and I was not brought into serious contact with the 'Species' question until after 1850. At that time, I had long done with the Pentateuchal cosmogony, which had been impressed upon my childish understanding as Divine truth, with all the authority of parents and instructors, and from which it had cost me many a struggle to get free. But my mind was unbiassed in respect of any doctrine which presented itself, if it professed to be based on purely philosophical and scientific reasoning. It seemed to me then (as it does now) that "creation," in the ordinary sense of the word, is perfectly conceivable. I find no difficulty in imagining that, at some former period, this universe was not in existence; and that it made its appearance in six days (or instantaneously, if that is preferred), in consequence of the volition of some pre-existent Being. Then, as now, the so-called *a priori* arguments against Theism, and, given a Deity, against the possibility of creative acts, appeared to me to be devoid of reasonable foundation. I had not then, and I have not now, the smallest *a priori* objection to raise to the account of the creation of animals and plants given in 'Paradise Lost,' in which Milton so vividly embodies the natural sense of Genesis. Far be it from me to say that it is untrue because it is impos-

© The Complete Work of Charles Darwin Online

sible. I confine myself to what must be regarded as a modest and reasonable request for some particle of evidence that the existing species of animals and plants did originate in that way, as a condition of my belief in a statement which appears to me to be highly improbable.

And, by way of being perfectly fair, I had exactly the same answer to give to the evolutionists of 1851–8. Within the ranks of the biologists, at that time, I met with nobody, except Dr. Grant, of University College, who had a word to say for Evolution—and his advocacy was not calculated to advance the cause. Outside these ranks, the only person known to me whose knowledge and capacity compelled respect, and who was, at the same time, a thorough-going evolutionist, was Mr. Herbert Spencer, whose acquaintance I made, I think, in 1852, and then entered into the bonds of a friendship which, I am happy to think, has known no interruption. Many and prolonged were the battles we fought on this topic. But even my friend's rare dialectic skill and copiousness of apt illustration could not drive me from my agnostic position. I took my stand upon two grounds: firstly, that up to that time, the evidence in favour of transmutation was wholly insufficient; and, secondly, that no suggestion respecting the causes of the transmutation assumed, which had been made, was in any way adequate to explain the phenomena. Looking back at the state of knowledge at that time, I really do not see that any other conclusion was justifiable.

In those days I had never even heard of Treviranus' 'Biologie.' However, I had studied Lamarck attentively and I had read the 'Vestiges' with due care; but neither of them afforded me any good ground for changing my negative and critical attitude. As for the 'Vestiges,' I confess that the book simply irritated me by the prodigious ignorance and thoroughly unscientific habit of mind manifested by the writer. If it had any influence on me at all, it set me against Evolution; and the only review I ever have qualms

© The Complete Work of Charles Darwin Online

of conscience about, on the ground of needless savagery, is one I wrote on the 'Vestiges' while under that influence.

With respect to the 'Philosophie Zoologique,' it is no reproach to Lamarck to say that the discussion of the Species question in that work, whatever might be said for it in 1809, was miserably below the level of the knowledge of half a century later. In that interval of time the elucidation of the structure of the lower animals and plants had given rise to wholly new conceptions of their relations; histology and embryology, in the modern sense, had been created; physiology had been reconstituted; the facts of distribution, geological and geographical, had been prodigiously multiplied and reduced to order. To any biologist whose studies had carried him beyond mere species-mongering in 1850, one-half of Lamarck's arguments were obsolete and the other half erroneous, or defective, in virtue of omitting to deal with the various classes of evidence which had been brought to light since his time. Moreover his one suggestion as to the cause of the gradual modification of species—effort excited by change of conditions—was, on the face of it, inapplicable to the whole vegetable world. I do not think that any impartial judge who reads the 'Philosophie Zoologique' now, and who afterwards takes up Lyell's trenchant and effectual criticism (published as far back as 1830), will be disposed to allot to Lamarck a much higher place in the establishment of biological evolution than that which Bacon assigns to himself in relation to physical science generally,—*buccinator tantum.**

But, by a curious irony of fate, the same influence which led me to put as little faith in modern speculations on this subject, as in the venerable traditions recorded in the first two chapters of Genesis, was perhaps more potent than any other

* Erasmus Darwin first promulgated Lamarck's fundamental conceptions, and, with greater logical consistency, he had applied them to plants. But the advocates of his claims have failed to show that he, in any respect, anticipated the central idea of the 'Origin of Species.'

© The Complete Work of Charles Darwin Online

in keeping alive a sort of pious conviction that Evolution, after all, would turn out true. I have recently read afresh the first edition of the 'Principles of Geology'; and when I consider that this remarkable book had been nearly thirty years in everybody's hands, and that it brings home to any reader of ordinary intelligence a great principle and a great fact—the principle, that the past must be explained by the present, unless good cause be shown to the contrary; and the fact, that, so far as our knowledge of the past history of life on our globe goes, no such cause can be shown *—I cannot but believe that Lyell, for others, as for myself, was the chief agent in smoothing the road for Darwin. For consistent uniformitarianism postulates evolution as much in the organic as in the inorganic world. The origin of a new species by other than ordinary agencies would be a vastly greater "catastrophe" than any of those which Lyell successfully eliminated from sober geological speculation.

In fact, no one was better aware of this than Lyell himself.† If one reads any of the earlier editions of the 'Principles' carefully (especially by the light of the interesting series of letters recently published by Sir Charles Lyell's biographer), it is easy to see that, with all his energetic opposition to Lamarck,

* The same principle and the same fact guide and result from all sound historical investigation. Grote's 'History of Greece' is a product of the same intellectual movement as Lyell's 'Principles.'

† Lyell, with perfect right, claims this position for himself. He speaks of having "advocated a law of continuity even in the organic world, so far as possible without adopting Lamarck's theory of transmutation....

"But while I taught that as often as certain forms of animals and plants disappeared, for reasons quite intelligible to us, others took their place by virtue of a causation which was beyond our comprehension; it remained for Darwin to accumulate proof that there is no break between the incoming and the outgoing species, that they are the work of evolution, and not of special creation....

"I had certainly prepared the way in this country, in six editions of my work before the 'Vestiges of Creation' appeared in 1842 [1844], for the reception of Darwin's gradual and insensible evolution of species."—'Life and Letters,' Letter to Haeckel, vol. ii. p. 436. Nov. 23, 1868.

© The Complete Work of Charles Darwin Online

on the one hand, and to the ideal quasi-progressionism of Agassiz, on the other, Lyell, in his own mind, was strongly disposed to account for the origination of all past and present species of living things by natural causes. But he would have liked, at the same time, to keep the name of creation for a natural process which he imagined to be incomprehensible.

In a letter addressed to Mantell (dated March 2, 1827), Lyell speaks of having just read Lamarck; he expresses his delight at Lamarck's theories, and his personal freedom from any objections based on theological grounds. And though he is evidently alarmed at the pithecoid origin of man involved in Lamarck's doctrine, he observes:—

"But, after all, what changes species may really undergo! How impossible will it be to distinguish and lay down a line, beyond which some of the so-called extinct species have never passed into recent ones."

Again, the following remarkable passage occurs in the postscript of a letter addressed to Sir John Herschel in 1836:—

"In regard to the origination of new species, I am very glad to find that you think it probable that it may be carried on through the intervention of intermediate causes. I left this rather to be inferred, not thinking it worth while to offend a certain class of persons by embodying in words what would only be a speculation." * He goes on to refer to the criticisms which have been directed against him on the ground that, by leaving species to be originated by miracle, he is inconsistent with his own doctrine of uniformitarianism; and he leaves it

* In the same sense, see the letter to Whewell, March 7, 1837, vol. ii., p. 5:—

"In regard to this last subject [the changes from one set of animal and vegetable species to another]... you remember what Herschel said in his letter to me. If I had stated as plainly as he has done the possibility of the introduction or origination of fresh species being a natural, in contradistinction to a miraculous process, I should have raised a host of prejudices against me, which are unfortunately opposed at every step to any philosopher who attempts to address the public on these mysterious subjects." See also letter to Sedgwick, Jan. 20, 1838, vol. ii. p. 35.

© The Complete Work of Charles Darwin Online

to be understood that he had not replied, on the ground of his general objection to controversy.

Lyell's contemporaries were not without some inkling of his esoteric doctrine. Whewell's 'History of the Inductive Sciences,' whatever its philosophical value, is always worth reading and always interesting, if under no other aspect than that of an evidence of the speculative limits within which a highly-placed divine might, at that time, safely range at will. In the course of his discussion of uniformitarianism, the encyclopædic Master of Trinity observes:—

"Mr. Lyell, indeed, has spoken of an hypothesis that 'the successive creation of species may constitute a regular part of the economy of nature,' but he has nowhere, I think, so described this process as to make it appear in what department of science we are to place the hypothesis. Are these new species created by the production, at long intervals, of an offspring different in species from the parents? Or are the species so created produced without parents? Are they gradually evolved from some embryo substance? Or do they suddenly start from the ground, as in the creation of the poet? . . .

"Some selection of one of these forms of the hypothesis, rather than the others, with evidence for the selection, is requisite to entitle us to place it among the known causes of change, which in this chapter we are considering. The bare conviction that a creation of species has taken place, whether once or many times, so long as it is unconnected with our organical sciences, is a tenet of Natural Theology rather than of Physical Philosophy." *

The earlier part of this criticism appears perfectly just and appropriate; but, from the concluding paragraph, Whewell evidently imagines that by "creation" Lyell means a preternatural intervention of the Deity; whereas the letter to Herschel shows that, in his own mind, Lyell meant natural

* Whewell's 'History,' vol. iii. p. 639–640 (ed. 2, 1847).

© The Complete Work of Charles Darwin Online

causation; and I see no reason to doubt * that, if Sir Charles could have avoided the inevitable corollary of the pithecoid origin of man—for which, to the end of his life, he entertained a profound antipathy—he would have advocated the efficiency of causes now in operation to bring about the condition of the organic world, as stoutly as he championed that doctrine in reference to inorganic nature.

The fact is, that a discerning eye might have seen that some form or other of the doctrine of transmutation was inevitable, from the time when the truth enunciated by William

* The following passages in Lyell's letters appear to me decisive on this point:—

To Darwin, Oct. 3, 1859 (ii. 325), on first reading the 'Origin.'

"I have long seen most clearly that if any concession is made, all that you claim in your concluding pages will follow.

"It is this which has made me so long hesitate, always feeling that the case of Man and his Races, and of other animals, and that of plants, is one and the same, and that if a *vera causa* be admitted for one instant, [instead] of a purely unknown and imaginary one, such as the word 'creation,' all the consequences must follow."

To Darwin, March 15, 1863 (vol. ii. p. 365).

"I remember that it was the conclusion he [Lamarck] came to about man that fortified me thirty years ago against the great impression which his arguments at first made on my mind, all the greater because Constant Prévost, a pupil of Cuvier's forty years ago, told me his conviction 'that Cuvier thought species not real, but that science could not advance without assuming that they were so.'"

To Hooker, March 9, 1863 (vol. ii. p. 361), in reference to Darwin's feeling about the 'Antiquity of Man.'

"He [Darwin] seems much disappointed that I do not go farther with him, or do not speak out more. I can only say that I have spoken out to the full extent of my present convictions, and even beyond my state of *feeling* as to man's unbroken descent from the brutes, and I find I am half converting not a few who were in arms against Darwin, and are even now against Huxley." He speaks of having had to abandon "old and long cherished ideas, which constituted the charm to me of the theoretical part of the science in my earlier days, when I believed with Pascal in the theory, as Hallam terms it, of 'the arch-angel ruined.'"

See the same sentiment in the letter to Darwin, March 11, 1863, p. 363:—

"I think the old 'creation' is almost as much required as ever, but of course it takes a new form if Lamarck's views improved by yours are adopted."

© The Complete Work of Charles Darwin Online

Smith, that successive strata are characterised by different kinds of fossil remains, became a firmly established law of nature. No one has set forth the speculative consequences of this generalisation better than the historian of the 'Inductive Sciences':—

"But the study of geology opens to us the spectacle of many groups of species which have, in the course of the earth's history, succeeded each other at vast intervals of time; one set of animals and plants disappearing, as it would seem, from the face of our planet, and others, which did not before exist, becoming the only occupants of the globe. And the dilemma then presents itself to us anew:—either we must accept the doctrine of the transmutation of species, and must suppose that the organized species of one geological epoch were transmuted into those of another by some long-continued agency of natural causes; or else, we must believe in many successive acts of creation and extinction of species, out of the common course of nature; acts which, therefore, we may properly call miraculous." *

Dr. Whewell decides in favour of the latter conclusion. And if any one had plied him with the four questions which he puts to Lyell in the passage already cited, all that can be said now is that he would certainly have rejected the first. But would he really have had the courage to say that a *Rhinoceros tichorhinus*, for instance, "was produced without parents;" or was "evolved from some embryo substance;" or that it suddenly started from the ground like Milton's lion "pawing to get free his hinder parts"? I permit myself to doubt whether even the Master of Trinity's well-tried courage—physical, intellectual, and moral—would have been equal to this feat. No doubt the sudden concurrence of half-a-ton of inorganic molecules into a live rhinoceros is conceivable, and therefore may be possible. But does such an event lie

* Whewell's 'History of the Inductive Sciences.' Ed. ii., 1847, vol. iii. p. 624–625. See, for the author's verdict, pp. 638–39.

© The Complete Work of Charles Darwin Online

sufficiently within the bounds of probability to justify the belief in its occurrence on the strength of any attainable, or, indeed, imaginable, evidence?

In view of the assertion (often repeated in the early days of the opposition to Darwin) that he had added nothing to Lamarck, it is very interesting to observe that the possibility of a fifth alternative, in addition to the four he has stated, has not dawned upon Dr. Whewell's mind. The suggestion that new species may result from the selective action of external conditions upon the variations from their specific type which individuals present—and which we call "spontaneous," because we are ignorant of their causation—is as wholly unknown to the historian of scientific ideas as it was to biological specialists before 1858. But that suggestion is the central idea of the 'Origin of Species,' and contains the quintessence of Darwinism.

Thus, looking back into the past, it seems to me that my own position of critical expectancy was just and reasonable, and must have been taken up, on the same grounds, by many other persons. If Agassiz told me that the forms of life which had successively tenanted the globe were the incarnations of successive thoughts of the Deity; and that He had wiped out one set of these embodiments by an appalling geological catastrophe as soon as His ideas took a more advanced shape, I found myself not only unable to admit the accuracy of the deductions from the facts of paleontology, upon which this astounding hypothesis was founded, but I had to confess my want of any means of testing the correctness of his explanation of them. And besides that, I could by no means see what the explanation explained. Neither did it help me to be told by an eminent anatomist that species had succeeded one another in time, in virtue of "a continuously operative creational law." That seemed to me to be no more than saying that species had succeeded one another, in the form of a vote-catching resolution, with "law" to please the

© The Complete Work of Charles Darwin Online

man of science, and "creational" to draw the orthodox. So I took refuge in that "*thätige Skepsis*" which Goethe has so well defined; and, reversing the apostolic precept to be all things to all men, I usually defended the tenability of the received doctrines, when I had to do with the transmutationists; and stood up for the possibility of transmutation among the orthodox—thereby, no doubt, increasing an already current, but quite undeserved, reputation for needless combativeness.

I remember, in the course of my first interview with Mr. Darwin, expressing my belief in the sharpness of the lines of demarcation between natural groups and in the absence of transitional forms, with all the confidence of youth and imperfect knowledge. I was not aware, at that time, that he had then been many years brooding over the species-question; and the humorous smile which accompanied his gentle answer, that such was not altogether his view, long haunted and puzzled me. But it would seem that four or five years' hard work had enabled me to understand what it meant; for Lyell,* writing to Sir Charles Bunbury (under date of April 30, 1856), says:—

"When Huxley, Hooker, and Wollaston were at Darwin's last week they (all four of them) ran a tilt against species—further, I believe, than they are prepared to go."

I recollect nothing of this beyond the fact of meeting Mr. Wollaston; and except for Sir Charles' distinct assurance as to "all four," I should have thought my *outrecuidance* was probably a counterblast to Wollaston's conservatism. With regard to Hooker, he was already, like Voltaire's Habakkuk, "*capable de tout*" in the way of advocating Evolution.

As I have already said, I imagine that most of those of my contemporaries who thought seriously about the matter, were very much in my own state of mind—inclined to say to both Mosaists and Evolutionists, "a plague on both your

* 'Life and Letters,' vol. ii. p. 212.

© The Complete Work of Charles Darwin Online

houses!" and disposed to turn aside from an interminable and apparently fruitless discussion, to labour in the fertile fields of ascertainable fact. And I may, therefore, further suppose that the publication of the Darwin and Wallace papers in 1858, and still more that of the 'Origin' in 1859, had the effect upon them of the flash of light, which to a man who has lost himself in a dark night, suddenly reveals a road which, whether it takes him straight home or not, certainly goes his way. That which we were looking for, and could not find, was a hypothesis respecting the origin of known organic forms, which assumed the operation of no causes but such as could be proved to be actually at work. We wanted, not to pin our faith to that or any other speculation, but to get hold of clear and definite conceptions which could be brought face to face with facts and have their validity tested. The 'Origin' provided us with the working hypothesis we sought. Moreover, it did the immense service of freeing us for ever from the dilemma—refuse to accept the creation hypothesis, and what have you to propose that can be accepted by any cautious reasoner? In 1857, I had no answer ready, and I do not think that any one else had. A year later, we reproached ourselves with dulness for being perplexed by such an inquiry. My reflection, when I first made myself master of the central idea of the 'Origin,' was, "How extremely stupid not to have thought of that!" I suppose that Columbus' companions said much the same when he made the egg stand on end. The facts of variability, of the struggle for existence, of adaptation to conditions, were notorious enough; but none of us had suspected that the road to the heart of the species problem lay through them, until Darwin and Wallace dispelled the darkness, and the beacon-fire of the 'Origin' guided the benighted.

Whether the particular shape which the doctrine of evolution, as applied to the organic world, took in Darwin's hands, would prove to be final or not, was, to me, a matter of indiffer-

© The Complete Work of Charles Darwin Online

ence. In my earliest criticisms of the 'Origin' I ventured to point out that its logical foundation was insecure so long as experiments in selective breeding had not produced varieties which were more or less infertile; and that insecurity remains up to the present time. But, with any and every critical doubt which my sceptical ingenuity could suggest, the Darwinian hypothesis remained incomparably more probable than the creation hypothesis. And if we had none of us been able to discern the paramount significance of some of the most patent and notorious of natural facts, until they were, so to speak, thrust under our noses, what force remained in the dilemma—creation or nothing? It was obvious that, hereafter, the probability would be immensely greater, that the links of natural causation were hidden from our purblind eyes, than that natural causation should be incompetent to produce all the phenomena of nature. The only rational course for those who had no other object than the attainment of truth, was to accept "Darwinism" as a working hypothesis, and see what could be made of it. Either it would prove its capacity to elucidate the facts of organic life, or it would break down under the strain. This was surely the dictate of common sense; and, for once, common sense carried the day. The result has been that complete *volte-face* of the whole scientific world, which must seem so surprising to the present generation. I do not mean to say that all the leaders of biological science have avowed themselves Darwinians; but I do not think that there is a single zoologist, or botanist, or palæontologist, among the multitude of active workers of this generation, who is other than an evolutionist, profoundly influenced by Darwin's views. Whatever may be the ultimate fate of the particular theory put forth by Darwin, I venture to affirm that, so far as my knowledge goes, all the ingenuity and all the learning of hostile critics has not enabled them to adduce a solitary fact, of which it can be said, this is irreconcilable with the Darwinian theory. In the prodigious variety and com-

© The Complete Work of Charles Darwin Online

plexity of organic nature, there are multitudes of phenomena which are not deducible from any generalisations we have yet reached. But the same may be said of every other class of natural objects. I believe that astronomers cannot yet get the moon's motions into perfect accordance with the theory of gravitation.

It would be inappropriate, even if it were possible, to discuss the difficulties and unresolved problems which have hitherto met the evolutionist, and which will probably continue to puzzle him for many generations to come, in the course of this brief history of the reception of Mr. Darwin's great work. But there are two or three objections of a more general character, based, or supposed to be based, upon philosophical and theological foundations, which were loudly expressed in the early days of the Darwinian controversy, and which, though they have been answered over and over again, crop up now and then at the present day.

The most singular of these, perhaps immortal, fallacies, which live on, Tithonus-like, when sense and force have long deserted them, is that which charges Mr. Darwin with having attempted to reinstate the old pagan goddess, Chance. It is said that he supposes variations to come about "by chance," and that the fittest survive the "chances" of the struggle for existence, and thus "chance" is substituted for providential design.

It is not a little wonderful that such an accusation as this should be brought against a writer who has, over and over again, warned his readers that when he uses the word "spontaneous," he merely means that he is ignorant of the cause of that which is so termed; and whose whole theory crumbles to pieces if the uniformity and regularity of natural causation for illimitable past ages is denied. But probably the best answer to those who talk of Darwinism meaning the reign of "chance," is to ask them what they themselves understand by

© The Complete Work of Charles Darwin Online

"chance." Do they believe that anything in this universe happens without reason or without a cause? Do they really conceive that any event has no cause, and could not have been predicted by any one who had a sufficient insight into the order of Nature? If they do, it is they who are the inheritors of antique superstition and ignorance, and whose minds have never been illumined by a ray of scientific thought. The one act of faith in the convert to science, is the confession of the universality of order and of the absolute validity, in all times and under all circumstances, of the law of causation. This confession is an act of faith, because, by the nature of the case, the truth of such propositions is not susceptible of proof. But such faith is not blind, but reasonable; because it is invariably confirmed by experience, and constitutes the sole trustworthy foundation for all action.

If one of these people, in whom the chance-worship of our remoter ancestors thus strangely survives, should be within reach of the sea when a heavy gale is blowing, let him betake himself to the shore and watch the scene. Let him note the infinite variety of form and size of the tossing waves out at sea; or of the curves of their foam-crested breakers, as they dash against the rocks; let him listen to the roar and scream of the shingle as it is cast up and torn down the beach; or look at the flakes of foam as they drive hither and thither before the wind; or note the play of colours, which answers a gleam of sunshine as it falls upon their myriad bubbles. Surely here, if anywhere, he will say that chance is supreme, and bend the knee as one who has entered the very penetralia of his divinity. But the man of science knows that here, as everywhere, perfect order is manifested; that there is not a curve of the waves, not a note in the howling chorus, not a rainbow-glint on a bubble, which is other than a necessary consequence of the ascertained laws of nature; and that with a sufficient knowledge of the conditions, competent physico-

© The Complete Work of Charles Darwin Online

mathematical skill could account for, and indeed predict, every one of these "chance" events.

A second very common objection to Mr. Darwin's views was (and is), that they abolish Teleology, and eviscerate the argument from design. It is nearly twenty years since I ventured to offer some remarks on this subject, and as my arguments have as yet received no refutation, I hope I may be excused for reproducing them. I observed, "that the doctrine of Evolution is the most formidable opponent of all the commoner and coarser forms of Teleology. But perhaps the most remarkable service to the philosophy of Biology rendered by Mr. Darwin is the reconciliation of Teleology and Morphology, and the explanation of the facts of both, which his views offer. The teleology which supposes that the eye, such as we see it in man, or one of the higher vertebrata, was made with the precise structure it exhibits, for the purpose of enabling the animal which possesses it to see, has undoubtedly received its death-blow. Nevertheless, it is necessary to remember that there is a wider teleology which is not touched by the doctrine of Evolution, but is actually based upon the fundamental proposition of Evolution. This proposition is that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces * possessed by the molecules of which the primitive nebulosity of the universe was composed. If this be true, it is no less certain that the existing world lay potentially in the cosmic vapour, and that a sufficient intelligence could, from a knowledge of the properties of the molecules of that vapour, have predicted, say the state of the fauna of Britain in 1869, with as much certainty as one can say what will happen to the vapour of the breath on a cold winter's day. . . . .

. . . . The teleological and the mechanical views of nature are not, necessarily, mutually exclusive. On the contrary, the more purely a mechanist the speculator is, the more firmly

* I should now like to substitute the word powers for "forces."

© The Complete Work of Charles Darwin Online

does he assume a primordial molecular arrangement of which all the phenomena of the universe are the consequences, and the more completely is he thereby at the mercy of the teleologist, who can always defy him to disprove that this primordial molecular arrangement was not intended to evolve the phenomena of the universe." *

The acute champion of Teleology, Paley, saw no difficulty in admitting that the "production of things" may be the result of trains of mechanical dispositions fixed beforehand by intelligent appointment and kept in action by a power at the centre, † that is to say, he proleptically accepted the modern doctrine of Evolution; and his successors might do well to follow their leader, or at any rate to attend to his weighty reasonings, before rushing into an antagonism which has no reasonable foundation.

Having got rid of the belief in chance and the disbelief in design, as in no sense appurtenances of Evolution, the third libel upon that doctrine, that it is anti-theistic, might perhaps be left to shift for itself. But the persistence with which many people refuse to draw the plainest consequences from the propositions they profess to accept, renders it advisable to remark that the doctrine of Evolution is neither Anti-theistic nor Theistic. It simply has no more to do with Theism than the first book of Euclid has. It is quite certain that a normal fresh-laid egg contains neither cock nor hen; and it is also as certain as any proposition in physics or morals, that if such an egg is kept under proper conditions for three weeks, a cock or hen chicken will be found in it. It is also quite certain that if the shell were transparent we should be able to watch the formation of the young fowl, day by day, by a process of evolution, from a microscopic cellular germ to its full size and complication of structure. Therefore

* The "Genealogy of Animals" ('The Academy,' 1869), reprinted in 'Critiques and Addresses.'

† 'Natural Theology,' chap. xxiii.

© The Complete Work of Charles Darwin Online

Evolution, in the strictest sense, is actually going on in this and analogous millions and millions of instances, wherever living creatures exist. Therefore, to borrow an argument from Butler, as that which now happens must be consistent with the attributes of the Deity, if such a Being exists, Evolution must be consistent with those attributes. And, if so, the evolution of the universe, which is neither more nor less explicable than that of a chicken, must also be consistent with them. The doctrine of Evolution, therefore, does not even come into contact with Theism, considered as a philosophical doctrine. That with which it does collide, and with which it is absolutely inconsistent, is the conception of creation, which theological speculators have based upon the history narrated in the opening of the book of Genesis.

There is a greal deal of talk and not a little lamentation about the so-called religious difficulties which physical science has created. In theological science, as a matter of fact, it has created none. Not a solitary problem presents itself to the philosophical Theist, at the present day, which has not existed from the time that philosophers began to think out the logical grounds and the logical consequences of Theism. All the real or imaginary perplexities which flow from the conception of the universe as a determinate mechanism, are equally involved in the assumption of an Eternal, Omnipotent and Omniscient Deity. The theological equivalent of the scientific conception of order is Providence; and the doctrine of determinism follows as surely from the attributes of foreknowledge assumed by the theologian, as from the universality of natural causation assumed by the man of science. The angels in 'Paradise Lost' would have found the task of enlightening Adam upon the mysteries of "Fate, Foreknowledge, and Free-will," not a whit more difficult, if their pupil had been educated in a "Real-schule" and trained in every laboratory of a modern university. In respect of the great problems of Philosophy, the post-Darwinian generation is,

© The Complete Work of Charles Darwin Online

in one sense, exactly where the præ-Darwinian generations were. They remain insoluble. But the present generation has the advantage of being better provided with the means of freeing itself from the tyranny of certain sham solutions.

The known is finite, the unknown infinite; intellectually we stand on an islet in the midst of an illimitable ocean of inexplicability. Our business in every generation is to reclaim a little more land, to add something to the extent and the solidity of our possessions. And even a cursory glance at the history of the biological sciences during the last quarter of a century is sufficient to justify the assertion, that the most potent instrument for the extension of the realm of natural knowledge which has come into men's hands, since the publication of Newton's 'Principia,' is Darwin's 'Origin of Species.'

It was badly received by the generation to which it was first addressed, and the outpouring of angry nonsense to which it gave rise is sad to think upon. But the present generation will probably behave just as badly if another Darwin should arise, and inflict upon them that which the generality of mankind most hate—the necessity of revising their convictions. Let them, then, be charitable to us ancients; and if they behave no better than the men of my day to some new benefactor, let them recollect that, after all, our wrath did not come to much, and vented itself chiefly in the bad language of sanctimonious scolds. Let them as speedily perform a strategic right-about-face, and follow the truth wherever it leads. The opponents of the new truth will discover, as those of Darwin are doing, that, after all, theories do not alter facts, and that the universe remains unaffected even though texts crumble. Or, it may be, that, as history repeats itself, their happy ingenuity will also discover that the new wine is exactly of the same vintage as the old, and that (rightly viewed) the old bottles prove to have been expressly made for holding it.

© The Complete Work of Charles Darwin Online

CHAPTER VI.

THE PUBLICATION OF THE 'ORIGIN OF SPECIES.'

OCTOBER 3, 1859, TO DECEMBER 31, 1859.

1859.

[UNDER the date of October 1st, 1859, in my father's Diary occurs the entry: "Finished proofs (thirteen months and ten days) of Abstract on 'Origin of Species'; 1250 copies printed. The first edition was published on November 24th, and all copies sold first day."

On October 2nd he started for a water-cure establishment at Ilkley, near Leeds, where he remained with his family until December, and on the 9th of that month he was again at Down. The only other entry in the Diary for this year is as follows: "During end of November and beginning of December, employed in correcting for second edition of 3000 copies; multitude of letters."

The first and a few of the subsequent letters refer to proof sheets, and to early copies of the 'Origin' which were sent to friends before the book was published.]

*C. Lyell to C. Darwin.**

October 3rd, 1859.

MY DEAR DARWIN,—I have just finished your volume and right glad I am that I did my best with Hooker to

* Part of this letter is given in the 'Life of Sir Charles Lyell,' vol. ii. p. 325.

© The Complete Work of Charles Darwin Online

persuade you to publish it without waiting for a time which probably could never have arrived, though you lived till the age of a hundred, when you had prepared all your facts on which you ground so many grand generalizations.

It is a splendid case of close reasoning, and long substantial argument throughout so many pages; the condensation immense, too great perhaps for the uninitiated, but an effective and important preliminary statement, which will admit, even before your detailed proofs appear, of some occasional useful exemplification, such as your pigeons and cirripedes, of which you make such excellent use.

I mean that, when, as I fully expect, a new edition is soon called for, you may here and there insert an actual case to relieve the vast number of abstract propositions. So far as I am concerned, I am so well prepared to take your statements of facts for granted, that I do not think the "pièces justificatives" when published will make much difference, and I have long seen most clearly that if any concession is made, all that you claim in your concluding pages will follow. It is this which has made me so long hesitate, always feeling that the case of Man and his races, and of other animals, and that of plants is one and the same, and that if a "vera causa" be admitted for one, instead of a purely unknown and imaginary one, such as the word "Creation," all the consequences must follow.

I fear I have not time to-day, as I am just leaving this place, to indulge in a variety of comments, and to say how much I was delighted with Oceanic Islands—Rudimentary Organs—Embryology—the genealogical key to the Natural System, Geographical Distribution, and if I went on I should be copying the heads of all your chapters. But I will say a word of the Recapitulation, in case some slight alteration, or, at least, omission of a word or two be still possible in that.

In the first place, at p. 480, it cannot surely be said that

© The Complete Work of Charles Darwin Online

the most eminent naturalists have rejected the view of the mutability of species? You do not mean to ignore G. St. Hilaire and Lamarck. As to the latter, you may say, that in regard to animals you substitute natural selection for volition to a certain considerable extent, but in his theory of the changes of plants he could not introduce volition; he may, no doubt, have laid an undue comparative stress on changes in physical conditions, and too little on those of contending organisms. He at least was for the universal mutability of species and for a genealogical link between the first and the present. The men of his school also appealed to domesticated varieties. (Do you mean *living* naturalists?) *

The first page of this most important summary gives the adversary an advantage, by putting forth so abruptly and crudely such a startling objection as the formation of "the eye," not by means analogous to man's reason, or rather by some power immeasurably superior to human reason, but by superinduced variation like those of which a cattle-breeder avails himself. Pages would be required thus to state an objection and remove it. It would be better, as you wish to persuade, to say nothing. Leave out several sentences, and in a future edition bring it out more fully. Between the throwing down of such a stumbling-block in the way of the reader, and the passage to the working ants, in p. 460, there are pages required; and these ants are a bathos to him before he has recovered from the shock of being called upon to believe the eye to have been brought to perfection, from a state of blindness or purblindness, by such variations as we witness. I think a little omission would greatly lessen the objectionableness of these sentences if you have not time to recast and amplify.

. . . . But these are small matters, mere spots on the sun. Your comparison of the letters retained in words, when

* In the published copies of the first edition, p. 480, the words are "eminent living naturalists."

© The Complete Work of Charles Darwin Online

no longer wanted for the sound, to rudimentary organs is excellent, as both are truly genealogical.

The want of peculiar birds in Madeira is a greater difficulty than seemed to me allowed for. I could cite passages where you show that variations are superinduced from the new circumstances of new colonists, which would require some Madeira birds, like those of the Galapagos, to be peculiar. There has been ample time in the case of Madeira and Porto Santo. . . .

You enclose your sheets in old MS., so the Post Office very properly charge them, as letters, 2*d.* extra. I wish all their fines on MS. were worth as much. I paid 4*s.* 6*d.* for such wash the other day from Paris, from a man who can prove 300 deluges in the valley of Seine.

With my hearty congratulations to you on your grand work, believe me,

Ever very affectionately yours,

CHAS. LYELL.

C. Darwin to C. Lyell.

Ilkley, Yorkshire,
October 11th [1859].

MY DEAR LYELL,—I thank you cordially for giving me so much of your valuable time in writing me the long letter of 3rd, and still longer of 4th. I wrote a line with the missing proof-sheet to Scarborough. I have adopted most thankfully all your minor corrections in the last chapter, and the greater ones as far as I could with little trouble. I damped the opening passage about the eye (in my bigger work I show the gradations in structure of the eye) by putting merely "complex organs." But you are a pretty Lord Chancellor to tell the barrister on one side how best to win the cause! The omission of "living" before eminent naturalists was a dreadful blunder.

© The Complete Work of Charles Darwin Online

Madeira and Bermuda Birds not peculiar.—You are right, there is a screw out here; I thought no one would have detected it; I blundered in omitting a discussion, which I have written out in full. But once for all, let me say as an excuse, that it was most difficult to decide what to omit. Birds, which have struggled in their own homes, when settled in a body, nearly simultaneously in a new country, would not be subject to much modification, for their mutual relations would not be much disturbed. But I quite agree with you, that in time they ought to undergo some. In Bermuda and Madeira they have, as I believe, been kept constant by the frequent arrival, and the crossing with unaltered immigrants of the same species from the main land. In Bermuda this can be proved, in Madeira highly probable, as shown me by letters from E. V. Harcourt. Moreover, there are ample ground. for believing that the crossed offspring of the new immigrants (fresh blood as breeders would say), and old colonists of the same species would be extra vigorous, and would be the most likely to survive; thus the effects of such crossing in keeping the old colonists unaltered would be much aided.

On Galapagos productions having American type on view of Creation.—I cannot agree with you, that species if created to struggle with American forms, would have to be created on the American type. Facts point diametrically the other way. Look at the unbroken and untilled ground in La Plata, *covered* with European products, which have no near affinity to the indigenous products. They are not American types which conquer the aborigines. So in every island throughout the world. Alph. De Candolle's result (though he does not see its full importance), that thoroughly well naturalised [plants] are in general very different from the aborigines (belonging in large proportion of cases to non-indigenous genera) is most important always to bear in mind. Once for all, I am sure, you will understand that I thus write dogmatically for brevity sake.

© The Complete Work of Charles Darwin Online

On the continued Creation of Monads.—This doctrine is superfluous (and groundless) on the theory of Natural Selection, which implies no *necessary* tendency to progression. A monad, if no deviation in its structure profitable to it under its *excessively simple* conditions of life occurred, might remain unaltered from long before the Silurian Age to the present day. I grant there will generally be a tendency to advance in complexity of organisation, though in beings fitted for very simple conditions it would be slight and slow. How could a complex organisation profit a monad? if it did not profit it there would be no advance. The Secondary Infusoria differ but little from the living. The parent monad form might perfectly well survive unaltered and fitted for its simple conditions, whilst the offspring of this very monad might become fitted for more complex conditions. The one primordial prototype of all living and extinct creatures may, it is possible, be now alive! Moreover, as you say, higher forms might be occasionally degraded, the snake Typhlops *seems* (?!) to have the habits of earth-worms. So that fresh creations of simple forms seem to me wholly superfluous.

"*Must you not assume a primeval creative power which does not act with uniformity, or how could man supervene?*"—I am not sure that I understand your remarks which follow the above. We must, under present knowledge, assume the creation of one or of a few forms in the same manner as philosophers assume the existence of a power of attraction without any explanation. But I entirely reject, as in my judgment quite unnecessary, any subsequent addition "of new powers and attributes and forces;" or of any "principle of improvement," except in so far as every character which is naturally selected or preserved is in some way an advantage or improvement, otherwise it would not have been selected. If I were convinced that I required such additions to the theory of natural selection, I would reject it as rubbish, but I have firm faith in it, as I cannot believe, that if false, it would explain so

© The Complete Work of Charles Darwin Online

many whole classes of facts, which, if I am in my senses, it seems to explain. As far as I understand your remarks and illustrations, you doubt the possibility of gradations of intellectual powers. Now, it seems to me, looking to existing animals alone, that we have a very fine gradation in the intellectual powers of the Vertebrata, with one rather wide gap (not half so wide as in many cases of corporeal structure), between say a Hottentot and an Ourang, even if civilised as much mentally as the dog has been from the wolf. I suppose that you do not doubt that the intellectual powers are as important for the welfare of each being as corporeal structure; if so, I can see no difficulty in the most intellectual individuals of a species being continually selected; and the intellect of the new species thus improved, aided probably by effects of inherited mental exercise. I look at this process as now going on with the races of man; the less intellectual races being exterminated. But there is not space to discuss this point. If I understand you, the turning-point in our difference must be, that you think it impossible that the intellectual powers of a species should be much improved by the continued natural selection of the most intellectual individuals. To show how minds graduate, just reflect how impossible every one has yet found it, to define the difference in mind of man and the lower animals; the latter seem to have the very same attributes in a much lower stage of perfection than the lowest savage. I would give absolutely nothing for the theory of Natural Selection, if it requires miraculous additions at any one stage of descent. I think Embryology, Homology, Classification, &c. &c., show us that all vertebrata have descended from one parent; how that parent appeared we know not. If you admit in ever so little a degree, the explanation which I have given of Embryology, Homology and Classification, you will find it difficult to say: thus far the explanation holds good, but no further; here we must call in "the addition of new creative forces." I think you

P 2

© The Complete Work of Charles Darwin Online

will be driven to reject all or admit all: I fear by your letter it will be the former alternative; and in that case I shall feel sure it is my fault, and not the theory's fault, and this will certainly comfort me. With regard to the descent of the great Kingdoms (as Vertebrata, Articulata, &c.) from one parent, I have said in the conclusion, that mere analogy makes me think it probable; my arguments and facts are sound in my judgment only for each separate kingdom.

The forms which are beaten inheriting some inferiority in common.—I dare say I have not been guarded enough, but might not the term inferiority include less perfect adaptation to physical conditions?

My remarks apply not to single species, but to groups or genera; the species of most genera are adapted at least to rather hotter, and rather less hot, to rather damper and dryer climates; and when the several species of a group are beaten and exterminated by the several species of another group, it will not, I think, generally be from *each* new species being adapted to the climate, but from all the new species having some common advantage in obtaining sustenance, or escaping enemies. As groups are concerned, a fairer illustration than negro and white in Liberia would be the almost certain future extinction of the genus ourang by the genus man, not owing to man being better fitted for the climate, but owing to the inherited intellectual inferiority of the Ourang-genus to Man-genus, by his intellect, inventing fire-arms and cutting down forests. I believe, from reasons given in my discussion, that acclimatisation is readily effected under nature. It has taken me so many years to disabuse my mind of the *too* great importance of climate—its important influence being so conspicuous, whilst that of a struggle between creature and creature is so hidden—that I am inclined to swear at the North Pole, and as Sydney Smith said, even to speak disrespectfully of the Equator. I beg you often to reflect (I have found *nothing* so instructive) on the case of thousands of plants in the

© The Complete Work of Charles Darwin Online

middle point of their respective ranges, and which, as we positively know, can perfectly well withstand a little more heat and cold, a little more damp and dry, but which in the metropolis of their range do not exist in vast numbers, although, if many of the other inhabitants were destroyed [they] would cover the ground. We thus clearly see that their numbers are kept down, in almost every case, not by climate, but by the struggle with other organisms. All this you will perhaps think very obvious; but, until I repeated it to myself thousands of times, I took, as I believe, a wholly wrong view of the whole economy of nature. . . .

Hybridism.—I am so much pleased that you approve of this chapter; you would be astonished at the labour this cost me; so often was I, on what I believe was, the wrong scent.

Rudimentary Organs.—On the theory of Natural Selection there is a wide distinction between Rudimentary Organs and what you call germs of organs, and what I call in my bigger book "nascent" organs. An organ should not be called rudimentary unless it be useless—as teeth which never cut through the gums—the papillæ, representing the pistil in male flowers, wing of Apteryx, or better, the little wings under soldered elytra. These organs are now plainly useless, and *à fortiori*, they would be useless in a less developed state. Natural Selection acts exclusively by preserving successive slight, *useful* modifications. Hence Natural Selection cannot possibly make a useless or rudimentary organ. Such organs are solely due to inheritance (as explained in my discussion), and plainly bespeak an ancestor having the organ in a useful condition. They may be, and often have been, worked in for other purposes, and then they are only rudimentary for the original function, which is sometimes plainly apparent. A nascent organ, though little developed, as it has to be developed must be useful in every stage of development. As we cannot prophesy, we cannot tell what organs are now nascent; and

© The Complete Work of Charles Darwin Online

nascent organs will rarely have been handed down by certain members of a class from a remote period to the present day, for beings with any important organ but little developed, will generally have been supplanted by their descendants with the organ well developed. The mammary glands in Ornithorhynchus may, perhaps, be considered as nascent compared with the udders of a cow—*Ovigerous frena*, in certain cirripedes, are nascent branchiæ—in [illegible] the swim bladder is almost rudimentary for this purpose, and is nascent as a lung. The small wing of penguin, used only as a fin, might be nascent as a wing; not that I think so; for the whole structure of the bird is adapted for flight, and a penguin so closely resembles other birds, that we may infer that its wings have probably been modified, and reduced by natural selection, in accordance with its sub-aquatic habits. Analogy thus often serves as a guide in distinguishing whether an organ is rudimentary or nascent. I believe the *Os coccyx* gives attachment to certain muscles, but I cannot doubt that it is a rudimentary tail. The bastard wing of birds is a rudimentary digit; and I believe that if fossil birds are found very low down in the series, they will be seen to have a double or bifurcated wing. Here is a bold prophecy!

To admit prophetic germs, is tantamount to rejecting the theory of Natural Selection.

I am very glad you think it worth while to run through my book again, as much, or more, for the subject's sake as for my own sake. But I look at your keeping the subject for some little time before your mind—raising your own difficulties and solving them—as far more important than reading my book. If you think enough, I expect you will be perverted, and if you ever are, I shall know that the theory of Natural Selection is, in the main, safe; that it includes, as now put forth, many errors, is almost certain, though I cannot see them. Do not, of course, think of answering this; but if you have other *occasion* to write again, just say whether I have, in ever

© The Complete Work of Charles Darwin Online

so slight a degree, shaken any of your objections. Farewell. With my cordial thanks for your long letters and valuable remarks,

Believe me, yours most truly,
C. DARWIN.

P.S.—You often allude to Lamarck's work; I do not know what you think about it, but it appeared to me extremely poor; I got not a fact or idea from it.

*C. Darwin to L. Agassiz.**

Down, November 11th [1859].

MY DEAR SIR,—I have ventured to send you a copy of my book (as yet only an abstract) on the 'Origin of Species.' As the conclusions at which I have arrived on several points differ so widely from yours, I have thought (should you at any time read my volume) that you might think that I had sent it to you out of a spirit of defiance or bravado; but I assure you that I act under a wholly different frame of mind. I hope that you will at least give me credit, however erroneous you may think my conclusions, for having earnestly endeavoured to arrive at the truth. With sincere respect, I beg leave to remain,

Yours very faithfully,
CHARLES DARWIN.

* Jean Louis Rodolphe Agassiz, born at Mortier, on the lake of Morat in Switzerland, on May 28, 1807. He emigrated to America in 1846, where he spent the rest of his life, and died Dec. 14, 1873. His 'Life,' written by his widow, was published in 1885. The following extract from a letter to Agassiz (1850) is worth giving, as showing how my father regarded him, and it may be added that his cordial feelings towards the great American naturalist remained strong to the end of his life:—

"I have seldom been more deeply gratified than by receiving your most kind present of 'Lake Superior.' I had heard of it, and had much wished to read it, but I confess that it was the very great honour of having in my possession a work with your autograph as a presentation copy, that has given me such lively and sincere pleasure. I cordially thank you for it. I have begun to read it with uncommon interest, which I see will increase as I go on."

© The Complete Work of Charles Darwin Online

C. Darwin to A. De Candolle.

Down, November 11th [1859].

DEAR SIR,—I have thought that you would permit me to send you (by Messrs. Williams and Norgate, booksellers) a copy of my work (as yet only an abstract) on the 'Origin of Species.' I wish to do this, as the only, though quite inadequate manner, by which I can testify to you the extreme interest which I have felt, and the great advantage which I have derived, from studying your grand and noble work on Geographical Distribution. Should you be induced to read my volume, I venture to remark that it will be intelligible only by reading the whole straight through, as it is very much condensed. It would be a high gratification to me if any portion interested you. But I am perfectly well aware that you will entirely disagree with the conclusion at which I have arrived.

You will probably have quite forgotten me; but many years ago you did me the honour of dining at my house in London to meet M. and Madame Sismondi,* the uncle and aunt of my wife. With sincere respect, I beg to remain,

Yours very faithfully,

CHARLES DARWIN.

C. Darwin to Hugh Falconer.

Down, November 11th [1859].

MY DEAR FALCONER,—I have told Murray to send you a copy of my book on the 'Origin of Species,' which as yet is only an abstract.

If you read it, you must read it straight through, otherwise from its extremely condensed state it will be unintelligible.

Lord, how savage you will be, if you read it, and how you will long to crucify me alive! I fear it will produce no other

* Jessie Allen, sister of Mrs. Josiah Wedgwood of Maer.

©.The Complete Work of Charles Darwin Online

effect on you; but if it should stagger you in ever so slight a degree, in this case, I am fully convinced that you will become, year after year, less fixed in your belief in the immutability of species. With this audacious and presumptuous conviction,

I remain, my dear Falconer,
Yours most truly,
CHARLES DARWIN.

C. Darwin to Asa Gray.

Down, November 11th [1859].

MY DEAR GRAY,—I have directed a copy of my book (as yet only an abstract) on the 'Origin of Species' to be sent you. I know how you are pressed for time; but if you can read it, I shall be infinitely gratified . . . . If ever you do read it, and can screw out time to send me (as I value your opinion so highly), however short a note, telling me what you think its weakest and best parts, I should be extremely grateful. As you are not a geologist, you will excuse my conceit in telling you that Lyell highly approves of the two Geological chapters, and thinks that on the Imperfection of the Geological Record not exaggerated. He is nearly a convert to my views. . . . .

Let me add I fully admit that there are very many difficulties not satisfactorily explained by my theory of descent with modification, but I cannot possibly believe that a false theory would explain so many classes of facts as I think it certainly does explain. On these grounds I drop my anchor, and believe that the difficulties will slowly disappear. . . .

C. Darwin to J. S. Henslow.

Down, November 11th, 1859.

MY DEAR HENSLOW,—I have told Murray to send a copy of my book on Species to you, my dear old master in Natural

© The Complete Work of Charles Darwin Online

History; I fear, however, that you will not approve of your pupil in this case. The book in its present state does not show the amount of labour which I have bestowed on the subject.

If you have time to read it carefully, and would take the trouble to point out what parts seem weakest to you and what best, it would be a most material aid to me in writing my bigger book, which I hope to commence in a few months. You know also how highly I value your judgment. But I am not so unreasonable as to wish or expect you to write detailed and lengthy criticisms, but merely a few general remarks, pointing out the weakest parts.

If you are *in even so slight a degree* staggered (which I hardly expect) on the immutability of species, then I am convinced with further reflection you will become more and more staggered, for this has been the process through which my mind has gone. My dear Henslow,

Yours affectionately and gratefully,

C. DARWIN.

*C. Darwin to John Lubbock.**

Ilkley, Yorkshire,
Saturday [November 12th, 1859].

. . . Thank you much for asking me to Brighton. I hope much that you will enjoy your holiday. I have told Murray to send a copy for you to Mansion House Street, and I am surprised that you have not received it. There are so many valid and weighty arguments against my notions, that you, or any one, if you wish on the other side, will easily persuade yourself that I am wholly in error, and no doubt I am in part in error, perhaps wholly so, though I cannot see the blindness of my ways. I dare say when thunder and lightning were first proved to be due to secondary causes, some regretted to

* The present Sir John Lubbock.

© The Complete Work of Charles Darwin Online

give up the idea that each flash was caused by the direct hand of God.

Farewell, I am feeling very unwell to-day, so no more.

Yours very truly,

C. DARWIN.

C. Darwin to John Lubbock.

Ilkley, Yorkshire,
Tuesday [November 15th, 1859].

MY DEAR LUBBOCK,—I beg pardon for troubling you again. I do not know how I blundered in expressing myself in making you believe that we accepted your kind invitation to Brighton. I meant merely to thank you sincerely for wishing to see such a worn-out old dog as myself. I hardly know when we leave this place,—not under a fortnight, and then we shall wish to rest under our own roof-tree.

I do not think I hardly ever admired a book more than Paley's 'Natural Theology.' I could almost formerly have said it by heart.

I am glad you have got my book, but I fear that you value it far too highly. I should be grateful for any criticisms. I care not for Reviews; but for the opinion of men like you and Hooker and Huxley and Lyell, &c.

Farewell, with our joint thanks to Mrs. Lubbock and yourself. Adios.

C. DARWIN.

*C. Darwin to L. Jenyns.**

Ilkley, Yorkshire,
November 13th, 1859.

MY DEAR JENYNS,—I must thank you for your very kind note forwarded to me from Down. I have been much out of health this summer, and have been hydropathising here for the last six weeks with very little good as yet. I shall stay

* Now Rev. L. Blomefield.

© The Complete Work of Charles Darwin Online

here for another fortnight at least. Please remember that my book is only an abstract, and very much condensed, and, to be at all intelligible, must be carefully read. I shall be very grateful for any criticisms. But I know perfectly well that you will not at all agree with the lengths which I go. It took long years to convert me. I may, of course, be egregiously wrong; but I cannot persuade myself that a theory which explains (as I think it certainly does) several large classes of facts, can be wholly wrong; notwithstanding the several difficulties which have to be surmounted somehow, and which stagger me even to this day.

I wish that my health had allowed me to publish in extenso; if ever I get strong enough I will do so, as the greater part is written out, and of which MS. the present volume is an abstract.

I fear this note will be almost illegible; but I am poorly, and can hardly sit up. Farewell; with thanks for your kind note, and pleasant remembrances of good old days.

Yours very sincerely,

C. DARWIN.

C. Darwin to A. R. Wallace.

Ilkley, November 13th, 1859.

MY DEAR SIR,—I have told Murray to send you by post (if possible) a copy of my book, and I hope that you will receive it at nearly the same time with this note. (N.B. I have got a bad finger, which makes me write extra badly.) If you are so inclined, I should very much like to hear your general impression of the book, as you have thought so profoundly on the subject, and in so nearly the same channel with myself. I hope there will be some little new to you, but I fear not much. Remember it is only an abstract, and very much condensed. God knows what the public will think. No one has read it, except Lyell, with whom I have had much correspondence. Hooker thinks him a complete convert, but

© The Complete Work of Charles Darwin Online

he does not seem so in his letters to me; but is evidently deeply interested in the subject. I do not think your share in the theory will be overlooked by the real judges, as Hooker, Lyell, Asa Gray, &c. I have heard from Mr. Sclater that your paper on the Malay Archipelago has been read at the Linnean Society, and that he was *extremely* much interested by it.

I have not seen one naturalist for six or nine months, owing to the state of my health, and therefore I really have no news to tell you. I am writing this at Ilkley Wells, where I have been with my family for the last six weeks, and shall stay for some few weeks longer. As yet I have profited very little. God knows when I shall have strength for my bigger book.

I sincerely hope that you keep your health; I suppose that you will be thinking of returning* soon with your magnificent collections, and still grander mental materials. You will be puzzled how to publish. The Royal Society fund will be worth your consideration. With every good wish, pray believe me,

Yours very sincerely,
CHARLES DARWIN.

P.S.—I think that I told you before that Hooker is a complete convert. If I can convert Huxley I shall be content.

C. Darwin to W. D. Fox.

Ilkley, Yorkshire,
Wednesday [November 16th, 1859].

. . . . . . I like the place very much, and the children have enjoyed it much, and it has done my wife good. It did H. good at first, but she has gone back again. I have had a series of calamities; first a sprained ankle, and then a badly

* Mr. Wallace was in the Malay Archipelago.

© The Complete Work of Charles Darwin Online

swollen whole leg and face, much rash, and a frightful succession of boils—four or five at once. I have felt quite ill, and have little faith in this "unique crisis," as the doctor calls it, doing me much good. . . . . . You will probably have received, or will very soon receive, my weariful book on species. I naturally believe it mainly includes the truth, but you will not at all agree with me. Dr. Hooker, whom I consider one of the best judges in Europe, is a complete convert, and he thinks Lyell is likewise; certainly, judging from Lyell's letters to me on the subject, he is deeply staggered. Farewell. If the spirit moves you, let me have a line. . . .

C. Darwin to W. B. Carpenter.

Ilkley, Yorkshire,
November 18th [1859].

MY DEAR CARPENTER,—I must thank you for your letter on my own account, and, if I know myself, still more warmly for the subject's sake. As you seem to have understood my last chapter without reading the previous chapters, you must have maturely and most profoundly self-thought out the subject; for I have found the most extraordinary difficulty in making even able men understand at what I was driving. There will be strong opposition to my views. If I am in the main right (of course including partial errors unseen by me), the admission of my views will depend far more on men, like yourself, with well-established reputations, than on my own writings. Therefore, on the supposition that when you have read my volume you think the view in the main true, I thank and honour you for being willing to run the chance of unpopularity by advocating the view. I know not in the least whether any one will review me in any of the Reviews. I do not see how an author could enquire or interfere; but if you are willing to review me anywhere, I am sure from the admiration which I have long felt and expressed for your 'Compara-

© The Complete Work of Charles Darwin Online

tive Physiology,' that your review will be excellently done, and will do good service in the cause for which I think I am not selfishly deeply interested. I am feeling very unwell to-day, and this note is badly, perhaps hardly intelligibly, expressed; but you must excuse me, for I could not let a post pass without thanking you for your note. You will have a tough job even to shake in the slightest degree Sir H. Holland. I do not think (privately I say it) that the great man has knowledge enough to enter on the subject. Pray believe me with sincerity,

Yours truly obliged,
C. DARWIN.

P.S.—As you are not a practical geologist, let me add that Lyell thinks the chapter on the Imperfection of the Geological Record *not* exaggerated.

C. Darwin to W. B. Carpenter.

Ilkley, Yorkshire,
November 19th [1859].

MY DEAR CARPENTER,—I beg pardon for troubling you again. If, after reading my book, you are able to come to a conclusion in any degree definite, will you think me very unreasonable in asking you to let me hear from you. I do not ask for a long discussion, but merely for a brief idea of your general impression. From your widely extended knowledge, habit of investigating the truth, and abilities, I should value your opinion in the very highest rank. Though I, of course, believe in the truth of my own doctrine, I suspect that no belief is vivid until shared by others. As yet I know only one believer, but I look at him as of the greatest authority, viz. Hooker. When I think of the many cases of men who have studied one subject for years, and have persuaded

© The Complete Work of Charles Darwin Online

themselves of the truth of the foolishest doctrines, I feel sometimes a little frightened, whether I may not be one of these monomaniacs.

Again pray excuse this, I fear, unreasonable request. A short note would suffice, and I could bear a hostile verdict, and shall have to bear many a one.

Yours very sincerely,
C. DARWIN.

C. Darwin to J. D. Hooker.

Ilkley, Yorkshire,
Sunday [November, 1859].

MY DEAR HOOKER,—I have just read a review on my book in the *Athenæum*,* and it excites my curiosity much who is the author. If you should hear who writes in the *Athenæum* I wish you would tell me. It seems to me well done, but the reviewer gives no new objections, and, being hostile, passes over every single argument in favour of the doctrine, . . . I fear from the tone of the review, that I have written in a conceited and cocksure style,† which shames me a little. There is another review of which I should like to know the author, viz. of H. C. Watson in the *Gardeners' Chronicle*.‡ Some of the remarks are like yours, and he does deserve punishment; but surely the review is too severe. Don't you think so? . . . .

I have heard from Carpenter, who, I think, is likely to be a convert. Also from Quatrefages, who is inclined to go a long way with us. He says that he exhibited in his lecture a diagram closely like mine!

* Nov. 19, 1859.

† The Reviewer speaks of the author's "evident self-satisfaction," and of his disposing of all difficulties "more or less confidently."

‡ A review of the fourth volume of Watson's 'Cybele Britannica,' *Gard. Chron.*, 1859, p. 911.

© The Complete Work of Charles Darwin Online

I shall stay here one fortnight more, and then go to Down, staying on the road at Shrewsbury a week. I have been very unfortunate: out of seven weeks I have been confined for five to the house. This has been bad for me, as I have not been able to help thinking to a foolish extent about my book. If some four or five *good* men came round nearly to our view, I shall not fear ultimate success. I long to learn what Huxley thinks. Is your Introduction* published? I suppose that you will sell it separately. Please answer this, for I want an extra copy to send away to Wallace. I am very bothersome, farewell.

Yours affectionately,
C. DARWIN.

I was very glad to see the Royal Medal for Mr. Bentham.

C. Darwin to J. D. Hooker.

Down [November 21st, 1859].

MY DEAR HOOKER,—Pray give my thanks to Mrs. Hooker for her extremely kind note, which has pleased me much. We are very sorry she cannot come here, but shall be delighted to see you and W. (our boys will be at home) here in the 2nd week of January, or any other time. I shall much enjoy discussing any points in my book with you. . . .

I hate to hear you abuse your own work. I, on the contrary, so sincerely value all that you have written. It is an old and firm conviction of mine, that the Naturalists who accumulate facts and make many partial generalisations are the *real* benefactors of science. Those who merely accumulate facts I cannot very much respect.

I had hoped to have come up for the Club to-morrow, but very much doubt whether I shall be able. Ilkley seems to have done me no essential good. I attended the Bench on

* Introduction to the 'Flora of Australia.'

© The Complete Work of Charles Darwin Online

Monday, and was detained in adjudicating some troublesome cases $1\frac{1}{2}$ hours longer than usual, and came home utterly knocked up, and cannot rally. I am not worth an old button. . . . . Many thanks for your pleasant note.

Ever yours,

C. DARWIN.

P.S.—I feel confident that for the future progress of the subject of the origin, and manner of formation of species, the assent and arguments and facts of working naturalists, like yourself, are far more important than my own book; so for God's sake do not abuse your Introduction.

H. C. Watson to C. Darwin.

Thames Ditton, November 21st [1859].

MY DEAR SIR,—Once commenced to read the 'Origin,' I could not rest till I had galloped through the whole. I shall now begin to re-read it more deliberately. Meantime I am tempted to write you the first impressions, not doubting that they will, in the main, be the permanent impressions:—

1st. Your leading idea will assuredly become recognised as an established truth in science, *i.e.* "Natural selection." It has the characteristics of all great natural truths, clarifying what was obscure, simplifying what was intricate, adding greatly to previous knowledge. You are the greatest revolutionist in natural history of this century, if not of all centuries.

2nd. You will perhaps need, in some degree, to limit or modify, possibly in some degree also to extend, your present applications of the principle of natural selection. Without going to matters of more detail, it strikes me that there is one considerable primary inconsistency, by one failure in the analogy between varieties and species; another by a sort of barrier assumed for nature on insufficient grounds, and arising from "divergence." These may, however, be faults in my

© The Complete Work of Charles Darwin Online

own mind, attributable to yet incomplete perception of your views. And I had better not trouble you about them before again reading the volume.

3rd. Now these novel views are brought fairly before the scientific public, it seems truly remarkable how so many of them could have failed to see their right road sooner. How could Sir C. Lyell, for instance, for thirty years read, write, and think, on the subject of species *and their succession*, and yet constantly look down the wrong road!

A quarter of a century ago, you and I must have been in something like the same state of mind on the main question. But you were able to see and work out the *quo modo* of the succession, the all-important thing, while I failed to grasp it. I send by this post a little controversial pamphlet of old date—Combe and Scott. If you will take the trouble to glance at the passages scored on the margin, you will see that, a quarter of a century ago, I was also one of the few who then doubted the absolute distinctness of species, and special creations of them. Yet I, like the rest, failed to detect the *quo modo* which was reserved for your penetration to *discover*, and your discernment to *apply*.

You answered my query about the hiatus between Satyrus and Homo as was expected. The obvious explanation really never occurred to me till some months after I had read the papers in the 'Linnean Proceedings.' The first species of *Fere-homo* * would soon make direct and exterminating war upon his *Infra-homo* cousins. The gap would thus be made, and then go on increasing, into the present enormous and still widening hiatus. But how greatly this, with your chronology of animal life, will shock the ideas of many men!

Very sincerely,

HEWETT C. WATSON.

* "Almost-man."

© The Complete Work of Charles Darwin Online

J. D. Hooker to C. Darwin.

Athenæum, Monday [Nov. 21, 1859].

MY DEAR DARWIN,—I am a sinner not to have written you ere this, if only to thank you for your glorious book—what a mass of close reasoning on curious facts and fresh phenomena—it is capitally written, and will be very successful. I say this on the strength of two or three plunges into as many chapters, for I have not yet attempted to read it. Lyell, with whom we are staying, is perfectly enchanted, and is absolutely gloating over it. I must accept your compliment to me, and acknowledgment of supposed assistance from me, as the warm tribute of affection from an honest (though deluded) man, and furthermore accept it as very pleasing to my vanity; but, my dear fellow, neither my name nor my judgment nor my assistance deserved any such compliments, and if I am dishonest enough to be pleased with what I don't deserve, it must just pass. How different the *book* reads from the MS. I see I shall have much to talk over with you. Those lazy printers have not finished my luckless Essay; which, beside your book, will look like a ragged handkerchief beside a Royal Standard . . .

All well, ever yours affectionately,

JOS. D. HOOKER.

C. Darwin to J. D. Hooker.

Ilkley, Yorkshire [November, 1859].

MY DEAR HOOKER,—I cannot help it, I must thank you for your affectionate and most kind note. My head will be turned. By Jove, I must try and get a bit modest. I was a little chagrined by the review.* I hope it was *not* ——.

* This refers to the review in the *Athenæum*, Nov. 19, 1859, where the reviewer, after touching on the theological aspects of the book, leaves the author to "the mercies of the Divinity Hall, the College, the Lecture Room, and the Museum."

© The Complete Work of Charles Darwin Online

As advocate, he might think himself justified in giving the argument only on one side. But the manner in which he drags in immortality, and sets the priests at me, and leaves me to their mercies, is base. He would, on no account, burn me, but he will get the wood ready, and tell the black beasts how to catch me. . . . It would be unspeakably grand if Huxley were to lecture on the subject, but I can see this is a mere chance; Faraday might think it too unorthodox.

. . . I had a letter from [Huxley] with such tremendous praise of my book, that modesty (as I am trying to cultivate that difficult herb) prevents me sending it to you, which I should have liked to have done, as he is very modest about himself.

You have cockered me up to that extent, that I now feel I can face a score of savage reviewers. I suppose you are still with the Lyells. Give my kindest remembrance to them. I triumph to hear that he continues to approve.

Believe me, your would-be modest friend,

C. D.

C. Darwin to C. Lyell.

Ilkley Wells, Yorkshire,
November 23rd [1859].

MY DEAR LYELL,—You seemed to have worked admirably on the species question; there could not have been a better plan than reading up on the opposite side. I rejoice profoundly that you intend admitting the doctrine of modification in your new edition;* nothing, I am convinced, could be more important for its success. I honour you most sincerely. To have maintained in the position of a master, one side of a question for thirty years, and then deliberately give it up, is a

* It appears from Sir Charles Lyell's published letters that he intended to admit the doctrine of evolution in a new edition of the 'Manual,' but this was not published till 1865. He was, however, at work on the 'Antiquity of Man' in 1860, and had already determined to discuss the 'Origin' at the end of the book.

© The Complete Work of Charles Darwin Online

fact to which I much doubt whether the records of science offer a parallel. For myself, also, I rejoice profoundly; for, thinking of so many cases of men pursuing an illusion for years, often and often a cold shudder has run through me, and I have asked myself whether I may not have devoted my life to a phantasy. Now I look at it as morally impossible that investigators of truth, like you and Hooker, can be wholly wrong, and therefore I rest in peace. Thank you for criticisms, which, if there be a second edition, I will attend to. I have been thinking that if I am much execrated as an atheist, &c., whether the admission of the doctrine of natural selection could injure your works; but I hope and think not, for, as far as I can remember, the virulence of bigotry is expended on the first offender, and those who adopt his views are only pitied as deluded, by the wise and cheerful bigots.

I cannot help thinking that you overrate the importance of the multiple origin of dogs. The only difference is, that in the case of single origins, all difference of the races has originated since man domesticated the species. In the case of multiple origins, part of the difference was produced under natural conditions. I should *infinitely* prefer the theory of single origin in all cases, if facts would permit its reception. But there seems to me some *à priori* improbability (seeing how fond savages are of taming animals), that throughout all times, and throughout all the world, man should have domesticated one single species alone, of the widely distributed genus Canis. Besides this, the close resemblance of at least three kinds of American domestic dogs to wild species still inhabiting the countries where they are now domesticated, seems to almost compel admission that more than one wild Canis has been domesticated by man.

I thank you cordially for all the generous zeal and interest you have shown about my book, and I remain, my dear Lyell,

Your affectionate friend and disciple,

CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

Sir J. Herschel, to whom I sent a copy, is going to read my book. He says he leans to the side opposed to me. If you should meet him after he has read me, pray find out what he thinks, for, of course, he will not write; and I should excessively like to hear whether I produce any effect on such a mind.

T. H. Huxley to C. Darwin.

Jermyn Street, W.,
November 23rd, 1859.

MY DEAR DARWIN,—I finished your book yesterday, a lucky examination having furnished me with a few hours of continuous leisure.

Since I read Von Bär's * essays, nine years ago, no work on Natural History Science I have met with has made so great an impression upon me, and I do most heartily thank you for the great store of new views you have given me. Nothing, I think, can be better than the tone of the book, it impresses those who know nothing about the subject. As for your doctrine, I am prepared to go to the stake, if requisite, in support of Chapter IX., and most parts of Chapters X., XI., XII., and Chapter XIII. contains much that is most admirable, but on one or two points I enter a *caveat* until I can see further into all sides of the question.

As to the first four chapters, I agree thoroughly and fully with all the principles laid down in them. I think you have demonstrated a true cause for the production of species, and have thrown the *onus probandi,* that species did not arise in the way you suppose, on your adversaries.

But I feel that I have not yet by any means fully realized the bearings of those most remarkable and original

* Karl Ernst von Baer, b. 1792, d. at Dorpat 1876—one of the most distinguished biologists of the century. He practically founded the modern science of embryology.

© The Complete Work of Charles Darwin Online

Chapters III., IV. and V., and I will write no more about them just now.

The only objections that have occurred to me are, 1st that you have loaded yourself with an unnecessary difficulty in adopting *Natura non facit saltum* so unreservedly. . . . And 2nd, it is not clear to me why, if continual physical conditions are of so little moment as you suppose, variation should occur at all.

However, I must read the book two or three times more before I presume to begin picking holes.

I trust you will not allow yourself to be in any way disgusted or annoyed by the considerable abuse and misrepresentation which, unless I greatly mistake, is in store for you. Depend upon it you have earned the lasting gratitude of all thoughtful men. And as to the curs which will bark and yelp, you must recollect that some of your friends, at any rate, are endowed with an amount of combativeness which (though you have often and justly rebuked it) may stand you in good stead.

I am sharpening up my claws and beak in readiness.

Looking back over my letter, it really expresses so feebly all I think about you and your noble book that I am half ashamed of it; but you will understand that, like the parrot in the story, "I think the more."

Ever yours faithfully,

T. H. HUXLEY.

C. Darwin to T. H. Huxley.

Ilkley, Nov. 25 [1859].

MY DEAR HUXLEY,—Your letter has been forwarded to me from Down. Like a good Catholic who has received extreme unction, I can now sing "nunc dimittis." I should have been more than contented with one quarter of what you have said. Exactly fifteen months ago, when I put pen to

© The Complete Work of Charles Darwin Online

paper for this volume, I had awful misgivings; and thought perhaps I had deluded myself, like so many have done, and I then fixed in my mind three judges, on whose decision I determined mentally to abide. The judges were Lyell, Hooker, and yourself. It was this which made me so excessively anxious for your verdict. I am now contented, and can sing my "nunc dimittis." What a joke it would be if I pat you on the back when you attack some immovable creationists! You have most cleverly hit on one point, which has greatly troubled me; if, as I must think, external conditions produce little *direct* effect, what the devil determines each particular variation? What makes a tuft of feathers come on a cock's head, or moss on a moss-rose? I shall much like to talk over this with you. . . .

My dear Huxley, I thank you cordially for your letter.

Yours very sincerely,

C. DARWIN.

P.S.—Hereafter I shall be particularly curious to hear what you think of my explanation of Embryological similarity. On classification I fear we shall split. Did you perceive the *argumentum ad hominem* Huxley about the kangaroo and bear?

Erasmus Darwin to C. Darwin.

November 23rd [1859].

DEAR CHARLES,—I am so much weaker in the head, that I hardly know if I can write, but at all events I will jot down a few things that the Dr.* has said. He has not read much above half, so as he says he can give no definite conclusion, and it is my private belief he wishes to remain in that state. . . . He is evidently in a dreadful state of indecision, and keeps stating that he is not tied down to either view, and that he has always left an escape by the way he has spoken of

* Dr., afterwards Sir Henry Holland.

© The Complete Work of Charles Darwin Online

varieties. I happened to speak of the eye before he had read that part, and it took away his breath—utterly impossible—structure—function, &c., &c., &c., but when he had read it he hummed and hawed, and perhaps it was partly conceivable, and then he fell back on the bones of the ear, which were beyond all probability or conceivability. He mentioned a slight blot, which I also observed, that in speaking of the slave-ants carrying one another, you change the species without giving notice first, and it makes one turn back. . . .

. . . For myself I really think it is the most interesting book I ever read, and can only compare it to the first knowledge of chemistry, getting into a new world or rather behind the scenes. To me the geographical distribution, I mean the relation of islands to continents is the most convincing of the proofs, and the relation of the oldest forms to the existing species. I dare say I don't feel enough the absence of varieties, but then I don't in the least know if everything now living were fossilized whether the palæontologists could distinguish them. In fact the *à priori* reasoning is so entirely satisfactory to me that if the facts won't fit in, why so much the worse for the facts is my feeling. My ague has left me in such a state of torpidity that I wish I had gone through the process of natural selection.

Yours affectionately,

E. A. D.

C. Darwin to C. Lyell.

Ilkley, November [24th, 1859].

MY DEAR LYELL,—Again I have to thank you for a most valuable lot of criticisms in a letter dated 22nd.

This morning I heard also from Murray that he sold the whole edition * the first day to the trade. He wants a new edition instantly, and this utterly confounds me. Now, under

* First edition, 1250 copies.

© The Complete Work of Charles Darwin Online

water-cure, with all nervous power directed to the skin, I cannot possibly do head-work, and I must make only actually necessary corrections. But I will, as far as I can without my manuscript, take advantage of your suggestions: I must not attempt much. Will you send me one line to say whether I must strike out about the secondary whale,* it goes to my heart. About the rattle-snake, look to my Journal, under Trigonocephalus, and you will see the probable origin of the rattle, and generally in transitions it is the *premier pas qui coûte.*

Madame Belloc wants to translate my book into French; I have offered to look over proofs for *scientific* errors. Did you ever hear of her? I believe Murray has agreed at my urgent advice, but I fear I have been rash and premature. Quatrefages has written to me, saying he agrees largely with my views. He is an excellent naturalist. I am pressed for time. Will you give us one line about the whales? Again I thank you for never-tiring advice and assistance; I do in truth reverence your unselfish and pure love of truth.

My dear Lyell, ever yours,

C. DARWIN.

[With regard to a French translation, he wrote to Mr. Murray in Nov. 1859: "I am *extremely* anxious, for the subject's sake (and God knows not for mere fame), to have my book translated; and indirectly its being known abroad will do good to the English sale. If it depended on me, I should agree without payment, and instantly send a copy, and only beg that she [Mme. Belloc] would get some scientific man to look over the translation. . . . You might say that, though I am a very poor French scholar, I could detect any scientific mistake, and would read over the French proofs."

The proposed translation was not made, and a second plan fell through in the following year. He wrote to M. de

* The passage was omitted in the second edition.

© The Complete Work of Charles Darwin Online

Quatrefages: "The gentleman who wished to translate my 'Origin of Species' has failed in getting a publisher. Baillière, Masson, and Hachette all rejected it with contempt. It was foolish and presumptuous in me, hoping to appear in a French dress; but the idea would not have entered my head had it not been suggested to me. It is a great loss. I must console myself with the German edition which Prof. Bronn is bringing out." *

A sentence in another letter to M. de Quatrefages shows how anxious he was to convert one of the greatest of contemporary Zoologists: "How I should like to know whether Milne-Edwards has read the copy which I sent him, and whether he thinks I have made a pretty good case on our side of the question. There is no naturalist in the world for whose opinion I have so profound a respect. Of course I am not so silly as to expect to change his opinion."]

C. Darwin to C. Lyell.

Ilkley, [November 25th, 1859].

MY DEAR LYELL,—I have received your letter of the 24th. It is no use trying to thank you; your kindness is beyond thanks. I will certainly leave out the whale and bear . . .

The edition was 1250 copies. When I was in spirits, I sometimes fancied that my book would be successful, but I never even built a castle in the air of such success as it has met with; I do not mean the sale, but the impression it has made on you (whom I have always looked at as chief judge) and Hooker and Huxley. The whole has infinitely exceeded my wildest hopes.

Farewell, I am tired, for I have been going over the sheets.

My kind friend, farewell, yours,

C. DARWIN.

* See letters to Bronn, p. 276.

© The Complete Work of Charles Darwin Online

C. Darwin to C. Lyell.

Ilkley, Yorkshire,
December 2nd [1859].

MY DEAR LYELL,—Every note which you have sent me has interested me much. Pray thank Lady Lyell for her remark.. In the chapters she refers to, I was unable to modify the passage in accordance to your suggestion; but in the final chapter I have modified three or four. Kingsley, in a note * to me, had a capital paragraph on such notions as mine being *not* opposed to a high conception of the Deity. I have inserted it as an extract from a letter to me from a celebrated author and divine. I have put in about nascent organs. I had the greatest difficulty in partially making out Sedgwick's letter, and I dare say I did greatly underrate its clearness. Do what I could, I fear I shall be greatly abused. In answer to Sedgwick's remark that my book would be "mischievous," I asked him whether truth can be known except by being victorious over all attacks. But it is no use. H. C. Watson tells me that one zoologist says he will read my book, "but I will never believe it." What a spirit to read any book in! Crawford writes to me that his notice † will be hostile, but that "he will not calumniate the author." He says he has read my book, "at least such parts as he could understand." He sent me some notes and suggestions (quite unimportant), and they show me that I have unavoidably done harm to the subject, by publishing an abstract. He is a real Pallasian; nearly all our domestic races descended from a multitude of wild species now commingled. I expected Murchison to be outrageous.

* The letter is given at Vol. II. p. 287.

† John Crawford, orientalist, ethnologist, &c., b. 1783, d. 1868. The review appeared in the *Examiner*, and, though hostile, is free from bigotry, as the following citation will show: "We cannot help saying that piety must be fastidious indeed that objects to a theory the tendency of which is to show that all organic beings, man included, are in a perpetual progress of amelioration, and that is expounded in the reverential language which we have quoted."

© The Complete Work of Charles Darwin Online

How little he could ever have grappled with the subject of denudation! How singular so great a geologist should have so unphilosophical a mind! I have had several notes from ——, very civil and less decided. Says he shall not pronounce against me without much reflection, *perhaps will say nothing* on the subject. X. says he will go to that part of hell, which Dante tells us is appointed for those who are neither on God's side nor on that of the devil.

I fully believe that I owe the comfort of the next few years of my life to your generous support, and that of a very few others. I do not think I am brave enough to have stood being odious without support; now I feel as bold as a lion. But there is one thing I can see I must learn, viz. to think less of myself and my book. Farewell, with cordial thanks,

Yours most truly,

C. DARWIN.

I return home on the 7th, and shall sleep at Erasmus's. I will call on you about ten o'clock, on Thursday, the 8th, and sit with you, as I have so often sat, during your breakfast.

[In December there appeared in 'Macmillan's Magazine' an article, "Time and Life," by Professor Huxley. It is mainly occupied by an analysis of the argument of the 'Origin,' but it also gives the substance of a lecture delivered at the Royal Institution before that book was published. Professor Huxley spoke strongly in favour of evolution in his Lecture, and explains that in so doing he was to a great extent resting on a knowledge of "the general tenor of the researches in which Mr. Darwin had been so long engaged," and was supported in so doing by his perfect confidence in his knowledge, perseverance, and "high-minded love of truth." He was evidently deeply pleased by Mr. Huxley's words, and wrote:

"I must thank you for your extremely kind notice of my book in 'Macmillan.' No one could receive a more delightful

© The Complete Work of Charles Darwin Online

and honourable compliment. I had not heard of your Lecture, owing to my retired life. You attribute much too much to me from our mutual friendship. You have explained my leading idea with admirable clearness. What a gift you have of writing (or more properly thinking) clearly."]

C. Darwin to W. B. Carpenter.

Ilkley, Yorkshire,
December 3rd [1859].

MY DEAR CARPENTER,—I am perfectly delighted at your letter. It is a great thing to have got a great physiologist on our side. I say "our" for we are now a good and compact body of really good men, and mostly not old men. In the long-run we shall conquer. I do not like being abused, but I feel that I can now bear it; and, as I told Lyell, I am well convinced that it is the first offender who reaps the rich harvest of abuse. You have done an essential kindness in checking the odium theologicum in the [?].* It much pains all one's female relations and injures the cause.

I look at it as immaterial whether we go quite the same lengths; and I suspect, judging from myself, that you will go further, by thinking of a population of forms like Ornithorhynchus, and by thinking of the common homological and embryological structure of the several vertebrate orders. But this is immaterial. I quite agree that the principle is everything. In my fuller MS. I have discussed a good many instincts; but there will surely be more unfilled gaps here than with corporeal structure, for we have no fossil instincts, and know scarcely any except of European animals. When I reflect how very slowly I came round myself, I am in truth astonished at the candour shown by Lyell, Hooker, Huxley,

* This must refer to Carpenter's critique, which would now have been ready to appear in the January number of the 'National Review,' 1860, and in which the *odium theologicum* is referred to.

© The Complete Work of Charles Darwin Online

and yourself. In my opinion it is grand. I thank you cordially for taking the trouble of writing a review for the 'National.' God knows I shall have few enough in any degree favourable.*

C. Darwin to C. Lyell.

Saturday [December 5th, 1859].

. . . I have had a letter from Carpenter this morning. He reviews me in the 'National.' He is a convert, but does not go quite so far as I, but quite far enough, for he admits that all birds are from one progenitor, and probably all fishes and reptiles from another parent. But the last mouthful chokes him. He can hardly admit all vertebrates from one parent. He will surely come to this from Homology and Embryology. I look at it as grand having brought round a great physiologist, for great I think he certainly is in that line. How curious I shall be to know what line Owen will take: dead against us, I fear; but he wrote me a most liberal note on the reception of my book, and said he was quite prepared to consider fairly and without prejudice my line of argument.

C. Darwin to C. Lyell.

Down, Saturday [December 12th, 1859].

. . . I had very long interviews with ——, which perhaps you would like to hear about. . . . I infer from several expressions that, at bottom, he goes an immense way with us. . . . .

He said to the effect that my explanation was the best ever published of the manner of formation of species. I said I was very glad to hear it. He took me up short: "You must not at all suppose that I agree with you in all respects." I said I thought it no more likely that I should be right in

* See a letter to Dr. Carpenter, Vol. II. p. 262.

© The Complete Work of Charles Darwin Online

nearly all points, than that I should toss up a penny and get heads twenty times running. I asked him what he thought the weakest part. He said he had no particular objection to any part. He added :—

"If I must criticise, I should say, we do not want to know what Darwin believes and is convinced of, but what he can prove." I agreed most fully and truly that I have probably greatly sinned in this line, and defended my general line of argument of inventing a theory and seeing how many classes of facts the theory would explain. I added that I would endeavour to modify the "believes" and "convinceds." He took me up short: "You will then spoil your book, the charm of (!) it is that it is Darwin himself." He added another objection, that the book was too *teres atque rotundus*—that it explained everything, and that it was improbable in the highest degree that I should succeed in this. I quite agree with this rather queer objection, and it comes to this that my book must be very bad or very good. . . .

I have heard, by a roundabout channel, that Herschel says my book "is the law of higgledy-piggledy." What this exactly means I do not know, but it is evidently very contemptuous. If true this is a great blow and discouragement.

C. Darwin to John Lubbock.

December 14th [1859].

. . . The latter part of my stay at Ilkley did me much good, but I suppose I never shall be strong, for the work I have had since I came back has knocked me up a little more than once. I have been busy in getting a reprint (with a very few corrections) through the press.

My book has been as yet *very much* more successful than I ever dreamed of: Murray is now printing 3000 copies. Have you finished it? If so, pray tell me whether you are

© The Complete Work of Charles Darwin Online

with me on the *general* issue, or against me. If you are against me, I know well how honourable, fair, and candid an opponent I shall have, and which is a good deal more than I can say of all my opponents. . . .

Pray tell me what you have been doing. Have you had time for any Natural History? . . .

P.S.—I have got—I wish and hope I might say that *we* have got—a fair number of excellent men on our side of the question on the mutability of species.

J. D. Hooker to C. Darwin.

Kew [1859].

DEAR DARWIN,—You have, I know, been drenched with letters since the publication of your book, and I have hence forborne to add my mite.* I hope now that you are well through Edition II., and I have heard that you were flourishing in London. I have not yet got half-through the book, not from want of will, but of time—for it is the very hardest book to read, to full profits, that I ever tried—it is so cram-full of matter and reasoning. I am all the more glad that you have published in this form, for the three volumes, unprefaced by this, would have choked any Naturalist of the nineteenth century, and certainly have softened my brain in the operation of assimilating their contents. I am perfectly tired of marvelling at the wonderful amount of facts you have brought to bear, and your skill in marshalling them and throwing them on the enemy; it is also extremely clear as far as I have gone, but very hard to fully appreciate. Somehow it reads very different from the MS., and I often fancy that I must have been very stupid not to have more fully followed it in MS. Lyell told me of his criticisms. I did not appreciate them all, and there are many little matters I hope one day to talk over with you. I saw a highly flattering notice

* See, however, Vol. II. p. 228.

© The Complete Work of Charles Darwin Online

in the 'English Churchman,' short and not at all entering into discussion, but praising you and your book, and talking patronizingly of the doctrine! . . . Bentham and Henslow will still shake their heads, I fancy. . . .

Ever yours affectionately,

JOS. D. HOOKER.

C. Darwin to J. D. Hooker.

Down, December 14th [1859].

MY DEAR HOOKER,—Your approval of my book, for many reasons, gives me intense satisfaction; but I must make some allowance for your kindness and sympathy. Any one with ordinary faculties, if he had *patience* enough and plenty of time, could have written my book. You do not know how I admire your and Lyell's generous and unselfish sympathy; I do not believe either of you would have cared so much about your own work. My book, as yet, has been far more successful than I ever even formerly ventured in the wildest day-dreams to anticipate. We shall soon be a good body of working men, and shall have, I am convinced, all young and rising naturalists on our side. I shall be intensely interested to hear whether my book produces any effect on A. Gray; from what I heard at Lyell's, I fancy your correspondence has brought him some way already. I fear that there is no chance of Bentham being staggered. Will he read my book? Has he a copy? I would send him one of the reprints if he has not. Old J. E. Gray,* at the British Museum, attacked me in fine style: "You have just reproduced Lamarck's doc-

* John Edward Gray (born 1800, died 1875) was the son of S. F. Gray, author of the 'Supplement to the Pharmacopœia.' In 1821 he published in his father's name 'The Natural Arrangement of British Plants,' one of the earliest works in English on the natural method. In 1824 he became connected with the Natural History Department of the British Museum, and was appointed Keeper of the Zoological collections in 1840. He was the author of 'Illustrations of Indian Zoology,' 'The Knowsley Menagerie,' &c., and of innumerable descriptive Zoological papers.

© The Complete Work of Charles Darwin Online

trine, and nothing else, and here Lyell and others have been attacking him for twenty years, and because *you* (with a sneer and laugh) say the very same thing, they are all coming round; it is the most ridiculous inconsistency, &c. &c."

You must be very glad to be settled in your house, and I hope all the improvements satisfy you. As far as my experience goes, improvements are never perfection. I am very sorry to hear that you are still so very busy, and have so much work. And now for the main purport of my note, which is to ask and beg you and Mrs. Hooker (whom it is really an age since I have seen), and all your children, if you like, to come and spend a week here. It would be a great pleasure to me and to my wife. . . . As far as we can see, we shall be at home all the winter; and all times probably would be equally convenient; but if you can, do not put it off very late, as it may slip through. Think of this and persuade Mrs. Hooker, and be a good man and come.

Farewell, my kind and dear friend,

Yours affectionately,

C. DARWIN.

P.S.—I shall be very curious to hear what you think of my discussion on Classification in Chap. XIII.; I believe Huxley demurs to the whole, and says he has nailed his colours to the mast, and I would sooner die than give up; so that we are in as fine a frame of mind to discuss the point as any two religionists.

Embryology is my pet bit in my book, and, confound my friends, not one has noticed this to me.

C. Darwin to Asa Gray.

Down, December 21st [1859].

MY DEAR GRAY,—I have just received your most kind, long, and valuable letter. I will write again in a few days, for I am at present unwell and much pressed with business:

© The Complete Work of Charles Darwin Online

to-day's note is merely personal. I should, for several reasons, be very glad of an American Edition. I have made up my mind to be well abused; but I think it of importance that my notions should be read by intelligent men, accustomed to scientific argument, though *not* naturalists. It may seem absurd, but I think such men will drag after them those naturalists who have too firmly fixed in their heads that a species is an entity. The first edition of 1250 copies was sold on the first day, and now my publisher is printing off, as *rapidly as possible*, 3000 more copies. I mention this solely because it renders probable a remunerative sale in America. I should be infinitely obliged if you could aid an American reprint; and could make, for my sake and the publisher's, any arrangement for any profit. The new edition is only a reprint, yet I have made a *few* important corrections. I will have the clean sheets sent over in a few days of as many sheets as are printed off, and the remainder afterwards, and you can do anything you like,—if nothing, there is no harm done. I should be glad for the new edition to be reprinted and not the old.—In great haste, and with hearty thanks,

Yours very sincerely,

C. DARWIN.

I will write soon again.

C. Darwin to C. Lyell.

Down, 22nd [December, 1859].

MY DEAR LYELL,—Thanks about "Bears," * a word of ill-omen to me.

I am too unwell to leave home, so shall not see you.

I am very glad of your remarks on Hooker.† I have not yet

* See 'Origin,' ed. i., p. 184.

† Sir C. Lyell wrote to Sir J. D. Hooker, Dec. 19, 1859 ('Life,' ii. p. 327): "I have just finished the reading of your splendid Essay [the 'Flora of Australia'] on the origin of species, as illustrated by your wide botanical experience, and think it goes very far to raise the variety-making hypothesis to the rank of a theory, as accounting for the manner in which new species enter the world."

© The Complete Work of Charles Darwin Online

got the Essay. The parts which I read in sheets seemed to me grand, especially the generalization about the Australian flora itself. How superior to Robert Brown's celebrated essay! I have not seen Naudin's paper,* and shall not be able till I hunt the libraries. I am very anxious to see it. Decaisne seems to think he gives my whole theory. I do not know when I shall have time and strength to grapple with Hooker. . . .

P.S.—I have heard from Sir W. Jardine :† his criticisms are quite unimportant ; some of the Galapagos so-called species ought to be called varieties, which I fully expected ; some of the sub-genera, thought to be wholly endemic, have been found on the Continent (not that he gives his authority), but I do not make out that the species are the same. His letter is brief and vague, but he says he will write again.

C. Darwin to J. D. Hooker.

Down [23rd December, 1859].

MY DEAR HOOKER,—I received last night your 'Introduction,' for which very many thanks ; I am surprised to see

* 'Revue Horticole,' 1852. See Historical Sketch in the later editions of the 'Origin of Species.'

† Jardine, Sir William, Bart., b. 1800, d. 1874, was the son of Sir A. Jardine of Applegarth, Dumfriesshire. He was educated at Edinburgh, and succeeded to the title on his father's decease in 1821. He published, jointly with Mr. Prideaux J. Selby, Sir Stamford Raffles, Dr. Horsfield, and other ornithologists, 'Illustrations of Ornithology,' and edited the 'Naturalist's Library,' in 40 vols. which included the four branches : Mammalia, Ornithology, Ichthyology, and Entomology. Of these 40 vols. 14 were written by himself. In 1836 he became editor of the 'Magazine of Zoology and Botany,' which, two years later, was transformed into 'Annals of Natural History,' but remained under his direction. For Bohn's Standard Library he edited White's 'Natural History of Selborne.' Sir W. Jardine was also joint editor of the 'Edinburgh Philosophical Journal,' and was author of 'British Salmonidae,' 'Ichthyology of Annandale,' 'Memoirs of the late Hugh Strickland,' 'Contributions to Ornithology,' 'Ornithological Synonyms,' &c. —(Taken from Ward, 'Men of the Reign,' and Cates, 'Dictionary of General Biography.')

© The Complete Work of Charles Darwin Online

how big it is: I shall not be able to read it very soon. It was very good of you to send Naudin, for I was very curious to see it. I am surprised that Decaisne should say it was the same as mine. Naudin gives artificial selection, as well as a score of English writers, and when he says species were formed in the same manner, I thought the paper would certainly prove exactly the same as mine. But I cannot find one word like the struggle for existence and natural selection. On the contrary, he brings in his principle (p. 103) of finality (which I do not understand), which, he says, with some authors is fatality, with others providence, and which adapts the forms of every being, and harmonises them all throughout nature.

He assumes like old geologists (who assumed that the forces of nature were formerly greater), that species were at first more plastic. His simile of tree and classification is like mine (and others), but he cannot, I think, have reflected much on the subject, otherwise he would see that genealogy by itself does not give classification; I declare I cannot see a *much* closer approach to Wallace and me in Naudin than in Lamarck—we all agree in modification and descent. If I do not hear from you I will return the 'Revue' in a few days (with the cover). I dare say Lyell would be glad to see it. By the way, I will retain the volume till I hear whether I shall or not send it to Lyell. I should rather like Lyell to see this note, though it is foolish work sticking up for independence or priority.

Ever yours,
C. DARWIN.

A. Sedgwick to C. Darwin.*

Cambridge, December 24th, 1859.

MY DEAR DARWIN,—I write to thank you for your work on the 'Origin of Species.' It came, I think, in the latter part

* Rev. Adam Sedgwick, Woodwardian Professor of Geology in the University of Cambridge. Born 1785, died 1873.

© The Complete Work of Charles Darwin Online

of last week; but it *may* have come a few days sooner, and been overlooked among my book-parcels, which often remain unopened when I am lazy or busy with any work before me. So soon as I opened it I began to read it, and I finished it, after many interruptions, on Tuesday. Yesterday I was employed—1st, in preparing for my lecture; 2ndly, in attending a meeting of my brother Fellows to discuss the final propositions of the Parliamentary Commissioners; 3rdly, in lecturing; 4thly, in hearing the conclusion of the discussion and the College reply, whereby, in conformity with my own wishes, we accepted the scheme of the Commissioners; 5thly, in dining with an old friend at Clare College; 6thly, in adjourning to the weekly meeting of the Ray Club, from which I returned at 10 P.M., dog-tired, and hardly able to climb my staircase. Lastly, in looking through the *Times* to see what was going on in the busy world.

I do not state this to fill space (though I believe that Nature does abhor a vacuum), but to prove that my reply and my thanks are sent to you by the earliest leisure I have, though that is but a very contracted opportunity. If I did not think you a good-tempered and truth-loving man, I should not tell you that (spite of the great knowledge, store of facts, capital views of the correlation of the various parts of organic nature, admirable hints about the diffusion, through wide regions, of many related organic beings, &c. &c.) I have read your book with more pain than pleasure. Parts of it I admired greatly, parts I laughed at till my sides were almost sore; other parts I read with absolute sorrow, because I think them utterly false and grievously mischievous. You have *deserted*—after a start in that tram-road of all solid physical truth—the true method of induction, and started us in machinery as wild, I think, as Bishop Wilkins's locomotive that was to sail with us to the moon. Many of your wide conclusions are based upon assumptions which can neither be proved nor disproved, why then express them in the language and arrangement

© The Complete Work of Charles Darwin Online

of philosophical induction? As to your grand principle—*natural selection*—what is it but a secondary consequence of supposed, or known, primary facts? Development is a better word, because more close to the cause of the fact? For you do not deny causation. I call (in the abstract) causation the will of God; and I can prove that He acts for the good of His creatures. He also acts by laws which we can study and comprehend. Acting by law, and under what is called final causes, comprehends, I think, your whole principle. You write of "natural selection" as if it were done consciously by the selecting agent. 'Tis but a consequence of the presupposed development, and the subsequent battle for life. This view of nature you have stated admirably, though admitted by all naturalists and denied by no one of common sense. We all admit development as a fact of history: but how came it about? Here, in language, and still more in logic, we are point-blank at issue. There is a moral or metaphysical part of nature as well as a physical. A man who denies this is deep in the mire of folly. 'Tis the crown and glory of organic science that it *does* through *final cause*, link material and moral; and yet *does not* allow us to mingle them in our first conception of laws, and our classification of such laws, whether we consider one side of nature or the other. You have ignored this link; and, if I do not mistake your meaning, you have done your best in one or two pregnant cases to break it. Were it possible (which, thank God, it is not) to break it, humanity, in my mind, would suffer a damage that might brutalize it, and sink the human race into a lower grade of degradation than any into which it has fallen since its written records tell us of its history. Take the case of the bee-cells. If your development produced the successive modification of the bee and its cells (which no mortal can prove), final cause would stand good as the directing cause under which the successive generations acted and gradually improved. Passages in your book, like that to which I have

© The Complete Work of Charles Darwin Online

alluded (and there are others almost as bad), greatly shocked my moral taste. I think, in speculating on organic descent, you *over*-state the evidence of geology; and that you *under*-state it while you are talking of the broken links of your natural pedigree: but my paper is nearly done, and I must go to my lecture-room. Lastly, then, I greatly dislike the concluding chapter—not as a summary, for in that light it appears good—but I dislike it from the tone of triumphant confidence in which you appeal to the rising generation (in a tone I condemned in the author of the 'Vestiges') and prophecy of things not yet in the womb of time, nor (if we are to trust the accumulated experience of human sense and the inferences of its logic) ever likely to be found anywhere but in the fertile womb of man's imagination. And now to say a word about a son of a monkey and an old friend of yours: I am better, far better, than I was last year. I have been lecturing three days a week (formerly I gave six a week) without much fatigue, but I find by the loss of activity and memory, and of all productive powers, that my bodily frame is sinking slowly towards the earth. But I have visions of the future. They are as much a part of myself as my stomach and my heart, and these visions are to have their antitype in solid fruition of what is best and greatest. But on one condition only—that I humbly accept God's revelation of Himself both in His works and in His word, and do my best to act in conformity with that knowledge which He only can give me, and He only can sustain me in doing. If you and I do all this, we shall meet in heaven.

I have written in a hurry, and in a spirit of brotherly love, therefore forgive any sentence you happen to dislike; and believe me, spite of any disagreement in some points of the deepest moral interest, your true-hearted old friend,

A. SEDGWICK.

© The Complete Work of Charles Darwin Online

C. Darwin to T. H. Huxley.

Down, Dec. 25th [1859].

My dear Huxley,—One part of your note has pleased me so much that I must thank you for it. Not only Sir H. H. [Holland], but several others, have attacked me about analogy leading to belief in one primordial *created* form.* (By which I mean only that we know nothing as yet [of] how life originates.) I thought I was universally condemned on this head. But I answered that though perhaps it would have been more prudent not to have put it in, I would not strike it out, as it seemed to me probable, and I give it on no other grounds. You will see in your mind the kind of arguments which made me think it probable, and no one fact had so great an effect on me as your most curious remarks on the apparent homologies of the head of Vertebrata and Articulata.

You have done a real good turn in the Agency business † (I never before heard of a hard-working, unpaid agent besides yourself), in talking with Sir H. H., for he will have great influence over many. He floored me from my ignorance about the bones of the ear, and I made a mental note to ask you what the facts were.

With hearty thanks and real admiration for your generous zeal for the subject.

Yours most truly,

C. Darwin.

You may smile about the care and precautions I have taken about my ugly MS.; ‡ it is not so much the value I set on

* 'Origin,' edit. i. p. 484.—"Therefore I should infer from analogy that probably all the organic beings which have ever lived on this earth have descended from some one primordial form, into which life was first breathed."

† "My General Agent" was a sobriquet applied at this time by my father to Mr. Huxley.

‡ Manuscript left with Mr. Huxley for his perusal.

© The Complete Work of Charles Darwin Online

them, but the remembrance of the intolerable labour—for instance, in tracing the history of the breeds of pigeons.

C. Darwin to J. D. Hooker.

Down, 25th [December, 1859].

. . . I shall not write to Decaisne;* I have always had a strong feeling that no one had better defend his own priority. I cannot say that I am as indifferent to the subject as I ought to be, but one can avoid doing anything in consequence.

I do not believe one iota about your having assimilated any of my notions unconsciously. You have always done me more than justice. But I do think I did you a bad turn by getting you to read the old MS., as it must have checked your own original thoughts. There is one thing I am fully convinced of, that the future progress (which is the really important point) of the subject will have depended on really good and well-known workers, like yourself, Lyell, and Huxley, having taken up the subject, than on my own work. I see plainly it is this that strikes my non-scientific friends.

Last night I said to myself, I would just cut your Introduction, but would not begin to read, but I broke down, and had a good hour's read.

Farewell, yours affectionately,
C. DARWIN.

C. Darwin to J. D. Hooker.

December 28th, 1859.

. . . Have you seen the splendid essay and notice of my book in the *Times?*† I cannot avoid a strong suspicion that it is by Huxley; but I never heard that he wrote in the *Times.* It will do grand service, . . .

* With regard to Naudin's paper in the 'Revue Horticole,' 1852.
† Dec. 26th.

© The Complete Work of Charles Darwin Online

C. Darwin to T. H. Huxley.

Down, Dec. 28th [1859].

MY DEAR HUXLEY,—Yesterday evening, when I read the *Times* of a previous day, I was amazed to find a splendid essay and review of me. Who can the author be? I am intensely curious. It included an eulogium of me which quite touched me, though I am not vain enough to think it all deserved. The author is a literary man, and German scholar. He has read my book very attentively; but, what is very remarkable, it seems that he is a profound naturalist. He knows my Barnacle-book, and appreciates it too highly. Lastly, he writes and thinks with quite uncommon force and clearness; and what is even still rarer, his writing is seasoned with most pleasant wit. We all laughed heartily over some of the sentences. I was charmed with those unreasonable mortals, who know anything, all thinking fit to range themselves on one side.* Who can it be? Certainly I should have said that there was only one man in England who could have written this essay, and that *you* were the man. But I suppose I am wrong, and that there is some hidden genius of great calibre. For how could you influence Jupiter Olympius and make him give three and a half columns to pure science? The old fogies will think the world will come to an end. Well, whoever the man is, he has done great service to the cause, far more than by a dozen reviews in common periodicals. The grand way he soars above common religious

* The reviewer proposes to pass by the orthodox view, according to which the phenomena of the organic world are "the immediate product of a creative fiat, and consequently are out of the domain of science altogether." And he does so "with less hesitation, as it so happens that those persons who are practically conversant with the facts of the case (plainly a considerable advantage) have always thought fit to range themselves" in the category of those holding "views which profess to rest on a scientific basis only, and therefore admit of being argued to their consequences."

© The Complete Work of Charles Darwin Online

prejudices, and the admission of such views into the *Times*, I look at as of the highest importance, quite independently of the mere question of species. If you should happen to be *acquainted* with the author, for Heaven-sake tell me who he is?

My dear Huxley, yours most sincerely,
C. DARWIN.

[It is impossible to give in a short space an adequate idea of Mr. Huxley's article in the *Times* of December 26. It is admirably planned, so as to claim for the 'Origin' a respectful hearing, and it abstains from anything like dogmatism in asserting the truth of the doctrines therein upheld. A few passages may be quoted:—"That this most ingenious hypothesis enables us to give a reason for many apparent anomalies in the distribution of living beings in time and space, and that it is not contradicted by the main phenomena of life and organisation, appear to us to be unquestionable." Mr. Huxley goes on to recommend to the readers of the 'Origin' a condition of "*thätige Skepsis*"—a state of "doubt which so loves truth that it neither dares rest in doubting, nor extinguish itself by unjustified belief." The final paragraph is in a strong contrast to Professor Sedgwick and his "ropes of bubbles" (see p. 298). Mr. Huxley writes: "Mr. Darwin abhors mere speculation as nature abhors a vacuum. He is as greedy of cases and precedents as any constitutional lawyer, and all the principles he lays down are capable of being brought to the test of observation and experiment. The path he bids us follow professes to be not a mere airy track, fabricated of ideal cobwebs, but a solid and broad bridge of facts. If it be so, it will carry us safely over many a chasm in our knowledge, and lead us to a region free from the snares of those fascinating but barren virgins, the Final Causes, against whom a high authority has so justly warned us."

There can be no doubt that this powerful essay, appearing

© The Complete Work of Charles Darwin Online

as it did in the leading daily Journal, must have had a strong influence on the reading public. Mr. Huxley allows me to quote from a letter an account of the happy chance that threw into his hands the opportunity of writing it.

"The 'Origin' was sent to Mr. Lucas, one of the staff of the *Times* writers at that day, in what I suppose was the ordinary course of business. Mr. Lucas, though an excellent journalist, and, at a later period, editor of 'Once a Week,' was as innocent of any knowledge of science as a babe, and bewailed himself to an acquaintance on having to deal with such a book. Whereupon he was recommended to ask me to get him out of his difficulty, and he applied to me accordingly, explaining, however, that it would be necessary for him formally to adopt anything I might be disposed to write, by prefacing it with two or three paragraphs of his own.

"I was too anxious to seize upon the opportunity thus offered of giving the book a fair chance with the multitudinous readers of the *Times* to make any difficulty about conditions; and being then very full of the subject, I wrote the article faster, I think, than I ever wrote anything in my life, and sent it to Mr. Lucas, who duly prefixed his opening sentences.

"When the article appeared, there was much speculation as to its authorship. The secret leaked out in time, as all secrets will, but not by my aid; and then I used to derive a good deal of innocent amusement from the vehement assertions of some of my more acute friends, that they knew it was mine from the first paragraph!

"As the *Times* some years since, referred to my connection with the review, I suppose there will be no breach of confidence in the publication of this little history, if you think it worth the space it will occupy."]

© The Complete Work of Charles Darwin Online

CHAPTER VII.

THE 'ORIGIN OF SPECIES'—(*continued*).

1860.

I EXTRACT a few entries from my father's Diary:—

"Jan. 7th. The second edition, 3000 copies, of 'Origin was published."

"May 22nd. The first edition of 'Origin' in the United States was 2500 copies."

My father has here noted down the sums received for the Origin.'

First Edition	..	..	.. £180 0 0
Second Edition	..	..	.. 636 13 4
			£816 13 4

After the publication of the second edition he began at once, on Jan. 9th, looking over his materials for the 'Variation of Animals and Plants;' the only other work of the year was on Drosera.

He was at Down during the whole of this year, except for a visit to Dr. Lane's Water-cure Establishment at Sudbrooke, in June, and for visits to Miss Elizabeth Wedgwood's house at Hartfield, in Sussex (July), and to Eastbourne, Sept. 22 to Nov. 16.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, January 3rd [1860].

MY DEAR HOOKER,—I have finished your Essay.* As probably you would like to hear my opinion, though a non-botanist, I will give it without any exaggeration. To my judgment it is by far the grandest and most interesting essay, on subjects of the nature discussed, I have ever read. You know how I admired your former essays, but this seems to me far grander. I like all the part after p. xxvi better than the first part, probably because newer to me. I dare say you will demur to this, for I think every author likes the most speculative parts of his own productions. How superior your essay is to the famous one of Brown (here will be sneer 1st from you). You have made all your conclusions so admirably clear, that it would be no use at all to be a botanist (sneer No. 2). By Jove, it would do harm to affix any idea to the long names of outlandish orders. One can look at your conclusions with the philosophic abstraction with which a mathematician looks at his $a \times x + \sqrt{z^2}$, &c. &c. I hardly know which parts have interested me most; for over and over again I exclaimed, "this beats all." The general comparison of the Flora of Australia with the rest of the world, strikes me (as before) as extremely original, good, and suggestive of many reflections.

. . . . The invading Indian Flora is very interesting, but I think the fact you mention towards the close of the essay—that the Indian vegetation, in contradistinction to the Malayan vegetation, is found in low and level parts of the Malay Islands, *greatly* lessens the difficulty which at first (page l) seemed so great. There is nothing like one's own hobby-horse. I suspect it is the same case as of glacial migration, and of naturalised production—of production of greater area

* 'Australian Flora.

© The Complete Work of Charles Darwin Online

conquering those of lesser; of course the Indian forms would have a greater difficulty in seizing on the cool parts of Australia. I demur to your remarks (page l), as not "conceiving anything in soil, climate, or vegetation of India," which could stop the introduction of Australian plants. Towards the close of the essay (page civ), you have admirable remarks on our profound ignorance of the cause of possible naturalisation or introduction; I would answer p. l, by a later page, viz. p. civ.

Your contrast of the south-west and south-east corners is one of the most wonderful cases I ever heard of. . . . You show the case with wonderful force. Your discussion on mixed invaders of the south-east corner (and of New Zealand) is as curious and intricate a problem as of the races of men in Britain. Your remark on a mixed invading Flora keeping down or destroying an original Flora, which was richer in number of species, strikes me as *eminently new and important.* I am not sure whether to me the discussion on the New Zealand Flora is not even more instructive. I cannot too much admire both. But it will require a long time to suck in all the facts. Your case of the largest Australian orders having none, or very few, species in New Zealand, is truly marvellous. Anyhow, you have now *demonstrated* (together with no mammals in New Zealand) (bitter sneer No. 3), that New Zealand has never been continuously, or even nearly continuously, united by land to Australia!! At p. lxxxix, is the only sentence (on this subject) in the whole essay at which I am much inclined to quarrel, viz. that no theory of trans-oceanic migration can explain, &c. &c. Now I maintain against all the world, that no man knows anything about the trans-oceanic power of migration. You do not know whether or not the absent orders have seeds which are killed by sea-water, like almost all Leguminosæ, and like another order which I forget. Birds do not migrate

© The Complete Work of Charles Darwin Online

from Australia to New Zealand, and therefore floatation *seems* the only possible means; but yet I maintain that we do not know enough to argue on the question, especially as we do not know the main fact whether the seeds of Australian orders are killed by sea-water.

The discussion on European Genera is profoundly interesting; but here alone I earnestly beg for more information, viz. to know which of these genera are absent in the Tropics of the world, *i.e.* confined to temperate regions. I excessively wish to know, *on the notion of Glacial Migration*, how much modification has taken place in Australia. I had better explain when we meet, and get you to go over and mark the list.

. . . . The list of naturalised plants is extremely interesting, but why at the end, in the name of all that is good and bad, do you not sum up and comment on your facts? Come, I will have a sneer at you in return for the many which you will have launched at this letter. Should you [not] have remarked on the number of plants naturalised in Australia and the United States *under extremely different climates*, as showing that climate is so important, and [on] the considerable sprinkling of plants from India, North America, and South Africa, as showing that the frequent introduction of seeds is so important? With respect to "abundance of unoccupied ground in Australia," do you believe that European plants introduced by man now grow on spots in Australia which were absolutely bare? But I am an impudent dog, one must defend one's own fancy theories against such cruel men as you. I dare say this letter will appear very conceited, but one must form an opinion on what one reads with attention, and in simple truth, I cannot find words strong enough to express my admiration of your essay.

My dear old friend, yours affectionately,

C. DARWIN.

© The Complete Work of Charles Darwin Online

P.S.—I differ about the *Saturday Review*.* One cannot expect fairness in a reviewer, so I do not complain of all the other arguments besides the 'Geological Record' being omitted. Some of the remarks about the lapse of years are very good, and the reviewer gives me some good and well-deserved raps—confound it. I am sorry to confess the truth: but it does not at all concern the main argument. That was a nice notice in the *Gardeners' Chronicle*. I hope and imagine that Lindley is almost a convert. Do not forget to tell me if Bentham gets at all more staggered.

With respect to tropical plants during the Glacial period, I throw in your teeth your own facts, at the base of the Himalaya, on the possibility of the co-existence of at least forms of the tropical and temperate regions. I can give a parallel case for animals in Mexico. Oh! my dearly beloved puny child, how cruel men are to you! I am very glad you approve of the Geographical chapters. . . .

C. Darwin to C. Lyell.

Down [January 4th, 1860].

MY DEAR L.—*Gardeners' Chronicle* returned safe. Thanks for note. I am beyond measure glad that you get more and more roused on the subject of species, for, as I have always said, I am well convinced that your opinions and writings will do far more to convince the world than mine. You will make a grand discussion on man. You are very bold in this, and I honour you. I have been, like you, quite surprised at the want of originality in opposed arguments and in favour too. Gwyn Jeffreys attacks me justly in his letter about strictly littoral shells not being often embedded at least

* *Saturday Review*, Dec. 24, 1859. The hostile arguments of the reviewer are geological, and he deals especially with the denudation of the Weald. The reviewer remarks that, "if a million of centuries, more or less, is needed for any part of his argument, he feels no scruple in taking them to suit his purpose."

© The Complete Work of Charles Darwin Online

in Tertiary deposits. I was in a muddle, for I was thinking of Secondary, yet Chthamalus applied to Tertiary. . . . . .

Possibly you might like to see the enclosed note * from Whewell, merely as showing that he is not horrified with us. You can return it whenever you have occasion to write, so as not to waste your time.

C. D.

C. Darwin to C. Lyell.

Down [January 4th ? 1860].

. . . . . . I have had a brief note from Keyserling,† but not worth sending you. He believes in change of species, grants that natural selection explains well adaptation of form, but thinks species change too regularly, as if by some chemical law, for natural selection to be the sole cause of change. I can hardly understand his brief note, but this is I think the upshot.

. . . . . I will send A. Murray's paper whenever published.‡

* Dr. Whewell wrote (Jan. 2, 1860) : " . . . I cannot, yet at least, become a convert. But there is so much of thought and of fact in what you have written that it is not to be contradicted without careful selection of the ground and manner of the dissent." Dr. Whewell dissented in a practical manner for some years, by refusing to allow a copy of the 'Origin of Species' to be placed in the Library of Trinity College.

† Count Keyserling, geologist, joint author with Murchison of the 'Geology of Russia,' 1845 ; and mentioned in Prof. Geikie's 'Life of Murchison.'

‡ The late Andrew Murray wrote two papers on the 'Origin' in the Proc. R. Soc. Edin. 1860. The one referred to here is dated Jan. 16, 1860. The following is quoted from p. 6 of the separate copy : "But the second, and, as it appears to me, by much the most important phase of reversion to type (and which is practically, if not altogether ignored by Mr. Darwin), is the instinctive inclination which induces individuals of the same species by preference to intercross with those possessing the qualities which they themselves want, so as to preserve the purity or equilibrium of the breed. . . . It is trite to a proverb, that tall men marry little women . . . a man of genius marries a fool . . . and we are told that this is the result of the charm of contrast, or of qualities admired in others because we do not possess them. I do not so explain it. I imagine it is the effort of nature to preserve the typical medium of the race."

© The Complete Work of Charles Darwin Online

It includes speculations (which perhaps he will modify) so rash, and without a single fact in support, that had I advanced them he or other reviewers would have hit me very hard. I am sorry to say that I have no "consolatory view" on the dignity of man. I am content that man will probably advance, and care not much whether we are looked at as mere savages in a remotely distant future. Many thanks for your last note.

Yours affectionately,

C. DARWIN.

I have received, in a Manchester newspaper, rather a good squib, showing that I have proved "might is right," and therefore that Napoleon is right, and every cheating tradesman is also right.

C. Darwin to W. B. Carpenter.

Down, January 6th [1860]?

MY DEAR CARPENTER,—I have just read your excellent article in the 'National.' It will do great good; especially if it becomes known as your production. It seems to me to give an excellently clear account of Mr. Wallace's and my views. How capitally you turn the flanks of the theological opposers by opposing to them such men as Bentham and the more philosophical of the systematists! I thank you sincerely for the *extremely* honourable manner in which you mention me. I should have liked to have seen some criticisms or remarks on embryology, on which subject you are so well instructed. I do not think any candid person can read your article without being much impressed with it. The old doctrine of immutability of specific forms will surely but slowly die away. It is a shame to give you trouble, but I should be very much obliged if you could tell me where differently coloured eggs in individuals of the cuckoo have been described, and their laying in twenty-seven kinds of nests. Also do you know from your own observation that the limbs of sheep imported

© The Complete Work of Charles Darwin Online

into the West Indies change colour? I have had detailed information about the loss of wool; but my accounts made the change slower than you describe.

With most cordial thanks and respect, believe me, my dear Carpenter, yours very sincerely,

CH. DARWIN.

*C. Darwin to L. Jenyns.**

Down, January 7th, 1860.

MY DEAR JENYNS,—I am very much obliged for your letter. It is of great use and interest to me to know what impression my book produces on philosophical and instructed minds. I thank you for the kind things which you say; and you go with me much further than I expected. You will think it presumptuous, but I am convinced, *if circumstances lead you to keep the subject in mind*, that you will go further. No one has yet cast doubts on my explanation of the subordination of group to group, on homologies, embryology, and rudimentary organs; and if my explanation of these classes of facts be at all right, whole classes of organic beings must be included in one line of descent.

The imperfection of the Geological Record is one of the greatest difficulties. . . . . During the earliest period the record would be most imperfect, and this seems to me sufficiently to account for our not finding intermediate forms between the classes in the same great kingdoms. It was certainly rash in me putting in my belief of the probability of all beings having descended from *one* primordial form; but as this seems yet to me probable, I am not willing to strike it out. Huxley alone supports me in this, and something could be said in its favour. With respect to man, I am very far from wishing to obtrude my belief; but I thought it dishonest to quite conceal my opinion. Of course it is

* Rev. L. Blomefield.

© The Complete Work of Charles Darwin Online

open to every one to believe that man appeared by a separate miracle, though I do not myself see the necessity or probability.

Pray accept my sincere thanks for your kind note. Your going some way with me gives me great confidence that I am not very wrong. For a very long time I halted half-way; but I do not believe that any enquiring mind will rest half-way. People will have to reject all or admit all; by *all*, I mean only the members of each great kingdom.

My dear Jenyns, yours most sincerely,

C. DARWIN.

C. Darwin to C. Lyell.

Down, January 10th [1860].

. . . It is perfectly true that I owe nearly all the corrections* to you, and several verbal ones to you and others; I am heartily glad you approve of them, as yet only two things have annoyed me; those confounded millions † of years (not that I think it is probably wrong), and my not having (by inadvertence) mentioned Wallace towards the close of the book in the summary, not that any one has noticed this to me. I have now put in Wallace's name at p. 484 in a conspicuous place. I cannot refer you to tables of mortality of children, &c. &c. I have notes somewhere, but I have not the *least* idea where to hunt, and my notes would now be old. I shall be truly glad to read carefully any MS. on man, and give my opinion. You used to caution me to be cautious about man.

* The second edition of 3000 copies of the 'Origin' was published on January 7th.

† This refers to the passage in the 'Origin of Species' (2nd edit. p. 285), in which the lapse of time implied by the denudation of the Weald is discussed. The discussion closes with the sentence: "So that it is not improbable that a longer period than 300 million years has elapsed since the latter part of the Secondary period." This passage is omitted in the later editions of the 'Origin,' against the advice of some of his friends, as appears from the pencil notes in my father's copy of the 2nd edition.

© The Complete Work of Charles Darwin Online

I suspect I shall have to return the caution a hundred fold! Yours will, no doubt, be a grand discussion; but it will horrify the world at first more than my whole volume; although by the sentence (p. 489, new edition *) I show that I believe man is in the same predicament with other animals. It is in fact impossible to doubt it. I have thought (only vaguely) on man. With respect to the races, one of my best chances of truth has broken down from the impossibility of getting facts. I have one good speculative line, but a man must have entire credence in Natural Selection before he will even listen to it. Psychologically, I have done scarcely anything. Unless, indeed, expression of countenance can be included, and on that subject I have collected a good many facts, and speculated, but I do not suppose I shall ever publish, but it is an uncommonly curious subject. By the way I sent off a lot of questions the day before yesterday to Tierra del Fuego on expression! I suspect (for I have never read it) that Spencer's 'Psychology' has a bearing on Psychology as we should look at it. By all means read the Preface, in about 20 pages, of Hensleigh Wedgwood's new Dictionary, on the first origin of Language; Erasmus would lend it. I agree about Carpenter, a very good article, but with not much original. . . . Andrew Murray has criticised, in an address to the Botanical Society of Edinburgh, the notice in the 'Linnean Journal,' and "has disposed of" the whole theory by an ingenious difficulty, which I was very stupid not to have thought of; for I express surprise at more and analogous cases not being known. The difficulty is, that amongst the blind insects of the caves in distant parts of the world there are some of the same genus, and yet the genus is not found out of the caves or living in the free world. I have little doubt that, like the fish Amblyopsis, and like Proteus in Europe, these insects are "wrecks of ancient life," or "living fossils," saved from competition and extermination. But that

* First edition, p. 488.

© The Complete Work of Charles Darwin Online

formerly *seeing* insects of the same genus roamed over the whole area in which the cases are included.

Farewell, yours affectionately,

C. DARWIN.

P.S.—*Our* ancestor was an animal which breathed water, had a swim bladder, a great swimming tail, an imperfect skull, and undoubtedly was an hermaphrodite!

Here is a pleasant genealogy for mankind.

C. Darwin to C. Lyell.

Down, January 14th [1860].

. . . I shall be much interested in reading your man discussion, and will give my opinion carefully, whatever that may be worth; but I have so long looked at you as the type of cautious scientific judgment (to my mind one of the highest and most useful qualities), that I suspect my opinion will be superfluous. It makes me laugh to think what a joke it will be if I have to caution you, after your cautions on the same subject to me!

I will order Owen's book;* I am very glad to hear Huxley's opinion on his classification of man; without having due knowledge, it seemed to me from the very first absurd; all classifications founded on single characters I believe have failed.

. . . What a grand immense benefit you conferred on me by getting Murray to publish my book. I never till to-day realised that it was getting widely distributed; for in a letter from a lady to-day to E., she says she heard a man enquiring for it at the *Railway Station!!!* at Waterloo Bridge; and the bookseller said that he had none till the new edition was out. The bookseller said he had not read it, but had heard it was a very remarkable book!!! . . . .

* 'Classification of the Mammalia,' 1859.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, 14th [January, 1860].

. . . . I heard from Lyell this morning, and he tells me a piece of news. You are a good-for-nothing man; here you are slaving yourself to death with hardly a minute to spare, and you must write a review on my book! I thought it * a very good one, and was so much struck with it, that I sent it to Lyell. But I assumed, as a matter of course, that it was Lindley's. Now that I know it is yours, I have re-read it, and my kind and good friend, it has warmed my heart with all the honourable and noble things you say of me and it. I was a good deal surprised at Lindley hitting on some of the remarks, but I never dreamed of you. I admired it chiefly as so well adapted to tell on the readers of the *Gardeners' Chronicle;* but now I admire it in another spirit. Farewell, with hearty thanks. . . . . Lyell is going at man with an audacity that frightens me. It is a good joke; he used always to caution me to slip over man.

[In the *Gardeners' Chronicle*, Jan. 21, 1860, appeared a short letter from my father, which was called forth by Mr. Westwood's communication to the previous number of the journal, in which certain phenomena of cross-breeding are discussed in relation to the 'Origin of Species.' Mr. Westwood wrote in reply (Feb. 11), and adduced further evidence against the doctrine of descent, such as the identity of the figures of ostriches on the ancient "Egyptian records," with the bird as we now know it. The correspondence is hardly worth mentioning, except as one of the very few cases in which my father was enticed into anything resembling a controversy.]

* *Gardeners' Chronicle*, 1860. Referred to above, at p. 260. Sir J. D. Hooker took the line of complete impartiality, so as not to commit Lindley.

© The Complete Work of Charles Darwin Online

Asa Gray to J. D. Hooker.

Cambridge, Mass.,
January 5th, 1860.

MY DEAR HOOKER,—Your last letter, which reached me just before Christmas, has got mislaid during the upturnings in my study which take place at that season, and has not yet been discovered. I should be very sorry to lose it, for there were in it some botanical mems. which I had not secured. . .

The principal part of your letter was high laudation of Darwin's book.

Well, the book has reached me, and I finished its careful perusal four days ago; and I freely say that your laudation is not out of place.

It is done in a *masterly manner.* It might well have taken twenty years to produce it. It is crammed full of most interesting matter—thoroughly digested—well expressed—close, cogent, and taken as a system it makes out a better case than I had supposed possible. . . .

Agassiz, when I saw him last, had read but a part of it. He says it is *poor—very poor!!* (entre nous). The fact [is] he is very much annoyed by it, ... and I do not wonder at it. To bring all *ideal* system within the domain of science, and give good physical or natural explanations of all his capital points, is as bad as to have Forbes take the glacier materials . . . and give scientific explanation of all the phenomena.

Tell Darwin all this. I will write to him when I get a chance. As I have promised, he and you shall have fair-play here. . . . I must myself write a review of Darwin's book for 'Silliman's Journal' (the more so that I suspect Agassiz means to come out upon it) for the next (March) No., and I am now setting about it (when I ought to be every moment working the Expl[oring] Expedition Compositæ, which I know far more about). And really it is no easy job as you may well imagine.

© The Complete Work of Charles Darwin Online

I doubt if I shall please you altogether. I know I shall not please Agassiz at all. I hear another reprint is in the Press, and the book will excite much attention here, and some controversy. . . .

C. Darwin to Asa Gray.

Down, January 28th [1860].

MY DEAR GRAY,—Hooker has forwarded to me your letter to him; and I cannot express how deeply it has gratified me. To receive the approval of a man whom one has long sincerely respected, and whose judgment and knowledge are most universally admitted, is the highest reward an author can possibly wish for; and I thank you heartily for your most kind expressions.

I have been absent from home for a few days, and so could not earlier answer your letter to me of the 10th of January. You have been extremely kind to take so much trouble and interest about the edition. It has been a mistake of my publisher not thinking of sending over the sheets. I had entirely and utterly forgotten your offer of receiving the sheets as printed off. But I must not blame my publisher, for had I remembered your most kind offer I feel pretty sure I should not have taken advantage of it; for I never dreamed of my book being so successful with general readers: I believe I should have laughed at the idea of sending the sheets to America.*

After much consideration, and on the strong advice of Lyell and others, I have resolved to have the present book as it is (excepting correcting errors, or here and there inserting short

* In a letter to Mr. Murray, 1860, my father wrote :—"I am amused by Asa Gray's account of the excitement my book has made amongst naturalists in the U. States. Agassiz has denounced it in a newspaper, but yet in such terms that it is in fact a fine advertisement!" This seems to refer to a lecture given before the Mercantile Library Association.

© The Complete Work of Charles Darwin Online

sentences) and to use all my strength, *which is but little*, to bring out the first part (forming a separate volume, with index, &c.) of the three volumes which will make my bigger work; so that I am very unwilling to take up time in making corrections for an American edition. I enclose a list of a few corrections in the second reprint, which you will have received by this time complete, and I could send four or five corrections or additions of equally small importance, or rather of equal brevity. I also intend to write a *short* preface with a brief history of the subject. These I will set about, as they must some day be done, and I will send them to you in a short time—the few corrections first, and the preface afterwards, unless I hear that you have given up all idea of a separate edition. You will then be able to judge whether it is worth having the new edition with *your review prefixed*. Whatever be the nature of your review, I assure you I should feel it a *great* honour to have my book thus preceded. . . . .

Asa Gray to C. Darwin.

Cambridge, January 23rd, 1860.

MY DEAR DARWIN,—You have my hurried letter telling you of the arrival of the remainder of the sheets of the reprint, and of the stir I had made for a reprint in Boston. Well, all looked pretty well, when, lo, we found that a second New York publishing house had announced a reprint also! I wrote then to both New York publishers, asking them to give way to the *author* and his reprint of a revised edition. I got an answer from the Harpers that they withdraw—from the Appletons that they had got the book *out* (and the next day I saw a copy); but that, "if the work should have any considerable sale, we certainly shall be disposed to pay the author reasonably and liberally."

The Appletons being thus out with their reprint, the Boston house declined to go on. So I wrote to the Appletons taking

© The Complete Work of Charles Darwin Online

them at their word, offering to aid their reprint, to give them the use of the alterations in the London reprint, as soon as I find out what they are, &c. &c. And I sent them the first leaf, and asked them to insert in their future issue the additional matter from Butler,* which tells just right. So there the matter stands. If you furnish any matter in advance of the London third edition, I will make them pay for it.

I may get something for you. All got is clear gain; but it will not be very much, I suppose.

Such little notices in the papers here as have yet appeared are quite handsome and considerate.

I hope next week to get printed sheets of my review from New Haven, and send [them] to you, and will ask you to pass them on to Dr. Hooker.

To fulfil your request, I ought to tell you what I think the weakest, and what the best, part of your book. But this is not easy, nor to be done in a word or two. The *best part*, I think, is the *whole*, *i.e.* its *plan* and *treatment*, the vast amount of facts and acute inferences handled as if you had a perfect mastery of them. I do not think twenty years too much time to produce such a book in.

Style clear and good, but now and then wants revision for little matters (p. 97, self-fertilises *itself*, &c.).

Then your candour is worth everything to your cause. It is refreshing to find a person with a new theory who frankly confesses that he finds difficulties, insurmountable, at least for the present. I know some people who never have any difficulties to speak of.

The moment I understood your premisses, I felt sure you had a real foundation to hold on. Well, if one admits your premisses, I do not see how he is to stop short of your conclusions, as a probable hypothesis at least.

* A quotation from Butler's 'Analogy,' on the use of the word natural, which in the second edition is placed with the passages from Whewell and Bacon on p. ii, opposite the title-page.

© The Complete Work of Charles Darwin Online

It naturally happens that my review of your book does not exhibit anything like the full force of the impression the book has made upon me. Under the circumstances I suppose I do your theory more good here, by bespeaking for it a fair and favourable consideration, and by standing non-committed as to its full conclusions, than I should if I announced myself a convert; nor could I say the latter, with truth.

Well, what seems to me the weakest point in the book is the attempt to account for the formation of organs, the making of eyes, &c., by natural selection. Some of this reads quite Lamarckian.

The chapter on *Hybridism* is not a *weak*, but a *strong* chapter. You have done wonders there. But still you have not accounted, as you may be held to account, for divergence up to a certain extent producing increased fertility of the crosses, but carried one short almost imperceptible step more, giving rise to sterility, or reversing the tendency. Very likely you are on the right track; but you have something to do yet in that department.

Enough for the present.

. . . . . . I am not insensible to your compliments, the very high compliment which you pay me in valuing my opinion. You evidently think more of it than I do, though from the way I write [to] you, and especially [to] Hooker, this might not be inferred from the reading of my letters.

I am free to say that I never learnt so much from one book as I have from yours. There remain a thousand things I long to say about it.

Ever yours,

ASA GRAY.

C. Darwin to Asa Gray.

[February? 1860.]

. . . . . Now I will just run through some points in your letter. What you say about my book gratifies me most deeply,

© The Complete Work of Charles Darwin Online

and I wish I could feel all was deserved by me. I quite think a review from a man, who is not an entire convert, if fair and moderately favourable, is in all respects the best kind of review. About the weak points I agree. The eye to this day gives me a cold shudder, but when I think of the fine known gradations, my reason tells me I ought to conquer the cold shudder.

Pray kindly remember and tell Prof. Wyman how very grateful I should be for any hints, information, or criticisms. I have the highest respect for his opinion. I am so sorry about Dana's health. I have already asked him to pay me a visit.

Farewell, you have laid me under a load of obligation—not that I feel it a load. It is the highest possible gratification to me to think that you have found my book worth reading and reflection ; for you and three others I put down in my own mind as the judges whose opinions I should value most of all.

My dear Gray, yours most sincerely,

C. DARWIN.

P.S.—I feel pretty sure, from my own experience, that if you are led by your studies to keep the subject of the origin of species before your mind, you will go further and further in your belief. It took me long years, and I assure you I am astonished at the impression my book has made on many minds. I fear twenty years ago I should not have been half as candid and open to conviction.

C. Darwin to J. D. Hooker.

Down [January 31st, 1860].

MY DEAR HOOKER,—I have resolved to publish a little sketch of the progress of opinion on the change of species. Will you or Mrs. Hooker do me the favour to copy *one* sentence out of Naudin's paper in the 'Revue Horticole,' 1852, p. 103, namely, that on his principle of Finalité. Can

© The Complete Work of Charles Darwin Online

you let me have it soon, with those confounded dashes over the vowels put in carefully? Asa Gray, I believe, is going to get a second edition of my book, and I want to send this little preface over to him soon. I did not think of the necessity of having Naudin's sentence on finality, otherwise I would have copied it.

Yours affectionately,

C. DARWIN.

P.S.—I shall end by just alluding to your Australian Flora Introduction. What was the date of publication: December 1859, or January 1860? Please answer this.

My preface will also do for the French edition, which, *I believe*, is agreed on.

C. Darwin to J. D. Hooker.

February [1860].

. . . . As the 'Origin' now stands, Harvey's * is a good hit against my talking so much of the insensibly fine gradations; and certainly it has astonished me that I should be pelted with the fact, that I had not allowed abrupt and great enough variations under nature. It would take a good deal more evidence to make me admit that forms have often changed by *saltum*.

* William Henry Harvey was descended from a Quaker family of Youghal, and was born in February, 1811, at Summerville, a country house on the banks of the Shannon. He died at Torquay in 1866. In 1835, Harvey went to Africa (Table Bay) to pursue his botanical studies, the results of which were given in his 'Genera of South African Plants.' In 1838, ill-health compelled him to obtain leave of absence, and return to England for a time; in 1840 he returned to Cape Town, to be again compelled by illness to leave. In 1843 he obtained the appointment of Botanical Professor at Trinity College, Dublin. In 1854, 1855, and 1856 he visited Australia, New Zealand, the Friendly and Fiji Islands. In 1857 Dr. Harvey reached home, and was appointed the successor of Professor Allman to the Chair of Botany in Dublin University. He was author of several botanical works, principally on Algæ.—(From a Memoir published in 1869.)

© The Complete Work of Charles Darwin Online

Have you seen Wollaston's attack in the 'Annals'?* The stones are beginning to fly. But Theology has more to do with these two attacks than Science. . . .

[In the above letter a paper by Harvey in the *Gardeners' Chronicle*, Feb. 18, 1860, is alluded to. He describes a case of monstrosity in *Begonia frigida*, in which the "sport" differed so much from a normal Begonia that it might have served as the type of a distinct natural order. Harvey goes on to argue that such a case is hostile to the theory of natural selection, according to which changes are not supposed to take place *per saltum*, and adds that "a few such cases would overthrow it [Mr. Darwin's hypothesis] altogether." In the following number of the *Gardeners' Chronicle* Sir J. D. Hooker showed that Dr. Harvey had misconceived the bearing of the Begonia case, which he further showed to be by no means calculated to shake the validity of the doctrine of modification by means of natural selection. My father mentions the Begonia case in a letter to Lyell (Feb. 18, 1860):—

"I send by this post an attack in the *Gardeners' Chronicle*, by Harvey (a first-rate Botanist, as you probably know). It seems to me rather strange; he assumes the permanence of monsters, whereas, monsters are generally sterile, and not often inheritable. But grant his case, it comes that I have been too cautious in not admitting great and sudden variations. Here again comes in the mischief of my *abstract*. In the fuller MS. I have discussed a parallel case of a normal fish like a monstrous gold-fish."

With reference to Sir J. D. Hooker's reply, my father wrote :]

Down [February 26th, 1860].

MY DEAR HOOKER,—Your answer to Harvey seems to me *admirably* good. You would have made a gigantic fortune as

* 'Annals and Magazine of Natural History,' 1860.

© The Complete Work of Charles Darwin Online

a barrister. What an omission of Harvey's about the graduated state of the flowers! But what strikes me most is that surely I ought to know my own book best, yet, by Jove, you have brought forward ever so many arguments which I did not think of! Your reference to classification (viz. I presume to such cases as Aspicarpa) is *excellent*, for the monstrous Begonia no doubt in all details would be a Begonia. I did not think of this, nor of the *retrograde* step from separated sexes to an hermaphrodite state; nor of the lessened fertility of the monster. Proh pudor to me.

The world would say what a lawyer has been lost in a *mere* botanist!

Farewell, my dear master in my own subject,

Yours affectionately,

C. DARWIN.

I am so heartily pleased to see that you approve of the chapter on Classification.

I wonder what Harvey will say. But no one hardly, I think, is able at first to see when he is beaten in an argument.

[The following letters refer to the first translation (1860) of the 'Origin of Species' into German, which was superintended by H. G. Bronn, a good zoologist and palæontologist, who was at the time at Freiburg, but afterwards Professor at Heidelberg. I have been told that the translation was not a success, it remained an obvious translation, and was correspondingly unpleasant to read. Bronn added to the translation an appendix on the difficulties that occurred to him. For instance, how can natural selection account for differences between species, when these differences appear to be of no service to their possessors; e.g., the length of the ears and tail, or the folds in the enamel of the teeth of various species of rodents? Krause, in his book, 'Charles Darwin,' p. 91, criticises Bronn's conduct in this matter, but it will be seen that my father actually suggested the addition of Bronn's

© The Complete Work of Charles Darwin Online

remarks. A more serious charge against Bronn made by Krause (*op. cit.* p. 87) is that he left out passages of which he did not approve, as, for instance, the passage ('Origin,' first edition, p. 488) "Light will be thrown on the origin of man and his history." I have no evidence as to whether my father did or did not know of these alterations.]

C. Darwin to H. G. Bronn.

Down, Feb. 4 [1860].

DEAR AND MUCH HONOURED SIR,—I thank you sincerely for your most kind letter; I feared that you would much disapprove of the 'Origin,' and I sent it to you merely as a mark of my sincere respect. I shall read with much interest your work on the productions of Islands whenever I receive it. I thank you cordially for the notice in the 'Neues Jahrbuch für Mineralogie,' and still more for speaking to Schweitzerbart about a translation; for I am most anxious that the great and intellectual German people should know something about my book.

I have told my publisher to send immediately a copy of the *new** edition to Schweitzerbart, and I have written to Schweitzerbart that I give up all right to profit for myself, so that I hope a translation will appear. I fear that the book will be difficult to translate, and if you could advise Schweitzerbart about a *good* translator, it would be of very great service. Still more, if you would run your eye over the more difficult parts of the translation; but this is too great a favour to expect. I feel sure that it will be difficult to translate, from being so much condensed.

Again I thank you for your noble and generous sympathy, and I remain, with entire respect,

Yours, truly obliged,

C. DARWIN.

* Second edition.

© The Complete Work of Charles Darwin Online

P.S.—The new edition has some few corrections, and I will send in MS. some additional corrections, and a short historical preface, to Schweitzerbart.

How interesting you could make the work by *editing* (I do not mean translating) the work, and appending notes of *refutation* or confirmation. The book has sold so very largely in England, that an editor would, I think, make profit by the translation.

C. Darwin to H. G. Bronn.

Down, Feb. 14 [1860].

My dear and much honoured Sir,—I thank you cordially for your extreme kindness in superintending the translation. I have mentioned this to some eminent scientific men, and they all agree that you have done a noble and generous service. If I am proved quite wrong, yet I comfort myself in thinking that my book may do some good, as truth can only be known by rising victorious from every attack. I thank you also much for the review, and for the kind manner in which you speak of me. I send with this letter some corrections and additions to M. Schweitzerbart, and a short historical preface. I am not much acquainted with German authors, as I read German very slowly; therefore I do not know whether any Germans have advocated similar views with mine; if they have, would you do me the favour to insert a foot-note to the preface? M. Schweitzerbart has now the reprint ready for a translator to begin. Several scientific men have thought the term "Natural Selection" good, because its meaning is *not* obvious, and each man could not put on it his own interpretation, and because it at once connects variation under domestication and nature. Is there any analogous term used by German breeders of animals? "Adelung," ennobling, would, perhaps, be too metaphorical. It is folly in me, but I cannot help doubting whether "Wahl der Lebensweise" expresses my notion. It leaves the impression on my

© The Complete Work of Charles Darwin Online

mind of the Lamarckian doctrine (which I reject) of habits of life being all-important. Man has altered, and thus improved the English race-horse by *selecting* successive fleeter individuals; and I believe, owing to the struggle for existence, that similar *slight* variations in a wild horse, *if advantageous to it*, would be *selected* or *preserved* by nature; hence Natural Selection. But I apologise for troubling you with these remarks on the importance of choosing good German terms for "Natural Selection." With my heartfelt thanks, and with sincere respect,

I remain, dear Sir, yours very sincerely,

CHARLES DARWIN.

C. Darwin to H. G. Bronn.

Down July 14 [1860].

DEAR AND HONOURED SIR,—On my return home, after an absence of some time, I found the translation of the third part * of the 'Origin,' and I have been delighted to see a final chapter of criticisms by yourself. I have read the first few paragraphs and final paragraph, and am perfectly contented, indeed more than contented, with the generous and candid spirit with which you have considered my views. You speak with too much praise of my work. I shall, of course, carefully read the whole chapter; but though I can read descriptive books like Gaertner's pretty easily, when any reasoning comes in, I find German excessively difficult to understand. At some *future* time I should very much like to hear how my book has been received in Germany, and I most sincerely hope M. Schweitzerbart will not lose money by the publication. Most of the reviews have been bitterly opposed to me in England, yet I have made some converts, and *several* naturalists who would not believe in a word of it, are now

* The German translation was published in three pamphlet-like numbers.

© The Complete Work of Charles Darwin Online

coming slightly round, and admit that natural selection may have done something. This gives me hope that more will ultimately come round to a certain extent to my views.

I shall ever consider myself deeply indebted to you for the immense service and honour which you have conferred on me in making the excellent translation of my book. Pray believe me, with most sincere respect,

Dear Sir, yours gratefully,

CHARLES DARWIN.

C. Darwin to C. Lyell.

Down [February 12th, 1860].

. . . I think it was a great pity that Huxley wasted so much time in the lecture on the preliminary remarks; . . . but his lecture seemed to me very fine and very bold. I have remonstrated (and he agrees) against the impression that he would leave, that sterility was a universal and infallible criterion of species.

You will, I am sure, make a grand discussion on man. I am so glad to hear that you and Lady Lyell will come here. Pray fix your own time; and if it did not suit us we would say so. We could then discuss man well. . . .

How much I owe to you and Hooker! I do not suppose I should hardly ever have published had it not been for you.

[The lecture referred to in the last letter was given at the Royal Institution, February 10, 1860. The following letter was written in reply to Mr. Huxley's request for information about breeding, hybridisation, &c. It is of interest as giving a vivid retrospect of the writer's experience on the subject.]

C. Darwin to T. H. Huxley.

Ilkley, Yorks, Nov. 27 [1859].

MY DEAR HUXLEY,—Gärtner grand, Kölreuter grand, but papers scattered through many volumes and very lengthy. I

© The Complete Work of Charles Darwin Online

had to make an abstract of the whole. Herbert's volume on Amaryllidaceæ very good, and two excellent papers in the 'Horticultural Journal.' For animals, no résumé to be trusted at all; facts are to be collected from all original sources.* I fear my MS. for the bigger book (twice or thrice as long as in present book), with all references, would be illegible, but it would save you infinite labour; of course I would gladly lend it, but I have no copy, so care would have to be taken of it. But my accursed handwriting would be fatal, I fear.

About breeding, I know of no one book. I did not think well of Lowe, but I can name none better. Youatt I look at as a far better and *more practical* authority; but then his views and facts are scattered through three or four thick volumes. I have picked up most by reading really numberless special treatises and *all* agricultural and horticultural journals; but it is a work of long years. *The difficulty is to know what to trust.* No one or two statements are worth a farthing; the facts are so complicated. I hope and think I have been really cautious in what I state on this subject, although all that I have given, as yet, is *far* too briefly. I have found it very important associating with fanciers and breeders. For instance, I sat one evening in a gin palace in the Borough amongst a set of pigeon fanciers, when it was hinted that Mr. Bull had crossed his Pouters with Runts to gain size; and

* This caution is exemplified in the following extract from an earlier letter to Professor Huxley:—"The inaccuracy of the blessed gang (of which I am one) of compilers passes all bounds. *Monsters* have frequently been described as hybrids without a tittle of evidence. I must give one other case to show how we jolly fellows work. A Belgian Baron (I forget his name at this moment) crossed two distinct geese and got *seven* hybrids, which he proved subsequently to be quite sterile; well, compiler the first, Chevreul, says that the hybrids were propagated for *seven* generations *inter se*. Compiler second (Morton) mistakes the French name, and gives Latin names for two more distinct geese, and says *Chevreul* himself propagated them *inter se* for seven generations; and the latter statement is copied from book to book."

© The Complete Work of Charles Darwin Online

if you had seen the solemn, the mysterious, and awful shakes of the head which all the fanciers gave at this scandalous proceeding, you would have recognised how little crossing has had to do with improving breeds, and how dangerous for endless generations the process was. All this was brought home far more vividly than by pages of mere statements, &c. But I am scribbling foolishly. I really do not know how to advise about getting up facts on breeding and improving breeds. Go to Shows is one way. Read *all* treatises on any *one* domestic animal, and believe nothing without largely confirmed. For your lectures I can give you a few amusing anecdotes and sentences, if you want to make the audience laugh.

I thank you particularly for telling me what naturalists think. If we can once make a compact set of believers we shall in time conquer. I am *eminently* glad Ramsay is on our side, for he is, in my opinion, a first-rate geologist. I sent him a copy. I hope he got it. I shall be very curious to hear whether any effect has been produced on Prestwich; I sent him a copy, not as a friend, but owing to a sentence or two in some paper, which made me suspect he was doubting.

Rev. C. Kingsley has a mind to come round. Quatrefages writes that he goes some long way with me; says he exhibited diagrams like mine. With most hearty thanks,

Yours very tired,

C. DARWIN.

[I give the conclusion of Professor Huxley's lecture, as being one of the earliest, as well as one of the most eloquent, of his utterances in support of the 'Origin of Species':

"I have said that the man of science is the sworn interpreter of nature in the high court of reason. But of what avail is his honest speech, if ignorance is the assessor of the judge, and prejudice the foreman of the jury? I hardly know

© The Complete Work of Charles Darwin Online

of a great physical truth, whose universal reception has not been preceded by an epoch in which most estimable persons have maintained that the phenomena investigated were directly dependent on the Divine Will, and that the attempt to investigate them was not only futile, but blasphemous. And there is a wonderful tenacity of life about this sort of opposition to physical science. Crushed and maimed in every battle, it yet seems never to be slain; and after a hundred defeats it is at this day as rampant, though happily not so mischievous, as in the time of Galileo.

"But to those whose life is spent, to use Newton's noble words, in picking up here a pebble and there a pebble on the shores of the great ocean of truth—who watch, day by day, the slow but sure advance of that mighty tide, bearing on its bosom the thousand treasures wherewith man ennobles and beautifies his life—it would be laughable, if it were not so sad, to see the little Canutes of the hour enthroned in solemn state, bidding that great wave to stay, and threatening to check its beneficent progress. The wave rises and they fly; but, unlike the brave old Dane, they learn no lesson of humility: the throne is pitched at what seems a safe distance, and the folly is repeated.

"Surely it is the duty of the public to discourage anything of this kind, to discredit these foolish meddlers who think they do the Almighty a service by preventing a thorough study of His works.

"The Origin of Species is not the first, and it will not be the last, of the great questions born of science, which will demand settlement from this generation. The general mind is seething strangely, and to those who watch the signs of the times, it seems plain that this nineteenth century will see revolutions of thought and practice as great as those which the sixteenth witnessed. Through what trials and sore contests the civilised world will have to pass in the course of this new reformation, who can tell?

© The Complete Work of Charles Darwin Online

"But I verily believe that come what will, the part which England may play in the battle is a grand and a noble one. She may prove to the world that, for one people, at any rate, despotism and demagogy are not the necessary alternatives of government; that freedom and order are not incompatible; that reverence is the handmaid of knowledge; that free discussion is the life of truth, and of true unity in a nation.

"Will England play this part? That depends upon how you, the public, deal with science. Cherish her, venerate her, follow her methods faithfully and implicitly in their application to all branches of human thought, and the future of this people will be greater than the past.

"Listen to those who would silence and crush her, and I fear our children will see the glory of England vanishing like Arthur in the mist; they will cry too late the woful cry of Guinever:—

'It was my duty to have loved the highest;
It surely was my profit had I known;
It would have been my pleasure had I seen.'"]

C. Darwin to C. Lyell.

Down [February 15th, 1860].

. . . I am perfectly convinced (having read this morning) that the review in the 'Annals'* is by Wollaston; no one else in the world would have used so many parentheses. I

* Annals and Mag. of Nat. Hist. third series, vol. 5, p. 132. My father has obviously taken the expression "pestilent" from the following passage (p. 138): "But who is this Nature, we have a right to ask, who has such tremendous power, and to whose efficiency such marvellous performances are ascribed? What are her image and attributes, when dragged from her wordy lurking-place? Is she ought but a pestilent abstraction, like dust cast in our eyes to obscure the workings of an Intelligent First Cause of all?" The reviewer pays a tribute to my father's candour, "so manly and outspoken as almost to 'cover a multitude of sins.'" The parentheses (to which allusion is made above) are so frequent as to give a characteristic appearance to Mr. Wollaston's pages.

© The Complete Work of Charles Darwin Online

have written to him, and told him that the "pestilent" fellow thanks him for his kind manner of speaking about him. I have also told him that he would be pleased to hear that the Bishop of Oxford says it is the most unphilosophical * work he ever read. The review seems to me clever, and only misinterprets me in a few places. Like all hostile men, he passes over the explanation given of Classification, Morphology, Embryology, and Rudimentary Organs, &c. I read Wallace's paper in MS.,† and thought it admirably good; he does not know that he has been anticipated about the depth of intervening sea determining distribution. . . . The most curious point in the paper seems to me that about the African character of the Celebes productions, but I should require further confirmation. . . .

Henslow is staying here; I have had some talk with him; he is in much the same state as Bunbury,‡ and will go a very little way with us, but brings up no real argument against going further. He also shudders at the eye! It is really curious (and perhaps is an argument in our favour) how differently different opposers view the subject. Henslow used to rest his opposition on the imperfection of the Geological Record, but he now thinks nothing of this, and says I have got well out of it; I wish I could quite agree with him. Baden Powell says he never read anything so conclusive as my statement about the eye!! A stranger writes to me about sexual selection, and regrets that I boggle about such a trifle as the brush of hair on the male turkey, and so on. As L. Jenyns has a really philosophical mind, and as you say you like to see everything, I send an old letter of his. In a later letter to Henslow, which I have seen, he is more candid than any opposer I have heard of, for he says, though he *cannot* go so

* Another version of the words is given by Lyell, to whom they were spoken, viz. "the most illogical book ever written."—'Life,' vol. ii. p. 358.

† "On the Zoological Geography of the Malay Archipelago."—Linn. Soc. Journ. 1860.

‡ The late Sir Charles Bunbury, well known as a Palæo-botanist.

© The Complete Work of Charles Darwin Online

far as I do, yet he can give no good reason why he should not. It is funny how each man draws his own imaginary line at which to halt. It reminds me so vividly what I was told * about you when I first commenced geology—to believe a *little*, but on no account to believe all.

Ever yours affectionately,

C. DARWIN.

C. Darwin to Asa Gray.

Down, February 18th [1860].

MY DEAR GRAY,—I received about a week ago two sheets of your Review; † read them, and sent them to Hooker; they are now returned and re-read with care, and to-morrow I send them to Lyell. Your Review seems to me *admirable;* by far the best which I have read. I thank you from my heart both for myself, but far more for the subject's sake. Your contrast between the views of Agassiz and such as mine is very curious and instructive.‡ By the way, if Agassiz writes anything on the subject, I hope you will tell me. I am charmed with your metaphor of the streamlet never running against the force of gravitation. Your distinction between an hypothesis and theory seems to me very ingenious; but I do not think it is ever followed. Every one now speaks of the undulatory *theory* of light; yet the ether is itself hypothetical, and the undulations are inferred only from explaining the phenomena of light. Even in the *theory* of gravitation is the attractive power in any way known, except by explaining the fall of the apple, and the movements of the Planets? It seems to me that an hypothesis is *developed* into a theory solely by explaining an ample lot of facts. Again and again I

* By Professor Henslow.

† The 'American Journal of Science and Arts,' March 1860. Reprinted in 'Darwiniana,' 1876.

‡ The contrast is briefly summed up thus: "The theory of Agassiz regards the origin of species and their present general distribution over the world as equally primordial, equally supernatural; that of Darwin as equally derivative, equally natural." — 'Darwiniana,' p. 14.

© The Complete Work of Charles Darwin Online

thank you for your generous aid in discussing a view, about which you very properly hold yourself unbiassed.

My dear Gray, yours most sincerely,

C. DARWIN.

P.S.—Several clergymen go far with me. Rev. L. Jenyns, a very good naturalist. Henslow will go a very little way with me, and is not shocked at me. He has just been visiting me.

[With regard to the attitude of the more liberal representatives of the Church, the following letter (already referred to) from Charles Kingsley is of interest:]

C. Kingsley to C. Darwin.

Eversley Rectory, Winchfield,
November 18th, 1859.

DEAR SIR,—I have to thank you for the unexpected honour of your book. That the Naturalist whom, of all naturalists living, I most wish to know and to learn from, should have sent a scientist like me his book, encourages me at least to observe more carefully, and think more slowly.

I am so poorly (in brain), that I fear I cannot read your book just now as I ought. All I have seen of it *awes* me; both with the heap of facts and the prestige of your name, and also with the clear intuition, that if you be right, I must give up much that I have believed and written.

In that I care little. Let God be true, and every man a liar! Let us know what *is*, and, as old Socrates has it, ἕπεσθαι τῷ λόγῳ—follow up the villainous shifty fox of an argument, into whatsoever unexpected bogs and brakes he may lead us, if we do but run into him at last.

From two common superstitions, at least, I shall be free while judging of your book:—

(1.) I have long since, from watching the crossing of domesticated animals and plants, learnt to disbelieve the dogma of the permanence of species.

© The Complete Work of Charles Darwin Online

(2.) I have gradually learnt to see that it is just as noble a conception of Deity, to believe that He created primal forms capable of self development into all forms needful *pro tempore* and *pro loco*, as to believe that He required a fresh act of intervention to supply the *lacunas* which He Himself had made. I question whether the former be not the loftier thought.

Be it as it may, I shall prize your book, both for itself, and as a proof that you are aware of the existence of such a person as

Your faithful servant,
C. KINGSLEY.

[My father's old friend, the Rev. J. Brodie Innes, of Milton Brodie, who was for many years Vicar of Down, writes in the same spirit:

"We never attacked each other. Before I knew Mr. Darwin I had adopted, and publicly expressed, the principle that the study of natural history, geology, and science in general, should be pursued without reference to the Bible. That the Book of Nature and Scripture came from the same Divine source, ran in parallel lines, and when properly understood would never cross. . . . . .

"His views on this subject were very much to the same effect from his side. Of course any conversations we may have had on purely religious subjects are as sacredly private now as in his life; but the quaint conclusion of one may be given. We had been speaking of the apparent contradiction of some supposed discoveries with the Book of Genesis; he said, 'you are (it would have been more correct to say you ought to be) a theologian, I am a naturalist, the lines are separate. I endeavour to discover facts without considering what is said in the Book of Genesis. I do not attack Moses, and I think Moses can take care of himself.' To the same effect he wrote more recently, 'I cannot remember that I ever published a

© The Complete Work of Charles Darwin Online

word directly against religion or the clergy; but if you were to read a little pamphlet which I received a couple of days ago by a clergyman, you would laugh, and admit that I had some excuse for bitterness. After abusing me for two or three pages, in language sufficiently plain and emphatic to have satisfied any reasonable man, he sums up by saying that he has vainly searched the English language to find terms to express his contempt for me and all Darwinians.' In another letter, after I had left Down, he writes, 'We often differed, but you are one of those rare mortals from whom one can differ and yet feel no shade of animosity, and that is a thing [of] which I should feel very proud, if any one could say [it] of me.'

"On my last visit to Down, Mr. Darwin said, at his dinner-table, 'Brodie Innes and I have been fast friends for thirty years, and we never thoroughly agreed on any subject but once, and then we stared hard at each other, and thought one of us must be very ill.'"]

C. Darwin to C. Lyell.

Down, February 23rd [1860].

MY DEAR LYELL,—That is a splendid answer of the father of Judge Crampton. How curious that the Judge should have hit on exactly the same points as yourself. It shows me what a capital lawyer you would have made, how many unjust acts you would have made appear just! But how much grander a field has science been than the law, though the latter might have made you Lord Kinnordy. I will, if there be another edition, enlarge on gradation in the eye, and on all forms coming from one prototype, so as to try and make both less glaringly improbable. . . .

With respect to Bronn's objection that it cannot be shown how life arises, and likewise to a certain extent Asa Gray's remark that natural selection is not a *vera causa*, I was much

© The Complete Work of Charles Darwin Online

interested by finding accidentally in Brewster's 'Life of Newton,' that Leibnitz objected to the law of gravity because Newton could not show what gravity itself is. As it has chanced, I have used in letters this very same argument, little knowing that any one had really thus objected to the law of gravity. Newton answers by saying that it is philosophy to make out the movements of a clock, though you do not know why the weight descends to the ground. Leibnitz further objected that the law of gravity was opposed to Natural Religion! Is this not curious? I really think I shall use the facts for some introductory remarks for my bigger book.

. . . You ask (I see) why we do not have monstrosities in higher animals; but when they live they are almost always sterile (even giants and dwarfs are *generally* sterile), and we do not know that Harvey's monster would have bred. There is I believe only one case on record of a peloric flower being fertile, and I cannot remember whether this reproduced itself.

To recur to the eye. I really think it would have been dishonest, not to have faced the difficulty; and worse (as Talleyrand would have said), it would have been impolitic I think, for it would have been thrown in my teeth, as H. Holland threw the bones of the ear, till Huxley shut him up by showing what a fine gradation occurred amongst living creatures.

I thank you much for your most pleasant letter.

Yours affectionately,

C. DARWIN.

P.S.—I send a letter by Herbert Spencer, which you can read or not as you think fit. He puts, to my mind, the philosophy of the argument better than almost any one, at the close of the letter. I could make nothing of Dana's idealistic notions about species; but then, as Wollaston says, I have not a metaphysical head.

By the way, I have thrown at Wollaston's head, a paper by Alexander Jordan, who demonstrates metaphysically that all our cultivated races are God-created species.

© The Complete Work of Charles Darwin Online

Wollaston misrepresents accidentally, to a wonderful extent, some passages in my book. He reviewed, without relooking at certain passages.

C. Darwin to C. Lyell.

Down, February 25th [1860].

. . . . . I cannot help wondering at your zeal about my book. I declare to heaven you seem to care as much about my book as I do myself. You have no right to be so eminently unselfish! I have taken off my spit [*i.e.* file] a letter of Ramsay's, as every geologist convert I think very important. By the way, I saw some time ago a letter from H. D. Rogers* to Huxley, in which he goes very far with us. . . . .

C. Darwin to J. D. Hooker.

Down, Saturday March 3rd, [1860].

MY DEAR HOOKER,—What a day's work you had on that Thursday! I was not able to go to London till Monday, and then I was a fool for going, for, on Tuesday night, I had an attack of fever (with a touch of pleurisy), which came on like a lion, but went off as a lamb, but has shattered me a good bit.

I was much interested by your last note. . . . I think you expect too much in regard to change of opinion on the subject of Species. One large class of men, more especially I suspect of naturalists, never will care about *any* general question, of which old Gray, of the British Museum, may be taken as a type; and secondly, nearly all men past a moderate age, either in actual years or in mind, are, I am fully convinced, incapable of looking at facts under a new point of view. Seriously, I am astonished and rejoiced at the progress which

* Professor of Geology in the University of Glasgow. Born in the United States 1809, died 1866.

U 2

© The Complete Work of Charles Darwin Online

the subject has made; look at the enclosed memorandum.* —— says my book will be forgotten in ten years, perhaps so; but, with such a list, I feel convinced the subject will not. The outsiders, as you say, are strong.

You say that you think that Bentham is touched, "but, like a wise man, holds his tongue." Perhaps you only mean that he cannot decide, otherwise I should think such silence the reverse of magnanimity; for if others behaved the same way, how would opinion ever progress? It is a dereliction of actual duty.†

I am so glad to hear about Thwaites.‡ . . . I have had an astounding letter from Dr. Boott; § it might be turned into ridicule against him and me, so I will not send it to any one. He writes in a noble spirit of love of truth.

I wonder what Lindley thinks; probably too busy to read or think on the question.

I am vexed about Bentham's reticence, for it would have been of real value to know what parts appeared weakest to a man of his powers of observation.

Farewell, my dear Hooker, yours affectionately,

C. DARWIN.

P.S.—Is not Harvey in the class of men who do not at all care for generalities? I remember your saying you could

* See table of names, p. 293.

† In a subsequent letter to Sir J. D. Hooker (March 12th, 1860), my father wrote, "I now quite understand Bentham's silence."

‡ Dr. G. J. K. Thwaites, who was born in 1811, established a reputation in this country as an expert microscopist and an acute observer, working especially at cryptogamic botany. On his appointment as Director of the Botanic Gardens at Peradenyia, Ceylon, Dr. Thwaites devoted himself to the flora of Ceylon. As a result of this he has left numerous and valuable collections, a description of which he embodied in his 'Enumeratio Plantarum Zeylaniae' (1864). Dr. Thwaites was a Fellow of the Linnean Society, but beyond the above facts, little seems to have been recorded of his life. His death occurred in Ceylon on September 11th, 1882, in his seventy-second year. *Athenæum*, October 14th, 1882, p. 500.

§ The letter is enthusiastically laudatory, and obviously full of genuine feeling.

© The Complete Work of Charles Darwin Online

not get him to write on Distribution. I have found his works very unfruitful in every respect.

[Here follows the memorandum referred to:]

Geologists.	Zoologists and Palæontologists.	Physiologists.	Botanists.
Lyell.	Huxley.	Carpenter.	Hooker.
Ramsay.*	J. Lubbock.	Sir H. Holland (to large extent).	H. C. Watson.
Jukes.†	L. Jenyns (to large extent).		Asa Gray (to some extent).
H. D. Rogers.	Searles Wood.‡		Dr. Boott (to large extent).
			Thwaites.

[The following letter is of interest in connection with the mention of Mr. Bentham in the last letter:]

G. Bentham to Francis Darwin.

25 Wilton Place, S.W.,
May 30th, 1882.

MY DEAR SIR.—In compliance with your note which I received last night, I send herewith the letters I have from your father. I should have done so on seeing the general request published in the papers, but that I did not think there were any among them which could be of any use to you. Highly flattered as I was by the kind and friendly notice with which Mr. Darwin occasionally honoured me, I

* Andrew Ramsay, late Director-General of the Geological Survey.

† Joseph Beete Jukes, M.A., F.R.S., born 1811, died 1869. He was educated at Cambridge, and from 1842 to 1846 he acted as naturalist to H.M.S. *Fly*, on an exploring expedition in Australia and New Guinea. He was afterwards appointed Director of the Geological Survey of Ireland. He was the author of many papers, and of more than one good hand-book of geology.

‡ Searles Valentine Wood, born Feb. 14, 1798, died 1880. Chiefly known for his work on the Mollusca of the 'Crag.'

© The Complete Work of Charles Darwin Online

was never admitted into his intimacy, and he therefore never made any communications to me in relation to his views and labours. I have been throughout one of his most sincere admirers, and fully adopted his theories and conclusions, notwithstanding the severe pain and disappointment they at first occasioned me. On the day that his celebrated paper was read at the Linnean Society, July 1st, 1858, a long paper of mine had been set down for reading, in which, in commenting on the British Flora, I had collected a number of observations and facts illustrating what I then believed to be a fixity in species, however difficult it might be to assign their limits, and showing a tendency of abnormal forms produced by cultivation or otherwise, to withdraw within those original limits when left to themselves. Most fortunately my paper had to give way to Mr. Darwin's, and when once that was read, I felt bound to defer mine for reconsideration; I began to entertain doubts on the subject, and on the appearance of the 'Origin of Species,' I was forced, however reluctantly, to give up my long-cherished convictions, the results of much labour and study, and I cancelled all that part of my paper which urged original fixity, and published only portions of the remainder in another form, chiefly in the 'Natural History Review.' I have since acknowledged on various occasions my full adoption of Mr. Darwin's views, and chiefly in my Presidential Address of 1863, and in my thirteenth and last address, issued in the form of a report to the British Association at its meeting at Belfast in 1874.

I prize so highly the letters that I have of Mr. Darwin's, that I should feel obliged by your returning them to me when you have done with them. Unfortunately I have not kept the envelopes, and Mr. Darwin usually only dated them by the month not by the year, so that they are not in any chronological order.

Yours very sincerely,
GEORGE BENTHAM.

© The Complete Work of Charles Darwin Online

C. Darwin to C. Lyell.

Down [March] 12th [1860].

MY DEAR LYELL,—Thinking over what we talked about, the high state of intellectual development of the old Grecians with the little or no subsequent improvement, being an apparent difficulty, it has just occurred to me that in fact the case harmonises perfectly with our views. The case would be a decided difficulty on the Lamarckian or Vestigian doctrine of necessary progression, but on the view which I hold of progression depending on the conditions, it is no objection at all, and harmonises with the other facts of progression in the corporeal structure of other animals. For in a state of anarchy, or despotism, or bad government, or after irruption of barbarians, force, strength, or ferocity, and not intellect, would be apt to gain the day.

We have so enjoyed your and Lady Lyell's visit.

Good-night.

C. DARWIN.

P.S.—By an odd chance (for I had not alluded even to the subject) the ladies attacked me this evening, and threw the high state of old Grecians into my teeth, as an unanswerable difficulty, but by good chance I had my answer all pat, and silenced them. Hence I have thought it worth scribbling to you. . . .

*C. Darwin to J. Prestwich.**

Down, March 12th [1860].

. . . At some future time, when you have a little leisure, and when you have read my 'Origin of Species,' I should esteem it a *singular* favour if you would send me any general criticisms. I do not mean of unreasonable length, but such

* Now Professor of Geology in the University of Oxford.

© The Complete Work of Charles Darwin Online

as you could include in a letter. I have always admired your various memoirs so much that I should be eminently glad to receive your opinion, which might be of real service to me.

Pray do not suppose that I expect to *convert* or *pervert* you; if I could stagger you in ever so slight a degree I should be satisfied; nor fear to annoy me by severe criticisms, for I have had some hearty kicks from some of my best friends. If it would not be disagreeable to you to send me your opinion, I certainly should be truly obliged. . . .

C. Darwin to Asa Gray.

Down, April 3 [1860].

. . . . I remember well the time when the thought of the eye made me cold all over, but I have got over this stage of the complaint, and now small trifling particulars of structure often make me very uncomfortable. The sight of a feather in a peacock's tail, whenever I gaze at it, makes me sick! . . .

You may like to hear about reviews on my book. Sedgwick (as I and Lyell feel *certain* from internal evidence) has reviewed me savagely and unfairly in the *Spectator*.* The notice includes much abuse, and is hardly fair in several respects. He would actually lead any one, who was ignorant of geology, to suppose that I had invented the great gaps between successive geological formations, instead of its being an almost universally admitted dogma. But my dear old friend Sedgwick, with his noble heart, is old, and is rabid with indignation. It is hard to please every one; you may remember that in my last letter I asked you to leave out about the Weald denudation: I told Jukes this (who is head man of the Irish geological survey), and he blamed me much, for he believed every word of it, and thought it not at all exaggerated! In fact, geologists have no means of gauging the infinitude of past time. There has been one prodigy of a

* See the quotations which follow the present letter.

© The Complete Work of Charles Darwin Online

review, namely, an *opposed* one (by Pictet,* the palæontologist, in the Bib. Universelle of Geneva) which is *perfectly* fair and just, and I agree to every word he says; our only difference being that he attaches less weight to arguments in favour, and more to arguments opposed, than I do. Of all the opposed reviews, I think this the only quite fair one, and I never expected to see one. Please observe that I do not class your review by any means as opposed, though you think so yourself! It has done me *much* too good service ever to appear in that rank in my eyes. But I fear I shall weary you with so much about my book. I should rather think there was a good chance of my becoming the most egotistical man in all Europe! What a proud pre-eminence! Well, you have helped to make me so, and therefore you must forgive me if you can.

My dear Gray, ever yours most gratefully,

C. DARWIN.

[In a letter to Sir Charles Lyell reference is made to Sedgwick's review in the *Spectator*, March 24:

"I now feel certain that Sedgwick is the author of the article in the *Spectator*. No one else could use such abusive terms. And what a misrepresentation of my notions! Any ignoramus would suppose that I had *first* broached the

* François Jules Pictet, in the 'Archives des Sciences de la Bibliothèque Universelle,' Mars 1860. The article is written in a courteous and considerate tone, and concludes by saying that the 'Origin' will be of real value to naturalists, especially if they are not led away by its seductive arguments to believe in the dangerous doctrine of modification. A passage which seems to have struck my father as being valuable, and opposite which he has made double pencil marks and written the word "good," is worth quoting: "La théorie de M. Darwin s'accorde mal avec l'histoire des types à formes bien tranchées et définies qui paraissent n'avoir vécu que pendant un temps limité. On en pourrait citer des centaines d'exemples, tel que les reptiles volants, les ichthyosaures, les bélemnites, les ammonites, &c." Pictet was born in 1809, died 1872; he was Professor of Anatomy and Zoology at Geneva.

© The Complete Work of Charles Darwin Online

doctrine, that the breaks between successive formations marked long intervals of time. It is very unfair. But poor dear old Sedgwick seems rabid on the question. "Demoralised understanding!" If ever I talk with him I will tell him that I never could believe that an inquisitor could be a good man; but now I know that a man may roast another, and yet have as kind and noble a heart as Sedgwick's."

The following passages are taken from the review:

"I need hardly go on any further with these objections. But I cannot conclude without expressing my detestation of the theory, because of its unflinching materialism;—because it has deserted the inductive track, the only track that leads to physical truth;—because it utterly repudiates final causes, and thereby indicates a demoralised understanding on the part of its advocates."

"Not that I believe that Darwin is an atheist; though I cannot but regard his materialism as atheistical. I think it untrue, because opposed to the obvious course of nature, and the very opposite of inductive truth. And I think it intensely mischievous."

"Each series of facts is laced together by a series of assumptions, and repetitions of the one false principle. You cannot make a good rope out of a string of air bubbles."

"But any startling and (supposed) novel paradox, maintained very boldly and with something of imposing plausibility, produces in some minds a kind of pleasing excitement which predisposes them in its favour; and if they are unused to careful reflection, and averse to the labour of accurate investigation, they will be likely to conclude that what is (apparently) *original*, must be a production of original *genius*, and that anything very much opposed to prevailing notions must be a grand *discovery*,— in short, that whatever comes from the 'bottom of a well' must be the 'truth' supposed to be hidden there."

© The Complete Work of Charles Darwin Online

In a review in the December number of 'Macmillan's Magazine,' 1860, Fawcett vigorously defended my father from the charge of employing a false method of reasoning; a charge which occurs in Sedgwick's review, and was made at the time *ad nauseam*, in such phrases as: "This is not the true Baconian method." Fawcett repeated his defence at the meeting of the British Association in 1861.*]

C. Darwin to W. B. Carpenter.

Down, April 6th [1860].

MY DEAR CARPENTER,—I have this minute finished your review in the 'Med. Chirurg. Review.'† You must let me express my admiration at this most able essay, and I hope to God it will be largely read, for it must produce a great effect. I ought not, however, to express such warm admiration, for you give my book, I fear, far too much praise. But you have gratified me extremely; and though I hope I do not care very much for the approbation of the non-scientific readers, I cannot say that this is at all so with respect to such few men as yourself. I have not a criticism to make, for I object to not a word; and I admire all, so that I cannot pick out one part as better than the rest. It is all so well balanced. But it is impossible not to be struck with your extent of knowledge in geology, botany, and zoology. The extracts which you give from Hooker seem to me *excellently* chosen, and most forcible. I am so much pleased in what you say also about Lyell. In fact I am in a fit of enthusiasm, and had better write no more. With cordial thanks,

Yours very sincerely,
C. DARWIN.

* See an interesting letter from my father in Mr. Stephen's 'Life of Henry Fawcett,' 1886, p. 101.

† April 1860.

© The Complete Work of Charles Darwin Online

C. Darwin to C. Lyell.

Down, April 10th [1860].

My dear Lyell,—Thank you much for your note of the 4th; I am very glad to hear that you are at Torquay. I should have amused myself earlier by writing to you, but I have had Hooker and Huxley staying here, and they have fully occupied my time, as a little of anything is a full dose for me. . . . There has been a plethora of reviews, and I am really quite sick of myself. There is a very long review by Carpenter in the 'Medical and Chirurg. Review,' very good and well balanced, but not brilliant. He discusses Hooker's books at as great length as mine, and makes excellent extracts; but I could not get Hooker to feel the least interest in being praised.

Carpenter speaks of you in thoroughly proper terms. There is a *brilliant* review by Huxley,* with capital hits, but I do not know that he much advances the subject. I *think* I have convinced him that he has hardly allowed weight enough to the case of varieties of plants being in some degrees sterile.

To diverge from reviews: Asa Gray sends me from Wyman (who will write), a good case of all the pigs being black in the Everglades of Virginia. On asking about the cause, it seems (I have got capital analogous cases) that when the *black* pigs eat a certain nut their bones become red, and they suffer to a certain extent, but that the *white* pigs lose their hoofs and perish, "and we aid by *selection*, for we kill most of the young white pigs." This was said by men who could hardly read. By the way, it is a great blow to me that you cannot admit the potency of natural selection. The more I think of it, the less I doubt its power for great and small changes. I have just read the 'Edinburgh,'† which without doubt is by ——. It is extremely malignant, clever, and I fear will be very damaging. He is atrociously severe on Huxley's lecture,

* 'Westminster Review,' April 1860.

† 'Edinburgh Review,' April 1860.

© The Complete Work of Charles Darwin Online

and very bitter against Hooker. So we three *enjoyed* it together. Not that I really enjoyed it, for it made me uncomfortable for one night; but I have got quite over it to-day. It requires much study to appreciate all the bitter spite of many of the remarks against me; indeed I did not discover all myself. It scandalously misrepresents many parts. He misquotes some passages, altering words within inverted commas. . . .

It is painful to be hated in the intense degree with which —— hates me.

Now for a curious thing about my book, and then I have done. In last Saturday's *Gardeners' Chronicle*,* a Mr. Patrick Matthew publishes a long extract from his work on 'Naval Timber and Arboriculture,' published in 1831, in which he briefly but completely anticipates the theory of Natural Selection. I have ordered the book, as some few passages are rather obscure, but it is certainly, I think, a complete but not developed anticipation! Erasmus always said that surely this would be shown to be the case some day. Anyhow, one may be excused in not having discovered the fact in a work on Naval Timber.

I heartily hope that your Torquay work may be successful. Give my kindest remembrances to Falconer, and I hope he is pretty well. Hooker and Huxley (with Mrs. Huxley) were extremely pleasant. But poor dear Hooker is tired to death of my book, and it is a marvel and a prodigy if you are not worse tired—if that be possible. Farewell, my dear Lyell,

Yours affectionately,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down [April 13th, 1860].

MY DEAR HOOKER,—Questions of priority so often lead to odious quarrels, that I should esteem it a great favour if you

* April 7th, 1860.

© The Complete Work of Charles Darwin Online

would read the enclosed.* If you think it proper that I should send it (and of this there can hardly be any question), and if you think it full and ample enough, please alter the date to the day on which you post it, and let that be soon. The case in the *Gardeners' Chronicle* seems a *little* stronger than in Mr. Matthew's book, for the passages are therein scattered in three places; but it would be mere hair-splitting to notice that. If you object to my letter, please return it; but I do not expect that you will, but I thought that you would not object to run your eye over it. My dear Hooker, it is a great thing for me to have so good, true, and old a friend as you. I owe much for science to my friends.

Many thanks for Huxley's lecture. The latter part seemed to be grandly eloquent.

. . . I have gone over [the 'Edinburgh'] review again, and compared passages, and I am astonished at the misrepresentations. But I am glad I resolved not to answer. Perhaps it is selfish, but to answer and think more on the subject is too unpleasant. I am so sorry that Huxley by my means has been thus atrociously attacked. I do not suppose you much care about the gratuitous attack on you.

* My father wrote (*Gardeners' Chronicle*, 1860, p. 362, April 21st): "I have been much interested by Mr. Patrick Matthew's communication in the number of your paper dated April 7th. I freely acknowledge that Mr. Matthew has anticipated by many years the explanation which I have offered of the origin of species, under the name of natural selection. I think that no one will feel surprised that neither I, nor apparently any other naturalist, had heard of Mr. Matthew's views, considering how briefly they are given, and that they appeared in the appendix to a work on Naval Timber and Arboriculture. I can do no more than offer my apologies to Mr. Matthew for my entire ignorance of his publication. If another edition of my work is called for, I will insert to the foregoing effect." In spite of my father's recognition of his claims, Mr. Matthew remained unsatisfied, and complained that an article in the 'Saturday Analyst and Leader' was "scarcely fair in alluding to Mr. Darwin as the parent of the origin of species, seeing that I published the whole that Mr. Darwin attempts to prove, more than twenty-nine years ago."—*Saturday Analyst and Leader*, Nov. 24, 1860.

© The Complete Work of Charles Darwin Online

Lyell in his letter remarked that you seemed to him as if you were overworked. Do, pray, be cautious, and remember how many and many a man has done this—who thought it absurd till too late. I have often thought the same. You know that you were bad enough before your Indian journey.

C. Darwin to C. Lyell.

Down, April [1860].

MY DEAR LYELL,—I was very glad to get your nice long letter from Torquay. A press of letters prevented me writing to Wells. I was particularly glad to hear what you thought about not noticing [the 'Edinburgh'] review. Hooker and Huxley thought it a sort of duty to point out the alteration of quoted citations, and there is truth in this remark; but I so hated the thought that I resolved not to do so. I shall come up to London on Saturday the 14th, for Sir B. Brodie's party, as I have an accumulation of things to do in London, and will (if I do not hear to the contrary) call about a quarter before ten on Sunday morning, and sit with you at breakfast, but will not sit long, and so take up much of your time. I must say one more word about our quasi-theological controversy about natural selection, and let me have your opinion when we meet in London. Do you consider that the successive variations in the size of the crop of the Pouter Pigeon, which man has accumulated to please his caprice, have been due to "the creative and sustaining powers of Brahma?" In the sense that an omnipotent and omniscient Deity must order and know everything, this must be admitted; yet, in honest truth, I can hardly admit it. It seems preposterous that a maker of a universe should care about the crop of a pigeon solely to please man's silly fancies. But if you agree with me in thinking such an interposition of the Deity uncalled for, I can see no reason whatever for believing in such interpositions in the case of natural beings, in which strange and admirable peculiarities

© The Complete Work of Charles Darwin Online

have been naturally selected for the creature's own benefit. Imagine a Pouter in a state of nature wading into the water and then, being buoyed up by its inflated crop, sailing about in search of food. What admiration this would have excited —adaptation to the laws of hydrostatic pressure, &c. &c. For the life of me I cannot see any difficulty in natural selection producing the most exquisite structure, *if such structure can be arrived at by gradation*, and I know from experience how hard it is to name any structure towards which at least some gradations are not known.

Ever yours,
C. DARWIN.

P.S.—The conclusion at which I have come, as I have told Asa Gray, is that such a question, as is touched on in this note, is beyond the human intellect, like "predestination and free will," or the "origin of evil."

C. Darwin to J. D. Hooker.

Down [April 18th, 1860].

MY DEAR HOOKER,—I return ——'s letter. . . . Some of my relations say it cannot *possibly* be ——'s article,* because the reviewer speaks so very highly of ——. Poor dear simple folk! My clever neighbour, Mr. Norman, says the article is so badly written, with no definite object, that no one will read it. . . . Asa Gray has sent me an article † from the United States, clever, and dead against me. But one argument is funny. The reviewer says, that if the doctrine were true, geological strata would be full of monsters which have failed. A very clear view this writer had of the struggle for existence!

* The 'Edinburgh Review.'

† 'North American Review,' April 1860. "By Professor Bowen," is written on my father's copy. The passage referred to occurs at p. 488, where the author says that we ought to find "an infinite number of other varieties—gross, rude, and purposeless—the unmeaning creations of an unconscious cause."

© The Complete Work of Charles Darwin Online

. . . . I am glad you like Adam Bede so much. I was charmed with it. . . .

We think you must by mistake have taken with your own numbers of the 'National Review' my precious number.*
I wish you would look.

C. Darwin to Asa Gray.

Down, April 25th [1860].

MY DEAR GRAY,—I have no doubt I have to thank you for the copy of a review on the 'Origin' in the 'North American Review.' It seems to me clever, and I do not doubt will damage my book. I had meant to have made some remarks on it; but Lyell wished much to keep it, and my head is quite confused between the many reviews which I have lately read. I am sure the reviewer is wrong about bees' cells, *i.e.* about the distance; any lesser distance would do, or even greater distance, but then some of the places would lie outside the generative spheres; but this would not add much difficulty to the work. The reviewer takes a strange view of instinct: he seems to regard intelligence as a developed instinct; which I believe to be wholly false. I suspect he has never much attended to instinct and the minds of animals, except perhaps by reading.

My chief object is to ask you if you could procure for me a copy of the *New York Times* for Wednesday, March 28th. It contains *a very striking* review of my book, which I should much like to keep. How curious that the two most striking reviews (*i.e.* yours and this) should have appeared in America. This review is not really useful, but somehow is impressive. There was a good review in the 'Revue des Deux Mondes,' April 1st, by M. Laugel, said to be a very clever man.

* This no doubt refers to the January number, containing Dr. Carpenter's review of the 'Origin.'

© The Complete Work of Charles Darwin Online

Hooker, about a fortnight ago, stayed here a few days, and was very pleasant; but I think he overworks himself. What a gigantic undertaking, I imagine, his and Bentham's 'Genera Plantarum' will be! I hope he will not get too much immersed in it, so as not to spare some time for Geographical Distribution and other such questions.

I have begun to work steadily, but very slowly as usual, at details on variation under domestication.

My dear Gray,

Yours always truly and gratefully,

C. DARWIN.

C. Darwin to C. Lyell.

Down [May 8th, 1860].

. . . . . . I have sent for the 'Canadian Naturalist.' If I cannot procure a copy I will borrow yours. I had a letter from Henslow this morning, who says that Sedgwick was, on last Monday night, to open a battery on me at the Cambridge Philosophical Society. Anyhow, I am much honoured by being attacked there, and at the Royal Society of Edinburgh.

I do not think it worth while to contradict single cases, nor is it worth while arguing against those who do not attend to what I state. A moment's reflection will show you that there must be (on our doctrine) large genera not varying (see p. 56 on the subject, in the second edition of the 'Origin'). Though I do not there discuss the case in detail.

It may be sheer bigotry for my own notions, but I prefer to the Atlantis, my notion of plants and animals having migrated from the Old to the New World, or conversely, when the climate was much hotter, by approximately the line of Behring's Straits. It is most important, as you say, to see living forms of plants going back so far in time. I wonder whether we shall ever discover the flora of the dry land of the coal period, and find it not so anomalous as the swamp or coal-making flora. I am working away over the blessed

© The Complete Work of Charles Darwin Online

Pigeon Manuscript; but, from one cause or another, I get on very slowly. . . .

This morning I got a letter from the Academy of Natural Sciences of Philadelphia, announcing that I am elected a correspondent. . . . It shows that some Naturalists there do not think me such a scientific profligate as many think me here.

My dear Lyell, yours gratefully,

C. DARWIN.

P.S.—What a grand fact about the extinct stag's horn worked by man!

C. Darwin to J. D. Hooker.

Down [May 13th, 1860].

MY DEAR HOOKER,—I return Henslow, which I was very glad to see. How good of him to defend me.* I will write and thank him.

As you said you were curious to hear Thomson's † opinion, I send his kind letter. He is evidently a strong opposer to us.

Yours affectionately,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down [May 15th, 1860].

. . . . . . How paltry it is in such men as X., Y. and Co. not reading your essay. It is incredibly paltry.‡ They may all attack me to their hearts' content. I am got case-hardened. As for the old fogies in Cambridge, it really signifies nothing. I look at their attacks as a proof that our work is worth the doing. It makes me resolve to buckle on my

* Against Sedgwick's attack before the Cambridge Philosophical Society.

† Dr. Thomas Thomson, the Indian botanist. He was a collaborateur in Hooker and Thomson's 'Flora Indica,' 1855.

‡ These remarks do not apply to Dr. Harvey, who was, however, in a somewhat similar position. See p. 313.

© The Complete Work of Charles Darwin Online

armour. I see plainly that it will be a long uphill fight. But think of Lyell's progress with Geology. One thing I see most plainly, that without Lyell's, yours, Huxley's, and Carpenter's aid, my book would have been a mere flash in the pan. But if we all stick to it, we shall surely gain the day. And I now see that the battle is worth fighting. I deeply hope that you think so. Does Bentham progress at all? I do not know what to say about Oxford.* I should like it much with you, but it must depend on health. . . .

Yours most affectionately,

C. DARWIN.

C. Darwin to C. Lyell.

Down, May 18th [1860].

MY DEAR LYELL,—I send a letter from Asa Gray to show how hotly the battle rages there. Also one from Wallace, very just in his remarks, though too laudatory and too modest, and how admirably free from envy or jealousy. He must be a good fellow. Perhaps I will enclose a letter from Thomson of Calcutta; not that it is much, but Hooker thinks so highly of him. . . .

Henslow informs me that Sedgwick† and then Professor Clarke [*sic*]‡ made a regular and savage onslaught on my book lately at the Cambridge Philosophical Society, but Henslow seems to have defended me well, and maintained that the subject was a legitimate one for investigation. Since

* His health prevented him from going to Oxford for the meeting of the British Association.

† Sedgwick's address is given somewhat abbreviated in *The Cambridge Chronicle*, May 19th, 1860.

‡ The late William Clark, Professor of Anatomy. My father seems to have misunderstood his informant. I am assured by Mr. J. W. Clark that his father (Prof. Clark) did not support Sedgwick in the attack.

© The Complete Work of Charles Darwin Online

then Phillips * has given lectures at Cambridge on the same subject, but treated it very fairly. How splendidly Asa Gray is fighting the battle. The effect on me of these multiplied attacks is simply to show me that the subject is worth fighting for, and assuredly I will do my best. . . . I hope all the attacks make you keep up your courage, and courage you assuredly will require. . . .

C. Darwin to A. R. Wallace.

Down, May 18th, 1860.

MY DEAR MR. WALLACE,—I received this morning your letter from Amboyna, dated February 16th, containing some remarks and your too high approval of my book. Your letter has pleased me very much, and I most completely agree with you on the parts which are strongest and which are weakest. The imperfection of the Geological Record is, as you say, the weakest of all; but yet I am pleased to find that there are almost more geological converts than of pursuers of other branches of natural science. . . . I think geologists are more easily converted than simple naturalists, because more accustomed to reasoning. Before telling you about the progress of opinion on the subject, you must let me say how I admire the generous manner in which you speak of my book. Most persons would in your position have felt some envy or jealousy. How nobly free you seem to be of this common failing of mankind. But you speak far too modestly of yourself. You would, if you had my leisure, have done the work just as well, perhaps better, than I have done it. . . . .

* John Phillips, M.A., F.R.S., born 1800, died 1874, from the effects of a fall. Professor of Geology at King's College, London, and afterwards at Oxford. He gave the 'Rede' lecture at Cambridge on May 15th, 1860, on 'The Succession of Life on the earth.' The Rede Lecturer is appointed annually by the Vice-Chancellor, and is paid by an endowment left in 1524 by Sir Robert Rede, Lord Chief Justice, in the reign of Henry VIII.

© The Complete Work of Charles Darwin Online

. . . Agassiz sends me a personal civil message, but incessantly attacks me; but Asa Gray fights like a hero in defence. Lyell keeps as firm as a tower, and this autumn will publish on the 'Geological History of Man,' and will then declare his conversion, which now is universally known. I hope that you have received Hooker's splendid essay. . . . Yesterday I heard from Lyell that a German, Dr. Schaaffhausen,* has sent him a pamphlet published some years ago, in which the same view is nearly anticipated; but I have not yet seen this pamphlet. My brother, who is a very sagacious man, always said, "you will find that some one will have been before you." I am at work at my larger work, which I shall publish in a separate volume. But from ill-health and swarms of letters, I get on very very slowly. I hope that I shall not have wearied you with these details. With sincere thanks for your letter, and with most deeply felt wishes for your success in science, and in every way, believe me,

Your sincere well-wisher,

C. DARWIN.

C. Darwin to Asa Gray.

Down, May 22nd [1860].

MY DEAR GRAY,—Again I have to thank you for one of your very pleasant letters of May 7th, enclosing a very pleasant remittance of £22. I am in simple truth astonished at all the kind trouble you have taken for me. I return Appletons' account. For the chance of your wishing for a formal acknowledgement I send one. If you have any further communication to the Appletons, pray express my acknowledgement for [their] generosity; for it is generosity in my opinion. I am not at all surprised at the sale diminishing; my extreme

* Hermann Schaaffhausen 'Ueber Beständigkeit und Umwandlung der Arten.' Verhandl. d. Naturhist. Vereins, Bonn, 1853. See 'Origin,' Historical Sketch.

© The Complete Work of Charles Darwin Online

surprise is at the greatness of the sale. No doubt the public has been *shamefully* imposed on! for they bought the book thinking that it would be nice easy reading. I expect the sale to stop soon in England, yet Lyell wrote to me the other day that calling at Murray's he heard that fifty copies had gone in the previous forty-eight hours. I am extremely glad that you will notice in 'Silliman' the additions in the 'Origin.' Judging from letters (and I have just seen one from Thwaites to Hooker), and from remarks, the most serious omission in my book was not explaining how it is, as I believe, that all forms do not necessarily advance, how there can now be *simple* organisms still existing. . . . I hear there is a *very* severe review on me in the 'North British,' by a Rev. Mr. Dunns,* a Free Kirk minister, and dabbler in Natural History. I should be very glad to see any good American reviews, as they are all more or less useful. You say that you shall touch on other reviews. Huxley told me some time ago that after a time he would write a review on all the reviews, whether he will I know not. If you allude to the 'Edinburgh,' pray notice *some* of the points which I will point out on a separate slip. In the *Saturday Review* (one of our cleverest periodicals) of May 5th, p. 573, there is a nice article on [the 'Edinburgh'] review, defending Huxley, but not Hooker; and the latter, I think, [the 'Edinburgh' reviewer] treats most ungenerously.† But surely you will get sick unto death of me and my reviewers.

With respect to the theological view of the question. This is always painful to me. I am bewildered. I had no inten-

* This statement as to authorship was made on the authority of Robert Chambers.

† In a letter to Mr. Huxley my father wrote: "Have you seen the last *Saturday Review*? I am very glad of the defence of you and of myself. I wish the reviewer had noticed Hooker. The reviewer, whoever he is, is a jolly good fellow, as this review and the last on me showed. He writes capitally, and understands well his subject. I wish he had slapped [the 'Edinburgh' reviewer] a little bit harder."

© The Complete Work of Charles Darwin Online

tion to write atheistically. But I own that I cannot see as plainly as others do, and as I should wish to do, evidence of design and beneficence on all sides of us. There seems to me too much misery in the world. I cannot persuade myself that a beneficent and omnipotent God would have designedly created the Ichneumonidæ with the express intention of their feeding within the living bodies of Caterpillars, or that a cat should play with mice. Not believing this, I see no necessity in the belief that the eye was expressly designed. On the other hand, I cannot anyhow be contented to view this wonderful universe, and especially the nature of man, and to conclude that everything is the result of brute force. I am inclined to look at everything as resulting from designed laws, with the details, whether good or bad, left to the working out of what we may call chance. Not that this notion *at all* satisfies me. I feel most deeply that the whole subject is too profound for the human intellect. A dog might as well speculate on the mind of Newton. Let each man hope and believe what he can. Certainly I agree with you that my views are not at all necessarily atheistical. The lightning kills a man, whether a good one or bad one, owing to the excessively complex action of natural laws. A child (who may turn out an idiot) is born by the action of even more complex laws, and I can see no reason why a man, or other animal, may not have been aboriginally produced by other laws, and that all these laws may have been expressly designed by an omniscient Creator, who foresaw every future event and consequence. But the more I think the more bewildered I become; as indeed I have probably shown by this letter.

Most deeply do I feel your generous kindness and interest.

Yours sincerely and cordially,

CHARLES DARWIN.

[Here follow my father's criticisms on the 'Edinburgh Review':

© The Complete Work of Charles Darwin Online

"What a quibble to pretend he did not understand what I meant by *inhabitants* of South America; and any one would suppose that I had not throughout my volume touched on Geographical Distribution. He ignores also everything which I have said on Classification, Geological Succession, Homologies, Embryology, and Rudimentary Organs—p. 496.

He falsely applies what I said (too rudely) about "blindness of preconceived opinions" to those who believe in creation, whereas I exclusively apply the remark to those who give up multitudes of species as true species, but believe in the remainder—p. 500.

He slightly alters what I say,—I *ask* whether creationists really believe that elemental atoms have flashed into life. He says that I describe them as so believing, and this, surely, is a difference—p. 501.

He speaks of my "clamouring against" all who believe in creation, and this seems to me an unjust accusation—p. 501.

He makes me say that the dorsal vertebræ vary; this is simply false: I nowhere say a word about dorsal vertebræ—p. 522.

What an illiberal sentence that is about my pretension to candour, and about my rushing through barriers which stopped Cuvier: such an argument would stop any progress in science—p. 525.

How disingenuous to quote from my remark to you about my *brief* letter [published in the 'Linn. Soc. Journal'], as if it applied to the whole subject—p. 530.

How disingenuous to say that we are called on to accept the theory, from the imperfection of the geological record, when I over and over again [say] how grave a difficulty the imperfection offers—p. 530."]

C. Darwin to J. D. Hooker.

Down, May 30th [1860].

My dear Hooker,—I return Harvey's letter, I have been very glad to see the reason why he has not read your Essay.

© The Complete Work of Charles Darwin Online

I feared it was bigotry, and I am glad to see that he goes a little way (*very much* further than I supposed) with us. . . .

I was not sorry for a natural opportunity of writing to Harvey, just to show that I was not piqued at his turning me and my book into ridicule,* not that I think it was a proceeding which I deserved, or worthy of him. It delights me that you are interested in watching the progress of opinion on the change of Species; I feared that you were weary of the subject; and therefore did not send A. Gray's letters. The battle rages furiously in the United States. Gray says he was preparing a speech, which would take 1½ hours to deliver, and which he "fondly hoped would be a stunner." He is fighting splendidly, and there seem to have been many discussions with Agassiz and others at the meetings. Agassiz pities me much at being so deluded. As for the progress of opinion, I clearly see that it will be excessively slow, almost as slow as the change of species. . . . I am getting wearied at the storm of hostile reviews and hardly any useful. . . .

C. Darwin to C. Lyell.

Down, Friday night [June 1st, 1860].

. . . Have you seen Hopkins † in the new 'Fraser'? the public will, I should think, find it heavy. He will be dead

* A "serio-comic squib," read before the 'Dublin University Zoological and Botanical Association,' Feb. 17, 1860, and privately printed. My father's presentation copy is inscribed, "With the writer's *repentance*, Oct. 1860."

† William Hopkins died in 1866, "in his seventy-third year." He began life with a farm in Suffolk, but ultimately entered, comparatively late in life, at Peterhouse, Cambridge; he took his degree in 1827, and afterwards became an Esquire Bedell of the University. He was chiefly known as a mathematical "coach," and was eminently successful in the manufacture of Senior Wranglers. Nevertheless Mr. Stephen says ('Life of Fawcett,' p. 26) that he "was conspicuous for inculcating" a "liberal view of the studies of the place. He endeavoured to stimulate a philosophical interest in the mathematical sciences, instead of simply rousing

© The Complete Work of Charles Darwin Online

against me as you prophesied; but he is generously civil to me personally.* On his standard of proof, *natural* science would never progress, for without the making of theories I am convinced there would be no observation.

. . . . I have begun reading the 'North British,'† which so far strikes me as clever.

Phillips's Lecture at Cambridge is to be published.

All these reiterated attacks will tell heavily; there will be no more converts, and probably some will go back. I hope you do not grow disheartened, I am determined to fight to the last. I hear, however, that the great Buckle highly approves of my book.

I have had a note from poor Blyth,‡ of Calcutta, who is

---

an ardour for competition." He contributed many papers on geological and mathematical subjects to the scientific journals. He had a strong influence for good over the younger men with whom he came in contact. The letter which he wrote to Henry Fawcett on the occasion of his blindness illustrates this. Mr. Stephen says ('Life of Fawcett,' p. 48) that by "this timely word of good cheer," Fawcett was roused from "his temporary prostration," and enabled to take a "more cheerful and resolute tone."

* 'Fraser's Magazine,' June 1860. My father, no doubt, refers to the following passage, p. 752, where the Reviewer expresses his "full participation in the high respect in which the author is universally held, both as a man and a naturalist; and the more so, because in the remarks which will follow in the second part of this Essay we shall be found to differ widely from him as regards many of his conclusions and the reasonings on which he has founded them, and shall claim the full right to express such differences of opinion with all that freedom which the interests of scientific truth demands, and which we are sure Mr. Darwin would be one of the last to refuse to any one prepared to exercise it with candour and courtesy." Speaking of this review, my father wrote to Dr. Asa Gray: "I have remonstrated with him [Hopkins] for so coolly saying that I base my views on what I reckon as great difficulties. Any one, by taking these difficulties alone, can make a most strong case against me. I could myself write a more damning review than has as yet appeared!" A second notice by Hopkins appeared in the July number of 'Fraser's Magazine.'

† May 1860.

‡ Edward Blyth, born 1810, died 1873. His indomitable love of natural history made him neglect the druggist's business with which he started in life, and he soon got into serious difficulties. After sup-

© The Complete Work of Charles Darwin Online

much disappointed at hearing that Lord Canning will not grant any money; so I much fear that all your great pains will be thrown away. Blyth says (and he is in many respects a very good judge) that his ideas on Species are quite revolutionized . . . .

C. Darwin to J. D. Hooker.

Down, June 5th [1860].

MY DEAR HOOKER,—It is a pleasure to me to write to you, as I have no one to talk about such matters as we write on. But I seriously beg you not to write to me unless so inclined; for busy as you are, and seeing many people, the case is very different between us. . . .

Have you seen ——'s abusive article on me? . . . It outdoes even the 'North British' and 'Edinburgh' in misapprehension and misrepresentation. I never knew anything so unfair as in discussing cells of bees, his ignoring the case of Melipona, which builds combs almost exactly intermediate between hive and humble bees. What has —— done that he feels so immeasurably superior to all us wretched naturalists, and to all political economists, including that great philosopher Malthus? This review, however, and Harvey's letter have convinced me that I must be a very bad explainer. Neither

porting himself for a few years as a writer on Field Natural History, he ultimately went out to India as Curator of the Museum of the R. Asiatic Soc. of Bengal, where the greater part of his working life was spent. His chief publications were the monthly reports made as part of his duty to the Society. He had stored in his remarkable memory a wonderful wealth of knowledge, especially with regard to the mammalia and birds of India—knowledge of which he freely gave to those who asked. His letters to my father give evidence of having been carefully studied, and the long list of entries after his name in the index to 'Animals and Plants,' show how much help was received from him. His life was an unprosperous and unhappy one, full of money difficulties and darkened by the death of his wife after a few years of marriage.

© The Complete Work of Charles Darwin Online

really understand what I mean by Natural Selection. I am inclined to give up the attempt as hopeless. Those who do not understand, it seems, cannot be made to understand.

By the way, I think, we entirely agree, except perhaps that I use too forcible language about selection. I entirely agree, indeed would almost go further than you when you say that climate (*i.e.* variability from all unknown causes) is "an active handmaid, influencing its mistress most materially." Indeed, I have never hinted that Natural Selection is "the efficient cause to the exclusion of the other," *i.e.* variability from Climate, &c. The very term *selection* implies something, *i.e.* variation or difference, to be selected. . . .

How does your book progress (I mean your general sort of book on plants), I hope to God you will be more successful than I have been in making people understand your meaning. I should begin to think myself wholly in the wrong, and that I was an utter fool, but then I cannot yet persuade myself, that Lyell, and you and Huxley, Carpenter, Asa Gray, and Watson, &c., are all fools together. Well, time will show, and nothing but time. Farewell. . . .

C. Darwin to C. Lyell.

Down, June 6th [1860].

. . . It consoles me that —— sneers at Malthus, for that clearly shows, mathematician though he may be, he cannot understand common reasoning. By the way what a discouraging example Malthus is, to show during what long years the plainest case may be misrepresented and misunderstood. I have read the 'Future'; how curious it is that several of my reviewers should advance such wild arguments, as that varieties of dogs and cats do not mingle; and should bring up the old exploded doctrine of definite analogies . . . I am beginning to despair of ever making the majority understand my notions. Even Hopkins

© The Complete Work of Charles Darwin Online

does not thoroughly. By the way, I have been so much pleased by the way he personally alludes to me. I must be a very bad explainer. I hope to Heaven that you will succeed better. Several reviews and several letters have shown me too clearly how little I am understood. I suppose "natural selection" was a bad term; but to change it now, I think, would make confusion worse confounded, nor can I think of a better; "Natural Preservation" would not imply a preservation of particular varieties, and would seem a truism, and would not bring man's and nature's selection under one point of view. I can only hope by reiterated explanations finally to make the matter clearer. If my MS. spreads out, I think I shall publish one volume exclusively on variation of animals and plants under domestication. I want to show that I have not been quite so rash as many suppose.

Though weary of reviews, I should like to see Lowell's* some time. . . . I suppose Lowell's difficulty about instinct is the same as Bowen's; but it seems to me wholly to rest on the assumption that instincts cannot graduate as finely as structures. I have stated in my volume that it is hardly possible to know which, *i.e.* whether instinct or structure, change first by insensible steps. Probably sometimes instinct, sometimes structure. When a British insect feeds on an exotic plant, instinct has changed by very small steps, and their structures might change so as to fully profit by the new food. Or structure might change first, as the direction of tusks in one variety of Indian elephants, which leads it to attack the tiger in a different manner from other kinds of elephants. Thanks for your letter of the 2nd, chiefly about Murray. (N.B. Harvey of Dublin gives me, in a letter, the argument of tall men marrying short women, as one of great weight!†)

* The late J. A. Lowell in the 'Christian Examiner' (Boston, U. S.), May, 1860.

† See footnote, *ante*, p. 261.

© The Complete Work of Charles Darwin Online

I do not quite understand what you mean by saying, "that the more they prove that you underrate physical conditions, the better for you, as Geology comes in to your aid."

. . . I see in Murray and many others one incessant fallacy, when alluding to slight differences of physical conditions as being very important; namely, oblivion of the fact that all species, except very local ones, range over a considerable area, and though exposed to what the world calls considerable *diversities*, yet keep constant. I have just alluded to this in the 'Origin' in comparing the productions of the Old and the New Worlds. Farewell, shall you be at Oxford? If H. gets quite well, perhaps I shall go there.

Yours affectionately,

C. DARWIN.

C. Darwin to C. Lyell.

Down [June 14th, 1860].

. . . Lowell's review * is pleasantly written, but it is clear that he is not a naturalist. He quite overlooks the importance of the accumulation of mere individual differences, and which, I think I can show, is the great agency of change under domestication. I have not finished Schaaffhausen, as I read German so badly. I have ordered a copy for myself, and should like to keep yours till my own arrives, but will return it to you instantly if wanted. He admits statements rather rashly, as I dare say I do. I see only one sentence as yet at all approaching natural selection.

There is a notice of me in the penultimate number of 'All the Year Round,' but not worth consulting; chiefly a well-done hash of my own words. Your last note was very interesting and consolatory to me.

I have expressly stated that I believe physical conditions have a more direct effect on plants than on animals. But the

* J. A. Lowell in the 'Christian Examiner,' May 1860.

© The Complete Work of Charles Darwin Online

more I study, the more I am led to think that natural selection regulates, in a state of nature, most trifling differences. As squared stone, or bricks, or timber, are the indispensable materials for a building, and influence its character, so is variability not only indispensable but influential. Yet in the same manner as the architect is the *all* important person in a building, so is selection with organic bodies. . . . .

[The meeting of the British Association at Oxford in 1860 is famous for two pitched battles over the 'Origin of Species.' Both of them originated in unimportant papers. On Thursday, June 28, Dr. Daubeny of Oxford made a communication to Section D: "On the final causes of the sexuality of plants, with particular reference to Mr. Darwin's work on the 'Origin of Species.'" Mr. Huxley was called on by the President, but tried (according to the *Athenæum* report) to avoid a discussion, on the ground "that a general audience, in which sentiment would unduly interfere with intellect, was not the public before which such a discussion should be carried on." However, the subject was not allowed to drop. Sir R. Owen (I quote from the *Athenæum*, July 7, 1860), who "wished to approach this subject in the spirit of the philosopher," expressed his "conviction that there were facts by which the public could come to some conclusion with regard to the probabilities of the truth of Mr. Darwin's theory." He went on to say that the brain of the gorilla "presented more differences, as compared with the brain of man, than it did when compared with the brains of the very lowest and most problematical of the Quadrumana." Mr. Huxley replied, and gave these assertions a "direct and unqualified contradiction," pledging himself to "justify that unusual procedure elsewhere," * a pledge which he amply fulfilled.† On Friday there was peace, but on Saturday 30th, the battle arose with

* 'Man's Place in Nature,' by T. H. Huxley, 1863, p. 114.

† See the 'Nat. Hist. Review,' 1861.

© The Complete Work of Charles Darwin Online

redoubled fury over a paper by Dr. Draper of New York, on the 'Intellectual development of Europe considered with reference to the views of Mr. Darwin.'

The following account is from an eye-witness of the scene.

"The excitement was tremendous. The Lecture-room, in which it had been arranged that the discussion should be held, proved far too small for the audience, and the meeting adjourned to the Library of the Museum, which was crammed to suffocation long before the champions entered the lists. The numbers were estimated at from 700 to 1000. Had it been term-time, or had the general public been admitted, it would have been impossible to have accommodated the rush to hear the oratory of the bold Bishop. Professor Henslow, the President of Section D, occupied the chair, and wisely announced *in limine* that none who had not valid arguments to bring forward on one side or the other, would be allowed to address the meeting: a caution that proved necessary, for no fewer than four combatants had their utterances burked by him, because of their indulgence in vague declamation.

"The Bishop was up to time, and spoke for full half-an-hour with inimitable spirit, emptiness and unfairness. It was evident from his handling of the subject that he had been 'crammed' up to the throat, and that he knew nothing at first hand; in fact, he used no argument not to be found in his 'Quarterly' article. He ridiculed Darwin badly, and Huxley savagely, but all in such dulcet tones, so persuasive a manner, and in such well-turned periods, that I who had been inclined to blame the President for allowing a discussion that could serve no scientific purpose, now forgave him from the bottom of my heart. Unfortunately the Bishop, hurried along on the current of his own eloquence, so far forgot himself as to push his attempted advantage to the verge of personality in a telling passage in which he turned round and addressed Huxley: I forget the precise words, and quote from Lyell. 'The Bishop asked whether Huxley was related by his grand-

© The Complete Work of Charles Darwin Online

father's or grandmother's side to an ape.'* Huxley replied to the scientific argument of his opponent with force and eloquence, and to the personal allusion with a self-restraint, that gave dignity to his crushing rejoinder."

Many versions of Mr. Huxley's speech were current: the following report of his conclusion is from a letter addressed by the late John Richard Green, then an undergraduate, to a fellow-student, now Professor Boyd Dawkins. "I asserted, and I repeat, that a man has no reason to be ashamed of having an ape for his grandfather. If there were an ancestor whom I should feel shame in recalling, it would be a *man*, a man of restless and versatile intellect, who, not content with an equivocal† success in his own sphere of activity, plunges into scientific questions with which he has no real acquaintance, only to obscure them by an aimless rhetoric, and distract the attention of his hearers from the real point at issue by eloquent digressions, and skilled appeals to religious prejudice." ‡

The letter above quoted continues:

"The excitement was now at its height; a lady fainted and had to be carried out, and it was some time before the discussion was resumed. Some voices called for Hooker, and his name having been handed up, the President invited him to give his view of the theory from the Botanical side. This he did, demonstrating that the Bishop, by his own showing, had never grasped the principles of the 'Origin,' and that he was absolutely ignorant of the elements of botanical science. The Bishop made no reply, and the meeting broke up.

"There was a crowded conversazione in the evening at the

* Lyell's 'Letters,' vol. ii. p. 335.

† Professor Victor Carus, who has a distinct recollection of the scene, does not remember the word *equivocal*. He believes, too, that Lyell's version of the ape sentence is slightly incorrect.

‡ Mr. Fawcett wrote ('Macmillan's Magazine,' 1860):

"The retort was so justly deserved and so inimitable in its manner, that no one who was present can ever forget the impression that it made."

© The Complete Work of Charles Darwin Online

rooms of the hospitable and genial Professor of Botany, Dr. Daubeny, where the almost sole topic was the battle of the 'Origin,' and I was much struck with the fair and unprejudiced way in which the black coats and white cravats of Oxford discussed the question, and the frankness with which they offered their congratulations to the winners in the combat."]

C. Darwin to J. D. Hooker.

Sudbrook Park, Monday night
[July 2nd, 1860].

MY DEAR HOOKER,—I have just received your letter. I have been very poorly, with almost continuous bad headache for forty-eight hours, and I was low enough, and thinking what a useless burthen I was to myself and all others, when your letter came, and it has so cheered me; your kindness and affection brought tears into my eyes. Talk of fame, honour, pleasure, wealth, all are dirt compared with affection; and this is a doctrine with which, I know, from your letter, that you will agree with from the bottom of your heart. . . . How I should have liked to have wandered about Oxford with you, if I had been well enough; and how still more I should have liked to have heard you triumphing over the Bishop. I am astonished at your success and audacity. It is something unintelligible to me how any one can argue in public like orators do. I had no idea you had this power. I have read lately so many hostile views, that I was beginning to think that perhaps I was wholly in the wrong, and that —— was right when he said the whole subject would be forgotten in ten years; but now that I hear that you and Huxley will fight publicly (which I am sure I never could do), I fully believe that our cause will, in the long-run, prevail. I am glad I was not in Oxford, for I should have been overwhelmed, with my [health] in its present state.

Y 2

© The Complete Work of Charles Darwin Online

C. Darwin to T. H. Huxley.

Sudbrook Park, Richmond,
July 3rd (1860).

.... I had a letter from Oxford, written by Hooker late on Sunday night, giving me some account of the awful battles which have raged about species at Oxford. He tells me you fought nobly with Owen (but I have heard no particulars), and that you answered the B. of O. capitally. I often think that my friends (and you far beyond others) have good cause to hate me, for having stirred up so much mud, and led them into so much odious trouble. If I had been a friend of myself, I should have hated me. (How to make that sentence good English, I know not.) But remember, if I had not stirred up the mud, some one else certainly soon would. I honour your pluck; I would as soon have died as tried to answer the Bishop in such an assembly. . . .

[On July 20th, my father wrote to Mr. Huxley:

"From all that I hear from several quarters, it seems that Oxford did the subject great good. It is of enormous importance, the showing the world that a few first-rate men are not afraid of expressing their opinion."]

C. Darwin to J. D. Hooker.

[July 1860.]

.... I have just read the 'Quarterly.'* It is uncommonly clever; it picks out with skill all the most conjectural

* 'Quarterly Review,' July 1860. The article in question was by Wilberforce, Bishop of Oxford, and was afterwards published in his "Essays Contributed to the 'Quarterly Review,' 1874." The passage from the 'Anti-Jacobin' gives the history of the evolution of space from the "primæval point or *punctum saliens* of the universe,"

© The Complete Work of Charles Darwin Online

parts, and brings forward well all the difficulties. It quizzes me quite splendidly by quoting the 'Anti-Jacobin' versus my Grandfather. You are not alluded to, nor, strange to say, Huxley; and I can plainly see, here and there, ——'s hand. The concluding pages will make Lyell shake in his shoes. By Jove, if he sticks to us, he will be a real hero. Good-night. Your well-quizzed, but not sorrowful, and affectionate friend. C. D.

I can see there has been some queer tampering with the Review, for a page has been cut out and reprinted.

---

which is conceived to have moved "forward in a right line, *ad infinitum*, till it grew tired; after which the right line, which it had generated, would begin to put itself in motion in a lateral direction, describing an area of infinite extent. This area, as soon as it became conscious of its own existence, would begin to ascend or descend according as its specific gravity would determine it, forming an immense solid space filled with vacuum, and capable of containing the present universe."

The following (p. 263) may serve as an example of the passages in which the reviewer refers to Sir Charles Lyell:—"That Mr. Darwin should have wandered from this broad highway of nature's works into the jungle of fanciful assumption is no small evil. We trust that he is mistaken in believing that he may count Sir C. Lyell as one of his converts. We know, indeed, the strength of the temptations which he can bring to bear upon his geological brother. . . . Yet no man has been more distinct and more logical in the denial of the transmutation of species than Sir C. Lyell, and that not in the infancy of his scientific life, but in its full vigour and maturity." The Bishop goes on to appeal to Lyell, in order that with his help "this flimsy speculation may be as completely put down as was what in spite of all denials we must venture to call its twin though less instructed brother, the 'Vestiges of Creation.'"

With reference to this article, Mr. Brodie Innes, my father's old friend and neighbour, writes:—"Most men would have been annoyed by an article written with the Bishop's accustomed vigour, a mixture of argument and ridicule. Mr. Darwin was writing on some parish matter, and put a postscript—'If you have not seen the last 'Quarterly,' do get it; the Bishop of Oxford has made such capital fun of me and my grandfather. By a curious coincidence, when I received the letter, I was staying in the same house with the Bishop, and showed it to him. He said, 'I am very glad he takes it in that way, he is such a capital fellow.'"

© The Complete Work of Charles Darwin Online

[Writing on July 22 to Dr. Asa Gray my father thus refers to Lyell's position :—

"Considering his age, his former views and position in society, I think his conduct has been heroic on this subject."]

C. Darwin to Asa Gray.

[Hartfield, Sussex] July 22nd [1860].

MY DEAR GRAY,—Owing to absence from home at water-cure and then having to move my sick girl to whence I am now writing, I have only lately read the discussion in Proc. American Acad.,* and now I cannot resist expressing my sincere admiration of your most clear powers of reasoning. As Hooker lately said in a note to me, you are more than *any one* else the thorough master of the subject. I declare that you know my book as well as I do myself; and bring to the question new lines of illustration and argument in a manner which excites my astonishment and almost my envy! I admire these discussions, I think, almost more than your article in Silliman's Journal. Every single word seems weighed carefully, and tells like a 32-pound shot. It makes me much wish (but I know that you have not time) that you could write more in detail, and give, for instance, the facts on the variability of the American wild fruits. The *Athenæum* has the largest circulation, and I have sent my copy to the editor with a request that he would republish the first discussion; I much fear he will not, as he reviewed the subject in so hostile a spirit . . . I shall be curious (and will order) the August number, as soon as I know that it contains your review of Reviews. My conclusion is that you have made a mistake in being a botanist, you ought to have been a lawyer.

* April 10, 1860. Dr. Gray criticised in detail "several of the positions taken at the preceding meeting by Mr. [J. A.] Lowell, Prof. Bowen and Prof. Agassiz." It was reprinted in the *Athenæum*, Aug. 4, 1860.

© The Complete Work of Charles Darwin Online

. . . . Henslow * and Daubeny are shaken. I hear from Hooker that he hears from Hochstetter that my views are making very considerable progress in Germany, and the good workers are discussing the question. Bronn at the end of his translation has a chapter of criticism, but it is such difficult German that I have not yet read it. Hopkins's review in 'Fraser' is thought the best which has appeared against us. I believe that Hopkins is so much opposed because his course of study has never led him to reflect much on such subjects as geographical distribution, classification, homologies, &c., so that he does not feel it a relief to have some kind of explanation.

C. Darwin to C. Lyell.

Hartfield [Sussex], July 30th [1860].

. . . . I had lots of pleasant letters about the Brit. Assoc., and our side seems to have got on very well. There has been as much discussion on the other side of the Atlantic as on this. No one I think understands the whole case better than Asa Gray, and he has been fighting nobly. He is a capital reasoner. I have sent one of his printed discussions to our *Athenæum*, and the editor says he will print it. The 'Quarterly' has been out some time. It contains no malice, which is wonderful. . . . It makes me say many things which

* Professor Henslow was mentioned in the December number of 'Macmillan's Magazine' as being an adherent of Evolution. In consequence of this he published, in the February number of the following year, a letter defining his position. This he did by means of an extract from a letter addressed to him by the Rev. L. Jenyns (Blomefield) which "very nearly," as he says, expressed his views. Mr. Blomefield wrote, "I was not aware that you had become a convert to his (Darwin's) theory, and can hardly suppose you have accepted it as a whole, though, like myself, you may go to the length of imagining that many of the smaller groups, both of animals and plants, may at some remote period have had a common parentage. I do not with some say that the whole of his theory cannot be true—but that it is very far from proved; and I doubt its ever being possible to prove it."

© The Complete Work of Charles Darwin Online

I do not say. At the end it quotes all your conclusions against Lamarck, and makes a solemn appeal to you to keep firm in the true faith. I fancy it will make you quake a little. —— has ingeniously primed the Bishop (with Murchison) against you as head of the uniformitarians. The only other review worth mentioning, which I can think of, is in the third No. of the 'London Review,' by some geologist, and favourable for a wonder. It is very ably done, and I should like much to know who is the author. I shall be very curious to hear on your return whether Bronn's German translation of the 'Origin' has drawn any attention to the subject. Huxley is eager about a 'Natural History Review,' which he and others are going to edit, and he has got so many first-rate assistants, that I really believe he will make it a first-rate production. I have been doing nothing, except a little botanical work as amusement. I shall hereafter be very anxious to hear how your tour has answered. I expect your book on the geological history of Man will, with a vengeance, be a bomb-shell. I hope it will not be very long delayed. Our kindest remembrances to Lady Lyell. This is not worth sending, but I have nothing better to say.

Yours affectionately,

C. DARWIN.

C. Darwin to F. Watkins. *

Down, July 30th, [1860].

MY DEAR WATKINS,—Your note gave me real pleasure. Leading the retired life which I do, with bad health, I oftener think of old times than most men probably do; and your face now rises before me, with the pleasant old expression, as vividly as if I saw you.

My book has been well abused, praised, and splendidly quizzed by the Bishop of Oxford; but from what I see of its

* See Vol. I. p. 168.

© The Complete Work of Charles Darwin Online

influence on really good workers in science, I feel confident that, *in the main*, I am on the right road. With respect to your question, I think the arguments are valid, showing that all animals have descended from four or five primordial forms; and that analogy and weak reasons go to show that all have descended from some single prototype.

Farewell, my old friend. I look back to old Cambridge days with unalloyed pleasure.

Believe me, yours most sincerely,

CHARLES DARWIN.

T. H. Huxley to C. Darwin.

August 6th, 1860.

MY DEAR DARWIN,—I have to announce a new and great ally for you. . . . .

Von Bär writes to me thus:—"Et outre cela, je trouve que vous écrivez encore des rédactions. Vous avez écrit sur l'ouvrage de M. Darwin une critique dont je n'ai trouvé que des débris dans un journal allemand. J'ai oublié le nom terrible du journal anglais dans lequel se trouve votre récension. En tout cas aussi je ne peux pas trouver le journal ici. Comme je m'intéresse beaucoup pour les idées de M. Darwin, sur lesquelles j'ai parlé publiquement et sur lesquelles je ferai peut-être imprimer quelque chose—vous m'obligeriez infiniment si vous pourriez me faire parvenir ce que vous avez écrit sur ces idées.

"J'ai énoncé les mêmes idées sur la transformation des types ou origine d'espèces que M. Darwin.* Mais c'est seulement sur la géographie zoologique que je m'appuie. Vous trouverez, dans le dernier chapitre du traité 'Ueber Papuas und Alfuren,' que j'en parle très décidément sans savoir que M. Darwin s'occupait de cet objet."

The treatise to which Von Bär refers he gave me when over

* See footnote, Vol. II. p. 186.

© The Complete Work of Charles Darwin Online

here, but I have not been able to lay hands on it since this letter reached me two days ago. When I find it I will let you know what there is in it.

Ever yours faithfully,
T. H. HUXLEY.

C. Darwin to T. H. Huxley.

Down, August 8 [1860].

MY DEAR HUXLEY—Your note contained magnificent news, and thank you heartily for sending it me. Von Baer weighs down with a vengeance all the virulence of [the 'Edinburgh' reviewer] and weak arguments of Agassiz. If you write to Von Baer, for heaven's sake tell him that we should think one nod of approbation on our side, of the greatest value; and if he does write anything, beg him to send us a copy, for I would try and get it translated and published in the *Athenæum* and in 'Silliman' to touch up Agassiz. . . . . Have you seen Agassiz's weak metaphysical and theological attack on the 'Origin' in the last 'Silliman'?* I would send it you, but apprehend it would be less trouble for you to look at it in London than return it to me. R. Wagner has sent me a German pamphlet, † giving an abstract of Agassiz's 'Essay on Classification,' "mit Rücksicht auf Darwins Ansichten," &c. &c. He won't go very "dangerous lengths," but thinks the truth lies half-way between Agassiz and the 'Origin.' As he goes thus far he will, nolens

* The 'American Journal of Science and Arts' (commonly called 'Silliman's Journal'), July 1860. Printed from advanced sheets of vol. iii. of 'Contributions to the Nat. Hist. of the U. S.' My father's copy has a pencilled "Truly" opposite the following passage:—"Unless Darwin and his followers succeed in showing that the struggle for life tends to something beyond favouring the existence of certain individuals over that of other individuals, they will soon find that they are following a shadow."

† 'Louis Agassiz's Prinzipien der Classification, &c., mit Rücksicht auf Darwins Ansichten. Separat-Abdruck aus den Göttingischen gelehrten Anzeigen,' 1860.

© The Complete Work of Charles Darwin Online

volens, have to go further. He says he is going to review me in [his] yearly Report. My good and kind agent for the propagation of the Gospel—*i. e.* the devil's gospel.

Ever yours,
C. DARWIN.

C. Darwin to C. Lyell.

Down, August 11th [1860].

. . . I have laughed at Woodward thinking that you were a man who could be influenced in your judgment by the voice of the public; and yet after mortally sneering at him, I was obliged to confess to myself, that I had had fears, what the effect might be of so many heavy guns fired by great men. As I have (sent by Murray) a spare 'Quarterly Review,' I send it by this post, as it may amuse you. The Anti-Jacobin part amused me. It is full of errors, and Hooker is thinking of answering it. There has been a cancelled page; I should like to know what gigantic blunder it contained. Hooker says that —— has played on the Bishop, and made him strike whatever note he liked; he has wished to make the article as disagreeable to you as possible. I will send the *Athenæum* in a day or two.

As you wish to hear what reviews have appeared, I may mention that Agassiz has fired off a shot in the last 'Silliman,' not good at all, denies variations and rests on the perfection of Geological evidence. Asa Gray tells me that a very clever friend has been almost converted to our side by this review of Agassiz's . . . Professor Parsons * has published in the same 'Silliman' a speculative paper correcting my notions, worth nothing. In the 'Highland Agricultural Journal' there is a review by some Entomologist, not worth much. This is all that I can remember. . . . As Huxley says, the platoon firing must soon cease. Hooker and

* Theophilus Parsons, Professor of Law in Harvard University.

© The Complete Work of Charles Darwin Online

Huxley, and Asa Gray, I see, are determined to stick to the battle and not give in; I am fully convinced that whenever you publish, it will produce a great effect on all *trimmers*, and on many others. By the way I forgot to mention Daubeny's pamphlet,* very liberal and candid, but scientifically weak. I believe Hooker is going nowhere this summer; he is excessively busy . . . He has written me many, most nice letters. I shall be very curious to hear on your return some account of your Geological doings. Talking of Geology, you used to be interested about the "pipes" in the chalk. About three years ago a perfectly circular hole suddenly appeared in a flat grass field to everyone's astonishment, and was filled up with many waggon loads of earth; and now two or three days ago, again it has circularly subsided about two feet more. How clearly this shows what is still slowly going on. This morning I recommenced work, and am at dogs; when I have written my short discussion on them, I will have it copied, and if you like, you can then see how the argument stands, about their multiple origin. As you seemed to think this important, it might be worth your reading; though I do not feel sure that you will come to the same probable conclusion that I have done. By the way, the Bishop makes a very telling case against me, by accumulating several instances where I speak very doubtfully; but this is very unfair, as in such cases as this of the dog, the evidence is and must be very doubtful. . . .

C. Darwin to Asa Gray.

Down, August 11 [1860].

MY DEAR GRAY,—On my return home from Sussex about a week ago, I found several articles sent by you. The first

* 'Remarks on the final causes of the sexuality of plants with particular reference to Mr. Darwin's work on the "Origin of Species."'—Brit. Assoc. Report, 1860.

© The Complete Work of Charles Darwin Online

article, from the 'Atlantic Monthly,' I am very glad to possess. By the way, the editor of the *Athenæum** has inserted your answer to Agassiz, Bowen, and Co., and when I therein read them, I admired them even more than at first. They really seemed to me admirable in their condensation, force, clearness and novelty.

I am surprised that Agassiz did not succeed in writing something better. How absurd that logical quibble—"if species do not exist, how can they vary?" As if any one doubted their temporary existence. How coolly he assumes that there is some clearly defined distinction between individual differences and varieties. It is no wonder that a man who calls identical forms, when found in two countries, distinct species, cannot find variation in nature. Again, how unreasonable to suppose that domestic varieties selected by man for his own fancy (p. 147) should resemble natural varieties or species. The whole article seems to me poor; it seems to me hardly worth a detailed answer (even if I could do it, and I much doubt whether I possess your skill in picking out salient points and driving a nail into them), and indeed you have already answered several points. Agassiz's name, no doubt, is a heavy weight against us. . . .

If you see Professor Parsons, will you thank him for the extremely liberal and fair spirit in which his Essay † is written. Please tell him that I reflected much on the chance of favourable monstrosities (*i.e.* great and sudden variation) arising. I have, of course, no objection to this, indeed it would be a great aid, but I did not allude to the subject, for, after much labour, I could find nothing which satisfied me of the probability of such occurrences. There seems to me in almost every case too much, too complex, and too beautiful adaptation, in every structure to believe in its sudden production. I have alluded under the head of beautifully hooked seeds to such possibility. Monsters are apt to be sterile, or *not* to transmit

* Aug. 4, 1860. † 'Silliman's Journal,' July 1860.

© The Complete Work of Charles Darwin Online

monstrous peculiarities. Look at the fineness of gradation in the shells of successive *sub-stages* of the same great formation; I could give many other considerations which made me doubt such view. It holds, to a certain extent, with domestic productions no doubt, where man preserves some abrupt change in structure. It amused me to see Sir R. Murchison quoted as a judge of affinities of animals, and it gave me a cold shudder to hear of any one speculating about a true crustacean giving birth to a true fish! *

Yours most truly,

C. DARWIN.

C. Darwin to C. Lyell.

Down, September 1st [1860].

MY DEAR LYELL,—I have been much interested by your letter of the 28th, received this morning. It has *delighted* me, because it demonstrates that you have thought a good deal lately on Natural Selection. Few things have surprised me more than the entire paucity of objections and difficulties new to me in the published reviews. Your remarks are of a different stamp and new to me. I will run through them, and make a few pleadings such as occur to me.

I put in the possibility of the Galapagos having been *continuously* joined to America, out of mere subservience to the many who believe in Forbes's doctrine, and did not see the danger of admission, about small mammals surviving there in such case. The case of the Galapagos, from certain facts on littoral sea-shells (viz. Pacific Ocean and South American littoral species), in fact convinced me more than in any other case of other islands, that the Galapagos had never been

* Parsons, *loc. cit.* p. 5, speaking of Pterichthys and Cephalaspis, says:—"Now is it too much to infer from these facts that either of these animals, if a crustacean, was so nearly a fish that some of its ova may have become fish; or, if itself a fish, was so nearly a crustacean that it may have been born from the ovum of a crustacean?"

© The Complete Work of Charles Darwin Online

continuously united with the mainland; it was mere base subservience, and terror of Hooker and Co.

With respect to atolls, I think mammals would hardly survive *very long*, even if the main islands (for as I have said in the Coral Book, the outline of groups of atolls do not look like a former *continent*) had been tenanted by mammals, from the extremely small area, the very peculiar conditions, and the probability that during subsidence all or nearly all atolls have been breached and flooded by the sea many times during their existence as atolls.

I cannot conceive any existing reptile being converted into a mammal. From homologies I should look at it as certain that all mammals had descended from some single progenitor. What its nature was, it is impossible to speculate. More like, probably, the Ornithorhynchus or Echidna than any known form; as these animals combine reptilian characters (and in a lesser degree bird character) with mammalian. We must imagine some form as intermediate, as is Lepidosiren now, between reptiles and fish, between mammals and birds on the one hand (for they retain longer the same embryological character) and reptiles on the other hand. With respect to a mammal not being developed on any island, besides want of time for so prodigious a development, there must have arrived on the island the necessary and peculiar progenitor, having a character like the embryo of a mammal; and not an *already developed* reptile, bird or fish.

We might give to a bird the habits of a mammal, but inheritance would retain almost for eternity some of the bird-like structure, and prevent a new creature ranking as a true mammal.

I have often speculated on antiquity of islands, but not with your precision, or at all under the point of view of Natural Selection *not* having done what might have been anticipated. The argument of littoral Miocene shells at the Canary Islands is new to me. I was deeply impressed (from

© The Complete Work of Charles Darwin Online

the amount of the denudation) [with the] antiquity of St. Helena, and its age agrees with the peculiarity of the flora. With respect to bats at New Zealand (N.B. There are two or three European bats in Madeira, and I think in the Canary Islands) not having given rise to a group of non-volant bats, it is, now you put the case, surprising; more especially as the genus of bats in New Zealand is very peculiar, and therefore has probably been long introduced, and they now speak of Cretacean fossils there. But the first necessary step has to be shown, namely, of a bat taking to feed on the ground, or anyhow, and anywhere, except in the air. I am bound to confess I do know one single such fact, viz. of an Indian species killing frogs. Observe, that in my wretched Polar Bear case, I do show the first step by which conversion into a whale "would be easy," "would offer no difficulty"!! So with seals, I know of no fact showing any the least incipient variation of seals feeding on the shore. Moreover, seals wander much; I searched in vain, and could not find *one* case of any species of seal confined to any islands. And hence wanderers would be apt to cross with individuals undergoing any change on an island, as in the case of land birds of Madeira and Bermuda. The same remark applies even to bats, as they frequently come to Bermuda from the mainland, though about 600 miles distant. With respect to the Amblyrhynchus of the Galapagos, one may infer as probable, from marine habits being so rare with Saurians, and from the terrestrial species being confined to a few central islets, that its progenitor first arrived at the Galapagos; from what country it is impossible to say, as its affinity I believe is not very clear to any known species. The offspring of the terrestrial species was probably rendered marine. Now in this case I do not pretend I can show variation in habits; but we have in the terrestrial species a vegetable feeder (in itself a rather unusual circumstance), largely on *lichens*, and it would not be a great change for its offspring to feed first on littoral algæ and then on sub-

© The Complete Work of Charles Darwin Online

marine algæ. I have said what I can in defence, but yours is a good line of attack. We should, however, always remember that no change will ever be effected till a variation in the habits or structure or of both *chance* to occur in the right direction, so as to give the organism in question an advantage over other already established occupants of land or water, and this may be in any particular case indefinitely long. I am very glad you will read my dogs MS., for it will be important to me to see what you think of the balance of evidence. After long pondering on a subject it is often hard to judge. With hearty thanks for your most interesting letter. Farewell.

My dear old master,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, September 2nd [1860].

MY DEAR HOOKER,—I am astounded at your news received this morning. I am become such an old fogy that I am amazed at your spirit. For God's sake do not go and get your throat cut. Bless my soul, I think you must be a little insane. I must confess it will be a most interesting tour; and, if you get to the top of Lebanon, I suppose extremely interesting—you ought to collect any beetles under stones there; but the Entomologists are such slow coaches. I dare say no result could be made out of them. [They] have never worked the Alpines of Britain.

If you come across any Brine lakes, do attend to their minute flora and fauna; I have often been surprised how little this has been attended to.

I have had a long letter from Lyell, who starts ingenious difficulties opposed to Natural Selection, because it has not done more than it has. This is very good, as it shows that he has thoroughly mastered the subject; and shows he is in

© The Complete Work of Charles Darwin Online

earnest. Very striking letter altogether and it rejoices the cockles of my heart.

. . . . How I shall miss you, my best and kindest of friends. God bless you.

Yours ever affectionately,
C. DARWIN.

C. Darwin to Asa Gray.

Down, Sept. 10 [1860].

. . . . You will be weary of my praise, but it * does strike me as quite admirably argued, and so well and pleasantly written. Your many metaphors are inimitably good. I said in a former letter that you were a lawyer, but I made a gross mistake, I am sure that you are a poet. No, by Jove, I will tell you what you are, a hybrid, a complex cross of lawyer, poet, naturalist and theologian! Was there ever such a monster seen before?

I have just looked through the passages which I have marked as appearing to me extra good, but I see that they are too numerous to specify, and this is no exaggeration. My eye just alights on the happy comparison of the colours of the prism and our artificial groups. I see one little error of fossil *cattle* in South America.

It is curious how each one, I suppose, weighs arguments in a different balance: embryology is to me by far the strongest single class of facts in favour of change of forms, and not one, I think, of my reviewers has alluded to this. Variation not coming on at a very early age, and being inherited at not a very early corresponding period, explains, as it seems to me, the grandest of all facts in natural history, or rather in zoology, viz. the resemblance of embryos.

[Dr. Gray wrote three articles in the 'Atlantic Monthly' for

* Dr. Gray in the 'Atlantic Monthly' for July, 1860.

© The Complete Work of Charles Darwin Online

July, August, and October, which were reprinted as a pamphlet in 1861, and now form chapter iii. in 'Darwiniana' (1876), with the heading 'Natural Selection not inconsistent with Natural Theology.']

C. Darwin to C. Lyell.

Down, September 12th [1860].

MY DEAR LYELL,—I never thought of showing your letter to any one. I mentioned in a letter to Hooker that I had been much interested by a letter of yours with original objections, founded chiefly on Natural Selection not having done so much as might have been expected. . . . . In your letter just received, you have improved your case versus Natural Selection; and it would tell with the public (do not be tempted by its novelty to make it too strong); yet it seems to me, not *really* very killing, though I cannot answer your case, especially, why Rodents have not become highly developed in Australia. You must assume that they have inhabited Australia for a very long period, and this may or may not be the case. But I feel that our ignorance is so profound, why one form is preserved with nearly the same structure, or advances in organisation or even retrogrades, or becomes extinct, that I cannot put very great weight on the difficulty. Then, as you say often in your letter, we know not how many geological ages it may have taken to make any great advance in organisation. Remember monkeys in the Eocene formations: but I admit that you have made out an excellent objection and difficulty, and I can give only unsatisfactory and quite vague answers, such as you have yourself put; however, you hardly put weight enough on the absolute necessity of variations first arising in the right direction, *videlicet*, of seals beginning to feed on the shore.

I entirely agree with what you say about only one species of many becoming modified. I remember this struck me

© The Complete Work of Charles Darwin Online

much when tabulating the varieties of plants, and I have a discussion somewhere on this point. It is absolutely implied in my ideas of classification and divergence that only one or two species, of even large genera, give birth to new species; and many whole genera become *wholly* extinct . . . . Please see p. 341 of the 'Origin.' But I cannot remember that I have stated in the 'Origin' the fact of only very few species in each genus varying. You have put the view much better in your letter. Instead of saying as I often have, that very few species vary at the same time, I ought to have said, that very few species of a genus *ever* vary so as to become modified; for this is the fundamental explanation of classification, and is shown in my engraved diagram. . . .

I quite agree with you on the strange and inexplicable fact of Ornithorhynchus having been preserved, and Australian Trigonia, or the Silurian Lingula. I always repeat to myself that we hardly know why any one single species is rare or common in the best-known countries. I have got a set of notes somewhere on the inhabitants of fresh water; and it is singular how many of these are ancient, or intermediate forms; which I think is explained by the competition having been less severe, and the rate of change of organic forms having been slower in small confined areas, such as all the fresh waters make compared with sea or land.

I see that you do allude in the last page, as a difficulty, to Marsupials not having become Placentals in Australia; but this I think you have no right at all to expect; for we ought to look at Marsupials and Placentals as having descended from some intermediate and lower form. The argument of Rodents not having become highly developed in Australia (supposing that they have long existed there) is much stronger. I grieve to see you hint at the creation "of distinct successive types, as well as of a certain number of distinct aboriginal types." Remember, if you admit this, you give up the embryological argument (*the weightiest of all to me*), and the

© The Complete Work of Charles Darwin Online

morphological or homological argument. You cut my throat, and your own throat; and I believe will live to be sorry for it. So much for species.

The striking extract which E. copied was your own writing!! in a note to me, many long years ago—which she copied and sent to Mme. Sismondi; and lately my aunt, in sorting her letters, found E.'s and returned them to her. . . . . I have been of late shamefully idle, *i.e.* observing * instead of writing, and how much better fun observing is than writing.

Yours affectionately,
C. DARWIN.

C. Darwin to C. Lyell.

15 Marine Parade, Eastbourne,
Sunday [September 23rd, 1860].

MY DEAR LYELL,—I got your letter of the 18th just before starting here. You speak of saving me trouble in answering Never think of this, for I look at every letter of yours as an honour and pleasure, which is a pretty deal more than I can say of some of the letters which I receive. I have now one of 13 *closely written folio pages* to answer on species! . . . .

I have a very decided opinion that all mammals must have descended from a *single* parent. Reflect on the multitude of details, very many of them of extremely little importance to their habits (as the number of bones of the head, &c., covering of hair, identical embryological development, &c. &c.). Now this large amount of similarity I must look at as certainly due to inheritance from a common stock. I am aware that some cases occur in which a similar or nearly similar organ has been acquired by independent acts of natural selection. But in most of such cases of these apparently so closely similar organs, some important homological difference may be detected. Please read p. 193, beginning, "The electric organs,"

* Drosera.

© The Complete Work of Charles Darwin Online

and trust me that the sentence, "In all these cases of two very distinct species," &c. &c., was not put in rashly, for I went carefully into every case. Apply this argument to the whole frame, internal and external, of mammifers, and you will see why I think so strongly that all have descended from one progenitor. I have just re-read your letter, and I am not perfectly sure that I understand your point.

I enclose two diagrams showing the sort of manner I *conjecture* that mammals have been developed. I thought a little on this when writing page 429, beginning, "Mr. Waterhouse." (Please read the paragraph.) I have not knowledge enough to choose between these two diagrams. If the brain of Marsupials in embryo closely resembles that of Placentals, I should strongly prefer No. 2, and this agrees with the antiquity of Microlestes. As a general rule I should prefer No. 1 diagram; whether or not Marsupials have gone on being developed, or rising in rank, from a very early period would depend on circumstances too complex for even a conjecture. Lingula has not risen since the Silurian epoch, whereas other molluscs may have risen.

A, in the following diagrams, represents an unknown form, probably intermediate between Mammals, Reptiles and Birds, as intermediate as Lepidosiren now is between Fish and Batrachians. This unknown form is probably more closely related to Ornithorhynchus than to any other known form.

I do not think that the multiple origin of dogs goes against the single origin of man. . . . . All the races of man are so infinitely closer together than to any ape, that (as in the case of descent of all mammals from one progenitor), I should look at all races of men as having certainly descended from one parent. I should look at it as probable that the races of men were less numerous and less divergent formerly than now, unless, indeed, some lower and more aberrant race even than the Hottentot has become extinct. Supposing, as I do for one believe, that our dogs have descended from two or three

© The Complete Work of Charles Darwin Online

wolves, jackals, &c.; yet these have, on *our view*, descended from a single remote unknown progenitor. With domestic dogs the question is simply whether the whole amount of difference has been produced since man domesticated a single species; or whether part of the difference arises in the state

DIAGRAM I.

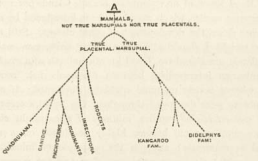
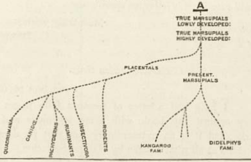


DIAGRAM II.



of nature. Agassiz and Co. think the negro and Caucasian are now distinct species, and it is a mere vain discussion whether, when they were rather less distinct, they would, on this standard of specific value, deserve to be called species.

© The Complete Work of Charles Darwin Online

I agree with your answer which you give to yourself on this point; and the simile of man now keeping down any new man which might be developed, strikes me as good and new. The white man is "improving off the face of the earth" even races nearly his equals. With respect to islands, I think I would trust to want of time alone, and not to bats and rodents.

N.B.—I know of no rodents on oceanic islands (except my Galapagos mouse, which *may* have been introduced by man) keeping down the development of other classes. Still *much* more weight I should attribute to there being now, neither in islands nor elsewhere, [any] known animals of a grade of organisation intermediate between mammals, fish, reptiles, &c., whence a new mammal could be developed. If every vertebrate were destroyed throughout the world, except our *now well-established* reptiles, millions of ages might elapse before reptiles could become highly developed on a scale equal to mammals; and, on the principle of inheritance, they would make some quite *new class*, and not mammals; though *possibly* more intellectual! I have not an idea that you will care for this letter, so speculative.

Most truly yours,
C. DARWIN.

C. Darwin to Asa Gray.

Down, Sept. 26 [1860].

. . . . I have had a letter of fourteen folio pages from Harvey against my book, with some ingenious and new remarks; but it is an extraordinary fact that he does not understand at all what I mean by Natural Selection. I have begged him to read the Dialogue in next 'Silliman,' as you never touch the subject without making it clearer. I look at it as even more extraordinary that you never say a word or use an epithet which does not express fully my meaning. Now Lyell, Hooker, and others, who perfectly understand my

© The Complete Work of Charles Darwin Online

book, yet sometimes use expressions to which I demur. Well, your extraordinary labour is over; if there is any fair amount of truth in my view, I am well assured that your great labour has not been thrown away. . . .

I yet hope and almost believe, that the time will come when you will go further, in believing a very large amount of modification of species, than you did at first or do now. Can you tell me whether you believe further or more firmly than you did at first? I should really like to know this. I can perceive in my immense correspondence with Lyell, who objected to much at first, that he has, perhaps unconsciously to himself, converted himself very much during the last six months, and I think this is the case even with Hooker. This fact gives me far more confidence than any other fact.

C. Darwin to C. Lyell.

15 Marine Parade, Eastbourne,
Friday evening [September 28th, 1860].

. . . . I am very glad to hear about the Germans reading my book. No one will be converted who has not independently begun to doubt about species. Is not Krohn * a good fellow? I have long meant to write to him. He has been working at Cirripedes, and has detected two or three gigantic blunders, ... about which, I thank Heaven, I spoke rather doubtfully. Such difficult dissection that even Huxley failed. It is chiefly the interpretation which I put on parts that is so wrong, and not the parts which I describe. But they were gigantic blunders, and why I say all this is because Krohn, instead of crowing at all, pointed out my errors with the utmost gentleness and pleasantness. I have always

* There are two papers by Aug. Krohn, one on the Cement Glands, and the other on the development of Cirripedes, 'Wiegmann's Archiv,' xxv. and xxvi. My father has remarked that he "blundered dreadfully about the cement glands," 'Autobiography,' p. 81.

© The Complete Work of Charles Darwin Online

meant to write to him and thank him. I suppose Dr. Krohn, Bonn, would reach him.

I cannot see yet how the multiple origin of dog can be properly brought as argument for the multiple origin of man. Is not your feeling a remnant of the deeply impressed one on all our minds, that a species is an entity, something quite distinct from a variety? Is it not that the dog case injures the argument from fertility, so that one main argument that the races of man are varieties and not species—*i.e.*, because they are fertile *inter se*, is much weakened?

I quite agree with what Hooker says, that whatever variation is possible under culture, is *possible* under nature; not that the same form would ever be accumulated and arrived at by selection for man's pleasure, and by natural selection for the organism's own good.

Talking of "natural selection;" if I had to commence *de novo*, I would have used "natural preservation." For I find men like Harvey of Dublin cannot understand me, though he has read the book twice. Dr. Gray of the British Museum remarked to me that, "*selection* was obviously impossible with plants! No one could tell him how it could be possible!" And he may now add that the author did not attempt it to him!

Yours ever affectionately,
C. DARWIN.

C. Darwin to C. Lyell.

15 Marine Parade, Eastbourne,
October 8th [1860].

MY DEAR LYELL,—I send the [English] translation of Bronn,* the first part of the chapter with generalities and praise is not translated. There are some good hits. He makes an apparently, and in part truly, telling case against me, says

* A MS. translation of Bronn's chapter of objections at the end of his German translation of the 'Origin of Species.'

© The Complete Work of Charles Darwin Online

that I cannot explain why one rat has a longer tail and another longer ears, &c. But he seems to muddle in assuming that these parts did not all vary together, or one part so insensibly before the other, as to be in fact contemporaneous. I might ask the creationist whether he thinks these differences in the two rats of any use, or as standing in some relation from laws of growth; and if he admits this, selection might come into play. He who thinks that God created animals unlike for mere sport or variety, as man fashions his clothes, will not admit any force in my *argumentum ad hominem.*

Bronn blunders about my supposing several Glacial periods, whether or no such ever did occur.

He blunders about my supposing that development goes on at the same rate in all parts of the world. I presume that he has misunderstood this from the supposed migration into all regions of the more dominant forms.

I have ordered Dr. Bree,* and will lend it to you, if you like, and if it turns out good.

. . . . . I am very glad that I misunderstood you about species not having the capacity to vary, though in fact few do give birth to new species. It seems that I am very apt to misunderstand you; I suppose I am always fancying objections. Your case of the Red Indian shows me that we agree entirely. . . . .

I had a letter yesterday from Thwaites of Ceylon, who was much opposed to me. He now says, "I find that the more familiar I become with your views in connection with the various phenomena of nature, the more they commend themselves to my mind."

* 'Species not Transmutable,' by C. R. Bree, 1860.

© The Complete Work of Charles Darwin Online

*C. Darwin to J. M. Rodwell.**

15 Marine Parade, Eastbourne.
November 5th [1860].

MY DEAR SIR,—I am extremely much obliged for your letter, which I can compare only to a plum-pudding, so full it is of good things. I have been rash about the cats: † yet I spoke on what seemed to me, good authority. The Rev. W. D. Fox gave me a list of cases of various foreign breeds in which he had observed the correlation, and for years he had vainly sought an exception. A French paper also gives numerous cases, and one very curious case of a kitten which *gradually* lost the blue colour in its eyes and as gradually acquired its power of hearing. I had not heard of your uncle, Mr. Kirby's case ‡ (whom I, for as long as I can remember, have venerated) of care in breeding cats. I do not know whether Mr. Kirby was your uncle by marriage, but your letters show me that you ought to have Kirby blood in your veins, and that if you had not taken to languages you would have been a first-rate naturalist.

I sincerely hope that you will be able to carry out your intention of writing on the "Birth, Life, and Death of Words." Anyhow, you have a capital title, and some think this the most difficult part of a book. I remember years ago at the Cape of Good Hope, Sir J. Herschell saying to me, I wish some one would treat language as Lyell has treated geology. What a linguist you must be to translate the Koran! Having a vilely bad head for languages, I feel an awful respect for linguists.

* Rev. J. M. Rodwell, who was at Cambridge with my father, remembers him saying :—"It strikes me that all our knowledge about the structure of our earth is very much like what an old hen would know of a hundred acre field, in a corner of which she is scratching."

† "Cats with blue eyes are invariably deaf," 'Origin of Species,' ed. i. p. 12.

‡ William Kirby, joint author with Spence, of the well-known 'Introduction to Entomology,' 1818.

© The Complete Work of Charles Darwin Online

I do not know whether my brother-in-law, Hensleigh Wedgwood's 'Etymological Dictionary' would be at all in your line; but he treats briefly on the genesis of words; and, as it seems to me, very ingeniously. You kindly say that you would communicate any facts which might occur to you, and I am sure that I should be most grateful. Of the multitude of letters which I receive, not one in a thousand is like yours in value.

With my cordial thanks, and apologies for this untidy letter written in haste, pray believe me, my dear Sir,

Yours sincerely obliged,

CH. DARWIN.

C. Darwin to C. Lyell.

November 20th [1860].

. . . . I have not had heart to read Phillips * yet, or a tremendous long hostile review by Professor Bowen in the 4to Mem. of the American Academy of Sciences.† (By the way, I hear Agassiz is going to thunder against me in the next part of the 'Contributions.') Thank you for telling me of the sale of the 'Origin,' of which I had not heard. There will be some time, I presume, a new edition, and I especially want your advice on one point, and you know I think you the wisest of men, and I shall be *absolutely guided by your advice.* It has occurred to me, that it would *perhaps* be a good plan to put a set of notes (some twenty to forty or fifty) to the 'Origin,' which now has none, exclusively devoted to errors of my reviewers. It has occurred to me that where a reviewer has erred, a common reader might err. Secondly, it will show the reader that he must not trust implicitly to reviewers. Thirdly, when any special fact has been attacked, I should like

* 'Life on the Earth.'

† "Remarks on the latest form of the Development Theory." By Francis Bowen, Professor of Natural Religion and Moral Philosophy, at Harvard University. 'American Academy of Arts and Sciences,' vol. viii.

© The Complete Work of Charles Darwin Online

to defend it. I would show no sort of anger. I enclose a mere rough specimen, done without any care or accuracy—done from memory alone—to be torn up, just to show the sort of thing that has occurred to me. *Will you do me the great kindness to consider this well?*

It seems to me it would have a good effect, and give some confidence to the reader. It would [be] a horrid bore going through all the reviews.

Yours affectionately,
C. DARWIN.

[Here follow samples of foot-notes, the references to volume and page being left blank. It will be seen that in some cases he seems to have forgotten that he was writing foot-notes, and to have continued as if writing to Lyell:—

* Dr. Bree (p.) asserts that I explain the structure of the cells of the Hive Bee by "the exploded doctrine of pressure." But I do not say one word which directly or indirectly can be interpreted into any reference to pressure.

* The 'Edinburgh' Reviewer (vol. , p.) quotes my work as saying that the "dorsal vertebræ of pigeons vary in number, and disputes the fact." I nowhere even allude to the dorsal vertebræ, only to the sacral and caudal vertebræ.

* The 'Edinburgh' Reviewer throws a doubt on these organs being the Branchiæ of Cirripedes. But Professor Owen in 1854 admits, without hesitation, that they are Branchiæ, as did John Hunter long ago.

* The confounded Wealden Calculation to be struck out, and a note to be inserted to the effect that I am convinced of its inaccuracy from a review in the *Saturday Review*, and from Phillips, as I see in his Table of Contents that he alludes to it.

* Mr. Hopkins ('Fraser,' vol. , p.) states—I am quoting only from vague memory—that, "I argue in favour of my views from the extreme imperfection of the Geological Record," and says this is the first time in the History of Science he has ever heard of ignorance being adduced as an argument. But I repeatedly admit, in the most emphatic language which I can use, that the imperfect evidence which Geology offers in regard to transitorial forms is most strongly opposed to my views. Surely there is a wide difference in fully admitting an objection, and then in endeavouring to show that it is not so strong as it at first appears, and in Mr. Hopkins's assertion that I found my argument on the Objection.

* I would also put a note to

© The Complete Work of Charles Darwin Online

"Natural Selection," and show how variously it has been misunderstood.

* A writer in the 'Edinburgh Philosophical Journal' denies my statement that the Woodpecker of La Plata never frequents trees. I observed its habits during two years, but, what is more to the purpose, Azara, whose accuracy all admit, is more emphatic than I am in regard to its never frequenting trees. Mr. A. Murray denies that it ought to be called a woodpecker; it has two toes in front and two behind, pointed tail feathers, a long pointed tongue, and the same general form of body, the same manner of flight, colouring and voice. It was classed, until recently, in the same genus—Picus—with all other woodpeckers, but now has been ranked as a distinct genus amongst the Picidæ. It differs from the typical Picus only in the beak, not being quite so strong, and in the upper mandible being slightly arched. I think these facts fully justify my statement that it is "in all essential parts of its organisation" a Woodpecker.]

C. Darwin to T. H. Huxley.

Down, Nov. 22 [1860].

MY DEAR HUXLEY,—For heaven's sake don't write an anti-Darwinian article; you would do it so confoundedly well. I have sometimes amused myself with thinking how I could best pitch into myself, and I believe I could give two or three good digs; but I will see you —— first, before I will try. I shall be very impatient to see the Review.* If it succeeds it may really do much, very much good. . . . .

I heard to-day from Murray that I must set to work at once on a new edition † of the 'Origin.' [Murray] says the Reviews have not improved the sale. I shall always think those early reviews, almost entirely yours, did the subject an *enormous* service. If you have any important suggestions or criticisms to make on any part of the 'Origin,' I should, of course, be very grateful for [them]. For I mean to correct as far as I can, but not enlarge. How you must be wearied with and hate the subject, and it is God's blessing if you do not get to hate me. Adios.

* The first number of the new series of the 'Nat. Hist. Review' appeared in 1861.

† The 3rd edition.

© The Complete Work of Charles Darwin Online

C. Darwin to C. Lyell.

Down, November 24th [1860].

MY DEAR LYELL,—I thank you much for your letter. I had got to take pleasure in thinking how I could best snub my reviewers; but I was determined, in any case, to follow your advice, and, before I had got to the end of your letter, I was convinced of the wisdom of your advice.* What an advantage it is to me to have such friends as you. I shall follow every hint in your letter exactly.

I have just heard from Murray; he says he sold 700 copies at his sale, and that he has not half the number to supply; so that I must begin at once.† . . . .

P.S.—I must tell you one little fact which has pleased me. You may remember that I adduce electrical organs of fish as one of the greatest difficulties which have occurred to me, and —— notices the passage in a singularly disingenuous spirit. Well, McDonnell, of Dublin (a first-rate man), writes to me that he felt the difficulty of the whole case as overwhelming against me. Not only are the fishes which have electric organs very remote in scale, but the organ is near the head in some, and near the tail in others, and supplied by wholly different nerves. It seems impossible that there could be any transition. Some friend, who is much opposed to me, seems to have crowed over McDonnell, who reports that he said to himself, that if Darwin is right, there must be homologous organs both near the head and tail in other non-electric fish.

* "I get on slowly with my new edition. I find that your advice was *excellent*. I can answer all reviews, without any direct notice of them, by a little enlargement here and there, with here and there a new paragraph. Bronn alone I shall treat with the respect of giving his objections with his name. I think I shall improve my book a good deal, and add only some twenty pages."—From a letter to Lyell, December 4th, 1860.

† On the third edition of the 'Origin of Species,' published in April 1861.

© The Complete Work of Charles Darwin Online

He set to work, and, by Jove, he has found them! * so that some of the difficulty is removed; and is it not satisfactory that my hypothetical notions should have led to pretty discoveries? McDonnell seems very cautious; he says, years must pass before he will venture to call himself a believer in my doctrine, but that on the subjects which he knows well, viz. Morphology and Embryology, my views accord well, and throw light on the whole subject.

C. Darwin to Asa Gray.

Down, November 26th, 1860.

MY DEAR GRAY,—I have to thank you for two letters. The latter with corrections, written before you received my letter asking for an American reprint, and saying that it was hopeless to print your reviews as a pamphlet, owing to the impossibility of getting pamphlets known. I am very glad to say that the August or second 'Atlantic' article has been reprinted in the 'Annals and Magazine of Natural History'; but I have not yet seen it there. Yesterday I read over with care the third article; and it seems to me, as before, *admirable.* But I grieve to say that I cannot honestly go as far as you do about Design. I am conscious that I am in an utterly hopeless muddle. I cannot think that the world, as we see it, is the result of chance; and yet I cannot look at each separate thing as the result of Design. To take a crucial example, you lead me to infer (p. 414) that you believe "that variation has been led along certain beneficial lines." I cannot believe this; and I think you would have to believe, that the tail of the Fantail was led to vary in the number and direction of its feathers in order to gratify the caprice of a few men. Yet if the Fantail had been a wild bird, and had

* 'On an organ in the Skate, which appears to be the homologue of the electrical organ of the Torpedo,' by R. McDonnell, 'Nat. Hist. Review,' 1861, p. 57.

© The Complete Work of Charles Darwin Online

used its abnormal tail for some special end, as to sail before the wind, unlike other birds, every one would have said, "What a beautiful and designed adaptation." Again, I say I am, and shall ever remain, in a hopeless muddle.

Thank you much for Bowen's 4to. review.* The coolness with which he makes all animals to be destitute of reason is simply absurd. It is monstrous at p. 103, that he should argue against the possibility of accumulative variation, and actually leave out, entirely, selection! The chance that an improved Short-horn, or improved Pouter-pigeon, should be produced by accumulative variation without man's selection, is as almost infinity to nothing; so with natural species without natural selection. How capitally in the 'Atlantic' you show that Geology and Astronomy are, according to Bowen, Metaphysics; but he leaves out this in the 4to Memoir.

I have not much to tell you about my Book. I have just heard that Du Bois-Reymond agrees with me. The sale of my book goes on well, and the multitude of reviews has not stopped the sale . . . ; so I must begin at once on a new corrected edition. I will send you a copy for the chance of your ever re-reading; but, good Heavens, how sick you must be of it!

C. Darwin to T. H. Huxley.

Down, Dec. 2nd [1860].

. . . . I have got fairly sick of hostile reviews. Nevertheless, they have been of use in showing me when to expatiate a little and to introduce a few new discussions. *Of course* I will send you a copy of the new edition

I entirely agree with you, that the difficulties on my notions are terrific, yet having seen what all the Reviews have said against me, I have far more confidence in the *general* truth of the doctrine than I formerly had. Another thing

* 'Memoirs of the American Academy of Arts and Sciences,' vol. viii.

© The Complete Work of Charles Darwin Online

gives me confidence, viz. that some who went half an inch with me now go further, and some who were bitterly opposed are now less bitterly opposed. And this makes me feel a little disappointed that you are not inclined to think the general view in some slight degree more probable than you did at first. This I consider rather ominous. Otherwise I should be more contented with your degree of belief. I can pretty plainly see that, if my view is ever to be generally adopted, it will be by young men growing up and replacing the old workers, and then young ones finding that they can group facts and search out new lines of investigation better on the notion of descent, than on that of creation. But forgive me for running on so egotistically. Living so solitary as I do, one gets to think in a silly manner of one's own work.

Ever yours very sincerely,

C. DARWIN.

C. Darwin to J. D. Hooker

Down, December 11th [1860].

. . . . . . I heard from A. Gray this morning; at my suggestion he is going to reprint the three 'Atlantic' articles as a pamphlet, and send 250 copies to England, for which I intend to pay half the cost of the whole edition, and shall give away, and try to sell by getting a few advertisements put in, and if possible notices in Periodicals.

. . . . . . David Forbes has been carefully working the Geology of Chile, and as I value praise for accurate observation far higher than for any other quality, forgive (if you can) the *insufferable* vanity of my copying the last sentence in his note: "I regard your Monograph on Chile as, without exception, one of the finest specimens of Geological enquiry." I feel inclined to strut like a Turkey-cock!

© The Complete Work of Charles Darwin Online

CHAPTER VIII.

THE SPREAD OF EVOLUTION.

1861–1862.

[THE beginning of the year 1861 saw my father with the third chapter of 'The Variation of Animals and Plants' still on his hands. It had been begun in the previous August, and was not finished until March 1861. He was, however, for part of this time (I believe during December 1860 and January 1861) engaged in a new edition (3000 copies) of the 'Origin,' which was largely corrected and added to, and was published in April 1861.

With regard to this, the third edition, he wrote to Mr. Murray in December 1860:—

"I shall be glad to hear when you have decided how many copies you will print off—the more the better for me in all ways, as far as compatible with safety; for I hope never again to make so many corrections, or rather additions, which I have made in hopes of making my many rather stupid reviewers at least understand what is meant. I hope and think I shall improve the book considerably."

An interesting feature in the new edition was the "Historical Sketch of the Recent Progress of Opinion on the Origin of Species" * which now appeared for the first time, and was continued in the later editions of the work. It bears a strong

* The Historical Sketch had already appeared in the first German edition (1860) and the American edition. Bronn states in the German edition (footnote, p. 1) that it was his critique in the 'N. Jahrbuch für Mineralogie' that suggested the idea of such a sketch to my father.

© The Complete Work of Charles Darwin Online

impress of the author's personal character in the obvious wish to do full justice to all his predecessors,—though even in this respect it has not escaped some adverse criticism.

Towards the end of the present year (1861), the final arrangements for the first French edition of the 'Origin' were completed, and in September a copy of the third English edition was despatched to Mdlle. Clémence Royer, who undertook the work of translation. The book was now spreading on the Continent, a Dutch edition had appeared, and, as we have seen, a German translation had been published in 1860. In a letter to Mr. Murray (September 10, 1861), he wrote, "My book seems exciting much attention in Germany, judging from the number of discussions sent me." The silence had been broken, and in a few years the voice of German science was to become one of the strongest of the advocates of evolution.

During all the early part of the year (1861) he was working at the mass of details which are marshalled in order in the early chapters of 'Animals and Plants.' Thus in his Diary occur the laconic entries, "May 16, Finished Fowls (eight weeks); May 31, Ducks."

On July 1, he started, with his family, for Torquay, where he remained until August 27—a holiday which he characteristically enters in his diary as "eight weeks and a day." The house he occupied was in Hesketh Crescent, a pleasantly placed row of houses close above the sea, somewhat removed from what was then the main body of the town, and not far from the beautiful cliffed coast-line in the neighbourhood of Anstey's Cove.

During the Torquay holiday, and for the remainder of the year, he worked at the fertilisation of orchids. This part of the year 1861 is not dealt with in the present chapter, because (as explained in the preface) the record of his life, as told in his letters, seems to become clearer when the whole of his botanical work is placed together and treated separately.

© The Complete Work of Charles Darwin Online

The present series of chapters will, therefore, include only the progress of his works in the direction of a general amplification of the 'Origin of Species'—e.g., the publication of 'Animals and Plants,' 'Descent of Man,' &c.]

C. Darwin to J. D. Hooker.

Down, Jan. 15 [1861].

MY DEAR HOOKER,—The sight of your handwriting always rejoices the very cockles of my heart. . . . .

I most fully agree to what you say about Huxley's Article,* and the power of writing. . . . . The whole review seems to me excellent. How capitally Oliver has done the résumé of botanical books. Good Heavens, how he must have read! . . . .

I quite agree that Phillips † is unreadably dull. You need not attempt Bree. ‡ . . . .

* 'Natural History Review,' 1861, p. 67, "On the Zoological Relations of Man with the Lower Animals." This memoir had its origin in a discussion at the previous meeting of the British Association, when Professor Huxley felt himself "compelled to give a diametrical contradiction to certain assertions respecting the differences which obtain between the brains of the higher apes and of man, which fell from Professor Owen." But in order that his criticisms might refer to deliberately recorded words, he bases them on Professor Owen's paper, "On the Characters, &c., of the Class Mammalia," read before the Linnean Society in February and April, 1857, in which he proposed to place man not only in a distinct order, but in "a distinct sub-class of the Mammalia"—the Archencephala.

† 'Life on the Earth' (1860), by Prof. Phillips, containing the substance of the Rede Lecture (May 1860).

‡ The following sentence (p. 16) from 'Species not Transmutable,' by Dr. Bree, illustrates the degree in which he understood the 'Origin of Species': "The only real difference between Mr. Darwin and his two predecessors" [Lamarck and the 'Vestiges'] "is this:—that while the latter have each given a mode by which they conceive the great changes they believe in have been brought about, Mr. Darwin does no such thing." After this we need not be surprised at a passage in the preface: "No one has derived greater pleasure than I have in past days from the study of Mr. Darwin's other works, and no one has felt a greater degree of regret that he should have imperilled his fame by the publication of his treatise upon the 'Origin of Species.'"

© The Complete Work of Charles Darwin Online

If you come across Dr. Freke on the 'Origin of Species by means of Organic Affinity,' read a page here and there. . . . He tells the reader to observe [that his result] has been arrived at by "induction," whereas all my results are arrived at only by "analogy." I see a Mr. Neale has read a paper before the Zoological Society on 'Typical Selection;' what it means I know not. I have not read H. Spencer, for I find that I must more and more husband the very little strength which I have. I sometimes suspect I shall soon entirely fail. . . . . As soon as this dreadful weather gets a little milder, I must try a little water cure. Have you read the 'Woman in White'? the plot is wonderfully interesting. I can recommend a book which has interested me greatly, viz. Olmsted's 'Journey in the Back Country.' It is an admirably lively picture of man and slavery in the Southern States. . . . .

C. Darwin to C. Lyell.

February 2, 1861.

MY DEAR LYELL,—I have thought you would like to read the enclosed passage in a letter from A. Gray (who is printing his reviews as a pamphlet,* and will send copies to England), as I think his account is really favourable in a high degree to us:—

"I wish I had time to write you an account of the lengths to which Bowen and Agassiz, each in their own way, are going. The first denying all heredity (all transmission except specific) whatever. The second coming near to deny that we are genetically descended from our great-great-grandfathers; and insisting that evidently affiliated languages, e.g. Latin, Greek, Sanscrit, owe none of their similarities to a community of origin, are all autochthonal; Agassiz admits that

* "Natural Selection not inconsistent with Natural Theology," from the 'Atlantic Monthly' for July, August, and October, 1860; published by Trübner.

ZOOLOGICAL DEPT.

© The Complete Work of Charles Darwin Online

the derivation of languages, and that of species or forms, stand on the same foundation, and that he must allow the latter if he allows the former, which I tell him is perfectly logical."

Is not this marvellous?

Ever yours,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, Feb. 4 [1861].

MY DEAR HOOKER,—I was delighted to get your long chatty letter, and to hear that you are thawing towards science. I almost wish you had remained frozen rather longer; but do not thaw too quickly and strongly. No one can work long as you used to do. Be idle; but I am a pretty man to preach, for I cannot be idle, much as I wish it, and am never comfortable except when at work. The word holiday is written in a dead language for me, and much I grieve at it. We thank you sincerely for your kind sympathy about poor H. [his daughter]. . . . . She has now come up to her old point, and can sometimes get up for an hour or two twice a day . . . . Never to look to the future or as little as possible is becoming our rule of life. What a different thing life was in youth with no dread in the future; all golden, if baseless, hopes.

. . . . With respect to the 'Natural History Review' I can hardly think that ladies would be so very sensitive about "lizards' guts;" but the publication is at present certainly a sort of hybrid, and original illustrated papers ought hardly to appear in a review. I doubt its ever paying; but I shall much regret if it dies. All that you say seems very sensible, but could a review in the strict sense of the word be filled with readable matter?

I have been doing little, except finishing the new edition

© The Complete Work of Charles Darwin Online

of the 'Origin,' and crawling on most slowly with my volume of 'Variation under Domestication.' . . . .

[The following letter refers to Mr. Bates's paper, "Contributions to an Insect Fauna of the Amazon Valley," in the 'Transactions of the Entomological Society.' vol. 5, N.S.* Mr. Bates points out that with the return, after the glacial period, of a warmer climate in the equatorial regions, the "species then living near the equator would retreat north and south to their former homes, leaving some of their congeners, slowly modified subsequently . . . to re-people the zone they had forsaken." In this case the species now living at the equator ought to show clear relationship to the species inhabiting the regions about the 25th parallel, whose distant relatives they would of course be. But this is not the case, and this is the difficulty my father refers to. Mr. Belt has offered an explanation in his 'Naturalist in Nicaragua' (1874), p. 266. "I believe the answer is that there was much extermination during the glacial period, that many species (and some genera, &c., as, for instance, the American horse), did not survive it . . . . but that a refuge was found for many species on lands now below the ocean, that were uncovered by the lowering of the sea, caused by the immense quantity of water that was locked up in frozen masses on the land."]

C. Darwin to J. D. Hooker.

Down, 27th [March 1861].

MY DEAR HOOKER,—I had intended to have sent you Bates's article this very day. I am so glad you like it. I have been extremely much struck with it. How well he argues, and with what crushing force against the glacial doctrine. I cannot wriggle out of it: I am dumbfounded; yet I do believe that some explanation some day will appear, and I

* The paper was read Nov. 24, 1860.

© The Complete Work of Charles Darwin Online

cannot give up equatorial cooling. It explains so much and harmonises with so much. When you write (and much interested I shall be in your letter) please say how far floras are generally uniform in generic character from 0° to 25° N. and S.

Before reading Bates, I had become thoroughly dissatisfied with what I wrote to you. I hope you may get Bates to write in the 'Linnean.'

Here is a good joke: H. C. Watson (who, I fancy and hope, is going to review the new edition * of the 'Origin') says that in the first four paragraphs of the introduction, the words "I," "me," "my," occur forty-three times! I was dimly conscious of the accursed fact. He says it can be explained phrenologically, which I suppose civilly means, that I am the most egotistically self-sufficient man alive; perhaps so. I wonder whether he will print this pleasing fact; it beats hollow the parentheses in Wollaston's writing.

I am, *my* dear Hooker, ever yours,

C. DARWIN.

P.S.—Do not spread this pleasing joke; it is rather too biting.

C. Darwin to J. D. Hooker.

Down, [April] 23? [1861.]

. . . . I quite agree with what you say on Lieutenant Hutton's Review † (who he is I know not); it struck me as very original. He is one of the very few who see that the change of species cannot be directly proved, and that the doctrine must sink or swim according as it groups and explains phenomena. It is really curious how few judge it in this way, which is clearly the right way. I have been much

* Third edition of 2000 copies, published in April 1861.

† In the 'Geologist,' 1861, p. 132, by Lieutenant Frederick Wollaston Hutton, of the Staff College. The 'Geologist' was afterwards merged in the 'Geological Magazine.'

© The Complete Work of Charles Darwin Online

interested by Bentham's paper* in the N. H. R., but it would not, of course, from familiarity, strike you as it did me. I liked the whole; all the facts on the nature of close and varying species. Good Heavens! to think of the British botanists turning up their noses, and saying that he knows nothing of British plants! I was also pleased at his remarks on classification, because it showed me that I wrote truly on this subject in the 'Origin.' I saw Bentham at the Linnean Society, and had some talk with him and Lubbock, and Edgeworth, Wallich, and several others. I asked Bentham to give us his ideas of species; whether partially with us or dead against us, he would write *excellent* matter. He made no answer, but his manner made me think he might do so if urged; so do you attack him. Every one was speaking with affection and anxiety of Henslow.† I dined with Bell at the Linnean Club, and liked my dinner. . . . . Dining out is such a novelty to me that I enjoyed it. Bell has a real good heart. I liked Rolleston's paper, but I never read anything so obscure and not self-evident as his 'Canons.'‡ . . . . I called on R. Chambers, at his very nice house in St. John's Wood, and had a very pleasant half-hour's talk; he is really a capital fellow. He made one good remark and chuckled over it, that the laymen universally had treated the controversy on the 'Essays and Reviews' as a merely professional subject, and had not joined in it, but had left it to the clergy. I shall be anxious for your next letter about Henslow.§ Farewell, with sincere sympathy, my old friend,

C. DARWIN.

* "On the Species and Genera of Plants, &c.," 'Natural History Review,' 1861, p. 133.

† Prof. Henslow was in his last illness.

‡ George Rolleston, M.D., F.R.S., b. 1829, d. 1881. Linacre Professor of Anatomy and Physiology at Oxford. A man of much learning, who left but few published works, among which may be mentioned his handbook, 'Forms of Animal Life.' For the 'Canons,' see 'Nat. Hist. Review,' 1861, p. 206.

§ Sir Joseph Hooker was Prof. Henslow's son-in-law.

© The Complete Work of Charles Darwin Online

P.S.—We are very much obliged for the 'London Review.' We like reading much of it, and the science is incomparably better than in the *Athenæum*. You shall not go on very long sending it, as you will be ruined by pennies and trouble, but I am under a horrid spell to the *Athenæum* and the *Gardeners' Chronicle*, but I have taken them in for so many years, that I *cannot* give them up.

[The next letter refers to Lyell's visit to the Biddenham gravel-pits near Bedford in April 1861. The visit was made at the invitation of Mr. James Wyatt, who had recently discovered two stone implements "at the depth of thirteen feet from the surface of the soil," resting "immediately on solid beds of oolitic-limestone." * Here, says Sir C. Lyell, "I . . . . for the first time, saw evidence which satisfied me of the chronological relations of those three phenomena—the antique tools, the extinct mammalia, and the glacial formation."]

C. Darwin to C. Lyell.

Down, April 12 [1861].

MY DEAR LYELL,—I have been most deeply interested by your letter. You seem to have done the grandest work, and made the greatest step, of any one with respect to man.

It is an especial relief to hear that you think the French superficial deposits are deltoid and semi-marine; but two days ago I was saying to a friend, that the unknown manner of the accumulation of these deposits, seemed the great blot in all the work done. I could not stomach debacles or lacustrine beds. It is grand. I remember Falconer told me that he

* 'Antiquity of Man,' fourth edition, p. 214.

© The Complete Work of Charles Darwin Online

thought some of the remains in the Devonshire caverns were pre-glacial, and this, I presume, is now your conclusion for the older celts with hyena and hippopotamus. It is grand. What a fine long pedigree you have given the human race!

I am sure I never thought of parallel roads having been accumulated during subsidence. I think I see some difficulties on this view, though, at first reading your note, I jumped at the idea. But I will think over all I saw there. I am (stomacho volente) coming up to London on Tuesday to work on cocks and hens, and on Wednesday morning, about a quarter before ten, I will call on you (unless I hear to the contrary), for I long to see you. I congratulate you on your grand work.

Ever yours,
C. DARWIN.

P.S.—Tell Lady Lyell that I was unable to digest the funereal ceremonies of the ants, notwithstanding that Erasmus has often told me that I should find some day that they have their bishops. After a battle I have always seen the ants carry away the dead for food. Ants display the utmost economy, and always carry away a dead fellow-creature as food. But I have just forwarded two most extraordinary letters to Busk, from a backwoodsman in Texas, who has evidently watched ants carefully, and declares most positively that they plant and cultivate a kind of grass for store food, and plant other bushes for shelter! I do not know what to think, except that the old gentleman is not fibbing intentionally. I have left the responsibility with Busk whether or no to read the letters.*

* *I.e.* to read them before the Linnean Society.

© The Complete Work of Charles Darwin Online

*C. Darwin to Thomas Davidson.**

Down, April 26, 1861.

My dear Sir,—I hope that you will excuse me for venturing to make a suggestion to you which I am perfectly well aware it is a very remote chance that you would adopt. I do not know whether you have read my 'Origin of Species'; in that book I have made the remark, which I apprehend will be universally admitted, that *as a whole*, the fauna of any formation is intermediate in character between that of the formations above and below. But several really good judges have remarked to me how desirable it would be that this should be exemplified and worked out in some detail and with some single group of beings. Now every one will admit that no one in the world could do this better than you with Brachiopods. The result might turn out very unfavourable to the views which I hold; if so, so much the better for those who are opposed to me.† But I am inclined to suspect that on the whole it would be favourable to the notion of descent with modification; for about a year ago, Mr. Salter ‡ in the museum in Jermyn Street, glued on a board some

* Thomas Davidson, F.R.S., born in Edinburgh, May 17, 1817; died 1885. His researches were chiefly connected with the sciences of geology and palæontology, and were directed especially to the elucidation of the characters, classification, history, geological and geographical distribution of recent and fossil Brachiopoda. On this subject he brought out an important work, 'British Fossil Brachiopoda,' 5 vols. 4to. (Cooper, 'Men of the Time,' 1884.)

† "Mr. Davidson is not at all a full believer in great changes of species, which will make his work all the more valuable."—C. Darwin to R. Chambers (April 30, 1861).

‡ John William Salter; b. 1820, d. 1869. He entered the service of the Geological Survey in 1846, and ultimately became its Palæontologist, on the retirement of Edward Forbes, and gave up the office in 1863. He was associated with several well-known naturalists in their work—with Sedgwick, Murchison, Lyell, Ramsay, and Huxley. There are sixty entries under his name in the Royal Society Catalogue. The above facts are taken from an obituary notice of Mr. Salter in the 'Geological Magazine,' 1869.

© The Complete Work of Charles Darwin Online

Spirifers, &c., from three palæozoic stages, and arranged them in single and branching lines, with horizontal lines marking the formations (like the diagram in my book, if you know it), and the result seemed to me very striking, though I was too ignorant fully to appreciate the lines of affinities. I longed to have had these shells engraved, as arranged by Mr. Salter, and connected by dotted lines, and would have gladly paid the expense: but I could not persuade Mr. Salter to publish a little paper on the subject. I can hardly doubt that many curious points would occur to any one thoroughly instructed in the subject, who would consider a group of beings under this point of view of descent with modification. All those forms which have come down from an ancient period very slightly modified ought, I think, to be omitted, and those forms alone considered which have undergone considerable change at each successive epoch. My fear is whether brachiopods have changed enough. The absolute amount of difference of the forms in such groups at the opposite extremes of time ought to be considered, and how far the early forms are intermediate in character between those which appeared much later in time. The antiquity of a group is not really diminished, as some seem vaguely to think, because it has transmitted to the present day closely allied forms. Another point is how far the succession of each genus is unbroken, from the first time it appeared to its extinction, with due allowance made for formations poor in fossils. I cannot but think that an important essay (far more important than a hundred literary reviews) might be written by one like yourself, and without very great labour. I know it is highly probable that you may not have leisure, or not care for, or dislike the subject, but I trust to your kindness to forgive me for making this suggestion. If by any extraordinary good fortune you were inclined to take up this notion, I would ask you to read my Chapter X. on Geological Succession. And I should like in this case to be

© The Complete Work of Charles Darwin Online

permitted to send you a copy of the new edition, just published, in which I have added and corrected somewhat in Chapters IX. and X.

Pray excuse this long letter, and believe me,

My dear Sir, yours very faithfully,

C. DARWIN.

P.S.—I write so bad a hand that I have had this note copied.

C. Darwin to Thomas Davidson.

Down, April 30, 1861.

MY DEAR SIR,—I thank you warmly for your letter; I did not in the least know that you had attended to my work. I assure you that the attention which you have paid to it, considering your knowledge and the philosophical tone of your mind (for I well remember one remarkable letter you wrote to me, and have looked through your various publications), I consider one of the highest, perhaps the very highest, compliments which I have received. I live so solitary a life that I do not often hear what goes on, and I should much like to know in what work you have published some remarks on my book. I take a deep interest in the subject, and I hope not simply an egotistical interest; therefore you may believe how much your letter has gratified me; I am perfectly contented if any one will fairly consider the subject, whether or not he fully or only very slightly agrees with me. Pray do not think that I feel the least surprise at your demurring to a ready acceptance; in fact, I should not much respect anyone's judgment who did so: that is, if I may judge others from the long time which it has taken me to go round. Each stage of belief cost me years. The difficulties are, as you say, many and very great; but the more I reflect, the more they seem to me to be due to our underestimating our ignorance. I belong so much to old times that I find that I weigh

© The Complete Work of Charles Darwin Online

the difficulties from the imperfection of the geological record, heavier than some of the younger men. I find, to my astonishment and joy, that such good men as Ramsay, Jukes, Geikie, and one old worker, Lyell, do not think that I have in the least exaggerated the imperfection of the record.* If my views ever are proved true, our current geological views will have to be considerably modified. My greatest trouble is, not being able to weigh the direct effects of the long-continued action of changed conditions of life without any selection, with the action of selection on mere accidental (so to speak) variability. I oscillate much on this head, but generally return to my belief that the direct action of the conditions of life have not been great. At least this direct action can have played an extremely small part in producing all the numberless and beautiful adaptations in every living creature. With respect to a person's belief, what does rather surprise me is that any one (like Carpenter) should be willing *to go so very far* as to believe that all birds may have descended from one parent, and not go a little farther and include all the members of the same great division; for on such a scale of belief, all the facts in Morphology and in Embryology (the most important in my opinion of all subjects) become mere Divine mockeries. . . . . I cannot express how profoundly glad I am that some day you will publish your theoretical view on the modification and endurance of

* Professor Sedgwick treated this part of the 'Origin of Species' very differently, as might have been expected from his vehement objection to Evolution in general. In the article in the *Spectator* of March 24, 1860, already noticed, Sedgwick wrote: "We know the complicated organic phenomena of the Mesozoic (or Oolitic) period. It defies the transmutationist at every step. Oh! but the document, says Darwin, is a fragment; I will interpolate long periods to account for all the changes. I say, in reply, if you deny my conclusion, grounded on positive evidence, I toss back your conclusion, derived from negative evidence,—the inflated cushion on which you try to bolster up the defects of your hypothesis." [The punctuation of the imaginary dialogue is slightly altered from the original, which is obscure in one place.]

© The Complete Work of Charles Darwin Online

Brachiopodous species; I am sure it will be a most valuable contribution to knowledge.

Pray forgive this very egotistical letter, but you yourself are partly to blame for having pleased me so much. I have told Murray to send a copy of my new edition to you, and have written your name.

With cordial thanks, pray believe me, my dear Sir,

Yours very sincerely,

CH. DARWIN.

[In Mr. Davidson's Monograph on British Brachiopoda, published shortly afterwards by the Palæontographical Society, results such as my father anticipated were to some extent obtained. "No less than fifteen commonly received species are demonstrated by Mr. Davidson by the aid of a long series of transitional forms to appertain to . . . one type." *

In the autumn of 1860, and the early part of 1861, my father had a good deal of correspondence with Professor Asa Gray on a subject to which reference has already been made—the publication, in the form of a pamphlet, of Professor Gray's three articles in the July, August, and October numbers of the 'Atlantic Monthly,' 1860. The pamphlet was published by Messrs. Trübner, with reference to whom my father wrote, "Messrs. Trübner have been most liberal and kind, and say they shall make no charge for all their trouble. I have settled about a few advertisements, and they will gratuitously insert one in their own periodicals."

The reader will find these articles republished in Dr. Gray's 'Darwiniana,' p. 87, under the title "Natural Selection not inconsistent with Natural Theology." The pamphlet found many admirers among those most capable of judging of its merits, and my father believed that it was of much value in lessening opposition, and making converts to Evolution. His

* Lyell, 'Antiquity of Man,' first edition, p. 428.

© The Complete Work of Charles Darwin Online

high opinion of it is shown not only in his letters, but by the fact that he inserted a special notice of it in a most prominent place in the third edition of the 'Origin.' Lyell, among others, recognised its value as an antidote to the kind of criticism from which the cause of Evolution suffered. Thus my father wrote to Dr. Gray:—"Just to exemplify the use of your pamphlet, the Bishop of London was asking Lyell what he thought of the review in the 'Quarterly,' and Lyell answered, 'Read Asa Gray in the 'Atlantic.'" It comes out very clearly that in the case of such publications as Dr. Gray's, my father did not rejoice over the success of his special view of Evolution, viz. that modification is mainly due to Natural Selection; on the contrary, he felt strongly that the really important point was that the doctrine of Descent should be accepted. Thus he wrote to Professor Gray (May 11, 1863), with reference to Lyell's 'Antiquity of Man':—

"You speak of Lyell as a judge; now what I complain of is that he declines to be a judge . . . . I have sometimes almost wished that Lyell had pronounced against me. When I say 'me,' I only mean *change of species by descent.* That seems to me the turning-point. Personally, of course, I care much about Natural Selection; but that seems to me utterly unimportant, compared to the question of Creation *or* Modification."]

C. Darwin to Asa Gray.

Down, April 11 [1861].

My dear Gray,—I was very glad to get your photograph: I am expecting mine, which I will send off as soon as it comes. It is an ugly affair, and I fear the fault does not lie with the photographer. . . . . Since writing last, I have had several letters full of the highest commendation of your Essay; all agree that it is by far the best thing written, and I do not doubt it has done the 'Origin' much good. I have not yet heard how it has sold. You will have seen a review in the

2 B 2

© The Complete Work of Charles Darwin Online

Gardeners' Chronicle. Poor dear Henslow, to whom I owe much, is dying, and Hooker is with him. Many thanks for two sets of sheets of your Proceedings. I cannot understand what Agassiz is driving at. You once spoke, I think, of Professor Bowen as a very clever man. I should have thought him a singularly unobservant man from his writings. He never can have seen much of animals, or he would have seen the difference of old and wise dogs and young ones. His paper about hereditariness beats everything. Tell a breeder that he might pick out his worst *individual* animals and breed from them, and hope to win a prize, and he would think you . . . insane.

[Professor Henslow died on May 16, 1861, from a complication of bronchitis, congestion of the lungs, and enlargement of the heart. His strong constitution was slow in giving way, and he lingered for weeks in a painful condition of weakness, knowing that his end was near, and looking at death with fearless eyes. In Mr. Blomefield's (Jenyns) 'Memoir of Henslow' (1862) is a dignified and touching description of Prof. Sedgwick's farewell visit to his old friend. Sedgwick said afterwards that he had never seen "a human being whose soul was nearer heaven."

My father wrote to Sir J. D. Hooker on hearing of Henslow's death, "I fully believe a better man never walked this earth."

He gave his impressions of Henslow's character in Mr. Blomefield's 'Memoir.' In reference to these recollections he wrote to Sir J. D. Hooker (May 30, 1861):—

"This morning I wrote my recollections and impressions of character of poor dear Henslow about the year 1830. I liked the job, and so have written four or five pages, now being copied. I do not suppose you will use all, of course you can chop and change as much as you like. If more than a sentence is used, I should like to see a proof-page, as I never can write decently till I see it in print. Very likely some of my remarks may appear too trifling, but I thought it best to

© The Complete Work of Charles Darwin Online

give my thoughts as they arose, for you or Jenyns to use as you think fit.

"You will see that I have exceeded your request, but, as I said when I began, I took pleasure in writing my impression of his admirable character."]

C. Darwin to Asa Gray.

Down, June 5 [1861].

My dear Gray,—I have been rather extra busy, so have been slack in answering your note of May 6th. I hope you have received long ago the third edition of the 'Origin.' . . . . I have heard nothing from Trübner of the sale of your Essay, hence fear it has not been great; I wrote to say you could supply more. I sent a copy to Sir J. Herschel, and in his new edition of his 'Physical Geography' he has a note on the 'Origin of Species,' and agrees, to a certain limited extent, but puts in a caution on design—much like yours. . . . . I have been led to think more on this subject of late, and grieve to say that I come to differ more from you. It is not that designed variation makes, as it seems to me, my deity "Natural Selection" superfluous, but rather from studying, lately, domestic variation, and seeing what an enormous field of undesigned variability there is ready for natural selection to appropriate for any purpose useful to each creature.

I thank you much for sending me your review of Phillips.* I remember once telling you a lot of trades which you ought to have followed, but now I am convinced that you are a born reviewer. By Jove, how well and often you hit the nail on the head! You rank Phillips's book higher than I do, or than Lyell does, who thinks it fearfully retrograde. I amused myself by parodying Phillips's argument as applied to domestic variation; and you might thus prove that the duck or

* 'Life on the Earth,' 1860.

© The Complete Work of Charles Darwin Online

pigeon has not varied because the goose has not, though more anciently domesticated, and no good reason can be assigned why it has not produced many varieties. . . . .

I never knew the newspapers so profoundly interesting. North America does not do England justice; I have not seen or heard of a soul who is not with the North. Some few, and I am one of them, even wish to God, though at the loss of millions of lives, that the North would proclaim a crusade against slavery. In the long-run, a million horrid deaths would be amply repaid in the cause of humanity. What wonderful times we live in! Massachusetts seems to show noble enthusiasm. Great God! how I should like to see the greatest curse on earth—slavery—abolished!

Farewell. Hooker has been absorbed with poor dear revered Henslow's affairs. Farewell.

Ever yours,
C. DARWIN.

Hugh Falconer to C. Darwin.

31 Sackville St., W., June 23, 1861.

MY DEAR DARWIN.—I have been to Adelsberg cave and brought back with me a live *Proteus anguinus*, designed for you from the moment I got it; *i.e.* if you have got an aquarium and would care to have it. I only returned last night from the Continent, and hearing from your brother that you are about to go to Torquay, I lose no time in making you the offer. The poor dear animal is still alive—although it has had no appreciable means of sustenance for a month—and I am most anxious to get rid of the responsibility of starving it longer. In your hands it will thrive and have a fair chance of being developed without delay into some type of the Columbidæ—say a Pouter or a Tumbler.

My dear Darwin, I have been rambling through the north of Italy, and Germany lately. Everywhere have I heard your views and your admirable essay canvassed—the views of

© The Complete Work of Charles Darwin Online

course often dissented from, according to the special bias of the speaker—but the work, its honesty of purpose, grandeur of conception, felicity of illustration, and courageous exposition, always referred to in terms of the highest admiration. And among your warmest friends no one rejoiced more heartily in the just appreciation of Charles Darwin than did,

Yours very truly,

H. FALCONER.

C. Darwin to Hugh Falconer.

Down [June 24, 1861].

MY DEAR FALCONER.—I have just received your note, and by good luck a day earlier than properly, and I lose not a moment in answering you, and thanking you heartily for your offer of the valuable specimen; but I have no aquarium and shall soon start for Torquay, so that it would be a thousand pities that I should have it. Yet I should certainly much like to see it, but I fear it is impossible. Would not the Zoological Society be the best place? and then the interest which many would take in this extraordinary animal would repay you for your trouble.

Kind as you have been in taking this trouble and offering me this specimen, to tell the truth I value your note more than the specimen. I shall keep your note amongst a very few precious letters. Your kindness has quite touched me.

Yours affectionately and gratefully,

CH. DARWIN.

C. Darwin to J. D. Hooker.

2 Hesketh Crescent, Torquay,
July 13 [1861].

. . . I hope Harvey is better; I got his review * of me a day or two ago, from which I infer he must be convalescent;

* The 'Dublin Hospital Gazette,' May 15, 1861. The passage referred to is at p. 150.

© The Complete Work of Charles Darwin Online

it's very good and fair; but it is funny to see a man argue on the succession of animals from Noah's Deluge; as God did not then wholly destroy man, probably he did not wholly destroy the races of other animals at each geological period! I never expected to have a helping hand from the Old Testament. . . .

C. Darwin to C. Lyell.

2, Hesketh Crescent, Torquay,
July 20 [1861].

MY DEAR LYELL.—I sent you two or three days ago a duplicate of a good review of the 'Origin' by a Mr. Maw,* evidently a thoughtful man, as I thought you might like to have it, as you have so many. . . .

This is a quite charming place, and I have actually walked, I believe, good two miles out and back, which is a grand feat.

I saw Mr. Pengelly † the other day, and was pleased at his enthusiasm. I do not in the least know whether you are in London. Your illness must have lost you much time, but I hope you have nearly got your great job of the new edition finished. You must be very busy, if in London, so I will be generous, and on honour bright do not expect any answer to this dull little note. . . .

C. Darwin to Asa Gray.

Down, September 17 [1861?]

MY DEAR GRAY.—I thank you sincerely for your very long and interesting letter, political and scientific, of August 27th

* Mr. George Maw, of Benthall Hall. The review was published in the 'Zoologist,' July, 1861. On the back of my father's copy is written, "Must be consulted before new edit. of 'Origin'"—words which are wanting on many more pretentious notices, on which frequently occur my father's brief 0/–, or "nothing new."

† William Pengelly, the geologist, and well-known explorer of the Devonshire caves.

© The Complete Work of Charles Darwin Online

and 29th, and Sept. 2nd received this morning. I agree with much of what you say, and I hope to God we English are utterly wrong in doubting (1) whether the N. can conquer the S.; (2) whether the N. has many friends in the South, and (3) whether you noble men of Massachusetts are right in transferring your own good feelings to the men of Washington. Again I say I hope to God we are wrong in doubting on these points. It is number (3) which alone causes England not to be enthusiastic with you. What it may be in Lancashire I know not, but in S. England cotton has nothing whatever to do with our doubts. If abolition does follow with your victory, the whole world will look brighter in my eyes, and in many eyes. It would be a great gain even to stop the spread of slavery into the Territories; if that be possible without abolition, which I should have doubted. You ought not to wonder so much at England's coldness, when you recollect at the commencement of the war how many propositions were made to get things back to the old state with the old line of latitude. But enough of this, all I can say is that Massachusetts and the adjoining States have the full sympathy of every good man whom I see; and this sympathy would be extended to the whole Federal States, if we could be persuaded that your feelings were at all common to them. But enough of this. It is out of my line, though I read every word of news, and formerly well studied Olmsted. . . . .

Your question what would convince me of Design is a poser. If I saw an angel come down to teach us good, and I was convinced from others seeing him that I was not mad, I should believe in design. If I could be convinced thoroughly that life and mind was in an unknown way a function of other imponderable force, I should be convinced. If man was made of brass or iron and no way connected with any other organism which had ever lived, I should perhaps be convinced. But this is childish writing.

© The Complete Work of Charles Darwin Online

I have lately been corresponding with Lyell, who, I think, adopts your idea of the stream of variation having been led or designed. I have asked him (and he says he will hereafter reflect and answer me) whether he believes that the shape of my nose was designed. If he does I have nothing more to say. If not, seeing what Fanciers have done by selecting individual differences in the nasal bones of pigeons, I must think that it is illogical to suppose that the variations, which natural selection preserves for the good of any being, have been designed. But I know that I am in the same sort of muddle (as I have said before) as all the world seems to be in with respect to free will, yet with everything supposed to have been foreseen or pre-ordained.

Farewell, my dear Gray, with many thanks for your interesting letter.

Your unmerciful correspondent,

C. DARWIN.

C. Darwin to H. W. Bates.

Down, Dec. 3 [1861].

MY DEAR SIR.—I thank you for your extremely interesting letter, and valuable references, though God knows when I shall come again to this part of my subject. One cannot of course judge of style when one merely hears a paper,* but yours seemed to me very clear and good. Believe me that I estimate its value most highly. Under a general point of view, I am quite convinced (Hooker and Huxley took the same view some months ago) that a philosophic view of nature can solely be driven into naturalists by treating special subjects as you have done. Under a special point of view, I think you have solved one of the most perplexing problems which could be given to solve. I am glad to hear from Hooker

* On Mimetic Butterflies, read before the Linnean Soc., Nov. 21, 1861. For my father's opinion of it when published, see p. 391.

© The Complete Work of Charles Darwin Online

that the Linnean Society will give plates if you can get drawings. . . .

Do not complain of want of advice during your travels; I dare say part of your great originality of views may be due to the necessity of self-exertion of thought. I can understand that your reception at the British Museum would damp you; they are a very good set of men, but not the sort to appreciate your work. In fact I have long thought that *too much* systematic work [and] description somehow blunts the faculties. The general public appreciates a good dose of reasoning, or generalisation, with new and curious remarks on habits, final causes, &c. &c., far more than do the regular naturalists.

I am extremely glad to hear that you have begun your travels . . . I am very busy, but I shall be *truly* glad to render any aid which I can by reading your first chapter or two. I do not think I shall be able to correct style, for this reason, that after repeated trials I find I cannot correct my own style till I see the MS. in type. Some are born with a power of good writing, like Wallace; others like myself and Lyell have to labour very hard and slowly at every sentence. I find it a very good plan, when I cannot get a difficult discussion to please me, to fancy that some one comes into the room and asks me what I am doing; and then try at once and explain to the imaginary person what it is all about. I have done this for one paragraph to myself several times, and sometimes to Mrs. Darwin, till I see how the subject ought to go. It is, I think, good to read one's MS. aloud. But style to me is a great difficulty; yet some good judges think I have succeeded, and I say this to encourage you.

What *I think* I can do will be to tell you whether parts had better be shortened. It is good, I think, to dash "in medias res," and work in later any descriptions of country, or any historical details which may be necessary. Murray likes

© The Complete Work of Charles Darwin Online

lots of wood-cuts—give some by all means of ants. The public appreciate monkeys—our poor cousins. What sexual differences are there in monkeys? Have you kept them tame? if so, about their expression. I fear that you will hardly read my vile hand-writing, but I cannot without killing trouble write better.

You shall have my candid opinion on your MS., but remember it is hard to judge from MS., one reads slowly, and heavy parts seem much heavier. A first-rate judge thought my Journal very poor; now that it is in print, I happen to know, he likes it. I am sure you will understand why I am so egotistical.

I was a *little* disappointed in Wallace's book * on the Amazon; hardly facts enough. On other hand, in Gosse's book † there is not reasoning enough to my taste. Heaven knows whether you will care to read all this scribbling. . . .

I am glad you had a pleasant day with Hooker,‡ he is an admirably good man in every sense.

[The following extract from a letter to Mr. Bates on the same subject is interesting as giving an idea of the plan followed by my father in writing his 'Naturalist's Voyage:'

"As an old hackneyed author, let me give you a bit of advice, viz. to strike out every word which is not quite necessary to the current subject, and which could not interest a stranger. I constantly asked myself, Would a stranger care for this? and struck out or left in accordingly. I think too much pains cannot be taken in making the style transparently clear and throwing eloquence to the dogs."

Mr. Bates's book, 'The Naturalist in the Amazons,' was published in 1863, but the following letter may be given here rather than in its due chronological position:]

* 'Travels on the Amazon and Rio Negro,' 1853.

† Probably the 'Naturalist's Sojourn in Jamaica,' 1851.

‡ In a letter to Sir J. D. Hooker (Dec. 1861), my father wrote: "I am very glad to hear that you like Bates. I have seldom in my life been more struck with a man's power of mind."

© The Complete Work of Charles Darwin Online

C. Darwin to H. W. Bates.

Down, April 18, 1863.

DEAR BATES,—I have finished vol. i. My criticisms may be condensed into a single sentence, namely, that it is the best work of Natural History Travels ever published in England. Your style seems to me admirable. Nothing can be better than the discussion on the struggle for existence, and nothing better than the description of the Forest scenery.* It is a grand book, and whether or not it sells quickly, it will last. You have spoken out boldly on Species; and boldness on the subject seems to get rarer and rarer. How beautifully illustrated it is. The cut on the back is most tasteful. I heartily congratulate you on its publication.

The *Athenæum* † was rather cold, as it always is, and insolent in the highest degree about your leading facts. Have you seen the *Reader?* I can send it to you if you have not seen it. . . .

C. Darwin to Asa Gray.

Down, Dec. 11 [1861].

MY DEAR GRAY,—Many and cordial thanks for your two last most valuable notes. What a thing it is that when you receive this we may be at war, and we two be bound, as good patriots, to hate each other, though I shall find this hating you very hard work. How curious it is to see two countries, just like two angry and silly men, taking so opposite a view of the same transaction! I fear there is no shadow of doubt we shall fight, if the two Southern rogues are not given

* In a letter to Lyell my father wrote: "He [*i.e.* Mr. Bates] is second only to Humboldt in describing a tropical forest."

† "I have read the first volume of Bates's Book; it is capital, and I think the best Natural History Travels ever published in England. He is bold about Species, &c., and the *Athenæum* coolly says 'he bends his facts' for this purpose."—(From a letter to Sir J. D. Hooker.)

© The Complete Work of Charles Darwin Online

up.* And what a wretched thing it will be if we fight on the side of slavery. No doubt it will be said that we fight to get cotton; but I fully believe that this has not entered into the motive in the least. Well, thank Heaven, we private individuals have nothing to do with so awful a responsibility. Again, how curious it is that you seem to think that you can conquer the South; and I never meet a soul, even those who would most wish it, who thinks it possible—that is, to conquer and retain it. I do not suppose the mass of people in your country will believe it, but I feel sure if we do go to war it will be with the utmost reluctance by all classes, Ministers of Government and all. Time will show, and it is no use writing or thinking about it. I called the other day on Dr. Boott, and was pleased to find him pretty well and cheerful. I see, by the way, he takes quite an English opinion of American affairs, though an American in heart.† Buckle might write a chapter on opinion being entirely dependent on longitude!

. . . With respect to Design, I feel more inclined to show a white flag than to fire my usual long-range shot. I like to try and ask you a puzzling question, but when you return the compliment I have great doubts whether it is a fair way of arguing. If anything is designed, certainly man must be: one's "inner consciousness" (though a false guide) tells one so; yet I cannot admit that man's rudimentary mammæ . . . were designed. If I was to say I believed this, I should believe it in the same incredible manner as the orthodox believe the Trinity in Unity. You say that you are in a haze; I am in thick mud; the orthodox would say in fetid, abominable mud; yet I cannot keep out of the question. My dear Gray, I have written a deal of nonsense.

Yours most cordially,

C. DARWIN.

* The Confederate Commissioners Slidell and Mason were forcibly removed from the *Trent*, a West India mail steamer, on Nov. 8, 1861. The news that the U.S. agreed to release them reached England on Jan. 8, 1862.

† Dr. Boott was born in the U.S.

© The Complete Work of Charles Darwin Online

1862.

[Owing to the illness from scarlet fever of one of his boys, he took a house at Bournemouth in the autumn. He wrote to Dr. Gray from Southampton (Aug. 21, 1862):—

"We are a wretched family, and ought to be exterminated. We slept here to rest our poor boy on his journey to Bournemouth, and my poor dear wife sickened with scarlet fever, and has had it pretty sharply, but is recovering well. There is no end of trouble in this weary world. I shall not feel safe till we are all at home together, and when that will be I know not. But it is foolish complaining."

Dr. Gray used to send postage stamps to the scarlet fever patient; with regard to this good-natured deed my father wrote—

"I must just recur to stamps; my little man has calculated that he will now have 6 stamps which no other boy in the school has. Here is a triumph. Your last letter was plaistered with many coloured stamps, and he long surveyed the envelope in bed with much quiet satisfaction."

The greater number of the letters of 1862 deal with the Orchid work, but the wave of conversion to Evolution was still spreading, and reviews and letters bearing on the subject still came in numbers. As an example of the odd letters he received may be mentioned one which arrived in January of this year "from a German homœopathic doctor, an ardent admirer of the 'Origin.' Had himself published nearly the same sort of book, but goes much deeper. Explains the origin of plants and animals on the principles of homœopathy or by the law of spirality. Book fell dead in Germany. Therefore would I translate it and publish it in England."]

© The Complete Work of Charles Darwin Online

C. Darwin to T. H. Huxley.

Down, [Jan. ?] 14 [1862].

MY DEAR HUXLEY,—I am heartily glad of your success in the North,* and thank you for your note and slip. By Jove you have attacked Bigotry in its stronghold. I thought you would have been mobbed. I am so glad that you will publish your Lectures. You seem to have kept a due medium between extreme boldness and caution. I am heartily glad that all went off so well. I hope Mrs. Huxley is pretty well. . . . . I must say one word on the Hybrid question. No doubt you are right that here is a great hiatus in the argument; yet I think you overrate it—you never allude to the excellent evidence of *varieties* of Verbascum and Nicotiana being partially sterile together. It is curious to me to read (as I have to-day) the greatest crossing *Gardener* utterly pooh-poohing the distinction which *Botanists* make on this head, and insisting how frequently crossed *varieties* produce sterile offspring. Do oblige me by reading the latter half of my Primula paper in the 'Linn. Journal,' for it leads me to suspect that sterility will hereafter have to be largely viewed as an acquired or *selected* character—a view which I wish I had had facts to maintain in the 'Origin.'†. . . .

C. Darwin to J. D. Hooker.

Down, Jan. 25 [1862].

MY DEAR HOOKER,—Many thanks for your last Sunday's letter, which was one of the pleasantest I ever received in my life. We are all pretty well redivivus, and I am at work again. I thought it best to make a clean breast to Asa

* This refers to two of Mr. Huxley's lectures, given before the Philosophical Institution of Edinburgh in 1862. The substance of them is given in 'Man's Place in Nature.'

† The view here given will be discussed in the chapter on heterostyled plants.

© The Complete Work of Charles Darwin Online

Gray; and told him that the Boston dinner, &c. &c., had quite turned my stomach, that I almost thought it would be good for the peace of the world if the United States were split up; on the other hand, I said that I groaned to think of the slave-holders being triumphant, and that the difficulties of making a line of separation were fearful. I wonder what he will say. . . . . Your notion of the Aristocrat being kenspeckle, and the best men of a good lot being thus easily selected is new to me, and striking. The 'Origin' having made you in fact a jolly old Tory, made us all laugh heartily. I have sometimes speculated on this subject; primogeniture* is dreadfully opposed to selection; suppose the first-born bull was necessarily made by each farmer the begetter of his stock! On the other hand, as you say, ablest men are continually raised to the peerage, and get crossed with the older Lord-breeds, and the Lords continually select the most beautiful and charming women out of the lower ranks; so that a good deal of indirect selection improves the Lords. Certainly I agree with you the present American row has a very Torifying influence on us all. I am very glad to hear you are beginning to print the 'Genera;' it is a wonderful satisfaction to be thus brought to bed, indeed it is one's chief satisfaction, I think, though one knows that another bantling will soon be developing. . . .

C. Darwin to Maxwell Masters.†

Down, Feb. 26 [1862].

MY DEAR SIR,—I am much obliged to you for sending me

* My father had a strong feeling as to the injustice of primogeniture, and in a similar spirit was often indignant over the unfair wills that appear from time to time. He would declare energetically that if he were law-giver no will should be valid that was not published in the testator's lifetime; and this he maintained would prevent much of the monstrous injustice and meanness apparent in so many wills.

† Dr. Masters is a well-known vegetable teratologist, and has been for many years the editor of the *Gardeners' Chronicle*.

© The Complete Work of Charles Darwin Online

your article,* which I have just read with much interest. The History, and a good deal besides, was quite new to me. It seems to me capitally done, and so clearly written. You really ought to write your larger work. You speak too generously of my book; but I must confess that you have pleased me not a little; for no one, as far as I know, has ever remarked on what I say on classification,—a part, which when I wrote it, pleased me. With many thanks to you for sending me your article, pray believe me,

My dear Sir, yours sincerely,

C. DARWIN.

[In the spring of this year (1862) my father read the second volume of Buckle's 'History of Civilization.' The following strongly expressed opinion about it may be worth quoting:—

"Have you read Buckle's second volume? it has interested me greatly; I do not care whether his views are right or wrong, but I should think they contained much truth. There is a noble love of advancement and truth throughout; and to my taste he is the very best writer of the English language that ever lived, let the other be who he may."]

C. Darwin to Asa Gray.

Down, March 15 [1862].

MY DEAR GRAY,—Thanks for the newspapers (though they did contain digs at England), and for your note of Feb. 18th. It is really almost a pleasure to receive stabs from so smooth, polished and sharp a dagger as your pen. I heartily wish I could sympathise more fully with you, instead of merely hating the South. We cannot enter into your feelings; if Scotland were to rebel, I presume we should be very wrath, but I do not think we should care a penny what other nations

* A paper on "Vegetable Morphology," by Dr. Masters, in the 'British and Foreign Medico-Chirurgical Review' for 1862.

© The Complete Work of Charles Darwin Online

thought. The millennium must come before nations love each other; but try and do not hate me. Think of me, if you will as a poor blinded fool. I fear the dreadful state of affairs must dull your interest in Science. . . . .

I believe that your pamphlet has done my book *great* good; and I thank you from my heart for myself; and believing that the views are in large part true, I must think that you have done natural science a good turn. Natural Selection seems to be making a little progress in England and on the Continent; a new German edition is called for, and a French * one has just appeared. One of the best men, though at present unknown, who has taken up these views, is Mr. Bates; pray read his 'Travels in Amazonia,' when they appear; they will be very good, judging from MS. of the first two chapters.

. . . . Again I say, do not hate me.

Ever yours most truly,
C. DARWIN.

C. Darwin to C. Lyell.

1 Carlton Terrace, Southampton,†
Aug. 22 [1862].

. . . . I heartily hope that you ‡ will be out in October. . . . . You say that the Bishop and Owen will be down on you; the latter hardly can, for I was assured that Owen in his Lectures this spring advanced as a new idea that

* In June, 1862, my father wrote to Dr. Gray: "I received, 2 or 3 days ago, a French translation of the 'Origin,' by a Madlle. Royer, who must be one of the cleverest and oddest women in Europe: is an ardent Deist, and hates Christianity, and declares that natural selection and the struggle for life will explain all morality, nature of man, politics, &c. &c.! She makes some very curious and good hits, and says she shall publish a book on these subjects." Madlle. Royer added foot-notes to her translation, and in many places where the author expresses great doubt, she explains the difficulty, or points out that no real difficulty exists.

† The house of his son William.

‡ *I.e.* 'The Antiquity of Man.'

© The Complete Work of Charles Darwin Online

wingless birds had lost their wings by disuse, also that magpies stole spoons, &c., from a *remnant* of some instinct like that of the Bower-Bird, which ornaments its playing-passage with pretty feathers. Indeed, I am told that he hinted plainly that all birds are descended from one . . . .

Your P.S. touches on, as it seems to me, very difficult points. I am glad to see [that] in the 'Origin,' I only say that the naturalists generally consider that low organisms vary more than high; and this I think certainly is the general opinion. I put the statement this way to show that I considered it only an opinion probably true. I must own that I do not at all trust even Hooker's contrary opinion, as I feel pretty sure that he has not tabulated any result. I have some materials at home, I think I attempted to make this point out, but cannot remember the result.

Mere variability, though the necessary foundation of all modifications, I believe to be almost always present, enough to allow of any amount of selected change; so that it does not seem to me at all incompatible that a group which at any one period (or during all successive periods) varies less, should in the long course of time have undergone more modification than a group which is generally more variable.

Placental animals, e.g. might be at each period less variable than Marsupials, and nevertheless have undergone more *differentiation* and development than marsupials, owing to some advantage, probably brain development.

I am surprised, but do not pretend to form an opinion at Hooker's statement that higher species, genera, &c., are best limited. It seems to me a bold statement.

Looking to the 'Origin,' I see that I state that the productions of the land seem to change quicker than those of the sea (Chapter X., p. 339, 3rd edition), and I add there is some reason to believe that organisms considered high in the scale change quicker than those that are low. I remember writing these sentences after much deliberation. I

© The Complete Work of Charles Darwin Online

remember well feeling much hesitation about putting in even the guarded sentences which I did. My doubts, I remember, related to the rate of change of the Radiata in the Secondary formation, and of the Foraminifera in the oldest Tertiary beds. . . . .

Good night,

C. DARWIN.

C. Darwin to C. Lyell.

Down, Oct. 1 [1862].

. . . . I found here * a short and very kind note of Falconer, with some pages of his 'Elephant Memoir,' which will be published, in which he treats admirably on long persistence of type. I thought he was going to make a good and crushing attack on me, but, to my great satisfaction, he ends by pointing out a loophole, and adds,† "with him I have no faith that the mammoth and other extinct elephants made their appearance suddenly. . . . . The most rational view seems to be that they are the modified descendants of earlier progenitors, &c." This is capital. There will not be soon one good palæontologist who believes in immutability. Falconer does not allow for the Proboscidean group being a failing one, and therefore not likely to be giving off new races.

He adds that he does not think Natural Selection suffices. I do not quite see the force of his argument, and he apparently overlooks that I say over and over again that Natural Selection can do nothing without variability, and that variability is subject to the most complex fixed laws. . . . .

[In his letters to Sir J. D. Hooker, about the end of this

* On his return from Bournemouth.

† Falconer, "On the American Fossil Elephant," in the 'Nat. Hist. Review,' 1863, p. 81. The words preceding those cited by my father make the meaning of his quotation clearer. The passage begins as follows: "The inferences which I draw from these facts are not opposed to one of the leading propositions of Darwin's theory. With him," &c. &c.

© The Complete Work of Charles Darwin Online

year, are occasional notes on the progress of the 'Variation of Animals and Plants.' Thus on November 24th he wrote: "I hardly know why I am a little sorry, but my present work is leading me to believe rather more direct in the action of physical conditions. I presume I regret it, because it lessens the glory of Natural Selection, and is so confoundedly doubtful. Perhaps I shall change again when I get all my facts under one point of view, and a pretty hard job this will be."

Again, on December 22nd, "To-day I have begun to think of arranging my concluding chapters on Inheritance, Reversion, Selection, and such things, and am fairly paralysed how to begin and how to end, and what to do, with my huge piles of materials."]

C. Darwin to Asa Gray.

Down, Nov. 6 [1862].

MY DEAR GRAY,—When your note of October 4th and 13th (chiefly about Max Müller) arrived, I was nearly at the end of the same book,* and had intended recommending you to read it. I quite agree that it is extremely interesting, but the latter part about the *first* origin of language much the least satisfactory. It is a marvellous problem. . . . . [There are] covert sneers at me, which he seems to get the better of towards the close of the book. I cannot quite see how it will forward "my cause," as you call it; but I can see how any one with literary talent (I do not feel up to it) could make great use of the subject in illustration.† What pretty metaphors you would make from it! I wish some one would

* 'Lectures on the Science of Language,' 1st edit. 1861.

† Language was treated in the manner here indicated by Sir C. Lyell in the 'Antiquity of Man.' Also by Prof. Schleicher, whose pamphlet was fully noticed in the *Reader*, Feb. 27, 1864 (as I learn from one of Prof. Huxley's 'Lay Sermons').

© The Complete Work of Charles Darwin Online

keep a lot of the most noisy monkeys, half free, and study their means of communication!

A book has just appeared here which will, I suppose, make a noise, by Bishop Colenso,* who, judging from extracts, smashes most of the Old Testament. Talking of books, I am in the middle of one which pleases me, though it is very innocent food, viz. Miss Cooper's 'Journal of a Naturalist.' Who is she? She seems a very clever woman, and gives a capital account of the battle between *our* and *your* weeds. Does it not hurt your Yankee pride that we thrash you so confoundedly? I am sure Mrs. Gray will stick up for your own weeds. Ask her whether they are not more honest, downright good sort of weeds. The book gives an extremely pretty picture of one of your villages; but I see your autumn, though so much more gorgeous than ours, comes on sooner, and that is one comfort. . . . .

C. Darwin to H. W. Bates.

Down, Nov. 20, [1862].

DEAR BATES,—I have just finished, after several reads, your paper.† In my opinion it is one of the most remarkable and

* 'The Pentateuch and Book of Joshua critically examined,' six parts, 1862–71.

† This refers to Mr. Bates's paper, "Contributions to an Insect Fauna of the Amazons Valley" ('Linn. Soc. Trans.' xxiii., 1862), in which the now familiar subject of mimicry was founded. My father wrote a short review of it in the 'Natural History Review,' 1863, p. 219, parts of which occur almost verbatim in the later editions of the 'Origin of Species.' A striking passage occurs showing the difficulties of the case from a creationist's point of view:— "By what means, it may be asked, have so many butterflies of the Amazonian region acquired their deceptive dress? Most naturalists will answer that they were thus clothed from the hour of their creation—an answer which will generally be so far triumphant that it can be met only by long-drawn arguments; but it is made at the expense of putting an effectual bar to all further inquiry. In this particular case, moreover, the creationist will meet with special difficulties; for many of the mimicking forms of *Leptalis* can be shown by a graduated series to be merely

© The Complete Work of Charles Darwin Online

admirable papers I ever read in my life. The mimetic cases are truly marvellous, and you connect excellently a host of analogous facts. The illustrations are beautiful, and seem very well chosen; but it would have saved the reader not a little trouble, if the name of each had been engraved below each separate figure. No doubt this would have put the engraver into fits, as it would have destroyed the beauty of the plate. I am not at all surprised at such a paper having consumed much time. I am rejoiced that I passed over the whole subject in the 'Origin,' for I should have made a precious mess of it. You have most clearly stated and solved a wonderful problem. No doubt with most people this will be the cream of the paper; but I am not sure that all your facts and reasonings on variation, and on the segregation of complete and semi-complete species, is not really more, or at least as valuable, a part. I never conceived the process nearly so clearly before; one feels present at the creation of new forms. I wish, however, you had enlarged a little more on the pairing of similar varieties; a rather more numerous body of facts seems here wanted. Then, again, what a host of curious miscellaneous observations there are—as on related

---

varieties of one species; other mimickers are undoubtedly distinct species, or even distinct genera. So again, some of the mimicked forms can be shown to be merely varieties; but the greater number must be ranked as distinct species. Hence the creationist will have to admit that some of these forms have become imitators, by means of the laws of variation, whilst others he must look at as separately created under their present guise; he will further have to admit that some have been created in imitation of forms not themselves created as we now see them, but due to the laws of variation! Prof. Agassiz, indeed, would think nothing of this difficulty; for he believes that not only each species and each variety, but that groups of individuals, though identically the same, when inhabiting distinct countries, have been all separately created in due proportional numbers to the wants of each land. Not many naturalists will be content thus to believe that varieties and individuals have been turned out all ready made, almost as a manufacturer turns out toys according to the temporary demand of the market."

© The Complete Work of Charles Darwin Online

sexual and individual variability: these will some day, if I live, be a treasure to me.

With respect to mimetic resemblance being so common with insects, do you not think it may be connected with their small size; they cannot defend themselves; they cannot escape by flight, at least, from birds, therefore they escape by trickery and deception?

I have one serious criticism to make, and that is about the title of the paper; I cannot but think that you ought to have called prominent attention in it to the mimetic resemblances. Your paper is too good to be largely appreciated by the mob of naturalists without souls; but, rely on it, that it will have *lasting* value, and I cordially congratulate you on your first great work. You will find, I should think, that Wallace will fully appreciate it. How gets on your book? Keep your spirits up. A book is no light labour. I have been better lately, and working hard, but my health is very indifferent. How is your health? Believe me, dear Bates,

Yours very sincerely,

C. DARWIN.

END OF VOL. II.

© The Complete Work of Charles Darwin Online

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

University of Cambridge
DEPARTMENT OF ZOOLOGY
Balfour & Newton Libraries

CAMBRIDGE PHILOSOPHICAL
LIBRARY.

© The Complete Work of Charles Darwin Online

LIFE

AND

LETTERS

OF

CHARLES

DARWIN.

VOL. III.

LONDON:

JOHN MURRAY.

© The Complete Work of Charles Darwin Online

CAMBRIDGE PHILOSOPHICAL
LIBRARY.
Deposited by Prof. Newton
1901.
D4 DAR Dar

© The Complete Work of Charles Darwin Online

BALFOUR & NEWTON LIBRARY
2L7EP 027929213

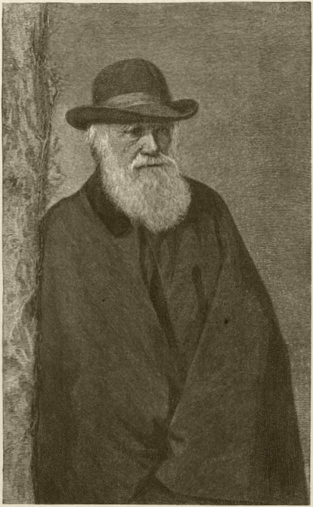
© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online



FROM A PHOTOGRAPH (1881) BY MESSRS. ELLIOTT AND FRY.

Frontispiece, Vol. III.

© The Complete Work of Charles Darwin Online

THE

LIFE AND LETTERS

OF

CHARLES DARWIN,

INCLUDING

AN AUTOBIOGRAPHICAL CHAPTER.

PHILOSOPHICAL LIBRARY
17 OC 1901
CAMBRIDGE

EDITED BY HIS SON,

FRANCIS DARWIN.

IN THREE VOLUMES:—VOL. III.

LONDON:
JOHN MURRAY, ALBEMARLE STREET.
1887.

All Rights Reserved.

© The Complete Work of Charles Darwin Online

369342

LONDON:
PRINTED BY WILLIAM CLOWES AND SONS, LIMITED,
STAMFORD STREET AND CHARING CROSS.

© The Complete Work of Charles Darwin Online

TABLE OF CONTENTS.

VOLUME III.

BOTANICAL LETTERS.

© The Complete Work of Charles Darwin Online

APPENDICES.

ILLUSTRATIONS.

ERRATA.

Volume III.

P. 40, line 13: *for* "Magazines" *read* "Magazine."
P. 46, footnote, last line: *for* "contemporaine" *read* "contemporain."
P. 58, line 8: *for* "laburnums, Adami-trifacial" *read* "laburnum Adami, trifacial."

© The Complete Work of Charles Darwin Online

LIFE AND LETTERS

OF

CHARLES DARWIN.

CHAPTER I.

THE SPREAD OF EVOLUTION.

'VARIATION OF ANIMALS AND PLANTS.'

1863–1866.

His book on animals and plants under domestication was my father's chief employment in the year 1863. His diary records the length of time spent over the composition of its chapters, and shows the rate at which he arranged and wrote out for printing the observations and deductions of several years.

The three chapters in vol. ii. on inheritance, which occupy 84 pages of print, were begun in January and finished on April 1st; the five on crossing, making 106 pages, were written in eight weeks, while the two chapters on selection, covering 57 pages, were begun on June 16th and finished on July 20th.

The work was more than once interrupted by ill-health, and, in September, what proved to be the beginning of a six months' illness forced him to leave home for the water-cure at Malvern. He returned in October, and remained ill and depressed, in spite of the hopeful opinion of one of the most cheery and skilful physicians of the day. Thus he wrote to Sir J. D. Hooker in November:—

"Dr. Brinton has been here (recommended by Busk); he

© The Complete Work of Charles Darwin Online

does not believe my brain or heart are primarily affected, but I have been so steadily going downhill, I cannot help doubting whether I can ever crawl a little uphill again. Unless I can, enough to work a little, I hope my life may be very short, for to lie on a sofa all day and do nothing but give trouble to the best and kindest of wives and good dear children is dreadful."

The minor works in this year were a short paper in the 'Natural History Review' (N.S. vol. iii. p. 115), entitled "On the so-called *Auditory-Sac* of Cirripedes," and one in the 'Geological Society's Journal' (vol. xix.), on the "Thickness of the Pampæan Formation near Buenos Ayres." The paper on Cirripedes was called forth by the criticisms of a German naturalist Krohn,* and is of some interest in illustration of my father's readiness to admit an error.

With regard to the spread of a belief in Evolution, it could not yet be said that the battle was won, but the growth of belief was undoubtedly rapid. So that, for instance, Charles Kingsley could write to F. D. Maurice :—†

"The state of the scientific mind is most curious; Darwin is conquering everywhere, and rushing in like a flood, by the mere force of truth and fact."

Mr. Huxley was as usual active in guiding and stimulating the growing tendency to tolerate or accept the views set forth in the 'Origin of Species.' He gave a series of lectures to working men at the School of Mines in November, 1862. These were printed in 1863 from the shorthand notes of Mr. May, as six little blue books, price 4*d.* each, under the title, 'Our Knowledge of the Causes of Organic Nature.' When published they were read with interest by my father, who thus refers to them in a letter to Sir J. D. Hooker :—

* Krohn stated that the structures described by my father as ovaries were in reality salivary glands, also that the oviduct runs down to the orifice described in the 'Monograph of the Cirripedia' as the auditory *meatus*.

† Kingsley's 'Life,' vol. ii. p. 171.

© The Complete Work of Charles Darwin Online

"I am very glad you like Huxley's lectures. I have been very much struck with them, especially with the 'Philosophy of Induction.' I have quarrelled with him for overdoing sterility and ignoring cases from Gärtner and Kölreuter about sterile varieties. His geology is obscure; and I rather doubt about man's mind and language. But it seems to me *admirably* done, and, as you say, "Oh my!" about the praise of the 'Origin.' I can't help liking it, which makes me rather ashamed of myself."

My father admired the clearness of exposition shown in the lectures, and in the following letter urges their author to make use of his powers for the advantage of students :]

C. Darwin to T. H. Huxley.

Nov. 5 [1864].

I want to make a suggestion to you, but which may probably have occurred to you. —— was reading your Lectures and ended by saying, "I wish he would write a book." I answered, "he has just written a great book on the skull." "I don't call that a book," she replied, and added, "I want something that people can read; he does write so well." Now, with your ease in writing, and with knowledge at your fingers' ends, do you not think you could write a popular Treatise on Zoology? Of course it would be some waste of time, but I have been asked more than a dozen times to recommend something for a beginner and could only think of Carpenter's Zoology. I am sure that a striking Treatise would do real service to science by educating naturalists. If you were to keep a portfolio open for a couple of years, and throw in slips of paper as subjects crossed your mind, you would soon have a skeleton (and that seems to me the difficulty) on which to put the flesh and colours in your inimitable manner. I believe such a book might have a brilliant success, but I did not intend to scribble so much about it.

Give my kindest remembrance to Mrs. Huxley, and tell

B 2

© The Complete Work of Charles Darwin Online

her I was looking at 'Enoch Arden,' and as I know how she admires Tennyson, I must call her attention to two sweetly pretty lines (p. 105) . . .

> . . . and he meant, he said he meant,
> Perhaps he meant, or partly meant, you well.

Such a gem as this is enough to make me young again, and like poetry with pristine fervour.

My dear Huxley,
Yours affectionately,
CH. DARWIN.

[In another letter (Jan. 1865) he returns to the above suggestion, though he was in general strongly opposed to men of science giving up to the writing of text-books, or to teaching, the time that might otherwise have been given to original research.

"I knew there was very little chance of your having time to write a popular treatise on Zoology, but you are about the one man who could do it. At the time I felt it would be almost a sin for you to do it, as it would of course destroy some original work. On the other hand I sometimes think that general and popular treatises are almost as important for the progress of science as original work."

The series of letters will continue the history of the year 1863.]

C. Darwin to J. D. Hooker.

Down, Jan. 3 [1863].

MY DEAR HOOKER.—I am burning with indignation and must exhale. . . . I could not get to sleep till past 3 last night for indignation.* . . .

* It would serve no useful purpose if I were to go into the matter which so strongly roused my father's anger. It was a question of literary dishonesty, in which a friend was the sufferer, but which in no way affected himself.

© The Complete Work of Charles Darwin Online

Now for pleasanter subjects; we were all amused at your defence of stamp collecting and collecting generally. . . . But, by Jove, I can hardly stomach a grown man collecting stamps. Who would ever have thought of your collecting Wedgwood-ware! but that is wholly different, like engravings or pictures. We are degenerate descendants of old Josiah W., for we have not a bit of pretty ware in the house.

. . . Notwithstanding the very pleasant reason you give for our not enjoying a holiday, namely, that we have no vices, it is a horrid bore. I have been trying for health's sake to be idle, with no success. What I shall now have to do, will be to erect a tablet in Down Church, "Sacred to the Memory, &c.," and officially die, and then publish books, "by the late Charles Darwin," for I cannot think what has come over me of late; I always suffered from the excitement of talking, but now it has become ludicrous. I talked lately $1\frac{1}{2}$ hours (broken by tea by myself) with my nephew, and I was [ill] half the night. It is a fearful evil for self and family.

Good-night. Ever yours,

C. DARWIN.

[The following letter to Sir Julius von Haast,* is an example of the sympathy which he felt with the spread and growth of science in the colonies. It was a feeling not expressed once only, but was frequently present in his mind, and often found utterance. When we, at Cambridge, had the satisfaction of receiving Sir J. von Haast into our body as a Doctor of Science (July 1886), I had the opportunity of hearing from him of the vivid pleasure which this, and other letters from my father, gave him. It was pleasant to see how strong had been the impression made by my father's warm-hearted sympathy—an impression which seemed,

* The late Sir Julius von Haast was a German by birth, but had long been resident in New Zealand. He was, in 1862, Government Geologist to the Province of Canterbury.

© The Complete Work of Charles Darwin Online

after more than twenty years, to be as fresh as when it was first received :]

C. Darwin to Julius von Haast.

Down, Jan. 22 [1863].

DEAR SIR,—I thank you most sincerely for sending me your Address and the Geological Report.* I have seldom in my life read anything more spirited and interesting than your address. The progress of your colony makes one proud, and it is really admirable to see a scientific institution founded in so young a nation. I thank you for the very honourable notice of my 'Origin of Species.' You will easily believe how much I have been interested by your striking facts on the old glacial period, and I suppose the world might be searched in vain for so grand a display of terraces. You have, indeed, a noble field for scientific research and discovery. I have been extremely much interested by what you say about the tracks of supposed [living] mammalia. Might I ask, if you succeed in discovering what the creatures are, you would have the great kindness to inform me? Perhaps they may turn out something like the Solenhofen bird creature, with its long tail and fingers, with claws to its wings! I may mention that in South America, in completely uninhabited regions, I found spring rat-traps, baited with *cheese*, were very successful in catching the smaller mammals. I would venture to suggest to you to urge on some of the capable members of your institution to observe annually the rate and manner of spreading of European weeds and insects, and especially to observe *what native plants most fail;* this latter point has never been attended to. Do the introduced hive-bees replace any other insect? &c. All such points are, in my opinion, great desiderata in

* Address to the 'Philosophical Institute of Canterbury (N.Z.).' The "Report" is given in the *New Zealand Government Gazette, Province of Canterbury*, Oct. 1862.

© The Complete Work of Charles Darwin Online

science. What an interesting discovery that of the remains of prehistoric man!

Believe me, dear Sir,

With the most cordial respect and thanks,

Yours very faithfully,

CHARLES DARWIN.

*C. Darwin to Camille Dareste.**

Down, Feb. 16 [1863].

DEAR AND RESPECTED SIR.—I thank you sincerely for your letter and your pamphlet. I had heard (I think in one of M. Quatrefages' books) of your work, and was most anxious to read it, but did not know where to find it. You could not have made me a more valuable present. I have only just returned home, and have not yet read your work; when I do if I wish to ask any questions I will venture to trouble you. Your approbation of my book on Species has gratified me extremely. Several naturalists in England, North America, and Germany, have declared that their opinions on the subject have in some degree been modified, but as far as I know, my book has produced no effect whatever in France, and this makes me the more gratified by your very kind expression of approbation. Pray believe me, dear Sir, with much respect,

Yours faithfully and obliged,

CH. DARWIN.

C. Darwin to J. D. Hooker.

Down, Feb. 24 [1863].

MY DEAR HOOKER.—I am astonished at your note. I have

* Professor Dareste is a well-known worker in Animal Teratology. He was in 1863 living at Lille, but has since then been called to Paris. My father took a special interest in Dareste's work on the production of monsters, as bearing on the causes of variation.

© The Complete Work of Charles Darwin Online

not seen the *Athenæum*,* but I have sent for it, and may get it to-morrow ; and will then say what I think.

I have read Lyell's book. ['The Antiquity of Man.'] The whole certainly struck me as a compilation, but of the highest class, for when possible the facts have been verified on the spot, making it almost an original work. The Glacial chapters seem to me best, and in parts magnificent. I could hardly judge about Man, as all the gloss of novelty was completely worn off. But certainly the aggregation of the evidence produced a very striking effect on my mind. The chapter comparing language and changes of species, seems most ingenious and interesting. He has shown great skill in picking out salient points in the argument for change of species ; but I am deeply disappointed (I do not mean personally) to find that his timidity prevents him giving any judgment. . . . From all my communications with him I must ever think that he has really entirely lost faith in the immutability of species ; and yet one of his strongest sentences is nearly as follows: "If it should *ever* † be rendered highly probable that species change by variation and natural selection," &c. &c. I had hoped he would have guided the public as far as his own belief went. . . . One thing does please me on this subject, that he seems to appreciate your work. No doubt the public or a part may be induced to think that, as he gives to us a larger space than to Lamarck, he must think there is something in our views. When reading the brain chapter, it struck me forcibly that if

* In the 'Antiquity of Man,' first edition, p. 480, Lyell criticised somewhat severely Owen's account of the difference between the Human and Simian brains. The number of the *Athenæum* here referred to (1863, p. 262) contains a reply by Professor Owen to Lyell's strictures. The surprise expressed by my father was at the revival of a controversy which every one believed to be closed. Prof. Huxley (*Medical Times*, Oct. 25, 1862, quoted in 'Man's Place in Nature,' p. 117) spoke of the "two years during which this preposterous controversy has dragged its weary length." And this no doubt expressed a very general feeling.

† The italics are not Lyell's.

© The Complete Work of Charles Darwin Online

he had said openly that he believed in change of species, and as a consequence that man was derived from some Quadrumanous animal, it would have been very proper to have discussed by compilation the differences in the most important organ, viz. the brain. As it is, the chapter seems to me to come in rather by the head and shoulders. I do not think (but then I am as prejudiced as Falconer and Huxley, or more so) that it is too severe; it struck me as given with judicial force. It might perhaps be said with truth that he had no business to judge on a subject on which he knows nothing; but compilers must do this to a certain extent. (You know I value and rank high compilers, being one myself!) I have taken you at your word, and scribbled at great length. If I get the *Athenæum* to-morrow, I will add my impression of Owen's letter.

. . . The Lyells are coming here on Sunday evening to stay till Wednesday. I dread it, but I must say how much disappointed I am that he has not spoken out on species, still less on man. And the best of the joke is that he thinks he has acted with the courage of a martyr of old. I hope I may have taken an exaggerated view of his timidity, and shall *particularly* be glad of your opinion on this head.* When I got his book I turned over the pages, and saw he had discussed the subject of species, and said that I thought he would do more to convert the public than all of us, and now (which makes the case worse for me) I must, in common honesty, retract. I wish to Heaven he had said not a word on the subject.

Wednesday morning: I have read the *Athenæum*. I do not think Lyell will be nearly so much annoyed as you expect. The concluding sentence is no doubt very stinging.

* On this subject my father wrote to Sir Joseph Hooker: "Cordial thanks for your deeply interesting letters about Lyell, Owen, and Co. I cannot say how glad I am to hear that I have not been unjust about the species-question towards Lyell. I feared I had been unreasonable."

© The Complete Work of Charles Darwin Online

No one but a good anatomist could unravel Owen's letter; at least it is quite beyond me.

. . . Lyell's memory plays him false when he says all anatomists were astonished at Owen's paper;* it was often quoted with approbation. I *well* remember Lyell's admiration at this new classification! (Do not repeat this.) I remember it, because, though I knew nothing whatever about the brain, I felt a conviction that a classification thus founded on a single character would break down, and it seemed to me a great error not to separate more completely the Marsupialia. . . .

What an accursed evil it is that there should be all this quarrelling within, what ought to be, the peaceful realms of science.

I will go to my own present subject of inheritance and forget it all for a time. Farewell, my dear old friend,

C. DARWIN.

C. Darwin to Asa Gray.

Down, Feb. 23 [1863].

. . . If you have time to read you will be interested by parts of Lyell's book on man; but I fear that the best part, about the Glacial period, may be too geological for any one except a regular geologist. He quotes you at the end with gusto. By the way, he told me the other day how pleased some had been by hearing that they could purchase your pamphlet. The *Parthenon* also speaks of it as the ablest contribution to the literature of the subject. It delights me when I see your work appreciated.

The Lyells come here this day week, and I shall grumble at his excessive caution. . . . The public may well say, if such a man dare not or will not speak out his mind, how can we who are ignorant form even a guess on the subject? Lyell was pleased when I told him lately that you thought that language might be used as an excellent illustration of deriva-

* "On the Characters, &c., of the Class Mammalia," 'Linn. Soc. Journal,' ii. 1858.

© The Complete Work of Charles Darwin Online

tion of species; you will see that he has an *admirable* chapter on this. . . .

I read Cairns's excellent Lecture,* which shows so well how your quarrel arose from Slavery. It made me for a time wish honestly for the North; but I could never help, though I tried, all the time thinking how we should be bullied and forced into a war by you, when you were triumphant. But I do most truly think it dreadful that the South, with its accursed slavery, should triumph, and spread the evil. I think if I had power, which, thank God, I have not, I would let you conquer the border States, and all west of the Mississippi, and then force you to acknowledge the cotton States. For do you not now begin to doubt whether you can conquer and hold them? I have inflicted a long tirade on you.

The *Times* is getting more detestable (but that is too weak a word) than ever. My good wife wishes to give it up, but I tell her that is a pitch of heroism to which only a woman is equal. To give up the "Bloody Old *Times*," as Cobbett used to call it, would be to give up meat, drink and air. Farewell, my dear Gray,

Yours most truly,
C. DARWIN.

C. Darwin to C. Lyell.

Down, March 6, [1863].

. . . I have been of course deeply interested by your book.† I have hardly any remarks worth sending, but will scribble a little on what most interested me. But I will first get out what I hate saying, viz. that I have been greatly disappointed that you have not given judgment and spoken fairly out what you think about the derivation of species. I should have been contented if you had boldly said that species have not

* Prof. J. E. Cairns, 'The Slave Power, &c.: an attempt to explain the real issues involved in the American contest.' 1862.

† 'Antiquity of Man.'

© The Complete Work of Charles Darwin Online

been separately created, and had thrown as much doubt as you like on how far variation and natural selection suffices. I hope to Heaven I am wrong (and from what you say about Whewell it seems so), but I cannot see how your chapters can do more good than an extraordinary able review. I think the *Parthenon* is right, that you will leave the public in a fog. No doubt they may infer that as you give more space to myself, Wallace, and Hooker, than to Lamarck, you think more of us. But I had always thought that your judgment would have been an epoch in the subject. All that is over with me, and I will only think on the admirable skill with which you have selected the striking points, and explained them. No praise can be too strong, in my opinion, for the inimitable chapter on language in comparison with species.

p. 505—A sentence * at the top of the page makes me groan. . . .

I know you will forgive me for writing with perfect freedom, for you must know how deeply I respect you as my old honoured guide and master. I heartily hope and expect that your book will have gigantic circulation and may do in many ways as much good as it ought to do. I am tired, so no more. I have written so briefly that you will have to guess my meaning. I fear my remarks are hardly worth sending. Farewell, with kindest remembrance to Lady Lyell.

Ever yours,

C. Darwin.

[Mr. Huxley has quoted (Vol. II. p. 193) some passages from Lyell's letters which show his state of mind at this time. The following passage, from a letter of March 11th to my father, is also of much interest:—

* After speculating on the sudden appearance of individuals far above the average of the human race, Lyell asks if such leaps upwards in the scale of intellect may not "have cleared at one bound the space which separated the highest stage of the unprogressive intelligence of the inferior animals from the first and lowest form of improvable reason manifested by man."

© The Complete Work of Charles Darwin Online

"My feelings, however, more than any thought about policy or expediency, prevent me from dogmatising as to the descent of man from the brutes, which, though I am prepared to accept it, takes away much of the charm from my speculations on the past relating to such matters. . . . But you ought to be satisfied, as I shall bring hundreds towards you who, if I treated the matter more dogmatically, would have rebelled."]

C. Darwin to C. Lyell.

Down, 12th [March, 1863].

MY DEAR LYELL,—I thank you for your very interesting and kind, I may say, charming letter. I feared you might be huffed for a little time with me. I know some men would have been so. I have hardly any more criticisms, anyhow, worth writing. But I may mention that I felt a little surprise that old B. de Perthes * was not rather more honourably mentioned. I would suggest whether you could not leave out some references to the 'Principles;' one for the real student is as good as a hundred, and it is rather irritating, and gives a feeling of incompleteness to the general reader to be often referred to other books. As you say that you have gone as far as you believe on the species question, I have not a word to say; but I must feel convinced that at times, judging from conversation, expressions, letters, &c., you have as completely given up belief in immutability of specific forms as I have done. I must still think a clear expression from you, *if you could have given it*, would have been potent with the public, and all the more so, as you formerly held opposite opinions. The more I work, the more satisfied I become with variation and natural selection, but that part of the case I look at as less important, though more interesting to me personally. As you ask for criticisms on this head (and believe me that

* Born 1788, died 1868. See footnote, p. 16.

© The Complete Work of Charles Darwin Online

I should not have made them unasked), I may specify (pp. 412, 413) that such words as "Mr. D. labours to show," "is believed by the author to throw light," would lead a common reader to think that you yourself do *not* at all agree, but merely think it fair to give my opinion. Lastly, you refer repeatedly to my view as a modification of Lamarck's doctrine of development and progression. If this is your deliberate opinion there is nothing to be said, but it does not seem so to me. Plato, Buffon, my grandfather before Lamarck, and others, propounded the *obvious* view that if species were not created separately they must have descended from other species, and I can see nothing else in common between the 'Origin' and Lamarck. I believe this way of putting the case is very injurious to its acceptance, as it implies necessary progression, and closely connects Wallace's and my views with what I consider, after two deliberate readings, as a wretched book, and one from which (I well remember my surprise) I gained nothing. But I know you rank it higher, which is curious, as it did not in the least shake your belief. But enough, and more than enough. Please remember you have brought it all down on yourself!!

I am very sorry to hear about Falconer's "reclamation." * I hate the very word, and have a sincere affection for him.

Did you ever read anything so wretched as the *Athenæum* reviews of you, and of Huxley † especially. Your *object* to make man old, and Huxley's *object* to degrade him. The wretched writer has not a glimpse what the discovery of scientific truth means. How splendid some pages are in Huxley, but I fear the book will not be popular. . . .

* "Falconer, whom I referred to oftener than to any other author, says I have not done justice to the part he took in resuscitating the cave question, and says he shall come out with a separate paper to prove it. I offered to alter anything in the new edition, but this he declined."—C. Lyell to C. Darwin, March 11, 1863; Lyell's 'Life,' vol. ii. p. 364.

† 'Man's Place in Nature,' 1863.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down [March 13, 1863].

I should have thanked you sooner for the *Athenæum* and very pleasant previous note, but I have been busy, and not a little uncomfortable from frequent uneasy feeling of fullness, slight pain and tickling about the heart. But as I have no other symptoms of heart complaint I do not suppose it is affected. . . . I have had a most kind and delightfully candid letter from Lyell, who says he spoke out as far as he believes. I have no doubt his belief failed him as he wrote, for I feel sure that at times he no more believed in Creation than you or I. I have grumbled a bit in my answer to him at his *always* classing my work as a modification of Lamarck's, which it is no more than any author who did not believe in immutability of species, and did believe in descent. I am very sorry to hear from Lyell that Falconer is going to publish a formal reclamation of his own claims. . . .

It is cruel to think of it, but we must go to Malvern in the middle of April; it is ruin to me.* . . .

C. Darwin to C. Lyell.

Down, March 17 [1863].

MY DEAR LYELL,—I have been much interested by your letters and enclosure, and thank you sincerely for giving me so much time when you must be so busy. What a curious letter from B. de P. [Boucher de Perthes]. He seems perfectly satisfied, and must be a very amiable man. I know something about his errors, and looked at his book many years ago, and am ashamed to think that I concluded the

* He went to Hartfield, in Sussex, on April 27, and to Malvern in the autumn.

© The Complete Work of Charles Darwin Online

whole was rubbish! Yet he has done for man something like what Agassiz did for glaciers.*

I cannot say that I agree with Hooker about the public not liking to be told what to conclude, *if coming from one in your position.* But I am heartily sorry that I was led to make complaints, or something very like complaints, on the manner in which you have treated the subject, and still more so anything about myself. I steadily *endeavour* never to forget my firm belief that no one can at all judge about his own work. As for Lamarck, as you have such a man as Grove with you, you are triumphant; not that I can alter my opinion that to me it was an absolutely useless book. Perhaps this was owing to my always searching books for facts, perhaps from knowing my grandfather's earlier and identically the same speculation. I will only further say that if I can analyse my own feelings (a very doubtful process), it is nearly as much for your sake as for my own, that I so much wish that your state of belief could have permitted you to say boldly and distinctly out that species were not separately created. I have generally told you the progress of opinion, as I have heard it, on the species question. A first-rate German naturalist † (I now forget the name!), who has lately published a grand folio, has spoken out to the utmost extent on the 'Origin.' De Candolle, in a very good paper on "Oaks," goes, in Asa Gray's opinion, as far as he himself does; but De Candolle, in writing to me, says *we*, "we think this and that;" so that I infer he really goes to the full extent with me, and tells me of a French good botanical palæontologist (name

* In his 'Antiquités Celtiques' (1847), Boucher de Perthes described the flint tools found at Abbeville with bones of rhinoceros, hyæna, &c. "But the scientific world had no faith in the statement that works of art, however rude, had been met with in undisturbed beds of such antiquity." ('Antiquity of Man,' first edition, p. 95.)

† No doubt Haeckel, whose monograph on the Radiolaria was published in 1862. In the same year Professor W. Preyer of Jena published a Dissertation on *Alca impennis*, which was one of the earliest pieces of special work on the basis of the 'Origin of Species.'

© The Complete Work of Charles Darwin Online

PHILOSOPHICAL ... 17 ... 1901

forgotten),* who writes to De Candolle that he is sure that my views will ultimately prevail. But I did not intend to have written all this. It satisfies me with the final results, but this result, I begin to see, will take two or three lifetimes. The entomologists are enough to keep the subject back for half a century. I really pity your having to balance the claims of so many eager aspirants for notice; it is clearly impossible to satisfy all. . . . Certainly I was struck with the full and due honour you conferred on Falconer. I have just had a note from Hooker. . . . I am heartily glad that you have made him so conspicuous; he is so honest, so candid, and so modest. . . .

I have read ——. I could find nothing to lay hold of, which in one sense I am very glad of, as I should hate a controversy; but in another sense I am very sorry for, as I long to be in the same boat with all my friends. . . . I am heartily glad the book is going off so well.

Ever yours,

C. DARWIN.

C. Darwin to J. D. Hooker.

Down [March 29, 1863].

. . . Many thanks for *Athenæum*, received this morning, and to be returned to-morrow morning. Who would have ever thought of the old stupid *Athenæum* taking to Oken-like transcendental philosophy written in Owenian style!† . . . .

* The Marquis de Saporta.

† This refers to a review of Dr. Carpenter's 'Introduction to the study of Foraminifera,' that appeared in the *Athenæum* of March 28, 1863 (p. 417). The reviewer attacks Dr. Carpenter's views in as much as they support the doctrine of Descent; and he upholds spontaneous generation (Heterogeny) in place of what Dr. Carpenter, naturally enough, believed in, viz. the genetic connection of living and extinct Foraminifera. In the next number is a letter by Dr. Carpenter, which chiefly consists of a protest against the reviewer's somewhat contemptuous classification of Dr. Carpenter and my father as disciple and master. In the course of the letter Dr. Carpenter says—p. 461 :—

© The Complete Work of Charles Darwin Online

It will be some time before we see "slime, protoplasm, &c." generating a new animal.* But I have long regretted that I truckled to public opinion, and used the Pentateuchal term of creation,† by which I really meant "appeared" by some wholly unknown process. It is mere rubbish, thinking at present of the origin of life; one might as well think of the origin of matter.

C. Darwin to J. D. Hooker.

Down, Friday night [April 17, 1863].

MY DEAR HOOKER,—I have heard from Oliver that you will be now at Kew, and so I am going to amuse myself by scribbling a bit. I hope you have thoroughly enjoyed your

---

"Under the influence of his foregone conclusion that I have accepted Mr. Darwin as my master, and his hypothesis as my guide, your reviewer represents me as blind to the significance of the general fact stated by me, that 'there has been no advance in the foraminiferous type from the palæozoic period to the present time.' But for such a foregone conclusion he would have recognised in this statement the expression of my conviction that the present state of scientific evidence, instead of sanctioning the idea that the descendants of the primitive type or types of Foraminifera can ever rise to any higher grade, justifies the ***anti-Darwinian*** inference, that however widely they diverge from each other and from their originals, ***they still remain Foraminifera.***"

* On the same subject my father wrote in 1871: "It is often said that all the conditions for the first production of a living organism are now present, which could ever have been present. But if (and oh! what a big if!) we could conceive in some warm little pond, with all sorts of ammonia and phosphoric salts, light, heat, electricity, &c., present, that a proteine compound was chemically formed ready to undergo still more complex changes, at the present day such matter would be instantly devoured or absorbed, which would not have been the case before living creatures were formed."

† This refers to a passage in which the reviewer of Dr. Carpenter's book speaks of "an operation of force," or "a concurrence of forces which have now no place in nature," as being, "a creative force, in fact, which Darwin could only express in Pentateuchal terms as the primordial form 'into which life was first breathed.'" The conception of expressing a creative force as a primordial form is the Reviewer's.

© The Complete Work of Charles Darwin Online

tour. I never in my life saw anything like the spring flowers this year. What a lot of interesting things have been lately published. I liked extremely your review of De Candolle. What an awfully severe article that by Falconer on Lyell; * I am very sorry for it; I think Falconer on his side does not do justice to old Perthes and Schmerling. . . . . I shall be very curious to see how he [Lyell] answers it to-morrow. (I have been compelled to take in the *Athenæum* for a while.) I am very sorry that Falconer should have written so spitefully, even if there is some truth in his accusations; I was rather disappointed in Carpenter's letter, no one could have given a better answer, but the chief object of his letter seems to me to be to show that though he has touched pitch he is not defiled. No one would suppose he went so far as to believe all birds came from one progenitor. I have written a letter to the *Athenæum* † (the first and last time I shall take such a step)

* *Athenæum*, April 4, 1863, p. 459. The writer asserts that justice has not been done either to himself or Mr. Prestwich—that Lyell has not made it clear that it was their original work which supplied certain material for the 'Antiquity of Man.' Falconer attempts to draw an unjust distinction between a "philosopher" (here used as a polite word for compiler) like Sir Charles Lyell, and original observers, presumably such as himself and Mr. Prestwich. Lyell's reply was published in the *Athenæum*, April 18, 1863. It ought to be mentioned that a letter from Mr. Prestwich (*Athenæum*, p. 555), which formed part of the controversy, though of the nature of a reclamation, was written in a very different spirit and tone from Dr. Falconer's.

† *Athenæum*, 1863, p. 554: "The view given by me on the origin or derivation of species, whatever its weaknesses may be, connects (as has been candidly admitted by some of its opponents, such as Pictet, Bronn, &c.), by an intelligible thread of reasoning, a multitude of facts: such as the formation of domestic races by man's selection,—the classification and affinities of all organic beings,—the innumerable gradations in structure and instincts,—the similarity of pattern in the hand, wing, or paddle of animals of the same great class,—the existence of organs become rudimentary by disuse,—the similarity of an embryonic reptile, bird and mammal, with the retention of traces of an apparatus fitted for aquatic respiration; the retention in the young calf of incisor teeth in the upper jaw, &c.—the distribution of animals and plants, and their mutual affinities within the same region, — their

© The Complete Work of Charles Darwin Online

to say, under the cloak of attacking Heterogeny, a word in my own defence. My letter is to appear next week, so the Editor says; and I mean to quote Lyell's sentence * in his second edition, on the principle if one puffs oneself, one had better puff handsomely. . . .

C. Darwin to C. Lyell.

Down, April 18 [1863].

MY DEAR LYELL,—I was really quite sorry that you had sent me a second copy † of your valuable book. But after a few hours my sorrow vanished for this reason: I have written a letter to the *Athenæum*, in order, under the cloak of attacking the monstrous article on Heterogeny, to say a word for myself in answer to Carpenter, and now I have inserted a few sentences in allusion to your analogous objection ‡ about

general geological succession, and the close relationship of the fossils in closely consecutive formations and within the same country; extinct marsupials having preceded living marsupials in Australia, and armadillo-like animals having preceded and generated armadilloes in South America,—and many other phenomena, such as the gradual extinction of old forms and their gradual replacement by new forms better fitted for their new conditions in the struggle for life. When the advocate of Heterogeny can thus connect large classes of facts, and not until then, he will have respectful and patient listeners."

* See the next letter.

† The second edit. of the 'Antiquity of Man' was published a few months after the first had appeared.

‡ Lyell objected that the mammalia (e.g. bats and seals) which alone have been able to reach oceanic islands ought to have become modified into various terrestrial forms fitted to fill various places in their new homes. My father pointed out in the *Athenæum* that Sir Charles has in some measure answered his own objection, and went on to quote the "amended sentence" ('Antiquity of Man,' 2nd edit. p. 469) as showing how far Lyell agreed with the general doctrines of the 'Origin of Species': "Yet we ought by no means to undervalue the importance of the step which will have been made, should it hereafter become the generally received opinion of men of science (as I fully expect it will) that the past changes of the organic world have been brought about by the subordinate agency of such causes as Variation and Natural Selection." In the first edition the words "as I fully expect it will," do not occur.

© The Complete Work of Charles Darwin Online

bats on islands, and then with infinite slyness have quoted your amended sentence, with your parenthesis ("as I fully believe") *; I do not think you can be annoyed at my doing this, and you see, that I am determined as far as I can, that the public shall see how far you go. This is the first time I have ever said a word for myself in any journal, and it shall, I think, be the last. My letter is short, and no great things. I was extremely concerned to see Falconer's disrespectful and virulent letter. I like extremely your answer just read; you take a lofty and dignified position, to which you are so well entitled.†

I suspect that if you had inserted a few more superlatives in speaking of the several authors there would have been none of this horrid noise. No one, I am sure, who knows you could doubt about your hearty sympathy with every one who makes any little advance in science. I still well remember my surprise at the manner in which you listened to me in Hart Street on my return from the *Beagle's* voyage. You did me a world of good. It is horridly vexatious that so frank and apparently amiable a man as Falconer should have behaved so.‡ Well, it will all soon be forgotten. . . . .

[In reply to the above-mentioned letter of my father's to the *Athenæum*, an article appeared in that Journal (May 2nd, 1863, p. 586), accusing my father of claiming for his views the exclusive merit of "connecting by an intelligible thread of reasoning" a number of facts in morphology, &c. The writer remarks that, "The different generalisations cited by Mr. Darwin as being connected by an intelligible thread of reasoning exclusively through his

* My father here quotes Lyell incorrectly; see the footnote on the previous page.

† In a letter to Sir J. D. Hooker he wrote: "I much like Lyell's letter. But all this squabbling will greatly sink scientific men. I have seen sneers already in the *Times*."

‡ It is to this affair that the extract from a letter to Falconer, given Vol. I. p. 158, refers.

© The Complete Work of Charles Darwin Online

attempt to explain specific transmutation are in fact related to it in this wise, that they have prepared the minds of naturalists for a better reception of such attempts to explain the way of the origin of species from species."

To this my father replied as follows in the *Athenæum* of May 9th, 1863 :]

Down, May 5 [1863].

I hope that you will grant me space to own that your reviewer is quite correct when he states that any theory of descent will connect, "by an intelligible thread of reasoning," the several generalizations before specified. I ought to have made this admission expressly; with the reservation, however, that, as far as I can judge, no theory so well explains or connects these several generalizations (more especially the formation of domestic races in comparison with natural species, the principles of classification, embryonic resemblance, &c.) as the theory, or hypothesis, or guess, if the reviewer so likes to call it, of Natural Selection. Nor has any other satisfactory explanation been ever offered of the almost perfect adaptation of all organic beings to each other, and to their physical conditions of life. Whether the naturalist believes in the views given by Lamarck, by Geoffroy St. Hilaire, by the author of the 'Vestiges,' by Mr. Wallace and myself, or in any other such view, signifies extremely little in comparison with the admission that species have descended from other species, and have not been created immutable ; for he who admits this as a great truth has a wide field opened to him for further inquiry. I believe, however, from what I see of the progress of opinion on the Continent, and in this country, that the theory of Natural Selection will ultimately be adopted, with, no doubt, many subordinate modifications and improvements.

CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

[In the following, he refers to the above letter to the *Athenæum*:]

C. Darwin to J. D. Hooker.

Leith Hill Place,
Saturday [May 11, 1863].

MY DEAR HOOKER,—You give good advice about not writing in newspapers; I have been gnashing my teeth at my own folly; and this not caused by ——'s sneers, which were so good that I almost enjoyed them. I have written once again to own to a certain extent of truth in what he says, and then if I am ever such a fool again, have no mercy on me. I have read the squib in *Public Opinion;** it is capital; if there is more, and you have a copy, do lend it. It shows well that a scientific man had better be trampled in dirt than squabble. I have been drawing diagrams, dissecting shoots, and muddling my brains to a hopeless degree about the divergence of leaves, and have of course utterly failed. But I can see that the subject is most curious, and indeed astonishing. . . . .

[The next letter refers to Mr. Bentham's presidential

* *Public Opinion*, April 23, 1863. A lively account of a police case, in which the quarrels of scientific men are satirised. Mr. John Bull gives evidence that—

"The whole neighbourhood was unsettled by their disputes; Huxley quarrelled with Owen, Owen with Darwin, Lyell with Owen, Falconer and Prestwich with Lyell, and Gray the menagerie man with everybody. He had pleasure, however, in stating that Darwin was the quietest of the set. They were always picking bones with each other and fighting over their gains. If either of the gravel sifters or stone breakers found anything, he was obliged to conceal it immediately, or one of the old bone collectors would be sure to appropriate it first and deny the theft afterwards, and the consequent wrangling and disputes were as endless as they were wearisome.

"Lord Mayor.—Probably the clergyman of the parish might exert some influence over them?

"The gentleman smiled, shook his head, and stated that he regretted to say that no class of men paid so little attention to the opinions of the clergy as that to which these unhappy men belonged."

© The Complete Work of Charles Darwin Online

address to the Linnean Society (May 25, 1863). Mr. Bentham does not yield to the new theory of Evolution, "cannot surrender at discretion so long as many important outworks remain contestable." But he shows that the great body of scientific opinion is flowing in the direction of belief.

The mention of Pasteur by Mr. Bentham is in reference to the promulgation "as it were *ex cathedrâ*," of a theory of spontaneous generation by the reviewer of Dr. Carpenter in the *Athenæum* (March 28, 1863). Mr. Bentham points out that in ignoring Pasteur's refutation of the supposed facts of spontaneous generation, the writer fails to act with "that impartiality which every reviewer is supposed to possess."]

C. Darwin to G. Bentham.

Down, May 22 [1863].

MY DEAR BENTHAM.—I am much obliged for your kind and interesting letter. I have no fear of anything that a man like you will say annoying me in the very least degree. On the other hand, any approval from one whose judgment and knowledge I have for many years so sincerely respected, will gratify me much. The objection which you well put, of certain forms remaining unaltered through long time and space, is no doubt formidable in appearance, and to a certain extent in reality according to my judgment. But does not the difficulty rest much on our silently assuming that we know more than we do? I have literally found nothing so difficult as to try and always remember our ignorance. I am never weary, when walking in any new adjoining district or country, of reflecting how absolutely ignorant we are why certain old plants are not there present, and other new ones are, and others in different proportions. If we once fully feel this, then in judging the theory of Natural Selection, which implies that a form will remain unaltered unless some alteration be to its

© The Complete Work of Charles Darwin Online

benefit, is it so very wonderful that some forms should change much slower and much less, and some few should have changed not at all under conditions which to us (who really know nothing what are the important conditions) seem very different. Certainly *a priori* we might have anticipated that all the plants anciently introduced into Australia would have undergone some modification; but the fact that they have not been modified does not seem to me a difficulty of weight enough to shake a belief grounded on other arguments. I have expressed myself miserably, but I am far from well to-day.

I am very glad that you are going to allude to Pasteur; I was struck with infinite admiration at his work. With cordial thanks, believe me, dear Bentham,

Yours very sincerely,
CH. DARWIN.

P.S.—In fact the belief in Natural Selection must at present be grounded entirely on general considerations. (1) On its being a *vera causa*, from the struggle for existence; and the certain geological fact that species do somehow change. (2) From the analogy of change under domestication by man's selection. (3) And chiefly from this view connecting under an intelligible point of view a host of facts. When we descend to details, we can prove that no one species has changed [*i.e.* we cannot prove that a single species has changed]; nor can we prove that the supposed changes are beneficial, which is the groundwork of the theory. Nor can we explain why some species have changed and others have not. The latter case seems to me hardly more difficult to understand precisely and in detail than the former case of supposed change. Bronn may ask in vain, the old creationist school and the new school, why one mouse has longer ears than another mouse, and one plant more pointed leaves than another plant.

© The Complete Work of Charles Darwin Online

C. Darwin to G. Bentham.

Down, June 19 [1863].

My dear Bentham,—I have been extremely much pleased and interested by your address, which you kindly sent me. It seems to be excellently done, with as much judicial calmness and impartiality as the Lord Chancellor could have shown. But whether the "immutable" gentlemen would agree with the impartiality may be doubted, there is too much kindness shown towards me, Hooker, and others, they might say. Moreover I verily believe that your address, written as it is, will do more to shake the unshaken and bring on those leaning to our side, than anything written directly in favour of transmutation. I can hardly tell why it is, but your address has pleased me as much as Lyell's book disappointed me, that is, the part on species, though so cleverly written. I agree with all your remarks on the reviewers. By the way, Lecoq* is a believer in the change of species. I, for one, can conscientiously declare that I never feel surprised at any one sticking to the belief of immutability; though I am often not a little surprised at the arguments advanced on this side. I remember too well my endless oscillations of doubt and difficulty. It is to me really laughable, when I think of the years which elapsed before I saw what I believe to be the explanation of some parts of the case; I believe it was fifteen years after I began before I saw the meaning and cause of the divergence of the descendants of any one pair. You pay me some most elegant and pleasing compliments. There is much in your address which has pleased me much, especially your remarks on various naturalists. I am so glad that you have alluded so honourably to Pasteur. I have just read over this note; it does not express strongly enough the interest which I have felt in reading your address. You have done, I

* Author of 'Géographie Botanique.' 9 vols. 1854–58.

© The Complete Work of Charles Darwin Online

believe, a real good turn to the *right side*. Believe me, dear Bentham,

Yours very sincerely,
CH. DARWIN.

1864.

[In my father's diary for 1864 is the entry, "Ill all January, February, March." About the middle of April (seven months after the beginning of the illness in the previous autumn) his health took a turn for the better. As soon as he was able to do any work, he began to write his papers on Lythrum, and on Climbing Plants, so that the work which now concerns us did not begin until September, when he again set to work on 'Animals and Plants.' A letter to Sir J. D. Hooker gives some account of the re-commencement of the work: "I have begun looking over my old MS., and it is as fresh as if I had never written it; parts are astonishingly dull, but yet worth printing, I think; and other parts strike me as very good. I am a complete millionaire in odd and curious little facts, and I have been really astounded at my own industry whilst reading my chapters on Inheritance and Selection. God knows when the book will ever be completed, for I find that I am very weak and on my best days cannot do more than one or one and a half hours' work. It is a good deal harder than writing about my dear climbing plants."

In this year he received the greatest honour which a scientific man can receive in this country—the Copley Medal of the Royal Society. It is presented at the Anniversary Meeting on St. Andrew's Day (Nov. 30), the medallist being usually present to receive it, but this the state of my father's health prevented. He wrote to Mr. Fox on this subject:—

"I was glad to see your hand-writing. The Copley, being open to all sciences and all the world, is reckoned a great honour; but excepting from several kind letters, such things make little difference to me. It shows, however, that

© The Complete Work of Charles Darwin Online

Natural Selection is making some progress in this country, and that pleases me. The subject, however, is safe in foreign lands."

To Sir J. D. Hooker, also, he wrote:—

"How kind you have been about this medal; indeed, I am blessed with many good friends, and I have received four or five notes which have warmed my heart. I often wonder that so old a worn-out dog as I am is not quite forgotten. Talking of medals, has Falconer had the Royal? he surely ought to have it, as ought John Lubbock. By the way, the latter tells me that some old members of the Royal are quite shocked at my having the Copley. Do you know who?"

He wrote to Mr. Huxley:—

"I must and will answer you, for it is a real pleasure for me to thank you cordially for your note. Such notes as this of yours, and a few others, are the real medal to me, and not the round bit of gold. These have given me a pleasure which will long endure; so believe in my cordial thanks for your note."

Sir Charles Lyell, writing to my father in November 1864 ('Life,' vol. ii. p. 384), speaks of the supposed malcontents as being afraid to crown anything so unorthodox as the 'Origin.' But he adds that if such were their feelings "they had the good sense to draw in their horns." It appears, however, from the same letter, that the proposal to give the Copley Medal to my father in the previous year failed owing to a similar want of courage—to Lyell's great indignation.

In the *Reader*, December 3, 1864, General Sabine's presidential address at the Anniversary Meeting is reported at some length. Special weight was laid on my father's work in Geology, Zoology, and Botany, but the 'Origin of Species' is praised chiefly as containing "a mass of observations," &c. It is curious that as in the case of his election to the French Institute, so in this case, he was honoured not for the great work of his life, but for his less important work in special lines. The paragraph in General Sabine's address which refers to the 'Origin of Species,' is as follows:—

© The Complete Work of Charles Darwin Online

"In his most recent work 'On the Origin of Species,' although opinions may be divided or undecided with respect to its merits in some respects, all will allow that it contains a mass of observations bearing upon the habits, structure, affinities, and distribution of animals, perhaps unrivalled for interest, minuteness, and patience of observation. Some amongst us may perhaps incline to accept the theory indicated by the title of this work, while others may perhaps incline to refuse, or at least to remit it to a future time, when increased knowledge shall afford stronger grounds for its ultimate acceptance or rejection. Speaking generally and collectively, we have expressly omitted it from the grounds of our award."

I believe I am right in saying that no little dissatisfaction at the President's manner of allusion to the 'Origin' was felt by some Fellows of the Society.

The presentation of the Copley Medal is of interest in another way, inasmuch as it led to Sir C. Lyell making, in his after-dinner speech, a "confession of faith as to the 'Origin.'" He wrote to my father ('Life,' vol. ii. p. 384), "I said I had been forced to give up my old faith without thoroughly seeing my way to a new one. But I think you would have been satisfied with the length I went."]

C. Darwin to T. H. Huxley.

Down, Oct. 3 [1864].

MY DEAR HUXLEY,—If I do not pour out my admiration of your article * on Kölliker, I shall explode. I never read

* "Criticisms on the Origin of Species," 'Nat. Hist. Review,' 1864. Republished in 'Lay Sermons,' 1870, p. 328. The work of Professor Kölliker referred to is 'Ueber die Darwin'sche Schöpfungstheorie' (Leipzig, 1864). Toward Professor Kölliker my father felt not only the respect due to so distinguished a naturalist (a sentiment well expressed in Professor Huxley's review), but he had also a personal regard for him, and often alluded with satisfaction to the visit which Professor Kölliker paid at Down.

© The Complete Work of Charles Darwin Online

anything better done. I had much wished his article answered, and indeed thought of doing so myself, so that I considered several points. You have hit on all, and on some in addition, and oh! by Jove, how well you have done it. As I read on and came to point after point on which I had thought, I could not help jeering and scoffing at myself, to see how infinitely better you had done it than I could have done. Well, if any one, who does not understand Natural Selection, will read this, he will be a blockhead if it is not as clear as daylight. Old Flourens * was hardly worth the powder and shot; but how capitally you bring in about the Academician, and your metaphor of the sea-sand is *inimitable*.

It is a marvel to me how you can resist becoming a regular reviewer. Well, I have exploded now, and it has done me a deal of good. . . .

[In the same article in the 'Natural History Review,' Mr. Huxley speaks of the book above alluded to by Flourens, the Secrétaire Perpétuel of the French Academy, as one of the two "most elaborate criticisms" of the 'Origin of Species' of the year. He quotes the following passage:—

"M. Darwin continue: 'Aucune distinction absolue n'a été et ne peut être établie entre les espèces et les variétés! Je vous ai déjà dit que vous vous trompiez; une distinction absolue sépare les variétés d'avec les espèces." Mr. Huxley remarks on this, "Being devoid of the blessings of an Academy in England, we are unaccustomed to see our ablest men treated in this way even by a Perpetual Secretary." After demonstrating M. Flourens' misapprehension of Natural Selection, Mr. Huxley says, "How one knows it all by heart, and with what relief one reads at p. 65, 'Je laisse M. Darwin.'"

On the same subject my father wrote to Mr. Wallace:—

"A great gun, Flourens, has written a little dull book

* 'Examen du livre de M. Darwin sur l'origine des espèces. Par P. Flourens.' 8vo. Paris, 1864.

© The Complete Work of Charles Darwin Online

against me, which pleases me much, for it is plain that our good work is spreading in France. He speaks of the 'engouement' about this book 'so full of empty and presumptuous thoughts.'" The passage here alluded to is as follows:—

"'Enfin l'ouvrage de M. Darwin a paru. On ne peut qu'être frappé du talent de l'auteur. Mais que d'idées obscures, que d'idées fausses! Quel jargon métaphysique jeté mal à propos dans l'histoire naturelle, qui tombe dans le galimatias dès qu'elle sort des idées claires, des idées justes. Quel langage prétentieux et vide! Quelles personnifications puériles et surannées! O lucidité! O solidité de l'esprit français, que devenez-vous?'"]

1865.

[This was again a time of much ill-health, but towards the close of the year he began to recover under the care of the late Dr. Bence-Jones, who dieted him severely, and as he expressed it, "half-starved him to death." He was able to work at 'Animals and Plants' until nearly the end of April, and from that time until December he did practically no work, with the exception of looking over the 'Origin of Species' for a second French edition. He wrote to Sir J. D. Hooker:—"I am, as it were, reading the 'Origin' for the first time, for I am correcting for a second French edition: and upon my life, my dear fellow, it is a very good book, but oh! my gracious, it is tough reading, and I wish it were done."*

The following letter refers to the Duke of Argyll's address to the Royal Society of Edinburgh, December 5th, 1864, in which he criticises the 'Origin of Species.' My father seems to have read the Duke's address as reported in the *Scotsman* of December 6th, 1865. In a letter to my father (Jan. 16,

* Towards the end of the year my father received the news of a new convert to his views, in the person of the distinguished American naturalist Lesquereux. He wrote to Sir J. D. Hooker: "I have had an enormous

© The Complete Work of Charles Darwin Online

1865, 'Life,' vol. ii. p. 385), Lyell wrote, "The address is a great step towards your views—far greater, I believe, than it seems when read merely with reference to criticisms and objections":]

C. Darwin to C. Lyell.

Down, January 22, 1865.

MY DEAR LYELL,—I thank you for your very interesting letter. I have the true English instinctive reverence for rank, and therefore liked to hear about the Princess Royal.* You ask what I think of the Duke's address, and I shall be glad to tell you. It seems to me *extremely* clever, like everything I have read of his; but I am not shaken—perhaps you will say that neither gods nor men could shake me. I demur to the Duke reiterating his objection that the brilliant plumage of the male humming-bird could not have been acquired through selection, at the same time entirely ignoring my discussion (p. 93, 3rd edition) on beautiful plumage being acquired through *sexual* selection. The Duke may think this insufficient, but that is another question. All analogy makes me quite disagree with the Duke that the difference in the beak, wing, and tail, are not of importance to the several species. In the only two species which I have watched, the difference in flight and in the use of the tail was conspicuously great.

The Duke, who knows my Orchid book so well, might have learnt a lesson of caution from it, with respect to his doctrine

---

letter from Leo Lesquereux (after doubts, I did not think it worth sending you) on Coal Flora. He wrote some excellent articles in 'Silliman' against 'Origin' views; but he says now, after repeated reading of the book, he is a convert!"

* "I had . . . an animated conversation on Darwinism with the Princess Royal, who is a worthy daughter of her father, in the reading of good books, and thinking of what she reads. She was very much *au fait* at the 'Origin,' and Huxley's book, the 'Antiquity,' &c."—Lyell's 'Life,' vol. ii. p. 385.

© The Complete Work of Charles Darwin Online

of differences for mere variety or beauty. It may be confidently said that no tribe of plants presents such grotesque and beautiful differences, which no one until lately, conjectured were of any use; but now in almost every case I have been able to show their important service. It should be remembered that with humming-birds or orchids, a modification in one part will cause correlated changes in other parts. I agree with what you say about beauty. I formerly thought a good deal on the subject, and was led quite to repudiate the doctrine of beauty being created for beauty's sake. I demur also to the Duke's expression of "new births." That may be a very good theory, but it is not mine, unless indeed he calls a bird born with a beak $\frac{1}{100}$th of an inch longer than usual "a new birth;" but this is not the sense in which the term would usually be understood. The more I work, the more I feel convinced that it is by the accumulation of such extremely slight variations that new species arise. I do not plead guilty to the Duke's charge, that I forget that natural selection means only the preservation of variations which independently arise.* I have expressed this in as strong language as I could use, but it would have been infinitely tedious had I on every occasion thus guarded myself. I will cry "peccavi" when I hear of the Duke or you attacking breeders for saying that man has made his improved shorthorns, or pouter pigeons, or bantams. And I could quote still stronger expressions used by agriculturists. Man does make his artificial breeds, for his selective power is of such importance relatively to that of the slight spontaneous variations. But no one will attack breeders for using such expressions, and the rising generation will not blame me.

Many thanks for your offer of sending me the 'Elements.'†

* "Strictly speaking, therefore, Mr. Darwin's theory is not a theory on the Origin of Species at all, but only a theory on the causes which lead to the relative success and failure of such new forms as may be born into the world."—*Scotsman*, Dec. 6, 1864.

† Sixth edition in one volume.

© The Complete Work of Charles Darwin Online

I hope to read it all, but unfortunately reading makes my head whiz more than anything else. I am able most days to work for two or three hours, and this makes all the difference in my happiness. I have resolved not to be tempted astray, and to publish nothing till my volume on Variation is completed. You gave me excellent advice about the footnotes in my Dog chapter, but their alteration gave me infinite trouble, and I often wished all the dogs, and I fear sometimes you yourself, in the nether regions.

We (dictator and writer) send our best love to Lady Lyell.

Yours affectionately,
CHARLES DARWIN.

P.S.—If ever you should speak with the Duke on the subject, please say how much interested I was with his address.

[In his autobiographical sketch, my father has remarked (p. 40) that owing to certain early memories he felt the honour of being elected to the Royal and Royal Medical Societies of Edinburgh "more than any similar honour." The following extract from a letter to Sir Joseph Hooker refers to his election to the former of these societies. The latter part of the extract refers to the Berlin Academy, to which he was elected in 1878:—

"Here is a really curious thing, considering that Brewster is President and Balfour Secretary. I have been elected Honorary Member of the Royal Society of Edinburgh. And this leads me to a third question. Does the Berlin Academy of Sciences send their Proceedings to Honorary Members? I want to know, to ascertain whether I am a member; I suppose not, for I think it would have made some impression on me; yet I distinctly remember receiving some diploma signed by Ehrenberg. I have been so careless; I have lost several diplomas, and now I want to know what Societies I belong to, as I observe every [one] tacks their titles to their names in the catalogue of the Royal Soc."]

© The Complete Work of Charles Darwin Online

C. Darwin to C. Lyell.

Down, Feb. 21 [1865].

MY DEAR LYELL,—I have taken a long time to thank you very much for your present of the 'Elements.'

I am going through it all, reading what is new, and what I have forgotten, and this is a good deal.

I am simply astonished at the amount of labour, knowledge, and clear thought condensed in this work. The whole strikes me as something quite grand. I have been particularly interested by your account of Heer's work and your discussion on the Atlantic Continent. I am particularly delighted at the view which you take on this subject; for I have long thought Forbes did an ill service in so freely making continents.

I have also been very glad to read your argument on the denudation of the Weald, and your excellent *résumé* on the Purbeck Beds; and this is the point at which I have at present arrived in your book. I cannot say that I am quite convinced that there is no connection beyond that pointed out by you, between glacial action and the formation of lake basins; but you will not much value my opinion on this head, as I have already changed my mind some half-dozen times.

I want to make a suggestion to you. I found the weight of your volume intolerable, especially when lying down, so with great boldness cut it into two pieces, and took it out of its cover; now could not Murray without any other change add to his advertisement a line saying, "if bound in two volumes, one shilling or one shilling and sixpence extra." You thus might originate a change which would be a blessing to all weak-handed readers.

Believe me, my dear Lyell,

Yours most sincerely,

CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

Originate a second *real blessing* and have the edges of the sheets cut like a bound book.*

C. Darwin to John Lubbock.

Down, June 11 [1865].

My dear Lubbock,—The latter half of your book † has been read aloud to me, and the style is so clear and easy (we both think it perfection) that I am now beginning at the beginning. I cannot resist telling you how excellently well, in my opinion, you have done the very interesting chapter on savage life. Though you have necessarily only compiled the materials the general result is most original. But I ought to keep the term original for your last chapter, which has struck me as an admirable and profound discussion. It has quite delighted me, for now the public will see what kind of man you are, which I am proud to think I discovered a dozen years ago.

I do sincerely wish you all success in your election and in politics; but after reading this last chapter, you must let me say: oh, dear! oh, dear! oh dear!

Yours affectionately,

Ch. Darwin.

P.S.—You pay me a superb compliment,‡ but I fear you

* This was a favourite reform of my father's. He wrote to the *Athenæum* on the subject, Feb. 5, 1867, pointing out that a book cut, even carefully, with a paper knife collects dust on its edges far more than a machine-cut book. He goes on to quote the case of a lady of his acquaintance who was in the habit of cutting books with her thumb, and finally appeals to the *Athenæum* to earn the gratitude of children "who have to cut through dry and pictureless books for the benefit of their elders." He tried to introduce the reform in the case of his own books, but found the conservatism of booksellers too strong for him. The presentation copies, however, of all his later books were sent out with the edges cut.

† 'Prehistoric Times,' 1865.

‡ 'Prehistoric Times,' p. 487, where the words, "the discoveries of a Newton or a Darwin," occur.

© The Complete Work of Charles Darwin Online

will be quizzed for it by some of your friends as too exaggerated.

[The following letter refers to Fritz Müller's book, 'Für Darwin,' which was afterwards translated, at my father's suggestion, by Mr. Dallas. It is of interest as being the first of the long series of letters which my father wrote to this distinguished naturalist. They never met, but the correspondence with Müller, which continued to the close of my father's life, was a source of very great pleasure to him. My impression is that of all his unseen friends Fritz Müller was the one for whom he had the strongest regard. Fritz Müller is the brother of another distinguished man, the late Hermann Müller, the author of 'Die Befruchtung der Blumen,' and of much other valuable work :]

C. Darwin to F. Müller.

Down, August 10 [1865].

MY DEAR SIR,—I have been for a long time so ill that I have only just finished hearing read aloud your work on species. And now you must permit me to thank you cordially for the great interest with which I have read it. You have done admirable service in the cause in which we both believe. Many of your arguments seem to me excellent, and many of your facts wonderful. Of the latter, nothing has surprised me so much as the two forms of males. I have lately investigated the cases of dimorphic plants, and I should much like to send you one or two of my papers if I knew how. I did send lately by post a paper on climbing plants, as an experiment to see whether it would reach you. One of the points which has struck me most in your paper is that on the differences in the air-breathing apparatus of the several forms. This subject appeared to me very important when I formerly considered the electric apparatus of fishes. Your

© The Complete Work of Charles Darwin Online

observations on Classification and Embryology seem to me very good and original. They show what a wonderful field there is for enquiry on the development of crustacea, and nothing has convinced me so plainly what admirable results we shall arrive at in Natural History in the course of a few years. What a marvellous range of structure the crustacea present, and how well adapted they are for your enquiry! Until reading your book I knew nothing of the Rhizocephala; pray look at my account and figures of Anelasma, for it seems to me that this latter cirripede is a beautiful connecting link with the Rhizocephala.

If ever you have any opportunity, as you are so skilful a dissector, I much wish that you would look to the orifice at the base of the first pair of cirrhi in cirripedes, and at the curious organ in it, and discover what its nature is; I suppose I was quite in error, yet I cannot feel fully satisfied at Krohn's * observations. Also if you ever find any species of Scalpellum, pray look for complemental males; a German author has recently doubted my observations, for no reason except that the facts appeared to him so strange.

Permit me again to thank you cordially for the pleasure which I have derived from your work, and to express my sincere admiration for your valuable researches.

Believe me, dear Sir, with sincere respect,

Yours very faithfully,

CH. DARWIN.

P.S.—I do not know whether you care at all about plants, but if so, I should much like to send you my little work on the 'Fertilization of Orchids,' and I think I have a German copy.

Could you spare me a photograph of yourself? I should much like to possess one.

* See Vol. II. p. 345, Vol. III. p. 2.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, Thursday, 27th [Sept. 1865].

MY DEAR HOOKER,—I had intended writing this morning to thank Mrs. Hooker most sincerely for her last and several notes about you, and now your own note in your hand has rejoiced me. To walk between five and six miles is splendid, with a little patience you must soon be well. I knew you had been very ill, but I hardly knew how ill, until yesterday, when Bentham (from the Cranworths *) called here, and I was able to see him for ten minutes. He told me also a little about the last days of your father; † I wish I had known your father better, my impression is confined to his remarkably cordial, courteous and frank bearing. I fully concur and understand what you say about the difference of feeling in the loss of a father and child. I do not think any one could love a father much more than I did mine, and I do not believe three or four days ever pass without my still thinking of him, but his death at eighty-four caused me nothing of that insufferable grief ‡ which the loss of poor dear Annie caused. And this seems to me perfectly natural, for one knows that for years previously

* Robert Rolfe, Lord Cranworth, and Lord Chancellor of England, lived at Holwood, near Down.

† Sir Wm. Hooker; b. 1785, d. 1865. He took charge of the Royal Gardens at Kew, in 1840, when they ceased to be the private gardens of the Royal Family. In doing so, he gave up his professorship at Glasgow—and with it half of his income. He founded the herbarium and library, and within ten years he succeeded in making the gardens the first in the world. It is, thus, not too much to say that the creation of the establishment at Kew is due to the abilities and self-devotion of Sir William Hooker. While, for the subsequent development of the gardens up to their present magnificent condition, the nation must thank Sir Joseph Hooker, in whom the same qualities are so conspicuous.

‡ I may quote here a passage from a letter of November 1863. It was written to a friend who had lost his child: "How well I remember your feeling, when we lost Annie. It was my greatest comfort that I had never spoken a harsh word to her. Your grief has made me shed a few tears over our poor darling; but believe me that these tears have lost that unutterable bitterness of former days."

© The Complete Work of Charles Darwin Online

that one's father's death is drawing slowly nearer and nearer, while the death of one's child is a sudden and dreadful wrench. What a wonderful deal you read; it is a horrid evil for me that I can read hardly anything, for it makes my head almost immediately begin to sing violently. My good womenkind read to me a great deal, but I dare not ask for much science, and am not sure that I could stand it. I enjoyed Tylor * *extremely*, and the first part of Lecky; † but I think the latter is often vague, and gives a false appearance of throwing light on his subject by such phrases as "spirit of the age," "spread of civilization," &c. I confine my reading to a quarter or half hour per day in skimming through the back volumes of the Annals and Magazines of Natural History, and find much that interests me. I miss my climbing plants very much, as I could observe them when very poorly.

I did not enjoy the 'Mill on the Floss' so much as you, but from what you say we will read it again. Do you know 'Silas Marner'? it is a charming little story; if you run short, and like to have it, we could send it by post. . . . We have almost finished the first volume of Palgrave,‡ and I like it much; but did you ever see a book so badly arranged? The frequency of the allusions to what will be told in the future are quite laughable. . . . By the way, I was very much pleased with the foot-note § about Wallace in Lubbock's last chapter. I had not heard that Huxley had backed up Lubbock about Parliament. . . . Did you see a sneer some time ago in the *Times* about how incomparably more interesting

* 'Researches into the Early History of Mankind,' by E. B. Tylor. 1865.

† 'The Rise of Rationalism in Europe,' by W. E. H. Lecky. 1865.

‡ William Gifford Palgrave's 'Travels in Arabia,' published in 1865.

§ The passage which seems to be referred to occurs in the text (p. 479) of 'Prehistoric Times.' It expresses admiration of Mr. Wallace's paper in the 'Anthropological Review' (May 1864), and speaks of the author's "characteristic unselfishness" in ascribing the theory of Natural Selection "unreservedly to Mr. Darwin."

© The Complete Work of Charles Darwin Online

politics were compared with science even to scientific men? Remember what Trollope says, in 'Can you Forgive her?' about getting into Parliament, as the highest earthly ambition. Jeffrey, in one of his letters, I remember, says that making an effective speech in Parliament is a far grander thing than writing the grandest history. All this seems to me a poor short-sighted view. I cannot tell you how it has rejoiced me once again seeing your handwriting—my best of old friends.

Yours affectionately,
CH. DARWIN.

[In October he wrote Sir J. D. Hooker:—

"Talking of the 'Origin,' a Yankee has called my attention to a paper attached to Dr. Wells' famous 'Essay on Dew,' which was read in 1813 to the Royal Soc., but not [then] printed, in which he applies most distinctly the principle of Natural Selection to the Races of Man. So poor old Patrick Matthew is not the first, and he cannot, or ought not, any longer to put on his title-pages, 'Discoverer of the principle of Natural Selection'!"]

*C. Darwin to F. W. Farrar.**

Down, Nov. 2 [1865?]

DEAR SIR,—As I have never studied the science of language, it may perhaps seem presumptuous, but I cannot resist the pleasure of telling you what interest and pleasure I have derived from hearing read aloud your volume.†

I formerly read Max Müller, and thought his theory (if it deserves to be called so) both obscure and weak; and now, after hearing what you say, I feel sure that this is the case, and that your cause will ultimately triumph. My indirect interest in your book has been increased from Mr. Hensleigh Wedgwood, whom you often quote, being my brother-in-law.

* Canon of Westminster.

† 'Chapters on Language,' 1865.

© The Complete Work of Charles Darwin Online

No one could dissent from my views on the modification of species with more courtesy than you do. But from the tenor of your mind I feel an entire and comfortable conviction (and which cannot possibly be disturbed) that if your studies led you to attend much to general questions in natural history you would come to the same conclusion that I have done.

Have you ever read Huxley's little book of Lectures? I would gladly send you a copy if you think you would read it.

Considering what Geology teaches us, the argument from the supposed immutability of specific types seems to me much the same as if, in a nation which had no old writings, some wise old savage was to say that his language had never changed; but my metaphor is too long to fill up.

Pray believe me, dear Sir, yours very sincerely obliged,

C. DARWIN.

1866.

[The year 1866 is given in my father's Diary in the following words:—

"Continued correcting chapters of 'Domestic Animals.'

March 1st.—Began on 4th edition of 'Origin' of 1250 copies (received for it £238), making 7500 copies altogether.

May 10th.—Finished 'Origin,' except revises, and began going over Chapter XIII. of 'Domestic Animals.'

Nov. 21st.—Finished 'Pangenesis.'

Dec. 21st.—Finished re-going over all chapters, and sent them to printers.

Dec. 22nd.—Began concluding chapter of book."

He was in London on two occasions for a week at a time, staying with his brother, and for a few days (May 29th–June 2nd) in Surrey; for the rest of the year he was at Down.

© The Complete Work of Charles Darwin Online

There seems to have been a gradual amendment in his health; thus he wrote to Mr. Wallace (January 1866):—"My health is so far improved that I am able to work one or two hours a day."

With respect to the 4th edition he wrote to Sir J. D. Hooker:—

"The new edition of the 'Origin' has caused me two great vexations. I forgot Bates's paper on variation,* but I remembered in time his mimetic work, and now, strange to say, I find I have forgotten your Arctic paper! I know how it arose; I indexed for my bigger work, and never expected that a new edition of the 'Origin' would be wanted.

"I cannot say how all this has vexed me. Everything which I have read during the last four years I find is quite washy in my mind." As far as I know, Mr. Bates's paper was not mentioned in the later editions of the 'Origin,' for what reason I cannot say.

In connection with his work on 'The Variation of Animals and Plants,' I give here extracts from three letters addressed to Mr. Huxley, which are of interest as giving some idea of the development of the theory of 'Pangenesis,' ultimately published in 1868 in the book in question:]

C. Darwin to T. H. Huxley.

Down, May 27, [1865?]

. . . I write now to ask a favour of you, a very great favour from one so hard worked as you are. It is to read thirty pages of MS., excellently copied out, and give me, not lengthened criticism, but your opinion whether I may venture to publish it. You may keep the MS. for a month or two. I would not ask this favour, but I *really* know no one else whose judgment on the subject would be final with me.

* This appears to refer to "Notes on South American Butterflies," Trans. Entomolog. Soc., vol. v. (N.S.).

© The Complete Work of Charles Darwin Online

The case stands thus: in my next book I shall publish long chapters on bud- and seminal-variation, on inheritance, reversion, effects of use and disuse, &c. I have also for many years speculated on the different forms of reproduction. Hence it has come to be a passion with me to try to connect all such facts by some sort of hypothesis. The MS. which I wish to send you gives such a hypothesis; it is a very rash and crude hypothesis, yet it has been a considerable relief to my mind, and I can hang on it a good many groups of facts. I well know that a mere hypothesis, and this is nothing more, is of little value; but it is very useful to me as serving as a kind of summary for certain chapters. Now I earnestly wish for your verdict given briefly as, "Burn it"—or, which is the most favourable verdict I can hope for, "It does rudely connect together certain facts, and I do not think it will immediately pass out of my mind." If you can say this much, and you do not think it absolutely ridiculous, I shall publish it in my concluding chapter. Now will you grant me this favour? You must refuse if you are too much over-worked.

I must say for myself that I am a hero to expose my hypothesis to the fiery ordeal of your criticism.

July 12, [1865 ?]

MY DEAR HUXLEY,—I thank you most sincerely for having so carefully considered my MS. It has been a real act of kindness. It would have annoyed me extremely to have re-published Buffon's views, which I did not know of, but I will get the book; and if I have strength I will also read Bonnet. I do not doubt your judgment is perfectly just, and I will try to persuade myself not to publish. The whole affair is much too speculative; yet I think some such view will have to be adopted, when I call to mind such facts as the inherited effects of use and disuse, &c. But I will try to be cautious. . . .

© The Complete Work of Charles Darwin Online

[1865?]

MY DEAR HUXLEY,—Forgive my writing in pencil, as I can do so lying down. I have read Buffon: whole pages are laughably like mine. It is surprising how candid it makes one to see one's views in another man's words. I am rather ashamed of the whole affair, but not converted to a no-belief. What a kindness you have done me with your "vulpine sharpness." Nevertheless, there is a fundamental distinction between Buffon's views and mine. He does not suppose that each cell or atom of tissue throws off a little bud; but he supposes that the sap or blood includes his "organic molecules," *which are ready formed*, fit to nourish each organ, and when this is fully formed, they collect to form buds and the sexual elements. It is all rubbish to speculate as I have done; yet, if I ever have strength to publish my next book, I fear I shall not resist "Pangenesis," but I assure you I will put it humbly enough. The ordinary course of development of beings, such as the Echinodermata, in which new organs are formed at quite remote spots from the analogous previous parts, seems to me extremely difficult to reconcile on any view except the free diffusion in the parent of the germs or gemmules of each separate new organ: and so in cases of alternate generation. But I will not scribble any more. Hearty thanks to you, you best of critics and most learned man. . . . . .

[The letters now take up the history of the year 1866.]

C. Darwin to A. R. Wallace.

Down, July 5 [1866].

MY DEAR WALLACE,—I have been much interested by your letter, which is as clear as daylight. I fully agree with all that you say on the advantages of H. Spencer's excellent

© The Complete Work of Charles Darwin Online

expression of "the survival of the fittest."* This, however, had not occurred to me till reading your letter. It is, however, a great objection to this term that it cannot be used as a substantive governing a verb; and that this is a real objection I infer from H. Spencer continually using the words, natural selection. I formerly thought, probably in an exaggerated degree, that it was a great advantage to bring into connection natural and artificial selection; this indeed led me to use a term in common, and I still think it some advantage. I wish I had received your letter two months ago, for I would have worked in "the survival, &c.," often in the new edition of the 'Origin,' which is now almost printed off, and of which I will of course send you a copy. I will use the term in my next book on Domestic Animals, &c., from which, by the way, I plainly see that you expect *much* too much. The term Natural Selection has now been so largely used abroad and at home, that I doubt whether it could be given up, and with all its faults I should be sorry to see the attempt made. Whether it will be rejected must now depend "on the survival of the fittest." As in time the term must grow intelligible the objections to its use will grow weaker and weaker. I doubt whether the use of any term would have made the subject intelligible to some minds, clear as it is to others; for do we not see even to the present day Malthus on Population absurdly misunderstood? This reflection about Malthus has often comforted me when I have been vexed at the misstatement of my views. As for M. Janet,† he is a metaphysician, and such gentlemen are so acute that I think they often misunderstand common folk. Your criticism on the

* Extract from a letter of Mr. Wallace's, July 2, 1866: "The term 'survival of the fittest' is the plain expression of the fact; 'natural selection' is a metaphorical expression of it, and to a certain degree indirect and incorrect, since . . . Nature . . . does not so much select special varieties as exterminate the most unfavourable ones."

† This no doubt refers to Janet's 'Matérialisme Contemporaine.'

© The Complete Work of Charles Darwin Online

double sense * in which I have used Natural Selection is new to me and unanswerable; but my blunder has done no harm, for I do not believe that any one, excepting you, has ever observed it. Again, I agree that I have said too much about "favourable variations;" but I am inclined to think that you put the opposite side too strongly; if every part of every being varied, I do not think we should see the same end, or object, gained by such wonderfully diversified means.

I hope you are enjoying the country, and are in good health, and are working hard at your Malay Archipelago book, for I will always put this wish in every note I write to you, as some good people always put in a text. My health keeps much the same, or rather improves, and I am able to work some hours daily. With many thanks for your interesting letter,

Believe me, my dear Wallace, yours sincerely,

CH. DARWIN.

C. Darwin to J. D. Hooker.

Down, Aug. 30 [1866].

MY DEAR HOOKER,—I was very glad to get your note and the Notts. Newspaper. I have seldom been more pleased in my life than at hearing how successfully your lecture † went off. Mrs. H. Wedgwood sent us an account, saying that you read capitally, and were listened to with profound attention and great applause. She says, when your final

* "I find you use 'Natural Selection' in two senses; 1st, for the simple preservation of favourable and rejection of unfavourable variations, in which case it is equivalent to the 'survival of the fittest,'—and 2ndly, for the *effect* or *change* produced by this preservation."—Extract from Mr. Wallace's letter above quoted.

† At the Nottingham meeting of the British Association, Aug. 27, 1866. The subject of the lecture was 'Insular Floras.' See *Gardeners' Chronicle*, 1866.

© The Complete Work of Charles Darwin Online

allegory * began, "for a minute or two we were all mystified, and then came such bursts of applause from the audience. It was thoroughly enjoyed amid roars of laughter and noise, making a most brilliant conclusion."

I am rejoiced that you will publish your lecture, and felt sure that sooner or later it would come to this, indeed it would have been a sin if you had not done so. I am especially rejoiced as you give the arguments for occasional transport with such perfect fairness; these will now receive a fair share of attention, as coming from you, a professed botanist. Thanks also for Grove's address; as a whole it strikes me as very good and original, but I was disappointed in the part about Species; it dealt in such generalities that it would apply to any view or no view in particular. . . . .

And now farewell. I do most heartily rejoice at your success, and for Grove's sake at the brilliant success of the whole meeting.

Yours affectionately,
CHARLES DARWIN.

[The next letter is of interest, as giving the beginning of the connection which arose between my father and Professor Victor Carus. The translation referred to is the third German edition, made from the fourth English one. From this time forward Professor Carus continued to translate my father's books into German. The conscientious care with which this work was done was of material service, and I well remember the admiration (mingled with a tinge of vexation at his own shortcomings) with which my father used to receive the lists of oversights, &c., which Professor Carus dis-

* Sir Joseph Hooker allegorised the Oxford meeting of the British Association as the gathering of a tribe of savages who believed that the new moon was created afresh each month. The anger of the priests and medicine men at a certain heresy, according to which the new moon is but the offspring of the old one, is excellently given.

© The Complete Work of Charles Darwin Online

covered in the course of translation. The connection was not a mere business one, but was cemented by warm feelings of regard on both sides.]

C. Darwin to Victor Carus.

Down, November 10, 1866.

MY DEAR SIR,—I thank you for your extremely kind letter. I cannot express too strongly my satisfaction that you have undertaken the revision of the new edition, and I feel the honour which you have conferred on me. I fear that you will find the labour considerable, not only on account of the additions, but I suspect that Bronn's translation is very defective, at least I have heard complaints on this head from quite a large number of persons. It would be a great gratification to me to know that the translation was a really good one, such as I have no doubt you will produce. According to our English practice, you will be fully justified in entirely omitting Bronn's Appendix, and I shall be very glad of its omission. A new edition may be looked at as a new work. . . . . You could add anything of your own that you liked, and I should be much pleased. Should you make any additions or append notes, it appears to me that Nägeli, "Entstehung und Begriff," &c.,* would be worth noticing, as one of the most able pamphlets on the subject. I am, however, far from agreeing with him that the acquisition of certain characters which appear to be of no service to plants, offers any great difficulty, or affords a proof of some innate tendency in plants towards perfection. If you intend to notice this pamphlet, I should like to write hereafter a little more in detail on the subject.

. . . . I wish I had known, when writing my Historical

* 'Entstehung und Begriff der Naturhistorischen Art.' An Address given at a public meeting of the Royal Academy of Sciences at Munich, Mar. 28, 1865.

© The Complete Work of Charles Darwin Online

Sketch, that you had in 1853 published your views on the genealogical connection of past and present forms.

I suppose you have the sheets of the last English edition on which I marked with pencil all the chief additions, but many little corrections of style were not marked.

Pray believe that I feel sincerely grateful for the great service and honour which you do me by the present translation.

I remain, my dear Sir, yours very sincerely,

CHARLES DARWIN.

P.S.—I should be *very much* pleased to possess your photograph, and I send mine in case you should like to have a copy.

*C. Darwin to C. Nägeli.**

Down, June 12 [1866].

DEAR SIR,—I hope you will excuse the liberty which I take in writing to you. I have just read, though imperfectly, your 'Entstehung und Begriff,' and have been so greatly interested by it, that I have sent it to be translated, as I am a poor German scholar. I have just finished a new [4th] edition of my 'Origin,' which will be translated into German, and my object in writing to you is to say that if you should see this edition you would think that I had borrowed from you, without acknowledgment, two discussions on the beauty of flowers and fruit; but I assure you every word was printed off before I had opened your pamphlet. Should you like to possess a copy of either the German or English new edition, I should be proud to send one. I may add, with respect to the beauty of flowers, that I have already hinted the same views as you hold in my paper on Lythrum.

Many of your criticisms on my views are the best which I have met with, but I could answer some, at least to my own satisfaction; and I regret extremely that I had not read your

* Professor of Botany at Munich.

© The Complete Work of Charles Darwin Online

pamphlet before printing my new edition.* On one or two points, I think, you have a little misunderstood me, though I dare say I have not been cautious in expressing myself. The remark which has struck me most, is that on the position of the leaves not having been acquired through natural selection, from not being of any special importance to the plant. I well remember being formerly troubled by an analogous difficulty, namely, the position of the ovules, their anatropous condition, &c. It was owing to forgetfulness that I did not notice this difficulty in the 'Origin.' Although I can offer no explanation of such facts, and only hope to see that they may be explained, yet I hardly see how they support the doctrine of some law of necessary development, for it is not clear to me that a plant, with its leaves placed at some particular angle, or with its ovules in some particular position, thus stands higher than another plant. But I must apologise for troubling you with these remarks.

As I much wish to possess your photograph, I take the liberty of enclosing my own, and with sincere respect I remain, dear Sir,

Yours faithfully,
CH. DARWIN.

[I give a few extracts from letters of various dates showing my father's interest, alluded to in the last letter, in the problem of the arrangement of the leaves on the stems of plants. It may be added that Professor Schwendener of Berlin has successfully attacked the question in his 'Mechanische Theorie der Blattstellungen,' 1878.

To Dr. Falconer.

August 26 [1863].

"Do you remember telling me that I ought to study Phyllotaxy? well I have often wished you at the bottom of

* Nägeli's Essay is noticed in the 5th edition.

© The Complete Work of Charles Darwin Online

the sea; for I could not resist, and I muddled my brains with diagrams, &c., and specimens, and made out, as might have been expected, nothing. Those angles are a most wonderful problem and I wish I could see some one give a rational explanation of them."

To Dr. Asa Gray.

May 11 [1861].

"If you wish to save me from a miserable death, do tell me why the angles of $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{5}$, $\frac{3}{8}$, &c., series occur, and no other angles. It is enough to drive the quietest man mad. Did you and some mathematician * publish some paper on the subject? Hooker says you did; where is it?

To Dr. Asa Gray.

[May 31, 1863?]

"I have been looking at Nägeli's work on this subject, and am astonished to see that the angle is not always the same in young shoots when the leaf-buds are first distinguishable, as in full-grown branches. This shows, I think, that there must be some potent cause for those angles which do occur: I dare say there is some explanation as simple as that for the angles of the Bees-cells."

My father also corresponded with Dr. Hubert Airy and was interested in his views on the subject, published in the Royal Soc. Proceedings, 1873, p. 176.

We now return to the year 1866. In November, when the prosecution of Governor Eyre was dividing England into two bitterly opposed parties, he wrote to Sir J. D. Hooker:—

* Probably my father was thinking of Chauncey Wright's work on Phyllotaxy, in Gould's 'Astronomical Journal,' No. 99, 1856, and in the 'Mathematical Monthly,' 1859. These papers are mentioned in the Letters of Chauncey Wright.' Mr. Wright corresponded with my father on the subject.

© The Complete Work of Charles Darwin Online

"You will shriek at me when you hear that I have just subscribed to the Jamaica Committee." *

On this subject I quote from a letter of my brother's:—

"With respect to Governor Eyre's conduct in Jamaica, he felt strongly that J. S. Mill was right in prosecuting him. I remember one evening, at my Uncle's, we were talking on the subject, and as I happened to think it was too strong a measure to prosecute Governor Eyre for murder, I made some foolish remark about the prosecutors spending the surplus of the fund in a dinner. My father turned on me almost with fury, and told me, if those were my feelings, I had better go back to Southampton; the inhabitants having given a dinner to Governor Eyre on his landing, but with which I had had nothing to do." The end of the incident, as told by my brother, is so characteristic of my father that I cannot resist giving it, though it has no bearing on the point at issue. "Next morning at 7 o'clock, or so, he came into my bedroom and sat on my bed, and said that he had not been able to sleep, from the thought that he had been so angry with me, and after a few more kind words he left me."

The same restless desire to correct a disagreeable or incorrect impression is well illustrated in a passage which I quote from some notes by Rev. J. Brodie Innes:—

"Allied to the extreme carefulness of observation was his most remarkable truthfulness in all matters. On one occasion, when a parish meeting had been held on some disputed point of no great importance, I was surprised by a visit from Mr. Darwin at night. He came to say that, thinking over the debate, though what he had said was quite accurate, he thought I might have drawn an erroneous conclusion, and he would not sleep till he had explained it. I believe that if on any day some certain fact had come to his knowledge which contradicted his most cherished theories, he would have placed the fact on record for publication before he slept."

* He subscribed £10.

© The Complete Work of Charles Darwin Online

This tallies with my father's habits, as described by himself. When a difficulty or an objection occurred to him, he thought it of paramount importance to make a note of it instantly, because he found hostile facts to be especially evanescent.

The same point is illustrated by the following incident, for which I am indebted to Mr. Romanes :—

"I have always remembered the following little incident as a good example of Mr. Darwin's extreme solicitude on the score of accuracy. One evening at Down there was a general conversation upon the difficulty of explaining the evolution of some of the distinctively human emotions, especially those appertaining to the recognition of beauty in natural scenery. I suggested a view of my own upon the subject, which, depending upon the principle of association, required the supposition that a long line of ancestors should have inhabited regions, the scenery of which is now regarded as beautiful. Just as I was about to observe that the chief difficulty attaching to my hypothesis arose from feelings of the sublime (seeing that these are associated with awe, and might therefore be expected not to be agreeable), Mr. Darwin anticipated the remark, by asking how the hypothesis was to meet the case of these feelings. In the conversation which followed, he said the occasion in his own life, when he was most affected by the emotions of the sublime was when he stood upon one of the summits of the Cordillera, and surveyed the magnificent prospect all around. It seemed, as he quaintly observed, as if his nerves had become fiddle-strings, and had all taken to rapidly vibrating. This remark was only made incidentally, and the conversation passed into some other branch. About an hour afterwards Mr. Darwin retired to rest, while I sat up in the smoking-room with one of his sons. We continued smoking and talking for several hours, when at about one o'clock in the morning the door gently opened and Mr. Darwin appeared, in his slippers and

© The Complete Work of Charles Darwin Online

dressing-gown. As nearly as I can remember, the following are the words he used :—

"'Since I went to bed I have been thinking over our conversation in the drawing-room, and it has just occurred to me that I was wrong in telling you I felt most of the sublime when on the top of the Cordillera; I am quite sure that I felt it even more when in the forests of Brazil. I thought it best to come and tell you this at once in case I should be putting you wrong. I am sure now that I felt most sublime in the forests.'

"This was all he had come to say, and it was evident that he had come to do so, because he thought that the fact of his feeling 'most sublime in forests' was more in accordance with the hypothesis which we had been discussing, than the fact which he had previously stated. Now, as no one knew better than Mr. Darwin the difference between a speculation and a fact, I thought this little exhibition of scientific conscientiousness very noteworthy, where the only question concerned was of so highly speculative a character. I should not have been so much impressed if he had thought that by his temporary failure of memory he had put me on a wrong scent in any matter of fact, although even in such a case he is the only man I ever knew who would care to get out of bed at such a time of night in order to make the correction immediately, instead of waiting till next morning. But as the correction only had reference to a flimsy hypothesis, I certainly was very much impressed by this display of character."]

C. Darwin to J. D. Hooker.

Down, December 10 [1866].

. . . . I have now read the last No. of H. Spencer.* I do not know whether to think it better than the previous number, but it is wonderfully clever, and I dare say mostly true. I feel rather mean when I read him: I could bear, and rather enjoy

* 'Principles of Biology.'

© The Complete Work of Charles Darwin Online

feeling that he was twice as ingenious and clever as myself, but when I feel that he is about a dozen times my superior, even in the master art of wriggling, I feel aggrieved. If he had trained himself to observe more, even if at the expense, by the law of balancement, of some loss of thinking power, he would have been a wonderful man.

. . . . I am *heartily* glad you are taking up the Distribution of Plants in New Zealand, and suppose it will make part of your new book. Your view, as I understand it, that New Zealand subsided and formed two or more small islands, and then rose again, seems to me extremely probable. . . . . When I puzzled my brains about New Zealand, I remember I came to the conclusion, as indeed I state in the 'Origin,' that its flora, as well as that of other southern lands, had been tinctured by an Antarctic flora, which must have existed before the Glacial period. I concluded that New Zealand never could have been closely connected with Australia, though I supposed it had received some few Australian forms by occasional means of transport. Is there any reason to suppose that New Zealand could have been more closely connected with South Australia during the Glacial period, when the Eucalypti, &c., might have been driven further North? Apparently there remains only the line, which I think you suggested, of sunken islands from New Caledonia. Please remember that the Edwardsia was certainly drifted there by the sea.

I remember in old days speculating on the amount of life, *i.e.* of organic chemical change, at different periods. There seems to me one very difficult element in the problem, namely, the state of development of the organic beings at each period, for I presume that a Flora and Fauna of cellular cryptogamic plants, of Protozoa and Radiata would lead to much less chemical change than is now going on. But I have scribbled enough.

Yours affectionately,

CH. DARWIN.

© The Complete Work of Charles Darwin Online

[The following letter is in acknowledgment of Mr. Rivers' * reply to an earlier letter in which my father had asked for information on bud-variation. It may find a place here in illustration of the manner of my father's intercourse with those "whose avocations in life had to do with the rearing or use of living things" †—an intercourse which bore such good fruit in the 'Variation of Animals and Plants.' Mr. Dyer has some excellent remarks on the unexpected value thus placed on the apparently trivial facts disinterred from weekly journals, or amassed by correspondence. He adds: "Horticulturists who had . . . moulded plants almost at their will, at the impulse of taste or profit, were at once amazed and charmed to find that they had been doing scientific work, and helping to establish a great theory."]

C. Darwin to T. Rivers.

Down, December 28, [1866?]

MY DEAR SIR,—Permit me to thank you cordially for your most kind letter. For years I have read with interest every scrap which you have written in periodicals, and abstracted in MS. your book on Roses, and several times I thought I would write to you, but did not know whether you would think me too intrusive. I shall, indeed, be truly obliged for any information you can supply me on bud-variation or sports. When any extra difficult points occur to me in my present subject (which is a mass of difficulties), I will apply to you, but I will not be unreasonable. It is most true what you say that any one to study well the physiology of the life of plants, ought to have under his eye a multitude of plants. I have endeavoured to do what I can by comparing statements by many writers and observing what I could myself. Unfortunately few have

* The late Mr. Rivers was an eminent horticulturist and writer on horticulture.

† Mr. Dyer in 'Charles Darwin.' —*Nature Series*, 1882, p. 39.

© The Complete Work of Charles Darwin Online

observed like you have done. As you are so kind, I will mention one other point on which I am collecting facts; namely, the effect produced on the stock by the graft; thus, it is *said*, that the purple-leaved filbert affects the leaves of the common hazel on which it is grafted (I have just procured a plant to try), so variegated jessamine is *said* to affect its stock. I want these facts partly to throw light on the marvellous laburnums, Adami-trifacial oranges, &c. That laburnum case seems one of the strangest in physiology. I have now growing splendid, *fertile*, yellow laburnums (with a long raceme like the so-called Waterer's laburnum) from seed of yellow flowers on the *C. Adami*. To a man like myself, who is compelled to live a solitary life, and sees few persons, it is no slight satisfaction to hear that I have been able at all [to] interest by my books observers like yourself.

As I shall publish on my present subject, I presume, within a year, it will be of no use your sending me the shoots of peaches and nectarines which you so kindly offer; I have recorded your facts.

Permit me again to thank you cordially; I have not often in my life received a kinder letter.

My dear Sir, yours sincerely,

CH. DARWIN.

© The Complete Work of Charles Darwin Online

CHAPTER II.

THE PUBLICATION OF THE 'VARIATION OF ANIMALS AND PLANTS UNDER DOMESTICATION.'

JANUARY 1867, TO JUNE 1868.

AT the beginning of the year 1867 he was at work on the final chapter—"Concluding Remarks" of the 'Variation of Animals and Plants under Domestication,' which was begun after the rest of the MS. had been sent to the printers in the preceding December. With regard to the publication of the book he wrote to Mr. Murray, on January 3:—

"I cannot tell you how sorry I am to hear of the enormous size of my book.* I fear it can never pay. But I cannot shorten it now; nor, indeed, if I had foreseen its length, do I see which parts ought to have been omitted.

"If you are afraid to publish it, say so at once, I beg you, and I will consider your note as cancelled. If you think fit, get any one whose judgment you rely on, to look over some of the more legible chapters, namely, the Introduction, and on dogs and plants, the latter chapters being, in my opinion, the dullest in the book. . . . The list of chapters, and the inspection of a few here and there, would give a good judge

* On January 9 he wrote to Sir J. D. Hooker: "I have been these last few days vexed and annoyed to a foolish degree by hearing that my MS. on Dom. An. and Cult. Plants will make 2 vols., both bigger than the 'Origin.' The volumes will have to be full-sized octavo, so I have written to Murray to suggest details to be printed in small type. But I feel that the size is quite ludicrous in relation to the subject. I am ready to swear at myself and at every fool who writes a book."

© The Complete Work of Charles Darwin Online

a fair idea of the whole book. Pray do not publish blindly, as it would vex me all my life if I led you to heavy loss."

Mr. Murray referred the MS. to a literary friend, and, in spite of a somewhat adverse opinion, willingly agreed to publish the book. My father wrote:—

"Your note has been a great relief to me. I am rather alarmed about the verdict of your friend, as he is not a man of science. I think if you had sent the 'Origin' to an unscientific man, he would have utterly condemned it. I am, however, *very glad* that you have consulted any one on whom you can rely.

"I must add, that my 'Journal of Researches' was seen in MS. by an eminent semi-scientific man, and was pronounced unfit for publication."

The proofs were begun in March, and the last revise was finished on November 15th, and during this period the only intervals of rest were two visits of a week each at his brother Erasmus's house in Queen Anne Street. He notes in his Diary:—

"I began this book [in the] beginning of 1860 (and then had some MS.), but owing to interruptions from my illness, and illness of children; from various editions of the 'Origin,' and Papers, especially Orchis book and Tendrils, I have spent four years and two months over it."

The edition of 'Animals and Plants' was of 1500 copies, and of these 1260 were sold at Mr. Murray's autumnal sale, but it was not published until January 30, 1868. A new edition of 1250 copies was printed in February of the same year.

In 1867 he received the distinction of being made a knight of the Prussian Order "Pour le Mérite." * He seems

* The Order "Pour le Mérite" was founded in 1740 by Frederick II. by the re-christening of an "Order of Generosity," founded in 1665. It was at one time strictly military, having been previously both civil and military, and in 1840 the Order was again opened to civilians. The order consists of thirty members of German extraction, but distinguished foreigners are admitted to a kind of extraordinary member-

© The Complete Work of Charles Darwin Online

not to have known how great the distinction was, for in June 1868 he wrote to Sir J. D. Hooker:—

"What a man you are for sympathy. I was made "Eques" some months ago, but did not think much about it. Now, by Jove, we all do; but you, in fact, have knighted me."

The letters may now take up the story.]

C. Darwin to J. D. Hooker.

Down, February 8 [1867].

MY DEAR HOOKER,—I am heartily glad that you have been offered the Presidentship of the British Association, for it is a great honour, and as you have so much work to do, I am equally glad that you have declined it. I feel, however, convinced that you would have succeeded very well; but if I fancy myself in such a position, it actually makes my blood run cold. I look back with amazement at the skill and taste with which the Duke of Argyll made a multitude of little speeches at Glasgow. By the way, I have not seen the Duke's book,* but I formerly thought that some of the articles which appeared in periodicals were very clever, but not very profound. One of these was reviewed in the *Saturday Review* † some years ago, and the fallacy of some main argument was admirably exposed, and I sent the article to you, and you agreed strongly with it. . . . There was the other day a rather good review of the Duke's book in the

ship. Faraday, Herschel, and Thomas Moore have belonged to it in this way. From the thirty members a chancellor is elected by the king (the first officer of this kind was Alexander v. Humboldt); and it is the duty of the chancellor to notify a vacancy in the Order to the remainder of the thirty, who then elect by vote the new member —but the king has technically the appointment in his own hands.

* 'The Reign of Law,' 1867.

† *Sat. Review*, Nov. 15, 1862, 'The *Edinburgh Review* on the Supernatural.' Written by my cousin, Mr. Henry Parker.

© The Complete Work of Charles Darwin Online

Spectator, and with a new explanation, either by the Duke or the reviewer (I could not make out which), of rudimentary organs, namely, that economy of labour and material was a great guiding principle with God (ignoring waste of seed and of young monsters, &c.), and that making a new plan for the structure of animals was thought, and thought was labour, and therefore God kept to a uniform plan, and left rudiments. This is no exaggeration. In short, God is a man, rather cleverer than us. . . . I am very much obliged for the *Nation* (returned by this post); it is *admirably* good. You say I always guess wrong, but I do not believe any one, except Asa Gray, could have done the thing so well. I would bet even, or three to two, that it is Asa Gray, though one or two passages staggered me.

I finish my book on 'Domestic Animals,' &c., by a single paragraph, answering, or rather throwing doubt, in so far as so little space permits, on Asa Gray's doctrine that each variation has been specially ordered or led along a beneficial line. It is foolish to touch such subjects, but there have been so many allusions to what I think about the part which God has played in the formation of organic beings,* that I thought it shabby to evade the question. . . . I have even received several letters on the subject. . . . I overlooked your sentence about Providence, and suppose I treated it as Buckland did his own theology, when his Bridgewater Treatise was read aloud to him for correction. . . .

* Prof. Judd allows me to quote from some notes which he has kindly given me: — "Lyell once told me that he had frequently been asked if Darwin was not one of the most unhappy of men, it being suggested that his outrage upon public opinion should have filled him with remorse." Sir Charles must have been able, I think, to give a conclusive answer on this point. Professor Judd continues :—

"I made a note of this and other conversations of Lyell's at the time. At the present time such statements must appear strange to any one who does not recollect the revolution in opinion which has taken place during the last 23 years [1882]."

© The Complete Work of Charles Darwin Online

[The following letter, from Mrs. Boole, is one of those referred to in the last letter to Sir J. D. Hooker:]

DEAR SIR,—Will you excuse my venturing to ask you a question, to which no one's answer but your own would be quite satisfactory?

Do you consider the holding of your theory of Natural Selection, in its fullest and most unreserved sense, to be inconsistent—I do not say with any particular scheme of theological doctrine—but with the following belief, namely:—

That knowledge is given to man by the direct inspiration of the Spirit of God.

That God is a personal and Infinitely good Being.

That the effect of the action of the Spirit of God on the brain of man is especially a moral effect.

And that each individual man has within certain limits a power of choice as to how far he will yield to his hereditary animal impulses, and how far he will rather follow the guidance of the Spirit, who is educating him into a power of resisting those impulses in obedience to moral motives?

The reason why I ask you is this: my own impression has always been, not only that your theory was perfectly *compatible* with the faith to which I have just tried to give expression, but that your books afforded me a clue which would guide me in applying that faith to the solution of certain complicated psychological problems which it was of practical importance to me as a mother to solve. I felt that you had supplied one of the missing links—not to say *the* missing link—between the facts of science and the promises of religion. Every year's experience tends to deepen in me that impression.

But I have lately read remarks on the probable bearing of your theory on religious and moral questions which have perplexed and pained me sorely. I know that the persons who make such remarks must be cleverer and wiser than

© The Complete Work of Charles Darwin Online

myself. I cannot feel sure that they are mistaken, unless you will tell me so. And I think—I cannot know for certain—but I *think*—that if I were an author, I would rather that the humblest student of my works should apply to me directly in a difficulty, than that she should puzzle too long over adverse and probably mistaken or thoughtless criticisms.

At the same time I feel that you have a perfect right to refuse to answer such questions as I have asked you. Science must take her path, and Theology hers, and they will meet when and where and how God pleases, and you are in no sense responsible for it if the meeting-point should still be very far off. If I receive no answer to this letter I shall infer nothing from your silence, except that you felt I had no right to make such inquiries of a stranger.

[My father replied as follows:]

Down, December 14, 1866.

DEAR MADAM,—It would have gratified me much if I could have sent satisfactory answers to your questions, or, indeed, answers of any kind. But I cannot see how the belief that all organic beings, including man, have been genetically derived from some simple being, instead of having been separately created, bears on your difficulties. These, as it seems to me, can be answered only by widely different evidence from science, or by the so-called "inner consciousness." My opinion is not worth more than that of any other man who has thought on such subjects, and it would be folly in me to give it. I may, however, remark that it has always appeared to me more satisfactory to look at the immense amount of pain and suffering in this world as the inevitable result of the natural sequence of events, *i.e.* general laws, rather than from the direct intervention of God, though I am aware this is not logical with reference to an omniscient Deity. Your last question seems to resolve itself into the problem of free will and necessity, which has been found by most persons insoluble.

© The Complete Work of Charles Darwin Online

I sincerely wish that this note had not been as utterly valueless as it is. I would have sent full answers, though I have little time or strength to spare, had it been in my power.

I have the honour to remain, dear Madam,

Yours very faithfully,

CHARLES DARWIN.

P.S.—I am grieved that my views should incidentally have caused trouble to your mind, but I thank you for your judgment, and honour you for it, that theology and science should each run its own course, and that in the present case I am not responsible if their meeting-point should still be far off.

[The next letter discusses the 'Reign of Law,' referred to a few pages back :]

C. Darwin to C. Lyell.

Down, June 1 [1867].

. . . I am at present reading the Duke, and am *very much* interested by him ; yet I cannot but think, clever as the whole is, that parts are weak, as when he doubts whether each curvature of the beak of humming-birds is of service to each species. He admits, perhaps too fully, that I have shown the use of each little ridge and shape of each petal in orchids, and how strange he does not extend the view to humming-birds. Still odder, it seems to me, all that he says on beauty, which I should have thought a nonentity, except in the mind of some sentient being. He might have as well said that love existed during the secondary or Palæozoic periods. I hope you are getting on with your book better than I am with mine, which kills me with the labour of correcting, and is intolerably dull, though I did not think so when I was writing it. A naturalist's life would be a happy one if he had only to observe, and never to write.

© The Complete Work of Charles Darwin Online

We shall be in London for a week in about a fortnight's time, and I shall enjoy having a breakfast talk with you.

Yours affectionately,

C. DARWIN.

[The following letter refers to the new and improved translation of the 'Origin,' undertaken by Professor Carus :]

C. Darwin to J. Victor Carus.

Down, February 17 [1867].

MY DEAR SIR,—I have read your preface with care. It seems to me that you have treated Bronn with complete respect and great delicacy, and that you have alluded to your own labour with much modesty. I do not think that any of Bronn's friends can complain of what you say and what you have done. For my own sake, I grieve that you have not added notes, as I am sure that I should have profited much by them; but as you have omitted Bronn's objections, I believe that you have acted with excellent judgment and fairness in leaving the text without comment to the independent verdict of the reader. I heartily congratulate you that the main part of your labour is over; it would have been to most men a very troublesome task, but you seem to have indomitable powers of work, judging from those two wonderful and most useful volumes on zoological literature* edited by you, and which I never open without surprise at their accuracy, and gratitude for their usefulness. I cannot sufficiently tell you how much I rejoice that you were persuaded to superintend the translation of the present edition of my book, for I have now the great satisfaction of knowing that the German public can judge fairly of its merits and demerits. . . . .

With my cordial and sincere thanks, believe me,

My dear Sir, yours very faithfully,

CH. DARWIN.

* 'Bibliotheca Zoologica,' 1861.

© The Complete Work of Charles Darwin Online

[The earliest letter which I have seen from my father to Professor Haeckel, was written in 1865, and from that time forward they corresponded (though not, I think, with any regularity) up to the end of my father's life. His friendship with Haeckel was not merely growth of correspondence, as was the case with some others, for instance, Fritz Müller. Haeckel paid more than one visit to Down, and these were thoroughly enjoyed by my father. The following letter will serve to show the strong feeling of regard which he entertained for his correspondent—a feeling which I have often heard him emphatically express, and which was warmly returned. The book referred to is Haeckel's 'Generelle Morphologie,' published in 1866, a copy of which my father received from the author in January 1867.

Dr. E. Krause * has given a good account of Professor Haeckel's services to the cause of Evolution. After speaking of the lukewarm reception which the 'Origin' met with in Germany on its first publication, he goes on to describe the first adherents of the new faith as more or less popular writers, not especially likely to advance its acceptance with the professorial or purely scientific world. And he claims for Haeckel that it was his advocacy of Evolution in his 'Radiolaria' (1862), and at the "Versammlung" of Naturalists at Stettin in 1863, that placed the Darwinian question for the first time publicly before the forum of German science, and his enthusiastic propagandism that chiefly contributed to its success.

Mr. Huxley, writing in 1869, paid a high tribute to Professor Haeckel as the Coryphæus of the Darwinian movement in Germany. Of his 'Generelle Morphologie,' "an attempt to work out the practical applications" of the doctrine of Evolution to their final results, he says that it has the "force and suggestiveness, and . . . systematising power of Oken without his extravagance." Professor Huxley also

* 'Charles Darwin und sein Verhältniss zu Deutschland,' 1885.

© The Complete Work of Charles Darwin Online

testifies to the value of Haeckel's 'Schöpfungs-Geschichte' as an exposition of the 'Generelle Morphologie' "for an educated public."

Again, in his 'Evolution in Biology,' * Mr. Huxley wrote: "Whatever hesitation may, not unfrequently, be felt by less daring minds, in following Haeckel in many of his speculations, his attempt to systematise the doctrine of Evolution, and to exhibit its influence as the central thought of modern biology, cannot fail to have a far-reaching influence on the progress of science."

In the following letter my father alludes to the somewhat fierce manner in which Professor Haeckel fought the battle of 'Darwinismus,' and on this subject Dr. Krause has some good remarks (p. 162). He asks whether much that happened in the heat of the conflict might not well have been otherwise, and adds that Haeckel himself is the last man to deny this. Nevertheless he thinks that even these things may have worked well for the cause of Evolution, inasmuch as Haeckel "concentrated on himself by his 'Ursprung des Menschen-Geschlechts,' his 'Generelle Morphologie,' and 'Schöpfungs-Geschichte,' all the hatred and bitterness which Evolution excited in certain quarters," so that, "in a surprisingly short time it became the fashion in Germany that Haeckel alone should be abused, while Darwin was held up as the ideal of forethought and moderation."]

C. Darwin to E. Haeckel.

Down, May 21, 1867.

DEAR HAECKEL.—Your letter of the 18th has given me great pleasure, for you have received what I said in the most kind and cordial manner. You have in part taken what I said much stronger than I had intended. It never occurred to me for a moment to doubt that your work, with the whole

* An article in the 'Encyclopædia Britannica,' 9th edit., reprinted in 'Science and Culture,' 1881, p. 298.

© The Complete Work of Charles Darwin Online

subject so admirably and clearly arranged, as well as fortified by so many new facts and arguments, would not advance our common object in the highest degree. All that I think is that you will excite anger, and that anger so completely blinds every one, that your arguments would have no chance of influencing those who are already opposed to our views. Moreover, I do not at all like that you, towards whom I feel so much friendship, should unnecessarily make enemies, and there is pain and vexation enough in the world without more being caused. But I repeat that I can feel no doubt that your work will greatly advance our subject, and I heartily wish it could be translated into English, for my own sake and that of others. With respect to what you say about my advancing too strongly objections against my own views, some of my English friends think that I have erred on this side; but truth compelled me to write what I did, and I am inclined to think it was good policy. The belief in the descent theory is slowly spreading in England,* even amongst those who can give no reason for their belief. No body of men were at first so much opposed to my views as the members of the London Entomological Society, but now I am assured that, with the exception of two or three old men, all the members concur with me to a certain extent. It has been a great disappointment to me that I have never received your long letter written to me from the Canary Islands. I am rejoiced to hear that your tour, which seems to have been a most interesting one, has done your health much good. I am working away at my new book, but make very slow progress, and the work tries my health, which is much the same as when you were here.

* In October 1867 he wrote to Mr. Wallace:—"Mr. Warrington has lately read an excellent and spirited abstract of the 'Origin' before the Victoria Institute, and as this is a most orthodox body, he has gained the name of the Devil's Advocate. The discussion which followed during three consecutive meetings is very rich from the nonsense talked. If you would care to see the number I could send it you."

© The Complete Work of Charles Darwin Online

Victor Carus is going to translate it, but whether it is worth translation, I am rather doubtful. I am very glad to hear that there is some chance of your visiting England this autumn, and all in this house will be delighted to see you here.

Believe me, my dear Haeckel,

Yours very sincerely,

CHARLES DARWIN.

C. Darwin to F. Müller.

Down, July 31 [1867].

MY DEAR SIR,—I received a week ago your letter of June 2, full as usual of valuable matter and specimens. It arrived at exactly the right time, for I was enabled to give a pretty full abstract of your observations on the plant's own pollen being poisonous. I have inserted this abstract in the proof-sheets in my chapter on sterility, and it forms the most striking part of my whole chapter.* I thank you very sincerely for the most interesting observations, which, however, I regret that you did not publish independently. I have been forced to abbreviate one or two parts more than I wished . . . Your letters always surprise me, from the number of points to which you attend. I wish I could make my letters of any interest to you, for I hardly ever see a naturalist, and live as retired a life as you in Brazil. With respect to mimetic plants, I remember Hooker many years ago saying he believed that there were many, but I agree with you that it would be most difficult to distinguish between mimetic resemblance and the effects of peculiar conditions. Who can say to which of these causes to attribute the several plants with heath-like foliage at the Cape of Good Hope? Is it not also a difficulty that quadrupeds appear to recognise plants more by their [scent] than their appearance?

* In 'The Variation of Animals and Plants.'

© The Complete Work of Charles Darwin Online

What I have just said reminds me to ask you a question. Sir J. Lubbock brought me the other day what appears to be a terrestrial Planaria (the first ever found in the northern hemisphere) and which was coloured exactly like our dark-coloured slugs. Now slugs are not devoured by birds, like the shell-bearing species, and this made me remember that I found the Brazilian Planariæ actually together with striped Vaginuli which I believe were similarly coloured. Can you throw any light on this? I wish to know, because I was puzzled some months ago how it would be possible to account for the bright colours of the Planariæ in reference to sexual selection. By the way, I suppose they are hermaphrodites.

Do not forget to aid me, if in your power, with answers to *any* of my questions on expression, for the subject interests me greatly. With cordial thanks for your never-failing kindness, believe me,

Yours very sincerely,
CHARLES DARWIN.

C. Darwin to C. Lyell.

Down, July 18 [1867].

MY DEAR LYELL,—Many thanks for your long letter. I am sorry to hear that you are in despair about your book; * I well know that feeling, but am now getting out of the lower depths. I shall be very much pleased, if you can make the least use of my present book, and do not care at all whether it is published before yours. Mine will appear towards the end of November of this year; you speak of yours as not coming out till November, 1868, which I hope may be an error. There is nothing about Man in my book which can interfere with you, so I will order all the completed clean sheets to be sent (and others as soon as ready) to you, but please observe you will not care for the first volume, which is a mere record

* The 2nd volume of the 10th edit. of the 'Principles.'

© The Complete Work of Charles Darwin Online

of the amount of variation; but I hope the second will be somewhat more interesting. Though I fear the whole must be dull.

I rejoice from my heart that you are going to speak out plainly about species. My book about Man, if published, will be short, and a large portion will be devoted to sexual selection, to which subject I alluded in the 'Origin' as bearing on Man. . . .

C. Darwin to C. Lyell.

Down, August 22 [1867].

MY DEAR LYELL,—I thank you cordially for your last two letters. The former one did me *real* good, for I had got so wearied with the subject that I could hardly bear to correct the proofs,* and you gave me fresh heart. I remember thinking that when you came to the Pigeon chapter you would pass it over as quite unreadable. Your last letter has interested me in very many ways, and I have been glad to hear about those horrid unbelieving Frenchmen. I have been particularly pleased that you have noticed Pangenesis. I do not know whether you ever had the feeling of having thought so much over a subject that you had lost all power of judging it. This is my case with Pangenesis (which is 26 or 27 years old), but I am inclined to think that if it be admitted as a probable hypothesis it will be a somewhat important step in Biology.

I cannot help still regretting that you have ever looked at the slips, for I hope to improve the whole a good deal. It is surprising to me, and delightful, that you should care in the least about the plants. Altogether you have given me one of the best cordials I ever had in my life, and I heartily thank you. I despatched this morning the French edition.† The

* The proofs of 'Animals and Plants,' which Lyell was then reading.

† Of the 'Origin.' It appears that my father was sending a copy of the French edition to Sir Charles. The introduction was by Mdlle. Royer, who translated the book.

© The Complete Work of Charles Darwin Online

introduction was a complete surprise to me, and I dare say has injured the book in France; nevertheless . . . it shows, I think, that the woman is uncommonly clever. Once again many thanks for the renewed courage with which I shall attack the horrid proof-sheets.

Yours affectionately,
CHARLES DARWIN.

P.S.—A Russian who is translating my new book into Russian has been here, and says you are immensely read in Russia, and many editions—how many I forget. Six editions of Buckle and four editions of the 'Origin.'

C. Darwin to Asa Gray.

Down, October 16 [1867].

MY DEAR GRAY,—I send by this post clean sheets of Vol. I. up to p. 336, and there are only 411 pages in this vol. I am *very* glad to hear that you are going to review my book; but if the *Nation* * is a newspaper I wish it were at the bottom of the sea, for I fear that you will thus be stopped reviewing me in a scientific journal. The first volume is all details, and you will not be able to read it; and you must remember that the chapters on plants are written for naturalists who are not botanists. The last chapter in Vol. I. is, however, I think, a curious compilation of facts; it is on bud-variation. In Vol. II. some of the chapters are more interesting; and I shall be very curious to hear your verdict on the chapter on close inter-breeding. The chapter on what I call Pangenesis will be called a mad dream, and I shall be pretty well satisfied if you think it a dream worth publishing; but at the bottom of my own mind I think it contains a great truth. I finish my book with a semi-theological paragraph, in which I quote and differ from you; what you will think of it, I know not. . . .

* The book was reviewed by Dr. Gray in the *Nation*, Mar. 19, 1868.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, November 17 [1867].

MY DEAR HOOKER,—Congratulate me, for I have finished the last revise of the last sheet of my book. It has been an awful job : seven and a half months correcting the press : the book, from much small type, does not look big, but is really very big. I have had hard work to keep up to the mark, but during the last week only few revises came, so that I have rested and feel more myself. Hence, after our long mutual silence, I enjoy myself by writing a note to you, for the sake of exhaling, and hearing from you. On account of the index,* I do not suppose that you will receive your copy till the middle of next month. I shall be intensely anxious to hear what you think about Pangenesis; though I can see how fearfully imperfect, even in mere conjectural conclusions, it is; yet it has been an infinite satisfaction to me somehow to connect the various large groups of facts, which I have long considered, by an intelligible thread. I shall not be at all surprised if you attack it and me with unparalleled ferocity. It will be my endeavour to do as little as possible for some time, but [I] shall soon prepare a paper or two for the Linnean Society. In a short time we shall go to London for ten days, but the time is not yet fixed. Now I have told you a deal about myself, and do let me hear a good deal about your own past and future doings. Can you pay us a visit, early in December? . . . . I have seen no one for an age, and heard no news.

. . . About my book I will give you a bit of advice. Skip the *whole* of Vol. I., except the last chapter (and that need only be skimmed) and skip largely in the 2nd volume; and then you will say it is a very good book.

* The index was made by Mr. W. S. Dallas; I have often heard my father express his admiration of this excellent piece of work.

© The Complete Work of Charles Darwin Online

1868.

['The Variation of Animals and Plants' was, as already mentioned, published on January 30, 1868, and on that day he sent a copy to Fritz Müller, and wrote to him :—

"I send by this post, by French packet, my new book, the publication of which has been much delayed. The greater part, as you will see, is not meant to be read ; but I should very much like to hear what you think of 'Pangenesis,' though I fear it will appear to *every one* far too speculative."]

C. Darwin to J. D. Hooker.

February 3 [1868].

. . . I am very much pleased at what you say about my Introduction ; after it was in type I was as near as possible cancelling the whole. I have been for some time in despair about my book, and if I try to read a few pages I feel fairly nauseated, but do not let this make you praise it ; for I have made up my mind that it is not worth a fifth part of the enormous labour it has cost me. I assure you that all that is worth your doing (if you have time for so much) is glancing at Chapter VI., and reading parts of the later chapters. The facts on self-impotent plants seem to me curious, and I have worked out to my own satisfaction the good from crossing and evil from interbreeding. I did read Pangenesis the other evening, but even this, my beloved child, as I had fancied, quite disgusted me. The devil take the whole book ; and yet now I am at work again as hard as I am able. It is really a great evil that from habit I have pleasure in hardly anything except Natural History, for nothing else makes me forget my ever-recurrent uncomfortable sensations. But I must not howl any more, and the critics may say what they like ; I did my best, and man can do no more. What a splendid pursuit Natural History would be if it was all observing and no writing ! . . . .

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, February 10 [1868].

MY DEAR HOOKER,—What is the good of having a friend, if one may not boast to him? I heard yesterday that Murray has sold in a week the whole edition of 1500 copies of my book, and the sale so pressing that he has agreed with Clowes to get another edition in fourteen days! This has done me a world of good, for I had got into a sort of dogged hatred of my book. And now there has appeared a review in the *Pall Mall* which has pleased me excessively, more perhaps than is reasonable. I am quite content, and do not care how much I may be pitched into. If by any chance you should hear who wrote the article in the *Pall Mall*, do please tell me; it is some one who writes capitally, and who knows the subject. I went to luncheon on Sunday, to Lubbock's, partly in hopes of seeing you, and, be hanged to you, you were not there.

Your cock-a-hoop friend,

C. D.

[Independently of the favourable tone of the able series of notices in the *Pall Mall Gazette* (Feb. 10, 15, 17, 1868), my father may well have been gratified by the following passages:—

"We must call attention to the rare and noble calmness with which he expounds his own views, undisturbed by the heats of polemical agitation which those views have excited, and persistently refusing to retort on his antagonists by ridicule, by indignation, or by contempt. Considering the amount of vituperation and insinuation which has come from the other side, this forbearance is supremely dignified."

And again in the third notice, Feb. 17:—

"Nowhere has the author a word that could wound the most sensitive self-love of an antagonist; nowhere does he, in text or note, expose the fallacies and mistakes of brother investigators . . . but while abstaining from impertinent censure,

© The Complete Work of Charles Darwin Online

he is lavish in acknowledging the smallest debts he may owe; and his book will make many men happy."

I am indebted to Messrs. Smith & Elder for the information that these articles were written by Mr. G. H. Lewes.]

C. Darwin to J. D. Hooker.

Down, February 23 [1868].

MY DEAR HOOKER,—I have had almost as many letters to write of late as you can have, viz. from 8 to 10 per diem, chiefly getting up facts on sexual selection, therefore I have felt no inclination to write to you, and now I mean to write solely about my book for my own satisfaction, and not at all for yours. The first edition was 1500 copies, and now the second is printed off; sharp work. Did you look at the review in the *Athenæum*,* showing profound contempt of me? . . . It is a shame that he should have said that I have taken much from Pouchet, without acknowledgment; for I took literally nothing, there being nothing to take. There is a capital review in the *Gardeners' Chronicle*, which will sell the book if anything will.

* *Athenæum*, February 15, 1868. My father quoted Pouchet's assertion that "variation under domestication throws no light on the natural modification of species." The reviewer quotes the end of a passage in which my father declares that he can see no force in Pouchet's arguments, or rather assertions, and then goes on: "We are sadly mistaken if there are not clear proofs in the pages of the book before us that, on the contrary, Mr. Darwin has perceived, felt, and yielded to the force of the arguments or assertions of his French antagonist." The following may serve as samples of the rest of the review:—

"Henceforth the rhetoricians will have a better illustration of anti-climax than the mountain which brought forth a mouse, . . . in the discoverer of the origin of species, who tried to explain the variation of pigeons!

"A few summary words. On the 'Origin of Species' Mr. Darwin has nothing, and is never likely to have anything, to say; but on the vastly important subject of inheritance, the transmission of peculiarities once acquired through successive generations, this work is a valuable store-house of facts for curious students and practical breeders."

© The Complete Work of Charles Darwin Online

I don't quite see whether I or the writer is in a muddle about man *causing* variability. If a man drops a bit of iron into sulphuric acid he does not cause the affinities to come into play, yet he may be said to make sulphate of iron. I do not know how to avoid ambiguity.

After what the *Pall Mall Gazette* and the *Chronicle* have said, I do not care a d—.

I fear Pangenesis is stillborn; Bates says he has read it twice, and is not sure that he understands it. H. Spencer says the view is quite different from his (and this is a great relief to me, as I feared to be accused of plagiarism, but utterly failed to be sure what he meant, so thought it safest to give my view as almost the same as his), and he says he is not sure he understands it. . . . Am I not a poor devil? yet I took such pains, I must think that I expressed myself clearly. Old Sir H. Holland says he has read it twice, and thinks it very tough; but believes that sooner or later "some view akin to it" will be accepted.

You will think me very self-sufficient, when I declare that I feel *sure* if Pangenesis is now stillborn it will, thank God, at some future time reappear, begotten by some other father, and christened by some other name.

Have you ever met with any tangible and clear view of what takes place in generation, whether by seeds or buds, or how a long-lost character can possibly reappear; or how the male element can possibly affect the mother plant, or the mother animal, so that her future progeny are affected? Now all these points and many others are connected together, whether truly or falsely is another question, by Pangenesis. You see I die hard, and stick up for my poor child.

This letter is written for my own satisfaction, and not for yours. So bear it.

Yours affectionately,
CH. DARWIN.

© The Complete Work of Charles Darwin Online

*C. Darwin to A. Newton.**

Down, February 9 [1870].

DEAR NEWTON,—I suppose it would be universally held extremely wrong for a defendant to write to a Judge to express his satisfaction at a judgment in his favour; and yet I am going thus to act. I have just read what you have said in the 'Record' † about my pigeon chapters, and it has gratified me beyond measure. I have sometimes felt a little disappointed that the labour of so many years seemed to be almost thrown away, for you are the first man capable of forming a judgment (excepting partly Quatrefages), who seems to have thought anything of this part of my work. The amount of labour, correspondence, and care, which the subject cost me, is more than you could well suppose. I thought the article in the *Athenæum* was very unjust; but now I feel amply repaid, and I cordially thank you for your sympathy and too warm praise. What labour you have bestowed on your part of the 'Record'! I ought to be ashamed to speak of my amount of work. I thoroughly enjoyed the Sunday which you and the others spent here, and

I remain, dear Newton, yours very sincerely,

CH. DARWIN.

C. Darwin to A. R. Wallace.

Down, February 27 [1868].

MY DEAR WALLACE,—You cannot well imagine how much I have been pleased by what you say about 'Pangenesis.' None of my friends will speak out. . . . Hooker, as far as I understand him, which I hardly do at present, seems to think that the hypothesis is little more than saying that organisms have such and such potentialities. What you

* Prof. of Zoology at Cambridge.

† 'Zoological Record.' The volume for 1868, published Dec. 1869.

© The Complete Work of Charles Darwin Online

say exactly and fully expresses my feeling, viz. that it is a relief to have some feasible explanation of the various facts, which can be given up as soon as any better hypothesis is found. It has certainly been an immense relief to my mind; for I have been stumbling over the subject for years, dimly seeing that some relation existed between the various classes of facts. I now hear from H. Spencer that his views quoted in my foot-note refer to something quite distinct, as you seem to have perceived.

I shall be very glad to hear at some future day your criticisms on the "causes of variability." Indeed I feel sure that I am right about sterility and natural selection. . . . I do not quite understand your case, and we think that a word or two is misplaced. I wish some time you would consider the case under the following point of view:—If sterility is caused or accumulated through natural selection, then as every degree exists up to absolute barrenness, natural selection must have the power of increasing it. Now take two species, A and B, and assume that they are (by any means) half-sterile, *i.e.* produce half the full number of offspring. Now try and make (by natural selection) A and B absolutely sterile when crossed, and you will find how difficult it is. I grant, indeed it is certain, that the degree of sterility of the individuals A and B will vary, but any such extra-sterile individuals of, we will say A, if they should hereafter breed with other individuals of A, will bequeath no advantage to their progeny, by which these families will tend to increase in number over other families of A, which are not more sterile when crossed with B. But I do not know that I have made this any clearer than in the chapter in my book. It is a most difficult bit of reasoning, which I have gone over and over again on paper with diagrams.

. . . Hearty thanks for your letter. You have indeed pleased me, for I had given up the great god Pan as a still-born deity. I wish you could be induced to make it clear,

© The Complete Work of Charles Darwin Online

with your admirable powers of elucidation, in one of the scientific journals. . . .

C. Darwin to J. D. Hooker.

Down, February 28 [1868].

MY DEAR HOOKER,—I have been deeply interested by your letter, and we had a good laugh over Huxley's remark, which was so deuced clever that you could not recollect it. I cannot quite follow your train of thought, for in the last page you admit all that I wish, having apparently denied all, or thought all mere words in the previous pages of your note; but it may be my muddle. I see clearly that any satisfaction which Pan may give will depend on the constitution of each man's mind. If you have arrived already at any similar conclusion, the whole will of course appear stale to you. I heard yesterday from Wallace, who says (excuse horrid vanity), "I can hardly tell you how much I admire the chapter on 'Pangenesis.' It is a *positive comfort* to me to have any feasible explanation of a difficulty that has always been haunting me, and I shall never be able to give it up till a better one supplies its place, and that I think hardly possible, &c." Now his foregoing [italicised] words express my sentiments exactly and fully: though perhaps I feel the relief extra strongly from having during many years vainly attempted to form some hypothesis. When you or Huxley say that a single cell of a plant, or the stump of an amputated limb, has the "potentiality" of reproducing the whole—or "diffuses an influence," these words give me no positive idea;—but, when it is said that the cells of a plant, or stump, include atoms derived from every other cell of the whole organism and capable of development, I gain a distinct idea. But this idea would not be worth a rush, if it applied to one case alone; but it seems to me to apply to all the forms of reproduction—inheritance—metamorphosis—to the

© The Complete Work of Charles Darwin Online

abnormal transposition of organs—to the direct action of the male element on the mother plant, &c. Therefore I fully believe that each cell does *actually* throw off an atom or gemmule of its contents;—but whether or not, this hypothesis serves as a useful connecting link for various grand classes of physiological facts, which at present stand absolutely isolated.

I have touched on the doubtful point (alluded to by Huxley) how far atoms derived from the same cell may become developed into different structure accordingly as they are differently nourished; I advanced as illustrations galls and polypoid excrescences. . . .

It is a real pleasure to me to write to you on this subject, and I should be delighted if we can understand each other; but you must not let your good nature lead you on. Remember we always fight tooth and nail. We go to London on Tuesday, first for a week to Queen Anne Street, and afterwards to Miss Wedgwood's, in Regent's Park, and stay the whole month, which, as my gardener truly says, is a "terrible thing" for my experiments.

*C. Darwin to W. Ogle.**

Down, March 6 [1868].

DEAR SIR,—I thank you most sincerely for your letter, which is very interesting to me. I wish I had known of these views of Hippocrates before I had published, for they seem almost identical with mine—merely a change of terms—and an application of them to classes of facts necessarily unknown to the old philosopher. The whole case is a good illustration of how rarely anything is new.

. . . Hippocrates has taken the wind out of my sails, but I care very little about being forestalled. I advance the views

* Dr. William Ogle, now the Superintendent of Statistics to the Registrar-General.

© The Complete Work of Charles Darwin Online

merely as a provisional hypothesis, but with the secret expectation that sooner or later some such view will have to be admitted.

. . . I do not expect the reviewers will be so learned as you: otherwise, no doubt, I shall be accused of wilfully stealing Pangenesis from Hippocrates,—for this is the spirit some reviewers delight to show.

C. Darwin to Victor Carus.

Down, March 21 [1868].

. . . I am very much obliged to you for sending me so frankly your opinion on Pangenesis, and I am sorry it is unfavourable, but I cannot quite understand your remark on pangenesis, selection, and the struggle for life not being more methodical. I am not at all surprised at your unfavourable verdict; I know many, probably most, will come to the same conclusion. One English Review says it is much too complicated. . .. . Some of my friends are enthusiastic on the hypothesis. . . . Sir C. Lyell says to every one, "You may not believe in 'Pangenesis,' but if you once understand it, you will never get it out of your mind." And with this criticism I am perfectly content. All cases of inheritance and reversion and development now appear to me under a new light. . . .

[An extract from a letter to Fritz Müller, though of later date (June), may be given here:—

"Your letter of April 22 has much interested me. I am delighted that you approve of my book, for I value your opinion more than that of almost any one. I have yet hopes that you will think well of Pangenesis. I feel sure that our minds are somewhat alike, and I find it a great relief to have some definite, though hypothetical view, when I reflect on the wonderful transformations of animals,—the re-growth of parts,—and especially the direct action of pollen on the

© The Complete Work of Charles Darwin Online

mother-form, &c. It often appears to me almost certain that the characters of the parents are 'photographed' on the child, only by means of material atoms derived from each cell in both parents, and developed in the child."]

C. Darwin to Asa Gray.

Down, May 8 [1868].

MY DEAR GRAY,—I have been a most ungrateful and ungracious man not to have written to you an immense time ago to thank you heartily for the *Nation*, and for all your most kind aid in regard to the American edition [of 'Animals and Plants']. But I have been of late overwhelmed with letters, which I was forced to answer, and so put off writing to you. This morning I received the American edition (which looks capital), with your nice preface, for which hearty thanks. I hope to heaven that the book will succeed well enough to prevent you repenting of your aid. This arrival has put the finishing stroke to my conscience, which will endure its wrongs no longer.

. . . Your article in the *Nation* [Mar. 19] seems to me very good, and you give an excellent idea of Pangenesis—an infant cherished by few as yet, except his tender parent, but which will live a long life. There is parental presumption for you! You give a good slap at my concluding metaphor: * undoubtedly I ought to have brought in and contrasted natural and artificial selection; but it seemed so obvious to me that natural selection depended on contingencies even more

* A short abstract of the precipice metaphor is given at p. 307, vol. i. Dr. Gray's criticism on this point is as follows: "But in Mr. Darwin's parallel, to meet the case of nature according to his own view of it, not only the fragments of rock (answering to variation) should fall, but the edifice (answering to natural selection) should rise, irrespective of will or choice!" But my father's parallel demands that natural selection shall be the architect, not the edifice—the question of design only comes in with regard to the form of the building materials.

© The Complete Work of Charles Darwin Online

complex than those which must have determined the shape of each fragment at the base of my precipice. What I wanted to show was that, in reference to pre-ordainment, whatever holds good in the formation of a pouter pigeon holds good in the formation of a natural species of pigeon. I cannot see that this is false. If the right variations occurred, and no others, natural selection would be superfluous. A reviewer in an Edinburgh paper, who treats me with profound contempt, says on this subject that Professor Asa Gray could with the greatest ease smash me into little pieces.*

Believe me, my dear Gray,

Your ungrateful but sincere friend,

CHARLES DARWIN.

C. Darwin to G. Bentham.

Down, June 23, 1868.

MY DEAR MR. BENTHAM,—As your address † is somewhat of the nature of a verdict from a judge, I do not know whether it is proper for me to do so, but I must and will thank you for the pleasure which you have given me. I am delighted at what you say about my book. I got so tired of it, that for months together I thought myself a perfect fool for having given up so much time in collecting and observing little facts, but now I do not care if a score of common critics speak as contemptuously of the book as did the *Athenæum*. I feel justified in this, for I have so complete a reliance on your judgment that I feel certain that I should have bowed to your

* The *Daily Review*, April 27, 1868. My father has given rather a highly coloured version of the reviewer's remarks: "We doubt not that Professor Asa Gray . . . could show that natural selection . . . is simply an instrument in the hands of an omnipotent and omniscient creator." The reviewer goes on to say that the passage in question is a "very melancholy one," and that the theory is the "apotheosis of materialism."

† Presidential Address to the Linnean Society.

© The Complete Work of Charles Darwin Online

judgment had it been as unfavourable as it is the contrary. What you say about Pangenesis quite satisfies me, and is as much perhaps as any one is justified in saying. I have read your whole Address with the greatest interest. It must have cost you a vast amount of trouble. With cordial thanks, pray believe me,

Yours very sincerely,
CH. DARWIN.

P.S.—I fear that it is not likely that you have a superfluous copy of your Address; if you have, I should much like to send one to Fritz Müller in the interior of Brazil. By the way, let me add that I discussed bud-variation chiefly from a belief which is common to several persons, that all variability is related to sexual generation; I wished to show clearly that this was an error.

[The above series of letters may serve to show, to some extent, the reception which the new book received. Before passing on (in the next chapter) to the 'Descent of Man,' I give a letter referring to the translation of Fritz Müller's book, 'Für Darwin.' It was originally published in 1864, but the English translation, by Mr. Dallas, which bore the title suggested by Sir C. Lyell, of 'Facts and Arguments for Darwin,' did not appear until 1869:]

C. Darwin to F. Müller.

Down, March 16 [1868].

MY DEAR SIR,—Your brother, as you will have heard from him, felt so convinced that you would not object to a translation of 'Für Darwin,'* that I have ventured to arrange for a translation. Engelmann has very liberally offered me

* In a letter to Fritz Müller, my father wrote:—"I am vexed to see that on the title my name is more conspicuous than yours, which I especially objected to, and I cautioned the printers after seeing one proof."

© The Complete Work of Charles Darwin Online

clichés of the woodcuts for 22 thalers; Mr. Murray has agreed to bring out a translation (and he is our best publisher) on commission, for he would not undertake the work on his own risk; and I have agreed with Mr. W. S. Dallas (who has translated Von Siebold on Parthenogenesis, and many German works, and who writes very good English) to translate the book. He thinks (and he is a good judge) that it is important to have some few corrections or additions, in order to account for a translation appearing so lately [*i.e.* at such a long interval of time] after the original; so that I hope you will be able to send some. . . . .

[Two letters may be placed here, as bearing on the spread of Evolutionary ideas in France and Germany:]

C. Darwin to A. Gaudry.

Down, January 21 [1868].

DEAR SIR,—I thank you for your interesting essay on the influence of the Geological features of the country on the mind and habits of the Ancient Athenians,* and for your very obliging letter. I am delighted to hear that you intend to consider the relations of fossil animals in connection with their genealogy; it will afford you a fine field for the exercise of your extensive knowledge and powers of reasoning. Your belief will I suppose, at present, lower you in the estimation of your countrymen; but judging from the rapid spread in all parts of Europe, excepting France, of the belief in the common descent of allied species, I must think that this belief will before long become universal. How strange it is that the country which gave birth to Buffon, the elder Geoffroy, and especially to Lamarck, should now cling so pertinaciously to the belief that species are immutable creations.

* This appears to refer to M. Gaudry's paper translated in the 'Geol. Mag.,' 1868, p. 372.

© The Complete Work of Charles Darwin Online

My work on Variation, &c., under domestication, will appear in a French translation in a few months' time, and I will do myself the pleasure and honour of directing the publisher to send a copy to you to the same address as this letter.

With sincere respect, I remain, dear sir,

Yours very faithfully,

CHARLES DARWIN.

[The next letter is of especial interest, as showing how high a value my father placed on the support of the younger German naturalists :]

*C. Darwin to W. Preyer.**

March 31, 1868.

. . . . I am delighted to hear that you uphold the doctrine of the Modification of Species, and defend my views. The support which I receive from Germany is my chief ground for hoping that our views will ultimately prevail. To the present day I am continually abused or treated with contempt by writers of my own country; but the younger naturalists are almost all on my side, and sooner or later the public must follow those who make the subject their special study. The abuse and contempt of ignorant writers hurts me very little. . . .

* Now Professor of Physiology at Jena.

© The Complete Work of Charles Darwin Online

CHAPTER III.

WORK ON 'MAN.'

1864–1870.

[In the autobiographical chapter (Vol. I. p. 93), my father gives the circumstances which led to his writing the 'Descent of Man.' He states that his collection of facts, begun in 1837 or 1838, was continued for many years without any definite idea of publishing on the subject. The following letter to Mr. Wallace shows that in the period of ill-health and depression about 1864 he despaired of ever being able to do so:]

C. Darwin to A. R. Wallace.

Down, [May ?] 28 [1864].

Dear Wallace,—I am so much better that I have just finished a paper for Linnean Society;* but I am not yet at all strong, I felt much disinclination to write, and therefore you must forgive me for not having sooner thanked you for your paper on 'Man,'† received on the 11th. But first let me say that I have hardly ever in my life been more struck by any paper than that on 'Variation,' &c. &c., in the *Reader*.‡ I feel sure that such papers will do more for the spreading of

* On the three forms, &c., of Lythrum.

† 'Anthropological Review,' March 1864.

‡ *Reader*, Ap. 16, 1864. "On the Phenomena of Variation," &c. Abstract of a paper read before the Linnean Society, Mar. 17, 1864.

© The Complete Work of Charles Darwin Online

our views on the modification of species than any separate Treatises on the simple subject itself. It is really admirable; but you ought not in the Man paper to speak of the theory as mine; it is just as much yours as mine. One correspondent has already noticed to me your "high-minded" conduct on this head. But now for your Man paper, about which I should like to write more than I can. The great leading idea is quite new to me, viz. that during late ages, the mind will have been modified more than the body; yet I had got as far as to see with you, that the struggle between the races of man depended entirely on intellectual and *moral* qualities. The latter part of the paper I can designate only as grand and most eloquently done. I have shown your paper to two or three persons who have been here, and they have been equally struck with it. I am not sure that I go with you on all minor points: when reading Sir G. Grey's account of the constant battles of Australian savages, I remember thinking that natural selection would come in, and likewise with the Esquimaux, with whom the art of fishing and managing canoes is said to be hereditary. I rather differ on the rank, under a classificatory point of view, which you assign to man; I do not think any character simply in excess ought ever to be used for the higher divisions. Ants would not be separated from other hymenopterous insects, however high the instinct of the one, and however low the instincts of the other. With respect to the differences of race, a conjecture has occurred to me that much may be due to the correlation of complexion (and consequently hair) with constitution. Assume that a dusky individual best escaped miasma, and you will readily see what I mean. I persuaded the Director-General of the Medical Department of the Army to send printed forms to the surgeons of all regiments in tropical countries to ascertain this point, but I dare say I shall never get any returns. Secondly, I suspect that a sort of sexual selection has been

© The Complete Work of Charles Darwin Online

the most powerful means of changing the races of man. I can show that the different races have a widely different standard of beauty. Among savages the most powerful men will have the pick of the women, and they will generally leave the most descendants. I have collected a few notes on man, but I do not suppose that I shall ever use them. Do you intend to follow out your views, and if so, would you like at some future time to have my few references and notes? I am sure I hardly know whether they are of any value, and they are at present in a state of chaos.

There is much more that I should like to write, but I have not strength.

Believe me, dear Wallace, yours very sincerely,

CH. DARWIN.

P.S.—Our aristocracy is handsomer (more hideous according to a Chinese or Negro) than the middle classes, from [having the] pick of the women; but oh, what a scheme is primogeniture for destroying natural selection! I fear my letter will be barely intelligible to you.

[In February 1867, when the manuscript of 'Animals and Plants' had been sent to Messrs. Clowes to be printed, and before the proofs began to come in, he had an interval of spare time, and began a "chapter on Man," but he soon found it growing under his hands, and determined to publish it separately as a "very small volume."

The work was interrupted by the necessity of correcting the proofs of 'Animals and Plants,' and by some botanical work, but was resumed with unremitting industry on the first available day in the following year. He could not rest, and he recognized with regret the gradual change in his mind that rendered continuous work more and more necessary to him as he grew older. This is expressed in a letter to Sir J. D. Hooker, June 17, 1868, which repeats to some extent what is given in the Autobiography:—

© The Complete Work of Charles Darwin Online

"I am glad you were at the 'Messiah,' it is the one thing that I should like to hear again, but I dare say I should find my soul too dried up to appreciate it as in old days; and then I should feel very flat, for it is a horrid bore to feel as I constantly do, that I am a withered leaf for every subject except Science. It sometimes makes me hate Science, though God knows I ought to be thankful for such a perennial interest, which makes me forget for some hours every day my accursed stomach."

The work on Man was interrupted by illness in the early summer of 1868, and he left home on July 16th for Freshwater, in the Isle of Wight, where he remained with his family until August 21st. Here he made the acquaintance of Mrs. Cameron. She received the whole family with open-hearted kindness and hospitality, and my father always retained a warm feeling of friendship for her. She made an excellent photograph of him, which was published with the inscription written by him: "I like this photograph very much better than any other which has been taken of me." Further interruption occurred in the autumn, so that continuous work on the 'Descent of Man' did not begin until 1869. The following letters give some idea of the earlier work in 1867:]

C. Darwin to A. R. Wallace.

Down, February 22, [1867?]

MY DEAR WALLACE,—I am hard at work on sexual selection, and am driven half mad by the number of collateral points which require investigation, such as the relative number of the two sexes, and especially on polygamy. Can you aid me with respect to birds which have strongly marked secondary sexual characters, such as birds of

© The Complete Work of Charles Darwin Online

paradise, humming-birds, the Rupicola, or any other such cases? Many gallinaceous birds certainly are polygamous. I suppose that birds may be known not to be polygamous if they are seen during the whole breeding season to associate in pairs, or if the male incubates or aids in feeding the young. Will you have the kindness to turn this in your mind? But it is a shame to trouble you now that, as I am *heartily* glad to hear, you are at work on your Malayan travels. I am fearfully puzzled how far to extend your protective views with respect to the females in various classes. The more I work, the more important sexual selection apparently comes out.

Can butterflies be polygamous? *i.e.* will one male impregnate more than one female? Forgive me troubling you, and I dare say I shall have to ask forgiveness again. . . .

C. Darwin to A. R. Wallace.

Down, February 23 [1867].

DEAR WALLACE,—I much regretted that I was unable to call on you, but after Monday I was unable even to leave the house. On Monday evening I called on Bates, and put a difficulty before him, which he could not answer, and, as on some former similar occasion, his first suggestion was, "You had better ask Wallace." My difficulty is, why are caterpillars sometimes so beautifully and artistically coloured? Seeing that many are coloured to escape danger, I can hardly attribute their bright colour in other cases to mere physical conditions. Bates says the most gaudy caterpillar he ever saw in Amazonia (of a sphinx) was conspicuous at the distance of yards, from its black and red colours, whilst feeding on large green leaves. If any one objected to male butterflies having been made beautiful by sexual selection, and asked why should they not have been made beautiful as

© The Complete Work of Charles Darwin Online

well as their caterpillars, what would you answer? I could not answer, but should maintain my ground. Will you think over this, and some time, either by letter or when we meet, tell me what you think? Also I want to know whether your *female* mimetic butterfly is more beautiful and brighter than the male. When next in London I must get you to show me your kingfishers. My health is a dreadful evil; I failed in half my engagements during this last visit to London.

Believe me, yours very sincerely,

C. DARWIN.

C. Darwin to A. R. Wallace.

Down, February 26 [1867].

MY DEAR WALLACE,—Bates was quite right; you are the man to apply to in a difficulty. I never heard anything more ingenious than your suggestion,* and I hope you may be able to prove it true. That is a splendid fact about the white moths; it warms one's very blood to see a theory thus almost proved to be true.† With respect to the beauty of male butterflies, I must as yet think that it is due to sexual selection. There is some evidence that dragon-flies are attracted by bright colours; but what leads me to the above belief, is so many male Orthoptera and Cicadas having musical instruments. This being the case, the analogy of birds makes me believe in sexual selection with respect to colour in insects. I wish I had strength and time to make some of the experiments suggested by you, but I thought butterflies would not pair in confinement. I am sure I have heard of some such difficulty. Many years ago I had a

* The suggestion that conspicuous caterpillars or perfect insects (e.g. white butterflies), which are distasteful to birds, are protected by being easily recognised and avoided. See Mr. Wallace's 'Natural Selection,' 2nd edit., p. 117.

† Mr. Jenner Weir's observations published in the Transactions of the Entomolog. Soc. (1869 and 1870) give strong support to the theory in question.

© The Complete Work of Charles Darwin Online

dragon-fly painted with gorgeous colours, but I never had an opportunity of fairly trying it.

The reason of my being so much interested just at present about sexual selection is, that I have almost resolved to publish a little essay on the origin of Mankind, and I still strongly think (though I failed to convince you, and this, to me, is the heaviest blow possible) that sexual selection has been the main agent in forming the races of man.

By the way, there is another subject which I shall introduce in my essay, namely, expression of countenance. Now, do you happen to know by any odd chance a very good-natured and acute observer in the Malay Archipelago, who you think would make a few easy observations for me on the expression of the Malays when excited by various emotions? For in this case I would send to such person a list of queries. I thank you for your most interesting letter, and remain,

Yours very sincerely,

CH. DARWIN.

C. Darwin to A. R. Wallace.

Down, March [1867].

MY DEAR WALLACE,—I thank you much for your two notes. The case of Julia Pastrana * is a splendid addition to my other cases of correlated teeth and hair, and I will add it in correcting the press of my present volume. Pray let me hear in the course of the summer if you get any evidence about the gaudy caterpillars. I should much like to give (or quote if published) this idea of yours, if in any way supported, as suggested by you. It will, however, be a long time hence, for I can see that sexual selection is growing into quite a large subject, which I shall introduce into my essay on Man, supposing that I ever publish it. I had

* A bearded woman having an irregular double set of teeth. See 'Animals and Plants,' vol. ii. p. 328.

© The Complete Work of Charles Darwin Online

intended giving a chapter on man, inasmuch as many call him (not *quite* truly) an eminently domesticated animal, but I found the subject too large for a chapter. Nor shall I be capable of treating the subject well, and my sole reason for taking it up is, that I am pretty well convinced that sexual selection has played an important part in the formation of races, and sexual selection has always been a subject which has interested me much. I have been very glad to see your impression from memory on the expression of Malays. I fully agree with you that the subject is in no way an important one; it is simply a "hobby-horse" with me, about twenty-seven years old; and *after* thinking that I would write an essay on Man, it flashed on me that I could work in some "supplemental remarks on expression." After the horrid, tedious, dull work of my present huge, and I fear unreadable, book ['The Variation of Animals and Plants'], I thought I would amuse myself with my hobby-horse. The subject is, I think, more curious and more amenable to scientific treatment than you seem willing to allow. I want, anyhow, to upset Sir C. Bell's view, given in his most interesting work, 'The Anatomy of Expression,' that certain muscles have been given to man solely that he may reveal to other men his feelings. I want to try and show how expressions have arisen. That is a good suggestion about newspapers, but my experience tells me that private applications are generally most fruitful. I will, however, see if I can get the queries inserted in some Indian paper. I do not know the names or addresses of any other papers.

. . . My two female amanuenses are busy with friends, and I fear this scrawl will give you much trouble to read. With many thanks,

Yours very sincerely,
CH. DARWIN.

[The following letter is worth giving, as an example

© The Complete Work of Charles Darwin Online

of his sources of information, and as showing what were the thoughts at this time occupying him :]

C. Darwin to F. Müller.

Down, June 3 [1868].

. . . Many thanks for all the curious facts about the unequal number of the sexes in Crustacea, but the more I investigate this subject the deeper I sink in doubt and difficulty. Thanks also for the confirmation of the rivalry of Cicadæ. I have often reflected with surprise on the diversity of the means for producing music with insects, and still more with birds. We thus get a high idea of the importance of song in the animal kingdom. Please to tell me where I can find any account of the auditory organs in the Orthoptera. Your facts are quite new to me. Scudder has described an insect in the Devonian strata, furnished with a stridulating apparatus. I believe he is to be trusted, and, if so, the apparatus is of astonishing antiquity. After reading Landois's paper I have been working at the stridulating organ in the Lamellicorn beetles, in expectation of finding it sexual; but I have only found it as yet in two cases, and in these it was equally developed in both sexes. I wish you would look at any of your common Lamellicorns, and take hold of both males and females, and observe whether they make the squeaking or grating noise equally. If they do not, you could, perhaps, send me a male and female in a light little box. How curious it is that there should be a special organ for an object apparently so unimportant as squeaking. Here is another point; have you any toucans? if so, ask any trustworthy hunter whether the beaks of the males, or of both sexes, are more brightly coloured during the breeding season than at other times of the year. . . . Heaven knows whether I shall ever live to make use of half the valuable facts which you have communicated to me! Your paper on *Balanus*

© The Complete Work of Charles Darwin Online

armatus, translated by Mr. Dallas, has just appeared in our 'Annals and Magazine of Natural History,' and I have read it with the greatest interest. I never thought that I should live to hear of a hybrid Balanus! I am very glad that you have seen the cement tubes; they appear to me extremely curious, and, as far as I know, you are the first man who has verified my observations on this point.

With most cordial thanks for all your kindness, my dear Sir,

Yours very sincerely,
C. DARWIN.

C. Darwin to A. De Candolle.

Down, July 6, 1868.

MY DEAR SIR,—I return you my *sincere* thanks for your long letter, which I consider a great compliment, and which is quite full of most interesting facts and views. Your references and remarks will be of great use should a new edition of my book * be demanded, but this is hardly probable, for the whole edition was sold within the first week, and another large edition immediately reprinted, which I should think would supply the demand for ever. You ask me when I shall publish on the 'Variation of Species in a State of Nature.' I have had the MS. for another volume almost ready during several years, but I was so much fatigued by my last book that I determined to amuse myself by publishing a short essay on the 'Descent of Man.' I was partly led to do this by having been taunted that I concealed my views, but chiefly from the interest which I had long taken in the subject. Now this essay has branched out into some collateral subjects, and I suppose will take me more than a year to complete. I shall then begin on 'Species,' but my health makes me a very slow workman. I hope that you will excuse these details, which I have given to show

* 'Variation of Animals and Plants.'

© The Complete Work of Charles Darwin Online

that you will have plenty of time to publish your views first, which will be a great advantage to me. Of all the curious facts which you mention in your letter, I think that of the strong inheritance of the scalp-muscles has interested me most. I presume that you would not object to my giving this very curious case on your authority. As I believe all anatomists look at the scalp-muscles as a remnant of the *Panniculus carnosus* which is common to all the lower quadrupeds, I should look at the unusual development and inheritance of these muscles as probably a case of reversion. Your observation on so many remarkable men in noble families having been illegitimate is extremely curious; and should I ever meet any one capable of writing an essay on this subject I will mention your remarks as a good suggestion. Dr. Hooker has several times remarked to me that morals and politics would be very interesting if discussed like any branch of natural history, and this is nearly to the same effect with your remarks. . . .

C. Darwin to L. Agassiz.

Down, August 19, 1868.

DEAR SIR,—I thank you cordially for your very kind letter. I certainly thought that you had formed so low an opinion of my scientific work that it might have appeared indelicate in me to have asked for information from you, but it never occurred to me that my letter would have been shown to you. I have never for a moment doubted your kindness and generosity, and I hope you will not think it presumption in me to say, that when we met, many years ago, at the British Association at Southampton, I felt for you the warmest admiration.

Your information on the Amazonian fishes has interested me *extremely*, and tells me exactly what I wanted to know. I was aware, through notes given me by Dr. Günther, that

© The Complete Work of Charles Darwin Online

many fishes differed sexually in colour and other characters, but I was particularly anxious to learn how far this was the case with those fishes in which the male, differently from what occurs with most birds, takes the largest share in the care of the ova and young. Your letter has not only interested me much, but has greatly gratified me in other respects, and I return you my sincere thanks for your kindness. Pray believe me, my dear Sir,

Yours very faithfully,
CHARLES DARWIN.

C. Darwin to J. D. Hooker.

Down, Sunday, August 23 [1868].

MY DEAR OLD FRIEND,—I have received your note. I can hardly say how pleased I have been at the success of your address,* and of the whole meeting. I have seen the *Times*, *Telegraph*, *Spectator*, and *Athenæum*, and have heard of other favourable newspapers, and have ordered a bundle. There is a "chorus of praise." The *Times* reported miserably, *i.e.* as far as errata were concerned; but I was very glad at the leader, for I thought the way you brought in the megalithic monuments most happy.† I particularly admired Tyndall's little speech.‡ . . . The *Spectator* pitches a little into you about Theology, in accordance with its usual spirit. . . .

Your great success has rejoiced my heart. I have just carefully read the whole address in the *Athenæum*; and though, as you know, I liked it very much when you read it to me, yet, as I was trying all the time to find fault, I missed to a certain extent the effect as a whole; and this now

* Sir Joseph Hooker was President of the British Association at the Norwich Meeting in 1868.

† The British Association was desirous of interesting the Government in certain modern cromlech builders, the Khasia race of East Bengal, in order that their megalithic monuments might be efficiently described.

‡ Professor Tyndall was President of Section A.

© The Complete Work of Charles Darwin Online

appears to me most striking and excellent. How you must rejoice at all your bothering labour and anxiety having had so grand an end. I must say a word about myself; never has such a eulogium been passed on me, and it makes me very proud. I cannot get over my *amazement* at what you say about my botanical work. By Jove, as far as my memory goes, you have strengthened instead of weakened some of the expressions. What is far more important than anything personal, is the conviction which I feel, that you will have immensely advanced the belief in the evolution of species. This will follow from the publicity of the occasion, your position, so responsible, as President, and your own high reputation. It will make a great step in public opinion, I feel sure, and I had not thought of this before. The *Athenæum* takes your snubbing * with the utmost mildness. I certainly do rejoice over the snubbing, and hope [the reviewer] will feel it a little. Whenever you have *spare* time to write again, tell me whether any astronomers † took your remarks in ill part; as they now stand they do not seem at all too harsh and presumptuous. Many of your sentences strike me as extremely felicitous and eloquent. That of Lyell's "underpinning," ‡ is capital. Tell me, was Lyell pleased? I am so glad that you remembered my old dedication.§ Was Wallace pleased?

* Sir Joseph Hooker made some reference to the review of 'Animals and Plants' in the *Athenæum* of Feb. 15, 1868.

† In discussing the astronomer's objection to Evolution, namely that our globe has not existed for a long enough period to give time for the assumed transmutation of living beings, Hooker challenged Whewell's dictum, that astronomy is the queen of sciences—the only perfect science.

‡ After a eulogium on Sir Charles Lyell's heroic renunciation of his old views in accepting Evolution, Sir J. D. Hooker continued, "Well may he be proud of a superstructure, raised on the foundations of an insecure doctrine, when he finds that he can underpin it and substitute a new foundation; and after all is finished, survey his edifice, not only more secure but more harmonious in its proportion than it was before."

§ The 'Naturalist's Voyage' was dedicated to Lyell.

© The Complete Work of Charles Darwin Online

How about photographs? Can you spare time for a line to our dear Mrs. Cameron?* She came to see us off, and loaded us with presents of photographs, and Erasmus called after her, "Mrs. Cameron, there are six people in this house all in love with you." When I paid her, she cried out, "Oh, what a lot of money!" and ran to boast to her husband.

I must not write any more, though I am in tremendous spirits at your brilliant success.

Yours ever affectionately,

C. DARWIN.

[In the *Athenæum* of November 29, 1868, appeared an article which was in fact a reply to Sir Joseph Hooker's remarks at Norwich. He seems to have consulted my father as to the wisdom of answering the article. My father wrote to him on December 1:—

"In my opinion Dr. Joseph Dalton Hooker need take no notice of the attack in the *Athenæum* in reference to Mr. Charles Darwin. What an ass the man is, to think he cuts one to the quick by giving one's Christian name in full. How transparently false is the statement that my sole groundwork is from pigeons, because I state I have worked them out more fully than other beings! He muddles together two books of Flourens."

The following letter refers to a paper† by Judge Caton, of which my father often spoke with admiration:]

C. Darwin to John D. Caton.

Down, September 18, 1868.

DEAR SIR,—I beg leave to thank you very sincerely for your kindness in sending me, through Mr. Walsh, your admirable paper on American Deer.

* See Vol. III. p. 92.

† 'Transactions of the Ottawa Academy of Natural Sciences,' 1868. By John D. Caton, late Chief Justice of Illinois.

© The Complete Work of Charles Darwin Online

It is quite full of most interesting observations, stated with the greatest clearness. I have seldom read a paper with more interest, for it abounds with facts of direct use for my work. Many of them consist of little points which hardly any one besides yourself has observed, or perceived the importance of recording. I would instance the age at which the horns are developed (a point on which I have lately been in vain searching for information), the rudiment of horns in the female elk, and especially the different nature of the plants devoured by the deer and elk, and several other points. With cordial thanks for the pleasure and instruction which you have afforded me, and with high respect for your power of observation, I beg leave to remain, dear Sir,

Yours faithfully and obliged,

CHARLES DARWIN.

[The following extract from a letter (Sept. 24, 1868) to the Marquis de Saporta, the eminent palæo-botanist, refers to the growth of Evolutionary views in France:—*

"As I have formerly read with great interest many of your papers on fossil plants, you may believe with what high satisfaction I hear that you are a believer in the gradual evolution of species. I had supposed that my book on the 'Origin of Species' had made very little impression in France, and therefore it delights me to hear a different statement from you. All the great authorities of the Institute seem firmly resolved to believe in the immutability of species, and this has always astonished me. . . . Almost the one exception, as far as I know, is M. Gaudry, and I think he will be soon one of the chief leaders in Zoological Palæontology in Europe; and now I am delighted to hear that in the sister department of Botany you take nearly the same view."]

* In 1868 he was pleased at being asked to authorise a French translation of his 'Naturalist's Voyage.'

© The Complete Work of Charles Darwin Online

C. Darwin to E. Haeckel.

Down, Nov. 19 [1868].

MY DEAR HAECKEL,—I must write to you again, for two reasons. Firstly, to thank you for your letter about your baby, which has quite charmed both me and my wife; I heartily congratulate you on its birth. I remember being surprised in my own case how soon the paternal instincts became developed, and in you they seem to be unusually strong, . . . I hope the large blue eyes and the principles of inheritance will make your child as good a naturalist as you are; but, judging from my own experience, you will be astonished to find how the whole mental disposition of your children changes with advancing years. A young child, and the same when nearly grown, sometimes differ almost as much as do a caterpillar and butterfly.

The second point is to congratulate you on the projected translation of your great work,* about which I heard from Huxley last Sunday. I am heartily glad of it, but how it has been brought about, I know not, for a friend who supported the proposed translation at Norwich, told me he thought there would be no chance of it. Huxley tells me that you consent to omit and shorten some parts, and I am confident that this is very wise. As I know your object is to instruct the public, you will assuredly thus get many more readers in England. Indeed, I believe that almost every book would be improved by condensation. I have been reading a good deal of your last book,† and the style is beautifully clear and easy to me; but why it should differ so much in this respect from your great work I cannot imagine. I have not yet read the first part, but began with the chapter on Lyell and myself, which you will easily believe

* 'Generelle Morphologie,' 1866. No English translation of this book has appeared.

† 'Die Natürliche Schöpfungs-Geschichte,' 1868. It was translated and published in 1876, under the title, 'The History of Creation.'

© The Complete Work of Charles Darwin Online

pleased me *very much*. I think Lyell, who was apparently much pleased by your sending him a copy, is also much gratified by this chapter.* Your chapters on the affinities and genealogy of the animal kingdom strike me as admirable and full of original thought. Your boldness, however, sometimes makes me tremble, but as Huxley remarked, some one must be bold enough to make a beginning in drawing up tables of descent. Although you fully admit the imperfection of the geological record, yet Huxley agreed with me in thinking that you are sometimes rather rash in venturing to say at what periods the several groups first appeared. I have this advantage over you, that I remember how wonderfully different any statement on this subject made 20 years ago, would have been to what would now be the case, and I expect the next 20 years will make quite as great a difference. Reflect on the monocotyledonous plant just discovered in the *primordial* formation in Sweden.

I repeat how glad I am at the prospect of the translation, for I fully believe that this work and all your works will have a great influence in the advancement of Science.

Believe me, my dear Häckel, your sincere friend,

CHARLES DARWIN.

[It was in November of this year that he sat for the bust by Mr. Woolner: he wrote :—

"I should have written long ago, but I have been pestered with stupid letters, and am undergoing the purgatory of sitting for hours to Woolner, who, however, is wonderfully pleasant, and lightens as much as man can, the penance ; as far as I can judge, it will make a fine bust."

If I may criticise the work of so eminent a sculptor as

* See Lyell's interesting letter to Haeckel. 'Life of Sir C. Lyell,' ii. p. 435.

© The Complete Work of Charles Darwin Online

Mr. Woolner, I should say that the point in which the bust fails somewhat as a portrait, is that it has a certain air, almost of pomposity, which seems to me foreign to my father's expression.]

1869.

[At the beginning of the year he was at work in preparing the fifth edition of the 'Origin.' This work was begun on the day after Christmas, 1868, and was continued for "forty-six days," as he notes in his diary, *i.e.* until February 10th, 1869. He then, February 11th, returned to Sexual Selection, and continued at this subject (excepting for ten days given up to Orchids, and a week in London), until June 10th, when he went with his family to North Wales, where he remained about seven weeks, returning to Down on July 31st.

Caerdeon, the house where he stayed, is built on the north shore of the beautiful Barmouth estuary, and is pleasantly placed in being close to wild hill country behind, as well as to the picturesque wooded "hummocks," between the steeper hills and the river. My father was ill and somewhat depressed throughout this visit, and I think felt saddened at being imprisoned by his want of strength, and unable even to reach the hills over which he had once wandered for days together.

He wrote from Caerdeon to Sir J. D. Hooker (June 22nd):—

"We have been here for ten days, how I wish it was possible for you to pay us a visit here; we have a beautiful house with a terraced garden, and a really magnificent view of Cader, right opposite. Old Cader is a grand fellow, and shows himself off superbly with every changing light. We remain here till the end of July, when the H. Wedgwoods have the house. I have been as yet in a very poor way; it seems as soon as the stimulus of mental work stops, my whole strength gives way. As yet I have hardly crawled half a mile from the house, and then have been fearfully fatigued. It is enough to make one wish oneself quiet in a comfortable tomb."

© The Complete Work of Charles Darwin Online

With regard to the fifth edition of the 'Origin,' he wrote to Mr. Wallace, January 22, 1869):—

"I have been interrupted in my regular work in preparing a new edition of the 'Origin,' which has cost me much labour, and which I hope I have considerably improved in two or three important points. I always thought individual differences more important than single variations, but now I have come to the conclusion that they are of paramount importance, and in this I believe I agree with you. Fleeming Jenkin's arguments have convinced me."

This somewhat obscure sentence was explained, February 2, in another letter to Mr. Wallace:—

"I must have expressed myself atrociously; I meant to say exactly the reverse of what you have understood. F. Jenkin argued in the 'North British Review' against single variations ever being perpetuated, and has convinced me, though not in quite so broad a manner as here put. I always thought individual differences more important; but I was blind and thought that single variations might be preserved much oftener than I now see is possible or probable. I mentioned this in my former note merely because I believed that you had come to a similar conclusion, and I like much to be in accord with you. I believe I was mainly deceived by single variations offering such simple illustrations, as when man selects."

The late Mr. Fleeming Jenkin's review, on the 'Origin of Species,' was published in the 'North British Review' for June 1867. It is not a little remarkable that the criticisms, which my father, as I believe, felt to be the most valuable ever made on his views should have come, not from a professed naturalist but from a Professor of Engineering.

It is impossible to give in a short compass an account of Fleeming Jenkin's argument. My father's copy of the paper (ripped out of the volume as usual, and tied with a bit of string) is annotated in pencil in many places. I may quote

© The Complete Work of Charles Darwin Online

one passage opposite which my father has written "good sneers"—but it should be remembered that he used the word "sneer" in rather a special sense, not as necessarily implying a feeling of bitterness in the critic, but rather in the sense of "banter." Speaking of the 'true believer,' Fleeming Jenkin says, p. 293 :—

"He can invent trains of ancestors of whose existence there is no evidence ; he can marshal hosts of equally imaginary foes ; he can call up continents, floods, and peculiar atmospheres ; he can dry up oceans, split islands, and parcel out eternity at will ; surely with these advantages he must be a dull fellow if he cannot scheme some series of animals and circumstances explaining our assumed difficulty quite naturally. Feeling the difficulty of dealing with adversaries who command so huge a domain of fancy, we will abandon these arguments, and trust to those which at least cannot be assailed by mere efforts of imagination."

In the fifth edition of the 'Origin,' my father altered a passage in the Historical Sketch (fourth edition, p. xviii). He thus practically gave up the difficult task of understanding whether or not Sir R. Owen claims to have discovered the principle of Natural Selection. Adding, "As far as the mere enunciation of the principle of Natural Selection is concerned, it is quite immaterial whether or not Professor Owen preceded me, for both of us . . . were long ago preceded by Dr. Wells and Mr. Matthew."

A somewhat severe critique on the fifth edition, by Mr. John Robertson, appeared in the *Athenæum*, August 14, 1869. The writer comments with some little bitterness on the success of the 'Origin :' "Attention is not acceptance. Many editions do not mean real success. The book has sold ; the guess has been talked over ; and the circulation and discussion sum up the significance of the editions." Mr. Robertson makes the true, but misleading statement : "Mr. Darwin prefaces his fifth English edition with an Essay, which he

© The Complete Work of Charles Darwin Online

calls 'An Historical Sketch,' &c." As a matter of fact a Sketch appeared in the third edition in 1861.

Mr. Robertson goes on to say that the Sketch ought to be called a collection of extracts anticipatory or corroborative of the hypothesis of Natural Selection. "For no account is given of any hostile opinions. The fact is very significant. This historical sketch thus resembles the histories of the reign of Louis XVIII., published after the Restoration, from which the Republic and the Empire, Robespierre and Buonaparte were omitted."

The following letter to Prof. Victor Carus gives an idea of the character of the new edition of the 'Origin:']

C. Darwin to Victor Carus.

Down, May 4, 1869.

. . . I have gone very carefully through the whole, trying to make some parts clearer, and adding a few discussions and facts of some importance. The new edition is only two pages at the end longer than the old; though in one part nine pages in advance, for I have condensed several parts and omitted some passages. The translation I fear will cause you a great deal of trouble; the alterations took me six weeks, besides correcting the press; you ought to make a special agreement with M. Koch [the publisher]. Many of the corrections are only a few words, but they have been made from the evidence on various points appearing to have become a little stronger or weaker.

Thus I have been led to place somewhat more value on the definite and direct action of external conditions; to think the lapse of time, as measured by years, not quite so great as most geologists have thought; and to infer that single variations are of even less importance, in comparison with individual differences, than I formerly thought. I mention these points because I have been thus led to alter in many places *a few words;* and unless you go through the whole new

© The Complete Work of Charles Darwin Online

edition, one part will not agree with another, which would be a great blemish. . . .

[The desire that his views might spread in France was always strong with my father, and he was therefore justly annoyed to find that in 1869 the publisher of the first French edition had brought out a third edition without consulting the author. He was accordingly glad to enter into an arrangement for a French translation of the fifth edition; this was undertaken by M. Reinwald, with whom he continued to have pleasant relations as the publisher of many of his books into French.

He wrote to Sir J. D. Hooker:—

"I must enjoy myself and tell you about Mdlle. C. Royer, who translated the 'Origin' into French, and for whose second edition I took infinite trouble. She has now just brought out a third edition without informing me, so that all the corrections, &c., in the fourth and fifth English editions are lost. Besides her enormously long preface to the first edition, she has added a second preface abusing me like a pickpocket for Pangenesis, which of course has no relation to the 'Origin.' So I wrote to Paris; and Reinwald agrees to bring out at once a new translation from the fifth English edition, in competition with her third edition. . . . This fact shows that "evolution of species" must at last be spreading in France."

With reference to the spread of Evolution among the orthodox, the following letter is of some interest. In March he received, from the author, a copy of a lecture by Rev. T. R. R. Stebbing, given before the Torquay Natural History Society, February 1, 1869, bearing the title "Darwinism." My father wrote to Mr. Stebbing:]

Down, March 3, 1869.

DEAR SIR,—I am very much obliged to you for your kindness in sending me your spirited and interesting lecture;

© The Complete Work of Charles Darwin Online

if a layman had delivered the same address, he would have done good service in spreading what, as I hope and believe, is to a large extent the truth; but a clergyman in delivering such an address does, as it appears to me, much more good by his power to shake ignorant prejudices, and by setting, if I may be permitted to say so, an admirable example of liberality.

With sincere respect, I beg leave to remain,

Dear Sir, yours faithfully and obliged,

CHARLES DARWIN.

[The references to the subject of expression in the following letter are explained by the fact, that my father's original intention was to give his essay on this subject as a chapter in the 'Descent of Man,' which in its turn grew, as we have seen, out of a proposed chapter in 'Animals and Plants:']

C. Darwin to F. Müller.

Down, February 22, [1869?]

. . . Although you have aided me to so great an extent in many ways, I am going to beg for any information on two other subjects. I am preparing a discussion on "Sexual Selection," and I want much to know how low down in the animal scale sexual selection of a particular kind extends. Do you know of any lowly organised animals, in which the sexes are separated, and in which the male differs from the female in arms of offence, like the horns and tusks of male mammals, or in gaudy plumage and ornaments, as with birds and butterflies? I do not refer to secondary sexual characters, by which the male is able to discover the female, like the plumed antennæ of moths, or by which the male is enabled to seize the female, like the curious pincers described by you in some of the lower Crustaceans. But what I want to know is, how low in the scale sexual differences occur which require some degree of self-consciousness in the males, as weapons by

© The Complete Work of Charles Darwin Online

which they fight for the female, or ornaments which attract the opposite sex. Any differences between males and females which follow different habits of life would have to be excluded. I think you will easily see what I wish to learn. *A priori*, it would never have been anticipated that insects would have been attracted by the beautiful colouring of the opposite sex, or by the sounds emitted by the various musical instruments of the male Orthoptera. I know no one so likely to answer this question as yourself, and should be grateful for any information, however small.

My second subject refers to expression of countenance, to which I have long attended, and on which I feel a keen interest; but to which, unfortunately, I did not attend, when I had the opportunity of observing various races of man. It has occurred to me that you might, without much trouble, make a *few* observations for me, in the course of some months, on Negroes, or possibly on native South Americans, though I care most about Negroes; accordingly I enclose some questions as a guide, and if you could answer me even one or two I should feel truly obliged. I am thinking of writing a little essay on the Origin of Mankind, as I have been taunted with concealing my opinions, and I should do this immediately after the completion of my present book. In this case I should add a chapter on the cause or meaning of expression. . . .

[The remaining letters of this year deal chiefly with the books, reviews, &c., which interested him.]

C. Darwin to H. Thiel.

Down, February 25, 1869.

DEAR SIR,—On my return home after a short absence, I found your very courteous note, and the pamphlet,* and I

* 'Ueber einige Formen der Landwirthschaftlichen Genossen-schaften.' By Dr. H. Thiel, then of the Agricultural Station at Poppelsdorf.

© The Complete Work of Charles Darwin Online

hasten to thank you for both, and for the very honourable mention which you make of my name. You will readily believe how much interested I am in observing that you apply to moral and social questions analogous views to those which I have used in regard to the modification of species. It did not occur to me formerly that my views could be extended to such widely different, and most important, subjects. With much respect, I beg leave to remain, dear Sir,

Yours faithfully and obliged,

CHARLES DARWIN.

C. Darwin to T. H. Huxley.

Down, March 19 [1869].

MY DEAR HUXLEY,—Thanks for your 'Address.'* People complain of the unequal distribution of wealth, but it is a much greater shame and injustice that any one man should have the power to write so many brilliant essays as you have lately done. There is no one who writes like you. . . . If I were in your shoes, I should tremble for my life. I agree with all you say, except that I must think that you draw too great a distinction between the evolutionists and the uniformitarians.

I find that the few sentences which I have sent to press in the 'Origin' about the age of the world will do fairly well . . .

Ever yours,

C. DARWIN.

C. Darwin to A. R. Wallace.

Down, March 22 [1869].

MY DEAR WALLACE,—I have finished your book; † it seems to me excellent, and at the same time most pleasant to

* In his 'Anniversary Address' to the Geological Society, 1869, Mr. Huxley criticised Sir William Thomson's paper ('Trans. Geol. Soc. Glasgow,' vol. iii.) "On Geological Time."

† 'The Malay Archipelago,' &c. 1869.

© The Complete Work of Charles Darwin Online

read. That you ever returned alive is wonderful after all your risks from illness and sea voyages, especially that most interesting one to Waigiou and back. Of all the impressions which I have received from your book, the strongest is that your perseverance in the cause of science was heroic. Your descriptions of catching the splendid butterflies have made me quite envious, and at the same time have made me feel almost young again, so vividly have they brought before my mind old days when I collected, though I never made such captures as yours. Certainly collecting is the best sport in the world. I shall be astonished if your book has not a great success; and your splendid generalizations on Geographical Distribution, with which I am familiar from your papers, will be new to most of your readers. I think I enjoyed most the Timor case, as it is best demonstrated: but perhaps Celebes is really the most valuable. I should prefer looking at the whole Asiatic continent as having formerly been more African in its fauna, than admitting the former existence of a continent across the Indian Ocean. . . .

[The following letter refers to Mr. Wallace's article in the April number of the 'Quarterly Review,'* 1869, which to a large extent deals with the tenth edition of Sir Charles Lyell's 'Principles,' published in 1867 and 1868. The review contains a striking passage on Sir Charles Lyell's confession of evolutionary faith in the tenth edition of his 'Principles,' which is worth quoting: "The history of science hardly presents so striking an instance of youthfulness of mind in advanced life as is shown by this abandonment of opinions so long held and so powerfully advocated; and if we bear in mind the extreme caution, combined with the ardent love of truth

* My father wrote to Mr. Murray: "The article by Wallace is inimitably good, and it is a great triumph that such an article should appear in the 'Quarterly,' and will make the Bishop of Oxford and —— gnash their teeth."

© The Complete Work of Charles Darwin Online

which characterize every work which our author has produced, we shall be convinced that so great a change was not decided on without long and anxious deliberation, and that the views now adopted must indeed be supported by arguments of overwhelming force. If for no other reason than that Sir Charles Lyell in his tenth edition has adopted it, the theory of Mr. Darwin deserves an attentive and respectful consideration from every earnest seeker after truth."]

C. Darwin to A. R. Wallace.

Down, April 14, 1869.

MY DEAR WALLACE,—I have been wonderfully interested by your article, and I should think Lyell will be much gratified by it. I declare if I had been editor, and had the power of directing you, I should have selected for discussion the very points which you have chosen. I have often said to younger geologists (for I began in the year 1830) that they did not know what a revolution Lyell had effected; nevertheless, your extracts from Cuvier have quite astonished me. Though not able really to judge, I am inclined to put more confidence in Croll than you seem to do; but I have been much struck by many of your remarks on degradation. Thomson's views of the recent age of the world have been for some time one of my sorest troubles, and so I have been glad to read what you say. Your exposition of Natural Selection seems to me inimitably good; there never lived a better expounder than you. I was also much pleased at your discussing the difference between our views and Lamarck's. One sometimes sees the odious expression, "Justice to myself compels me to say," &c., but you are the only man I ever heard of who persistently does himself an injustice, and never demands justice. Indeed, you ought in the review to have alluded to your paper in the 'Linnean Journal,' and I feel sure all our friends will agree in this. But you cannot

© The Complete Work of Charles Darwin Online

"Burke" yourself, however much you may try, as may be seen in half the articles which appear. I was asked but the other day by a German professor for your paper, which I sent him. Altogether I look at your article as appearing in the 'Quarterly' as an immense triumph for our cause. I presume that your remarks on Man are those to which you alluded in your note. If you had not told me I should have thought that they had been added by some one else. As you expected, I differ grievously from you, and I am very sorry for it. I can see no necessity for calling in an additional and proximate cause in regard to man.* But the subject is too long for a letter. I have been particularly glad to read your discussion because I am now writing and thinking much about man.

I hope that your Malay book sells well; I was extremely pleased with the article in the 'Quarterly Journal of Science,' inasmuch as it is thoroughly appreciative of your work: alas! you will probably agree with what the writer says about the uses of the bamboo.

I hear that there is also a good article in the *Saturday Review*, but have heard nothing more about it. Believe me, my dear Wallace,

Yours ever sincerely,
CH. DARWIN.

C. Darwin to C. Lyell.

Down, May 4 [1869].

MY DEAR LYELL,—I have been applied to for some photo-

* Mr. Wallace points out that any one acquainted merely with the "unaided productions of nature," might reasonably doubt whether a dray-horse, for example, could have been developed by the power of man directing the "action of the laws of variation, multiplication, and survival, for his own purpose. We know, however, that this has been done, and we must therefore admit the possibility that in the development of the human race, a higher intelligence has guided the same laws for nobler ends."

© The Complete Work of Charles Darwin Online

graphs (carte de visite) to be copied to ornament the diplomas of honorary members of a new Society in Servia! Will you give me one for this purpose? I possess only a full-length one of you in my own album, and the face is too small, I think, to be copied.

I hope that you get on well with your work, and have satisfied yourself on the difficult point of glacier lakes. Thank heaven, I have finished correcting the new edition of the 'Origin,' and am at my old work of Sexual Selection.

Wallace's article struck me as *admirable;* how well he brought out the revolution which you effected some 30 years ago. I thought I had fully appreciated the revolution, but I was astounded at the extracts from Cuvier. What a good sketch of natural selection! but I was dreadfully disappointed about Man, it seems to me incredibly strange . . . ; and had I not known to the contrary, would have sworn it had been inserted by some other hand. But I believe that you will not agree quite in all this.

My dear Lyell, ever yours sincerely,

C. DARWIN.

C. Darwin to J. L. A. de Quatrefages.

Down, May 28 [1869 or 1870].

DEAR SIR,—I have received and read your volume,* and am much obliged for your present. The whole strikes me as a wonderfully clear and able discussion, and I was much interested by it to the last page. It is impossible that any account of my views could be fairer, or, as far as space permitted, fuller, than that which you have given. The way in which you repeatedly mention my name is most gratifying to me. When I had finished the second part, I thought that you had stated the case so favourably that you would make

* Essays reprinted from the 'Revue des Deux Mondes,' under the title 'Histoire Naturelle Générale,' &c., 1869.

© The Complete Work of Charles Darwin Online

more converts on my side than on your own side. On reading the subsequent parts I had to change my sanguine view. In these latter parts many of your strictures are severe enough, but all are given with perfect courtesy and fairness. I can truly say I would rather be criticised by you in this manner than praised by many others. I agree with some of your criticisms, but differ entirely from the remainder; but I will not trouble you with any remarks. I may, however, say, that you must have been deceived by the French translation, as you infer that I believe that the Parus and the Nuthatch (or Sitta) are related by direct filiation. I wished only to show, by an imaginary illustration, how either instincts or structures might first change. If you had seen *Canis Magellanicus* alive you would have perceived how foxlike its appearance is, or if you had heard its voice, I think that you would never have hazarded the idea that it was a domestic dog run wild; but this does not much concern me. It is curious how nationality influences opinion; a week hardly passes without my hearing of some naturalist in Germany who supports my views, and often puts an exaggerated value on my works; whilst in France I have not heard of a single zoologist, except M. Gaudry (and he only partially), who supports my views. But I must have a good many readers as my books are translated, and I must hope, notwithstanding your strictures, that I may influence some embryo naturalists in France.

You frequently speak of my good faith, and no compliment can be more delightful to me, but I may return you the compliment with interest, for every word which you write bears the stamp of your cordial love for the truth. Believe me, dear Sir, with sincere respect,

Yours very faithfully,
CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to T. H. Huxley.

Down, October 14, 1869.

MY DEAR HUXLEY,—I have been delighted to see your review of Häckel,* and as usual you pile honours high on my head. But I write now (*requiring no answer*) to groan a little over what you have said about rudimentary organs.† Many heretics will take advantage of what you have said. I cannot but think that the explanation given at p. 541 of the last edition of the 'Origin,' of the long retention of rudimentary organs and of their greater relative size during early life, is satisfactory. Their final and complete abortion seems to me a much greater difficulty. Do look in my 'Variations under Domestication,' vol. ii. p. 397, at what Pangenesis suggests on this head, though I did not dare to put it in the 'Origin.' The passage bears also a little on the struggle between the molecules or gemmules.‡ There is likewise a word or two indirectly bearing on this subject at pp. 394–395. It won't take you five minutes, so do look at these passages. I am very glad that you have been bold enough to give your idea about Natural Selection amongst the molecules, though I cannot quite follow you.

* A review of Haeckel's 'Schöpfungs-Geschichte.' The *Academy*, 1869. Reprinted in 'Critiques and Addresses,' p. 303.

† In discussing Teleology and Haeckel's "Dysteleology," Prof. Huxley says:—"Such cases as the existence of lateral rudiments of toes, in the foot of a horse, place us in a dilemma. For either these rudiments are of no use to the animals, in which case . . . they surely ought to have disappeared; or they are of some use to the animal, in which case they are of no use as arguments against Teleology."—'Critiques and Addresses,' p. 308.

‡ "It is a probable hypothesis, that what the world is to organisms in general, each organism is to the molecules of which it is composed. Multitudes of these having diverse tendencies, are competing with one another for opportunity to exist and multiply; and the organism, as a whole, is as much the product of the molecules which are victorious as the Fauna, or Flora, of a country is the product of the victorious organic beings in it."—'Critiques and Addresses,' p. 309.

© The Complete Work of Charles Darwin Online

1870.

[My father wrote in his Diary:—"The whole of this year [1870] at work on the 'Descent of Man.' . . . Went to Press August 30, 1870."

The letters are again of miscellaneous interest, dealing, not only with his work, but also serving to indicate the course of his reading.]

C. Darwin to E. Ray Lankester.

Down, March 15 [1870].

MY DEAR SIR,—I do not know whether you will consider me a very troublesome man, but I have just finished your book,* and cannot resist telling you how the whole has much interested me. No doubt, as you say, there must be much speculation on such a subject, and certain results cannot be reached; but all your views are highly suggestive, and to my mind that is high praise. I have been all the more interested, as I am now writing on closely allied though not quite identical points. I was pleased to see you refer to my much despised child, 'Pangenesis,' who I think will some day, under some better nurse, turn out a fine stripling. It has also pleased me to see how thoroughly you appreciate (and I do not think that this is general with the men of science) H. Spencer; I suspect that hereafter he will be looked at as by far the greatest living philosopher in England; perhaps equal to any that have lived. But I have no business to trouble you with my notions. With sincere thanks for the interest which your work has given me,

I remain, yours very faithfully,
CH. DARWIN.

[The next letter refers to Mr. Wallace's 'Natural Selec-

* 'Comparative Longevity.'

© The Complete Work of Charles Darwin Online

tion' (1870), a collection of essays reprinted with certain alterations of which a list is given in the volume:]

C. Darwin to A. R. Wallace.

Down, April 20 [1870].

MY DEAR WALLACE,—I have just received your book, and read the preface. There never has been passed on me, or indeed on any one, a higher eulogium than yours. I wish that I fully deserved it. Your modesty and candour are very far from new to me. I hope it is a satisfaction to you to reflect—and very few things in my life have been more satisfactory to me—that we have never felt any jealousy towards each other, though in one sense rivals. I believe that I can say this of myself with truth, and I am absolutely sure that it is true of you.

You have been a good Christian to give a list of your additions, for I want much to read them, and I should hardly have had time just at present to have gone through all your articles. Of course I shall immediately read those that are new or greatly altered, and I will endeavour to be as honest as can reasonably be expected. Your book looks remarkably well got up.

Believe me, my dear Wallace, to remain,

Yours very cordially,

CH. DARWIN.

[Here follow one or two letters indicating the progress of the 'Descent of Man;' the woodcuts referred to were being prepared for that work:]

*C. Darwin to A. Günther.**

March 23, [1870?]

DEAR GÜNTHER,—As I do not know Mr. Ford's address, will you hand him this note, which is written solely to express

* Dr. Günther, Keeper of Zoology in the British Museum.

© The Complete Work of Charles Darwin Online

my unbounded admiration of the woodcuts. I fairly gloat over them. The only evil is that they will make all the other woodcuts look very poor! They are all excellent, and for the feathers I declare I think it the most wonderful woodcut I ever saw; I cannot help touching it to make sure that it is smooth. How I wish to see the two other, and even more important, ones of the feathers, and the four [of] reptiles, &c. Once again accept my very sincere thanks for all your kindness. I am greatly indebted to Mr. Ford. Engravings have always hitherto been my greatest misery, and now they are a real pleasure to me.

Yours very sincerely,
CH. DARWIN.

P.S.—I thought I should have been in press by this time, but my subject has branched off into sub-branches, which have cost me infinite time, and heaven knows when I shall have all my MS. ready; but I am never idle.

C. Darwin to A. Günther.

May 15 [1870].

MY DEAR DR. GÜNTHER,—Sincere thanks. Your answers are wonderfully clear and complete. I have some analogous questions on reptiles, &c., which I will send in a few days, and then I think I shall cause no more trouble. I will get the books you refer me to. The case of the Solenostoma* is magnificent, so exactly analogous to that of those birds in which the female is the more gay, but ten times better for me, as she is the incubator. As I crawl on with the successive

* In most of the Lophobranchii the male has a marsupial sack in which the eggs are hatched, and in these species the male is slightly brighter coloured than the female. But in Solenostoma the female is the hatcher, and is also the more brightly coloured.—'Descent of Man,' ii. 21.

© The Complete Work of Charles Darwin Online

classes I am astonished to find how similar the rules are about the nuptial or "wedding dress" of all animals. The subject has begun to interest me in an extraordinary degree; but I must try not to fall into my common error of being too speculative. But a drunkard might as well say he would drink a little and not too much! My essay, as far as fishes, batrachians and reptiles are concerned, will be in fact yours, only written by me. With hearty thanks,

Yours very sincerely,

CH. DARWIN.

[The following letter is of interest, as showing the excessive care and pains which my father took in forming his opinion on a difficult point:]

C. Darwin to A. R. Wallace.

Down, September 23 [undated].

MY DEAR WALLACE,—I am very much obliged for all your trouble in writing me your long letter, which I will keep by me and ponder over. To answer it would require at least 200 folio pages! If you could see how often I have re-written some pages you would know how anxious I am to arrive as near as I can to the truth. I lay great stress on what I know takes place under domestication; I think we start with different fundamental notions on inheritance. I find it is most difficult, but not I think impossible, to see how, for instance, a few red feathers appearing on the head of a male bird, and which *are at first transmitted to both sexes*, could come to be transmitted to males alone. It is not enough that females should be produced from the males with red feathers, which should be destitute of red feathers; but these females must have a *latent tendency* to produce such feathers, otherwise they would cause deterioration in the red head-feathers of their male offspring. Such

© The Complete Work of Charles Darwin Online

latent tendency would be shown by their producing the red feathers when old, or diseased in their ovaria. But I have no difficulty in making the whole head red if the few red feathers in the male from the first tended to be sexually transmitted. I am quite willing to admit that the female may have been modified, either at the same time or subsequently, for protection by the accumulation of variations limited in their transmission to the female sex. I owe to your writings the consideration of this latter point. But I cannot yet persuade myself that females *alone* have often been modified for protection. Should you grudge the trouble briefly to tell me, whether you believe that the plainer head and less bright colours of ♀ chaffinch,* the less red on the head and less clean colours of ♀ goldfinch, the much less red on the breast of ♀ bullfinch, the paler crest of golden-crested wren, &c., have been acquired by them for protection. I cannot think so, any more than I can that the considerable differences between ♀ and ♂ house sparrow, or much greater brightness of ♂ *Parus cœruleus* (both of which build under cover) than of ♀ *Parus*, are related to protection. I even misdoubt much whether the less blackness of ♀ blackbird is for protection.

Again, can you give me reasons for believing that the moderate differences between the female pheasant, the female *Gallus bankiva*, the female of black grouse, the pea-hen, the female partridge, have all special references to protection under slightly different conditions? I, of course, admit that they are all protected by dull colours, derived, as I think, from some dull-ground progenitor; and I account partly for their difference by partial transference of colour from the male, and by other means too long to specify; but I earnestly wish to see reason to believe that each is specially adapted for concealment to its environment.

I grieve to differ from you, and it actually terrifies me and

* The symbols ♂, ♀, stand for male and female.

© The Complete Work of Charles Darwin Online

makes me constantly distrust myself. I fear we shall never quite understand each other. I value the cases of bright-coloured, incubating male fishes, and brilliant female butterflies, solely as showing that one sex may be made brilliant without any necessary transference of beauty to the other sex; for in these cases I cannot suppose that beauty in the other sex was checked by selection.

I fear this letter will trouble you to read it. A very short answer about your belief in regard to the ♀ finches and gallinaceæ would suffice.

Believe me, my dear Wallace,
Yours very sincerely,
CH. DARWIN.

C. Darwin to J. D. Hooker.

Down, May 25 [1870].

. . . . Last Friday we all went to the Bull Hotel at Cambridge to see the boys, and for a little rest and enjoyment. The backs of the Colleges are simply paradisaical. On Monday I saw Sedgwick, who was most cordial and kind; in the morning I thought his brain was enfeebled; in the evening he was brilliant and quite himself. His affection and kindness charmed us all. My visit to him was in one way unfortunate; for after a long sit he proposed to take me to the museum, and I could not refuse, and in consequence he utterly prostrated me; so that we left Cambridge next morning, and I have not recovered the exhaustion yet. Is it not humiliating to be thus killed by a man of eighty-six, who evidently never dreamed that he was killing me? As he said to me, "Oh, I consider you as a mere baby to me!" I saw Newton several times, and several nice friends of F.'s. But Cambridge without dear Henslow was not itself; I tried to get to the two old houses, but it was too far for me. . . .

© The Complete Work of Charles Darwin Online

*C. Darwin to B. J. Sulivan.**

Down, June 30 [1870].

MY DEAR SULIVAN,—It was very good of you to write to me so long a letter, telling me much about yourself and your children, which I was extremely glad to hear. Think what a benighted wretch I am, seeing no one and reading but little in the newspapers, for I did not know (until seeing the paper of your Natural History Society) that you were a K.C.B. Most heartily glad I am that the Government have at last appreciated your most just claim for this high distinction. On the other hand, I am sorry to hear so poor an account of your health; but you were surely very rash to do all that you did and then pass through so exciting a scene as a ball at the Palace. It was enough to have tired a man in robust health. Complete rest will, however, I hope, quite set you up again. As for myself, I have been rather better of late, and if nothing disturbs me I can do some hours' work every day. I shall this autumn publish another book partly on man, which I dare say many will decry as very wicked. I could have travelled to Oxford, but could no more have withstood the excitement of a commemoration † than I could a ball at Buckingham Palace. Many thanks for your kind remarks about my boys. Thank God, all give me complete satisfaction; my fourth stands second at Woolwich, and will be an Engineer Officer at Christmas. My wife desires to be very kindly remembered to Lady Sulivan, in which I very sincerely join, and in congratulation about your daughter's marriage. We are at present solitary, for all our younger children are

* Admiral Sir James Sulivan was a lieutenant on board the *Beagle*.

† This refers to an invitation to receive the honorary degree of D.C.L. He was one of those nominated for the degree by Lord Salisbury on assuming the office of Chancellor of the University of Oxford. The fact that the honour was declined on the score of ill-health was published in the *Oxford University Gazette*, June 17, 1870.

© The Complete Work of Charles Darwin Online

gone a tour in Switzerland. I had never heard a word about the success of the T. del Fuego mission. It is most wonderful, and shames me, as I always prophesied utter failure. It is a grand success. I shall feel proud if your Committee think fit to elect me an honorary member of your society. With all good wishes and affectionate remembrances of ancient days,

Believe me, my dear Sulivan,
Your sincere friend,
CH. DARWIN.

[My father's connection with the South American Mission, which is referred to in the above letter, has given rise to some public comment, and has been to some extent misunderstood. The Archbishop of Canterbury, speaking at the annual meeting of the South American Missionary Society, April 21st, 1885,* said that the Society "drew the attention of Charles Darwin, and made him, in his pursuit of the wonders of the kingdom of nature, realise that there was another kingdom just as wonderful and more lasting." Some discussion on the subject appeared in the *Daily News* of April 23rd, 24th, 29th, 1885, and finally Admiral Sir James Sulivan, on April 24th, wrote to the same journal, giving a clear account of my father's connection with the Society:—

"Your article in the *Daily News* of yesterday induces me to give you a correct statement of the connection between the South American Missionary Society and Mr. Charles Darwin, my old friend and shipmate for five years. I have been closely connected with the Society from the time of Captain Allen Gardiner's death, and Mr. Darwin had often expressed to me his conviction that it was utterly useless to send Missionaries to such a set of savages as the Fuegians, prob-

* I quote a 'Leaflet,' published by the Society.

© The Complete Work of Charles Darwin Online

ably the very lowest of the human race. I had always replied that I did not believe any human beings existed too low to comprehend the simple message of the Gospel of Christ. After many years, I think about 1869,* but I cannot find the letter, he wrote to me that the recent accounts of the Mission proved to him that he had been wrong and I right in our estimates of the native character, and the possibility of doing them good through Missionaries; and he requested me to forward to the Society an enclosed cheque for £5, as a testimony of the interest he took in their good work. On June 6th, 1874, he wrote: 'I am very glad to hear so good an account of the Fuegians, and it is wonderful.' On June 10th, 1879: 'The progress of the Fuegians is wonderful, and had it not occurred would have been to me quite incredible.' On January 3rd, 1880: 'Your extracts [from a journal] 'about the Fuegians are extremely curious, and have interested me much. I have often said that the progress of Japan was the greatest wonder in the world, but I declare that the progress of Fuegia is almost equally wonderful.' On March 20th, 1881: 'The account of the Fuegians interested not only me, but all my family. It is truly wonderful what you have heard from Mr. Bridges about their honesty and their language. I certainly should have predicted that not all the Missionaries in the world could have done what has been done.' On December 1st, 1881, sending me his annual subscription to the Orphanage at the Mission Station, he wrote: 'Judging from the *Missionary Journal*, the Mission in Tierra del Fuego seems going on quite wonderfully well.' "]

* It seems to have been in 1867.

© The Complete Work of Charles Darwin Online

C. Darwin to John Lubbock.

Down, July 17, 1870.

MY DEAR LUBBOCK,—As I hear that the Census will be brought before the House to-morrow, I write to say how much I hope that you will express your opinion on the desirability of queries in relation to consanguineous marriages being inserted. As you are aware, I have made experiments on the subject during several years; *and it is my clear conviction that there is now ample evidence of the existence of a great physiological law, rendering an enquiry with reference to mankind of much importance. In England and many parts of Europe the marriages of cousins are objected to from their supposed injurious consequences; but this belief rests on no direct evidence. It is therefore manifestly desirable that the belief should either be proved false, or should be confirmed*, so that in this latter case the marriages of cousins might be discouraged. If the proper queries are inserted, the returns would show whether married cousins have in their households on the night of the census as many children as have parents who are not related; and should the number prove fewer, we might safely infer either lessened fertility in the parents, or which is more probable, lessened vitality in the offspring.

It is, moreover, much to be wished that the truth of the often repeated assertion that consanguineous marriages lead to deafness, and dumbness, blindness, &c., should be ascertained; and all such assertions could be easily tested by the returns from a single census.

Believe me,

Yours very sincerely,

CHARLES DARWIN.

[When the Census Act was passing through the House of Commons, Sir John Lubbock and Dr. Playfair attempted to carry out this suggestion. The question came to a division, which was lost, but not by many votes.

© The Complete Work of Charles Darwin Online

The subject of cousin marriages was afterwards investigated by my brother.* The results of this laborious piece of work were negative ; the author sums up in the sentence :—

"My paper is far from giving anything like a satisfactory solution of the question as to the effects of consanguineous marriages, but it does, I think, show that the assertion that this question has already been set at rest, cannot be substantiated."]

* "Marriages between First Cousins in England, and their Effects." By George Darwin. 'Journal of the Statistical Society,' June 1875.

© The Complete Work of Charles Darwin Online

CHAPTER IV.

PUBLICATION OF THE 'DESCENT OF MAN.'

THE 'EXPRESSION OF THE EMOTIONS.'

1871–1873.

[THE last revise of the 'Descent of Man' was corrected on January 15th, 1871, so that the book occupied him for about three years. He wrote to Sir J. Hooker: "I finished the last proofs of my book a few days ago; the work half-killed me, and I have not the most remote idea whether the book is worth publishing."

He also wrote to Dr. Gray:—

"I have finished my book on the 'Descent of Man,' &c., and its publication is delayed only by the Index: when published, I will send you a copy, but I do not know that you will care about it. Parts, as on the moral sense, will, I dare say, aggravate you, and if I hear from you, I shall probably receive a few stabs from your polished stiletto of a pen."

The book was published on February 24, 1871. 2500 copies were printed at first, and 5000 more before the end of the year. My father notes that he received for this edition £1470. The letters given in the present chapter deal with its reception, and also with the progress of the work on Expression. The letters are given, approximately, in chronological order, an arrangement which necessarily separates

K 2

© The Complete Work of Charles Darwin Online

letters of kindred subject-matter, but gives perhaps a truer picture of the mingled interests and labours of my father's life.

Nothing can give a better idea (in a small compass) of the growth of Evolutionism, and its position at this time, than a quotation from Mr. Huxley *:—

"The gradual lapse of time has now separated us by more than a decade from the date of the publication of the 'Origin of Species;' and whatever may be thought or said about Mr. Darwin's doctrines, or the manner in which he has propounded them, this much is certain, that in a dozen years the 'Origin of Species' has worked as complete a revolution in Biological Science as the 'Principia' did in Astronomy;" and it has done so, "because, in the words of Helmholtz, it contains 'an essentially new creative thought.' And, as time has slipped by, a happy change has come over Mr. Darwin's critics. The mixture of ignorance and insolence which at first characterised a large proportion of the attacks with which he was assailed, is no longer the sad distinction of anti-Darwinian criticism."

A passage in the Introduction to the 'Descent of Man' shows that the author recognised clearly this improvement in the position of Evolutionism. "When a naturalist like Carl Vogt ventures to say in his address, as President of the National Institution of Geneva (1869), 'personne, en Europe au moins, n'ose plus soutenir la création indépendante et de toutes pièces, des espèces,' it is manifest that at least a large number of naturalists must admit that species are the modified descendants of other species; and this especially holds good with the younger and rising naturalists. . . . Of the older and honoured chiefs in natural science, many, unfortunately, are still opposed to Evolution in every form."

In Mr. James Hague's pleasantly written article, "A Reminiscence of Mr. Darwin" ('Harper's Magazine,' October 1884),

* 'Contemporary Review,' 1871.

© The Complete Work of Charles Darwin Online

he describes a visit to my father "early in 1871," * shortly after the publication of the 'Descent of Man.' Mr. Hague represents my father as "much impressed by the general assent with which his views had been received," and as remarking that "everybody is talking about it without being shocked."

Later in the year the reception of the book is described in different language in the 'Edinburgh Review': † "On every side it is raising a storm of mingled wrath, wonder and admiration."

With regard to the subsequent reception of the 'Descent of Man,' my father wrote to Dr. Dohrn, February 3, 1872:—

"I did not know until reading your article,‡ that my 'Descent of Man' had excited so much *furore* in Germany. It has had an immense circulation in this country and in America, but has met the approval of hardly any naturalists as far as I know. Therefore I suppose it was a mistake on my part to publish it; but, anyhow, it will pave the way for some better work."

The book on the 'Expression of the Emotions' was begun on January 17th, 1871, the last proof of the 'Descent of Man' having been finished on January 15th. The rough copy was finished by April 27th, and shortly after this (in June) the work was interrupted by the preparation of a sixth edition of the 'Origin.' In November and December the proofs of the 'Expression' book were taken in hand, and occupied him until the following year, when the book was published.

Some references to the work on Expression have occurred in letters already given, showing that the foundation of the book was, to some extent, laid down for some years before he

* It must have been at the end of February, within a week after the publication of the book.

† July 1871. An adverse criticism. The reviewer sums up by saying that: "Never perhaps in the history of philosophy have such wide generalisations been derived from such a small basis of fact."

‡ In 'Das Ausland.'

© The Complete Work of Charles Darwin Online

began to write it. Thus he wrote to Dr. Asa Gray, April 15, 1867:—

"I have been lately getting up and looking over my old notes on Expression, and fear that I shall not make so much of my hobby-horse as I thought I could; nevertheless, it seems to me a curious subject which has been strangely neglected."

It should, however, be remembered that the subject had been before his mind, more or less, from 1837 or 1838, as I judge from entries in his early note-books. It was in December 1839, that he began to make observations on children.

The work required much correspondence, not only with missionaries and others living among savages, to whom he sent his printed queries, but among physiologists and physicians. He obtained much information from Professor Donders, Sir W. Bowman, Sir James Paget, Dr. W. Ogle, Dr. Crichton Browne, as well as from other observers.

The first letter refers to the 'Descent of Man.']

C. Darwin to A. R. Wallace.

Down, January 30 [1871].

MY DEAR WALLACE,—Your note * has given me very great pleasure, chiefly because I was so anxious not to treat you

* In the note referred to, dated January 27, Mr. Wallace wrote:—"Many thanks for your first volume which I have just finished reading through with the greatest pleasure and interest; and I have also to thank you for the great tenderness with which you have treated me and my heresies."

The heresy is the limitation of natural selection as applied to man. My father wrote ('Descent of Man,' i. p. 137):—"I cannot therefore understand how it is that Mr. Wallace maintains that 'natural selection could only have endowed the savage with a brain a little superior to that of an ape.'" In the above quoted letter Mr. Wallace wrote:—"Your chapters on 'Man' are of intense interest, but as touching my special heresy not as yet altogether convincing, though of course I fully agree with every word and every argument which goes to prove the evolution or development of man out of a lower form."

© The Complete Work of Charles Darwin Online

with the least disrespect, and it is so difficult to speak fairly when differing from any one. If I had offended you, it would have grieved me more than you will readily believe. Secondly, I am greatly pleased to hear that Vol. I. interests you; I have got so sick of the whole subject that I felt in utter doubt about the value of any part. I intended, when speaking of females not having been specially modified for protection, to include the prevention of characters acquired by the ♂ being transmitted to ♀; but I now see it would have been better to have said "specially acted on," or some such term. Possibly my intention may be clearer in Vol. II. Let me say that my conclusions are chiefly founded on the consideration of all animals taken in a body, bearing in mind how common the rules of sexual differences appear to be in all classes. The first copy of the chapter on Lepidoptera agreed pretty closely with you. I then worked on, came back to Lepidoptera, and thought myself compelled to alter it—finished Sexual Selection and for the last time went over Lepidoptera, and again I felt forced to alter it. I hope to God there will be nothing disagreeable to you in Vol. II., and that I have spoken fairly of your views; I am fearful on this head, because I have just read (but not with sufficient care) Mivart's book,* and I feel *absolutely certain* that he meant to be fair (but he was stimulated by theological fervour); yet I do not think he has been quite fair. . . . The part which, I think, will have most influence is where he gives the whole series of cases like that of the whalebone, in which we cannot explain the gradational steps; but such cases have no weight on my mind—if a few fish were extinct, who on earth would have ventured even to conjecture that lungs had originated in a swim-bladder? In such a case as the Thylacine, I think he was bound to say that the resemblance of the jaw to that of the dog is superficial; the number and correspondence and development of teeth being widely different. I think again when speaking

* 'The Genesis of Species,' by St. G. Mivart, 1871.

© The Complete Work of Charles Darwin Online

of the necessity of altering a number of characters together, he ought to have thought of man having power by selection to modify simultaneously or almost simultaneously many points, as in making a greyhound or racehorse—as enlarged upon in my 'Domestic Animals.' Mivart is savage or contemptuous about my "moral sense," and so probably will you be. I am extremely pleased that he agrees with my position, *as far as animal nature is concerned*, of man in the series; or if anything, thinks I have erred in making him too distinct.

Forgive me for scribbling at such length. You have put me quite in good spirits; I did so dread having been unintentionally unfair towards your views. I hope earnestly the second volume will escape as well. I care now very little what others say. As for our not quite agreeing, really in such complex subjects, it is almost impossible for two men who arrive independently at their conclusions to agree fully, it would be unnatural for them to do so.

Yours ever, very sincerely,
CH. DARWIN.

[Professor Haeckel seems to have been one of the first to write to my father about the 'Descent of Man.' I quote from his reply:—

"I must send you a few words to thank you for your interesting, and I may truly say, charming letter. I am delighted that you approve of my book, as far as you have read it. I felt very great difficulty and doubt how often I ought to allude to what you have published; strictly speaking every idea, although occurring independently to me, if published by you previously ought to have appeared as if taken from your works, but this would have made my book very dull reading; and I hoped that a full acknowledgment at the beginning would suffice.* I cannot tell you how glad I am to

* In the introduction to the 'Descent of Man' the author wrote:— "This last naturalist [Haeckel]... has recently... published his 'Na-

© The Complete Work of Charles Darwin Online

find that I have expressed my high admiration of your labours with sufficient clearness; I am sure that I have not expressed it too strongly."]

C. Darwin to A. R. Wallace.

Down, March 16, 1871.

MY DEAR WALLACE,—I have just read your grand review.* It is in every way as kindly expressed towards myself as it is excellent in matter. The Lyells have been here, and Sir C. remarked that no one wrote such good scientific reviews as you, and as Miss Buckley added, you delight in picking out all that is good, though very far from blind to the bad. In all this I most entirely agree. I shall always consider your review as a great honour; and however much my book may hereafter be abused, as no doubt it will be, your review will console me, notwithstanding that we differ so greatly. I will keep your objections to my views in my mind, but I fear that the latter are almost stereotyped in my mind. I thought for long weeks about the inheritance and selection difficulty, and covered quires of paper with notes in trying to get out of it, but could not, though clearly seeing that it would be a great relief if I could. I will confine myself to two or three remarks. I have been much impressed with what you urge against colour,† in the case of insects, having been acquired

türliche Schöpfungs - geschichte,' in which he fully discusses the genealogy of man. If this work had appeared before my essay had been written, I should probably never have completed it. Almost all the conclusions at which I have arrived, I find confirmed by this naturalist, whose knowledge on many points is much fuller than mine."

* *Academy*, March 15, 1871.

† Mr. Wallace says that the pairing of butterflies is probably determined by the fact that one male is stronger-winged, or more pertinacious than the rest, rather than by the choice of the females. He quotes the case of caterpillars which are brightly coloured and yet sexless. Mr. Wallace also makes the good criticism, that the 'Descent of Man' consists of two books mixed together.

© The Complete Work of Charles Darwin Online

through sexual selection. I always saw that the evidence was very weak; but I still think, if it be admitted that the musical instruments of insects have been gained through sexual selection, that there is not the least improbability in colour having been thus gained. Your argument with respect to the denudation of mankind and also to insects, that taste on the part of one sex would have to remain nearly the same during many generations, in order that sexual selection should produce any effect, I agree to; and I think this argument would be sound if used by one who denied that, for instance, the plumes of birds of Paradise had been so gained. I believe you admit this, and if so I do not see how your argument applies in other cases. I have recognised for some short time that I have made a great omission in not having discussed, as far as I could, the acquisition of taste, its inherited nature, and its permanence within pretty close limits for long periods.

[With regard to the success of the 'Descent of Man,' I quote from a letter to Professor Ray Lankester (March 22, 1871):—

"I think you will be glad to hear, as a proof of the increasing liberality of England, that my book has sold wonderfully . . . . and as yet no abuse (though some, no doubt, will come, strong enough), and only contempt even in the poor old *Athenæum*."

As to reviews that struck him he wrote to Mr. Wallace (March 24, 1871):—

"There is a very striking second article on my book in the *Pall Mall*. The articles in the *Spectator* * have also interested me much."

* *Spectator*, March 11 and 18, 1871. With regard to the evolution of conscience the reviewer thinks that my father comes much nearer to the "kernel of the psychological problem" than many of his predecessors. The second article contains a good discussion of the bearing of the book on the question of design, and concludes by finding in it a vindication of Theism more wonderful than that in Paley's 'Natural Theology.'

© The Complete Work of Charles Darwin Online

On March 20 he wrote to Mr. Murray :—

"Many thanks for the *Nonconformist* [March 8, 1871]. I like to see all that is written, and it is of some real use. If you hear of reviewers in out-of-the-way papers, especially the religious, as *Record, Guardian, Tablet*, kindly inform me. It is wonderful that there has been no abuse * as yet, but I suppose I shall not escape. On the whole, the reviews have been highly favourable."

The following extract from a letter to Mr. Murray (April 13, 1871) refers to a review in the *Times*.†

"I have no idea who wrote the *Times* review. He has no knowledge of science, and seems to me a wind-bag full of metaphysics and classics, so that I do not much regard his adverse judgment, though I suppose it will injure the sale."

A review of the 'Descent of Man,' which my father spoke of as "capital," appeared in the *Saturday Review* (Mar. 4 and 11, 1871). A passage from the first notice (Mar. 4) may be quoted in illustration of the broad basis, as regards general acceptance, on which the doctrine of Evolution now stood: "He claims to have brought man himself, his origin and constitution, within that unity which he had previously sought to trace through all lower animal forms. The growth of opinion in the interval, due in chief measure to his own intermediate works, has placed the discussion of this problem

* "I feel a full conviction that my chapter on man will excite attention and plenty of abuse, and I suppose abuse is as good as praise for selling a book."—(From a letter to Mr. Murray, Jan. 31, 1867.)

† *Times*, April 7 and 8, 1871. The review is not only unfavourable as regards the book under discussion, but also as regards Evolution in general, as the following citation will show : "Even had it been rendered highly probable, which we doubt, that the animal creation has been developed into its numerous and widely different varieties by mere evolution, it would still require an independent investigation of overwhelming force and completeness to justify the presumption that man is but a term in this self-evolving series."

© The Complete Work of Charles Darwin Online

in a position very much in advance of that held by it fifteen years ago. The problem of Evolution is hardly any longer to be treated as one of first principles; nor has Mr. Darwin to do battle for a first hearing of his central hypothesis, upborne as it is by a phalanx of names full of distinction and promise, in either hemisphere."

The infolded point of the human ear, discovered by Mr. Woolner, and described in the 'Descent of Man,' seems especially to have struck the popular imagination; my father wrote to Mr. Woolner:—

"The tips to the ears have become quite celebrated. One reviewer ('Nature') says they ought to be called, as I suggested in joke, *Angulus Woolnerianus.** A German is very proud to find that he has the tips well developed, and I believe will send me a photograph of his ears."]

C. Darwin to John Brodie Innes.†

Down, May 29 [1871].

MY DEAR INNES,—I have been very glad to receive your pleasant letter, for, to tell you the truth, I have sometimes wondered whether you would not think me an outcast and a reprobate after the publication of my last book ['Descent'].‡ I do not wonder at all at your not agreeing with me, for a good many professed naturalists do not. Yet when I see in how extraordinary a manner the judgment of naturalists has changed since I published the 'Origin,' I feel convinced that there will be in ten years quite as much unanimity about man, as far as his corporeal frame is concerned. . . .

* 'Nature,' April 6, 1871. The term suggested is *Angulus Woolnerii.*

† Rev. J. Brodie Innes, of Milton Brodie, formerly Vicar of Down.

‡ In a letter of my father's to Mr. Innes, he says:—"We often differed, but you are one of those rare mortals from whom one can differ and yet feel no shade of animosity, and that is a thing which I should feel very proud of, if any one could say it of me."

© The Complete Work of Charles Darwin Online

[The following letters, addressed to Dr. Ogle, deal with the progress of the work on Expression.]

Down, March 12 [1871].

My dear Dr. Ogle,—I have received both your letters, and they tell me all that I wanted to know in the clearest possible way, as, indeed, all your letters have ever done. I thank you cordially. I will give the case of the murderer* in my hobby-horse essay on Expression. I fear that the Eustachian tube question must have cost you a deal of labour; it is quite a complete little essay. It is pretty clear that the mouth is not opened under surprise merely to improve the hearing. Yet why do deaf men generally keep their mouths open? The other day a man here was mimicking a deaf friend, leaning his head forward and sideways to the speaker, with his mouth well open; it was a lifelike representation of a deaf man. Shakespeare somewhere says: 'Hold your breath, listen" or "hark," I forget which. Surprise hurries the breath, and it seems to me one can breathe, at least hurriedly, much quieter through the open mouth than through the nose. I saw the other day you doubted this. As objection is your province at present, I think breathing through the nose ought to come within it likewise, so do pray consider this point, and let me hear your judgment. Consider the nose to be a flower to be fertilised, and then you will make out all about it.† I have had to allude to your paper on 'Sense of Smell;'‡ is the paging right, namely, 1, 2, 3? If not, I protest by all the gods against the plan followed by some, of having presentation copies falsely paged; and so does Rolleston, as he wrote to me the other day. In haste.

Yours very sincerely,

C. Darwin.

* 'Expression of the Emotions,' p. 294. The arrest of a murderer in a hospital, as witnessed by Dr. Ogle.

† Dr. Ogle had corresponded with my father on the subject of the fertilisation of flowers.

‡ Medico-chirurg. Trans. liii.

© The Complete Work of Charles Darwin Online

C. Darwin to W. Ogle.

Down, March 25 [1871].

MY DEAR DR. OGLE,—You will think me a horrid bore, but I beg you, *in relation to a new point for observation*, to imagine as well as you can that you suddenly come across some dreadful object, and act with a sudden little start, a *shudder of horror;* please do this once or twice, and observe yourself as well as you can, and *afterwards* read the rest of this note, which I have consequently pinned down. I find, to my surprise, whenever I act thus my platysma contracts. Does yours? (N.B.—See what a man will do for science; I began this note with a horrid fib, namely, that I want you to attend to a new point.*) I will try and get some persons thus to act who are so lucky as not to know that they even possess this muscle, so troublesome for any one making out about expression. Is a shudder akin to the rigor or shivering before fever? If so, perhaps the platysma could be observed in such cases. Paget told me that he had attended much to shivering, and had written in MS. on the subject, and been much perplexed about it. He mentioned that passing a catheter often causes shivering. Perhaps I will write to him about the platysma. He is always most kind in aiding me in all ways, but he is so overworked that it hurts my conscience to trouble him, for I have a conscience, little as you have reason to think so. Help me if you can, and forgive me. Your murderer case has come in splendidly as the acme of prostration from fear.

Yours very sincerely,
CH. DARWIN.

* The point was doubtless described as a new one, to avoid the possibility of Dr. Ogle's attention being directed to the platysma, a muscle which had been the subject of discussion in other letters.

© The Complete Work of Charles Darwin Online

C. Darwin to Dr. Ogle.

Down, April 29 [1871].

My dear Dr. Ogle,—I am truly obliged for all the great trouble which you have so kindly taken. I am sure you have no cause to say that you are sorry you can give me no definite information, for you have given me far more than I ever expected to get. The action of the platysma is not very important for me, but I believe that you will fully understand (for I have always fancied that our minds were very similar) the intolerable desire I had not to be utterly baffled. Now I know that it sometimes contracts from fear and from shuddering, but not apparently from a prolonged state of fear such as the insane suffer. . . .

[Mr. Mivart's 'Genesis of Species,'—a contribution to the literature of Evolution, which excited much attention,—was published in 1871, before the appearance of the 'Descent of Man.' To this book the following letter (June 21, 1871) from the late Chauncey Wright * to my father, refers :—

"I send . . . revised proofs of an article which will be published in the July number of the 'North American Review,' sending it in the hope that it will interest or even be of greater value to you. Mr. Mivart's book ['Genesis of Species'] of which this article is substantially a review, seems to me a very good background from which to present the considerations which I have endeavoured to set forth in the article, in defence and illustration of the theory of Natural

* Chauncey Wright was born at Northampton, Massachusetts, Sept. 20, 1830, and came of a family settled in that town since 1654. He became in 1852 a computer in the Nautical Almanac office at Cambridge, Mass., and lived a quiet uneventful life, supported by the small stipend of his office, and by what he earned from his occasional articles, as well by a little teaching. He thought and read much on metaphysical subjects, but on the whole with an outcome (as far as the world was concerned) not commensurate to the power of his mind. He seems to have been a man of strong individuality, and to have made a lasting impression on his friends. He died in Sept. 1875.

© The Complete Work of Charles Darwin Online

Selection. My special purpose has been to contribute to the theory by placing it in its proper relations to philosophical inquiries in general." *

With regard to the proofs received from Mr. Wright, my father wrote to Mr. Wallace :]

Down, July 9 [1871].

MY DEAR WALLACE,—I send by this post a review by Chauncey Wright, as I much want your opinion of it as soon as you can send it. I consider you an incomparably better critic than I am. The article, though not very clearly written, and poor in parts from want of knowledge, seems to me admirable. Mivart's book is producing a great effect against Natural Selection, and more especially against me. Therefore if you think the article even somewhat good I will write and get permission to publish it as a shilling pamphlet, together with the MS. additions (enclosed), for which there was not room at the end of the review. . . .

I am now at work at a new and cheap edition of the 'Origin,' and shall answer several points in Mivart's book, and introduce a new chapter for this purpose; but I treat the subject so much more concretely, and I dare say less philosophically, than Wright, that we shall not interfere with each other. You will think me a bigot when I say, after studying Mivart, I was never before in my life so convinced of the *general* (*i.e.* not in detail) truth of the views in the 'Origin.' I grieve to see the omission of the words by Mivart, detected by Wright. † I complained to Mivart that in two cases he quotes only the commencement of sentences by me, and thus

* 'Letters of Chauncey Wright,' by J. B. Thayer. Privately printed, 1878, p. 230.

† 'North American Review' vol. 113, pp. 83, 84. Chauncey Wright points out that the words omitted are "essential to the point on which he [Mr. Mivart] cites Mr. Darwin's authority." It should be mentioned that the passage from which words are omitted is not given within inverted commas by Mr. Mivart.

© The Complete Work of Charles Darwin Online

modifies my meaning; but I never supposed he would have omitted words. There are other cases of what I consider unfair treatment. I conclude with sorrow that though he means to be honourable, he is so bigoted that he cannot act fairly. . . .

C. Darwin to Chauncey Wright.

Down, July 14, 1871.

MY DEAR SIR,—I have hardly ever in my life read an article which has given me so much satisfaction as the review which you have been so kind as to send me. I agree to almost everything which you say. Your memory must be wonderfully accurate, for you know my works as well as I do myself, and your power of grasping other men's thoughts is something quite surprising; and this, as far as my experience goes, is a very rare quality. As I read on I perceived how you have acquired this power, viz. by thoroughly analyzing each word.

. . . Now I am going to beg a favour. Will you provisionally give me permission to reprint your article as a shilling pamphlet? I ask only provisionally, as I have not yet had time to reflect on the subject. It would cost me, I fancy, with advertisements, some £20 or £30; but the worst is that, as I hear, pamphlets never will sell. And this makes me doubtful. Should you think it too much trouble to send me a title *for the chance?* The title ought, I think, to have Mr. Mivart's name on it.

. . . If you grant permission and send a title, you will kindly understand that I will first make further enquiries whether there is any chance of a pamphlet being read.

Pray believe me yours very sincerely obliged,

CH. DARWIN.

[The pamphlet was published in the autumn, and on October 23 my father wrote to Mr. Wright :—

© The Complete Work of Charles Darwin Online

"It pleases me much that you are satisfied with the appearance of your pamphlet. I am sure it will do our cause good service; and this same opinion Huxley has expressed to me. ('Letters of Chauncey Wright,' p. 235.)"]

C. Darwin to A. R. Wallace.

Down, July 12 [1871].

. . . . I feel very doubtful how far I shall succeed in answering Mivart, it is so difficult to answer objections to doubtful points, and make the discussion readable. I shall make only a selection. The worst of it is, that I cannot possibly hunt through all my references for isolated points, it would take me three weeks of intolerably hard work. I wish I had your power of arguing clearly. At present I feel sick of everything, and if I could occupy my time and forget my daily discomforts, or rather miseries, I would never publish another word. But I shall cheer up, I dare say, soon, having only just got over a bad attack. Farewell; God knows why I bother you about myself. I can say nothing more about missing-links than what I have said. I should rely much on pre-silurian times; but then comes Sir W. Thomson like an odious spectre. Farewell.

. . . There is a most cutting review of me in the 'Quarterly';* I have only read a few pages. The skill and style make me think of Mivart. I shall soon be viewed as the most despicable of men. This 'Quarterly Review' tempts me to republish Ch. Wright, even if not read by any one, just to show some one will say a word against Mivart, and that his (*i.e.* Mivart's) remarks ought not to be swallowed without some reflection. . . . God knows whether my strength and spirit will last out to write a chapter versus Mivart and others; I do so hate controversy and feel I shall do it so badly.

* July 1871.

© The Complete Work of Charles Darwin Online

[The above-mentioned 'Quarterly' review was the subject of an article by Mr. Huxley in the November number of the 'Contemporary Review.' Here, also, are discussed Mr. Wallace's 'Contribution to the Theory of Natural Selection,' and the second edition of Mr. Mivart's 'Genesis of Species.' What follows is taken from Mr. Huxley's article. The 'Quarterly' reviewer, though being to some extent an evolutionist, believes that Man "differs more from an elephant or a gorilla, than do these from the dust of the earth on which they tread." The reviewer also declares that my father has "with needless opposition, set at naught the first principles of both philosophy and religion." Mr. Huxley passes from the 'Quarterly' reviewer's further statement, that there is no necessary opposition between evolution and religion, to the more definite position taken by Mr. Mivart, that the orthodox authorities of the Roman Catholic Church agree in distinctly asserting derivative creation, so that "their teachings harmonize with all that modern science can possibly require." Here Mr. Huxley felt the want of that "study of Christian philosophy" (at any rate, in its Jesuitic garb), which Mr. Mivart speaks of, and it was a want he at once set to work to fill up. He was then staying at St. Andrews, whence he wrote to my father:—

"By great good luck there is an excellent library here, with a good copy of Suarez,* in a dozen big folios. Among these I dived, to the great astonishment of the librarian, and looking into them 'as the careful robin eyes the delver's toil' (*vide* 'Idylls'), I carried off the two venerable clasped volumes which were most promising." Even those who know Mr. Huxley's unrivalled power of tearing the heart out of a book must marvel at the skill with which he has made Suarez speak on his side. "So I have come out," he wrote, "in the new character of a defender of Catholic orthodoxy, and upset Mivart out of the mouth of his own prophet."

* The learned Jesuit on whom Mr. Mivart mainly relies.

L 2

© The Complete Work of Charles Darwin Online

The remainder of Mr. Huxley's critique is largely occupied with a dissection of the 'Quarterly' reviewer's psychology, and his ethical views. He deals, too, with Mr. Wallace's objections to the doctrine of Evolution by natural causes when applied to the mental faculties of Man. Finally, he devotes a couple of pages to justifying his description of the 'Quarterly' reviewer's "treatment of Mr. Darwin as alike unjust and unbecoming."

It will be seen that the two following letters were written before the publication of Mr. Huxley's article.]

C. Darwin to T. H. Huxley.

Down, September 21 [1871].

MY DEAR HUXLEY,—Your letter has pleased me in many ways, to a wonderful degree. . . . What a wonderful man you are to grapple with those old metaphysico-divinity books. It quite delights me that you are going to some extent to answer and attack Mivart. His book, as you say, has produced a great effect; yesterday I perceived the reverberations from it, even from Italy. It was this that made me ask Chauncey Wright to publish at my expense his article, which seems to me very clever, though ill-written. He has not knowledge enough to grapple with Mivart in detail. I think there can be no shadow of doubt that he is the author of the article in the 'Quarterly Review' . . . I am preparing a new edition of the 'Origin,' and shall introduce a new chapter in answer to miscellaneous objections, and shall give up the greater part to answer Mivart's cases of difficulty of incipient structures being of no use: and I find it can be done easily. He never states his case fairly, and makes wonderful blunders. . . . The pendulum is now swinging against our side, but I feel positive it will soon swing the other way; and no mortal man will do half as much as you in giving it a start in the right direction, as you did at the first commencement. God forgive me for writing so long and egotistical a letter; but it

© The Complete Work of Charles Darwin Online

is your fault, for you have so delighted me; I never dreamed that you would have time to say a word in defence of the cause which you have so often defended. It will be a long battle, after we are dead and gone. . . . Great is the power of misrepresentation. . . .

C. Darwin to T. H. Huxley.

Down, September 30 [1871].

MY DEAR HUXLEY,—It was very good of you to send the proof-sheets, for I was *very* anxious to read your article. I have been delighted with it. How you do smash Mivart's theology: it is almost equal to your article versus Comte,—* that never can be transcended. . . . But I have been pre-eminently glad to read your discussion on [the 'Quarterly' reviewer's] metaphysics, especially about reason and his definition of it. I felt sure he was wrong, but having only common observation and sense to trust to, I did not know what to say in my second edition of my 'Descent.' Now a footnote and reference to you will do the work. . . . For me, this is one of the most *important* parts of the review. But for *pleasure*, I have been particularly glad that my few words † on the distinction, if it can be so called, between Mivart's two forms of morality, caught your attention. I am so pleased that you take the same view, and give authorities for it; but I searched Mill in vain on this head. How well you argue the whole case. I am mounting climax on climax; for after all there is nothing, I think, better in your whole review than your

* 'Fortnightly Review,' 1869. With regard to the relations of Positivism to Science, my father wrote to Mr. Spencer in 1875: "How curious and amusing it is to see to what an extent the Positivists hate all men of science; I fancy they are dimly conscious what laughable and gigantic blunders their prophet made in predicting the course of science."

† 'Descent of Man,' vol. i. p. 87. A discussion on the question whether an act done impulsively or instinctively can be called moral.

© The Complete Work of Charles Darwin Online

arguments *v.* Wallace on the intellect of savages. I must tell you what Hooker said to me a few years ago. "When I read Huxley, I feel quite infantile in intellect." By Jove I have felt the truth of this throughout your review. What a man you are. There are scores of splendid passages, and vivid flashes of wit. I have been a good deal more than merely pleased by the concluding part of your review; and all the more, as I own I felt mortified by the accusation of bigotry, arrogance, &c., in the 'Quarterly Review.' But I assure you, he may write his worst, and he will never mortify me again.

My dear Huxley, yours gratefully,

CHARLES DARWIN.

C. Darwin to F. Müller.

Haredene, Albury, August 2 [1871].

MY DEAR SIR,—Your last letter has interested me greatly; it is wonderfully rich in facts and original thoughts. First, let me say that I have been much pleased by what you say about my book. It has had a *very large* sale; but I have been much abused for it, especially for the chapter on the moral sense; and most of my reviewers consider the book as a poor affair. God knows what its merits may really be; all that I know is that I did my best. With familiarity I think naturalists will accept sexual selection to a greater extent than they now seem inclined to do. I should very much like to publish your letter, but I do not see how it could be made intelligible, without numerous coloured illustrations, but I will consult Mr. Wallace on this head. I earnestly hope that you keep notes of all your letters and that some day you will publish a book: 'Notes of a Naturalist in S. Brazil,' or some such title. Wallace will hardly admit the possibility of sexual selection with Lepidoptera, and no doubt it is very improbable. Therefore, I am very glad to hear of your cases (which I will quote in the next edition) of the two sets of

© The Complete Work of Charles Darwin Online

Hesperiadæ, which display their wings differently, according to which surface is coloured. I cannot believe that such display is accidental and purposeless. . . .

No fact of your letter has interested me more than that about mimicry. It is a capital fact about the males pursuing the wrong females. You put the difficulty of the first steps in imitation in a most striking and *convincing* manner. Your idea of sexual selection having aided protective imitation interests me greatly, for the same idea had occurred to me in quite different cases, viz. the dulness of all animals in the Galapagos Islands, Patagonia, &c., and in some other cases; but I was afraid even to hint at such an idea. Would you object to my giving some such sentence as follows: "F Müller suspects that sexual selection may have come into play, in aid of protective imitation, in a very peculiar manner, which will appear extremely improbable to those who do not fully believe in sexual selection. It is that the appreciation of certain colour is developed in those species which frequently behold other species thus ornamented." Again let me thank you cordially for your most interesting letter. . . .

*C. Darwin to E. B. Tylor.**

Down [Sept. 24, 1871].

My dear Sir,—I hope that you will allow me to have the pleasure of telling you how greatly I have been interested by your 'Primitive Culture,' now that I have finished it. It seems to me a most profound work, which will be certain to have permanent value, and to be referred to for years to come. It is wonderful how you trace animism from the lower races up to the religious belief of the highest races. It will make me for the future look at religion—a belief in the soul, &c.—from a new point of view. How curious, also, are the survivals or

* Keeper of the Museum, and Reader in Anthropology at Oxford.

© The Complete Work of Charles Darwin Online

rudiments of old customs. . . . You will perhaps be surprised at my writing at so late a period, but I have had the book read aloud to me, and from much ill-health of late, could only stand occasional short reads. The undertaking must have cost you gigantic labour. Nevertheless, I earnestly hope that you may be induced to treat morals in the same enlarged yet careful manner, as you have animism. I fancy from the last chapter that you have thought of this. No man could do the work so well as you, and the subject assuredly is a most important and interesting one. You must now possess references which would guide you to a sound estimation of the morals of savages; and how writers like Wallace, Lubbock, &c. &c., do differ on this head. Forgive me for troubling you, and believe me, with much respect,

Yours very sincerely,

CH. DARWIN.

1872.

[At the beginning of the year the sixth edition of the 'Origin,' which had been begun in June 1871, was nearly completed. The last sheet was revised on January 10, 1872, and the book was published in the course of the month. This volume differs from the previous ones in appearance and size—it consists of 458 pp. instead of 596 pp., and is a few ounces lighter; it is printed on bad paper, in small type, and with the lines unpleasantly close together. It had, however, one advantage over the previous editions, namely that it was issued at a lower price. It is to be regretted that this the final edition of the 'Origin' should have appeared in so unattractive a form; a form which has doubtless kept many readers from the book.

The discussion suggested by the 'Genesis of Species' was perhaps the most important addition to the book. The objection that incipient structures cannot be of use, was dealt with in some detail, because it seemed to the author that this

© The Complete Work of Charles Darwin Online

was the point in Mr. Mivart's book which had struck most readers in England.

It is a striking proof of how wide and general had become the acceptance of his views, that my father found it necessary to insert (sixth edition, p. 424), the sentence: "As a record of a former state of things, I have retained in the foregoing paragraphs and also elsewhere, several sentences which imply that naturalists believe in the separate creation of each species; and I have been much censured for having thus expressed myself. But undoubtedly this was the general belief when the first edition of the present work appeared. . . Now things are wholly changed, and almost every naturalist admits the great principle of evolution."

A small correction introduced into this sixth edition is connected with one of his minor papers: "Note on the habits of the Pampas Woodpecker."* The paper in question was a reply to Mr. Hudson's remarks on the woodpecker in a previous number of the same journal. The last sentence of my father's paper is worth quoting for its temperate tone: "Finally, I trust that Mr. Hudson is mistaken when he says that any one acquainted with the habits of this bird might be induced to believe that I 'had purposely wrested the truth' in order to prove my theory. He exonerates me from this charge; but I should be loath to think that there are many naturalists who, without any evidence, would accuse a fellow-worker of telling a deliberate falsehood to prove his theory." In the fifth edition of the 'Origin,' p. 220, he wrote:—

"Yet as I can assert not only from my own observation, but from that of the accurate Azara, it [the ground woodpecker] never climbs a tree." In the sixth edition, p. 142, the passage runs "in certain large districts it does not climb trees." And he goes on to give Mr. Hudson's statement, that in other regions it does frequent trees.

* Zoolog. Soc. Proc. 1870.

© The Complete Work of Charles Darwin Online

One of the additions in the sixth edition (p. 149), was a reference to Mr. A. Hyatt's and Professor Cope's theory of "acceleration." With regard to this he wrote (October 10, 1872) in characteristic words to Mr. Hyatt:—

"Permit me to take this opportunity to express my sincere regret at having committed two grave errors in the last edition of my 'Origin of Species,' in my allusion to yours and Professor Cope's views on acceleration and retardation of development. I had thought that Professor Cope had preceded you; but I now well remember having formerly read with lively interest, and marked, a paper by you somewhere in my library, on fossil Cephalopods with remarks on the subject. It seems also that I have quite misrepresented your joint view. This has vexed me much. I confess that I have never been able to grasp fully what you wish to show, and I presume that this must be owing to some dulness on my part."

The sixth edition of the 'Origin' being intended as a popular one, was made to include a glossary of technical terms, "given because several readers have complained . . . that some of the terms used were unintelligible to them." The glossary was compiled by Mr. Dallas, and being an excellent collection of clear and sufficient definitions, must have proved useful to many readers.]

C. Darwin to J. L. A. de Quatrefages.

Down, January 15, 1872.

MY DEAR SIR,—I am much obliged for your very kind letter and exertions in my favour. I had thought that the publication of my last book ['Descent of Man'] would have destroyed all your sympathy with me, but though I estimated very highly your great liberality of mind, it seems that I underrated it.

© The Complete Work of Charles Darwin Online

I am gratified to hear that M. Lacaze-Duthiers will vote for me,* for I have long honoured his name. I cannot help regretting that you should expend your valuable time in trying to obtain for me the honour of election, for I fear, judging from the last time, that all your labour will be in vain. Whatever the result may be, I shall always retain the most lively recollection of your sympathy and kindness, and this will quite console me for my rejection.

With much respect and esteem, I remain, dear Sir,

Yours truly obliged,

CHARLES DARWIN.

P.S.—With respect to the great stress which you lay on man walking on two legs, whilst the quadrumana go on all fours, permit me to remind you that no one much values the great difference in the mode of locomotion, and consequently in structure, between seals and the terrestrial carnivora, or between the almost biped kangaroos and other marsupials.

C. Darwin to August Weismann.†

Down, April 5, 1872.

MY DEAR SIR,—I have now read your essay ‡ with very great interest. Your view of the origin of local races through "Amixie," is altogether new to me, and seems to throw an important light on an obscure problem. There is, however, something strange about the periods or endurance of variability. I formerly endeavoured to investigate the subject, not by looking to past time, but to species of the same genus widely distributed; and I found in many cases that all the species, with perhaps one or two exceptions, were variable. It would be a very interesting subject for a con-

* He was not elected as a corresponding member of the French Academy until 1878.

† Professor of Zoology in Freiburg.

‡ 'Ueber den Einfluss der Isolirung auf die Artbildung.' Leipzig, 1872.

© The Complete Work of Charles Darwin Online

chologist to investigate, viz.: whether the species of the same genus were variable during many successive geological formations. I began to make enquiries on this head, but failed in this, as in so many other things, from the want of time and strength. In your remarks on crossing, you do not, as it seems to me, lay nearly stress enough on the increased vigour of the offspring derived from parents which have been exposed to different conditions. I have during the last five years been making experiments on this subject with plants, and have been astonished at the results, which have not yet been published.

In the first part of your essay, I thought that you wasted (to use an English expression) too much powder and shot on M. Wagner;* but I changed my opinion when I saw how admirably you treated the whole case, and how well you used the facts about the Planorbis. I wish I had studied this latter case more carefully. The manner in which, as you show, the different varieties blend together and make a constant whole, agrees perfectly with my hypothetical illustrations.

Many years ago the late E. Forbes described three closely consecutive beds in a secondary formation, each with representative forms of the same fresh-water shells: the case is evidently analogous with that of Hilgendorf,† but the interesting connecting varieties or links were here absent. I rejoice to think that I formerly said as emphatically as I could, that neither isolation nor time by themselves do anything for the modification of species. Hardly anything in your essay has pleased me so much personally, as to find that you believe to a certain extent in sexual selection. As far as I can judge,

* Prof. Wagner has written two essays on the same subject. 'Die Darwin'sche Theorie und das Migrationsgesetz,' in 1868, and 'Ueber den Einfluss der Geographischen Isolirung, &c.', an address to the Bavarian Academy of Sciences at Munich, 1870.

† "Ueber *Planorbis multiformis* im Steinheimer Süsswasser-kalk." 'Monatsbericht' of the Berlin Academy, 1866.

© The Complete Work of Charles Darwin Online

very few naturalists believe in this. I may have erred on many points, and extended the doctrine too far, but I feel a strong conviction that sexual selection will hereafter be admitted to be a powerful agency. I cannot agree with what you say about the taste for beauty in animals not easily varying. It may be suspected that even the habit of viewing differently coloured surrounding objects would influence their taste, and Fritz Müller even goes so far as to believe that the sight of gaudy butterflies might influence the taste of distinct species. There are many remarks and statements in your essay which have interested me greatly, and I thank you for the pleasure which I have received from reading it.

With sincere respect, I remain,

My dear Sir, yours very faithfully,

CHARLES DARWIN.

P.S.—If you should ever be induced to consider the whole doctrine of sexual selection, I think that you will be led to the conclusion, that characters thus gained by one sex are very commonly transferred in a greater or less degree to the other sex.

[With regard to Moritz Wagner's first Essay, my father wrote to that naturalist, apparently in 1868:]

DEAR AND RESPECTED SIR,—I thank you sincerely for sending me your 'Migrationsgesetz, &c.,' and for the very kind and most honourable notice which you have taken of my works. That a naturalist who has travelled into so many and such distant regions, and who has studied animals of so many classes, should, to a considerable extent, agree with me, is, I can assure you, the highest gratification of which I am capable. . . . Although I saw the effects of isolation in the case of islands and mountain-ranges, and knew of a few instances of rivers, yet the greater number of your facts were quite unknown to me. I now see that from the want of

© The Complete Work of Charles Darwin Online

knowledge I did not make nearly sufficient use of the views which you advocate; and I almost wish I could believe in its importance to the same extent with you; for you well show, in a manner which never occurred to me, that it removes many difficulties and objections. But I must still believe that in many large areas all the individuals of the same species have been slowly modified, in the same manner, for instance, as the English race-horse has been improved, that is by the continued selection of the fleetest individuals, without any separation. But I admit that by this process two or more new species could hardly be found within the same limited area; some degree of separation, if not indispensable, would be highly advantageous; and here your facts and views will be of great value. . . .

[The following letter bears on the same subject. It refers to Professor M. Wagner's Essay, published in *Das Ausland*, May 31, 1875:]

C. Darwin to Moritz Wagner.

Down, October 13, 1876.

DEAR SIR,—I have now finished reading your essays, which have interested me in a very high degree, notwithstanding that I differ much from you on various points. For instance, several considerations make me doubt whether species are much more variable at one period than at another, except through the agency of changed conditions. I wish, however, that I could believe in this doctrine, as it removes many difficulties. But my strongest objection to your theory is that it does not explain the manifold adaptations in structure in every organic being—for instance in a Picus for climbing trees and catching insects—or in a Strix for catching animals at night, and so on *ad infinitum*. No theory is in the least satisfactory to me unless it clearly explains such

© The Complete Work of Charles Darwin Online

adaptations. I think that you misunderstand my views on isolation. I believe that all the individuals of a species can be slowly modified within the same district, in nearly the same manner as man effects by what I have called the process of unconscious selection. . . . I do not believe that one species will give birth to two or more new species, as long as they are mingled together within the same district. Nevertheless I cannot doubt that many new species have been simultaneously developed within the same large continental area; and in my 'Origin of Species' I endeavoured to explain how two new species might be developed, although they met and intermingled on the *borders* of their range. It would have been a strange fact if I had overlooked the importance of isolation, seeing that it was such cases as that of the Galapagos Archipelago, which chiefly led me to study the origin of species. In my opinion the greatest error which I have committed, has been not allowing sufficient weight to the direct action of the environment, *i.e.* food, climate, &c., independently of natural selection. Modifications thus caused, which are neither of advantage nor disadvantage to the modified organism, would be especially favoured, as I can now see chiefly through your observations, by isolation in a small area, where only a few individuals lived under nearly uniform conditions.

When I wrote the 'Origin,' and for some years afterwards, I could find little good evidence of the direct action of the environment; now there is a large body of evidence, and your case of the Saturnia is one of the most remarkable of which I have heard. Although we differ so greatly, I hope that you will permit me to express my respect for your long-continued and successful labours in the good cause of natural science.

I remain, dear Sir, yours very faithfully,

CHARLES DARWIN.

[The two following letters are also of interest as bearing

© The Complete Work of Charles Darwin Online

on my father's views on the action of isolation as regards the origin of new species :]

C. Darwin to K. Semper.

Down, November 26, 1878.

My dear Professor Semper,—When I published the sixth edition of the 'Origin,' I thought a good deal on the subject to which you refer, and the opinion therein expressed was my deliberate conviction. I went as far as I could, perhaps too far, in agreement with Wagner ; since that time I have seen no reason to change my mind, but then I must add that my attention has been absorbed on other subjects. There are two different classes of cases, as it appears to me, viz. those in which a species becomes slowly modified in the same country (of which I cannot doubt there are innumerable instances) and those cases in which a species splits into two or three or more new species ; and in the latter case, I should think nearly perfect separation would greatly aid in their "specification," to coin a new word.

I am very glad that you are taking up this subject, for you will be sure to throw much light on it. I remember well, long ago, oscillating much ; when I thought of the Fauna and Flora of the Galapagos Islands I was all for isolation, when I thought of S. America I doubted much. Pray believe me,

Yours very sincerely,

Ch. Darwin.

P.S.—I hope that this letter will not be quite illegible, but I have no amanuensis at present.

C. Darwin to K. Semper.

Down, November 30, 1878.

Dear Professor Semper,—Since writing I have recalled some of the thoughts and conclusions which have passed

© The Complete Work of Charles Darwin Online

through my mind of late years. In North America, in going from north to south or from east to west, it is clear that the changed conditions of life have modified the organisms in the different regions, so that they now form distinct races or even species. It is further clear that in isolated districts, however small, the inhabitants almost always get slightly modified, and how far this is due to the nature of the slightly different conditions to which they are exposed, and how far to mere interbreeding, in the manner explained by Weismann, I can form no opinion. The same difficulty occurred to me (as shown in my 'Variation of Animals and Plants under Domestication') with respect to the aboriginal breeds of cattle, sheep, &c., in the separated districts of Great Britain, and indeed throughout Europe. As our knowledge advances, very slight differences, considered by systematists as of no importance in structure, are continually found to be functionally important; and I have been especially struck with this fact in the case of plants to which my observations have of late years been confined. Therefore it seems to me rather rash to consider the slight differences between representative species, for instance those inhabiting the different islands of the same archipelago, as of no functional importance, and as not in any way due to natural selection. With respect to all adopted structures, and these are innumerable, I cannot see how M. Wagner's view throws any light, nor indeed do I see at all more clearly than I did before, from the numerous cases which he has brought forward, how and why it is that a long isolated form should almost always become slightly modified. I do not know whether you will care about hearing my further opinion on the point in question, for as before remarked I have not attended much of late years to such questions, thinking it prudent, now that I am growing old, to work at easier subjects.

Believe me, yours very sincerely,

CH. DARWIN.

© The Complete Work of Charles Darwin Online

I hope and trust that you will throw light on these points.

P.S.—I will add another remark which I remember occurred to me when I first read M. Wagner. When a species first arrives on a small island, it will probably increase rapidly, and unless all the individuals change instantaneously (which is improbable in the highest degree), the slowly, more or less, modifying offspring must intercross one with another, and with their unmodified parents, and any offspring not as yet modified. The case will then be like that of domesticated animals which have slowly become modified, either by the action of the external conditions or by the process which I have called the *unconscious selection* by man—*i.e.*, in contrast with methodical selection.

[The letters continue the history of the year 1872, which has been interrupted by a digression on Isolation.]

C. Darwin to the Marquis de Saporta.

Down, April 8, 1872.

DEAR SIR,—I thank you very sincerely and feel much honoured by the trouble which you have taken in giving me your reflections on the origin of Man. It gratifies me extremely that some parts of my work have interested you, and that we agree on the main conclusion of the derivation of man from some lower form.

I will reflect on what you have said, but I cannot at present give up my belief in the close relationship of Man to the higher Simiæ. I do not put much trust in any single character, even that of dentition ; but I put the greatest faith in resemblances in many parts of the whole organisation, for I cannot believe that such resemblances can be due to any cause except close blood relationship. That man is closely allied to the higher Simiæ is shown by the classification of

© The Complete Work of Charles Darwin Online

Linnæus, who was so good a judge of affinity. The man who in England knows most about the structure of the Simiæ, namely, Mr. Mivart, and who is bitterly opposed to my doctrines about the derivation of the mental powers, yet has publicly admitted that I have not put man too close to the higher Simiæ, as far as bodily structure is concerned. I do not think the absence of reversions of structure in man is of much weight; C. Vogt, indeed, argues that [the existence of] Micro-cephalous idiots is a case of reversion. No one who believes in Evolution will doubt that the Phocæ are descended from some terrestrial Carnivore. Yet no one would expect to meet with any such reversion in them. The lesser divergence of character in the races of man in comparison with the species of Simiadæ may perhaps be accounted for by man having spread over the world at a much later period than did the Simiadæ. I am fully prepared to admit the high antiquity of man; but then we have evidence, in the Dryopithecus, of the high antiquity of the Anthropomorphous Simiæ.

I am glad to hear that you are at work on your fossil plants, which of late years have afforded so rich a field for discovery. With my best thanks for your great kindness, and with much respect, I remain,

Dear Sir, yours very faithfully,

CHARLES DARWIN.

[In April, 1872, he was elected to the Royal Society of Holland, and wrote to Professor Donders :—

"Very many thanks for your letter. The honour of being elected a foreign member of your Royal Society has pleased me much. The sympathy of his fellow workers has always appeared to me by far the highest reward to which any scientific man can look. My gratification has been not a little increased by first hearing of the honour from you."]

© The Complete Work of Charles Darwin Online

C. Darwin to Chauncey Wright.

Down, June 3, 1872.

MY DEAR SIR,—Many thanks for your article * in the 'North American Review,' which I have read with great interest. Nothing can be clearer than the way in which you discuss the permanence or fixity of species. It never occurred to me to suppose that any one looked at the case as it seems Mr. Mivart does. Had I read his answer to you, perhaps I should have perceived this; but I have resolved to waste no more time in reading reviews of my works or on Evolution, excepting when I hear that they are good and contain new matter. . . . It is pretty clear that Mr. Mivart has come to the end of his tether on this subject.

As your mind is so clear, and as you consider so carefully the meaning of words, I wish you would take some incidental occasion to consider when a thing may properly be said to be effected by the will of man. I have been led to the wish by reading an article by your Professor Whitney *versus* Schleicher. He argues, because each step of change in language is made by the will of man, the whole language so changes; but I do not think that this is so, as man has no intention or wish to change the language. It is a parallel case with what I have called "unconscious selection," which depends on men consciously preserving the best individuals, and thus unconsciously altering the breed.

My dear Sir, yours sincerely,
CHARLES DARWIN.

[Not long afterwards (September) Mr. Chauncey Wright paid

* The proof-sheets of an article which appeared in the July number of the 'North American Review.' It was a rejoinder to Mr. Mivart's reply ('N. Am. Review,' April 1872) to Mr. Chauncey Wright's pamphlet. Chauncey Wright says of it ('Letters,' p. 238):—"It is not properly a rejoinder but a new article, repeating and expounding some of the points of my pamphlet, and answering some of Mr. Mivart's replies incidentally."

© The Complete Work of Charles Darwin Online

a visit to Down,* which he described in a letter † to Miss S. Sedgwick (now Mrs. William Darwin): "If you can imagine me enthusiastic—absolutely and unqualifiedly so, without a *but* or criticism, then think of my last evening's and this morning's talks with Mr. Darwin. . . . I was never so worked up in my life, and did not sleep many hours under the hospitable roof. . . . It would be quite impossible to give by way of report any idea of these talks before and at and after dinner, at breakfast, and at leave-taking; and yet I dislike the egotism of 'testifying' like other religious enthusiasts without any verification, or hint of similar experience."]

C. Darwin to Herbert Spencer.

Bassett, Southampton, June 10 [1872].

Dear Spencer,—I dare say you will think me a foolish fellow, but I cannot resist the wish to express my unbounded admiration of your article ‡ in answer to Mr. Martineau. It is, indeed, admirable, and hardly less so your second article on Sociology (which, however, I have not yet finished): I never believed in the reigning influence of great men on the world's progress; but if asked why I did not believe, I should have been sorely perplexed to have given a good answer. Every one with eyes to see and ears to hear (the number, I

* Mr. and Mrs. C. L. Brace, who had given much of their lives to philanthropic work in New York, also paid a visit at Down in this summer. Some of their work is recorded in Mr. Brace's 'The Dangerous Classes of New York,' and of this book my father wrote to the author:—

"Since you were here my wife has read aloud to me more than half of your work, and it has interested us both in the highest degree, and we shall read every word of the remainder. The facts seem to me very well told, and the inferences very striking. But after all, this is but a weak part of the impression left on our minds by what we have read; for we are both filled with earnest admiration at the heroic labours of yourself and others."

† 'Letters,' p. 246–248.

‡ "Mr. Martineau on Evolution," by Herbert Spencer, 'Contemporary Review,' July 1872.

© The Complete Work of Charles Darwin Online

fear, are not many) ought to bow their knee to you, and I for one do.

Believe me, yours most sincerely,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, July 12 [1872].

MY DEAR HOOKER,—I must exhale and express my joy at the way in which the newspapers have taken up your case. I have seen the *Times*, the *Daily News*, and the *Pall Mall*, and hear that others have taken up the case.

The Memorial has done great good this way, whatever may be the result in the action of our wretched Government. On my soul, it is enough to make one turn into an old honest Tory. . . .

If you answer this, I shall be sorry that I have relieved my feelings by writing.

Yours affectionately,
C. DARWIN.

[The memorial here referred to was addressed to Mr. Gladstone, and was signed by a number of distinguished men, including Sir Charles Lyell, Mr. Bentham, Mr. Huxley, and Sir James Paget. It gives a complete account of the arbitrary and unjust treatment received by Sir J. D. Hooker at the hands of his official chief, the First Commissioner of Works. The document is published in full in 'Nature' (July 11, 1872), and is well worth studying as an example of the treatment which it is possible for science to receive from officialism. As 'Nature' observes, it is a paper which must be read with the greatest indignation by scientific men in every part of the world, and with shame by all Englishmen. The signatories of the memorial conclude by protesting against the expected consequences of Sir Joseph Hooker's persecution—namely his resignation, and the loss of "a man honoured for his integrity,

© The Complete Work of Charles Darwin Online

beloved for his courtesy and kindliness of heart; and who has spent in the public service not only a stainless but an illustrious life."

Happily this misfortune was averted, and Sir Joseph was freed from further molestation.]

C. Darwin to A. R. Wallace.

Down, August 3 [1872].

MY DEAR WALLACE,—I hate controversy, chiefly perhaps because I do it badly; but as Dr. Bree accuses you * of "blundering," I have thought myself bound to send the enclosed letter † to 'Nature,' that is, if you in the least desire it. In this case please post it. If you do not *at all* wish it, I should rather prefer not sending it, and in this case please to tear it up. And I beg you to do the same, if you intend answering Dr. Bree yourself, as you will do it incomparably better than I should. Also please tear it up if you don't like the letter.

My dear Wallace, yours very sincerely,

CH. DARWIN.

* Mr. Wallace had reviewed Dr. Bree's book, 'An Exposition of Fallacies in the Hypothesis of Mr. Darwin,' in 'Nature,' July 25, 1872.

† "Bree on Darwinism." 'Nature,' Aug. 8, 1872. The letter is as follows:—"Permit me to state—though the statement is almost superfluous—that Mr. Wallace, in his review of Dr. Bree's work, gives with perfect correctness what I intended to express, and what I believe was expressed clearly, with respect to the probable position of man in the early part of his pedigree. As I have not seen Dr. Bree's recent work, and as his letter is unintelligible to me, I cannot even conjecture how he has so completely mistaken my meaning: but, perhaps, no one who has read Mr. Wallace's article, or who has read a work formerly published by Dr. Bree on the same subject as his recent one, will be surprised at any amount of misunderstanding on his part.—CHARLES DARWIN."

Aug. 3.

© The Complete Work of Charles Darwin Online

C. Darwin to A. R. Wallace.

Down, August 28, 1872.

MY DEAR WALLACE,—I have at last finished the gigantic job of reading Dr. Bastian's book,* and have been deeply interested by it. You wished to hear my impression, but it is not worth sending.

He seems to me an extremely able man, as, indeed, I thought when I read his first essay. His general argument in favour of Archebiosis † is wonderfully strong, though I cannot think much of some few of his arguments. The result is that I am bewildered and astonished by his statements, but am not convinced, though, on the whole, it seems to me probable that Archebiosis is true. I am not convinced, partly I think owing to the deductive cast of much of his reasoning; and I know not why, but I never feel convinced by deduction, even in the case of H. Spencer's writings. If Dr. Bastian's book had been turned upside down, and he had begun with the various cases of Heterogenesis, and then gone on to organic, and afterwards to saline solutions, and had then given his general arguments, I should have been, I believe, much more influenced. I suspect, however, that my chief difficulty is the effect of old convictions being stereotyped on my brain. I must have more evidence that germs, or the minutest fragments of the lowest forms, are always killed by 212° of Fahr. Perhaps the mere reiteration of the statements given by Dr. Bastian [of] other men, whose judgment I respect, and who have worked long on the lower organisms, would suffice to convince me. Here is a fine confession of intellectual weakness; but what an inexplicable frame of mind is that of belief!

As for Rotifers and Tardigrades being spontaneously gener-

* 'The Beginnings of Life.' H. C. Bastian, 1872.

† That is to say, Spontaneous Generation. For the distinction between Archebiosis and Heterogenesis, see Bastian, chapter vi.

© The Complete Work of Charles Darwin Online

ated, my mind can no more digest such statements, whether true or false, than my stomach can digest a lump of lead. Dr. Bastian is always comparing Archebiosis, as well as growth, to crystallisation; but, on this view, a Rotifer or Tardigrade is adapted to its humble conditions of life by a happy accident, and this I cannot believe. . . . He must have worked with very impure materials in some cases, as plenty of organisms appeared in a saline solution not containing an atom of nitrogen.

I wholly disagree with Dr. Bastian about many points in his latter chapters. Thus the frequency of generalised forms in the older strata seems to me clearly to indicate the common descent with divergence of more recent forms. Notwithstanding all his sneers, I do not strike my colours as yet about Pangenesis. I should like to live to see Archebiosis proved true, for it would be a discovery of transcendent importance; or, if false, I should like to see it disproved, and the facts otherwise explained; but I shall not live to see all this. If ever proved, Dr. Bastian will have taken a prominent part in the work. How grand is the onward rush of science; it is enough to console us for the many errors which we have committed, and for our efforts being overlaid and forgotten in the mass of new facts and new views which are daily turning up.

This is all I have to say about Dr. Bastian's book, and it certainly has not been worth saying. . . .

C. Darwin to A. De Candolle.

Down, December 11, 1872.

MY DEAR SIR—I began reading your new book * sooner than I intended, and when I once began, I could not stop; and now you must allow me to thank you for the very great pleasure which it has given me. I have hardly ever read

* 'Histoire des Sciences et des Savants,' 1873.

© The Complete Work of Charles Darwin Online

anything more original and interesting than your treatment of the causes which favour the development of scientific men. The whole was quite new to me, and most curious. When I began your essay I was afraid that you were going to attack the principle of inheritance in relation to mind, but I soon found myself fully content to follow you and accept your limitations. I have felt, of course, special interest in the latter part of your work, but there was here less novelty to me. In many parts you do me much honour, and everywhere more than justice. Authors generally like to hear what points most strike different readers, so I will mention that of your shorter essays, that on the future prevalence of languages, and on vaccination interested me the most, as, indeed, did that on statistics, and free will. Great liability to certain diseases, being probably liable to atavism, is quite a new idea to me. At p. 322 you suggest that a young swallow ought to be separated, and then let loose in order to test the power of instinct; but nature annually performs this experiment, as old cuckoos migrate in England some weeks before the young birds of the same year. By the way, I have just used the forbidden word "nature," which, after reading your essay, I almost determined never to use again. There are very few remarks in your book to which I demur, but when you back up Asa Gray in saying that all instincts are congenital habits, I must protest.

Finally, will you permit me to ask you a question: have you yourself, or [has] some one who can be quite trusted, observed (p. 322) that the butterflies on the Alps are tamer than those on the lowlands? Do they belong to the same species? Has this fact been observed with more than one species? Are they brightly coloured kinds? I am especially curious about their alighting on the brightly coloured parts of ladies' dresses, more especially because I have been more than once assured that butterflies like bright colours, for instance, in India the scarlet leaves of Pointsettia.

© The Complete Work of Charles Darwin Online

. Once again allow me to thank you for having sent me your work, and for the very unusual amount of pleasure which I have received in reading it.

With much respect, I remain, my dear Sir,

Yours very sincerely,

CHARLES DARWIN.

[The last revise of the 'Expression of the Emotions' was finished on August 22nd, 1872, and he wrote in his Diary:—"Has taken me about twelve months." As usual he had no belief in the possibility of the book being generally successful. The following passage in a letter to Haeckel serves to show that he had felt the writing of this book as a somewhat severe strain:—

"I have finished my little book on 'Expression,' and when it is published in November I will of course send you a copy, in case you would like to read it for amusement. I have resumed some old botanical work, and perhaps I shall never again attempt to discuss theoretical views.

"I am growing old and weak, and no man can tell when his intellectual powers begin to fail. Long life and happiness to you for your own sake and for that of science."

It was published in the autumn. The edition consisted of 7000, and of these 5267 copies were sold at Mr. Murray's sale in November. Two thousand were printed at the end of the year, and this proved a misfortune, as they did not afterwards sell so rapidly, and thus a mass of notes collected by the author was never employed for a second edition during his lifetime.

Among the reviews of the 'Expression of the Emotions' may be mentioned the not unfavourable notices in the *Athenæum*, Nov. 9, 1872, and the *Times*, Dec. 13, 1872. A good review by Mr. Wallace appeared in the 'Quarterly Journal of Science,' Jan. 1873. Mr. Wallace truly remarks that the book exhibits certain "characteristics of the author's mind in

© The Complete Work of Charles Darwin Online

an eminent degree," namely, "the insatiable longing to discover the causes of the varied and complex phenomena presented by living things." He adds that in the case of the author "the restless curiosity of the child to know the 'what for?' the 'why?' and the 'how?' of everything" seems "never to have abated its force."

A writer in one of the theological reviews describes the book as "the most powerful and insidious" of all the author's works.

Professor Alexander Bain criticised the book in a postscript to the 'Senses and the Intellect;' to this essay the following letter refers:]

C. Darwin to Alexander Bain.

Down, October 9, 1873.

MY DEAR SIR,—I am particularly obliged to you for having sent me your essay. Your criticisms are all written in a quite fair spirit, and indeed no one who knows you or your works would expect anything else. What you say about the vagueness of what I have called the direct action of the nervous system, is perfectly just. I felt it so at the time, and even more of late. I confess that I have never been able fully to grasp your principle of spontaneity,* as well as some other of your points, so as to apply them to special cases,

* Professor Bain expounded his theory of Spontaneity in the essay here alluded to. It would be impossible to do justice to it within the limits of a foot-note. The following quotations may give some notion of it:—

"By Spontaneity I understand the readiness to pass into movement, in the absence of all stimulation whatever; the essential requisite being that the nerve-centres and muscles shall be fresh and vigorous. . . . . The gesticulations and the carols of young and active animals are mere overflow of nervous energy; and although they are very apt to concur with pleasing emotion, they have an independent source. . . . . They are not properly movements of expression; they express nothing at all except an abundant stock of physical power."

© The Complete Work of Charles Darwin Online

But as we look at everything from different points of view, it is not likely that we should agree closely.

I have been greatly pleased by what you say about the crying expression and about blushing. Did you read a review in a late 'Edinburgh'?* It was magnificently contemptuous towards myself and many others.

I retain a very pleasant recollection of our sojourn together at that delightful place, Moor Park.

With my renewed thanks, I remain, my dear Sir,

Yours sincerely,

CH. DARWIN.

C. Darwin to Mrs. Haliburton.†

Down, November 1 [1872].

MY DEAR MRS. HALIBURTON,—I dare say you will be surprised to hear from me. My object in writing now is to

* The review on the 'Expression of the Emotions' appeared in the April number of the 'Edinburgh Review,' 1873. The opening sentence is a fair sample of the general tone of the article: "Mr. Darwin has added another volume of amusing stories and grotesque illustrations to the remarkable series of works already devoted to the exposition and defence of the evolutionary hypothesis." A few other quotations may be worth giving. "His one-sided devotion to an *à priori* scheme of interpretation seems thus steadily tending to impair the author's hitherto unrivalled powers as an observer. However this may be, most impartial critics will, we think, admit that there is a marked falling off, both in philosophical tone and scientific interest, in the works produced since Mr. Darwin committed himself to the crude metaphysical conception so largely associated with his name." The article is directed against Evolution as a whole, almost as much as against the doctrines of the book under discussion. We find throughout plenty of that effective style of criticism which consists in the use of such expressions as "dogmatism," "intolerance," "presumptuous," "arrogant;" together with accusations of such various faults as "virtual abandonment of the inductive method," and the use of slang and vulgarisms.

The part of the article which seems to have interested my father is the discussion on the use which he ought to have made of painting and sculpture.

† Mrs. Haliburton is a daughter of my father's old friend, Mr. Owen of Woodhouse. Her husband, Judge Haliburton, was the well-known author of 'Sam Slick.'

© The Complete Work of Charles Darwin Online

say that I have just published a book on the 'Expression of the Emotions in Man and Animals;' and it has occurred to me that you might possibly like to read some parts of it; and I can hardly think that this would have been the case with any of the books which I have already published. So I send by this post my present book. Although I have had no communication with you or the other members of your family for so long a time, no scenes in my whole life pass so frequently or so vividly before my mind as those which relate to happy old days spent at Woodhouse. I should very much like to hear a little news about yourself and the other members of your family, if you will take the trouble to write to me. Formerly I used to glean some news about you from my sisters.

I have had many years of bad health and have not been able to visit anywhere; and now I feel very old. As long as I pass a perfectly uniform life, I am able to do some daily work in Natural History, which is still my passion, as it was in old days, when you used to laugh at me for collecting beetles with such zeal at Woodhouse. Excepting from my continued ill-health, which has excluded me from society, my life has been a very happy one; the greatest drawback being that several of my children have inherited from me feeble health. I hope with all my heart that you retain, at least to a large extent, the famous "Owen constitution." With sincere feelings of gratitude and affection for all bearing the name of Owen, I venture to sign myself,

Yours affectionately,
CHARLES DARWIN.

C. Darwin to Mrs. Haliburton.

Down, November 6 [1872].

MY DEAR SARAH,—I have been very much pleased by your letter, which I must call charming. I hardly ventured

© The Complete Work of Charles Darwin Online

to think that you would have retained a friendly recollection of me for so many years. Yet I ought to have felt assured that you would remain as warm-hearted and as true-hearted as you have ever been from my earliest recollection. I know well how many grievous sorrows you have gone through; but I am very sorry to hear that your health is not good. In the spring or summer, when the weather is better, if you can summon up courage to pay us a visit here, both my wife, as she desires me to say, and myself, would be truly glad to see you, and I know that you would not care about being rather dull here. It would be a real pleasure to me to see you.—Thank you much for telling about your family,—much of which was new to me. How kind you all were to me as a boy, and you especially, and how much happiness I owe to you.

Believe me your affectionate and obliged friend,

CHARLES DARWIN.

P.S.—Perhaps you would like to see a photograph of me now that I am old.

1873.

[The only work (other than botanical) of this year was the preparation of a second edition of the 'Descent of Man,' the publication of which is referred to in the following chapter. This work was undertaken much against the grain, as he was at the time deeply immersed in the manuscript of 'Insectivorous Plants.' Thus he wrote to Mr. Wallace (November 19), "I never in my lifetime regretted an interruption so much as this new edition of the 'Descent.'" And later (in December) he wrote to Mr. Huxley: "The new edition of the 'Descent' has turned out an awful job. It took me ten days merely to glance over letters and reviews with criticisms and new facts. It is a devil of a job."

The work was continued until April 1, 1874, when he was

© The Complete Work of Charles Darwin Online

able to return to his much loved Drosera. He wrote to Mr. Murray:—

"I have at last finished, after above three months as hard work as I have ever had in my life, a corrected edition of the 'Descent,' and I much wish to have it printed off as soon as possible. As it is to be stereotyped I shall never touch it again."

The first of the miscellaneous letters of 1873 refers to a pleasant visit received from Colonel Higginson of Newport, U.S.]

C. Darwin to Thos. Wentworth Higginson.

Down, February 27th [1873].

MY DEAR SIR,—My wife has just finished reading aloud your 'Life with a Black Regiment,' and you must allow me to thank you heartily for the very great pleasure which it has in many ways given us. I always thought well of the negroes, from the little which I have seen of them; and I have been delighted to have my vague impressions confirmed, and their character and mental powers so ably discussed. When you were here I did not know of the noble position which you had filled. I had formerly read about the black regiments, but failed to connect your name with your admirable undertaking. Although we enjoyed greatly your visit to Down, my wife and myself have over and over again regretted that we did not know about the black regiment, as we should have greatly liked to have heard a little about the South from your own lips.

Your descriptions have vividly recalled walks taken forty years ago in Brazil. We have your collected Essays, which were kindly sent us by Mr. [Moncure] Conway, but have not yet had time to read them. I occasionally glean a little news of you in the 'Index'; and within the last hour have read an interesting article of yours on the progress of Free Thought.

Believe me, my dear Sir, with sincere admiration,

Yours very faithfully,

CH. DARWIN.

© The Complete Work of Charles Darwin Online

[On May 28th he sent the following answers to the questions that Mr. Galton was at that time addressing to various scientific men, in the course of the inquiry which is given in his 'English Men of Science, their Nature and Nurture,' 1874. With regard to the questions, my father wrote, "I have filled up the answers as well as I could, but it is simply impossible for me to estimate the degrees." For the sake of convenience, the questions and answers relating to "Nurture" are made to precede those on "Nature."

Education?	How taught?	I consider that all I have learnt of any value has been self-taught.
	Conducive to or restrictive of habits of observation.	Restrictive of observation, being almost entirely classical.
	Conducive to health or otherwise?	Yes.
	Peculiar merits?	None whatever.
	Chief omissions.	No mathematics or modern languages, nor any habits of observation or reasoning.
	Has the religious creed taught in your youth had any deterrent effect on the freedom of your researches?	No.
	Do your scientific tastes appear to have been innate?	Certainly innate.
	Were they determined by any and what events?	My innate taste for natural history strongly confirmed and directed by the voyage in the *Beagle*.

© The Complete Work of Charles Darwin Online

QUESTION.	YOURSELF.			YOUR FATHER.	
Specify any interests that have been very actively pursued	Science, and field sports to a passionate degree during youth.				
Religion?	Nominally to Church of England.			Nominally to Church of England.	
Politics?	Liberal or Radical.			Liberal.	
Health?	Good when young—bad for last 33 years.			Good throughout life, except from gout.	
Height, &c.?	Height?	Figure, &c.?	Measurement round inside of hat.	Height?	Figure, &c.?
	6 ft.	Spare, whilst young rather stout.	$22\frac{1}{2}$ in.	6 ft. 2 in.	Very broad and corpulent.
	Colour of Hair?	Complexion?		Colour of Hair?	Complexion?
	Brown.	Rather sallow.*		Brown.	Ruddy.
Temperament?	Somewhat nervous.			Sanguine.	
Energy of body, &c.?	Energy shown by much activity, and whilst I had health, power of resisting fatigue. I and one other man were alone able to fetch water for a large party of officers and sailors utterly prostrated. Some of my expeditions in S. America were adventurous. An early riser in the morning.			Great power of endurance although feeling much fatigue, as after consultations, after long journeys; very active—not restless—very early riser, no travels. My father said his father suffered much from sense of fatigue, that he worked very hard.	

Energy of mind, &c.?	Shown by rigorous and long-continued work on same subject, as 20 years on the 'Origin of Species,' and 9 years on 'Cirripedia.'	Habitually very active mind—shown in conversation with a succession of people during the whole day.
Memory?	Memory very bad for dates, and for learning by rote; but good in retaining a general or vague recollection of many facts.	Wonderful memory for dates. In old age he told a person, reading aloud to him a book only read in youth, the passages which were coming—knew the birthdays and death, &c., of all friends and acquaintances.
Studiousness?	Very studious, but not large acquirements.	Not very studious or mentally receptive, except for facts in conversation—great collector of anecdotes.
Independence of Judgment?	I think fairly independent; but I can give no instances. I gave up common religious belief almost independently from my own reflections.	Free thinker in religious matters. Liberal, with rather a tendency to Toryism.
Originality, or Eccentricity?	—— thinks this applies to me; I do not think so—*i.e.*, as far as eccentricity. I suppose that I have shown originality in science, as I have made discoveries with regard to common objects.	Original character, had great personal influence, and power of producing fear of himself in others. He kept his accounts with great care in a peculiar way, in a number of separate little books, without any general ledger.
Special talents?	None, except for business as evinced by keeping accounts, replies to correspondence, and investing money very well. Very methodical in all my habits.	Practical business—made a large fortune and incurred no losses.
Strongly marked mental peculiarities, bearing on scientific success, and not specified above?	Steadiness—great curiosity about facts and their meaning. Some love of the new and marvellous.	Strong social affection and great sympathy in the pleasures of others. Sceptical as to new things. Curious as to facts. Great foresight. Not much public spirit—great generosity in giving money and assistance.

* His complexion was ruddy rather than sallow.

N 2

© The Complete Work of Charles Darwin Online

The following refers *inter alia* to a letter which appeared in 'Nature' (Sept. 25, 1873), "On the Males and Complemental Males of certain Cirripedes, and on Rudimentary Organs:"]

C. Darwin to E. Haeckel.

Down, September 25, 1873.

MY DEAR HÄCKEL,—I thank you for the present of your book,* and I am heartily glad to see its great success. You will do a wonderful amount of good in spreading the doctrine of Evolution, supporting it as you do by so many original observations. I have read the new preface with very great interest. The delay in the appearance of the English translation vexes and surprises me, for I have never been able to read it thoroughly in German, and I shall assuredly do so when it appears in English. Has the problem of the later stages of reduction of useless structures ever perplexed you? This problem has of late caused me much perplexity. I have just written a letter to 'Nature' with a hypothetical explanation of this difficulty, and I will send you the paper with the passage marked. I will at the same time send a paper which has interested me; it need not be returned. It contains a singular statement bearing on so-called Spontaneous Generation. I much wish that this latter question could be settled, but I see no prospect of it. If it could be proved true this would be most important to us. . . .

Wishing you every success in your admirable labours,
I remain, my dear Häckel, yours very sincerely,
CHARLES DARWIN.

* 'Schöpfungs-Geschichte,' 4th ed. The translation ('The History of Creation') was not published until 1876.

© The Complete Work of Charles Darwin Online

CHAPTER V.

MISCELLANEA, INCLUDING SECOND EDITIONS OF 'CORAL REEFS,' THE 'DESCENT OF MAN,' AND THE 'VARIATION OF ANIMALS AND PLANTS.'

1874 AND 1875.

[THE year 1874 was given up to 'Insectivorous Plants,' with the exception of the months devoted to the second edition of the 'Descent of Man,' (see Vol. III. p. 175) and with the further exception of the time given to a second edition of his 'Coral Reefs' (1874). The Preface to the latter states that new facts have been added, the whole book revised, and "the latter chapters almost rewritten." In the Appendix some account is given of Professor Semper's objections, and this was the occasion of correspondence between that naturalist and my father. In Professor Semper's volume, 'Animal Life' (one of the International Series), the author calls attention to the subject in the following passage which I give in German, the published English translation being, as it seems to me, incorrect: "Es scheint mir als ob er in der zweiten Ausgabe seines allgemein bekannten Werks über Korallenriffe einem Irrthume über meine Beobachtungen zum Opfer gefallen ist, indem er die Angaben, die ich allerdings bisher immer nur sehr kurz gehalten hatte, vollständig falsch wiedergegeben hat."

The proof-sheets containing this passage were sent by Professor Semper to my father before 'Animal Life' was published, and this was the occasion for the following letter, which was afterwards published in Professor Semper's book.]

© The Complete Work of Charles Darwin Online

C. Darwin to K. Semper.

Down, October 2, 1879.

MY DEAR PROFESSOR SEMPER,—I thank you for your extremely kind letter of the 19th, and for the proof-sheets. I believe that I understand all, excepting one or two sentences, where my imperfect knowledge of German has interfered. This is my sole and poor excuse for the mistake which I made in the second edition of my 'Coral' book. Your account of the Pellew Islands is a fine addition to our knowledge on coral reefs. I have very little to say on the subject, even if I had formerly read your account and seen your maps, but had known nothing of the proofs of recent elevation, and of your belief that the islands have not since subsided. I have no doubt that I should have considered them as formed during subsidence. But I should have been much troubled in my mind by the sea not being so deep as it usually is round atolls, and by the reef on one side sloping so gradually beneath the sea; for this latter fact, as far as my memory serves me, is a very unusual and almost unparalleled case. I always foresaw that a bank at the proper depth beneath the surface would give rise to a reef which could not be distinguished from an atoll, formed during subsidence. I must still adhere to my opinion, that the atolls and barrier reefs in the middle of the Pacific and Indian Oceans indicate subsidence; but I fully agree with you that such cases as that of the Pellew Islands, if of at all frequent occurrence, would make my general conclusions of very little value. Future observers must decide between us. It will be a strange fact if there has not been subsidence of the beds of the great oceans, and if this has not affected the forms of the coral reefs.

In the last three pages of the last sheet sent I am extremely glad to see that you are going to treat of the dispersion of animals. Your preliminary remarks seem to me quite ex-

© The Complete Work of Charles Darwin Online

cellent. There is nothing about M. Wagner, as I expected to find. I suppose that you have seen Moseley's last book, which contains some good observations on dispersion.

I am glad that your book will appear in English, for then I can read it with ease. Pray believe me,

Yours very sincerely,
CHARLES DARWIN.

[The most recent criticism on the Coral-reef theory is by Mr. Murray, one of the staff of the *Challenger*, who read a paper before the Royal Society of Edinburgh, April 5, 1880.* The chief point brought forward is the possibility of the building up of submarine mountains, which may serve as foundations for coral reefs. Mr. Murray also seeks to prove that "the chief features of coral reefs and islands can be accounted for without calling in the aid of great and general subsidence." The following letter refers to this subject:]

C. Darwin to A. Agassiz.

Down, May 5, 1881.

. . . You will have seen Mr. Murray's views on the formation of atolls and barrier reefs. Before publishing my book, I thought long over the same view, but only as far as ordinary marine organisms are concerned, for at that time little was known of the multitude of minute oceanic organisms. I rejected this view, as from the few dredgings made in the *Beagle*, in the south temperate regions, I concluded that shells, the smaller corals, &c., decayed, and were dissolved, when not protected by the deposition of sediment, and sediment could not accumulate in the open ocean. Certainly, shells, &c., were in several cases completely rotten, and crumbled into mud between my fingers; but you will know well whether

* An abstract is published in vol. x. of the 'Proceedings,' p. 505, and in 'Nature,' August 12, 1880.

© The Complete Work of Charles Darwin Online

this is in any degree common. I have expressly said that a bank at the proper depth would give rise to an atoll, which could not be distinguished from one formed during subsidence. I can, however, hardly believe in the former presence of as many banks (there having been no subsidence) as there are atolls in the great oceans, within a reasonable depth, on which minute oceanic organisms could have accumulated to the thickness of many hundred feet. . . . Pray forgive me for troubling you at such length, but it has occurred [to me] that you might be disposed to give, after your wide experience, your judgment. If I am wrong, the sooner I am knocked on the head and annihilated so much the better. It still seems to me a marvellous thing that there should not have been much, and long continued, subsidence in the beds of the great oceans. I wish that some doubly rich millionaire would take it into his head to have borings made in some of the Pacific and Indian atolls, and bring home cores for slicing from a depth of 500 or 600 feet. . . .

[The second edition of the 'Descent of Man' was published in the autumn of 1874. Some severe remarks on the "monistic hypothesis" appeared in the July * number of the 'Quarterly Review' (p. 45). The reviewer expresses his astonishment at the ignorance of certain elementary distinctions and principles (*e.g.* with regard to the *verbum mentale*) exhibited, among others, by Mr. Darwin, who "does not exhibit the faintest indication of having grasped them, yet a clear perception of them, and a direct and detailed examination of his facts with regard to them, was a *sine quâ non* for attempting, with a chance of success, the solution of the mystery as to the descent of man."

Some further criticisms of a later date may be here alluded to. In the 'Academy,' 1876 (pp. 562, 587), appeared a review of Mr. Mivart's 'Lessons from Nature,' by Mr. Wallace.

* The review necessarily deals with the first edition of the 'Descent of Man.'

© The Complete Work of Charles Darwin Online

When considering the part of Mr. Mivart's book relating to Natural and Sexual Selection, Mr. Wallace says: "In his violent attack on Mr. Darwin's theories our author uses unusually strong language. Not content with mere argument, he expresses 'reprobation of Mr. Darwin's views'; and asserts that though he (Mr. Darwin) has been obliged, virtually, to give up his theory, it is still maintained by Darwinians with 'unscrupulous audacity,' and the actual repudiation of it concealed by the 'conspiracy of silence.'" Mr. Wallace goes on to show that these charges are without foundation, and points out that, "If there is one thing more than another for which Mr. Darwin is pre-eminent among modern literary and scientific men, it is for his perfect literary honesty, his self-abnegation in confessing himself wrong, and the eager haste with which he proclaims and even magnifies small errors in his works, for the most part discovered by himself."

The following extract from a letter to Mr. Wallace (June 17th) refers to Mr. Mivart's statement ('Lessons from Nature,' p. 144) that Mr. Darwin at first studiously disguised his views as to the "bestiality of man":—

"I have only just heard of and procured your two articles in the 'Academy.' I thank you most cordially for your generous defence of me against Mr. Mivart. In the 'Origin' I did not discuss the derivation of any one species; but that I might not be accused of concealing my opinion, I went out of my way, and inserted a sentence which seemed to me (and still so seems) to disclose plainly my belief. This was quoted in my 'Descent of Man.' Therefore it is very unjust . . . of Mr. Mivart to accuse me of base fraudulent concealment."

The letter which here follows is of interest in connection with the discussion, in the 'Descent of Man,' on the origin of the musical sense in man :]

© The Complete Work of Charles Darwin Online

*C. Darwin to E. Gurney.**

Down, July 8, 1876.

MY DEAR MR. GURNEY,—I have read your article † with much interest, except the latter part, which soared above my ken. I am greatly pleased that you uphold my views to a certain extent. Your criticism of the rasping noise made by insects being necessarily rhythmical is very good; but though not made intentionally, it may be pleasing to the females, from the nerve cells being nearly similar in function throughout the animal kingdom. With respect to your letter, I believe that I understand your meaning, and agree with you. I never supposed that the different degrees and kinds of pleasure derived from different music could be explained by the musical powers of our semi-human progenitors. Does not the fact that different people belonging to the same civilized nation are very differently affected by the same music, almost show that these diversities of taste and pleasure have been acquired during their individual lives? Your simile of architecture seems to me particularly good; for in this case the appreciation almost must be individual, though possibly the sense of sublimity excited by a grand cathedral may have some connection with the vague feelings of terror and superstition in our savage ancestors, when they entered a great cavern or gloomy forest. I wish some one could analyse the feeling of sublimity. It amuses me to think how horrified some high-flying æsthetic men will be, at your encouraging such low degraded views as mine.

Believe me, yours very sincerely,
CHARLES DARWIN.

[The letters which follow are of a miscellaneous interest. The first extract (from a letter, Jan. 18, 1874) refers to a spiritualistic séance, held at Erasmus Darwin's house, 6

* Author of 'The Power of Sound.'

† "Some disputed Points in Music."—'Fortnightly Review,' July 1876.

© The Complete Work of Charles Darwin Online

Queen Anne Street, under the auspices of a well-known medium:

"... We had grand fun, one afternoon, for George hired a medium, who made the chairs, a flute, a bell, and candlestick, and fiery points jump about in my brother's dining-room, in a manner that astounded every one, and took away all their breaths. It was in the dark, but George and Hensleigh Wedgwood held the medium's hands and feet on both sides all the time. I found it so hot and tiring that I went away before all these astounding miracles, or jugglery, took place. How the man could possibly do what was done passes my understanding. I came downstairs, and saw all the chairs, &c., on the table, which had been lifted over the heads of those sitting round it.

The Lord have mercy on us all, if we have to believe in such rubbish. F. Galton was there, and says it was a good séance. . . ."

The séance in question led to a smaller and more carefully organised one being undertaken, at which Mr. Huxley was present, and on which he reported to my father:]

C. Darwin to Professor T. H. Huxley.

Down, January 29 [1874].

MY DEAR HUXLEY,—It was very good of you to write so long an account. Though the séance did tire you so much it was, I think, really worth the exertion, as the same sort of things are done at all the séances, even at ——'s; and now to my mind an enormous weight of evidence would be requisite to make one believe in anything beyond mere trickery. . . . I am pleased to think that I declared to all my family, the day before yesterday, that the more I thought of all that I had heard happened at Queen Anne St., the more convinced I was it was all imposture . . . . my theory was that [the

© The Complete Work of Charles Darwin Online

medium] managed to get the two men on each side of him to hold each other's hands, instead of his, and that he was thus free to perform his antics. I am very glad that I issued my ukase to you to attend.

Yours affectionately,
CH. DARWIN.

[In the spring of this year (1874) he read a book which gave him great pleasure and of which he often spoke with admiration:—The 'Naturalist in Nicaragua,' by the late Thomas Belt. Mr. Belt, whose untimely death may well be deplored by naturalists, was by profession an Engineer, so that all his admirable observations in natural history, in Nicaragua and elsewhere, were the fruit of his leisure. The book is direct and vivid in style and is full of description and suggestive discussions. With reference to it my father wrote to Sir J. D. Hooker:—

"Belt I have read, and I am delighted that you like it so much; it appears to me the best of all natural history journals which have ever been published."]

C. Darwin to the Marquis de Saporta.

Down, May 30, 1874.

DEAR SIR,—I have been very neglectful in not having sooner thanked you for your kindness in having sent me your 'Études sur la Végétation,' &c., and other memoirs. I have read several of them with very great interest, and nothing can be more important, in my opinion, than your evidence of the extremely slow and gradual manner in which specific forms change. I observe that M. A. De Candolle has lately quoted you on this head *versus* Heer. I hope that you may be able to throw light on the question whether such protean, or polymorphic forms, as those of Rubus, Hieracium, &c., at the present day, are those which generate new species; as for

© The Complete Work of Charles Darwin Online

myself, I have always felt some doubt on this head. I trust that you may soon bring many of your countrymen to believe in Evolution, and my name will then perhaps cease to be scorned. With the most sincere respect, I remain, dear Sir,

Yours faithfully,

CH. DARWIN.

C. Darwin to Asa Gray.

Down, June 5 [1874].

MY DEAR GRAY,—I have now read your article* in 'Nature,' and the last two paragraphs were not included in the slip sent before. I wrote yesterday and cannot remember exactly what I said, and now cannot be easy without again telling you how profoundly I have been gratified. Every one, I suppose, occasionally thinks that he has worked in vain, and when one of these fits overtakes me, I will think of your article, and if that does not dispel the evil spirit, I shall know that I am at the time a little bit insane, as we all are occasionally.

What you say about Teleology† pleases me especially, and I do not think any one else ‡ has ever noticed the point. I have always said you were the man to hit the nail on the head.

Yours gratefully and affectionately,

CH. DARWIN.

[As a contribution to the history of the reception of the 'Origin of Species,' the meeting of the British Association in 1874, at Belfast, should be mentioned. It is memorable for

* The article, "Charles Darwin," in the series of *Scientific Worthies* ('Nature,' June 4, 1874). This admirable estimate of my father's work in science is given in the form of a comparison and contrast between Robert Brown and Charles Darwin.

† "Let us recognise Darwin's great service to Natural Science in bringing back to it Teleology: so that instead of Morphology *versus* Teleology, we shall have Morphology wedded to Teleology."

‡ Similar remarks had been previously made by Mr. Huxley. See Vol. II. p. 201.

© The Complete Work of Charles Darwin Online

Professor Tyndall's brilliant presidential address, in which a sketch of the history of Evolution is given, culminating in an eloquent analysis of the 'Origin of Species,' and of the nature of its great success. With regard to Prof. Tyndall's address, Lyell wrote ('Life,' vol. ii. p. 455) congratulating my father on the meeting, "on which occasion you and your theory of Evolution may be fairly said to have had an ovation." In the same letter Sir Charles speaks of a paper * by Professor Judd, and it is to this that the following letter refers :]

C. Darwin to C. Lyell.

Down, September 23, 1874.

MY DEAR LYELL,—I suppose that you have returned, or will soon return, to London ; † and, I hope, reinvigorated by your outing. In your last letter you spoke of Mr. Judd's paper on the Volcanoes of the Hebrides. I have just finished it, and to ease my mind must express my extreme admiration.

It is years since I have read a purely geological paper which has interested me so greatly. I was all the more interested, as in the Cordillera I often speculated on the sources of the deluges of submarine porphyritic lavas, of which they are built ; and, as I have stated, I saw to a certain extent the causes of the obliteration of the points of eruption. I was also not a little pleased to see my volcanic book quoted, for I thought it was completely dead and forgotten. What fine work will Mr. Judd assuredly do ! . . . . Now I have eased my mind ; and so farewell, with both E. D.'s and C. D.'s very kind remembrances to Miss Lyell.

Yours affectionately,

CHARLES DARWIN.

* "On the Ancient Volcanoes of the Highlands."—'Journal of Geolog. Soc.,' 1874.

† Sir Charles Lyell returned from Scotland towards the end of September.

© The Complete Work of Charles Darwin Online

[Sir Charles Lyell's reply to the above letter must have been one of the latest that my father received from his old friend, and it is with this letter that the last volume of Lyell's published correspondence closes.]

C. Darwin to Aug. Forel.

Down, October 15, 1874.

MY DEAR SIR,—I have now read the whole of your admirable work * and seldom in my life have I been more interested by any book. There are so many interesting facts and discussions, that I hardly know which to specify; but I think, firstly, the newest points to me have been about the size of the brain in the three sexes, together with your suggestion that increase of mind-power may have led to the sterility of the workers. Secondly about the battles of the ants, and your curious account of the enraged ants being held by their comrades until they calmed down. Thirdly, the evidence of ants of the same community being the offspring of brothers and sisters. You admit, I think, that new communities will often be the product of a cross between not-related ants. Fritz Müller has made some interesting observations on this head with respect to Termites. The case of *Anergates* is most perplexing in many ways, but I have such faith in the law of occasional crossing that I believe an explanation will hereafter be found, such as the dimorphism of either sex and the occasional production of winged males. I see that you are puzzled how ants of the same community recognize each other; I once placed two (*F. rufa*) in a pill-box smelling strongly of asafœtida and after a day returned them to their homes; they were threatened, but at last recognized. I made the trial thinking that they might know each other by

* 'Les Fourmis de la Suisse,' 4to, 1874.

© The Complete Work of Charles Darwin Online

their odour; but this cannot have been the case, and I have often fancied that they must have some common signal. Your last chapter is one great mass of wonderful facts and suggestions, and the whole profoundly interesting. I have seldom been more gratified than by [your] honourable mention of my work.

I should like to tell you one little observation which I made with care many years ago; I saw ants (*Formica rufa*) carrying cocoons from a nest which was the largest I ever saw and which was well known to all the country people near, and an old man, apparently about eighty years of age, told me that he had known it ever since he was a boy. The ants carrying the cocoons did not appear to be emigrating; following the line, I saw many ascending a tall fir-tree still carrying their cocoons. But when I looked closely I found that all the cocoons were empty cases. This astonished me, and next day I got a man to observe with me, and we again saw ants bringing empty cocoons out of the nest; each of us fixed on one ant and slowly followed it, and repeated the observation on many others. We thus found that some ants soon dropped their empty cocoons; others carried them for many yards, as much as thirty paces, and others carried them high up the fir-tree out of sight. Now here I think we have one instinct in contest with another and mistaken one. The first instinct being to carry the empty cocoons out of the nest, and it would have been sufficient to have laid them on the heap of rubbish, as the first breath of wind would have blown them away. And then came in the contest with the other very powerful instinct of preserving and carrying their cocoons as long as possible; and this they could not help doing although the cocoons were empty. According as the one or other instinct was the stronger in each individual ant, so did it carry the empty cocoon to a greater or less distance. If this little observation should ever prove of any use to you, you are quite at liberty to use it. Again thanking you

© The Complete Work of Charles Darwin Online

cordially for the great pleasure which your work has given me, I remain with much respect,

Yours sincerely,
CH. DARWIN.

P.S.—If you read English easily I should like to send you Mr. Belt's book, as I think you would like it as much as did Fritz Müller.

C. Darwin to J. Fiske.

Down, December 8, 1874.

MY DEAR SIR,—You must allow me to thank you for the very great interest with which I have at last slowly read the whole of your work.* I have long wished to know something about the views of the many great men whose doctrines you give. With the exception of special points I did not even understand H. Spencer's general doctrine; for his style is too hard work for me. I never in my life read so lucid an expositor (and therefore thinker) as you are; and I think that I understand nearly the whole—perhaps less clearly about Cosmic Theism and Causation than other parts. It is hopeless to attempt out of so much to specify what has interested me most, and probably you would not care to hear. I wish some chemist would attempt to ascertain the result of the cooling of heated gases of the proper kinds, in relation to your hypothesis of the origin of living matter. It pleased me to find that here and there I had arrived from my own crude thoughts at some of the same conclusions with you; though I could seldom or never have given my reasons for such conclusions. I find that my mind is so fixed by the inductive method, that I cannot appreciate deductive reasoning: I must begin with a good body of facts and not from a principle (in which I always suspect some fallacy) and then

* 'Outlines of Cosmic Philosophy,' 2 vols. 8vo. 1874.

© The Complete Work of Charles Darwin Online

as much deduction as you please. This may be very narrow-minded; but the result is that such parts of H. Spencer as I have read with care impress my mind with the idea of his inexhaustible wealth of suggestion, but never convince me; and so I find it with some others. I believe the cause to lie in the frequency with which I have found first-formed theories [to be] erroneous. I thank you for the honourable mention which you make of my works. Parts of the 'Descent of Man' must have appeared laughably weak to you: nevertheless, I have sent you a new edition just published. Thanking you for the profound interest and profit with which I have read your work, I remain,

My dear Sir, yours very faithfully,

CH. DARWIN.

1875.

[The only work, not purely botanical, which occupied my father in the present year was the correction of the second edition of 'The Variation of Animals and Plants,' and on this he was engaged from the beginning of July till October 3rd. The rest of the year was taken up with his work on insectivorous plants, and on cross-fertilisation, as will be shown in a later chapter. The chief alterations in the second edition of 'Animals and Plants' are in the eleventh chapter on "Bud-variation and on certain anomalous modes of reproduction;" the chapter on Pangenesis "was also largely altered and remodelled." He mentions briefly some of the authors who have noticed the doctrine. Professor Delpino's 'Sulla Darwiniana Teoria della Pangenesi' (1869), an adverse but fair criticism, seems to have impressed him as valuable. Of another critic my father characteristically says,* "Dr. Lionel Beale ('Nature,' May 11, 1871, p. 26) sneers at the whole doctrine with much acerbity and some justice." He also

* 'Animals and Plants,' 2nd edit. vol. ii. p. 350.

© The Complete Work of Charles Darwin Online

points out that, in Mantegazza's 'Elementi di Igiene,' the theory of Pangenesis was clearly forestalled.

In connection with this subject, a letter of my father's to 'Nature' (April 27, 1871) should be mentioned. A paper by Mr. Galton had been read before the Royal Society (March 30, 1871) in which were described experiments, on intertransfusion of blood, designed to test the truth of the hypothesis of pangenesis. My father, while giving all due credit to Mr. Galton for his ingenious experiments, does not allow that pangenesis has "as yet received its death-blow, though from presenting so many vulnerable points its life is always in jeopardy."

He seems to have found the work of correcting very wearisome, for he wrote:—

"I have no news about myself, as I am merely slaving over the sickening work of preparing new editions. I wish I could get a touch of poor Lyell's feelings, that it was delightful to improve a sentence, like a painter improving a picture."

The feeling of effort or strain over this piece of work, is shown in a letter to Professor Haeckel:—

"What I shall do in future if I live, Heaven only knows; I ought perhaps to avoid general and large subjects, as too difficult for me with my advancing years, and I suppose enfeebled brain."

At the end of March, in this year, the portrait for which he was sitting to Mr. Ouless was finished. He felt the sittings a great fatigue, in spite of Mr. Ouless's considerate desire to spare him as far as was possible. In a letter to Sir J. D. Hooker he wrote, "I look a very venerable, acute, melancholy old dog; whether I really look so I do not know." The picture is in the possession of the family, and is known to many through M. Rajon's etching. Mr. Ouless's portrait is, in my opinion, the finest representation of my father that has been produced.

The following letter refers to the death of Sir Charles Lyell,

© The Complete Work of Charles Darwin Online

which took place on February 22nd, 1875, in his seventy-eighth year.]

*C. Darwin to Miss Buckley (now Mrs. Fisher).**

Down, February 23, 1875.

MY DEAR MISS BUCKLEY,—I am grieved to hear of the death of my old and kind friend, though I knew that it could not be long delayed, and that it was a happy thing that his life should not have been prolonged, as I suppose that his mind would inevitably have suffered. I am glad that Lady Lyell† has been saved this terrible blow. His death makes me think of the time when I first saw him, and how full of sympathy and interest he was about what I could tell him of coral reefs and South America. I think that this sympathy with the work of every other naturalist was one of the finest features of his character. How completely he revolutionised Geology: for I can remember something of pre-Lyellian days.

I never forget that almost everything which I have done in science I owe to the study of his great works. Well, he has had a grand and happy career, and no one ever worked with a truer zeal in a noble cause. It seems strange to me that I shall never again sit with him and Lady Lyell at their breakfast. I am very much obliged to you for having so kindly written to me.

Pray give our kindest remembrances to Miss Lyell, and I hope that she has not suffered much in health, from fatigue and anxiety.

Believe me, my dear Miss Buckley,

Yours very sincerely,

CHARLES DARWIN.

* Mrs. Fisher acted as Secretary to Sir Charles Lyell.

† Lady Lyell died in 1873.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, February 25 [1875].

MY DEAR HOOKER,—Your letter so full of feeling has interested me greatly. I cannot say that I felt his [Lyell's] death much, for I fully expected it, and have looked for some little time at his career as finished.

I dreaded nothing so much as his surviving with impaired mental powers. He was, indeed, a noble man in very many ways; perhaps in none more than in his warm sympathy with the work of others. How vividly I can recall my first conversation with him, and how he astonished me by his interest in what I told him. How grand also was his candour and pure love of truth. Well, he is gone, and I feel as if we were all soon to go. . . . I am deeply rejoiced about Westminster Abbey,* the possibility of which had not occurred to me when I wrote before. I did think that his works were the most enduring of all testimonials (as you say) to him; but then I did not like the idea of his passing away with no outward sign of what scientific men thought of his merits. Now all this is changed, and nothing can be better than Westminster Abbey. Mrs. Lyell has asked me to be one of the pall-bearers, but I have written to say that I dared not, as I should so likely fail in the midst of the ceremony, and have my head whirling off my shoulders. All this affair must have cost you much fatigue and worry, and how I do wish you were out of England. . . .

[In 1881 he wrote to Mrs. Fisher in reference to her article on Sir Charles Lyell in the 'Encyclopædia Britannica':—

"For such a publication I suppose you do not want to say much about his private character, otherwise his strong sense of humour and love of society might have been added. Also his extreme interest in the progress of the world, and in the

* Sir Charles Lyell was buried in Westminster Abbey.

© The Complete Work of Charles Darwin Online

happiness of mankind. Also his freedom from all religious bigotry, though these perhaps would be a superfluity."

The following refers to the Zoological station at Naples, a subject on which my father felt an enthusiastic interest :]

C. Darwin to Anton Dohrn.

Down [1875 ?].

MY DEAR DR. DOHRN,—Many thanks for your most kind letter. I most heartily rejoice [at your improved health and at the success of your grand undertaking, which will have so much influence on the progress of Zoology throughout Europe.

If we look to England alone, what capital work has already been done at the Station by Balfour and Ray Lankester..... When you come to England, I suppose that you will bring Mrs. Dohrn, and we shall be delighted to see you both here. I have often boasted that I have had a live Uhlan in my house! It will be very interesting to me to read your new views on the ancestry of the Vertebrates. I shall be sorry to give up the Ascidians, to whom I feel profound gratitude ; but the great thing, as it appears to me, is that any link whatever should be found between the main divisions of the Animal Kingdom. . . .

C. Darwin to August Weismann.

Down, December 6, 1875.

MY DEAR SIR,—I have been profoundly interested by your essay on Amblystoma,* and think that you have removed a great stumbling-block in the way of Evolution. I once thought of reversion in this case; but in a crude and imperfect manner. I write now to call your attention to the sterility of moths when hatched out of their proper season ; I give references in chapter 18 of my 'Variation under Domestication' (vol. ii.

* 'Umwandlung des Axolotl.'

© The Complete Work of Charles Darwin Online

p. 157, of English edition), and these cases illustrate, I think, the sterility of Amblystoma. Would it not be worth while to examine the reproductive organs of those individuals of *wingless* Hemiptera which occasionally have wings, as in the case of the bed-bug? I think I have heard that the females of Mutilla sometimes have wings. These cases must be due to reversion. I dare say many anomalous cases will be hereafter explained on the same principle.

I hinted at this explanation in the extraordinary case of the black-shouldered peacock, the so-called *Pavo nigripennis* given in my 'Var. under Domest.;' and I might have been bolder, as the variety is in many respects intermediate between the two known species.

With much respect,
Yours sincerely,
CH. DARWIN.

THE VIVISECTION QUESTION.

[It was in November 1875 that my father gave his evidence before the Royal Commission on Vivisection.* I have, therefore, placed together here the matter relating to this subject, irrespective of date. Something has already been said of my father's strong feeling with regard to suffering † both in man and beast. It was indeed one of the strongest feelings in his nature, and was exemplified in matters small and great, in his sympathy with the educational miseries of dancing dogs, or in his horror at the sufferings of slaves.

* See Vol. I. p. 141.

† He once made an attempt to free a patient in a mad-house, who (as he wrongly supposed) was sane. He had some correspondence with the gardener at the asylum, and on one occasion he found a letter from a patient enclosed with one from the gardener. The letter was rational in tone and declared that the writer was sane and wrongfully confined.

My father wrote to the Lunacy Commissioners (without explaining the source of his information) and in due time heard that the man had been visited by the Commissioners, and that he was certainly insane.

[Some

© The Complete Work of Charles Darwin Online

The remembrance of screams, or other sounds heard in Brazil, when he was powerless to interfere with what he believed to be the torture of a slave, haunted him for years, especially at night. In smaller matters, where he could interfere, he did so vigorously. He returned one day from his walk pale and faint from having seen a horse ill-used, and from the agitation of violently remonstrating with the man. On another occasion he saw a horse-breaker teaching his son to ride, the little boy was frightened and the man was rough; my father stopped, and jumping out of the carriage reproved the man in no measured terms.

One other little incident may be mentioned, showing that his humanity to animals was well known in his own neighbourhood. A visitor, driving from Orpington to Down, told the cabman to go faster. "Why," said the man, "if I had whipped the horse *this* much, driving Mr. Darwin, he would have got out of the carriage and abused me well."

With respect to the special point under consideration,—the sufferings of animals subjected to experiment,—nothing could show a stronger feeling than the following extract from a letter to Professor Ray Lankester (March 22, 1871):—

"You ask about my opinion on vivisection. I quite agree that it is justifiable for real investigations on physiology; but not for mere damnable and detestable curiosity. It is a subject which makes me sick with horror, so I will not say another word about it, else I shall not sleep to-night."

An extract from Sir Thomas Farrer's notes shows how strongly he expressed himself in a similar manner in conversation:—

"The last time I had any conversation with him was at my house in Bryanston Square, just before one of his last seizures. He was then deeply interested in the vivisection question;

Some time afterwards the patient was discharged, and wrote to thank my father for his interference, adding that he had undoubtedly been insane when he wrote his former letter.

© The Complete Work of Charles Darwin Online

and what he said made a deep impression on me. He was a man eminently fond of animals and tender to them; he would not knowingly have inflicted pain on a living creature; but he entertained the strongest opinion that to prohibit experiments on living animals, would be to put a stop to the knowledge of and the remedies for pain and disease."

The Anti-Vivisection agitation, to which the following letters refer, seems to have become specially active in 1874, as may be seen, *e.g.* by the index to 'Nature' for that year, in which the word "Vivisection" suddenly comes into prominence. But before that date the subject had received the earnest attention of biologists. Thus at the Liverpool Meeting of the British Association in 1870, a Committee was appointed, whose report defined the circumstances and conditions under which, in the opinion of the signatories, experiments on living animals were justifiable. In the spring of 1875, Lord Hartismere introduced a Bill into the Upper House to regulate the course of physiological research. Shortly afterwards a Bill more just towards science in its provisions was introduced to the House of Commons by Messrs. Lyon Playfair, Walpole, and Ashley. It was however, withdrawn on the appointment of a Royal Commission to inquire into the whole question. The Commissioners were Lords Cardwell and Winmarleigh, Mr. W. E. Forster, Sir J. B. Karslake, Mr. Huxley, Professor Erichssen, and Mr. R. H. Hutton: they commenced their inquiry in July, 1875, and the Report was published early in the following year.

In the early summer of 1876, Lord Carnarvon's Bill, entitled, "An Act to amend the Law relating to Cruelty to Animals," was introduced. It cannot be denied that the framers of this Bill, yielding to the unreasonable clamour of the public, went far beyond the recommendations of the Royal Commission. As a correspondent in 'Nature' put it (1876, p. 248), "the evidence on the strength of which legisla-

© The Complete Work of Charles Darwin Online

tion was recommended went beyond the facts, the Report went beyond the evidence, the Recommendations beyond the Report; and the Bill can hardly be said to have gone beyond the Recommendations; but rather to have contradicted them."

The legislation which my father worked for, as described in the following letters, was practically what was introduced as Dr. Lyon Playfair's Bill.]

*C. Darwin to Mrs. Litchfield.**

January 4, 1875.

MY DEAR H.—Your letter has led me to think over vivisection (I wish some new word like anæs-section could be invented †) for some hours, and I will jot down my conclusions, which will appear very unsatisfactory to you. I have long thought physiology one of the greatest of sciences, sure sooner, or more probably later, greatly to benefit mankind; but, judging from all other sciences, the benefits will accrue only indirectly in the search for abstract truth. It is certain that physiology can progress only by experiments on living animals. Therefore the proposal to limit research to points of which we can now see the bearings in regard to health, &c., I look at as puerile. I thought at first it would be good to limit vivisection to public laboratories; but I have heard only of those in London and Cambridge, and I think Oxford; but probably there may be a few others. Therefore only men living in a few great towns would carry on investigation, and this I should consider a great evil. If private men were permitted to work in their own houses, and required a licence, I do not see who is to determine whether any particular man should receive one. It is young unknown men who are the

* His daughter.

† He communicated to 'Nature' (Sept. 30, 1880) an article by Dr. Wilder, of Cornell University, an abstract of which was published (p. 517). Dr. Wilder advocated the use of the word 'Callisection' for painless operations on animals.

© The Complete Work of Charles Darwin Online

most likely to do good work. I would gladly punish severely any one who operated on an animal not rendered insensible, if the experiment made this possible; but here again I do not see that a magistrate or jury could possibly determine such a point. Therefore I conclude, if (as is likely) some experiments have been tried too often, or anæsthetics have not been used when they could have been, the cure must be in the improvement of humanitarian feelings. Under this point of view I have rejoiced at the present agitation. If stringent laws are passed, and this is likely, seeing how unscientific the House of Commons is, and that the gentlemen of England are humane, as long as their sports are not considered, which entail a hundred or thousand-fold more suffering than the experiments of physiologists—if such laws are passed, the result will assuredly be that physiology, which has been until within the last few years at a standstill in England, will languish or quite cease. It will then be carried on solely on the Continent; and there will be so many the fewer workers on this grand subject, and this I should greatly regret. By the way, F. Balfour, who has worked for two or three years in the laboratory at Cambridge, declares to George that he has never seen an experiment, except with animals rendered insensible. No doubt the names of doctors will have great weight with the House of Commons; but very many practitioners neither know nor care anything about the progress of knowledge. I cannot at present see my way to sign any petition, without hearing what physiologists thought would be its effect, and then judging for myself. I certainly could not sign the paper sent me by Miss Cobbe, with its monstrous (as it seems to me) attack on Virchow for experimenting on the Trichinæ. I am tired and so no more.

Yours affectionately,
CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

C. Darwin to J. D. Hooker.

Down, April 14 [1875].

MY DEAR HOOKER.—I worked all the time in London on the vivisection question; and we now think it advisable to go further than a mere petition. Litchfield* drew up a sketch of a Bill, the essential features of which have been approved by Sanderson, Simon and Huxley, and from conversation, will, I believe, be approved by Paget, and almost certainly, I think, by Michael Foster. Sanderson, Simon and Paget wish me to see Lord Derby, and endeavour to gain his advocacy with the Home Secretary. Now, if this is carried into effect, it will be of great importance to me to be able to say that the Bill in its essential features has the approval of some half-dozen eminent scientific men. I have therefore asked Litchfield to enclose a copy to you in its first rough form; and if it is not essentially modified, may I say that it meets with your approval as President of the Royal Society? The object is to protect animals, and at the same time not to injure Physiology, and Huxley and Sanderson's approval almost suffices on this head. Pray let me have a line from you soon.

Yours affectionately,
CHARLES DARWIN.

[The Physiological Society, which was founded in 1876, was in some measure the outcome of the anti-vivisection movement, since it was this agitation which impressed on Physiologists the need of a centre for those engaged in this particular branch of science. With respect to the Society, my father wrote to Mr. Romanes (May 29, 1876):—

"I was very much gratified by the wholly unexpected honour of being elected one of the Honorary Members. This mark of sympathy has pleased me to a very high degree."

* Mr. R. B. Litchfield, his son-in-law.

© The Complete Work of Charles Darwin Online

The following letter appeared in the *Times*, April 18th, 1881:]

*C. Darwin to Frithiof Holmgren.**

Down, April 14, 1881.

DEAR SIR,—In answer to your courteous letter of April 7, I have no objection to express my opinion with respect to the right of experimenting on living animals. I use this latter expression as more correct and comprehensive than that of vivisection. You are at liberty to make any use of this letter which you may think fit, but if published I should wish the whole to appear. I have all my life been a strong advocate for humanity to animals, and have done what I could in my writings to enforce this duty. Several years ago, when the agitation against physiologists commenced in England, it was asserted that inhumanity was here practised, and useless suffering caused to animals; and I was led to think that it might be advisable to have an Act of Parliament on the subject. I then took an active part in trying to get a Bill passed, such as would have removed all just cause of complaint, and at the same time have left physiologists free to pursue their researches,—a Bill very different from the Act which has since been passed. It is right to add that the investigation of the matter by a Royal Commission proved that the accusations made against our English physiologists were false. From all that I have heard, however, I fear that in some parts of Europe little regard is paid to the sufferings of animals, and if this be the case, I should be glad to hear of legislation against inhumanity in any such country. On the other hand, I know that physiology cannot possibly progress except by means of experiments on living animals, and I feel the deepest conviction that he who retards the progress of physiology commits a crime against mankind. Any one

* Professor of Physiology at Upsala.

© The Complete Work of Charles Darwin Online

who remembers, as I can, the state of this science half a century ago, must admit that it has made immense progress, and it is now progressing at an ever-increasing rate. What improvements in medical practice may be directly attributed to physiological research is a question which can be properly discussed only by those physiologists and medical practitioners who have studied the history of their subjects; but, as far as I can learn, the benefits are already great. However this may be, no one, unless he is grossly ignorant of what science has done for mankind, can entertain any doubt of the incalculable benefits which will hereafter be derived from physiology, not only by man, but by the lower animals. Look for instance at Pasteur's results in modifying the germs of the most malignant diseases, from which, as it so happens, animals will in the first place receive more relief than man. Let it be remembered how many lives and what a fearful amount of suffering have been saved by the knowledge gained of parasitic worms through the experiments of Virchow and others on living animals. In the future every one will be astonished at the ingratitude shown, at least in England, to these benefactors of mankind. As for myself, permit me to assure you that I honour, and shall always honour, every one who advances the noble science of physiology.

Dear Sir, yours faithfully,

CHARLES DARWIN.

[In the *Times* of the following day appeared a letter headed "Mr. Darwin and Vivisection," signed by Miss Frances Power Cobbe. To this my father replied in the *Times* of April 22, 1881. On the same day he wrote to Mr. Romanes:—

"As I have a fair opportunity, I sent a letter to the *Times* on Vivisection, which is printed to-day. I thought it fair to bear my share of the abuse poured in so atrocious a manner on all physiologists."]

© The Complete Work of Charles Darwin Online

C. Darwin to the Editor of the 'Times.'

SIR,—I do not wish to discuss the views expressed by Miss Cobbe in the letter which appeared in the *Times* of the 19th inst.; but as she asserts that I have "misinformed" my correspondent in Sweden in saying that "the investigation of the matter by a Royal Commission proved that the accusations made against our English physiologists were false," I will merely ask leave to refer to some other sentences from the report of the Commission.

(1.) The sentence—"It is not to be doubted that inhumanity may be found in persons of very high position as physiologists," which Miss Cobbe quotes from page 17 of the report, and which, in her opinion, "can necessarily concern English physiologists alone and not foreigners," is immediately followed by the words "We have seen that it was so in Majendie." Majendie was a French physiologist who became notorious some half century ago for his cruel experiments on living animals.

(2.) The Commissioners, after speaking of the "general sentiment of humanity" prevailing in this country, say (p. 10):—

"This principle is accepted generally by the very highly educated men whose lives are devoted either to scientific investigation and education or to the mitigation or the removal of the sufferings of their fellow-creatures; though differences of degree in regard to its practical application will be easily discernible by those who study the evidence as it has been laid before us."

Again, according to the Commissioners (p. 10):—

"The secretary of the Royal Society for the Prevention of Cruelty to Animals, when asked whether the general tendency of the scientific world in this country is at variance with humanity, says he believes it to be very different, indeed, from that of foreign physiologists; and while giving it as the

© The Complete Work of Charles Darwin Online

opinion of the society that experiments are performed which are in their nature beyond any legitimate province of science, and that the pain which they inflict is pain which it is not justifiable to inflict even for the scientific object in view, he readily acknowledges that he does not know a single case of wanton cruelty, and that in general the English physiologists have used anæsthetics where they think they can do so with safety to the experiment."

I am, Sir, your obedient servant,

CHARLES DARWIN.

April 21.

[In the *Times* of Saturday, April 23, 1881, appeared a letter from Miss Cobbe in reply.]

C. Darwin to G. J. Romanes.

Down, April 25, 1881.

MY DEAR ROMANES,—I was very glad to read your last note with much news interesting to me. But I write now to say how I, and indeed all of us in the house, have admired your letter in the *Times*.* It was so simple and direct. I was particularly glad about Burdon Sanderson, of whom I have been for several years a great admirer. I was also especially glad to read the last sentences. I have been bothered with several letters, but none abusive. Under a *selfish* point of view I am very glad of the publication of your letter, as I was at first inclined to think that I had done mischief by stirring up the mud. Now I feel sure that I have done good. Mr. Jesse has written to me very politely, he says his Society has had nothing to do with placards and diagrams against physiology, and I suppose, therefore, that these all originate with Miss Cobbe. . . . . Mr. Jesse complains bitterly that the

* April 25, 1881.—Mr. Romanes defended Dr. Sanderson against the accusations made by Miss Cobbe.

© The Complete Work of Charles Darwin Online

Times will "burke" all his letters to this newspaper, nor am I surprised, judging from the laughable tirades advertised in 'Nature.'

Ever yours, very sincerely,
CH. DARWIN.

[The next letter refers to a projected conjoint article on vivisection, to which Mr. Romanes wished my father to contribute :]

C. Darwin to G. J. Romanes.

Down, September 2, 1881.

MY DEAR ROMANES,—Your letter has perplexed me beyond all measure. I fully recognise the duty of every one whose opinion is worth anything, expressing his opinion publicly on vivisection; and this made me send my letter to the *Times*. I have been thinking at intervals all morning what I could say, and it is the simple truth that I have nothing worth saying. You and men like you, whose ideas flow freely, and who can express them easily, cannot understand the state of mental paralysis in which I find myself. What is most wanted is a careful and accurate attempt to show what physiology has already done for man, and even still more strongly what there is every reason to believe it will hereafter do. Now I am absolutely incapable of doing this, or of discussing the other points suggested by you.

If you wish for my name (and I should be glad that it should appear with that of others in the same cause), could you not quote some sentence from my letter in the *Times* which I enclose, but please return it. If you thought fit you might say you quoted it with my approval, and that after still further reflection I still abide most strongly in my expressed conviction.

For Heaven's sake, do think of this. I do not grudge the labour and thought; but I could write nothing worth any one reading.

© The Complete Work of Charles Darwin Online

Allow me to demur to your calling your conjoint article a "symposium" strictly a "drinking party." This seems to me very bad taste, and I do hope every one of you will avoid any semblance of a joke on the subject. I *know* that words, like a joke, on this subject have quite disgusted some persons not at all inimical to physiology. One person lamented to me that Mr. Simon, in his truly admirable Address at the Medical Congress (by far the best thing which I have read), spoke of the fantastic *sensuality* * (or some such term) of the many mistaken, but honest men and women who are half mad on the subject. . . .

[To Dr. Lauder Brunton my father wrote in February 1882 :—

"Have you read Mr. [Edmund] Gurney's articles in the 'Fortnightly' † and 'Cornhill'? ‡ They seem to me very clever, though obscurely written, and I agree with almost everything he says, except with some passages which appear to imply that no experiments should be tried unless some immediate good can be predicted, and this is a gigantic mistake contradicted by the whole history of science."]

* 'Transactions of the International Medical Congress,' 1881, vol. iv. p. 413. The expression "lackadaisical" (not fantastic), and "feeble sensuality," are used with regard to the feelings of the anti-vivisectionists.

† "A chapter in the Ethics of Pain," 'Fortnightly Review,' 1881, vol. xxx. p. 778.

‡ "An Epilogue on Vivisection," 'Cornhill Magazine,' 1882, vol. xlv. p. 191.

© The Complete Work of Charles Darwin Online

CHAPTER VI.

MISCELLANEA (*continued*) — A REVIVAL OF GEOLOGICAL WORK—THE BOOK ON EARTHWORMS—LIFE OF ERASMUS DARWIN—MISCELLANEOUS LETTERS.

1876–1882.

[WE have now to consider the work (other than botanical) which occupied the concluding six years of my father's life. A letter to his old friend Rev. L. Blomfield (Jenyns), written in March, 1877, shows what was my father's estimate of his own powers of work at this time :—

"MY DEAR JENYNS (I see I have forgotten your proper names),—Your extremely kind letter has given me warm pleasure. As one gets old, one's thoughts turn back to the past rather than to the future, and I often think of the pleasant, and to me valuable, hours which I spent with you on the borders of the Fens.

"You ask about my future work; I doubt whether I shall be able to do much more that is new, and I always keep before my mind the example of poor old ——, who in his old age had a cacoethes for writing. But I cannot endure doing nothing, so I suppose that I shall go on as long as I can without obviously making a fool of myself. I have a great mass of matter with respect to variation under nature; but so much has been published since the appearance of the 'Origin of Species,' that I very much doubt whether I retain power of mind and strength to reduce the mass into a digested whole. I have sometimes thought that I would try, but dread the attempt. . . ."

© The Complete Work of Charles Darwin Online

His prophecy proved to be a true one with regard to any continuation of any general work in the direction of Evolution, but his estimate of powers which could afterwards prove capable of grappling with the 'Movements of Plants,' and with the work on 'Earthworms,' was certainly a low one.

The year 1876, with which the present chapter begins, brought with it a revival of geological work. He had been astonished, as I hear from Professor Judd, and as appears in his letters, to learn that his books on 'Volcanic Islands,' 1844, and on 'South America,' 1846, were still consulted by geologists, and it was a surprise to him that new editions should be required. Both these works were originally published by Messrs. Smith and Elder, and the new edition of 1876 was also brought out by them. This appeared in one volume with the title 'Geological Observations on the Volcanic Islands, and Parts of South America visited during the Voyage of H.M.S. *Beagle*.' He has explained in the preface his reasons for leaving untouched the text of the original editions: "They relate to parts of the world which have been so rarely visited by men of science, that I am not aware that much could be corrected or added from observations subsequently made. Owing to the great progress which Geology has made within recent times, my views on some few points may be somewhat antiquated; but I have thought it best to leave them as they originally appeared."

It may have been the revival of geological speculation, due to the revision of his early books, that led to his recording the observations of which some account is given in the following letter. Part of it has been published in Professor James Geikie's 'Prehistoric Europe,' chaps. vii. and ix.,* a few verbal alterations having been made at my father's request in the passages quoted. Mr. Geikie lately wrote to me: "The

* My father's suggestion is also noticed in Prof. Geikie's address on the 'Ice Age in Europe and North America,' given at Edinburgh, Nov. 20, 1884.

© The Complete Work of Charles Darwin Online

views suggested in his letter as to the origin of the angular gravels, &c., in the South of England will, I believe, come to be accepted as the truth. This question has a much wider bearing than might at first appear. In point of fact it solves one of the most difficult problems in Quaternary Geology—and has already attracted the attention of German geologists."]

C. Darwin to James Geikie.

Down, November 16, 1876.

MY DEAR SIR,—I hope that you will forgive me for troubling you with a very long letter. But first allow me to tell you with what extreme pleasure and admiration I have just finished reading your 'Great Ice Age.' It seems to me admirably done, and most clear. Interesting as many chapters are in the history of the world, I do not think that any one comes [up] nearly to the glacial period or periods. Though I have steadily read much on the subject, your book makes the whole appear almost new to me.

I am now going to mention a small observation, made by me two or three years ago, near Southampton, but not followed out, as I have no strength for excursions. I need say nothing about the character of the drift there (which includes palæolithic celts), for you have described its essential features in a few words at p. 506. It covers the whole country [in an] even plain-like surface, almost irrespective of the present outline of the land.

The coarse stratification has sometimes been disturbed. I find that you allude "to the larger stones often standing on end;" and this is the point which struck me so much. Not only moderately sized angular stones, but small oval pebbles often stand vertically up, in a manner which I have never seen in ordinary gravel beds. This fact reminded me of what occurs near my home, in the stiff red clay, full of unworn flints over the chalk, which is no doubt the residue left undissolved

© The Complete Work of Charles Darwin Online

by rain water. In this clay, flints as long and thin as my arm often stand perpendicularly up; and I have been told by the tank-diggers that it is their "natural position"! I presume that this position may safely be attributed to the differential movement of parts of the red clay as it subsided very slowly from the dissolution of the underlying chalk; so that the flints arrange themselves in the lines of least resistance. The similar but less strongly marked arrangement of the stones in the drift near Southampton makes me suspect that it also must have slowly subsided; and the notion has crossed my mind that during the commencement and height of the glacial period great beds of frozen snow accumulated over the south of England, and that, during the summer, gravel and stones were washed from the higher land over its surface, and in superficial channels. The larger streams may have cut right through the frozen snow, and deposited gravel in lines at the bottom. But on each succeeding autumn, when the running water failed, I imagine that the lines of drainage would have been filled up by blown snow afterwards congealed, and that, owing to great surface accumulations of snow, it would be a mere chance whether the drainage, together with gravel and sand, would follow the same lines during the next summer. Thus, as I apprehend, alternate layers of frozen snow and drift, in sheets and lines, would ultimately have covered the country to a great thickness, with lines of drift probably deposited in various directions at the bottom by the larger streams. As the climate became warmer, the lower beds of frozen snow would have melted with extreme slowness, and the many irregular beds of interstratified drift would have sunk down with equal slowness; and during this movement the elongated pebbles would have arranged themselves more or less vertically. The drift would also have been deposited almost irrespective of the outline of the underlying land. When I viewed the country I could not persuade myself that any flood, however great, could have depo-

© The Complete Work of Charles Darwin Online

sited such coarse gravel over the almost level platforms between the valleys. My view differs from that of Holst, p. 415 ['Great Ice Age'], of which I had never heard, as his relates to channels cut through glaciers, and mine to beds of drift interstratified with frozen snow where no glaciers existed. The upshot of this long letter is to ask you to keep my notion in your head, and look out for upright pebbles in any lowland country which you may examine, where glaciers have not existed. Or if you think the notion deserves any further thought, but not otherwise, to tell any one of it, for instance Mr. Skertchly, who is examining such districts. Pray forgive me for writing so long a letter, and again thanking you for the great pleasure derived from your book,

I remain yours very faithfully,

CH. DARWIN.

P.S. . . . I am glad that you have read Blytt;* his paper seemed to me a most important contribution to Botanical Geography. How curious that the same conclusions should have been arrived at by Mr. Skertchly, who seems to be a first-rate observer; and this implies, as I always think, a sound theoriser.

I have told my publisher to send you in two or three days a copy (second edition) of my geological work during the voyage of the *Beagle*. The sole point which would perhaps interest you is about the steppe-like plains of Patagonia.

For many years past I have had fearful misgivings that it must have been the level of the sea, and not that of the land which has changed.

I read a few months ago your [brother's] very interesting life of Murchison.† Though I have always thought that he ranked next to W. Smith in the classification of formations,

* Axel Blytt.—'Essay on the Immigration of the Norwegian Flora during alternate rainy and dry Seasons.' Christiania, 1876.

† By Mr. Archibald Geikie.

© The Complete Work of Charles Darwin Online

and though I knew how kind-hearted [he was], yet the book has raised him greatly in my respect, notwithstanding his foibles and want of broad philosophical views.

[The only other geological work of his later years was embodied in his book on earthworms (1881), which may therefore be conveniently considered in this place. This subject was one which had interested him many years before this date, and in 1838 a paper on the formation of mould was published in the Proceedings of the Geological Society (see vol. i. p. 284).

Here he showed that "fragments of burnt marl, cinders, &c., which had been thickly strewed over the surface of several meadows were found after a few years lying at a depth of some inches beneath the turf, but still forming a layer." For the explanation of this fact, which forms the central idea of the geological part of the book, he was indebted to his uncle Josiah Wedgwood, who suggested that worms, by bringing earth to the surface in their castings, must undermine any objects lying on the surface and cause an apparent sinking.

In the book of 1881 he extended his observations on this burying action, and devised a number of different ways of checking his estimates as to the amount of work done.* He also added a mass of observations on the habits, natural history and intelligence of worms, a part of the work which added greatly to its popularity.

In 1877 Sir Thomas Farrer had discovered close to his garden the remains of a building of Roman-British times, and thus gave my father the opportunity of seeing for himself

* He received much valuable help from Dr. King, of the Botanical Gardens, Calcutta. The following passage is from a letter to Dr. King, dated January 18, 1873:—

"I really do not know how to thank you enough for the immense trouble which you have taken. You have attended *exactly* and *fully* to the points about which I was most anxious. If I had been each evening by your side, I could not have suggested anything else."

© The Complete Work of Charles Darwin Online

the effects produced by earthworms on the old concrete-floors, walls, &c. On his return he wrote to Sir Thomas Farrer:—

"I cannot remember a more delightful week than the last. I know very well that E. will not believe me, but the worms were by no means the sole charm."

In the autumn of 1880, when the 'Power of Movements in Plants' was nearly finished, he began once more on the subject. He wrote to Professor Carus (September 21):—

"In the intervals of correcting the press, I am writing a very little book, and have done nearly half of it. Its title will be (as at present designed), 'The Formation of Vegetable Mould through the Action of Worms.'* As far as I can judge it will be a curious little book."

The manuscript was sent to the printers in April, 1881, and when the proof-sheets were coming in he wrote to Professor Carus: "The subject has been to me a hobby-horse, and I have perhaps treated it in foolish detail."

It was published on October 10, and 2000 copies were sold at once. He wrote to Sir J. D. Hooker, "I am glad that you approve of the 'Worms.' When in old days I was to tell you whatever I was doing, if you were at all interested, I always felt as most men do when their work is finally published."

To Mr. Mellard Reade he wrote (November 8): "It has been a complete surprise to me how many persons have cared for the subject." And to Mr. Dyer (in November): "My book has been received with almost laughable enthusiasm, and 3500 copies have been sold! ! !" Again, to his friend Mr. Anthony Rich, he wrote on February 4, 1882, "I have been plagued with an endless stream of letters on the subject; most of them very foolish and enthusiastic; but some containing good facts which I have used in correcting yesterday the 'Sixth Thousand.'" The popularity of the

* The full title is 'The Formation of Vegetable Mould through the Action of Worms, with Observations on their Habits,' 1881.

© The Complete Work of Charles Darwin Online

book may be roughly estimated by the fact that, in the three years following its publication, 8500 copies were sold—a sale relatively greater than that of the 'Origin of Species.'

It is not difficult to account for its success with the non-scientific public. Conclusions so wide and so novel, and so easily understood, drawn from the study of creatures so familiar, and treated with unabated vigour and freshness, may well have attracted many readers. A reviewer remarks: "In the eyes of most men. . . the earthworm is a mere blind, dumb, senseless, and unpleasantly slimy annelid. Mr. Darwin undertakes to rehabilitate his character, and the earthworm steps forth at once as an intelligent and beneficent personage, a worker of vast geological changes, a planer down of mountain sides . . . a friend of man . . . and an ally of the Society for the preservation of ancient monuments." The *St. James's Gazette*, of October 17th, 1881, pointed out that the teaching of the cumulative importance of the infinitely little is the point of contact between this book and the author's previous work.

One more book remains to be noticed, the 'Life of Erasmus Darwin.'

In February 1879 an essay by Dr. Ernst Krause, on the scientific work of Erasmus Darwin, appeared in the evolutionary journal, 'Kosmos,' The number of 'Kosmos' in question was a "Gratulationsheft," * or special congratulatory issue in honour of my father's birthday, so that Dr. Krause's essay, glorifying the older evolutionist, was quite in its place. He wrote to Dr. Krause, thanking him cordially for the honour paid to Erasmus, and asking his permission to publish † an English translation of the Essay.

* The same number contains a good biographical sketch of my father, of which the material was to a large extent supplied by him to the writer, Prof. Preyer of Jena. The article contains an excellent list of my father's publications.

† The wish to do so was shared by his brother, Erasmus Darwin the younger, who continued to be associated with the project.

© The Complete Work of Charles Darwin Online

His chief reason for writing a notice of his grandfather's life was "to contradict flatly some calumnies by Miss Seward." This appears from a letter of March 27, 1879, to his cousin Reginald Darwin, in which he asks for any documents and letters which might throw light on the character of Erasmus. This led to Mr. Reginald Darwin placing in my father's hands a quantity of valuable material, including a curious folio common-place book, of which he wrote: "I have been deeply interested by the great book, ... reading and looking at it is like having communion with the dead . . . . [it] has taught me a good deal about the occupations and tastes of our grandfather." A subsequent letter (April 8) to the same correspondent describes the source of a further supply of material:—

"Since my last letter I have made a strange discovery; for an old box from my father marked 'Old Deeds,' and which consequently I had never opened, I found full of letters—hundreds from Dr. Erasmus—and others from old members of the family: some few very curious. Also a drawing of Elston before it was altered, about 1750, of which I think I will give a copy."

Dr. Krause's contribution formed the second part of the 'Life of Erasmus Darwin,' my father supplying a "preliminary notice." This expression on the title-page is somewhat misleading; my father's contribution is more than half the book, and should have been described as a biography. Work of this kind was new to him, and he wrote doubtfully to Mr. Thiselton Dyer, June 18th: "God only knows what I shall make of his life, it is such a new kind of work to me." The strong interest he felt about his forebears helped to give zest to the work, which became a decided enjoyment to him. With the general public the book was not markedly successful, but many of his friends recognised its merits. Sir J. D. Hooker was one of these, and to him my father wrote, "Your praise of the Life of Dr. D. has pleased me exceed-

© The Complete Work of Charles Darwin Online

ingly, for I despised my work, and thought myself a perfect fool to have undertaken such a job."

To Mr. Galton, too, he wrote, November 14:—

"I am *extremely* glad that you approve of the little 'Life' of our grandfather, for I have been repenting that I ever undertook it, as the work was quite beyond my tether."

The publication of the 'Life of Erasmus Darwin' led to an attack by Mr. Samuel Butler, which amounted to a charge of falsehood against my father. After consulting his friends, he came to the determination to leave the charge unanswered, as being unworthy of his notice.* Those who wish to know more of the matter, may gather the facts of the case from Ernst Krause's 'Charles Darwin,' and they will find Mr. Butler's statement of his grievance in the *Athenæum*, January 31, 1880, and in the *St. James's Gazette*, December 8, 1880. The affair gave my father much pain, but the warm sympathy of those whose opinion he respected soon helped him to let it pass into a well-merited oblivion.

The following letter refers to M. J. H. Fabre's 'Souvenirs Entomologiques.' It may find a place here, as it contains a defence of Erasmus Darwin on a small point. The post-script is interesting, as an example of one of my father's bold ideas both as to experiment and theory:]

C. Darwin to J. H. Fabre.

Down, January 31, 1880.

MY DEAR SIR,—I hope that you will permit me to have the satisfaction of thanking you cordially for the lively pleasure which I have derived from reading your book. Never have the wonderful habits of insects been more vividly described, and it is almost as good to read about them as to

* He had, in a letter to Mr. Butler, expressed his regret at the oversight which caused so much offence.

© The Complete Work of Charles Darwin Online

see them. I feel sure that you would not be unjust to even an insect, much less to a man. Now, you have been misled by some translator, for my grandfather, Erasmus Darwin, states ('Zoonomia,' vol. i. p. 183, 1794) that it was a wasp (guêpe) which he saw cutting off the wings of a large fly. I have no doubt that you are right in saying that the wings are generally cut off instinctively; but in the case described by my grandfather, the wasp, after cutting off the two ends of the body, rose in the air, and was turned round by the wind; he then alighted and cut off the wings. I must believe, with Pierre Huber, that insects have "une petite dose de raison." In the next edition of your book, I hope that you will alter *part* of what you say about my grandfather.

I am sorry that you are so strongly opposed to the Descent theory; I have found the searching for the history of each structure or instinct an excellent aid to observation; and wonderful observer as you are, it would suggest new points to you. If I were to write on the evolution of instincts, I could make good use of some of the facts which you give. Permit me to add, that when I read the last sentence in your book, I sympathised deeply with you.*

With the most sincere respect,

I remain, dear Sir, yours faithfully,

CHARLES DARWIN.

P.S.—Allow me to make a suggestion in relation to your wonderful account of insects finding their way home. I formerly wished to try it with pigeons: namely, to carry the insects in their paper "cornets," about a hundred paces in the opposite direction to that which you ultimately intended to carry them; but before turning round to return, to put the insect in a circular box, with an axle which could be made to

* The book is intended as a memorial of the early death of M. Fabre's son, who had been his father's assistant in his observations on insect life.

© The Complete Work of Charles Darwin Online

revolve very rapidly, first in one direction, and then in another, so as to destroy for a time all sense of direction in the insects. I have sometimes *imagined* that animals may feel in which direction they were at the first start carried.* If this plan failed, I had intended placing the pigeons within an induction coil, so as to disturb any magnetic or dia-magnetic sensibility, which it seems just possible that they may possess. C. D.

[During the latter years of my father's life there was a growing tendency in the public to do him honour. In 1877 he received the honorary degree of LL.D. from the University of Cambridge. The degree was conferred on November 17, and with the customary Latin speech from the Public Orator, concluding with the words: "Tu vero, qui leges naturæ tam docte illustraveris, legum doctor nobis esto."

The honorary degree led to a movement being set on foot in the University to obtain some permanent memorial of my father. A sum of about £400 was subscribed, and after the rejection of the idea that a bust would be the best memorial, a picture was determined on. In June 1879 he sat to Mr. W. Richmond for the portrait in the possession of the University, now placed in the Library of the Philosophical Society at Cambridge. He is represented seated in a Doctor's gown, the head turned towards the spectator: the picture has many admirers, but, according to my own view, neither the attitude nor the expression are characteristic of my father.

A similar wish on the part of the Linnean Society—with which my father was so closely associated—led to his sitting

* This idea was a favourite one with him, and he has described in 'Nature' (vol. vii. 1873, p. 360) the behaviour of his cob Tommy, in whom he fancied he detected a sense of direction. The horse had been taken by rail from Kent to the Isle of Wight; when there he exhibited a marked desire to go eastward, even when his stable lay in the opposite direction. In the same volume of 'Nature,' p. 417, is a letter on the 'Origin of Certain Instincts,' which contains a short discussion on the sense of direction.

© The Complete Work of Charles Darwin Online

in August, 1881, to Mr. John Collier, for the portrait now in the possession of the Society. Of the artist, he wrote, 'Collier was the most considerate, kind and pleasant painter a sitter could desire." The portrait represents him standing facing the observer in the loose cloak so familiar to those who knew him, and with his slouch hat in his hand. Many of those who knew his face most intimately, think that Mr. Collier's picture is the best of the portraits, and in this judgment the sitter himself was inclined to agree. According to my feeling it is not so simple or strong a representation of him as that given by Mr. Ouless. There is a certain expression in Mr. Collier's portrait which I am inclined to consider an exaggeration of the almost painful expression which Professor Cohn has described in my father's face, and which he had previously noticed in Humboldt. Professor Cohn's remarks occur in a pleasantly written account of a visit to Down* in 1876, published in the *Breslauer Zeitung*, April 23, 1882.

Besides the Cambridge degree, he received about the same time honours of an academic kind from some foreign societies.

On August 5, 1878, he was elected a Corresponding Member of the French Institute† in the Botanical Section,‡ and wrote to Dr. Asa Gray:—

"I see that we are both elected Corresponding Members

* In this connection may be mentioned a visit (1881) from another distinguished German, Hans Richter. The occurrence is otherwise worthy of mention, inasmuch as it led to the publication, after my father's death, of Herr Richter's recollections of the visit. The sketch is simply and sympathetically written, and the author has succeeded in giving a true picture of my father as he lived at Down. It appeared in the *Neue Tagblatt* of Vienna, and was republished by Dr. O. Zacharias in his 'Charles R. Darwin,' Berlin, 1882.

† "Lyell always spoke of it as a great scandal that Darwin was so long kept out of the French Institute. As he said, even if the development hypothesis were objected to, Darwin's original works on Coral Reefs, the Cirripedia, and other subjects, constituted a more than sufficient claim."—From Professor Judd's notes.

‡ The statement has been more than once published that he was elected to the Zoological Section, but this was not the case.

© The Complete Work of Charles Darwin Online

of the Institute. It is rather a good joke that I should be elected in the Botanical Section, as the extent of my knowledge is little more than that a daisy is a Compositous plant and a pea a Leguminous one."

In the early part of the same year he was elected a Corresponding Member of the Berlin Academy of Sciences, and he wrote (March 12) to Professor Du Bois Reymond, who had proposed him for election :—

"I thank you sincerely for your most kind letter, in which you announce the great honour conferred on me. The knowledge of the names of the illustrious men, who seconded the proposal is even a greater pleasure to me than the honour itself."

The seconders were Helmholtz, Peters, Ewald, Pringsheim and Virchow.

In 1879 he received the Baly Medal of the Royal College of Physicians.*

He received twenty-six votes out of a possible 39, five blank papers were sent in, and eight votes were recorded for the other candidates.

In 1872 an attempt had been made to elect him to the Section of Zoology, when, however, he only received 15 out of 48 votes, and Lovén was chosen for the vacant place. It appears ('Nature,' August 1, 1872), that an eminent member of the Academy, wrote to *Les Mondes* to the following effect :—

"What has closed the doors of the Academy to Mr. Darwin is that the science of those of his books which have made his chief title to fame—the 'Origin of Species,' and still more the 'Descent of Man,' is not science, but a mass of assertions and absolutely gratuitous hypotheses, often evidently fallacious. This kind of publication and these theories are a bad example, which a body that respects itself cannot encourage."

* The visit to London, necessitated by the presentation of the Baly Medal, was combined with a visit to Miss Forster's house at Abinger, in Surrey, and this was the occasion of the following characteristic letter :—"I must write a few words to thank you cordially for lending us your house. It was a most kind thought, and has pleased me greatly; but I know well that I do not deserve such kindness from any one. On the other hand, no one can be too kind to my dear wife, who is worth her weight in gold many times over, and she was anxious that I should get some complete rest, and here I cannot rest. Your house will be a delightful haven, and again I thank you truly."

© The Complete Work of Charles Darwin Online

Again in 1879 he received from the Royal Academy of Turin the *Bressa* Prize for the years 1875–78, amounting to the sum of 12,000 francs. In the following year he received on his birthday, as on previous occasions, a kind letter of congratulation from Dr. Dohrn of Naples. In writing (February 15th) to thank him and the other naturalists at the Zoological Station, my father added:—

"Perhaps you saw in the papers that the Turin Society honoured me to an extraordinary degree by awarding me the *Bressa* Prize. Now it occurred to me that if your station wanted some piece of apparatus, of about the value of £100, I should very much like to be allowed to pay for it. Will you be so kind as to keep this in mind, and if any want should occur to you, I would send you a cheque at any time."

I find from my father's accounts that £100 was presented to the Naples Station.

He received also several tokens of respect and sympathy of a more private character from various sources. With regard to such incidents, and to the estimation of the public generally, his attitude may be illustrated by a passage from a letter to Mr. Romanes:*—

"You have indeed passed a most magnificent eulogium upon me, and I wonder that you were not afraid of hearing 'oh! oh!' or some other sign of disapprobation. Many persons think that what I have done in science has been much overrated, and I very often think so myself; but my comfort is that I have never consciously done anything to gain applause. Enough and too much about my dear self."

Among such expressions of regard he valued very highly the two photographic albums received from Germany and Holland on his birthday, 1877. Herr Emil Rade of Münster, originated the idea of the German birthday gift, and under-

* The lecture referred to was given at the Dublin meeting of the British Association.

© The Complete Work of Charles Darwin Online

took the necessary arrangements. To him my father wrote (February 16, 1877):—

"I hope that you will inform the one hundred and fifty-four men of science, including some of the most highly honoured names in the world, how grateful I am for their kindness and generous sympathy in having sent me their photographs on my birthday."

To Professor Haeckel he wrote (February 16, 1877):—

"The album has just arrived quite safe. It is most superb.* It is by far the greatest honour which I have ever received, and my satisfaction has been greatly enhanced, by your most kind letter of February 9. . . . I thank you all from my heart. I have written by this post to Herr Rade, and I hope he will somehow manage to thank all my generous friends."

To Professor A. van Bemmelen he wrote, on receiving a similar present from a number of distinguished men and lovers of Natural History in the Netherlands:—

"SIR,—I received yesterday the magnificent present of the album, together with your letter. I hope that you will endeavour to find some means to express to the two hundred and seventeen distinguished observers and lovers of natural science, who have sent me their photographs, my gratitude for their extreme kindness. I feel deeply gratified by this gift, and I do not think that any testimonial more honourable to me could have been imagined. I am well aware that my books could never have been written, and would not have made any impression on the public mind, had not an immense amount of material been collected by a long series of admirable observers; and it is to them that honour is chiefly due. I suppose that every worker at science occasionally feels depressed, and doubts whether what he has published has been worth the labour which it has cost him, but for the few

* The album is magnificently bound and decorated with a beautifully illuminated titlepage, the work of an artist, Herr A. Fitger of Bremen, who also contributed the dedicatory poem.

© The Complete Work of Charles Darwin Online

remaining years of my life, whenever I want cheering, I will look at the portraits of my distinguished co-workers in the field of science, and remember their generous sympathy. When I die, the album will be a most precious bequest to my children. I must further express my obligation for the very interesting history contained in your letter of the progress of opinion in the Netherlands, with respect to Evolution, the whole of which is quite new to me. I must again thank all my kind friends, from my heart, for their ever-memorable testimonial, and I remain, Sir,

Your obliged and grateful servant,

CHARLES R. DARWIN."

[In the June of the following year (1878) he was gratified by learning that the Emperor of Brazil had expressed a wish to meet him. Owing to absence from home my father was unable to comply with this wish; he wrote to Sir J. D. Hooker:—

"The Emperor has done so much for science, that every scientific man is bound to show him the utmost respect, and I hope that you will express in the strongest language, and which you can do with entire truth, how greatly I feel honoured by his wish to see me; and how much I regret my absence from home."

Finally it should be mentioned that in 1880 he received an address personally presented by members of the Council of the Birmingham Philosophical Society, as well as a memorial from the Yorkshire Naturalist Union presented by some of the members, headed by Dr. Sorby. He also received in the same year a visit from some of the members of the Lewisham and Blackheath Scientific Association,—a visit which was, I think, enjoyed by both guests and host.]

© The Complete Work of Charles Darwin Online

MISCELLANEOUS LETTERS—1876–1882.

[The chief incident of a personal kind (not already dealt with) in the years which we are now considering was the death of his brother Erasmus, who died at his house in Queen Anne Street, on August 26th, 1881. My father wrote to Sir J. D. Hooker (Aug. 30):—

"The death of Erasmus is a very heavy loss to all of us, for he had a most affectionate disposition. He always appeared to me the most pleasant and clearest headed man, whom I have ever known. London will seem a strange place to me without his presence; I am deeply glad that he died without any great suffering, after a very short illness from mere weakness and not from any definite disease.*

"I cannot quite agree with you about the death of the old and young. Death in the latter case, when there is a bright future ahead, causes grief never to be wholly obliterated."

An incident of a happy character may also be selected for especial notice, since it was one which strongly moved my father's sympathy. A letter (Dec. 17, 1879) to Sir Joseph Hooker shows that the possibility of a Government Pension being conferred on Mr. Wallace first occurred to my father at this time. The idea was taken up by others, and my father's letters show that he felt the most lively interest in the success of the plan. He wrote, for instance, to Mrs. Fisher, "I hardly ever wished for anything more than I do for the success of our plan." He was deeply pleased when this thoroughly deserved honour was bestowed on his friend, and wrote to the same correspondent (January 7, 1881), on receiving a letter from Mr. Gladstone announcing the fact: "How extraordinarily kind of Mr. Gladstone to find time to write under

* "He was not, I think, a happy man, and for many years did not value life, though never complaining."—From a letter to Sir Thomas Farrer.

© The Complete Work of Charles Darwin Online

the present circumstances.* Good heavens! how pleased I am!"

The letters which follow are of a miscellaneous character and refer principally to the books he read, and to his minor writings.]

C. Darwin to Miss Buckley (Mrs. Fisher).

Down, February 11 [1876].

MY DEAR MISS BUCKLEY,—You must let me have the pleasure of saying that I have just finished reading with very great interest your new book.† The idea seems to me a capital one, and as far as I can judge very well carried out. There is much fascination in taking a bird's eye view of all the grand leading steps in the progress of science. At first I regretted that you had not kept each science more separate; but I dare say you found it impossible. I have hardly any criticisms, except that I think you ought to have introduced Murchison as a great classifier of formations, second only to W. Smith. You have done full justice, and not more than justice, to our dear old master, Lyell. Perhaps a little more ought to have been said about botany, and if you should ever add this, you would find Sachs' 'History,' lately published, very good for your purpose.

You have crowned Wallace and myself with much honour and glory. I heartily congratulate you on having produced so novel and interesting a work, and remain,

My dear Miss Buckley, yours very faithfully,

CH. DARWIN.

* Mr. Gladstone was then in office, and the letter must have been written when he was overwhelmed with business connected with the opening of Parliament (Jan. 6).

† 'A Short History of Natural Science.'

© The Complete Work of Charles Darwin Online

C. Darwin to A. R. Wallace.

[Hopedene] *, June 5, 1876.

MY DEAR WALLACE,—I must have the pleasure of expressing to you my unbounded admiration of your book,† tho' I have read only to page 184—my object having been to do as little as possible while resting. I feel sure that you have laid a broad and safe foundation for all future work on Distribution. How interesting it will be to see hereafter plants treated in strict relation to your views; and then all insects, pulmonate molluscs and fresh-water fishes, in greater detail than I suppose you have given to these lower animals. The point which has interested me most, but I do not say the most valuable point, is your protest against sinking imaginary continents in a quite reckless manner, as was stated by Forbes, followed, alas, by Hooker, and caricatured by Wollaston and [Andrew] Murray! By the way, the main impression that the latter author has left on my mind is his utter want of all scientific judgment. I have lifted up my voice against the above view with no avail, but I have no doubt that you will succeed, owing to your new arguments and the coloured chart. Of a special value, as it seems to me, is the conclusion that we must determine the areas, chiefly by the nature of the mammals. When I worked many years ago on this subject, I doubted much whether the now called Palæarctic and Nearctic regions ought to be separated; and I determined if I made another region that it should be Madagascar. I have, therefore, been able to appreciate your evidence on these points. What progress Palæontology has made during the last 20 years; but if it advances at the same rate in the future, our views on the migration and birth-place of the various groups will, I fear, be greatly altered. I cannot feel quite easy about the Glacial period, and the extinction of large

* Mr. Hensleigh Wedgwood's house in Surrey.

† 'Geographical Distribution,' 1876.

© The Complete Work of Charles Darwin Online

mammals, but I must hope that you are right. I think you will have to modify your belief about the difficulty of dispersal of land molluscs; I was interrupted when beginning to experimentize on the just hatched young adhering to the feet of ground-roosting birds. I differ on one other point, viz. in the belief that there must have existed a Tertiary Antarctic continent, from which various forms radiated to the southern extremities of our present continents. But I could go on scribbling for ever. You have written, as I believe, a grand and memorable work which will last for years as the foundation for all future treatises on Geographical Distribution.

My dear Wallace, yours very sincerely,

CHARLES DARWIN.

P.S.—You have paid me the highest conceivable compliment, by what you say of your work in relation to my chapters on distribution in the 'Origin,' and I heartily thank you for it.

[The following letters illustrate my father's power of taking a vivid interest in work bearing on Evolution, but unconnected with his own special researches at the time. The books referred to in the first letter are Professor Weismann's 'Studien zur Descendenzlehre,'* being part of the series of essays by which the author has done such admirable service to the cause of Evolution:]

C. Darwin to Aug. Weismann.

. . . I read German so slowly, and have had lately to read several other papers, so that I have as yet finished only half of your first essay and two-thirds of your second. They have excited my interest and admiration in the highest degree, and whichever I think of last, seems to me the most

* My father contributed a prefatory note to Mr. Meldola's translation of Prof. Weismann's 'Studien,' 1880–81.

© The Complete Work of Charles Darwin Online

valuable. I never expected to see the coloured marks on caterpillars so well explained; and the case of the ocelli delights me especially. . . .

. . . There is one other subject which has always seemed to me more difficult to explain than even the colours of caterpillars, and that is the colour of birds' eggs, and I wish you would take this up.

C. Darwin to Melchior Neumayr, Vienna.*

Down, Beckenham, Kent, March 9, 1877.

DEAR SIR,—From having been obliged to read other books, I finished only yesterday your essay on 'Die Congerien,' &c.†

I hope that you will allow me to express my gratitude for the pleasure and instruction which I have derived from reading it. It seems to me to be an admirable work; and is by far the best case which I have ever met with, showing the direct influence of the conditions of life on the organization.

Mr. Hyatt, who has been studying the Hilgendorf case, writes to me with respect to the conclusions at which he has arrived, and these are nearly the same as yours. He insists that closely similar forms may be derived from distinct lines of descent; and this is what I formerly called analogical variation. There can now be no doubt that species may become greatly modified through the direct action of the environment. I have some excuse for not having formerly insisted more strongly on this head in my 'Origin of Species,' as most of the best facts have been observed since its publication.

With my renewed thanks for your most interesting essay, and with the highest respect, I remain, dear Sir,

Yours very faithfully,
CHARLES DARWIN.

* Professor of Palæontology at Vienna.

† 'Die Congerien und Paludinenschichten Slavoniens,' 4to, 1875.

© The Complete Work of Charles Darwin Online

C. Darwin to E. S. Morse.

Down, April 23, 1877.

MY DEAR SIR,—You must allow me just to tell you how very much I have been interested with the excellent Address * which you have been so kind as to send me, and which I had much wished to read. I believe that I had read all, or very nearly all, the papers by your countrymen to which you refer, but I have been fairly astonished at their number and importance when seeing them thus put together. I quite agree about the high value of Mr. Allen's works,† as showing how much change may be expected apparently through the direct action of the conditions of life. As for the fossil remains in the West, no words will express how wonderful they are. There is one point which I regret that you did not make clear in your Address, namely what is the meaning and importance of Professors Cope and Hyatt's views on acceleration and retardation. I have endeavoured, and given up in despair, the attempt to grasp their meaning.

Permit me to thank you cordially for the kind feeling shown towards me through your Address, and I remain, my dear Sir,

Yours faithfully,

CH. DARWIN.

[The next letter refers to his 'Biographical Sketch of an Infant,' written from notes made 37 years previously, and published in 'Mind,' July, 1877. The article attracted a good deal of attention, and was translated at the time in 'Kosmos,' and the 'Revue Scientifique,' and has been recently published in Dr. Krause's 'Gesammelte kleinere Schriften von Charles Darwin,' 1887 :]

* "What American Zoologists have done for Evolution," an Address to the American Association for the Advancement of Science, August, 1876. Vol. xxv. of the Proceedings of the Association.

† Mr. J. A. Allen shows the existence of geographical races of birds and mammals. Proc. Boston Soc. Nat. Hist. vol. xv.

© The Complete Work of Charles Darwin Online

*C. Darwin to G. Croom Robertson.**

Down, April 27, 1877.

DEAR SIR,—I hope that you will be so good as to take the trouble to read the enclosed MS., and if you think it fit for publication in your admirable journal of 'Mind,' I shall be gratified. If you do not think it fit, as is very likely, will you please to return it to me. I hope that you will read it in an extra critical spirit, as I cannot judge whether it is worth publishing from having been so much interested in watching the dawn of the several faculties in my own infant. I may add that I should never have thought of sending you the MS., had not M. Taine's article appeared in your Journal.† If my MS. is printed, I think that I had better see a proof. I remain, dear Sir,

Yours faithfully,
CH. DARWIN.

[The two following extracts show the lively interest he preserved in diverse fields of inquiry. Professor Cohn, of Breslau, had mentioned, in a letter, Koch's researches on Splenic Fever; my father replied, January 3 :—

"I well remember saying to myself, between twenty and thirty years ago, that if ever the origin of any infectious disease could be proved, it would be the greatest triumph to science; and now I rejoice to have seen the triumph."

In the spring he received a copy of Dr. E. von Mojsisovics' 'Dolomit Riffe;' his letter to the author (June 1, 1878) is interesting, as bearing on the influence of his own work on the methods of geology.

"I have at last found time to read the first chapter of your 'Dolomit Riffe,' and have been *exceedingly* interested by it. What a wonderful change in the future of geological chronology you indicate, by assuming the descent theory to be

* The editor of 'Mind.'

† 1877, p. 252. The original appeared in the 'Revue Philosophique," 1876.

© The Complete Work of Charles Darwin Online

established, and then taking the graduated changes of the same group of organisms as the true standard! I never hoped to live to see such a step even proposed by any one."

Another geological research which roused my father's admiration was Mr. D. Mackintosh's work on erratic blocks. Apart from its intrinsic merit the work keenly excited his sympathy from the conditions under which it was executed, Mr. Mackintosh being compelled to give nearly his whole time to tuition. The following passage is from a letter to Mr. Mackintosh of October 9, 1879, and refers to his paper in the Journal of the Geological Society, 1878:—

"I hope that you will allow me to have the pleasure of thanking you for the very great pleasure which I have derived from just reading your paper on erratic blocks. The map is wonderful, and what labour each of those lines shows! I have thought for some years that the agency of floating ice, which nearly half a century ago was overrated, has of late been underrated. You are the sole man who has ever noticed the distinction suggested by me * between flat or planed scored rocks, and mammillated scored rocks."]

C. Darwin to C. Ridley.

Down, November 28, 1878.

Dear Sir,—I just skimmed through Dr. Pusey's sermon, as published in the *Guardian*, but it did [not] seem to me worthy of any attention. As I have never answered criticisms excepting those made by scientific men, I am not willing that this letter should be published; but I have no objection to your saying that you sent me the three questions, and that I answered that Dr. Pusey was mistaken in imagining that I wrote the 'Origin' with any relation whatever to Theology. I should have thought that this would have been evident to

* In his paper on the 'Ancient Glaciers of Carnarvonshire,' Phil. Mag. xxi. 1842. See p. 187.

© The Complete Work of Charles Darwin Online

any one who had taken the trouble to read the book, more especially as in the opening lines of the introduction I specify how the subject arose in my mind. This answer disposes of your two other questions; but I may add that, many years ago, when I was collecting facts for the 'Origin,' my belief in what is called a personal God was as firm as that of Dr. Pusey himself, and as to the eternity of matter I have never troubled myself about such insoluble questions. Dr. Pusey's attack will be as powerless to retard by a day the belief in Evolution, as were the virulent attacks made by divines fifty years ago against Geology, and the still older ones of the Catholic Church against Galileo, for the public is wise enough always to follow Scientific men when they agree on any subject; and now there is almost complete unanimity amongst Biologists about Evolution, though there is still considerable difference as to the means, such as how far natural selection has acted, and how far external conditions, or whether there exists some mysterious innate tendency to perfectibility. I remain, dear Sir,

Yours faithfully,
CH. DARWIN.

[Theologians were not the only adversaries of freedom in science. On Sept. 22, 1877, Prof. Virchow delivered an address at the Munich meeting of German Naturalists and Physicians, which had the effect of connecting Socialism with the Descent theory. This point of view was taken up by anti-evolutionists to such an extent that, according to Haeckel, the *Kreuz Zeitung* threw "all the blame" of the "treasonable attempts of the democrats Hödel and Nobiling . . . directly on the theory of Descent." Prof. Haeckel replied with vigour and ability in his 'Freedom in Science and Teaching' (Eng. Transl. 1879), an essay which must have the sympathy of all lovers of freedom.

The following passage from a letter (December 26, 1879) to

© The Complete Work of Charles Darwin Online

Dr. Scherzer, the author of the 'Voyage of the *Novara*,' gives a hint of my father's views on this once burning question:—

"What a foolish idea seems to prevail in Germany on the connection between Socialism and Evolution through Natural Selection."]

*C. Darwin to H. N. Moseley.**

Down, January 20, 1879.

DEAR MOSELEY,—I have just received your book, and I declare that never in my life have I seen a dedication which I admired so much.† Of course I am not a fair judge, but I hope that I speak dispassionately, though you have touched me in my very tenderest point, by saying that my old Journal mainly gave you the wish to travel as a Naturalist. I shall begin to read your book this very evening, and am sure that I shall enjoy it much.

Yours very sincerely,
CH. DARWIN.

C. Darwin to H. N. Moseley.

Down, February 4, 1879.

DEAR MOSELEY,—I have at last read every word of your book, and it has excited in me greater interest than any other scientific book which I have read for a long time. You will perhaps be surprised how slow I have been, but my head prevents me reading except at intervals. If I were asked which parts have interested me most, I should be somewhat

* Professor of Zoology at Oxford. The book alluded to is Prof. Moseley's 'Notes by a Naturalist on the *Challenger*.'

† "To Charles Darwin, Esquire, LL.D., F.R.S., &c., from the study of whose 'Journal of Researches' I mainly derived my desire to travel round the world; to the development of whose theory I owe the principal pleasures and interests of my life, and who has personally given me much kindly encouragement in the prosecution of my studies, this book is, by permission, gratefully dedicated."

© The Complete Work of Charles Darwin Online

puzzled to answer. I fancy that the general reader would prefer your account of Japan. For myself I hesitate between your discussions and description of the Southern ice, which seems to me admirable, and the last chapter which contained many facts and views new to me, though I had read your papers on the stony Hydroid Corals, yet your *résumé* made me realise better than I had done before, what a most curious case it is.

You have also collected a surprising number of valuable facts bearing on the disposal of plants, far more than in any other book known to me. In fact your volume is a mass of interesting facts and discussions, with hardly a superfluous word; and I heartily congratulate you on its publication.

Your dedication makes me prouder than ever.

Believe me, yours sincerely,

CH. DARWIN.

[In November, 1879, he answered for Mr. Galton a series of questions for his 'Inquiries into Human Faculty,' 1883. He wrote to Mr. Galton:—

"I have answered the questions as well as I could, but they are miserably answered, for I have never tried looking into my own mind. Unless others answer very much better than I can do, you will get no good from your queries. Do you not think you ought to have the age of the answerer? I think so, because I can call up faces of many schoolboys, not seen for sixty years, with *much distinctness*, but nowadays I may talk with a man for an hour, and see him several times consecutively, and, after a month, I am utterly unable to recollect what he is at all like. The picture is quite washed out."

The greater number of the answers are given in the annexed table:]

© The Complete Work of Charles Darwin Online

Questions on the Faculty of Visualising.

	QUESTIONS.	REPLIES.
1	*Illumination?*	Moderate, but my solitary breakfast was early, and the morning dark.
2	*Definition?*	Some objects quite defined, a slice of cold beef, some grapes and a pear, the state of my plate when I had finished, and a few other objects, are as distinct as if I had photos before me.
3	*Completeness?*	Very moderately so.
4	*Colouring?*	The objects above-named, perfectly coloured.
5	*Extent of Field of View.*	Rather small.
	DIFFERENT KINDS OF IMAGERY.	
6	*Printed pages?*	I cannot remember a single sentence, but I remember the place of the sentence and the kind of type.
7	*Furniture?*	I have never attended to it.
8	*Persons?*	I remember the faces of persons formerly well-known vividly, and can make them do anything I like.
9	*Scenery?*	Remembrance vivid and distinct, and gives me pleasure.
10	*Geography?*	No.
11	*Military movements?*	No.
12	*Mechanism?*	Never tried.
13	*Geometry?*	I do not think I have any power of the kind.
14	*Numerals?*	When I think of any number, printed figures arise before my mind. I can't remember for an hour four consecutive figures.
15	*Card playing?*	Have not played for many years, but I am sure should not remember.
16	*Chess?*	Never played.

© The Complete Work of Charles Darwin Online

[In 1880 he published a short paper in 'Nature' (vol. xxi. p. 207) on the "Fertility of Hybrids from the common and Chinese goose." He received the hybrids from the Rev. Dr. Goodacre, and was glad of the opportunity of testing the accuracy of the statement that these species are fertile *inter se.* This fact, which was given in the 'Origin' on the authority of Mr. Eyton, he considered the most remarkable as yet recorded with respect to the fertility of hybrids. The fact (as confirmed by himself and Dr. Goodacre) is of interest as giving another proof that sterility is no criterion of specific difference, since the two species of goose now shown to be fertile *inter se* are so distinct that they have been placed by some authorities in distinct genera or subgenera.

The following letter refers to Mr. Huxley's lecture: "The Coming of Age of the Origin of Species," * given at the Royal Institution, April 9, 1880, published in 'Nature,' and in 'Science and Culture,' p. 310:]

C. Darwin to T. H. Huxley.

Abinger Hall, Dorking, Sunday, April 11, 1880.

MY DEAR HUXLEY,—I wished much to attend your Lecture, but I have had a bad cough, and we have come here to see whether a change would do me good, as it has done. What a magnificent success your lecture seems to

* This same "Coming of Age" was the subject of an address from the Council of the Otago Institute. It is given in 'Nature,' February 24, 1881.

© The Complete Work of Charles Darwin Online

have been, as I judge from the reports in the *Standard* and *Daily News*, and more especially from the accounts given me by three of my children. I suppose that you have not written out your lecture, so I fear there is no chance of its being printed *in extenso*. You appear to have piled, as on so many other occasions, honours high and thick on my old head. But I well know how great a part you have played in establishing and spreading the belief in the descent-theory, ever since that grand review in the *Times* and the battle royal at Oxford up to the present day.

Ever, my dear Huxley,

Yours sincerely and gratefully,

CHARLES DARWIN.

P.S.—It was absurdly stupid in me, but I had read the announcement of your Lecture, and thought that you meant the maturity of the subject, until my wife one day remarked, "it is almost twenty-one years since the 'Origin' appeared," and then for the first time the meaning of your words flashed on me!

[In the above-mentioned lecture Mr. Huxley made a strong point of the accumulation of palæontological evidence which the years between 1859 and 1880 have given us in favour of Evolution. On this subject my father wrote (August 31, 1880):]

MY DEAR PROFESSOR MARSH,—I received some time ago your very kind note of July 28th, and yesterday the magnificent volume.* I have looked with renewed admiration at the plates, and will soon read the text. Your work on these old birds, and on the many fossil animals of North America, has afforded the best support to the theory of Evolution,

* Odontornithes. A monograph on the extinct Toothed Birds of N. America. 1880. By O. C. Marsh.

© The Complete Work of Charles Darwin Online

which has appeared within the last twenty years.* The general appearance of the copy which you have sent me is worthy of its contents, and I can say nothing stronger than this.

With cordial thanks, believe me,

Yours very sincerely,

CHARLES DARWIN.

[In November, 1880, he received an account of a flood in Brazil, from which his friend Fritz Müller had barely escaped with his life. My father immediately wrote to Hermann Müller anxiously enquiring whether his brother had lost books, instruments, &c., by this accident, and begging in that case "for the sake of science, so that science should not suffer," to be allowed to help in making good the loss. Fortunately, however, the injury to Fitz Müller's possessions was not so great as was expected, and the incident remains only as a memento, which I trust cannot be otherwise than pleasing to the survivor, of the friendship of the two naturalists.

In 'Nature' (November 11, 1880) appeared a letter from my father, which is, I believe, the only instance in which he wrote publicly with anything like severity. The late Sir Wyville Thomson wrote, in the Introduction to the 'Voyage of the *Challenger*': "The character of the abyssal fauna refuses to give the least support to the theory which refers the evolution of species to extreme variation guided only by natural selection." My father, after characterising these remarks as a "standard of criticism, not uncommonly reached by theologians and metaphysicians," goes on to take

* Mr. Huxley has well pointed out ('Science and Culture,' p. 317) that: "In 1875, the discovery of the toothed birds of the cretaceous formation in N. America, by Prof. Marsh, completed the series of transitional forms between birds and reptiles, and removed Mr. Darwin's proposition that, 'many animal forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other vertebrate classes,' from the region of hypothesis to that of demonstrable fact."

© The Complete Work of Charles Darwin Online

exception to the term "extreme variation," and challenges Sir Wyville to name any one who has "said that the evolution of species depends only on natural selection." The letter closes with an imaginary scene between Sir Wyville and a breeder, in which Sir Wyville criticises artificial selection in a somewhat similar manner. The breeder is silent, but on the departure of his critic he is supposed to make use of "emphatic but irreverent language about naturalists." The letter, as originally written, ended with a quotation from Sedgwick on the invulnerability of those who write on what they do not understand, but this was omitted on the advice of a friend, and curiously enough a friend whose combativeness in the good cause my father had occasionally curbed.]

C. Darwin to G. J. Romanes.

Down, April 16, 1881.

MY DEAR ROMANES,—My MS. on 'Worms' has been sent to the printers, so I am going to amuse myself by scribbling to you on a few points; but you must not waste your time in answering at any length this scribble.

Firstly, your letter on intelligence was very useful to me and I tore up and re-wrote what I sent to you. I have not attempted to define intelligence; but have quoted your remarks on experience, and have shown how far they apply to worms. It seems to me that they must be said to work with some intelligence, anyhow they are not guided by a blind instinct.

Secondly, I was greatly interested by the abstract in 'Nature' of your work on Echinoderms,* the complexity with simplicity, and with such curious co-ordination of the nervous system is marvellous; and you showed me before what splendid gymnastic feats they can perform.

* "On the locomotor system of Echinoderms," by G. J. Romanes and J. Cossar Ewart. 'Philosophical Transactions,' 1881, p. 829.

R 2

© The Complete Work of Charles Darwin Online

Thirdly, Dr. Roux has sent me a book just published by him: 'Der Kampf der Theile,' &c., 1881 (240 pages in length).

He is manifestly a well-read physiologist and pathologist, and from his position a good anatomist. It is full of reasoning, and this in German is very difficult to me, so that I have only skimmed through each page; here and there reading with a little more care. As far as I can imperfectly judge, it is the most important book on Evolution which has appeared for some time. I believe that G. H. Lewes hinted at the same fundamental idea, viz. that there is a struggle going on within every organism between the organic molecules, the cells and the organs. I think that his basis is, that every cell which best performs its function is, in consequence, at the same time best nourished and best propagates its kind. The book does not touch on mental phenomena, but there is much discussion on rudimentary or atrophied parts, to which subject you formerly attended. Now if you would like to read this book, I would send it. . . . If you read it, and are struck with it (but I may be *wholly* mistaken about its value), you would do a public service by analysing and criticising it in 'Nature.'

Dr. Roux makes, I think, a gigantic oversight in never considering plants; these would simplify the problem for him.

Fourthly, I do not know whether you will discuss in your book on the mind of animals any of the more complex and wonderful instincts. It is unsatisfactory work, as there can be no fossilised instincts, and the sole guide is their state in other members of the same order, and mere *probability*.

But if you do discuss any (and it will perhaps be expected of you), I should think that you could not select a better case than that of the sand wasps, which paralyse their prey, as formerly described by Fabre, in his wonderful paper in the 'Annales des Sciences,' and since amplified in his admirable 'Souvenirs.'

© The Complete Work of Charles Darwin Online

Whilst reading this latter book, I speculated a little on the subject. Astonishing nonsense is often spoken of the sand wasp's knowledge of anatomy. Now will any one say that the Gauchos on the plains of La Plata have such knowledge, yet I have often seen them pith a struggling and lassoed cow on the ground with unerring skill, which no mere anatomist could imitate. The pointed knife was infallibly driven in between the vertebræ by a single slight thrust. I presume that the art was first discovered by chance, and that each young Gaucho sees exactly how the others do it, and then with a very little practice learns the art. Now I suppose that the sand wasps originally merely killed their prey by stinging them in many places (see p. 129 of Fabre's 'Souvenirs,' and p. 241) on the lower and softest side of the body—and that to sting a certain segment was found by far the most successful method; and was inherited like the tendency of a bulldog to pin the nose of a bull, or of a ferret to bite the cerebellum. It would not be a very great step in advance to prick the ganglion of its prey only slightly, and thus to give its larvæ fresh meat instead of old dried meat. Though Fabre insists so strongly on the unvarying character of instinct, yet it is shown that there is some variability, as at p. 176, 177.

I fear that I shall have utterly wearied you with my scribbling and bad handwriting.

My dear Romanes, yours very sincerely,

CH. DARWIN.

Postscript of a Letter to Professor A. Agassiz, May 5th, 1881:—

"I read with much interest your address before the American Association. However true your remarks on the genealogies of the several groups may be, I hope and believe that you have over-estimated the difficulties to be encountered in the future:—A few days after reading your address, I interpreted

© The Complete Work of Charles Darwin Online

to myself your remarks on one point (I hope in some degree correctly) in the following fashion :—

Any character of an ancient, generalised, or intermediate form may, and often does, re-appear in its descendants, after countless generations, and this explains the extraordinarily complicated affinities of existing groups. This idea seems to me to throw a flood of light on the lines, sometimes used to represent affinities, which radiate in all directions, often to very distant sub-groups,—a difficulty which has haunted me for half a century. A strong case could be made out in favour of believing in such reversion after immense intervals of time. I wish the idea had been put into my head in old days, for I shall never again write on difficult subjects, as I have seen too many cases of old men becoming feeble in their minds, without being in the least conscious of it. If I have interpreted your ideas at all correctly, I hope that you will re-urge, on any fitting occasion, your view. I have mentioned it to a few persons capable of judging, and it seemed quite new to them. I beg you to forgive the proverbial garrulity of old age.

C. D."

[The following letter refers to Sir J. D. Hooker's Geographical address at the York Meeting (1881) of the British Association :]

C. Darwin to J. D. Hooker.

Down, August 6, 1881.

MY DEAR HOOKER,—For Heaven's sake never speak of boring me, as it would be the greatest pleasure to aid you in the slightest degree and your letter has interested me exceedingly. I will go through your points seriatim, but I have never attended much to the history of any subject, and my memory has become atrociously bad. It will therefore be a mere chance whether any of my remarks are of any use.

© The Complete Work of Charles Darwin Online

Your idea, to show what travellers have done, seems to me a brilliant and just one, especially considering your audience.

1. I know nothing about Tournefort's works.

2. I believe that you are fully right in calling Humboldt the greatest scientific traveller who ever lived. I have lately read two or three volumes again. His Geology is funny stuff; but that merely means that he was not in advance of his age. I should say he was wonderful, more for his near approach to omniscience than for originality. Whether or not his position as a scientific man is as eminent as we think, you might truly call him the parent of a grand progeny of scientific travellers, who, taken together, have done much for science.

3. It seems to me quite just to give Lyell (and secondarily E. Forbes) a very prominent place.

4. Dana was, I believe, the first man who maintained the permanence of continents and the great oceans. . . . When I read the '*Challenger's*' conclusion that sediment from the land is not deposited at greater distances than 200 to 300 miles from the land, I was much strengthened in my old belief. Wallace seems to me to have argued the case excellently. Nevertheless, I would speak, if I were in your place, rather cautiously; for T. Mellard Reade has argued lately with some force against the view; but I cannot call to mind his arguments. If forced to express a judgment, I should abide by the view of approximate permanence since Cambrian days.

5. The extreme importance of the Arctic fossil plants, is self-evident. Take the opportunity of groaning over [our] ignorance of the Lignite Plants of Kerguelen Land, or any Antarctic land. It might do good.

6. I cannot avoid feeling sceptical about the travelling of plants from the North *except during the Tertiary period*. It may of course have been so and probably was so from one of the two poles at the earliest period, during Pre-Cambrian ages; but such speculations seem to me hardly scientific, seeing how little we know of the old Floras.

© The Complete Work of Charles Darwin Online

I will now jot down without any order a few miscellaneous remarks.

I think you ought to allude to Alph. De Candolle's great book, for though it (like almost everything else) is washed out of my mind, yet I remember most distinctly thinking it a very valuable work. Anyhow, you might allude to his excellent account of the history of all cultivated plants.

How shall you manage to allude to your New Zealand and Tierra del Fuego work? if you do not allude to them you will be scandalously unjust.

The many Angiosperm plants in the Cretacean beds of the United States (and as far as I can judge the age of these beds has been fairly well made out) seems to me a fact of very great importance, so is their relation to the existing flora of the United States under an Evolutionary point of view. Have not some Australian extinct forms been lately found in Australia? or have I dreamed it?

Again, the recent discovery of plants rather low down in our Silurian beds is very important.

Nothing is more extraordinary in the history of the Vegetable Kingdom, as it seems to me, than the *apparently* very sudden or abrupt development of the higher plants. I have sometimes speculated whether there did not exist somewhere during long ages an extremely isolated continent, perhaps near the South Pole.

Hence I was greatly interested by a view which Saporta propounded to me, a few years ago, at great length in MS. and which I fancy he has since published, as I urged him to do—viz., that as soon as flower-frequenting insects were developed, during the latter part of the secondary period, an enormous impulse was given to the development of the higher plants by cross-fertilization being thus suddenly formed.

A few years ago I was much struck with Axel Blytt's* Essay showing from observation, on the peat beds in Scandi-

* See footnote, Vol. iii. p. 215.

© The Complete Work of Charles Darwin Online

navia, that there had apparently been long periods with more rain and other with less rain (perhaps connected with Croll's recurrent astronomical periods), and that these periods had largely determined the present distribution of the plants of Norway and Sweden. This seemed to me a very important essay.

I have just read over my remarks and I fear that they will not be of the slightest use to you.

I cannot but think that you have got through the hardest, or at least the most difficult, part of your work in having made so good and striking a sketch of what you intend to say; but I can quite understand how you must groan over the great necessary labour.

I most heartily sympathise with you on the successes of B. and R.: as years advance what happens to oneself becomes of very little consequence, in comparison with the careers of our children.

Keep your spirits up, for I am convinced that you will make an excellent address.

Ever yours affectionately,
CHARLES DARWIN.

[In September he wrote :—

"I have this minute finished reading your splendid but too short address. I cannot doubt that it will have been fully appreciated by the Geographers at York; if not, they are asses and fools."]

C. Darwin to John Lubbock.

Sunday evening [1881].

MY DEAR L.,—Your address * has made me think over what have been the great steps in Geology during the last fifty years, and there can be no harm in telling you my impression. But it is very odd that I cannot remember what

* Presidential Address at the York Meeting of the British Association.

© The Complete Work of Charles Darwin Online

you have said on Geology. I suppose that the classification of the Silurian and Cambrian formations must be considered the greatest or most important step; for I well remember when all these older rocks were called grau-wacke, and nobody dreamed of classing them; and now we have three azoic formations pretty well made out beneath the Cambrian! But the most striking step has been the discovery of the Glacial period: you are too young to remember the prodigious effect this produced about the year 1840 (?) on all our minds. Elie de Beaumont never believed in it to the day of his death! the study of the glacial deposits led to the study of the superficial drift, which was formerly *never studied* and called Diluvium, as I well remember. The study under the microscope of rock-sections is another not inconsiderable step. So again the making out of cleavage and the foliation of the metamorphic rocks. But I will not run on, having now eased my mind. Pray do not waste even one minute in acknowledging my horrid scrawls.

Ever yours,
CH. DARWIN.

[The following extracts referring to the late Francis Maitland Balfour,* show my father's estimate of his work and intellectual qualities, but they give merely an indication of his strong appreciation of Balfour's most loveable personal character:—

From a letter to Fritz Müller, January 5, 1882:—

"Your appreciation of Balfour's book ['Comparative Embryology'] has pleased me excessively, for though I could not properly judge of it, yet it seemed to me one of the most remarkable books which have been published for some considerable time. He is quite a young man, and if he keeps

* Professor of Animal Morphology at Cambridge. He was born 1851, and was killed, with his guide, on the Aiguille Blanche, near Courmayeur, in July, 1882.

© The Complete Work of Charles Darwin Online

his health, will do splendid work. . . . He has a fair fortune of his own, so that he can give up his whole time to Biology. He is very modest, and very pleasant, and often visits here and we like him very much."

From a letter to Dr. Dohrn, February 13, 1882:—

"I have got one very bad piece of news to tell you, that F. Balfour is very ill at Cambridge with typhoid fever. . . . I hope that he is not in a very dangerous state; but the fever is severe. Good Heavens, what a loss he would be to Science, and to his many loving friends!"]

C. Darwin to T. H. Huxley.

Down, January 12, 1882.

MY DEAR HUXLEY,—Very many thanks for 'Science and Culture,' and I am sure that I shall read most of the essays with much interest. With respect to Automatism,* I wish that you could review yourself in the old, and of course forgotten, trenchant style, and then you would here answer yourself with equal incisiveness; and thus, by Jove, you might go on *ad infinitum*, to the joy and instruction of the world.

Ever yours very sincerely,
CHARLES DARWIN.

[The following letter refers to Dr. Ogle's translation of Aristotle, 'On the Parts of Animals' (1882):]

C. Darwin to W. Ogle.

Down, February 22, 1882.

MY DEAR DR. OGLE,—You must let me thank you for the pleasure which the introduction to the Aristotle book

* "On the hypothesis that animals are automata and its history," an Address given at the Belfast meeting of the British Association, 1874, and published in the 'Fortnightly Review,' 1874, and in 'Science and Culture.'

© The Complete Work of Charles Darwin Online

has given me. I have rarely read anything which has interested me more, though I have not read as yet more than a quarter of the book proper.

From quotations which I had seen, I had a high notion of Aristotle's merits, but I had not the most remote notion what a wonderful man he was. Linnæus and Cuvier have been my two gods, though in very different ways, but they were mere schoolboys to old Aristotle. How very curious, also, his ignorance on some points, as on muscles as the means of movement. I am glad that you have explained in so probable a manner some of the grossest mistakes attributed to him. I never realized, before reading your book, to what an enormous summation of labour we owe even our common knowledge. I wish old Aristotle could know what a grand Defender of the Faith he had found in you. Believe me, my dear Dr. Ogle,

Yours very sincerely,
CH. DARWIN.

[In February, he received a letter and a specimen from a Mr. W. D. Crick, which illustrated a curious mode of dispersal of bivalve shells, namely, by closure of their valves so as to hold on to the leg of a water-beetle. This class of fact had a special charm for him, and he wrote to 'Nature' describing the case.*

In April, he received a letter from Dr. W. Van Dyck, Lecturer in Zoology at the Protestant College of Beyrout. The letter showed that the street dogs of Beyrout had been rapidly mongrelised by introduced European dogs, and the facts have an interesting bearing on my father's theory of Sexual Selection.]

* 'Nature,' April 6, 1882.

© The Complete Work of Charles Darwin Online

C. Darwin to W. Van Dyck.

Down, April 3, 1882.

Dear Sir,—After much deliberation, I have thought it best to send your very interesting paper to the Zoological Society, in hopes that it will be published in their Journal. This journal goes to every scientific institution in the world, and the contents are abstracted in all year-books on Zoology. Therefore I have preferred it to 'Nature,' though the latter has a wider circulation, but is ephemeral.

I have prefaced your essay by a few general remarks, to which I hope that you will not object.

Of course I do not know that the Zoological Society, which is much addicted to mere systematic work, will publish your essay. If it does, I will send you copies of your essay, but these will not be ready for some months. If not published by the Zoological Society, I will endeavour to get 'Nature' to publish it. I am very anxious that it should be published and preserved. Dear Sir,

Yours faithfully,
Ch. Darwin.

[The paper was read at a meeting of the Zoological Society on April 18th—the day before my father's death.

The preliminary remarks with which Dr. Van Dyck's paper is prefaced are thus the latest of my father's writings.]

We must now return to an early period of his life, and give a connected account of his botanical work, which has hitherto been omitted.

© The Complete Work of Charles Darwin Online

CHAPTER VII.

FERTILISATION OF FLOWERS.

[In the letters already given we have had occasion to notice the general bearing of a number of botanical problems on the wider question of Evolution. The detailed work in botany which my father accomplished by the guidance of the light cast on the study of natural history by his own work on Evolution remains to be noticed. In a letter to Mr. Murray, September 24th, 1861, speaking of his book on the 'Fertilisation of Orchids,' he says: "It will perhaps serve to illustrate how Natural History may be worked under the belief of the modification of species." This remark gives a suggestion as to the value and interest of his botanical work, and it might be expressed in far more emphatic language without danger of exaggeration.

In the same letter to Mr. Murray, he says: "I think this little volume will do good to the 'Origin,' as it will show that I have worked hard at details." It is true that his botanical work added a mass of corroborative detail to the case for Evolution, but the chief support to his doctrines given by these researches was of another kind. They supplied an argument against those critics who have so freely dogmatised as to the uselessness of particular structures, and as to the consequent impossibility of their having been developed by means of natural selection. His observations on Orchids enabled him to say: "I can show the meaning of some of the apparently meaningless ridges, horns; who will now

© The Complete Work of Charles Darwin Online

venture to say that this or that structure is useless?" A kindred point is expressed in a letter to Sir J. D. Hooker (May 14th, 1862):—

"When many parts of structure, as in the woodpecker, show distinct adaptation to external bodies, it is preposterous to attribute them to the effects of climate, &c., but when a single point alone, as a hooked seed, it is conceivable it may thus have arisen. I have found the study of Orchids eminently useful in showing me how nearly all parts of the flower are co-adapted for fertilisation by insects, and therefore the results of natural selection,—even the most trifling details of structure."

One of the greatest services rendered by my father to the study of Natural History is the revival of Teleology. The evolutionist studies the purpose or meaning of organs with the zeal of the older Teleology, but with far wider and more coherent purpose. He has the invigorating knowledge that he is gaining not isolated conceptions of the economy of the present, but a coherent view of both past and present. And even where he fails to discover the use of any part, he may, by a knowledge of its structure, unravel the history of the past vicissitudes in the life of the species. In this way a vigour and unity is given to the study of the forms of organised beings, which before it lacked. This point has already been discussed in Mr. Huxley's chapter on the 'Reception of the *Origin of Species*,' and need not be here considered. It does, however, concern us to recognize that this "great service to natural science," as Dr. Gray describes it, was effected almost as much by his special botanical work as by the 'Origin of Species.'

For a statement of the scope and influence of my father's botanical work, I may refer to Mr. Thiselton Dyer's article in 'Charles Darwin,' one of the *Nature Series*. Mr. Dyer's wide knowledge, his friendship with my father, and especially his power of sympathising with the work of others, combine

© The Complete Work of Charles Darwin Online

to give this essay a permanent value. The following passage (p. 43) gives a true picture :—

"Notwithstanding the extent and variety of his botanical work, Mr. Darwin always disclaimed any right to be regarded as a professed botanist. He turned his attention to plants, doubtless because they were convenient objects for studying organic phenomena in their least complicated forms; and this point of view, which, if one may use the expression without disrespect, had something of the amateur about it, was in itself of the greatest importance. For, from not being, till he took up any point, familiar with the literature bearing on it, his mind was absolutely free from any prepossession. He was never afraid of his facts, or of framing any hypothesis, however startling, which seemed to explain them. . . . In any one else such an attitude would have produced much work that was crude and rash. But Mr. Darwin—if one may venture on language which will strike no one who had conversed with him as over-strained—seemed by gentle persuasion to have penetrated that reserve of nature which baffles smaller men. In other words, his long experience had given him a kind of instinctive insight into the method of attack of any biological problem, however unfamiliar to him, while he rigidly controlled the fertility of his mind in hypothetical explanations by the no less fertility of ingeniously devised experiment."

To form any just idea of the greatness of the revolution worked by my father's researches in the study of the fertilisation of flowers, it is necessary to know from what a condition this branch of knowledge has emerged. It should be remembered that it was only during the early years of the present century that the idea of sex, as applied to plants, became firmly established. Sachs, in his 'History of Botany' (1875), has given some striking illustrations of the remarkable slowness with which its acceptance gained ground. He remarks that when we consider the experimental proofs given

© The Complete Work of Charles Darwin Online

by Camerarius (1694), and by Kölreuter (1761–66), it appears incredible that doubts should afterwards have been raised as to the sexuality of plants. Yet he shows that such doubts did actually repeatedly crop up. These adverse criticisms rested for the most part on careless experiments, but in many cases on *à priori* arguments. Even as late as 1820, a book of this kind, which would now rank with circle squaring, or flat-earth philosophy, was seriously noticed in a botanical journal.

A distinct conception of sex as applied to plants had not long emerged from the mists of profitless discussion and feeble experiment, at the time when my father began botany by attending Henslow's lectures at Cambridge.

When the belief in the sexuality of plants had become established as an incontrovertible piece of knowledge, a weight of misconception remained, weighing down any rational view of the subject. Camerarius * believed (naturally enough in his day) that hermaphrodite flowers are necessarily self-fertilised. He had the wit to be astonished at this, a degree of intelligence which, as Sachs points out, the majority of his successors did not attain to.

The following extracts from a note-book show that this point occurred to my father as early as 1837:—

"Do not plants which have male and female organs together [*i.e.* in the same flower] yet receive influence from other plants? Does not Lyell give some argument about varieties being difficult to keep [true] on account of pollen from other plants? Because this may be applied to show all plants do receive intermixture."

Sprengel, † indeed, understood that the hermaphrodite structure of flowers by no means necessarily leads to self-fertilisation. But although he discovered that in many cases pollen is of necessity carried to the stigma of another *flower*, he did not understand that in the advantage gained by the

* Sachs, 'Geschichte,' p. 419.

† Christian Conrad Sprengel, born 1750, died 1816.

© The Complete Work of Charles Darwin Online

intercrossing of distinct *plants* lies the key to the whole question. Hermann Müller has well remarked that this "omission was for several generations fatal to Sprengel's work. . . . . For both at the time and subsequently, botanists felt above all the weakness of his theory, and they set aside, along with his defective ideas, his rich store of patient and acute observations and his comprehensive and accurate interpretations." It remained for my father to convince the world that the meaning hidden in the structure of flowers was to be found by seeking light in the same direction in which Sprengel, seventy years before, had laboured. Robert Brown was the connecting link between them; for although, according to Dr. Gray,* Brown, in common with the rest of the world, looked on Sprengel's ideas as fantastic, yet it was at his recommendation that my father in 1841 read Sprengel's now celebrated 'Secret of Nature Displayed.† The book impressed him as being "full of truth," although "with some little nonsense." It not only encouraged him in kindred speculation, but guided him in his work, for in 1844 he speaks of verifying Sprengel's observations. It may be doubted whether Robert Brown ever planted a more fruitful seed than in putting such a book into such hands.

A passage in the 'Autobiography' (vol. i. p. 90) shows how it was that my father was attracted to the subject of fertilisation: "During the summer of 1839, and I believe during the previous summer, I was led to attend to the cross-fertilisation of flowers by the aid of insects, from having come to the conclusion in my speculations on the origin of species, that crossing played an important part in keeping specific forms constant."

The original connection between the study of flowers and the problem of Evolution is curious, and could hardly have been predicted. Moreover, it was not a permanent bond.

* 'Nature,' 1874, p. 80.

† 'Das entdeckte Geheimniss der Natur im Baue und in der Befruchtung der Blumen.' Berlin, 1793.

© The Complete Work of Charles Darwin Online

As soon as the idea arose that the offspring of cross-fertilisation is, in the struggle for life, likely to conquer the seedlings of self-fertilised parentage, a far more vigorous belief in the potency of natural selection in moulding the structure of flowers is attained. A central idea is gained towards which experiment and observation may be directed.

Dr. Gray has well remarked with regard to this central idea ('Nature,' June 4, 1874):—"The aphorism, 'Nature abhors a vacuum,' is a characteristic specimen of the science of the middle ages. The aphorism, 'Nature abhors close fertilisation,' and the demonstration of the principle, belong to our age and to Mr. Darwin. To have originated this, and also the principle of Natural Selection . . . . and to have applied these principles to the system of nature, in such a manner as to make, within a dozen years, a deeper impression upon natural history than has been made since Linnæus, is ample title for one man's fame."

The flowers of the Papilionaceæ attracted his attention early, and were the subject of his first paper on fertilisation.* The following extract from an undated letter to Dr. Asa Gray seems to have been written before the publication of this paper, probably in 1856 or 1857:—

". . . . What you say on Papilionaceous flowers is very true; and I have no facts to show that varieties are crossed; but yet (and the same remark is applicable in a beautiful way to Fumaria and Dielytra, as I noticed many years ago), I must believe that the flowers are constructed partly in direct relation to the visits of insects; and how insects can avoid bringing pollen from other individuals I cannot understand. It is really pretty to watch the action of a Humble-bee on the scarlet kidney bean, and in this genus (and in *Lathyrus*

* *Gardeners' Chronicle*, 1857, p. 725. It appears that this paper was a piece of "over-time" work. He wrote to a friend, "that confounded leguminous paper was done in the afternoon, and the consequence was I had to go to Moor Park for a week."

© The Complete Work of Charles Darwin Online

grandiflorus) the honey is so placed that the bee invariably alights on that *one* side of the flower towards which the spiral pistil is protruded (bringing out with it pollen), and by the depression of the wing-petal is forced against the bee's side all dusted with pollen.* In the broom the pistil is rubbed on the centre of the back of the bee. I suspect there is something to be made out about the Leguminosæ, which will bring the case within *our* theory; though I have failed to do so. Our theory will explain why in the vegetable and animal kingdom the act of fertilisation even in hermaphrodites usually takes place sub-jove, though thus exposed to *great* injury from damp and rain. In animals which cannot be [fertilised] by insects or wind, there is *no case* of *land*-animals being hermaphrodite without the concourse of two individuals."

A letter to Dr. Asa Gray (Sept. 5th, 1857) gives the substance of the paper in the *Gardeners' Chronicle*:—

"Lately I was led to examine buds of kidney bean with the pollen shed; but I was led to believe that the pollen could *hardly* get on the stigma by wind or otherwise, except by bees visiting [the flower] and moving the wing petals: hence I included a small bunch of flowers in two bottles in every way treated the same: the flowers in one I daily just momentarily moved, as if by a bee; these set three fine pods, the other *not one*. Of course this little experiment must be tried again, and this year in England it is too late, as the flowers seem now seldom to set. If bees are necessary to this flower's self-fertilisation, bees must almost cross them, as their dusted right-side of head and right legs constantly touch the stigma.

"I have, also, lately been re-observing daily *Lobelia fulgens*—this in my garden is never visited by insects, and never sets

* If you will look at a bed of scarlet kidney beans you will find that the wing-petals on the *left* side alone are all scratched by the tarsi of the bees. [Note in the original letter by C. Darwin.]

© The Complete Work of Charles Darwin Online

seeds, without pollen be put on the stigma (whereas the small blue Lobelia is visited by bees and does set seed); I mention this because there are such beautiful contrivances to prevent the stigma ever getting its own pollen; which seems only explicable on the doctrine of the advantage of crosses."

The paper was supplemented by a second in 1858.* The chief object of these publications seems to have been to obtain information as to the possibility of growing varieties of leguminous plants near each other, and yet keeping them true. It is curious that the Papilionaceæ should not only have been the first flowers which attracted his attention by their obvious adaptation to the visits of insects, but should also have constituted one of his sorest puzzles. The common pea and the sweet pea gave him much difficulty, because, although they are as obviously fitted for insect-visits as the rest of the order, yet their varieties keep true. The fact is that neither of these plants being indigenous, they are not perfectly adapted for fertilisation by British insects. He could not, at this stage of his observations, know that the co-ordination between a flower and the particular insect which fertilises it may be as delicate as that between a lock and its key, so that this explanation was not likely to occur to him.†

Besides observing the Leguminosæ, he had already begun, as shown in the foregoing extracts, to attend to the structure of other flowers in relation to insects. At the beginning of 1860 he worked at Leschenaultia,‡ which at first puzzled him,

* *Gardeners' Chronicle*, 1858, p. 828. In 1861 another paper on Fertilisation appeared in the *Gardeners' Chronicle*, p. 552, in which he explained the action of insects on *Vinca major*. He was attracted to the periwinkle by the fact that it is not visited by insects and never sets seeds.

† He was of course alive to variety in the habits of insects. He published a short note in the *Entomologist's Weekly Intelligencer*, 1860, asking whether the Tineina and other small moths suck flowers.

‡ He published a short paper on the manner of fertilisation of this flower, in the *Gardeners' Chronicle*, 1871, p. 1166.

© The Complete Work of Charles Darwin Online

but was ultimately made out. A passage in a letter chiefly relating to Leschenaultia seems to show that it was only in the spring of 1860 that he began widely to apply his knowledge to the relation of insects to other flowers. This is somewhat surprising, when we remember that he had read Sprengel many years before. He wrote (May 14):—

"I should look at this curious contrivance as specially related to visits of insects; as I begin to think is almost universally the case."

Even in July 1862 he wrote to Dr. Asa Gray:—

"There is no end to the adaptations. Ought not these cases to make one very cautious when one doubts about the use of all parts? I fully believe that the structure of all irregular flowers is governed in relation to insects. Insects are the Lords of the floral (to quote the witty *Athenæum*) world."

He was probably attracted to the study of Orchids by the fact that several kinds are common near Down. The letters of 1860 show that these plants occupied a good deal of his attention; and in 1861 he gave part of the summer and all the autumn to the subject. He evidently considered himself idle for wasting time on Orchids which ought to have been given to 'Variation under Domestication.' Thus he wrote:—

"There is to me incomparably more interest in observing than in writing; but I feel quite guilty in trespassing on these subjects, and not sticking to varieties of the confounded cocks, hens and ducks. I hear that Lyell is savage at me. I shall never resist Linum next summer."

It was in the summer of 1860 that he made out one of the most striking and familiar facts in the book, namely, the manner in which the pollen masses in Orchis are adapted for removal by insects. He wrote to Sir J. D. Hooker July 12:—

"I have been examining *Orchis pyramidalis*, and it almost

© The Complete Work of Charles Darwin Online

equals, perhaps even beats, your Listera case; the sticky glands are congenitally united into a saddle-shaped organ, which has great power of movement, and seizes hold of a bristle (or proboscis) in an admirable manner, and then another movement takes place in the pollen masses, by which they are beautifully adapted to leave pollen on the two *lateral* stigmatic surfaces. I never saw anything so beautiful."

In June of the same year he wrote:—

"You speak of adaptation being rarely *visible*, though present in plants. I have just recently been looking at the common Orchis, and I declare I think its adaptations in every part of the flower quite as beautiful and plain, or even more beautiful than in the Woodpecker. I have written and sent a notice for the *Gardeners' Chronicle*,* on a curious difficulty in the Bee Orchis, and should much like to hear what you think of the case. In this article I have incidentally touched on adaptation to visits of insects; but the contrivance to keep the sticky glands fresh and sticky beats almost everything in nature. I never remember having seen it described, but it must have been, and, as I ought not in my book to give the observation as my own, I should be very glad to know where this beautiful contrivance is described."

He wrote also to Dr. Gray, June 8, 1860:—

"Talking of adaptation, I have lately been looking at our common orchids, and I dare say the facts are as old and well-known as the hills, but I have been so struck with admiration at the contrivances, that I have sent a notice to the *Gardeners' Chronicle*. The *Ophrys apifera*, offers, as you will see, a curious contradiction in structure."

Besides attending to the fertilisation of the flowers he was already, in 1860, busy with the homologies of the parts, a

* June 9, 1860. This seems to have attracted some attention, especially among entomologists, as it was reprinted in the *Entomologist's Weekly Intelligencer*, 1860.

© The Complete Work of Charles Darwin Online

subject of which he made good use in the Orchid book. He wrote to Sir Joseph Hooker (July):—

"It is a real good joke my discussing homologies of Orchids with you, after examining only three or four genera; and this very fact makes me feel positive I am right!! I do not quite understand some of your terms; but sometime I must get you to explain the homologies; for I am intensely interested on the subject, just as at a game of chess."

This work was valuable from a systematic point of view. In 1880 he wrote to Mr. Bentham:—

"It was very kind in you to write to me about the Orchideæ, for it has pleased me to an extreme degree that I could have been of the *least* use to you about the nature of the parts."

The pleasure which his early observations on Orchids gave him is shown in such extracts as the following from a letter to Sir J. D. Hooker (July 27, 1861):—

"You cannot conceive how the Orchids have delighted me. They came safe, but box rather smashed; cylindrical old cocoa- or snuff-canister much safer. I enclose postage. As an account of the movement, I shall allude to what I suppose is Oncidium, to make *certain*,—is the enclosed flower with crumpled petals this genus? Also I most specially want to know what the enclosed little globular brown Orchid is. I have only seen pollen of a Cattleya on a bee, but surely have you not unintentionally sent me what I wanted most (after Catasetum or Mormodes), viz. one of the Epidendreæ?! I *particularly* want (and will presently tell you why) another spike of this little Orchid, with older flowers, some even almost withered."

His delight in observation is again shown in a letter to Dr. Gray (1863). Referring to Crüger's letters from Trinidad, he wrote:—"Happy man, he has actually seen crowds of bees flying round Catasetum, with the pollinia sticking to their backs!"

© The Complete Work of Charles Darwin Online

The following extracts of letters to Sir J. D. Hooker illustrate further the interest which his work excited in him:—

"Veitch sent me a grand lot this morning. What wonderful structures!

"I have now seen enough, and you must not send me more, for though I enjoy looking at them *much*, and it has been very useful to me, seeing so many different forms, it is idleness. For my object each species requires studying for days. I wish you had time to take up the group. I would give a good deal to know what the rostellum is, of which I have traced so many curious modifications. I suppose it cannot be one of the stigmas,* there seems a great tendency for two lateral stigmas to appear. My paper, though touching on only subordinate points will run, I fear, to 100 MS. folio pages! The beauty of the adaptation of parts seems to me unparalleled. I should think or guess waxy pollen was most differentiated. In Cypripedium which seems least modified, and a much exterminated group, the grains are single. In *all others*, as far as I have seen, they are in packets of four; and these packets cohere into many wedge-formed masses in Orchis; into eight, four, and finally two. It seems curious that a flower should exist, which could *at most* fertilise only two other flowers, seeing how abundant pollen generally is; this fact I look at as explaining the perfection of the contrivance by which the pollen, so important from its fewness, is carried from flower to flower" (1861).

"I was thinking of writing to you to-day, when your note with the Orchids came. What frightful trouble you have taken about Vanilla; you really must not take an atom more; for the Orchids are more play than real work. I have been much interested by Epidendrum, and have worked all morning at them; for heaven's sake, do not corrupt me by any more" (August 30, 1861).

* It is a modification of the upper stigma.

© The Complete Work of Charles Darwin Online

He originally intended to publish his notes on Orchids as a paper in the Linnean Society's Journal, but it soon became evident that a separate volume would be a more suitable form of publication. In a letter to Sir J. D. Hooker, Sept. 24, 1861, he writes:—

"I have been acting, I fear that you will think, like a goose; and perhaps in truth I have. When I finished a few days ago my Orchis paper, which turns out 140 folio pages!! and thought of the expense of woodcuts, I said to myself, I will offer the Linnean Society to withdraw it, and publish it in a pamphlet. It then flashed on me that perhaps Murray would publish it, so I gave him a cautious description, and offered to share risks and profits. This morning he writes that he will publish and take all risks, and share profits and pay for all illustrations. It is a risk, and heaven knows whether it will not be a dead failure, but I have not deceived Murray, and [have] told him that it would interest those alone who cared much for natural history. I hope I do not exaggerate the curiosity of the many special contrivances."

He wrote the two following letters to Mr. Murray about the publication of the book:]

Down, Sept. 21 [1861].

My dear Sir,—Will you have the kindness to give me your opinion, which I shall implicitly follow. I have just finished a very long paper intended for Linnean Society (the title is enclosed), and yesterday for the first time it occurred to me that *possibly* it might be worth publishing separately, which would save me trouble and delay. The facts are new, and have been collected during twenty years and strike me as curious. Like a Bridgewater treatise, the chief object is to show the perfection of the many contrivances in Orchids. The subject of propagation is interesting to most people, and is treated in my paper so that any woman could read it. Parts are dry and purely scientific; but I

© The Complete Work of Charles Darwin Online

think my paper would interest a good many of such persons who care for Natural History, but no others.

. . . It would be a very little book, and I believe you think very little books objectionable. I have myself *great* doubts on the subject. I am very apt to think that my geese are swans; but the subject seems to me curious and interesting.

I beg you not to be guided in the least in order to oblige me, but as far as you can judge, please give me your opinion. If I were to publish separately, I would agree to any terms, such as half risk and half profit, or what you liked; but I would not publish on my sole risk, for to be frank, I have been told that no publisher whatever, under such circumstances, cares for the success of a book.

C. Darwin to J. Murray.

Down, Sept. 24 [1861].

MY DEAR SIR,—I am very much obliged for your note and very liberal offer. I have had some qualms and fears. All that I can feel sure of is that the MS. contains many new and curious facts, and I am sure the Essay would have interested me, and will interest those who feel lively interest in the wonders of nature; but how far the public will care for such minute details, I cannot at all tell. It is a bold experiment; and at worst, cannot entail much loss; as a certain amount of sale will, I think, be pretty certain. A large sale is out of the question. As far as I can judge, generally the points which interest me I find interest others; but I make the experiment with fear and trembling,—not for my own sake, but for yours. . . .

[On Sept. 28th he wrote to Sir J. D. Hooker:—

"What a good soul you are not to sneer at me, but to pat me on the back. I have the greatest doubt whether I am not going to do, in publishing my paper, a most ridiculous thing.

© The Complete Work of Charles Darwin Online

It would annoy me much, but only for Murray's sake, if the publication were a dead failure."

There was still much work to be done, and in October he was still receiving Orchids from Kew, and wrote to Hooker:—

"It is impossible to thank you enough. I was almost mad at the wealth of Orchids." And again—

"Mr. Veitch most generously has sent me two splendid buds of Mormodes, which will be capital for dissection, but I fear will never be irritable; so for the sake of charity and love of heaven do, I beseech you, observe what movement takes place in Cychnoches, and what part must be touched. Mr. V. has also sent me one splendid flower of Catasetum, the most wonderful Orchid I have seen."

On Oct. 13th he wrote to Sir Joseph Hooker:—

"It seems that I cannot exhaust your good nature. I have had the hardest day's work at Catasetum and buds of Mormodes, and believe I understand at last the mechanism of movements and the functions. Catasetum is a beautiful case of slight modification of structure leading to new functions. I never was more interested in any subject in my life than in this of Orchids. I owe very much to you."

Again to the same friend, Nov. 1, 1861:—

"If you really can spare another Catasetum, when nearly ready, I shall be most grateful; had I not better send for it? The case is truly marvellous; the (so-called) sensation, or stimulus from a light touch is certainly transmitted through the antennæ for more than one inch *instantaneously*. . . . A cursed insect or something let my last flower off last night."

Professor de Candolle has remarked * of my father, "Ce n'est pas lui qui aurait demandé de construire des palais pour y loger des laboratoires." This was singularly true of his orchid work, or rather it would be nearer the truth to say that he had no laboratory, for it was only after the publication

* 'Darwin considéré, &c.,' 'Archives des Sciences Physiques et Naturelles,' 3ème période. Tome vii. 481, 1882 (May).

© The Complete Work of Charles Darwin Online

of the 'Fertilisation of Orchids,' that he built himself a greenhouse. He wrote to Sir J. D. Hooker (Dec. 24th, 1862):—

"And now I am going to tell you a *most* important piece of news!! I have almost resolved to build a small hot-house; my neighbour's really first-rate gardener has suggested it, and offered to make me plans, and see that it is well done, and he is really a clever fellow, who wins lots of prizes, and is very observant. He believes that we should succeed with a little patience; it will be a grand amusement for me to experiment with plants."

Again he wrote (Feb. 15th, 1863):—

"I write now because the new hot-house is ready, and I long to stock it, just like a schoolboy. Could you tell me pretty soon what plants you can give me; and then I shall know what to order? And do advise me how I had better get such plants as you can *spare*. Would it do to send my tax-cart early in the morning, on a day that was not frosty, lining the cart with mats, and arriving here before night? I have no idea whether this degree of exposure (and of course the cart would be cold) could injure stove-plants; they would be about five hours (with bait) on the journey home."

A week later he wrote:—

"You cannot imagine what pleasure your plants give me (far more than your dead Wedgwood ware can give you); H. and I go and gloat over them, but we privately confessed to each other, that if they were not our own, perhaps we should not see such transcendent beauty in each leaf."

And in March, when he was extremely unwell he wrote:—

"A few words about the Stove-plants; they do so amuse me. I have crawled to see them two or three times. Will you correct and answer, and return enclosed. I have hunted in all my books and cannot find these names,* and I like much to know the family."

* His difficulty with regard to the names of plants is illustrated, with regard to a Lupine on which he was at work, in an extract from

© The Complete Work of Charles Darwin Online

The book was published May 15th, 1862. Of its reception he writes to Mr. Murray, June 13th and 18th:—

"The Botanists praise my Orchid-book to the skies. Some one sent me (perhaps you) the 'Parthenon,' with a good review. The *Athenæum* * treats me with very kind pity and contempt; but the reviewer knew nothing of his subject."

"There is a superb, but I fear exaggerated, review in the 'London Review.' † But I have not been a fool, as I thought I was, to publish; ‡ for Asa Gray, about the most competent judge in the world, thinks almost as highly of the book as does the 'London Review.' The *Athenæum* will hinder the sale greatly."

The Rev. M. J. Berkeley was the author of the notice in the 'London Review,' as my father learned from Sir J. D. Hooker, who added, "I thought it very well done indeed. I have read a good deal of the Orchid-book, and echo all he says."

To this my father replied (June 30th, 1862):—

"MY DEAR OLD FRIEND,—You speak of my warming the cockles of your heart, but you will never know how often you have warmed mine. It is not your approbation of my scientific work (though I care for that more than for any one's): it is something deeper. To this day I remember keenly a letter you wrote to me from Oxford, when I was at the Water-cure, and how it cheered me when I was utterly weary of life.

---

a letter (July 21, 1866) to Sir J. D. Hooker: "I sent to the nursery garden, whence I bought the seed, and could only hear that it was 'the common blue Lupine,' the man saying 'he was no scholard, and did not know Latin, and that parties who make experiments ought to find out the names.'"

* May 24, 1862.

† June 14, 1862.

‡ Doubts on this point still, however, occurred to him about this time. He wrote to Prof. Oliver (June 8): "I am glad that you have read my Orchis-book and seem to approve of it; for I never published anything which I so much doubted whether it was worth publishing, and indeed I still doubt. The subject interested me beyond what, I suppose, it is worth."

© The Complete Work of Charles Darwin Online

Well, my Orchis-book is a success (but I do not know whether it sells)."

In another letter to the same friend, he wrote :—

"You have pleased me much by what you say in regard to Bentham and Oliver approving of my book ; for I had got a sort of nervousness, and doubted whether I had not made an egregious fool of myself, and concocted pleasant little stinging remarks for reviews, such as 'Mr. Darwin's head seems to have been turned by a certain degree of success, and he thinks that the most trifling observations are worth publication.' "

Mr. Bentham's approval was given in his Presidential Address to the Linnean Society, May 24, 1862, and was all the more valuable, because it came from one who was by no means supposed to be favourable to Evolutionary doctrines.]

C. Darwin to Asa Gray.

Down, June 10 [1862].

MY DEAR GRAY,—Your generous sympathy makes you over-estimate what you have read of my Orchid-book. But your letter of May 18th and 26th has given me an almost foolish amount of satisfaction. The subject interested me, I knew, beyond its real value ; but I had lately got to think that I had made myself a complete fool by publishing in a semi-popular form. Now I shall confidently defy the world. I have heard that Bentham and Oliver approve of it ; but I have heard the opinion of no one else whose opinion is worth a farthing. . . . No doubt my volume contains much error : how curiously difficult it is to be accurate, though I try my utmost. Your notes have interested me beyond measure. I can now afford to d— my critics with ineffable complacency of mind. Cordial thanks for this benefit. It is surprising to me that you should have strength of mind to care for science, amidst the awful events daily occurring in your country. I daily look at the *Times* with almost as much interest as an American could do.

© The Complete Work of Charles Darwin Online

When will peace come? it is dreadful to think of the desolation of large parts of your magnificent country; and all the speechless misery suffered by many. I hope and think it not unlikely that we English are wrong in concluding that it will take a long time for prosperity to return to you. It is an awful subject to reflect on. . . .

[Dr. Asa Gray reviewed the book in 'Silliman's Journal,'* where he speaks, in strong terms, of the fascination which it must have for even slightly instructed readers. He made, too, some original observations on an American orchid, and these first-fruits of the subject, sent in MS. or proof sheet to my father, were welcomed by him in a letter (July 23rd):—

"Last night, after writing the above, I read the great bundle of notes. Little did I think what I had to read. What admirable observations! You have distanced me on my own hobby-horse! I have not had for weeks such a glow of pleasure as your observations gave me."

The next letter refers to the publication of the review:]

C. Darwin to Asa Gray.

Down, July 28, [1862].

MY DEAR GRAY,—I hardly know what to thank for first. Your stamps gave infinite satisfaction. I took him † first one lot, and then an hour afterwards another lot. He actually raised himself on one elbow to look at them. It was the first animation he showed. He said only: "You must thank Professor Gray awfully." In the evening after a long silence, there came out the oracular sentence: "He is awfully kind." And indeed you are, overworked as you are, to take so much trouble for our

* 'Silliman's Journal,' vol. xxiv. p. 138. Here is given an account of the fertilisation of *Platanthera Hookeri*. *P. hyperborea* is discussed in Dr. Gray's 'Enumeration' in the same volume, p. 259; also, with other species, in a second notice of the Orchid-book at p. 420.

† One of his boys who was ill.

© The Complete Work of Charles Darwin Online

poor dear little man.—And now I must begin the "awfullys" on my own account: what a capital notice you have published on the Orchids! It could not have been better; but I fear that you overrate it. I am very sure that I had not the least idea that you or any one would approve of it so much. I return your last note for the chance of your publishing any notice on the subject; but after all perhaps you may not think it worth while; yet in my judgment *several* of your facts, especially *Platanthera hyperborea*, are *much* too good to be merged in a review. But I have always noticed that you are prodigal in originality in your reviews. . . .

[Sir Joseph Hooker reviewed the book in the *Gardeners' Chronicle*, writing in a successful imitation of the style of Lindley, the Editor. My father wrote to Sir Joseph (Nov. 12, 1862):—

"So you did write the review in the *Gardeners' Chronicle.* Once or twice I doubted whether it was Lindley; but when I came to a little slap at R. Brown, I doubted no longer. You arch-rogue! I do not wonder you have deceived others also. Perhaps I am a conceited dog; but if so, you have much to answer for; I never received so much praise, and coming from you I value it much more than from any other."

With regard to botanical opinion generally, he wrote to Dr. Gray, "I am fairly astonished at the success of my book with botanists." Among naturalists who were not botanists, Lyell was pre-eminent in his appreciation of the book. I have no means of knowing when he read it, but in later life, as I learn from Professor Judd, he was enthusiastic in praise of the 'Fertilisation of Orchids,' which he considered "next to the 'Origin,' as the most valuable of all Darwin's works." Among the general public the author did not at first hear of many disciples, thus he wrote to his cousin Fox in September 1862: "Hardly any one not a botanist, except yourself, as far as I know, has cared for it."

© The Complete Work of Charles Darwin Online

A favourable notice appeared in the *Saturday Review*, October 18th, 1862; the reviewer points out that the book would escape the angry polemics aroused by the 'Origin.'* This is illustrated by a review in the *Literary Churchman*, in which only one fault is found, namely, that Mr. Darwin's expression of admiration at the contrivances in orchids is too indirect a way of saying, "O Lord, how manifold are Thy works!"

A somewhat similar criticism occurs in the 'Edinburgh Review' (October 1862). The writer points out that Mr. Darwin constantly uses phrases, such as "beautiful contrivance," "the labellum is . . . *in order* to attract," "the nectar is *purposely* lodged." The Reviewer concludes his discussion thus: "We know, too, that these purposes and ideas are not our own, but the ideas and purposes of Another."

The 'Edinburgh' reviewer's treatment of his subject was criticised in the *Saturday Review*, November 15th, 1862. With reference to this article my father wrote to Sir Joseph Hooker (December 29th, 1862):—

"Here is an odd chance; my nephew Henry Parker, an Oxford Classic, and Fellow of Oriel, came here this evening; and I asked him whether he knew who had written the little article in the *Saturday*, smashing the [Edinburgh reviewer], which we liked; and after a little hesitation he owned he had. I never knew that he wrote in the *Saturday*; and was it not an odd chance?"

The 'Edinburgh' article was written by the Duke of Argyll, and has since been made use of in his 'Reign of Law,' 1867. Mr. Wallace replied † to the Duke's criticisms, making some especially good remarks on those which refer to orchids. He shows how, by a "beautiful self-acting adjustment," the nectary of the orchid Angræcum (from 10 to 14 inches in

* Dr. Gray pointed out that if the Orchid-book (with a few trifling omissions) had appeared before the 'Origin,' the author would have been canonised rather than anathematised by the natural theologians.

† 'Quarterly Journal of Science,' October 1867. Republished in 'Natural Selection,' 1871.

© The Complete Work of Charles Darwin Online

length), and the proboscis of a moth sufficiently long to reach the nectar, might be developed by natural selection. He goes on to point out that on any other theory we must suppose that the flower was created with an enormously long nectary, and that then by a special act, an insect was created fitted to visit the flower, which would otherwise remain sterile. With regard to this point my father wrote (October 12 or 13, 1867):—

"I forgot to remark how capitally you turn the tables on the Duke, when you make him create the Angræcum and Moth by special creation."

If we examine the literature relating to the fertilisation of flowers, we do not find that this new branch of study showed any great activity immediately after the publication of the Orchid-book. There are a few papers by Asa Gray, in 1862 and 1863, by Hildebrand in 1864, and by Moggridge in 1865, but the great mass of work by Axell, Delpino, Hildebrand, and the Müllers, did not begin to appear until about 1867. The period during which the new views were being assimilated, and before they became thoroughly fruitful, was, however, surprisingly short. The later activity in this department may be roughly gauged by the fact that the valuable 'Bibliography,' given by Prof. D'Arcy Thompson in his translation of Müller's 'Befruchtung' (1883), contains references to 814 papers.

Besides the book on Orchids, my father wrote two or three papers on the subject, which will be found mentioned in the Appendix. The earliest of these, on the three sexual forms of Catasetum, was published in 1862; it is an anticipation of part of the Orchid-book, and was merely published in the Linnean Society's Journal, in acknowledgment of the use made of a specimen in the Society's possession. The possibility of apparently distinct species being merely sexual forms of a single species, suggested a characteristic experiment, which is alluded to in the following letter to one of his earliest disciples in the study of the fertilisation of flowers:]

T 2

© The Complete Work of Charles Darwin Online

*C. Darwin to J. Traherne Moggridge.**

Down, October 13 [1865].

MY DEAR SIR,—I am especially obliged to you for your beautiful plates and letter-press; for no single point in natural history interests and perplexes me so much as the self-fertilisation † of the Bee-orchis. You have already thrown some light on the subject, and your present observations promise to throw more.

I formed two conjectures: first, that some insect during certain seasons might cross the plants, but I have almost given up this; nevertheless, pray have a look at the flowers next season. Secondly, I conjectured that the Spider and Bee-orchids might be a crossing and self-fertile form of the same species. Accordingly I wrote some years ago to an acquaintance, asking him to mark some Spider-orchids, and observe whether they retained the same character; but he evidently thought the request as foolish as if I had asked him to mark one of his cows with a ribbon, to see if it would turn next spring into a horse. Now will you be so kind as to tie a string round the stem of half-a-dozen Spider-orchids, and when you leave Mentone dig them up, and I would try and cultivate them and see if they kept constant; but I should require to know in what sort of soil and situations they grow. It would be indispensable to mark the plant so that there could be no mistake about the individual. It is also just possible that the same plant would throw up, at different seasons different flower-scapes, and the marked plants would serve as evidence.

With many thanks, my dear sir,

Yours sincerely,

CH. DARWIN.

* The late Mr. Moggridge, author of 'Harvesting Ants and Trap-door Spiders,' 'Flora of Mentone,' &c.

† He once remarked to Dr. Norman Moore that one of the things that made him wish to live a few thousand years, was his desire to see the extinction of the Bee-orchis,—an end to which he believed its self-fertilising habit was leading.

© The Complete Work of Charles Darwin Online

P.S.—I send by this post my paper on climbing plants, parts of which you might like to read.

[Sir Thomas Farrer and Dr. W. Ogle were also guided and encouraged by my father in their observations. The following refers to a paper by Sir Thomas Farrer, in the 'Annals and Magazine of Natural History,' 1868, on the fertilisation of the Scarlet Runner:]

C. Darwin to T. H. Farrer.

Down, Sept. 15, 1868.

MY DEAR MR. FARRER,—I grieve to say that the *main* features of your case are known. I am the sinner and described them some ten years ago. But I overlooked many details, as the appendage to the single stamen, and several other points. I send my notes, but I must beg for their return, as I have *no other copy*. I quite agree, the facts are most striking, especially as you put them. Are you sure that the Hive-bee is the cutter? it is against my experience. If sure, make the point more prominent, or if not sure, erase it. I do not think the subject is quite new enough for the Linnean Society; but I dare say the 'Annals and Magazine of Natural History,' or *Gardeners' Chronicle* would gladly publish your observations, and it is a great pity they should be lost. If you like I would send your paper to either quarter with a note. In this case you must give a title, and your name, and perhaps it would be well to premise your remarks with a line of reference to my paper stating that you had observed independently and more fully.

I have read my own paper over after an interval of several years, and am amused at the caution with which I put the case that the final end was for crossing distinct individuals, of which I was then as fully convinced as now, but I knew that the doctrine would shock all botanists. Now the opinion is becoming familiar.

© The Complete Work of Charles Darwin Online

To see penetration of pollen-tubes is not difficult, but in most cases requires some practice with dissecting under a one-tenth of an inch focal distance single lens; and just at first this will seem to you extremely difficult.

What a capital observer you are—a first-rate Naturalist has been sacrificed, or partly sacrificed, to Public life.

Believe me, yours very sincerely,

CH. DARWIN.

P.S.—If you come across any large Salvia, look at it—the contrivance is admirable. It went to my heart to tell a man who came here a few weeks ago with splendid drawings and MS. on Salvia, that the work had been all done in Germany.*

[The following extract is from a letter, November 26th, 1868, to Sir Thomas Farrer, written as I learn from him, "in answer to a request for some advice as to the best modes of observation."

"In my opinion the best plan is to go on working and making copious notes, without much thought of publication, and then if the results turn out striking publish them. It is my impression, but I do not feel sure that I am right, that the best and most novel plan would be, instead of describing the means of fertilisation in particular plants, to investigate the part which certain structures play with all plants or throughout certain orders; for instance, the brush of hairs on the style, or the diadelphous condition of the stamens in the Leguminosæ, or the hairs within the corolla, &c. &c. Looking to your note, I think that this is perhaps the plan which you suggest.

It is well to remember that Naturalists value observations

* Dr. W. Ogle, the observer of the fertilisation of Salvia here alluded to, published his results in the 'Pop. Science Review,' 1869. He refers both gracefully and gratefully to his relationship with my father in the introduction to his translation of Kerner's 'Flowers and their Unbidden Guests.'

© The Complete Work of Charles Darwin Online

far more than reasoning; therefore your conclusions should be as often as possible fortified by noticing how insects actually do the work."

In 1869, Sir Thomas Farrer corresponded with my father on the fertilisation of Passiflora and of Tacsonia. He has given me his impressions of the correspondence:—

"I had suggested that the elaborate series of *chevaux-de-frise*, by which the nectary of the common Passiflora is guarded, were specially calculated to protect the flower from the stiff-beaked humming birds which would not fertilize it, and to facilitate the access of the little proboscis of the humble bee, which would do so; whilst, on the other hand, the long pendent tube and flexible valve-like corona which retains the nectar of Tacsonia would shut out the bee, which would not, and admit the humming bird which would, fertilize that flower. The suggestion is very possibly worthless, and could only be verified or refuted by examination of flowers in the countries where they grow naturally. . . . What interested me was to see that on this as on almost any other point of detailed observation, Mr. Darwin could always say, 'Yes; but at one time I made some observations myself on this particular point; and I think you will find, &c. &c.' That he should after years of interval remember that he had noticed the peculiar structure to which I was referring in the *Passiflora princeps* struck me at the time as very remarkable."

With regard to the spread of a belief in the adaptation of flowers for cross-fertilisation, my father wrote to Mr. Bentham April 22, 1868:—

"Most of the criticisms which I sometimes meet with in French works against the frequency of crossing, I am certain are the result of mere ignorance. I have never hitherto found the rule to fail that when an author describes the structure of a flower as specially adapted for self-fertilisation, it is really adapted for crossing. The Fumariaceæ offer a

© The Complete Work of Charles Darwin Online

good instance of this, and Treviranus threw this order in my teeth; but in Corydalis, Hildebrand shows how utterly false the idea of self-fertilisation is. This author's paper on Salvia is really worth reading, and I have observed some species, and know that he is accurate."

The next letter refers to Professor Hildebrand's paper on Corydalis, published in the 'Proc. Internat. Hort. Congress,' London, 1866, and in Pringsheim's 'Jahrbücher,' vol. v. The memoir on Salvia alluded to is contained in the previous volume of the same Journal:]

*C. Darwin to F. Hildebrand.**

Down, May 16 [1866].

My dear Sir,—The state of my health prevents my attending the Hort. Congress; but I forwarded yesterday your paper to the secretary, and if they are not overwhelmed with papers, yours will be gladly received. I have made many observations on the Fumariaceæ, and convinced myself that they were adapted for insect agency; but I never observed anything nearly so curious as your most interesting facts. I hope you will repeat your experiments on the Corydalis on a larger scale, and especially on several distinct plants; for your plant might have been individually peculiar, like certain individual plants of Lobelia, &c., described by Gärtner, and of Passiflora and Orchids described by Mr. Scott. . . .

Since writing to you before, I have read your admirable memoir on Salvia, and it has interested me almost as much as when I first investigated the structure of Orchids. Your paper illustrates several points in my 'Origin of Species,' especially the transition of organs. Knowing only two or three species in the genus, I had often marvelled how one cell of the anther could have been transformed into the movable plate or spoon; and how well you show the gradations;

* Professor of Botany at Freiburg.

© The Complete Work of Charles Darwin Online

but I am surprised that you did not more strongly insist on this point.

I shall be still more surprised if you do not ultimately come to the same belief with me, as shown by so many beautiful contrivances, that all plants require, from some unknown cause, to be occasionally fertilized by pollen from a distinct individual. With sincere respect, believe me, my dear Sir,

Yours very faithfully,

CH. DARWIN.

[The following letter refers to the late Hermann Müller's 'Befruchtung der Blumen,' by far the most valuable of the mass of literature originating in the 'Fertilisation of Orchids.' An English translation, by Prof. D'Arcy Thompson was published in 1883. My father's "Prefatory Notice" to this work is dated February 6, 1882, and is therefore almost the last of his writings:]

C. Darwin to H. Müller.

Down, May 5, 1873.

MY DEAR SIR,—Owing to all sorts of interruptions and to my reading German so slowly, I have read only to p. 88 of your book; but I must have the pleasure of telling you how very valuable a work it appears to me. Independently of the many original observations, which of course form the most important part, the work will be of the highest use as a means of reference to all that has been done on the subject. I am fairly astonished at the number of species of insects, the visits of which to different flowers you have recorded. You must have worked in the most indefatigable manner. About half a year ago the editor of 'Nature' suggested that it would be a grand undertaking if a number of naturalists were to do what you have already done on so large a scale with respect to the visits of insects. I have been particularly glad to read your historical sketch, for I had never before seen all the references

© The Complete Work of Charles Darwin Online

put together. I have sometimes feared that I was in error when I said that C. K. Sprengel did not fully perceive that cross-fertilisation was the final end of the structure of flowers; but now this fear is relieved, and it is a great satisfaction to me to believe that I have aided in making his excellent book more generally known. Nothing has surprised me more than to see in your historical sketch how much I myself have done on the subject, as it never before occurred to me to think of all my papers as a whole. But I do not doubt that your generous appreciation of the labours of others has led you to over-estimate what I have done. With very sincere thanks and respect, believe me,

Yours faithfully,
CHARLES DARWIN.

P.S.—I have mentioned your book to almost every one who, as far as I know, cares for the subject in England; and I have ordered a copy to be sent to our Royal Society.

[The next letter, to Dr. Behrens, refers to the same subject as the last:]

C. Darwin to W. Behrens.

Down, August 29 [1878].

DEAR SIR,—I am very much obliged to you for having sent me your 'Geschichte der Bestaubungs-Theorie,' * and which has interested me much. It has put some things in a new light, and has told me other things which I did not know. I heartily agree with you in your high appreciation of poor old C. Sprengel's work; and one regrets bitterly that he did not live to see his labours thus valued. It rejoices me also to notice how highly you appreciate H. Müller, who has always seemed to me an admirable observer and reasoner. I am at present endeavouring to persuade an English publisher to bring out a translation of his 'Befruchtung.'

* Progr. der K. Gewerbschule zu Elberfeld, 1877, 1878.

© The Complete Work of Charles Darwin Online

Lastly, permit me to thank you for your very generous remarks on my works. By placing what I have been able to do on this subject in systematic order, you have made me think more highly of my own work than I ever did before! Nevertheless, I fear that you have done me more than justice.

I remain, dear Sir, yours faithfully and obliged,

CHARLES DARWIN.

[The letter which follows was called forth by Dr. Gray's article in 'Nature,' to which reference has already been made, and which appeared June 4, 1874:]

C. Darwin to Asa Gray.

Down, June 3 [1874].

MY DEAR GRAY,—I was rejoiced to see your handwriting again in your note of the 4th, of which more anon. I was astonished to see announced about a week ago that you were going to write in 'Nature' an article on me, and this morning I received an advance copy. It is the grandest thing ever written about me, especially as coming from a man like yourself. It has deeply pleased me, particularly some of your side remarks. It is a wonderful thing to me to live to see my name coupled in any fashion with that of Robert Brown. But you are a bold man, for I am sure that you will be sneered at by not a few botanists. I have never been so honoured before, and I hope it will do me good and make me try to be as careful as possible; and good heavens, how difficult accuracy is! I feel a very proud man, but I hope this won't last. . . .

[Fritz Müller has observed that the flowers of Hedychium are so arranged that the pollen is removed by the wings of hovering butterflies. My father's prediction of this observation is given in the following letter:—]

© The Complete Work of Charles Darwin Online

C. Darwin to H. Müller.

Down, August 7, 1876.

. . . . I was much interested by your brother's article on Hedychium ; about two years ago I was so convinced that the flowers were fertilized by the tips of the wings of large moths, that I wrote to India to ask a man to observe the flowers and catch the moths at work, and he sent me 20 to 30 Sphinx-moths, but so badly packed that they all arrived in fragments ; and I could make out nothing. . . .

Yours sincerely,
CH. DARWIN.

[The following extract from a letter (Feb. 25, 1864), to Dr. Gray refers to another prediction fulfilled :—

" I have of course seen no one, and except good dear Hooker, I hear from no one. He, like a good and true friend, though so overworked, often writes to me.

" I have had one letter which has interested me greatly, with a paper, which will appear in the Linnean Journal, by Dr. Crüger of Trinidad, which shows that I am all right about Catasetum, even to the spot where the pollinia adhere to the bees, which visit the flower, as I said, to gnaw the labellum. Crüger's account of Coryanthes and the use of the bucket-like labellum full of water beats everything : I *suspect* that the bees being well wetted flattens their hairs, and allows the viscid disc to adhere."]

C. Darwin to the Marquis de Saporta.

Down, December 24, 1877.

MY DEAR SIR,—I thank you sincerely for your long and most interesting letter, which I should have answered sooner had it not been delayed in London. I had not heard before that I was to be proposed as a Corresponding Member of the Institute. Living so retired a life as I do, such honours

© The Complete Work of Charles Darwin Online

affect me very little, and I can say with entire truth that your kind expression of sympathy has given and will give me much more pleasure than the election itself, should I be elected.

Your idea that dicotyledonous plants were not developed in force until sucking insects had been evolved seems to me a splendid one. I am surprised that the idea never occurred to me, but this is always the case when one first hears a new and simple explanation of some mysterious phenomenon . . . . I formerly showed that we might fairly assume that the beauty of flowers, their sweet odour and copious nectar, may be attributed to the existence of flower-haunting insects, but your idea, which I hope you will publish, goes much further and is much more important. With respect to the great development of mammifers in the later Geological periods following from the development of dicotyledons, I think it ought to be proved that such animals as deer, cows, horses, &c. could not flourish if fed exclusively on the gramineæ and other anemophilous monocotyledons; and I do not suppose that any evidence on this head exists.

Your suggestion of studying the manner of fertilisation of the surviving members of the most ancient forms of the dicotyledons is a very good one, and I hope that you will keep it in mind yourself, for I have turned my attention to other subjects. Delpino I think says that Magnolia is fertilised by insects which gnaw the petals, and I should not be surprised if the same fact holds good with Nymphæa. Whenever I have looked at the flowers of these latter plants I have felt inclined to admit the view that petals are modified stamens, and not modified leaves; though Poinsettia seems to show that true leaves might be converted into coloured petals. I grieve to say that I have never been properly grounded in Botany and have studied only special points—therefore I cannot pretend to express any opinion on your remarks on the origin of the flowers of the Coniferæ, Gneta-

© The Complete Work of Charles Darwin Online

ceæ, &c; but I have been delighted with what you say on the conversion of a monœcious species into a hermaphrodite one by the condensations of the verticils on a branch bearing female flowers near the summit, and male flowers below.

I expect Hooker to come here before long, and I will then show him your drawing, and if he makes any important remarks I will communicate with you. He is very busy at present in clearing off arrears after his American Expedition, so that I do not like to trouble him, even with the briefest note. I am at present working with my son at some Physiological subjects, and we are arriving at very curious results, but they are not as yet sufficiently certain to be worth communicating to you. . . .

[In 1877 a second edition of the 'Fertilisation of Orchids' was published, the first edition having been for some time out of print. The new edition was remodelled and almost rewritten, and a large amount of new matter added, much of which the author owed to his friend Fritz Müller.

With regard to this edition he wrote to Dr. Gray:—

"I do not suppose I shall ever again touch the book. After much doubt I have resolved to act in this way with all my books for the future; that is to correct them once and never touch them again, so as to use the small quantity of work left in me for new matter."

He may have felt a diminution of his power of reviewing large bodies of facts, such as would be needed in the preparation of new editions, but his powers of observation were certainly not diminished. He wrote to Mr. Dyer on July 14, 1878:—]

MY DEAR DYER,—*Thalia dealbata* was sent me from Kew: it has flowered and after looking casually at the flowers, they have driven me almost mad, and I have worked at them for a week: it is as grand a case as that of Catasetum.

© The Complete Work of Charles Darwin Online

Pistil vigorously motile (so that whole flower shakes when pistil suddenly coils up); when excited by a touch the two filaments [are] produced laterally and transversely across the flower (just over the nectar) from one of the petals or modified stamens. It is splendid to watch the phenomenon under a weak power when a bristle is inserted into a *young* flower which no insect has visited. As far as I know Stylidium is the sole case of sensitive pistil and here it is the pistil + stamens. In Thalia * cross-fertilisation is ensured by the wonderful movement, if bees visit several flowers.

I have now relieved my mind and will tell the purport of this note—viz. if any other species of Thalia besides *T. dealbata* should flower with you, for the love of heaven and all the saints, send me a few in *tin box with damp moss*.

Your insane friend,
CH. DARWIN.

[In 1878 Dr. Ogle's translation of Kerner's interesting book, 'Flowers and their Unbidden Guests,' was published. My father, who felt much interest in the translation (as appears in the following letter), contributed some prefatory words of approval:]

C. Darwin to W. Ogle.

Down, December 16 [1878].

. . . . I have now read Kerner's book, which is better even than I anticipated. The translation seems to me as clear as daylight, and written in forcible and good familiar English. I am rather afraid that it is too good for the English public, which seems to like very washy food, unless it be administered by some one whose name is well known, and then I suspect a good deal of the unintelligible is very pleasing to them. I hope to heaven that I may be wrong.

* Hildebrand has described an explosive arrangement in some of the Maranteæ—the tribe to which Thalia belongs.

© The Complete Work of Charles Darwin Online

Anyhow, you and Mrs. Ogle have done a right good service for Botanical Science.

Yours very sincerely,
CH. DARWIN.

P.S.—You have done me much honour in your prefatory remarks.

[One of the latest references to his Orchid-work occurs in a letter to Mr. Bentham, February 16, 1880. It shows the amount of pleasure which this subject gave to my father, and (what is characteristic of him) that his reminiscence of the work was one of delight in the observations which preceded its publication, not to the applause which followed it:—

"They are wonderful creatures, these Orchids, and I sometimes think with a glow of pleasure, when I remember making out some little point in their method of fertilisation."]

© The Complete Work of Charles Darwin Online

CHAPTER VIII.

THE 'EFFECTS OF CROSS- AND SELF-FERTILISATION IN THE VEGETABLE KINGDOM.' 1876.

[THIS book, as pointed out in the 'Autobiography,' is a complement to the 'Fertilisation of Orchids,' because it shows how important are the results of cross-fertilisation which are ensured by the mechanisms described in that book. By proving that the offspring of cross-fertilisation are more vigorous than the offspring of self-fertilisation, he showed that one circumstance which influences the fate of young plants in the struggle for life is the degree to which their parents are fitted for cross-fertilisation. He thus convinced himself that the intensity of the struggle (which he had elsewhere shown to exist among young plants) is a measure of the strength of a selective agency perpetually sifting out every modification in the structure of flowers which can affect its capabilities for cross-fertilisation.

The book is also valuable in another respect, because it throws light on the difficult problems of the origin of sexuality. The increased vigour resulting from cross-fertilisation is allied in the closest manner to the advantage gained by change of conditions. So strongly is this the case, that in some instances cross-fertilisation gives no advantage to the offspring, unless the parents have lived under slightly different conditions. So that the really important thing is not that two individuals of different *blood* shall unite, but two individuals

© The Complete Work of Charles Darwin Online

which have been subjected to different conditions. We are thus led to believe that sexuality is a means for infusing vigour into the offspring by the coalescence of differentiated elements, an advantage which could not follow if reproductions were entirely asexual.

It is remarkable that this book, the result of eleven years of experimental work, owed its origin to a chance observation. My father had raised two beds of *Linaria vulgaris*—one set being the offspring of cross- and the other of self-fertilisation. These plants were grown for the sake of some observations on inheritance, and not with any view to cross-breeding, and he was astonished to observe that the offspring of self-fertilisation were clearly less vigorous than the others. It seemed incredible to him that this result could be due to a single act of self-fertilisation, and it was only in the following year, when precisely the same result occurred in the case of a similar experiment on inheritance in Carnations, that his attention was "thoroughly aroused," and that he determined to make a series of experiments specially directed to the question. The following letters give some account of the work in question :]

C. Darwin to Asa Gray.

September 10, [1866?]

. . . . I have just begun a large course of experiments on the germination of the seed, and on the growth of the young plants when raised from a pistil fertilised by pollen from the same flower, and from pollen from a distinct plant of the same, or of some other variety. I have not made sufficient experiments to judge certainly, but in some cases the difference in the growth of the young plants is highly remarkable. I have taken every kind of precaution in getting seed from the same plant, in germinating the seed on my own chimney-piece, in planting the seedlings in the same flower-pot, and under this similar treatment I have seen the young seedlings

© The Complete Work of Charles Darwin Online

from the crossed seed exactly twice as tall as the seedlings from the self-fertilised seed; both seeds having germinated on same day. If I can establish this fact (but perhaps it will all go to the dogs), in some fifty cases, with plants of different orders, I think it will be very important, for then we shall positively know why the structure of every flower permits, or favours, or necessitates an occasional cross with a distinct individual. But all this is rather cooking my hare before I have caught it. But somehow it is a great pleasure to me to tell you what I am about.

Believe me, my dear Gray,
Ever yours most truly, and with cordial thanks,
CH. DARWIN.

C. Darwin to G. Bentham.

April 22, 1868.

. . . . I am experimenting on a very large scale on the difference in power of growth between plants raised from self-fertilised and crossed seeds; and it is no exaggeration to say that the difference in growth and vigour is sometimes truly wonderful. Lyell, Huxley and Hooker have seen some of my plants, and been astonished; and I should much like to show them to you. I always supposed until lately that no evil effects would be visible until after several generations of self-fertilisation; but now I see that one generation sometimes suffices; and the existence of dimorphic plants and all the wonderful contrivances of orchids are quite intelligible to me.

With cordial thanks for your letter, which has pleased me greatly,

Yours very sincerely,
CHARLES DARWIN.

[An extract from a letter to Dr. Gray (March 11, 1873) mentions the progress of the work:—

U 2

© The Complete Work of Charles Darwin Online

"I worked last summer hard at Drosera, but could not finish till I got fresh plants, and consequently took up the effects of crossing and self-fertilising plants, and am got so interested that Drosera must go to the dogs till I finish with this, and get it published; but then I will resume my beloved Drosera, and I heartily apologise for having sent the precious little things even for a moment to the dogs."

The following letters give the author's impression of his own book.]

C. Darwin to J. Murray.

Down, September 16, 1876.

MY DEAR SIR,—I have just received proofs in sheet of five sheets, so you will have to decide soon how many copies will have to be struck off. I do not know what to advise. The greater part of the book is extremely dry, and the whole on a special subject. Nevertheless, I am convinced that the book is of value, and I am convinced that for *many* years copies will be occasionally sold. Judging from the sale of my former books, and from supposing that some persons will purchase it to complete the set of my works, I would suggest 1500. But you must be guided by your larger experience. I will only repeat that I am convinced the book is of some permanent value. . . .

C. Darwin to Victor Carus.

Down, September 27, 1876.

MY DEAR SIR,—I sent by this morning's post the four first perfect sheets of my new book, the title of which you will see on the first page, and which will be published early in November.

I am sorry to say that it is only shorter by a few pages than my 'Insectivorous Plants.' The whole is now in type, though I have corrected finally only half the volume. You will, therefore, rapidly receive the remainder. The book is

© The Complete Work of Charles Darwin Online

very dull. Chapters II. to VI., inclusive, are simply a record of experiments. Nevertheless, I believe (though a man can never judge his own books) that the book is valuable. You will have to decide whether it is worth translating. I hope so. It has cost me very great labour, and the results seem to me remarkable and well established.

If you translate it, you could easily get aid for Chapters II. to VI., as there is here endless, but, I have thought, necessary repetition. I shall be anxious to hear what you decide. . . . . .

I most sincerely hope that your health has been fairly good this summer.

My dear Sir, yours very truly,

CH. DARWIN.

C. Darwin to Asa Gray.

Down, October 28, 1876.

MY DEAR GRAY,—I send by this post all the clean sheets as yet printed, and I hope to send the remainder within a fortnight. Please observe that the first six chapters are not readable, and the six last very dull. Still I believe that the results are valuable. If you review the book, I shall be very curious to see what you think of it, for I care more for your judgment than for that of almost any one else. I know also that you will speak the truth, whether you approve or disapprove. Very few will take the trouble to read the book, and I do not expect you to read the whole, but I hope you will read the latter chapters.

. . . I am so sick of correcting the press and licking my horrid bad style into intelligible English.

[The 'Effects of Cross and Self-Fertilisation' was published on November 10, 1876, and 1500 copies were sold before the end of the year. The following letter refers to a review in 'Nature:' *]

* February 15, 1877.

© The Complete Work of Charles Darwin Online

C. Darwin to W. Thiselton Dyer.

Down, February 16, 1877.

DEAR DYER,—I must tell you how greatly I am pleased and honoured by your article in 'Nature,' which I have just read. You are an adept in saying what will please an author, not that I suppose you wrote with this express intention. I should be very well contented to deserve a fraction of your praise. I have also been much interested, and this is better than mere pleasure, by your argument about the separation of the sexes. I dare say that I am wrong, and will hereafter consider what you say more carefully: but at present I cannot drive out of my head that the sexes must have originated from two individuals, slightly different, which conjugated. But I am aware that some cases of conjugation are opposed to any such views.

With hearty thanks,

Yours sincerely,

CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

CHAPTER IX.

'DIFFERENT FORMS OF FLOWERS ON PLANTS OF THE SAME SPECIES.' 1877.

[THE volume bearing the above title was published in 1877, and was dedicated by the author to Professor Asa Gray, "as a small tribute of respect and affection." It consists of certain earlier papers re-edited, with the addition of a quantity of new matter. The subjects treated in the book are:—

(i.) Heterostyled Plants.

(ii.) Polygamous, Diœcious, and Gynodiœcious Plants.

(iii.) Cleistogamic Flowers.

The nature of heterostyled plants may be illustrated in the primrose, one of the best known examples of the class. If a number of primroses be gathered, it will be found that some plants yield nothing but "pin-eyed" flowers, in which the style (or organ for the transmission of the pollen to the ovule) is long, while the others yield only "thrum-eyed" flowers with short styles. Thus primroses are divided into two sets or castes differing structurally from each other. My father showed that they also differ sexually, and that in fact the bond between the two castes more nearly resembles that between separate sexes than any other known relationship. Thus for example a long-styled primrose, though it can be fertilised by its own pollen, is not *fully* fertile unless it is impregnated by the pollen of a short-styled flower. Heterostyled plants are comparable to hermaphrodite animals, such as snails, which require the concourse of two individuals, although each pos-

© The Complete Work of Charles Darwin Online

sesses both the sexual elements. The difference is that in the case of the primrose it is *perfect fertility*, and not simply *fertility*, that depends on the mutual action of the two sets of individuals.

The work on heterostyled plants has a special bearing, to which the author attached much importance, on the problem of origin of species.*

He found that a wonderfully close parallelism exists between hybridisation and certain forms of fertilisation among heterostyled plants. So that it is hardly an exaggeration to say that the "illegitimately" reared seedlings are hybrids, although both their parents belong to identically the same species. In a letter to Professor Huxley, given in the second volume (p. 384), my father writes as if his researches on heterostyled plants tended to make him believe that sterility is a selected or acquired quality. But in his later publications, *e.g.* in the sixth edition of the 'Origin,' he adheres to the belief that sterility is an incidental rather than a selected quality. The result of his work on heterostyled plants is of importance as showing that sterility is no test of specific distinctness, and that it depends on differentiation of the sexual elements which is independent of any racial difference. I imagine that it was his instinctive love of making out a difficulty which to a great extent kept him at work so patiently on the heterostyled plants. But it was the fact that general conclusions of the above character could be drawn from his results which made him think his results worthy of publication.†

The papers which on this subject preceded and contributed to 'Forms of Flowers' were the following :—

"On the two Forms or Dimorphic Condition in the Species of Primula, and on their remarkable Sexual Relations." Linn. Soc. Journal, 1862.

* See 'Autobiography,' vol. i. p. 97.

† See 'Forms of Flowers,' p. 243.

© The Complete Work of Charles Darwin Online

"On the Existence of Two Forms, and on their Reciprocal Sexual Relations, in several Species of the Genus Linum." Linn. Soc. Journal, 1863.

"On the Sexual Relations of the Three Forms of *Lythrum salicaria*," Ibid. 1864.

"On the Character and Hybrid-like Nature of the Offspring from the Illegitimate Unions of Dimorphic and Trimorphic Plants." Ibid. 1869.

On the Specific Differences between *Primula veris*, Brit. Fl. (var *officinalis*, Linn.), *P. vulgaris*, Brit. Fl. (var. *acaulis*, Linn.), and *P. elatior*, Jacq.; and on the Hybrid Nature of the Common Oxlip. With Supplementary Remarks on Naturally Produced Hybrids in the Genus Verbascum." Ibid. 1869.

The following letter shows that he began the work on heterostyled plants with an erroneous view as to the meaning of the facts.]

C. Darwin to J. D. Hooker.

Down, May 7 [1860].

. . . . I have this morning been looking at my experimental cowslips, and I find some plants have all flowers with long stamens and short pistils, which I will call "male plants," others with short stamens and long pistils, which I will call "female plants." This I have somewhere seen noticed, I think by Henslow; but I find (after looking at my two sets of [plants) that the stigmas of the male and female are of slightly different shape, and certainly different degree of roughness, and what has astonished me, the pollen of the so-called female plant, though very abundant, is more transparent, and each granule is exactly only $\frac{2}{3}$ of the size of the pollen of the so-called male plants. Has this been observed? I cannot help suspecting [that] the cowslip is in fact diœcious, but it may turn out all a blunder, but anyhow I will mark with sticks the so-called male and female plants and watch their

© The Complete Work of Charles Darwin Online

seeding. It would be a fine case of gradation between an hermaphrodite and unisexual condition. Likewise a sort of case of balancement of long and short pistils and stamens. Likewise perhaps throws light on oxlips. . . .

I have now examined primroses and find exactly the same difference in the size of the pollen, correlated with the same difference in the length of the style and roughness of the stigmas.

C. Darwin to Asa Gray.

June 8 [1860].

. . . . I have been making some little trifling observations which have interested and perplexed me much. I find with primroses and cowslips, that about an equal number of plants are thus characterised.

So-called (by me) *male* plant. Pistil much shorter than stamens; stigma rather smooth,—*pollen grains large*, throat of corolla short.

So-called female plant. Pistil much longer than stamens, stigma rougher, *pollen-grains smaller*,—throat of corolla long.

I have marked a lot of plants, and expected to find the so-called male plant barren; but judging from the feel of the capsules, this is not the case, and I am very much surprised at the difference in the size of the pollen. . . . If it should prove that the so-called male plants produce less seed than the so-called females, what a beautiful case of gradation from hermaphrodite to unisexual condition it will be! If they produce about equal number of seed, how perplexing it will be.

C. Darwin to J. D. Hooker.

Down, December 17, [1860?].

. . . . I have just been ordering a photograph of myself for a friend; and have ordered one for you, and for heaven's sake oblige me, and burn that now hanging up in your room.—It makes me look atrociously wicked.

© The Complete Work of Charles Darwin Online

. . . . In the spring I must get you to look for long pistils and short pistils in the rarer species of Primula and in some allied Genera. It holds with *P. Sinensis.* You remember all the fuss I made on this subject last spring; well, the other day at last I had time to weigh the seeds, and by Jove the plants of primrose and cowslip with short pistils and large grained pollen * are rather more fertile than those with long pistils, and small-grained pollen. I find that they require the action of insects to set them, and I never will believe that these differences are without some meaning.

Some of my experiments lead me to suspect that the large-grained pollen suits the long pistils and the small-grained pollen suits the short pistils; but I am determined to see if I cannot make out the mystery next spring.

How does your book on plants brew in your mind? Have you begun it? . . .

Remember me most kindly to Oliver. He must be astonished at not having a string of questions, I fear he will get out of practice!

[The Primula-work was finished in the autumn of 1861, and on Nov. 8th he wrote to Sir J. D. Hooker:—

"I have sent my paper on dimorphism in Primula to the Linn. Soc. I shall go up and read it whenever it comes on; I hope you may be able to attend, for I do not suppose many will care a penny for the subject."

With regard to the reading of the paper (on Nov. 21st), he wrote to the same friend:—

"I by no means thought that I produced a "tremendous effect" in the Linn. Soc., but by Jove the Linn. Soc., produced a tremendous effect on me, for I could not get out of bed till late next evening, so that I just crawled home. I fear I must give up trying to read any paper or speak; it is a horrid bore, I can do nothing like other people.

* Thus the plants which he imagined to be tending towards a male condition were more productive than the supposed females.

© The Complete Work of Charles Darwin Online

To Dr. Gray he wrote, (Dec. 1861):—

"You may rely on it, I will send you a copy of my Primula paper as soon as I can get one; but I believe it will not be printed till April 1st, and therefore after my Orchid Book. I care more for your and Hooker's opinion than for that of all the rest of the world, and for Lyell's on geological points. Bentham and Hooker thought well of my paper when read; but no one can judge of evidence by merely hearing a paper."

The work on Primula was the means of bringing my father in contact with the late Mr. John Scott, then working as a gardener in the Botanic Gardens at Edinburgh,—an employment which he seems to have chosen in order to gratify his passion for natural history. He wrote one or two excellent botanical papers, and ultimately obtained a post in India.* He died in 1880.

A few phrases may be quoted from letters to Sir J. D. Hooker, showing my father's estimate of Scott:—

"If you know, do please tell me who is John Scott of the Botanical Gardens of Edinburgh; I have been corresponding largely with him; he is no common man."

"If he had leisure he would make a wonderful observer; to my judgment I have come across no one like him."

"He has interested me strangely, and I have formed a very high opinion of his intellect. I hope he will accept pecuniary assistance from me; but he has hitherto refused." (He ultimately succeeded in being allowed to pay for Mr. Scott's passage to India.)

"I know nothing of him excepting from his letters; these show remarkable talent, astonishing perseverance, much modesty, and what I admire, determined difference from me on many points."

So highly did he estimate Scott's abilities that he formed

* While in India he made some admirable observations on expression for my father.

© The Complete Work of Charles Darwin Online

a plan (which however never went beyond an early stage of discussion) of employing him to work out certain problems connected with intercrossing.

The following letter refers to my father's investigations on Lythrum,* a plant which reveals even a more wonderful condition of sexual complexity than that of Primula. For in Lythrum there are not merely two, but three castes, differing structurally and physiologically from each other :]

C. Darwin to Asa Gray.

Down, August 9 [1862].

MY DEAR GRAY,—It is late at night, and I am going to write briefly, and of course to beg a favour.

The Mitchella very good, but pollen apparently equal-sized. I have just examined Hottonia, grand difference in pollen. *Echium vulgare*, a humbug, merely a case like Thymus. But I am almost stark staring mad over Lythrum ;† if I can prove what I fully believe ; it is a grand case of TRIMORPHISM, with three different pollens and three stigmas ; I have castrated and fertilised above ninety flowers, trying all the eighteen distinct crosses which are possible within the limits of this one species! I cannot explain, but I feel sure you would think it a grand case. I have been writing to Botanists to see if I can possibly get *L. hyssopifolia*, and it has just flashed on me that you might have Lythrum in North America, and I have looked to your Manual. For the love

* He was led to this, his first case of trimorphism, by Lecoq's 'Géographie Botanique,' and this must have consoled him for the trick this work played him in turning out to be so much larger than he expected. He wrote to Sir J. D. Hooker : "Here is a good joke : I saw an extract from Lecoq, 'Géograph. Bot.,' and ordered it and hoped that it was a good sized pamphlet, and nine thick volumes have arrived !"

† On another occasion he wrote (to Dr. Gray) with regard to Lythrum : "I must hold hard, otherwise I shall spend my life over dimorphism."

© The Complete Work of Charles Darwin Online

of heaven have a look at some of your species, and if you can get me seed, do; I want much to try species with few stamens, if they are dimorphic; *Nesæa verticillata* I should expect to be trimorphic. Seed! Seed! Seed! I should rather like seed of Mitchella. But oh, Lythrum!

Your utterly mad friend,

C. DARWIN.

P.S.—There is reason in my madness, for I can see that to those who already believe in change of species, these facts will modify to a certain extent the whole view of Hybridity.*

[On the same subject he wrote to Sir Joseph Hooker in August 1862:—

"Is Oliver at Kew? When I am established at Bournemouth I am completely mad to examine any fresh flowers of any Lythraceous plant, and I would write and ask him if any are in bloom."

Again he wrote to the same friend in October:—

"If you ask Oliver, I think he will tell you I have got a real odd case in Lythrum, it interests me extremely, and seems to me the strangest case of propagation recorded amongst plants or animals, viz. a necessary triple alliance between three hermaphrodites. I feel sure I can now prove the truth of the case from a multitude of crosses made this summer."

* A letter to Dr. Gray (July, 1862) bears on this point: "A few days ago I made an observation which has surprised me more than it ought to do—it will have to be repeated several times, but I have scarcely a doubt of its accuracy. I stated in my Primula paper that the long-styled form of *Linum grandiflorum* was utterly sterile with its own pollen; I have lately been putting the pollen of the two forms on the division of the stigma of the *same* flower; and it strikes me as truly wonderful, that the stigma distinguishes the pollen; and is penetrated by the tubes of the one and not by those of the other; nor are the tubes exserted. Or (which is the same thing) the stigma of the one form acts on and is acted on by pollen, which produces not the least effect on the stigma of the other form. Taking sexual power as the criterion of difference, the two forms of this one species may be said to be generically distinct."

© The Complete Work of Charles Darwin Online

In an article, 'Dimorphism in the Genitalia of Plants' ('Silliman's Journal,' 1862, vol. xxxiv. p. 419), Dr. Gray points out that the structural difference between the two forms of Primula had already been defined in the 'Flora of N. America,' as *diœcio-dimorphism*. The use of this term called forth the following remarks from my father. The letter also alludes to a review of the 'Fertilisation of Orchids' in the same volume of 'Silliman's Journal.']

C. Darwin to Asa Gray.

Down, November 26 [1862].

MY DEAR GRAY,—The very day after my last letter, yours of November 10th, and the review in 'Silliman,' which I feared might have been lost, reached me. We were all very much interested by the political part of your letter; and in some odd way one never feels that information and opinions printed in a newspaper come from a living source; they seem dead, whereas all that you write is full of life. The reviews interested me profoundly; you rashly ask for my opinion, and you must consequently endure a long letter. First for Dimorphism; I do not *at present* like the term "Diœcio-dimorphism;" for I think it gives quite a false notion, that the phenomena are connected with a separation of the sexes. Certainly in Primula there is unequal fertility in the two forms, and I suspect this is the case with Linum; and, therefore, I felt bound in the Primula paper to state that it might be a step towards a diœcious condition; though I believe there are no diœcious forms in Primulaceæ or Linaceæ. But the three forms in Lythrum convince me that the phenomenon is in no way necessarily connected with any tendency to separation of sexes. The case seems to me in result or function to be almost identical with what old C. K. Sprengel called "dichogamy," and which is so frequent in truly hermaphrodite groups; namely, the pollen and stigma

© The Complete Work of Charles Darwin Online

of each flower being mature at different periods. If I am right, it is very advisable not to use the term "diœcious," as this at once brings notions of separation of sexes.

. . . I was much perplexed by Oliver's remarks in the 'Natural History Review' on the Primula case, on the lower plants having sexes more often of the separated than in the higher plants,—so exactly the reverse of what takes place in animals. Hooker in his review of the 'Orchids' repeats this remark. There seems to be much truth in what you say,* and it did not occur to me, about no improbability of specialisation in *certain* lines in lowly organised beings. I could hardly doubt that the hermaphrodite state is the aboriginal one. But how is it in the conjugation of Confervæ—is not one of the two individuals here in fact male, and the other female? I have been much puzzled by this contrast in sexual arrangements between plants and animals. Can there be anything in the following consideration: By *roughest* calculation about one-third of the British *genera* of aquatic plants belong to the Linnean classes of Mono and Diœcia; whilst of terrestrial plants (the aquatic genera being subtracted) only one-thirteenth of the genera belong to these two classes. Is there any truth in this fact generally? Can aquatic plants, being confined to a small area or small community of individuals, require more free crossing, and therefore have separate sexes? But to return to one point, does not Alph. de Candolle say that aquatic plants taken as a whole are lowly organised, compared with terrestrial; and may not Oliver's remark on the separation of the sexes in lowly organised plants stand in some relation to their being frequently aquatic? Or is this all rubbish?

. . . . What a magnificent compliment you end your review with! You and Hooker seem determined to turn my head

* "Forms which are low in the scale as respects morphological completeness may be high in the scale of rank founded on specialisation of structure and function."—Dr. Gray, in 'Silliman's Journal.'

© The Complete Work of Charles Darwin Online

with conceit and vanity (if not already turned), and make me an unbearable wretch.

With most cordial thanks, my good and kind friend,

Farewell,

C. DARWIN.

[The following passage from a letter (July 28, 1863), to Prof. Hildebrand, contains a reference to the reception of the dimorphic work in France:—

"I am extremely much pleased to hear that you have been looking at the manner of fertilisation of your native Orchids, and still more pleased to hear that you have been experimenting on Linum. I much hope that you may publish the result of these experiments; because I was told that the most eminent French botanists of Paris said that my paper on Primula was the work of imagination, and that the case was so improbable they did not believe in my results."]

C. Darwin to Asa Gray.

April 19 [1864].

.... I received a little time ago a paper with a good account of your Herbarium and Library, and a long time previously your excellent review of Scott's 'Primulaceæ,' and I forwarded it to him in India, as it would much please him. I was very glad to see in it a new case of Dimorphism (I forget just now the name of the plant); I shall be grateful to hear of any other cases, as I still feel an interest in the subject. I should be very glad to get some seed of your dimorphic Plantagos; for I cannot banish the suspicion that they must belong to a very different class like that of the common Thyme.* How could the wind, which is the agent of fertilisation, with Plantago, fertilise "reciprocally dimorphic" flowers like Primula? Theory says this cannot be, and in such cases

* In this prediction he was right. See 'Forms of Flowers,' p. 307.

© The Complete Work of Charles Darwin Online

of one's own theories I follow Agassiz and declare, "that nature never lies." I should even be very glad to examine the two dried forms of Plantago. Indeed, any dried dimorphic plants would be gratefully received. . . .

Did my Lythrum paper interest you? I crawl on at the rate of two hours per diem, with 'Variation under Domestication.'

C. Darwin to J. D. Hooker.

Down, November 26 [1864].

. . . . You do not know how pleased I am that you have read my Lythrum paper; I thought you would not have time, and I have for long years looked at you as my Public, and care more for your opinion than that of all the rest of the world. I have done nothing which has interested me so much as Lythrum, since making out the complemental males of Cirripedes. I fear that I have dragged in too much miscellaneous matter into the paper.

. . . I get letters occasionally, which show me that Natural Selection is making *great* progress in Germany, and some amongst the young in France. I have just received a pamphlet from Germany, with the complimentary title of "Darwinische Arten-Enstehung-Humbug"!

Farewell, my best of old friends,

C. DARWIN.

C. Darwin to Asa Gray.

September 10, [1867?]

. . . . The only point which I have made out this summer, which could possibly interest you, is that the common Oxlip found everywhere, more or less commonly in England, is certainly a hybrid between the primrose and cowslip; whilst the *P. elatior* (Jacq.), found only in the Eastern Counties, is a perfectly distinct and good species; hardly distinguishable

© The Complete Work of Charles Darwin Online

from the common oxlip, except by the length of the seed-capsule relatively to the calyx. This seems to me rather a horrid fact for all systematic botanists. . . . .

C. Darwin to F. Hildebrand.

Down, November 16, 1868.

MY DEAR SIR,—I wrote my last note in such a hurry from London, that I quite forgot what I chiefly wished to say, namely to thank you for your excellent notices in the 'Bot. Zeitung' of my paper on the offspring of dimorphic plants. The subject is so obscure that I did not expect that any one would have noticed my paper, and I am accordingly very much pleased that you should have brought the subject before the many excellent naturalists of Germany.

Of all the German authors (but they are not many) whose works I have read, you write by far the clearest style, but whether this is a compliment to a German writer I do not know.

[The two following letters refer to the small bud-like "Cleistogamic" flowers found in the violet and many other plants. They do not open and are necessarily self-fertilised:]

C. Darwin to J. D. Hooker.

Down, May 30 [1862].

. . . . What will become of my book on Variation? I am involved in a multiplicity of experiments. I have been amusing myself by looking at the small flowers of Viola. If Oliver * has had time to study them, he will have seen the curious case (as it seems to me) which I have just made clearly out, viz. that in these flowers, the *few* pollen grains are

* Shortly afterwards he wrote: "Oliver, the omniscient, has sent me a paper in the 'Bot. Zeitung,' with most accurate description of all that I saw in Viola."

© The Complete Work of Charles Darwin Online

never shed, or never leave the anther-cells, but emit long pollen tubes, which penetrate the stigma. To-day I got the anther with the included pollen grain (now empty) at one end, and a bundle of tubes penetrating the stigmatic tissue at the other end; I got the whole under a microscope without breaking the tubes; I wonder whether the stigma pours some fluid into the anther so as to excite the included grains. It is a rather odd case of correlation, that in the double sweet violet the little flowers are double; *i.e.*, have a multitude of minute scales representing the petals. What queer little flowers they are.

Have you had time to read poor dear Henslow's life? it has interested me for the man's sake, and, what I did not think possible, has even exalted his character in my estimation. . . . .

[The following is an extract from the letter given in part at p. 303, and refers to Dr. Gray's article on the sexual differences of plants:]

C. Darwin to Asa Gray.

November 26 [1862].

. . . . You will think that I am in the most unpleasant, contradictory, fractious humour, when I tell you that I do not like your term of "precocious fertilisation" for your second class of dimorphism [*i.e.* for cleistogamic fertilisation]. If I can trust my memory, the state of the corolla, of the stigma, and the pollen-grains is different from the state of the parts in the bud; that they are in a condition of special modification. But upon my life I am ashamed of myself to differ so much from my betters on this head. The *temporary* theory* which I have formed on this class of dimorphism, just to guide experiment, is that the *perfect* flowers can only be perfectly

* This view is now generally accepted.

© The Complete Work of Charles Darwin Online

fertilised by insects, and are in this case abundantly crossed; but that the flowers are not always, especially in early spring, visited enough by insects, and therefore the little imperfect self-fertilising flowers are developed to ensure a sufficiency of seed for present generations. *Viola canina* is sterile, when not visited by insects, but when so visited forms plenty of seed. I infer from the structure of three or four forms of *Balsamineæ*, that these require insects; at least there is almost as plain adaptation to insects as in Orchids. I have *Oxalis acetosella* ready in pots for experiment next spring; and I fear this will upset my little theory. . . . *Campanula carpathica*, as I found this summer, is absolutely sterile if insects are excluded. *Specularia speculum* is fairly fertile when enclosed; and this seemed to me to be partially effected by the frequent closing of the flower; the inward angular folds of the corolla corresponding with the clefts of the open stigma, and in this action pushing pollen from the outside of the stigma on to its surface. Now can you tell me, does *S. perfoliata* close its flower like *S. speculum*, with angular inward folds? if so, I am smashed without some fearful "wriggling." Are the *imperfect* flowers of your Specularia the early or the later ones? very early or very late? It is rather pretty to see the importance of the closing of flowers of *S. speculum*.

['Forms of Flowers' was published in July 1877; in June he wrote to Professor Carus with regard to the translation:—

"My new book is not a long one, viz. 350 pages, chiefly of the larger type, with fifteen simple woodcuts. All the proofs are corrected except the Index, so that it will soon be published.

". . . . I do not suppose that I shall publish any more books, though perhaps a few more papers. I cannot endure being idle, but heaven knows whether I am capable of any more good work."

© The Complete Work of Charles Darwin Online

The review alluded to in the next letter is at p. 445 of the volume of 'Nature' for 1878:]

C. Darwin to W. Thiselton Dyer.

Down, April 5, 1878.

MY DEAR DYER,—I have just read in 'Nature' the review of 'Forms of Flowers,' and I am sure that it is by you. I wish with all my heart that it deserved one quarter of the praises which you give it. Some of your remarks have interested me greatly. . . . Hearty thanks for your generous and most kind sympathy, which does a man real good, when he is as dog-tired as I am at this minute with working all day, so good-bye.

C. DARWIN.

© The Complete Work of Charles Darwin Online

CHAPTER X.

CLIMBING AND INSECTIVOROUS PLANTS.

[MY father mentions in his 'Autobiography' (vol. i. p. 92) that he was led to take up the subject of climbing plants by reading Dr. Gray's paper, "Note on the Coiling of the Tendrils of Plants." * This essay seems to have been read in 1862, but I am only able to guess at the date of the letter in which he asks for a reference to it, so that the precise date of his beginning this work cannot be determined.

In June 1863 he was certainly at work, and wrote to Sir J. D. Hooker for information as to previous publications on the subject, being then in ignorance of Palm's and H. v. Mohl's works on climbing plants, both of which were published in 1827.]

C. Darwin to J. D. Hooker.

Down [June] 25 [1863].

MY DEAR HOOKER,—I have been observing pretty carefully a little fact which has surprised me; and I want to know from you and Oliver whether it seems new or odd to you, so just tell me whenever you write; it is a very trifling fact, so do not answer on purpose.

I have got a plant of *Echinocystis lobata* to observe the irritability of the tendrils described by Asa Gray, and which of course, is plain enough. Having the plant in my study, I have been surprised to find that the uppermost part of each

* 'Proc. Amer. Acad. of Arts and Sciences,' 1858.

© The Complete Work of Charles Darwin Online

branch (*i.e.* the stem between the two uppermost leaves excluding the growing tip) is *constantly* and slowly twisting round making a circle in from one and a half to two hours; it will sometimes go round two or three times, and then at the same rate untwists and twists in opposite directions. It generally rests half an hour before it retrogrades. The stem does not become permanently twisted. The stem beneath the twisting portion does not move in the least, though not tied. The movement goes on all day and all early night. It has no relation to light, for the plant stands in my window and twists from the light just as quickly as towards it. This may be a common phenomenon for what I know, but it confounded me quite, when I began to observe the irritability of the tendrils. I do not say it is the final cause, but the result is pretty, for the plant every one and a half or two hours sweeps a circle (according to the length of the bending shoot and the length of the tendril) of from one foot to twenty inches in diameter, and immediately that the tendril touches any object its sensitiveness causes it immediately to seize it; a clever gardener, my neighbour, who saw the plant on my table last night, said: "I believe, Sir, the tendrils can see, for wherever I put a plant it finds out any stick near enough." I believe the above is the explanation, viz. that it sweeps slowly round and round. The tendrils have some sense, for they do not grasp each other when young.

Yours affectionately,
C. DARWIN.

C. Darwin to J. D. Hooker.

Down, July 14 [1863].

MY DEAR HOOKER,—I am getting very much amused by my tendrils, it is just the sort of niggling work which suits me, and takes up no time and rather rests me whilst writing. So will you just think whether you know any plant, which

© The Complete Work of Charles Darwin Online

you could give or lend me, or I could buy, with tendrils, remarkable in any way for development, for odd or peculiar structure, or even for an odd place in natural arrangement. I have seen or can see Cucurbitaceæ, Passion-flower, Virginian-creeper, *Cissus discolor*, Common-pea and Everlasting-pea. It is really curious the diversification of irritability (I do not mean the spontaneous movement, about which I wrote before and correctly, as further observation shows); for instance, I find a slight pinch between the thumb and finger at the end of the tendril of the Cucurbitaceæ causes prompt movement, but a pinch excites no movement in Cissus. The cause is that one side alone (the concave) is irritable in the former; whereas both sides are irritable in Cissus, so if you excite at the same time both *opposite* sides there is no movement, but by touching with a pencil the two branches of the tendril, in any part whatever, you cause movement towards that point; so that I can mould, by a mere touch, the two branches into any shape I like. . . .

C. Darwin to Asa Gray.

Down, August 4 [1863].

My present hobby-horse I owe to you, viz. the tendrils: their irritability is beautiful, as beautiful in all its modifications as anything in Orchids. About the *spontaneous* movement (independent of touch) of the tendrils and upper internodes, I am rather taken aback by your saying, "is it not well known?" I can find nothing in any book which I have. . . . The spontaneous movement of the tendrils is independent of the movement of the upper internodes, but both work harmoniously together in sweeping a circle for the tendrils to grasp a stick. So with all climbing plants (without tendrils) as yet examined, the upper internodes go on night and day sweeping a circle in one fixed direction. It is surprising to watch the Apocyneæ with shoots 18 inches long (beyond the supporting stick), steadily searching for something to climb

© The Complete Work of Charles Darwin Online

up. When the shoot meets a stick, the motion at that point is arrested, but in the upper part is continued; so that the climbing of all plants yet examined is the simple result of the spontaneous circulatory movement of the upper internodes. Pray tell me whether anything has been published on this subject? I hate publishing what is old; but I shall hardly regret my work if it is old, as it has much amused me. . . .

C. Darwin to Asa Gray.

May 28, 1864.

. . . . An Irish nobleman on his death-bed declared that he could conscientiously say that he had never throughout life denied himself any pleasure; and I can conscientiously say that I have never scrupled to trouble you; so here goes.—Have you travelled South, and can you tell me whether the trees, which *Bignonia capreolata* climbs, are covered with moss or filamentous lichen or Tillandsia?* I ask because its tendrils abhor a simple stick, do not much relish rough bark, but delight in wool or moss. They adhere in a curious manner by making little disks, like the Ampelopsis. . . . By the way, I will enclose some specimens, and if you think it worth while, you can put them under the simple microscope. It is remarkable how specially adapted some tendrils are; those of *Eccremocarpus scaber* do not like a stick, will have nothing to say to wool; but give them a bundle of culms of grass, or a bundle of bristles and they seize them well.

C. Darwin to J. D. Hooker.

Down, June 10 [1864].

. . . I have now read two German books, and all I believe that has been written on climbers, and it has stirred me up to

* He subsequently learned from Dr. Gray that *Polypodium incanum* abounds on the trees in the districts where this species of Bignonia grows. See 'Climbing Plants,' p. 103.

© The Complete Work of Charles Darwin Online

find that I have a good deal of new matter. It is strange, but I really think no one has explained simple twining plants. These books have stirred me up, and made me wish for plants specified in them. I shall be very glad of those you mention. I have written to Veitch for young Nepenthes and Vanilla (which I believe will turn out a grand case, though a root creeper), and if I cannot buy young Vanilla I will ask you. I have ordered a leaf-climbing fern, Lygodium. All this work about climbers would hurt my conscience, did I think I could do harder work.*

[He continued his observations on climbing plants during the prolonged illness from which he suffered in the autumn of 1863, and in the following spring. He wrote to Sir J. D. Hooker, apparently in March 1864:—

"For several days I have been decidedly better, and what I lay much stress on (whatever doctors say), my brain feels far stronger, and I have lost many dreadful sensations. The hot-house is such an amusement to me, and my amusement I owe to you, as my delight is to look at the many odd leaves and plants from Kew. . . . The only approach to work which I can do is to look at tendrils and climbers, this does not distress my weakened brain. Ask Oliver to look over the enclosed queries (and do you look) and amuse a broken-down brother naturalist by answering any which he can. If you ever lounge through your houses, remember me and climbing plants."

On October 29, 1864, he wrote to Dr. Gray:—

"I have not been able to resist doing a little more at your godchild, my climbing paper, or rather in size little book, which by Jove I will have copied out, else I shall never stop. This has been new sort of work for me, and I have been pleased to find what a capital guide for observations a full conviction of the change of species is."

On Jan. 19, 1865, he wrote to Sir J. D. Hooker:—

* He was much out of health at this time.

© The Complete Work of Charles Darwin Online

"It is working hours, but I am trying to take a day's holiday, for I finished and despatched yesterday my climbing paper. For the last ten days I have done nothing but correct refractory sentences, and I loathe the whole subject."

A letter to Dr. Gray, April 9, 1865, has a word or two on the subject.—

"I have begun correcting proofs of my paper on 'Climbing Plants.' I suppose I shall be able to send you a copy in four or five weeks. I think it contains a good deal new and some curious points, but it is so fearfully long, that no one will ever read it. If, however, you do not *skim* through it, you will be an unnatural parent, for it is your child."

Dr. Gray not only read it but approved of it, to my father's great satisfaction, as the following extracts show:—

"I was much pleased to get your letter of July 24th. Now that I can do nothing, I maunder over old subjects, and your approbation of my climbing paper gives me *very* great satisfaction. I made my observations when I could do nothing else and much enjoyed it, but always doubted whether they were worth publishing. I demur to its not being necessary to explain in detail about the spires in *caught* tendrils running in opposite directions; for the fact for a long time confounded me, and I have found it difficult enough to explain the cause to two or three persons." (Aug. 15, 1865.)

"I received yesterday your article * on climbers, and it has pleased me in an extraordinary and even silly manner. You pay me a superb compliment, and as I have just said to my wife, I think my friends must perceive that I like praise, they give me such hearty doses. I always admire your skill in reviews or abstracts, and you have done this article excellently and given the whole essence of my paper. .... I have had a letter from a good Zoologist in S. Brazil, F. Müller, who has been stirred up to observe climbers and

* In the September number of 'Silliman's Journal,' concluded in the January number, 1866.

© The Complete Work of Charles Darwin Online

gives me some curious cases of *branch*-climbers, in which branches are converted into tendrils, and then continue to grow and throw out leaves and new branches, and then lose their tendril character." (October 1865.)

The paper on Climbing Plants was republished in 1875, as a separate book. The author had been unable to give his customary amount of care to the style of the original essay, owing to the fact that it was written during a period of continued ill-health, and it was now found to require a great deal of alteration. He wrote to Sir J. D. Hooker (March 3, 1875): "It is lucky for authors in general that they do not require such dreadful work in merely licking what they write into shape." And to Mr. Murray in September he wrote: "The corrections are heavy in 'Climbing Plants,' and yet I deliberately went over the MS. and old sheets three times." The book was published in September 1875, an edition of 1500 copies was struck off; the edition sold fairly well, and 500 additional copies were printed in June of the following year.]

INSECTIVOROUS PLANTS.

[In the summer of 1860 he was staying at the house of his sister-in-law, Miss Wedgwood, in Ashdown Forest, whence he wrote (July 29, 1860), to Sir Joseph Hooker :—

"Latterly I have done nothing here; but at first I amused myself with a few observations on the insect-catching power of Drosera; and I must consult you some time whether my 'twaddle' is worth communicating to the Linnean Society."

In August he wrote to the same friend :—

"I will gratefully send my notes on Drosera when copied by my copier: the subject amused me when I had nothing to do."

He has described in the 'Autobiography' (vol. i. p. 95), the general nature of these early experiments. He noticed insects sticking to the leaves, and finding that flies, &c., placed on

© The Complete Work of Charles Darwin Online

the adhesive glands were held fast and embraced, he suspected that the leaves were adapted to supply nitrogenous food to the plant. He therefore tried the effect on the leaves of various nitrogenous fluids—with results which, as far as they went, verified his surmise. In September, 1860, he wrote to Dr. Gray:—

"I have been infinitely amused by working at Drosera: the movements are really curious; and the manner in which the leaves detect certain nitrogenous compounds is marvellous. You will laugh; but it is, at present, my full belief (after endless experiments) that they detect (and move in consequence of) the $\frac{1}{2880}$ part of a single grain of nitrate of ammonia; but the muriate and sulphate of ammonia bother their chemical skill, and they cannot make anything of the nitrogen in these salts! I began this work on Drosera in relation to *gradation* as throwing light on Dionæa."

Later in the autumn he was again obliged to leave home for Eastbourne, where he continued his work on Drosera. The work was so new to him that he found himself in difficulties in the preparation of solutions, and became puzzled over fluid and solid ounces, &c. &c. To a friend, the late Mr. E. Cresy, who came to his help in the matter of weights and measures, he wrote giving an account of the experiments. The extract (November 2, 1860) which follows illustrates the almost superstitious precautions he often applied to his researches:—

"Generally I have scrutinised every gland and hair on the leaf before experimenting; but it occurred to me that I might in some way affect the leaf; though this is almost impossible, as I scrutinised with equal care those that I put into distilled water (the same water being used for dissolving the carbonate of ammonia). I then cut off four leaves (not touching them with my fingers), and put them in plain water, and four other leaves into the weak solution, and after leaving them for an hour and a half, I examined every hair on all eight leaves;

© The Complete Work of Charles Darwin Online

no change on the four in water; every gland and hair affected in those in ammonia.

"I had measured the quantity of weak solution, and I counted the glands which had absorbed the ammonia, and were plainly affected; the result convinced me that each gland could not have absorbed more than $\frac{1}{64000}$ or $\frac{1}{65000}$ of a grain. I have tried numbers of other experiments all pointing to the same result. Some experiments lead me to believe that very sensitive leaves are acted on by much smaller doses. Reflect how little ammonia a plant can get growing on poor soil—yet it is nourished. The really surprising part seems to me that the effect should be visible, and not under very high power; for after trying a high power, I thought it would be safer not to consider any effect which was not plainly visible under a two-thirds object glass and middle eye-piece. The effect which the carbonate of ammonia produces is the segregation of the homogeneous fluid in the cells into a cloud of granules and colourless fluid; and subsequently the granules coalesce into larger masses, and for hours have the oddest movements—coalescing, dividing, coalescing *ad infinitum*. I do not know whether you will care for these ill-written details; but, as you asked, I am sure I am bound to comply, after all the very kind and great trouble which you have taken."

On his return home he wrote to Sir J. D. Hooker (November 21, 1860):—

"I have been working like a madman at Drosera. Here is a fact for you which is certain as you stand where you are, though you won't believe it, that a bit of hair $\frac{1}{78000}$ of one grain in weight placed on gland, will cause *one* of the gland-bearing hairs of Drosera to curve inwards, and will alter the condition of the contents of every cell in the foot-stalk of the gland."

And a few days later to Lyell:—

"I will and must finish my Drosera MS., which will take

© The Complete Work of Charles Darwin Online

me a week, for, at the present moment, I care more about Drosera than the origin of all the species in the world. But I will not publish on Drosera till next year, for I am frightened and astounded at my results. I declare it is a certain fact, that one organ is so sensitive to touch, that a weight seventy-eight-times less than that, viz., $\frac{1}{1000}$ of a grain, which will move the best chemical balance, suffices to cause a conspicuous movement. Is it not curious that a plant should be far more sensitive to the touch than any nerve in the human body? Yet I am perfectly sure that this is true. When I am on my hobby-horse, I never can resist telling my friends how well my hobby goes, so you must forgive the rider."

The work was continued, as a holiday task, at Bournemouth, where he stayed during the autumn of 1862. The discussion in the following letter on "nervous matter" in Drosera is of interest in relation to recent researches on the continuity of protoplasm from cell to cell :]

C. Darwin to J. D. Hooker.

Cliff Cottage, Bournemouth.
September 26 [1862].

My dear Hooker,—Do not read this till you have leisure. If that blessed moment ever comes, I should be very glad to have your opinion on the subject of this letter. I am led to the opinion that Drosera must have diffused matter in organic connection, closely analogous to the nervous matter of animals. When the glands of one of the papillæ or tentacles, in its natural position is supplied with nitrogenised fluid and certain other stimulants, or when loaded with an extremely slight weight, or when struck several times with a needle, the pedicel bends near its base in under one minute. These varied stimulants are conveyed down the pedicel by some means; it cannot be vibration, for drops of fluid put on quite quietly cause the movement; it cannot be absorption of the

© The Complete Work of Charles Darwin Online

fluid from cell to cell, for I can see the rate of absorption, which though quick, is far slower, and in Dionæa the transmission is instantaneous ; analogy from animals would point to transmission through nervous matter. Reflecting on the rapid power of absorption in the glands, the extreme sensibility of the whole organ, and the conspicuous movement caused by varied stimulants, I have tried a number of substances which are not caustic or corrosive, ... but most of which are known to have a remarkable action on the nervous matter of animals. You will see the results in the enclosed paper. As the nervous matter of different animals are differently acted on by the same poisons, one would not expect the same action on plants and animals ; only, if plants have diffused nervous matter, some degree of analogous action. And this is partially the case. Considering these experiments, together with the previously made remarks on the functions of the parts, I cannot avoid the conclusion, that Drosera possesses matter at least in some degree analogous in constitution and function to nervous matter. Now do tell me what you think, as far as you can judge from my abstract ; of course many more experiments would have to be tried ; but in former years I tried on the whole leaf, instead of on separate glands, a number of innocuous * substances, such as sugar, gum, starch, &c., and they produced no effect. Your opinion will aid me in deciding some future year in going on with this subject. I should not have thought it worth attempting, but I had nothing on earth to do.

My dear Hooker, yours very sincerely,

CH. DARWIN.

P.S.—We return home on Monday 28th. Thank Heaven !

* This line of investigation made him wish for information on the action of poisons on plants ; as in many other cases he applied to Professor Oliver, and in reference to the result wrote to Hooker : "Pray thank Oliver heartily for his heap of references on poisons."

© The Complete Work of Charles Darwin Online

[A long break now ensued in his work on insectivorous plants, and it was not till 1872 that the subject seriously occupied him again. A passage in a letter to Dr. Asa Gray, written in 1863 or 1864, shows, however, that the question was not altogether absent from his mind in the interim :—

"Depend on it you are unjust on the merits of my beloved Drosera; it is a wonderful plant, or rather a most sagacious animal. I will stick up for Drosera to the day of my death. Heaven knows whether I shall ever publish my pile of experiments on it."

He notes in his diary that the last proof of the 'Expression of the Emotions' was finished on August 22, 1872, and that he began to work on Drosera on the following day.]

C. Darwin to Asa Gray.

[Sevenoaks], October 22 [1872].

. . . I have worked pretty hard for four or five weeks on Drosera, and then broke down; so that we took a house near Sevenoaks for three weeks (where I now am) to get complete rest. I have very little power of working now, and must put off the rest of the work on Drosera till next spring, as my plants are dying. It is an endless subject, and I must cut it short, and for this reason shall not do much on Dionæa. The point which has interested me most is tracing the *nerves!* which follow the vascular bundles. By a prick with a sharp lancet at a certain point, I can paralyse one-half the leaf, so that a stimulus to the other half causes no movement. It is just like dividing the spinal marrow of a frog:—no stimulus can be sent from the brain or anterior part of the spine to the hind legs; but if these latter are stimulated, they move by reflex action. I find my old results about the astonishing sensitiveness of the nervous system (!?) of Drosera to various stimulants fully confirmed and extended. . . .

[His work on digestion in Drosera and on other points in

© The Complete Work of Charles Darwin Online

the physiology of the plant soon led him into regions where his knowledge was defective, and here the advice and assistance which he received from Dr. Burdon Sanderson was of much value :]

C. Darwin to J. Burdon Sanderson.

Down, July 25, 1873.

MY DEAR DR. SANDERSON,—I should like to tell you a little about my recent work with Drosera, to show that I have profited by your suggestions, and to ask a question or two.

1. It is really beautiful how quickly and well Drosera and Dionæa dissolve little cubes of albumen and gelatine. I kept the same sized cubes on wet moss for comparison. When you were here I forgot that I had tried gelatine, but albumen is far better for watching its dissolution and absorption. Frankland has told me how to test in a rough way for pepsine; and in the autumn he will discover what acid the digestive juice contains.

2. A decoction of cabbage-leaves and green peas causes as much inflection as an infusion of raw meat; a decoction of grass is less powerful. Though I hear that the chemists try to precipitate all albumen from the extract of belladonna, I think they must fail, as the extract causes inflection, whereas a new lot of atropine, as well as the valerianate [of atropine], produce no effect.

3. I have been trying a good many experiments with heated water. . . . Should you not call the following case one of heat rigor? Two leaves were heated to 130°, and had every tentacle closely inflected; one was taken out and placed in cold water, and it re-expanded; the other was heated to 145°, and had not the least power of re-expansion. Is not this latter case heat rigor? If you can inform me, I should very much like to hear at what temperature cold-blooded and invertebrate animals are killed.

© The Complete Work of Charles Darwin Online

4. I must tell you my final result, of which I am sure, [as to] the sensitiveness of Drosera. I made a solution of one part of phosphate of ammonia by weight to 218,750 of water; of this solution I gave so much that a leaf got $\frac{1}{8000}$ of a grain of the phosphate. I then counted the glands, and each could have got only $\frac{1}{1552000}$ of a grain; this being absorbed by the glands, sufficed to cause the tentacles bearing these glands to bend through an angle of 180°. Such sensitiveness requires hot weather, and carefully selected young yet mature leaves. It strikes me as a wonderful fact. I must add that I took every precaution, by trying numerous leaves at the same time in the solution and in the same water which was used for making the solution.

5. If you can persuade your friend to try the effects of carbonate of ammonia on the aggregation of the white blood corpuscles, I should very much like to hear the result.

I hope this letter will not have wearied you.

Believe me, yours very sincerely,

CHARLES DARWIN.

C. Darwin to W. Thiselton Dyer.

Down, 24 [December 1873?].

MY DEAR MR. DYER,—I fear that you will think me a great bore, but I cannot resist telling you that I have just found out that the leaves of Pinguicula possess a beautifully adapted power of movement. Last night I put on a row of little flies near one edge of two *youngish* leaves; and after 14 hours these edges are beautifully folded over so as to clasp the flies, thus bringing the glands into contact with the upper surfaces of the flies, and they are now secreting copiously above and below the flies and no doubt absorbing. The acid secretion has run down the channelled edge and has collected in the spoon-shaped extremity, where no doubt the glands are absorbing the delicious soup. The leaf on one side looks

© The Complete Work of Charles Darwin Online

just like the helix of a human ear, if you were to stuff flies within the fold.

Yours most sincerely,
CH. DARWIN.

C. Darwin to Asa Gray.

Down, June 3 [1874].

. . . . I am now hard at work getting my book on Drosera & Co. ready for the printers, but it will take some time, for I am always finding out new points to observe. I think you will be interested by my observations on the digestive process in Drosera; the secretion contains an acid of the acetic series, and some ferment closely analogous to, but not identical with, pepsine; for I have been making a long series of comparative trials. No human being will believe what I shall publish about the smallness of the doses of phosphate of ammonia which act.

. . . . I began reading the Madagascar squib * quite gravely, and when I found it stated that Felis and Bos inhabited Madagascar, I thought it was a false story, and did not perceive it was a hoax till I came to the woman. . . .

C. Darwin to F. C. Donders.†

Down, July 7, 1874.

MY DEAR PROFESSOR DONDERS,—My son George writes to me that he has seen you, and that you have been very kind to him, for which I return to you my cordial thanks. He tells me on your authority, of a fact which interests me in the highest degree, and which I much wish to be allowed to quote. It relates to the action of one millionth of a grain of atropine on the eye. Now will you be so kind, whenever you can find a little leisure, to tell me whether you yourself have

* A description of a carnivorous plant supposed to subsist on human beings.

† Professor Donders, the well-known physiologist of Utrecht.

© The Complete Work of Charles Darwin Online

observed this fact, or believe it on good authority. I also wish to know what proportion by weight the atropine bore to the water of solution, and how much of the solution was applied to the eye. The reason why I am so anxious on this head is that it gives some support to certain facts repeatedly observed by me with respect to the action of phosphate of ammonia on Drosera. The $\frac{1}{4000000}$ of a grain absorbed by a gland clearly makes the tentacle which bears this gland become inflected ; and I am fully convinced that $\frac{1}{20000000}$ of a grain of the crystallised salt (*i.e.* containing about one-third of its weight of water of crystallisation) does the same. Now I am quite unhappy at the thought of having to publish such a statement. It will be of great value to me to be able to give any analogous facts in support. The case of Drosera is all the more interesting as the absorption of the salt or any other stimulant applied to the gland causes it to transmit a motor influence to the base of the tentacle which bears the gland.

Pray forgive me for troubling you, and do not trouble yourself to answer this until your health is fully re-established.

Pray believe me,

Yours very sincerely,

CHARLES DARWIN.

[During the summer of 1874 he was at work on the genus Utricularia, and he wrote (July 16th) to Sir J. D. Hooker giving some account of the progress of his work :—

"I am rather glad you have not been able to send Utricularia, for the common species has driven F. and me almost mad. The structure is *most* complex. The bladders catch a multitude of Entomostraca, and larvæ of insects. The mechanism for capture is excellent. But there is much that we cannot understand. From what I have seen to-day, strongly suspect that it is necrophagous, *i.e.* that it cannot digest, but absorbs decaying matter."

© The Complete Work of Charles Darwin Online

He was indebted to Lady Dorothy Nevill for specimens of the curious *Utricularia montana*, which is not aquatic like the European species, but grows among the moss and *débris* on the branches of trees. To this species the following letter refers:]

C. Darwin to Lady Dorothy Nevill.

Down, September 18 [1874].

DEAR LADY DOROTHY NEVILL,—I am so much obliged to you. I was so convinced that the bladders were with the leaves that I never thought of removing the moss, and this was very stupid of me. The great solid bladder-like swellings almost on the surface are wonderful objects, but are not the true bladders. These I found on the roots near the surface, and down to a depth of two inches in the sand. They are as transparent as glass, from $\frac{1}{20}$ to $\frac{1}{100}$ of an inch in size, and hollow. They have all the important points of structure of the bladders of the floating English species, and I felt confident I should find captured prey. And so I have to my delight in two bladders, with clear proof that they had absorbed food from the decaying mass. For Utricularia is a carrion-feeder, and not strictly carnivorous like Drosera.

The great solid bladder-like bodies, I believe, are reservoirs of water like a camel's stomach. As soon as I have made a few more observations, I mean to be so cruel as to give your plant no water, and observe whether the great bladders shrink and contain air instead of water; I shall then also wash all earth from all roots, and see whether there are true bladders for capturing subterranean insects down to the very bottom of the pot. Now shall you think me very greedy, if I say that supposing the species is not very precious, and you have several, will you give me one more plant, and if so, please to send it to "Orpington Station, S. E. R., to be forwarded by foot messenger."

I have hardly ever enjoyed a day more in my life than I

© The Complete Work of Charles Darwin Online

have this day's work; and this I owe to your Ladyship's great kindness.

The seeds are very curious monsters; I fancy of some plant allied to Medicago, but I will show them to Dr. Hooker.

Your Ladyship's very gratefully,

CH. DARWIN.

C. Darwin to J. D. Hooker.

Down, September 30, 1874.

MY DEAR H.,—Your magnificent present of Aldrovanda has arrived quite safe. I have enjoyed greatly a good look at the shut leaves, one of which I cut open. It is an aquatic Dionæa, which has acquired some structures identical with those of Utricularia!

If the leaves open, and I can transfer them open under the microscope, I will try some experiments, for mortal man cannot resist the temptation. If I cannot transfer, I will do nothing, for otherwise it would require hundreds of leaves.

You are a good man to give me such pleasure.

Yours affectionately,

C. DARWIN.

[The manuscript of 'Insectivorous Plants' was finished in March 1875. He seems to have been more than usually oppressed by the writing of this book, thus he wrote to Sir J. D. Hooker in February :—

"You ask about my book, and all that I can say is that I am ready to commit suicide; I thought it was decently written, but find so much wants rewriting, that it will not be ready to go to printers for two months, and will then make a confoundedly big book. Murray will say that it is no use publishing in the middle of summer, so I do not know what will be the upshot; but I begin to think that every one who publishes a book is a fool."

The book was published on July 2nd, 1875, and 2700 copies were sold out of the edition of 3000.]

© The Complete Work of Charles Darwin Online

CHAPTER XI.

THE 'POWER OF MOVEMENT IN PLANTS.' 1880.

[THE few sentences in the autobiographical chapter give with sufficient clearness the connection between the 'Power of Movement,' and one of the author's earlier books, that on 'Climbing Plants.' The central idea of the book is that the movements of plants in relation to light, gravitation, &c., are modifications of a spontaneous tendency to revolve or circumnutate, which is widely inherent in the growing parts of plants. This conception has not been generally adopted, and has not taken a place among the canons of orthodox physiology. The book has been treated by Professor Sachs with a few words of professorial contempt; and by Professor Wiesner it has been honoured by careful and generously expressed criticism.

Mr. Thiselton Dyer * has well said: "Whether this masterly conception of the unity of what has hitherto seemed a chaos of unrelated phenomena will be sustained, time alone will show. But no one can doubt the importance of what Mr. Darwin has done, in showing that for the future the phenomena of plant movement can and indeed must be studied from a single point of view."

The work was begun in the summer of 1877, after the publication of 'Different Forms of Flowers,' and by the autumn his enthusiasm for the subject was thoroughly established, and he wrote to Mr. Dyer: "I am all on fire at the

* 'Charles Darwin' ('Nature' Series), p. 41.

© The Complete Work of Charles Darwin Online

work." At this time he was studying the movements of cotyledons, in which the sleep of plants is to be observed in its simplest form; in the following spring he was trying to discover what useful purpose these sleep-movements could serve, and wrote to Sir Joseph Hooker (March 25th, 1878):—

"I think we have *proved* that the sleep of plants is to lessen the injury to the leaves from radiation. This has interested me much, and has cost us great labour, as it has been a problem since the time of Linnæus. But we have killed or badly injured a multitude of plants: N.B.—*Oxalis carnosa* was most valuable, but last night was killed."

His letters of this period do not give any connected account of the progress of the work. The two following seem worth giving as being characteristic of the author:]

C. Darwin to W. Thiselton Dyer.

Down, June 2, 1878.

MY DEAR DYER,—I remember saying that I should die a disgraced man if I did not observe a seedling Cactus and Cycas, and you have saved me from this horrible fate, as they move splendidly and normally. But I have two questions to ask: the Cycas observed was a huge seed in a broad and very shallow pot with cocoa-nut fibre as I suppose. It was named only Cycas. Was it *Cycas pectinata?* I suppose that I cannot be wrong in believing that what first appears above ground is a true leaf, for I can see no stem or axis. Lastly, you may remember that I said that we could not raise *Opuntia nigricans;* now I must confess to a piece of stupidity; one did come up, but my gardener and self stared at it, and concluded that it could not be a seedling Opuntia, but now that I have seen one of *O. basilaris,* I am sure it was; I observed it only casually, and saw movements, which makes me wish

© The Complete Work of Charles Darwin Online

to observe carefully another. If you have any fruit, will Mr. Lynch * be so kind as to send one more?

I am working away like a slave at radicles [roots] and at movements of true leaves, for I have pretty well done with cotyledons. . . .

That was an *excellent* letter about the Gardens: † I had hoped that the agitation was over. Politicians are a poor truckling lot, for [they] must see the wretched effects of keeping the gardens open all day long.

Your ever troublesome friend,
CH. DARWIN.

C. Darwin to W. Thiselton Dyer.

4 Bryanston St., Portman Square,
November 21 [1878].

MY DEAR DYER,—I must thank you for all the wonderful trouble which you have taken about the seeds of *Impatiens* and on scores of other occasions. It in truth makes me feel ashamed of myself, and I cannot help thinking: "Oh Lord, when he sees our book he will cry out, is this all for which I have helped so much!" In seriousness, I hope that we have made out some points, but I fear that we have done very little for the labour which we have expended on our work. We are here for a week for a little rest, which I needed.

If I remember right, November 30th, is the anniversary at the Royal, and I fear Sir Joseph must be almost at the last gasp. I shall be glad when he is no longer President.

Yours very sincerely,
CH. DARWIN.

[In the spring of the following year, 1879, when he was engaged in putting his results together, he wrote somewhat

* Mr. R. I. Lynch, now Curator of the Botanic Garden at Cambridge, was at this time in the Royal Gardens, Kew.

† This refers to an attempt to induce the Government to open the Royal Gardens at Kew in the morning.

© The Complete Work of Charles Darwin Online

despondingly to Mr. Dyer: "I am overwhelmed with my notes, and almost too old to undertake the job which I have in hand—*i.e.*, movements of all kinds. Yet it is worse to be idle."

Later on in the year, when the work was approaching completion, he wrote to Prof. Carus (July 17, 1879), with respect to a translation:—

"Together with my son Francis, I am preparing a rather large volume on the general movements of Plants, and I think that we have made out a good many new points and views.

"I fear that our views will meet a good deal of opposition in Germany; but we have been working very hard for some years at the subject.

"I shall be *much* pleased if you think the book worth translating, and proof-sheets shall be sent you, whenever they are ready."

In the autumn he was hard at work on the manuscript, and wrote to Dr. Gray (October 24, 1879):—

"I have written a rather big book—more is the pity—on the movements of plants, and I am now just beginning to go over the MS. for the second time, which is a horrid bore."

Only the concluding part of the next letter refers to the 'Power of Movement':]

C. Darwin to A. De Candolle.

May 28, 1880.

MY DEAR SIR,—I am particularly obliged to you for having so kindly sent me your 'Phytographie;' * for if I had merely seen it advertised, I should not have supposed that it could have concerned me. As it is, I have read with very great interest about a quarter, but will not delay longer thanking you. All that you say seems to me very clear and convincing, and as in all your writings I find a large number of

* A book on the methods of botanical research, more especially of systematic work.

© The Complete Work of Charles Darwin Online

philosophical remarks new to me, and no doubt shall find many more. They have recalled many a puzzle through which I passed when monographing the Cirripedia; and your book in those days would have been quite invaluable to me. It has pleased me to find that I have always followed your plan of making notes on separate pieces of paper; I keep several scores of large portfolios, arranged on very thin shelves about two inches apart, fastened to the walls of my study, and each shelf has its proper name or title; and I can thus put at once every memorandum into its proper place. Your book will, I am sure, be very useful to many young students, and I shall beg my son Francis (who intends to devote himself to the physiology of plants) to read it carefully.

As for myself I am taking a fortnight's rest, after sending a pile of MS. to the printers, and it was a piece of good fortune that your book arrived as I was getting into my carriage, for I wanted something to read whilst away from home. My MS. relates to the movements of plants, and I think that I have succeeded in showing that all the more important great classes of movements are due to the modification of a kind of movement common to all parts of all plants from their earliest youth.

Pray give my kind remembrances to your son, and with my highest respect and best thanks,

Believe me, my dear Sir, yours very sincerely,

CHARLES DARWIN.

P.S.—It always pleases me to exalt plants in the organic scale, and if you will take the trouble to read my last chapter when my book (which will be sadly too big) is published and sent to you, I hope and think that you also will admire some of the beautiful adaptations by which seedling plants are enabled to perform their proper functions.

[The book was published on November 6, 1880, and 1500

© The Complete Work of Charles Darwin Online

copies were disposed of at Mr. Murray's sale. With regard to it he wrote to Sir J. D. Hooker (November 23):—

"Your note has pleased me much—for I did not expect that you would have had time to read *any* of it. Read the last chapter, and you will know the whole result, but without the evidence. The case, however, of radicles bending after exposure for an hour to geotropism, with their tips (or brains) cut off is, I think, worth your reading (bottom of p. 525); it astounded me. The next most remarkable fact, as it appeared to me (p. 148), is the discrimination of the tip of the radicle between a slightly harder and softer object affixed on opposite sides of tip. But I will bother you no more about my book. The sensitiveness of seedlings to light is marvellous."

To another friend, Mr. Thiselton Dyer, he wrote (November 28, 1880):—

"Very many thanks for your most kind note, but you think too highly of our work, not but what this is very pleasant. . . . . Many of the Germans are very contemptuous about making out the use of organs; but they may sneer the souls out of their bodies, and I for one shall think it the most interesting part of Natural History. Indeed you are greatly mistaken if you doubt for one moment on the very great value of your constant and most kind assistance to us."

The book was widely reviewed, and excited much interest among the general public. The following letter refers to a leading article in the *Times*, November 20, 1880:]

*C. Darwin to Mrs. Haliburton.**

Down, November 22, 1880.

MY DEAR SARAH,—You see how audaciously I begin; but I have always loved and shall ever love this name. Your

* Mrs. Haliburton is a daughter of my father's early friend, the late Mr. Owen, of Woodhouse.

© The Complete Work of Charles Darwin Online

letter has done more than please me, for its kindness has touched my heart. I often think of old days and of the delight of my visits to Woodhouse, and of the deep debt of gratitude which I owe to your father. It was very good of you to write. I had quite forgotten my old ambition about the Shrewsbury newspaper;* but I remember the pride which I felt when I saw in a book about beetles the impressive words "captured by C. Darwin." Captured sounded so grand compared with caught. This seemed to me glory enough for any man! I do not know in the least what made the *Times* glorify me,† for it has sometimes pitched into me ferociously.

I should very much like to see you again, but you would find a visit here very dull, for we feel very old and have no amusement, and lead a solitary life. But we intend in a few weeks to spend a few days in London, and then if you have anything else to do in London, you would perhaps come and lunch with us.‡

Believe me, my dear Sarah,

Yours gratefully and affectionately,

CHARLES DARWIN.

[The following letter was called forth by the publication of a volume devoted to the criticism of the 'Power of Movement in Plants' by an accomplished botanist, Dr. Julius Wiesner, Professor of Botany in the University of Vienna:]

* Mrs. Haliburton had reminded him of his saying as a boy that if Eddowes' newspaper ever alluded to him as "our deserving fellow-townsman," his ambition would be amply gratified.

† The following is the opening sentence of the leading article:—"Of all our living men of science none have laboured longer and to more splendid purpose than Mr. Darwin."

‡ My father had the pleasure of seeing Mrs. Haliburton at his brother's house in Queen Anne Street.

© The Complete Work of Charles Darwin Online

C. Darwin to Julius Wiesner.

Down, October 25th, 1881.

MY DEAR SIR,—I have now finished your book,* and have understood the whole except a very few passages. In the first place, let me thank you cordially for the manner in which you have everywhere treated me. You have shown how a man may differ from another in the most decided manner, and yet express his difference with the most perfect courtesy. Not a few English and German naturalists might learn a useful lesson from your example; for the coarse language often used by scientific men towards each other does no good, and only degrades science.

I have been profoundly interested by your book, and some of your experiments are so beautiful, that I actually felt pleasure while being vivisected. It would take up too much space to discuss all the important topics in your book. I fear that you have quite upset the interpretation which I have given of the effects of cutting off the tips of horizontally extended roots, and of those laterally exposed to moisture; but I cannot persuade myself that the horizontal position of lateral branches and roots is due simply to their lessened power of growth. Nor when I think of my experiments with the cotyledons of *Phalaris*, can I give up the belief of the transmission of some stimulus due to light from the upper to the lower part. At p. 60 you have misunderstood my meaning, when you say that I believe that the effects from light are transmitted to a part which is not itself heliotropic. I never considered whether or not the short part beneath the ground was heliotropic; but I believe that with young seedlings the part which bends *near*, but *above* the ground is heliotropic, and I believe so from this part bending only moderately when the light is oblique, and bending rectangularly when the light is horizontal. Nevertheless the bending

* 'Das Bewegungsvermögen der Pflanzen.' Vienna, 1881.

© The Complete Work of Charles Darwin Online

of this lower part, as I conclude from my experiments with opaque caps, is influenced by the action of light on the upper part. My opinion, however, on the above and many other points, signifies very little, for I have no doubt that your book will convince most botanists that I am wrong in all the points on which we differ.

Independently of the question of transmission, my mind is so full of facts leading me to believe that light, gravity, &c., act not in a direct manner on growth, but as stimuli, that I am quite unable to modify my judgment on this head. I could not understand the passage at p. 78, until I consulted my son George, who is a mathematician. He supposes that your objection is founded on the diffused light from the lamp illuminating both sides of the object, and not being reduced, with increasing distance in the same ratio as the direct light; but he doubts whether this *necessary* correction will account for the very little difference in the heliotropic curvature of the plants in the successive pots.

With respect to the sensitiveness of the tips of roots to contact, I cannot admit your view until it is proved that I am in error about bits of card attached by liquid gum causing movement; whereas no movement was caused if the card remained separated from the tip by a layer of the liquid gum. The fact also of thicker and thinner bits of card attached on opposite sides of the same root by shellac, causing movement in one direction, has to be explained. You often speak of the tip having been injured; but externally there was no sign of injury: and when the tip was plainly injured, the extreme part became curved *towards* the injured side. I can no more believe that the tip was injured by the bits of card, at least when attached by gum-water, than that the glands of Drosera are injured by a particle of thread or hair placed on it, or that the human tongue is so when it feels any such object.

About the most important subject in my book, namely circumnutation, I can only say that I feel utterly bewildered

© The Complete Work of Charles Darwin Online

at the difference in our conclusions; but I could not fully understand some parts which my son Francis will be able to translate to me when he returns home. The greater part of your book is beautifully clear.

Finally, I wish that I had enough strength and spirit to commence a fresh set of experiments, and publish the results, with a full recantation of my errors when convinced of them; but I am too old for such an undertaking, nor do I suppose that I shall be able to do much, or any more, original work. I imagine that I see one possible source of error in your beautiful experiment of a plant rotating and exposed to a lateral light.

With high respect and with sincere thanks for the kind manner in which you have treated me and my mistakes, I remain,

My dear Sir, yours sincerely,

CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

CHAPTER XII.

MISCELLANEOUS BOTANICAL LETTERS.

1873–1882.

[THE present chapter contains a series of miscellaneous letters on botanical subjects. Some of them show my father's varied interests in botanical science, and others give account of researches which never reached completion.]

BLOOM ON LEAVES AND FRUIT.

[His researches into the meaning of the "bloom," or waxy coating found on many leaves, was one of those inquiries which remained unfinished at the time of his death. He amassed a quantity of notes on the subject, part of which I hope to publish at no distant date.*

One of his earliest letters on this subject was addressed in August, 1873, to Sir Joseph Hooker:—

"I want a little information from you, and if you do not yourself know, please to enquire of some of the wise men of Kew.

"Why are the leaves and fruit of so many plants protected by a thin layer of waxy matter (like the common cabbage),

* A small instalment, on the relation between bloom and the distribution of the stomata on leaves, has appeared in the 'Journal of the Linnean Society,' 1886. Tschirsch (*Linnæa*, 1881) has published results identical with some which my father and myself obtained, viz. that bloom diminishes transpiration. The same fact was previously published by Garreau, in 1850.

Z 2

© The Complete Work of Charles Darwin Online

or with fine hair, so that when such leaves or fruit are immersed in water they appear as if encased in thin glass? It is really a pretty sight to put a pod of the common pea, or a raspberry into water. I find several leaves are thus protected on the under surface and not on the upper.

"How can water injure the leaves if indeed this is at all the case?"

On this latter point he wrote to Sir Thomas Farrer:—

"I am now become mad about drops of water injuring leaves. Please ask Mr. Paine* whether he believes, *from his own experience*, that drops of water injure leaves or fruit in his conservatories. It is said that the drops act as burning-glasses; if this is true, they would not be at all injurious on cloudy days. As he is so acute a man, I should very much like to hear his opinion. I remember when I grew hot-house orchids I was cautioned not to wet their leaves; but I never then thought on the subject.

"I enjoyed my visit greatly with you, and I am very sure that all England could not afford a kinder and pleasanter host."

Some years later he took up the subject again, and wrote to Sir Joseph Hooker (May 25, 1877):—

"I have been looking over my old notes about the "bloom" on plants, and I think that the subject is well worth pursuing, though I am very doubtful of any success. Are you inclined to aid me on the mere chance of success, for without your aid I could do hardly anything?"]

C. Darwin to Asa Gray.

Down, June 4 [1877].

. . . . I am now trying to make out the use or function of "bloom," or the waxy secretion on the leaves and fruit of plants, but am *very* doubtful whether I shall succeed. Can

* Sir Thomas Farrer's gardener.

© The Complete Work of Charles Darwin Online

you give me any light? Are such plants commoner in warm than in colder climates? I ask because I often walk out in heavy rain, and the leaves of very few wild dicotyledons can be here seen with drops of water rolling off them like quicksilver. Whereas in my flower garden, greenhouse, and hot-houses there are several. Again, are bloom-protected plants common on your *dry* western plains? Hooker *thinks* that they are common at the Cape of Good Hope. It is a puzzle to me if they are common under very dry climates, and I find bloom very common on the Acacias and Eucalypti of Australia. Some of the Eucalypti which do not appear to be covered with bloom have the epidermis protected by a layer of some substance which is dissolved in boiling alcohol. Are there any bloom-protected leaves or fruit in the Arctic regions? If you can illuminate me, as you so often have done, pray do so; but otherwise do not bother yourself by answering.

Yours affectionately,
C. DARWIN.

C. Darwin to W. Thiselton Dyer.

Down, September 5 [1877].

MY DEAR DYER,—One word to thank you. I declare had it not been for your kindness, we should have broken down. As it is we have made out clearly that with some plants (chiefly succulent) the bloom checks evaporation—with some certainly prevents attacks of insects; with *some* sea-shore plants prevents injury from salt-water, and, I believe, with a few prevents injury from pure water resting on the leaves. This latter is as yet the most doubtful and the most interesting point in relation to the movements of plants.

© The Complete Work of Charles Darwin Online

C. Darwin to F. Müller.

Down, July 4 [1881].

MY DEAR SIR,—Your kindness is unbounded, and I cannot tell you how much your last letter (May 31) has interested me. I have piles of notes about the effect of water resting on leaves, and their movements (as I supposed) to shake off the drops. But I have not looked over these notes for a long time, and had come to think that perhaps my notion was mere fancy, but I had intended to begin experimenting as soon as I returned home; and now with your *invaluable* letter about the position of the leaves of various plants during rain (I have one analogous case with Acacia from South Africa), I shall be stimulated to work in earnest.

VARIABILITY.

[The following letter refers to a subject on which my father felt the strongest interest:—the experimental investigation of the causes of variability. The experiments alluded to were to some extent planned out, and some preliminary work was begun in the direction indicated below, but the research was ultimately abandoned.]

*C. Darwin to J. H. Gilbert.**

Down, February 16, 1876.

MY DEAR SIR,—When I met you at the Linnean Society, you were so kind as to say that you would aid me with advice, and this will be of the utmost value to me and my son. I will first state my object, and hope that you will excuse a long letter. It is admitted by all naturalists that no problem is so perplexing as what causes almost every cultivated plant to

* Dr. Gilbert, F.R.S., joint author with Sir John Bennett Lawes of a long series of valuable researches in Scientific Agriculture.

© The Complete Work of Charles Darwin Online

vary, and no experiments as yet tried have thrown any light on the subject. Now for the last ten years I have been experimenting in crossing and self-fertilising plants ; and one indirect result has surprised me much ; namely, that by taking pains to cultivate plants in pots under glass during several successive generations, under nearly similar conditions, and by self-fertilising them in each generation, the colour of the flowers often changes, and, what is very remarkable, they became in some of the most variable species, such as Mimulus, Carnation, &c., quite constant, like those of a wild species.

This fact and several others have led me to the suspicion that the cause of variation must be in different substances absorbed from the soil by these plants when their powers of absorption are not interfered with by other plants with which they grow mingled in a state of nature. Therefore my son and I wish to grow plants in pots in soil entirely, or as nearly entirely as is possible, destitute of all matter which plants absorb, and then to give during several successive generations to several plants of the same species as different solutions as may be compatible with their life and health. And now, can you advise me how to make soil approximately free of all the substances which plants naturally absorb ? I suppose white silver sand, sold for cleaning harness, &c., is nearly pure silica, but what am I to do for alumina ? Without some alumina I imagine that it would be impossible to keep the soil damp and fit for the growth of plants. I presume that clay washed over and over again in water would still yield mineral matter to the carbonic acid secreted by the roots. I should want a good deal of soil, for it would be useless to experimentise unless we could fill from twenty to thirty moderately sized flower-pots every year. Can you suggest any plan ? for unless you can it would, I fear, be useless for us to commence an attempt to discover whether variability depends at all on matter absorbed from the soil. After obtaining the requisite kind of soil, my notion is to water one set of plants with

© The Complete Work of Charles Darwin Online

nitrate of potassium, another set with nitrate of sodium, and another with nitrate of lime, giving all as much phosphate of ammonia as they seemed to support, for I wish the plants to grow as luxuriantly as possible. The plants watered with nitrate of Na and of Ca would require, I suppose, some K; but perhaps they would get what is absolutely necessary from such soil as I should be forced to employ, and from the rain-water collected in tanks. I could use hard water from a deep well in the chalk, but then all the plants would get lime. If the plants to which I give Nitrate of Na and of Ca would not grow I might give them a little alum.

I am well aware how very ignorant I am, and how crude my notions are; and if you could suggest any other solutions by which plants would be likely to be affected it would be a very great kindness. I suppose that there are no organic fluids which plants would absorb, and which I could procure?

I must trust to your kindness to excuse me for troubling you at such length, and,

I remain, dear Sir, yours sincerely,

CHARLES DARWIN.

[The next letter to Professor Semper bears on the same subject:]

*From C. Darwin to K. Semper.**

Down, July 19, 1881.

MY DEAR PROFESSOR SEMPER,—I have been much pleased to receive your letter, but I did not expect you to answer my former one. . . . . I cannot remember what I wrote to you, but I am sure that it must have expressed the interest which I felt in reading your book.† I thought that you attributed too much weight to the *direct* action of the

* Professor of Zoology at Würzburg.

† Published in the 'International Scientific Series,' in 1881, under the title, 'The Natural Conditions of Existence as they affect Animal Life.'

© The Complete Work of Charles Darwin Online

environment; but whether I said so I know not, for without being asked I should have thought it presumptuous to have criticised your book, nor should I now say so had I not during the last few days been struck with Professor Hoffmann's review of his own work in the 'Botanische Zeitung,' on the variability of plants; and it is really surprising how little effect he produced by cultivating certain plants under unnatural conditions, as the presence of salt, lime, zinc, &c., &c., during *several* generations. Plants, moreover, were selected which were the most likely to vary under such conditions, judging from the existence of closely-allied forms adapted for these conditions. No doubt I originally attributed too little weight to the direct action of conditions, but Hoffmann's paper has staggered me. Perhaps hundreds of generations of exposure are necessary. It is a most perplexing subject. I wish I was not so old, and had more strength, for I see lines of research to follow. Hoffmann even doubts whether plants vary more under cultivation than in their native home and under their natural conditions. If so, the astonishing variations of almost all cultivated plants must be due to selection and breeding from the varying individuals. This idea crossed my mind many years ago, but I was afraid to publish it, as I thought that people would say, "how he does exaggerate the importance of selection."

I still *must* believe that changed conditions give the impulse to variability, but that they act *in most cases* in a very indirect manner. But, as I said, it is a most perplexing problem. Pray forgive me for writing at such length; I had no intention of doing so when I sat down to write.

I am extremely sorry to hear, for your own sake and for that of Science, that you are so hard worked, and that so much of your time is consumed in official labour.

Pray believe me, dear Professor Semper,

Yours sincerely,

CHARLES DARWIN.

© The Complete Work of Charles Darwin Online

GALLS.

[Shortly before his death, my father began to experimentise on the possibility of producing galls artificially. A letter to Sir J. D. Hooker (Nov. 3, 1880) shows the interest which he felt in the question:—

"I was delighted with Paget's Essay; * I hear that he has occasionally attended to this subject from his youth . . . . I am very glad he has called attention to galls: this has always seemed to me a profoundly interesting subject; and if I had been younger would take it up."

His interest in this subject was connected with his ever-present wish to learn something of the causes of variation. He imagined to himself wonderful galls caused to appear on the ovaries of plants, and by these means he thought it possible that the seed might be influenced, and thus new varieties arise. He made a considerable number of experiments by injecting various reagents into the tissues of leaves, and with some slight indications of success.]

AGGREGATION.

[The following letter gives an idea of the subject of the last of his published papers.† The appearances which he observed in leaves and roots attracted him, on account of their relation to the phenomena of aggregation which had so deeply interested him when he was at work on Drosera:]

C. Darwin to S. H. Vines.‡

Down, November 1, 1881.

MY DEAR MR. VINES,—As I know how busy you are, it is a great shame to trouble you. But you are so rich in

* 'Disease in Plants,' by Sir James Paget. — See *Gardeners' Chronicle*, 1880.

† 'Journal of the Linnean Society.' Vol. xix., 1882, pp. 239 and 262.

‡ Reader in Botany in the University of Cambridge.

© The Complete Work of Charles Darwin Online

chemical knowledge about plants, and I am so poor, that I appeal to your charity as a pauper. My question is—Do you know of any solid substance in the cells of plants which glycerine and water dissolves? But you will understand my perplexity better if I give you the facts: I mentioned to you that if a plant of *Euphorbia peplus* is gently dug up and the roots placed for a short time in a weak solution (1 to 10,000 of water suffices in 24 hours) of carbonate of ammonia the (generally) alternate longitudinal rows of cells in every rootlet, from the root-cap up to the very top of the root (but not as far as I have yet seen in the green stem) become filled with translucent, brownish grains of matter. These rounded grains often cohere and even become confluent. Pure phosphate and nitrate of ammonia produce (though more slowly) the same effect, as does pure carbonate of soda.

Now, if slices of root under a cover-glass are irrigated with glycerine and water, every one of the innumerable grains in the cells disappear after some hours. What am I to think of this? . . . .

Forgive me for bothering you to such an extent; but I must mention that if the roots are dipped in boiling water there is no deposition of matter, and carbonate of ammonia afterwards produces no effect. I should state that I now find that the granular matter is formed in the cells immediately beneath the thin epidermis, and a few other cells near the vascular tissue. If the granules consisted of living protoplasm (but I can see no traces of movement in them), then I should infer that the glycerine killed them and aggregation ceased with the diffusion of invisibly minute particles, for I have seen an analogous phenomenon in Drosera.

If you can aid me, pray do so, and anyhow forgive me.

Yours very sincerely,

CH. DARWIN.

© The Complete Work of Charles Darwin Online

Mr. Torbitt's Experiments on the Potato-Disease.

[Mr. James Torbitt, of Belfast, has been engaged for the last twelve years in the difficult undertaking, in which he has been to a large extent successful, of raising fungus-proof varieties of the potato. My father felt great interest in Mr. Torbitt's work, and corresponded with him from 1876 onwards. The following letter, giving a clear account of Mr. Torbitt's method and of my father's opinion of the probability of its success, was written with the idea that Government aid for the work might possibly be obtainable:]

C. Darwin to T. H. Farrer.

Down, March 2, 1878.

My dear Farrer,—Mr. Torbitt's plan of overcoming the potato-disease seems to me by far the best which has ever been suggested. It consists, as you know from his printed letter, of rearing a vast number of seedlings from cross-fertilised parents, exposing them to infection, ruthlessly destroying all that suffer, saving those which resist best, and repeating the process in successive seminal generations. My belief in the probability of good results from this process rests on the fact of all characters whatever occasionally varying. It is known, for instance, that certain species and varieties of the vine resist phylloxera better than others. Andrew Knight found one variety or species of the apple which was not in the least attacked by coccus, and another variety has been observed in South Australia. Certain varieties of the peach resist mildew, and several other such cases could be given. Therefore there is no great improbability in a new variety of potato arising which would resist the fungus completely, or at least much better than any existing variety. With respect to the cross-fertilisation of two distinct seedling plants, it has been ascertained that the offspring thus raised inherit much

© The Complete Work of Charles Darwin Online

more vigorous constitutions and generally are more prolific than seedlings from self-fertilised parents. It is also probable that cross-fertilisation would be especially valuable in the case of the potato, as there is reason to believe that the flowers are seldom crossed by our native insects; and some varieties are absolutely sterile unless fertilised with pollen from a distinct variety. There is some evidence that the good effects from a cross are transmitted for several generations; it would not, therefore be necessary to cross-fertilise the seedlings in each generation, though this would be desirable, as it is almost certain that a greater number of seeds would thus be obtained. It should be remembered that a cross between plants raised from the tubers of the same plant, though growing on distinct roots, does no more good than a cross between flowers on the same individual. Considering the whole subject, it appears to me that it would be a national misfortune if the cross-fertilised seeds in Mr. Torbitt's possession produced by parents which have already shown some power of resisting the disease, are not utilised by the Government, or some public body, and the process of selection continued during several more generations.

Should the Agricultural Society undertake the work, Mr. Torbitt's knowledge gained by experience would be especially valuable; and an outline of the plan is given in his printed letter. It would be necessary that all the tubers produced by each plant should be collected separately, and carefully examined in each succeeding generation.

It would be advisable that some kind of potato eminently liable to the disease should be planted in considerable numbers near the seedlings so as to infect them.

Altogether the trial would be one requiring much care and extreme patience, as I know from experience with analogous work, and it may be feared that it would be difficult to find any one who would pursue the experiment with sufficient energy. It seems, therefore, to me highly desirable that

© The Complete Work of Charles Darwin Online

Mr. Torbitt should be aided with some small grant so as to continue the work himself.

Judging from his reports, his efforts have already been crowned in so short a time with more success than could have been anticipated; and I think you will agree with me, that any one who raises a fungus-proof potato will be a public benefactor of no common kind.

My dear Farrer, yours sincerely,
CHARLES DARWIN.

[After further consultation with Sir Thomas Farrer and with Mr. Caird, my father became convinced that it was hopeless to attempt to obtain Government aid. He wrote to Mr. Torbitt to this effect, adding, "it would be less trouble to get up a subscription from a few rich leading agriculturists than from Government. This plan I think you cannot object to, as you have asked nothing, and will have nothing whatever to do with the subscription. In fact, the affair is, in my opinion, a compliment to you." The idea thus broached was carried out, and Mr. Torbitt was enabled to continue his work by the aid of a sum to which Sir T. Farrer, Mr. Caird, my father, and a few friends, subscribed.

My father's sympathy and encouragement were highly valued by Mr. Torbitt, who tells me that without them he should long ago have given up his attempt. A few extracts will illustrate his fellow-feeling with Mr. Torbitt's energy and perseverance:—

"I admire your indomitable spirit. If any one ever deserved success, you do so, and I keep to my original opinion that you have a very good chance of raising a fungus-proof variety of the potato.

"A pioneer in a new undertaking is sure to meet with many disappointments, so I hope that you will keep up your courage, though we have done so very little for you."

© The Complete Work of Charles Darwin Online

Mr. Torbitt tells me that he still (1887) succeeds in raising varieties possessing well-marked powers of resisting disease; but this immunity is not permanent, and, after some years, the varieties become liable to the attacks of the fungus.]

The Kew Index of Plant-Names, or 'Nomenclator Botanicus Darwinianus'.

[Some account of my father's connection with the Index of Plant-names now (1887) in course of preparation at Kew will be found in Mr. B. Daydon Jackson's paper in the 'Journal of Botany,' 1887, p. 151. Mr. Jackson quotes the following statement by Sir J. D. Hooker:—

"Shortly before his death, Mr. Charles Darwin informed Sir Joseph Hooker that it was his intention to devote a considerable sum of money annually for some years in aid or furtherance of some work or works of practical utility to biological science, and to make provisions in his will in the event of these not being completed during his lifetime.

"Amongst other objects connected with botanical science, Mr. Darwin regarded with especial interest the importance of a complete index to the names and authors of the genera and species of plants known to botanists, together with their native countries. Steudel's 'Nomenclator' is the only existing work of this nature, and although now nearly half a century old, Mr. Darwin had found it of great aid in his own researches. It has been indispensable to every botanical institution, whether as a list of all known flowering plants, as an indication of their authors, or as a digest of botanical geography."

Since 1840, when the 'Nomenclator' was published, the number of described plants may be said to have doubled, so

© The Complete Work of Charles Darwin Online

that the 'Nomenclator' is now seriously below the requirements of botanical work. To remedy this want, the 'Nomenclator' has been from time to time posted up in an interleaved copy in the Herbarium at Kew, by the help of "funds supplied by private liberality." *

My father, like other botanists, had as Sir Joseph Hooker points out, experienced the value of Steudel's work. He obtained plants from all sorts of sources, which were often incorrectly named, and he felt the necessity of adhering to the accepted nomenclature, so that he might convey to other workers precise indications as to the plants which he had studied. It was also frequently a matter of importance to him to know the native country of his experimental plants. Thus it was natural that he should recognize the desirability of completing and publishing the interleaved volume at Kew. The wish to help in this object was heightened by the admiration he felt for the results for which the world has to thank the Royal Gardens at Kew, and by his gratitude for the invaluable aid which for so many years he received from its Director and his staff. He expressly stated that it was his wish "to aid in some way the scientific work carried on at the Royal Gardens" †—which induced him to offer to supply funds for the completion of the Kew 'Nomenclator.'

The following passage, for which I am indebted to Professor Judd, is of interest, as illustrating the motives that actuated my father in this matter. Professor Judd writes:—

"On the occasion of my last visit to him, he told me that his income having recently greatly increased, while his wants remained the same, he was most anxious to devote what he could spare to the advancement of Geology or Biology. He dwelt in the most touching manner on the fact that he owed so much happiness and fame to the natural-history sciences

* Kew Gardens Report, 1881, p. 62.

† See 'Nature,' January 5, 1882.

© The Complete Work of Charles Darwin Online

which had been the solace of what might have been a painful existence;—and he begged me, if I knew of any research which could be aided by a grant of a few hundreds of pounds, to let him know, as it would be a delight to him to feel that he was helping in promoting the progress of science. He informed me at the same time that he was making the same suggestion to Sir Joseph Hooker and Professor Huxley with respect to Botany and Zoology respectively. I was much impressed by the earnestness, and, indeed, deep emotion, with which he spoke of his indebtedness to Science, and his desire to promote its interests."

Sir Joseph Hooker was asked by my father "to take into consideration, with the aid of the botanical staff at Kew and the late Mr. Bentham, the extent and scope of the proposed work, and to suggest the best means of having it executed. In doing this, Sir Joseph had further the advantage of the great knowledge and experience of Professor Asa Gray, of Cambridge, U.S.A., and of Mr. John Ball, F.R.S." *

The plan of the proposed work having been carefully considered, Sir Joseph Hooker was able to confide its elaboration in detail to Mr. B. Daydon Jackson, Secretary of the Linnean Society, whose extensive knowledge of botanical literature qualifies him for the task. My father's original idea of producing a modern edition of Steudel's 'Nomenclator' has been practically abandoned, the aim now kept in view is rather to construct a list of genera and species (with references) founded on Bentham and Hooker's 'Genera Plantarum.' The colossal nature of the work in progress at Kew may be estimated by the fact that the manuscript of the 'Index' is at the present time (1887) believed to weigh more than a ton. Under Sir Joseph Hooker's supervision the work goes steadily forward, being carried out with admirable zeal by Mr. Jackson, who devotes himself unsparingly to the enterprise, in which,

* 'Journal of Botany,' *loc. cit.*

© The Complete Work of Charles Darwin Online

too, he has the advantage of the interest in the work felt by Professor Oliver and Mr. Thiselton Dyer.

The Kew 'Index,' which will, in all probability, be ready to go to press in four or five years, will be a fitting memorial of my father: and his share in its completion illustrates a part of his character—his ready sympathy with work outside his own lines of investigation—and his respect for minute and patient labour in all branches of science.]

© The Complete Work of Charles Darwin Online

CHAPTER XIII.

CONCLUSION.

SOME idea of the general course of my father's health may have been gathered from the letters given in the preceding pages. The subject of health appears more prominently than is often necessary in a Biography, because it was, unfortunately, so real an element in determining the outward form of his life.

During the last ten years of his life the state of his health was a cause of satisfaction and hope to his family. His condition showed signs of amendment in several particulars. He suffered less distress and discomfort, and was able to work more steadily. Something has been already said of Dr. Bence Jones's treatment, from which my father certainly derived benefit. In later years he became a patient of Sir Andrew Clark, under whose care he improved greatly in general health. It was not only for his generously rendered service that my father felt a debt of gratitude towards Sir Andrew Clark. He owed to his cheering personal influence an often-repeated encouragement, which latterly added something real to his happiness, and he found sincere pleasure in Sir Andrew's friendship and kindness towards himself and his children.

Scattered through the past pages are one or two references to pain or uneasiness felt in the region of the heart. How far these indicate that the heart was affected early in life, I cannot pretend to say; in any case it is certain that he had no serious or permanent trouble of this nature until

© The Complete Work of Charles Darwin Online

shortly before his death. In spite of the general improvement in his health, which has been above alluded to, there was a certain loss of physical vigour occasionally apparent during the last few years of his life. This is illustrated by a sentence in a letter to his old friend Sir James Sulivan, written on January 10, 1879: "My scientific work tires me more than it used to do, but I have nothing else to do, and whether one is worn out a year or two sooner or later signifies but little."

A similar feeling is shown in a letter to Sir J. D. Hooker of June 15, 1881. My father was staying at Patterdale, and wrote: "I am rather despondent about myself . . . . I have not the heart or strength to begin any investigation lasting years, which is the only thing which I enjoy, and I have no little jobs which I can do."

In July, 1881, he wrote to Mr. Wallace, "We have just returned home after spending five weeks on Ullswater; the scenery is quite charming, but I cannot walk, and everything tires me, even seeing scenery . . . . What I shall do with my few remaining years of life I can hardly tell. I have everything to make me happy and contented, but life has become very wearisome to me." He was, however, able to do a good deal of work, and that of a trying sort,* during the autumn of 1881, but towards the end of the year he was clearly in need of rest; and during the winter was in a lower condition than was usual with him.

On December 13, he went for a week to his daughter's house in Bryanston Street. During his stay in London he went to call on Mr. Romanes, and was seized when on the door-step with an attack apparently of the same kind as those which afterwards became so frequent. The rest of the incident, which I give in Mr. Romanes' words, is interesting too from a different point of view, as giving one more illustration of my father's scrupulous consideration for others :—

* On the action of carbonate of ammonia on roots and leaves.

© The Complete Work of Charles Darwin Online

"I happened to be out, but my butler, observing that Mr. Darwin was ill, asked him to come in. He said he would prefer going home, and although the butler urged him to wait at least until a cab could be fetched, he said he would rather not give so much trouble. For the same reason he refused to allow the butler to accompany him. Accordingly he watched him walking with difficulty towards the direction in which cabs were to be met with, and saw that, when he had got about three hundred yards from the house, he staggered and caught hold of the park-railings as if to prevent himself from falling. The butler therefore hastened to his assistance, but after a few seconds saw him turn round with the evident purpose of retracing his steps to my house. However, after he had returned part of the way he seems to have felt better, for he again changed his mind, and proceeded to find a cab."

During the last week of February and in the beginning of March, attacks of pain in the region of the heart, with irregularity of the pulse, became frequent, coming on indeed nearly every afternoon. A seizure of this sort occurred about March 7, when he was walking alone at a short distance from the house; he got home with difficulty, and this was the last time that he was able to reach his favourite 'Sand-walk.' Shortly after this, his illness became obviously more serious and alarming, and he was seen by Sir Andrew Clark, whose treatment was continued by Dr. Norman Moore, of St. Bartholomew's Hospital, and Mr. Allfrey, of St. Mary Cray. He suffered from distressing sensations of exhaustion and faintness, and seemed to recognise with deep depression the fact that his working days were over. He gradually recovered from this condition, and became more cheerful and hopeful, as is shown in the following letter to Mr. Huxley, who was anxious that my father should have closer medical supervision than the existing arrangements allowed:—

© The Complete Work of Charles Darwin Online

Down, March 27, 1882.

"MY DEAR HUXLEY,—Your most kind letter has been a real cordial to me. I have felt better to-day than for three weeks, and have felt as yet no pain. Your plan seems an excellent one, and I will probably act upon it, unless I get very much better. Dr. Clark's kindness is unbounded to me, but he is too busy to come here. Once again, accept my cordial thanks, my dear old friend. I wish to God there were more automata * in the world like you.

Ever yours,
CH. DARWIN."

The allusion to Sir Andrew Clark requires a word of explanation. Sir Andrew Clark himself was ever ready to devote himself to my father, who, however, could not endure the thought of sending for him, knowing how severely his great practice taxed his strength.

No especial change occurred during the beginning of April, but on Saturday 15th he was seized with giddiness while sitting at dinner in the evening, and fainted in an attempt to reach his sofa. On the 17th he was again better, and in my temporary absence recorded for me the progress of an experiment in which I was engaged. During the night of April 18th, about a quarter to twelve, he had a severe attack and passed into a faint, from which he was brought back to consciousness with great difficulty. He seemed to recognise the approach of death, and said, "I am not the least afraid to die." All the next morning he suffered from terrible nausea and faintness, and hardly rallied before the end came.

He died at about four o'clock on Wednesday, April 19th, 1882.

* The allusion is to Mr. Huxley's address, "On the hypothesis that animals are automata, and its history," given at the Belfast Meeting of the British Association, 1874, and republished in 'Science and Culture.'

© The Complete Work of Charles Darwin Online

I close the record of my father's life with a few words of retrospect added to the manuscript of his 'Autobiography' in 1879:—

"As for myself, I believe that I have acted rightly in steadily following and devoting my life to Science. I feel no remorse from having committed any great sin, but have often and often regretted that I have not done more direct good to my fellow creatures."

THE END.

© The Complete Work of Charles Darwin Online

APPENDIX I.

THE FUNERAL IN WESTMINSTER ABBEY.

ON the Friday succeeding my father's death, the following letter, signed by twenty Members of Parliament, was addressed to Dr. Bradley, Dean of Westminster:—

HOUSE OF COMMONS, April 21, 1882.

VERY REV. SIR,—We hope you will not think we are taking a liberty if we venture to suggest that it would be acceptable to a very large number of our fellow-countrymen of all classes and opinions that our illustrious countryman, Mr. Darwin, should be buried in Westminster Abbey.

We remain your obedient servants,

JOHN LUBBOCK,	RICHARD B. MARTIN,
NEVIL STOREY MASKELYNE,	FRANCIS W. BUXTON,
A. J. MUNDELLA,	E. L. STANLEY,
G. O. TREVELYAN,	HENRY BROADHURST,
LYON PLAYFAIR,	JOHN BARRAN,
CHARLES W. DILKE,	J. F. CHEETHAM,
DAVID WEDDERBURN,	H. S. HOLLAND,
ARTHUR RUSSELL,	H. CAMPBELL-BANNERMAN,
HORACE DAVEY,	CHARLES BRUCE,
BENJAMIN ARMITAGE,	RICHARD FORT.

The Dean was abroad at the time, and telegraphed his cordial acquiescence.

The family had desired that my father should be buried at Down: with regard to their wishes, Sir John Lubbock wrote :—

© The Complete Work of Charles Darwin Online

HOUSE OF COMMONS, April 25, 1882.

MY DEAR DARWIN,—I quite sympathise with your feeling, and personally I should greatly have preferred that your father should have rested in Down amongst us all. It is, I am sure, quite understood that the initiative was not taken by you. Still, from a national point of view, it is clearly right that he should be buried in the Abbey. I esteem it a great privilege to be allowed to accompany my dear master to the grave.

Believe me, yours most sincerely,
JOHN LUBBOCK.

W. E. DARWIN, ESQ.

The family gave up their first-formed plans, and the funeral took place in Westminster Abbey on April 26th. The pall-bearers were:—

SIR JOHN LUBBOCK,	CANON FARRAR,
MR. HUXLEY,	SIR JOSEPH HOOKER,
MR. JAMES RUSSELL LOWELL (American Minister),	MR. WM. SPOTTISWOODE (President of the Royal Society),
MR. A. R. WALLACE,	The Earl of DERBY,
The DUKE OF DEVONSHIRE,	The DUKE OF ARGYLL.

The funeral was attended by the representatives of France, Germany, Italy, Spain, Russia, and by those of the Universities and learned Societies, as well as by large numbers of personal friends and distinguished men.

The grave is in the north aisle of the Nave, close to the angle of the choir-screen, and a few feet from the grave of Sir Isaac Newton. The stone bears the inscription—

CHARLES ROBERT DARWIN.
Born 12 February, 1809.
Died 19 April, 1882.

© The Complete Work of Charles Darwin Online

APPENDIX II.

I.—List of Works by C. Darwin.

Narrative of the Surveying Voyages of Her Majesty's Ships 'Adventure' and 'Beagle' between the years 1826 and 1836, describing their examination of the Southern shores of South America, and the 'Beagle's' circumnavigation of the globe. Vol. iii. Journal and Remarks, 1832–1836. By Charles Darwin. 8vo. London, 1839.

Journal of Researches into the Natural History and Geology of the countries visited during the Voyage of H.M.S. 'Beagle' round the world, under the command of Capt. Fitz-Roy, R.N. 2nd edition, corrected, with additions. 8vo. London, 1845. (Colonial and Home Library.)

A Naturalist's Voyage. Journal of Researches, &c. 8vo. London, 1860. [Contains a postscript dated Feb. 1, 1860.]

Zoology of the Voyage of H.M.S. 'Beagle.' Edited and superintended by Charles Darwin. Part I. Fossil Mammalia, by Richard Owen. With a Geological Introduction, by Charles Darwin. 4to. London, 1840.

—— Part II. Mammalia, by George R. Waterhouse. With a notice of their habits and ranges, by Charles Darwin. 4to. London, 1839.

—— Part III. Birds, by John Gould. An "Advertisement" (2 pp.) states that in consequence of Mr. Gould's having left England for Australia, many descriptions were supplied by Mr. G. R. Gray of the British Museum. 4to. London, 1841.

—— Part IV. Fish, by Rev. Leonard Jenyns. 4to. London, 1842.

—— Part V. Reptiles, by Thomas Bell. 4to. London, 1843.

The Structure and Distribution of Coral Reefs. Being the First

© The Complete Work of Charles Darwin Online

Part of the Geology of the Voyage of the 'Beagle.' 8vo. London, 1842.

The Structure and Distribution of Coral Reefs. 2nd edition. 8vo. London, 1874.

Geological Observations on the Volcanic Islands, visited during the Voyage of H.M.S. 'Beagle.' Being the Second Part of the Geology of the Voyage of the 'Beagle.' 8vo. London, 1844.

Geological Observations on South America. Being the Third Part of the Geology of the Voyage of the 'Beagle.' 8vo. London, 1846.

Geological Observations on the Volcanic Islands and parts of South America visited during the Voyage of H.M.S. 'Beagle.' 2nd edition. 8vo. London, 1876.

A Monograph of the Fossil Lepadidæ; or, Pedunculated Cirripedes of Great Britain. 4to. London, 1851. (Palæontographical Society.)

A Monograph of the Sub-class Cirripedia, with Figures of all the Species. The Lepadidæ; or, Pedunculated Cirripedes. 8vo. London, 1851. (Ray Society.)

—— The Balanidæ (or Sessile Cirripedes); the Verrucidæ, &c. 8vo. London, 1854. (Ray Society.)

A Monograph of the Fossil Balanidæ and Verrucidæ of Great Britain. 4to. London, 1854. (Palæontographical Society.)

On the Origin of Species by means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. 8vo. London, 1859. (Dated Oct. 1st, 1859, published Nov. 24, 1859.)

—— Fifth thousand. 8vo. London, 1860.

—— Third edition, with additions and corrections. (Seventh thousand.) 8vo. London, 1861. (Dated March, 1861.)

—— Fourth edition, with additions and corrections. (Eighth thousand.) 8vo. London, 1866. (Dated June, 1866.)

—— Fifth edition, with additions and corrections. (Tenth thousand.) 8vo. London, 1869. (Dated May, 1869.)

—— Sixth edition, with additions and corrections to 1872. (Twenty-fourth thousand.) 8vo. London, 1882. (Dated Jan., 1872.)

On the various contrivances by which Orchids are fertilised by Insects. 8vo. London, 1862.

—— Second edition. 8vo. London, 1877. [In the second edition the word "On" is omitted from the title.]

© The Complete Work of Charles Darwin Online

The Movements and Habits of Climbing Plants. Second edition. 8vo. London, 1875. [First appeared in the ninth volume of the 'Journal of the Linnean Society.']

The Variation of Animals and Plants under Domestication. 2 vols. 8vo. London, 1868.

—— Second edition, revised. 2 vols. 8vo. London, 1875.

The Descent of Man, and Selection in Relation to Sex. 2 vols. 8vo. London, 1871.

—— Second edition. 8vo. London, 1874. (In 1 vol.)

The Expression of the Emotions in Man and Animals. 8vo. London, 1872.

The Effects of Cross and Self Fertilisation in the Vegetable Kingdom. 8vo. London, 1876.

—— Second edition. 8vo. London, 1878.

The different Forms of Flowers on Plants of the same Species. 8vo. London, 1877.

—— Second edition. 8vo. London, 1880.

The Power of Movement in Plants. By Charles Darwin, assisted by Francis Darwin. 8vo. London, 1880.

The Formation of Vegetable Mould, through the Action of Worms, with Observations on their Habits. 8vo. London, 1881.

II.—List of Books containing Contributions by C. Darwin.

A manual of scientific enquiry; prepared for the use of Her Majesty's Navy: and adapted for travellers in general. Ed. by Sir John F. W. Herschel, Bart. 8vo. London, 1849. (Section VI. Geology. By Charles Darwin.)

Memoir of the Rev. John Stevens Henslow. By the Rev. Leonard Jenyns. 8vo. London, 1862. [In Chapter III., Recollections by C. Darwin.]

A letter (1876) on the 'Drift' near Southampton, published in Prof. J. Geikie's 'Prehistoric Europe.'

Flowers and their unbidden guests. By A. Kerner. With a Prefatory Letter by Charles Darwin. The translation revised and edited by W. Ogle. 8vo. London, 1878.

Erasmus Darwin. By Ernst Krause. Translated from the German by W. S. Dallas. With a preliminary notice by Charles Darwin. 8vo. London, 1879.

Studies in the Theory of Descent. By Aug. Weismann. Translated

© The Complete Work of Charles Darwin Online

and edited by Raphael Meldola. With a Prefatory Notice by Charles Darwin. 8vo. London, 1880—.

The Fertilisation of Flowers. By Hermann Müller. Translated and edited by D'Arcy W. Thompson. With a Preface by Charles Darwin. 8vo. London, 1883.

Mental Evolution in Animals. By G. J. Romanes. With a posthumous essay on instinct by Charles Darwin, 1883. [Also published in the Journal of the Linnean Society.]

Some Notes on a curious habit of male humble bees were sent to Prof. Hermann Müller, of Lippstadt, who had permission from Mr. Darwin to make what use he pleased of them. After Müller's death the Notes were given by his son to Dr. E. Krause, who published them under the title, "Ueber die Wege der Hummel-Männchen" in his book, 'Gesammelte kleinere Schriften von Charles Darwin' (1887).

III.—List of Scientific Papers, including a selection of Letters and Short Communications to Scientific Journals.

Letters to Professor Henslow, read by him at the meeting of the Cambridge Philosophical Society, held Nov. 16, 1835. 31 pp. 8vo. Privately printed for distribution among the members of the Society.

Geological Notes made during a survey of the East and West Coasts of South America in the years 1832, 1833, 1834, and 1835; with an account of a transverse section of the Cordilleras of the Andes between Valparaiso and Mendoza. [Read Nov. 18, 1835.] Geol. Soc. Proc. ii. 1838, pp. 210–212. [This Paper is incorrectly described in Geol. Soc. Proc. ii., p. 210 as follows:—"Geological notes, &c., by F. Darwin, Esq., of St. John's College, Cambridge: communicated by Prof. Sedgwick." It is Indexed under C. Darwin.]

Notes upon the Rhea Americana. Zool. Soc. Proc., Part v. 1837, pp. 35–36.

Observations of proofs of recent elevation on the coast of Chili, made during the survey of H.M.S. "Beagle," commanded by Capt. FitzRoy. [1837.] Geol. Soc. Proc. ii. 1838, pp. 446–449.

A sketch of the deposits containing extinct Mammalia in the neighbourhood of the Plata. [1837.] Geol. Soc. Proc. ii. 1838, pp. 542–544.

On certain areas of elevation and subsidence in the Pacific and

© The Complete Work of Charles Darwin Online

Indian oceans, as deduced from the study of coral formations. [1837.] Geol. Soc. Proc. ii. 1838, pp. 552–554.

On the Formation of Mould. [Read Nov. 1, 1837.] Geol. Soc. Proc. ii. 1838, pp. 574–576; Geol. Soc. Trans. v. 1840, pp. 505–510.

On the Connexion of certain Volcanic Phenomena and on the formation of mountain-chains and the effects of continental elevations. [Read March 7, 1838.] Geol. Soc. Proc. ii. 1838, pp. 654–660; Geol. Soc. Trans. v. 1840, pp. 601–632. [In the Society's Transactions the wording of the title is slightly different.]

Origin of saliferous deposits. Salt Lakes of Patagonia and La Plata. Geol. Soc. Journ. ii. (Part ii.), 1838, pp. 127–128.

Note on a Rock seen on an Iceberg in 16° South Latitude. Geogr. Soc. Journ. ix. 1839, pp. 528–529.

Observations on the Parallel Roads of Glen Roy, and of other parts of Lochaber in Scotland, with an attempt to prove that they are of marine origin. Phil. Trans. 1839, pp. 39–82.

On a remarkable Bar of Sandstone off Pernambuco, on the Coast of Brazil. Phil. Mag. xix. 1841, pp. 257–260.

On the Distribution of the Erratic Boulders and on the Contemporaneous Unstratified Deposits of South America. [1841.] Geol. Soc. Proc. iii. 1842, pp. 425–430; Geol. Soc. Trans. [1841.] vi. 1842, pp. 415–432.

Notes on the Effects produced by the Ancient Glaciers of Caernarvonshire, and on the Boulders transported by Floating Ice. London Philosoph. Mag. vol. xxi. p. 180. 1842.

Remarks on the preceding paper, in a Letter from Charles Darwin, Esq., to Mr. Maclaren. Edinb. New Phil. Journ. xxxiv. 1843, pp. 47–50. [The "preceding" paper is: "On Coral Islands and Reefs as described by Mr. Darwin. By Charles Maclaren, Esq., F.R.S.E."]

Observations on the Structure and Propagation of the genus *Sagitta*. Ann. and Mag. Nat. Hist. xiii. 1844, pp. 1–6.

Brief Descriptions of several Terrestrial *Planariæ*, and of some remarkable Marine Species, with an Account of their Habits. Ann. and Mag. Nat. Hist. xiv. 1844, pp. 241–251.

An account of the Fine Dust which often falls on Vessels in the Atlantic Ocean. Geol. Soc. Journ. ii. 1846, pp. 26–30.

On the Geology of the Falkland Islands. Geol. Soc. Journ. ii. 1846, pp. 267–274.

© The Complete Work of Charles Darwin Online

A review of Waterhouse's 'Natural History of the Mammalia.' [Not signed.] Ann. and Mag. of Nat. Hist. 1847. Vol. xix. p. 53.

On the Transportal of Erratic Boulders from a lower to a higher level. Geol. Soc. Journ. iv. 1848, pp. 315–323.

On British fossil Lepadidæ. Geol. Soc. Journ. vi. 1850, pp. 439–440. [The G. S. J. says, "This paper was withdrawn by the author with the permission of the Council."].

Analogy of the Structure of some Volcanic Rocks with that of Glaciers. Edinb. Roy. Soc. Proc. ii. 1851, pp. 17–18.

On the power of Icebergs to make rectilinear, uniformly-directed Grooves across a Submarine Undulatory Surface. Phil. Mag. x. 1855, pp. 96–98.

Vitality of Seeds. *Gardeners' Chronicle*, Nov. 17, 1855, p. 758.

On the action of Sea-water on the Germination of Seeds. [1856.] Linn. Soc. Journ. i. 1857 (*Botany*), pp. 130–140.

On the Agency of Bees in the Fertilisation of Papilionaceous Flowers. *Gardeners' Chronicle*, p. 725, 1857.

On the Tendency of Species to form Varieties; and on the Perpetuation of Varieties and Species by Natural Means of Selection. By Charles Darwin, Esq., F.R.S., F.L.S., and F.G.S., and Alfred Wallace, Esq. [Read July 1st, 1858.] Journ. Linn. Soc. 1859, vol. iii. (*Zoology*), p. 45.

Special titles of C. Darwin's contributions to the foregoing:— (i) Extract from an unpublished work on Species by C. Darwin, Esq., consisting of a portion of a chapter entitled, "On the Variation of Organic Beings in a State of Nature; on the Natural Means of Selection; on the Comparison of Domestic Races and true Species." (ii) Abstract of a Letter from C. Darwin, Esq., to Professor Asa Gray, of Boston, U.S., dated Sept. 5, 1857.

On the Agency of Bees in the Fertilization of Papilionaceous Flowers, and on the Crossing of Kidney Beans. *Gardeners' Chronicle*, 1858, p. 828 and Ann. Nat. Hist. 3rd series ii. 1858, pp. 459–465.

Do the Tineina or other small Moths suck Flowers, and if so what Flowers? *Entom. Weekly Intell.* vol. viii. 1860, p. 103.

Note on the achenia of *Pumilio Argyrolepis*. *Gardeners' Chronicle*, Jan. 5, 1861, p. 4.

Fertilisation of Vincas. *Gardeners' Chronicle*, pp. 552, 831, 832. 1861.

On the Two Forms, or Dimorphic Condition, in the species of

© The Complete Work of Charles Darwin Online

Primula, and on their remarkable Sexual Relations. Linn. Soc. Journ. vi. 1862 (*Botany*), pp. 77–96.

On the Three remarkable Sexual Forms of *Catasetum tridentatum*, an Orchid in the possession of the Linnean Society. Linn. Soc. Journ. vi. 1862 (*Botany*), pp. 151–157.

Yellow Rain. *Gardeners' Chronicle*, July 18, 1863, p. 675.

On the thickness of the Pampean formation near Buenos Ayres. Geol. Soc. Journ. xix. 1863, pp. 68–71.

On the so-called "Auditory-sac" of Cirripedes. Nat. Hist. Review, 1863, pp. 115–116.

A review of Mr. Bates' paper on 'Mimetic Butterflies.' Nat. Hist. Review, 1863, p. 221—. [Not signed.]

On the existence of two forms, and on their reciprocal sexual relation, in several species of the genus *Linum*. Linn. Soc. Journ. vii. 1864 (*Botany*), pp. 69–83.

On the Sexual Relations of the Three Forms of *Lythrum salicaria*. [1864.] Linn. Soc. Journ. viii. 1865 (*Botany*), pp. 169–196.

On the Movement and Habits of Climbing Plants. [1865.] Linn. Soc. Journ. ix. 1867 (*Botany*), pp. 1–118.

Note on the Common Broom (*Cytisus scoparius*). [1866.] Linn. Soc. Journ. ix. 1867 (*Botany*), p. 358.

Notes on the Fertilization of Orchids. Ann. and Mag. Nat. Hist. 4th series, iv. 1869, pp. 141–159.

On the Character and Hybrid-like Nature of the Offspring from the Illegitimate Unions of Dimorphic and Trimorphic Plants. [1868.] Linn. Soc. Jour. x. 1869 (*Botany*), pp. 393–437.

On the Specific Difference between *Primula veris*, Brit. Fl. (var. *officinalis*, of Linn.), *P. vulgaris*, Brit. Fl. (var. *acaulis*, Linn.), and *P. elatior*, Jacq.; and on the Hybrid Nature of the common Oxlip. With Supplementary Remarks on naturally-produced Hybrids in the genus *Verbascum*. [1868.] Linn. Soc. Journ. x. 1869 (*Botany*), pp. 437–454.

Note on the Habits of the Pampas Woodpecker (*Colaptes campestris*). Zool. Soc. Proc. Nov. 1, 1870, pp. 705–706.

Fertilisation of *Leschenaultia*. *Gardeners' Chronicle*, p. 1166, 1871.

The Fertilisation of Winter-flowering Plants. 'Nature,' Nov. 18, 1869, vol. i. p. 85.

Pangenesis. 'Nature,' April 27, 1871, vol. iii. p. 502.

A new view of Darwinism. 'Nature,' July 6, 1871, vol. iv. p. 180.

Bree on Darwinism. 'Nature,' Aug. 8, 1872, vol. vi. p. 279.

© The Complete Work of Charles Darwin Online

Inherited Instinct. 'Nature,' Feb. 13, 1873, vol. vii. p. 281.

Perception in the Lower Animals. 'Nature,' March 13, 1873, vol. vii. p. 360.

Origin of certain instincts. 'Nature,' April 3, 1873, vol. vii. p. 417.

Habits of Ants. 'Nature,' July 24, 1873, vol. viii. p. 244.

On the Males and Complemental Males of Certain Cirripedes, and on Rudimentary Structures. 'Nature,' Sept. 25, 1873, vol. viii. p. 431.

Recent researches on Termites and Honey-bees. 'Nature,' Feb. 19, 1874, vol. ix. p. 308.

Fertilisation of the Fumariaceæ. 'Nature,' April 16, 1874, vol. ix. p. 460.

Flowers of the Primrose destroyed by Birds. 'Nature,' April 23, 1874, vol. ix. p. 482; May 14, 1874, vol. x. p. 24.

Cherry Blossoms. 'Nature,' May 11, 1876, vol. xiv. p. 28.

Sexual Selection in relation to Monkeys. 'Nature,' Nov. 2, 1876, vol. xv. p. 18.

Fritz Müller on Flowers and Insects. 'Nature,' Nov. 29, 1877, vol. xvii. p. 78.

The Scarcity of Holly Berries and Bees. *Gardeners' Chronicle*, Jan. 20, 1877, p. 83.

Note on Fertilisation of Plants. *Gardeners' Chronicle*, vol. vii. p. 246, 1877.

A biographical sketch of an infant. 'Mind,' No. 7, July, 1877.

Transplantation of Shells. 'Nature,' May 30, 1878, vol. xviii. p. 120.

Fritz Müller on a Frog having Eggs on its back—on the abortion of the hairs on the legs of certain Caddis-Flies, &c. 'Nature,' March 20, 1879, vol. xix. p. 462.

Rats and Water-Casks. 'Nature,' March 27, 1879, vol. xix. p. 481.

Fertility of Hybrids from the common and Chinese Goose. 'Nature,' Jan. 1, 1880, vol. xxi. p. 207.

The Sexual Colours of certain Butterflies. 'Nature,' Jan. 8, 1880, vol. xxi. p. 237.

The Omori Shell Mounds. 'Nature,' April 15, 1880, vol. xxi. p. 561.

Sir Wyville Thomson and Natural Selection. 'Nature,' Nov. 11, 1880, vol. xxiii. p. 32.

Black Sheep. 'Nature,' Dec. 30, 1880, vol. xxiii. p. 193.

Movements of Plants. 'Nature,' March 3, 1881, vol. xxiii. p. 409.

© The Complete Work of Charles Darwin Online

The Movements of Leaves. 'Nature,' April 28, 1881, vol. xxiii. p. 603.

Inheritance. 'Nature,' July 21, 1881, vol. xxiv. p. 257.

Leaves injured at Night by Free Radiation. 'Nature,' Sept. 15, 1881, vol. xxiv. p. 459.

The Parasitic Habits of Molothrus. 'Nature,' Nov. 17, 1881, vol. xxv. p. 51.

On the Dispersal of Freshwater Bivalves. 'Nature,' April 6, 1882, vol. xxv. p. 529.

The Action of Carbonate of Ammonia on the Roots of certain Plants. [Read March 16, 1882.] Linn. Soc. Journ. (*Botany*), vol. xix. 1882, pp. 239–261.

The Action of Carbonate of Ammonia on Chlorophyll-bodies. [Read March 6, 1882.] Linn. Soc. Journ. (*Botany*), vol. xix. 1882, pp. 262–284.

On the Modification of a Race of Syrian Street-Dogs by means of Sexual Selection. By W. Van Dyck. With a preliminary notice by Charles Darwin. [Read April 18, 1882.] Proc. Zoolog. Soc. 1882, pp. 367–370.

© The Complete Work of Charles Darwin Online

APPENDIX III.

PORTRAITS.

Date.	Description.	Artist.	In the Possession of
1838	Water-colour .	G. Richmond .	The Family.
1851	Lithograph . .	Ipswich British Assn. Series.	
1853	Chalk Drawing .	Samuel Lawrence	The Family.
1853?	Chalk Drawing *	Samuel Lawrence	Prof. Hughes, Cambridge.
1869	Bust, marble .	T. Woolner, R.A.	The Family.
1875	Oil Painting † . Etched by	W. Ouless, R.A. P. Rajon.	The Family.
1879	Oil Painting .	W. B. Richmond	The University of Cambridge.
1881	Oil Painting ‡ .	Hon. John Collier	The Linnean Society.
	Etched by	Leopold Flameng	

CHIEF PORTRAITS AND MEMORIALS NOT TAKEN FROM LIFE

Statue . . .	Joseph Boehm, R.A.	Museum, South Kensington.
Bust . . .	Chr. Lehr, Junr.	
Plaque . . .	T. Woolner, R.A., and Josiah Wedgwood and Sons.	Christ's College, in Charles Darwin's Room.
Deep Medallion	J. Boehm, R.A.	To be placed in Westminster Abbey.

* Probably a sketch made at one of the sittings for the last-mentioned.

† A *replica* by the artist is in the possession of Christ's College, Cambridge.

‡ A *replica* by the artist is in the possession of W. E. Darwin, Esq., Southampton.

© The Complete Work of Charles Darwin Online

CHIEF ENGRAVINGS FROM PHOTOGRAPHS.

*1854? By Messrs. Maull and Fox, engraved on wood for 'Harper's Magazine' (Oct. 1884). Frontispiece, vol. i.

*1870? By O. J. Rejlander, engraved on steel by C. H. Jeens for 'Nature' (June 4, 1874).

*1874? By Capt. Darwin, R.E., engraved on wood for the 'Century Magazine' (Jan. 1883). Frontispiece, vol. ii.

1881 By Messrs. Elliott and Fry, engraved on wood by G. Kruells, for vol. iii. of the present work.

* The dates of these photographs must, from various causes, remain uncertain. Owing to a loss of books by fire, Messrs. Maull and Fox can give only an approximate date. Mr. Rejlander died some years ago, and his business was broken up. My brother, Captain Darwin, has no record of the date at which his photograph was taken.

© The Complete Work of Charles Darwin Online

APPENDIX IV.*

HONOURS, DEGREES, SOCIETIES, &c.

Order.—Prussian Order, 'Pour le Mérite.' 1867.
Office.—County Magistrate. 1857.
Degrees.—Cambridge { B.A. 1831 [1832].†
{ M.A. 1837.
Hon. LL.D. 1877.
Bonn . . Hon. Doctor in Medicine and Surgery. 1868.
Breslau . Hon. Doctor in Medicine and Surgery. 1862.
Leyden . Hon. M.D. 1875.
Societies.—London . Zoological. Corresp. Member. 1831.‡
Entomological. 1833, Orig. Member.
Geological. 1836. Wollaston Medal, 1859.
Royal Geographical. 1838.
Royal. 1839. Royal Society's Medal, 1853
Copley Medal, 1864.
Linnean. 1854.
Ethnological. 1861.
Medico-Chirurgical. Hon. Member. 1868.
Baly Medal of the Royal College of Physicians, 1879.

Societies.—PROVINCIAL, COLONIAL AND INDIAN.

Royal Society of Edinburgh, 1865.
Royal Medical Society of Edinburgh, 1826. Hon. Member, 1861.
Royal Irish Academy. Hon. Member, 1866.

* The list has been compiled from the diplomas and letters in my father's possession, and is no doubt incomplete, as he seems to have lost or mislaid some of the papers received from foreign Societies. Where the name of a foreign Society (excluding those in the United States) is given in English, it is a translation of the Latin (or in one case Russian) of the original Diploma.

† See vol. i. p. 163.

‡ He afterwards became a Fellow of the Society.

© The Complete Work of Charles Darwin Online

Literary and Philosophical Society of Manchester. Hon. Member, 1868.
Watford Nat. Hist. Society. Hon. Member, 1877.
Asiatic Society of Bengal. Hon. Member, 1871.
Royal Society of New South Wales. Hon. Member, 1879.
Philosophical Institute of Canterbury, New Zealand. Hon. Member, 1863.
New Zealand Institute. Hon. Member, 1872.

Foreign Societies.

AMERICA.

Sociedad Científica Argentina. Hon. Member, 1877.
Academia Nacional de Ciencias, Argentine Republic. Hon. Member, 1878.
Sociedad Zoolójica Arjentina. Hon. Member, 1874.
Boston Society of Natural History. Hon. Member, 1873.
American Academy of Arts and Sciences (Boston). Foreign Hon. Member, 1874.
California Academy of Sciences. Hon. Member, 1872.
California State Geological Society. Corresp. Member, 1877.
Franklin Literary Society, Indiana. Hon. Member, 1878.
Sociedad de Naturalistas Neo-Granadinos. Hon. Member, 1860.
New York Academy of Sciences. Hon. Member, 1879.
Gabinete Portuguez de Leitura em Pernambuco. Corresp. Member, 1879.
Academy of Natural Sciences of Philadelphia. Correspondent, 1860.
American Philosophical Society, Philadelphia. Member, 1869.

AUSTRIA-HUNGARY.

Imperial Academy of Sciences of Vienna. Foreign Corresponding Member, 1871; Hon. Foreign Member, 1875.
Anthropologische Gesellschaft in Wien. Hon. Member, 1872.
K. k. Zoologische botanische Gesellschaft in Wien. Member, 1867.
Magyar Tudományos Akadémia, Pest, 1872.

BELGIUM.

Société Royale des Sciences Médicales et Naturelles de Bruxelles. Hon. Member, 1878.
Société Royale de Botanique de Belgique. 'Membre Associé,' 1881

© The Complete Work of Charles Darwin Online

Académie Royale des Sciences, &c., de Belgique. 'Associé de la Classe des Sciences.' 1870.

Denmark.

Royal Society of Copenhagen. Fellow, 1879.

France.

Société d'Anthropologie de Paris. Foreign Member, 1871.
Société Entomologique de France. Hon. Member, 1874.
Société Géologique de France. Life Member, 1837.
Institut de France. 'Correspondant' Section of Botany, 1878.

Germany.

Royal Prussian Academy of Sciences (Berlin). Corresponding Member, 1863; Fellow, 1878.
Berliner Gesellschaft für Anthropologie, &c. Corresponding Member, 1877.
Schlesische Gesellschaft für Vaterländische Cultur (Breslau). Hon. Member, 1878.
Cæsarea Leopoldino-Carolina Academia Naturæ Curiosorum (Dresden).* 1857.
Senkenbergische Naturforschende Gesellschaft zu Frankfurt am Main. Corresponding Member, 1873.
Naturforschende Gesellschaft zu Halle. Member, 1879.
Siebenbürgische Verein für Naturwissenschaften (Hermannstadt). Hon. Member, 1877.
Medicinisch-naturwissenschaftliche Gesellschaft zu Jena. Hon. Member, 1878.
Royal Bavarian Academy of Literature and Science (Munich). Foreign Member, 1878.

Holland.

Koninklijke Natuurkundige Vereeniging in Nederlandsch-Indie (Batavia). Corresponding Member, 1880.

* The diploma contains the words "accipe . . . ex antiqua nostra consuetudine cognomen Forster." It was formerly the custom in the *Cæsarea Leopoldino-Carolina Academia*, that each new member should receive as a 'cognomen,' a name celebrated in that branch of science to which he belonged. Thus a physician might be christened Boerhaave, or an astronomer, Kepler. My father seems to have been named after the traveller John Reinhold Forster.

© The Complete Work of Charles Darwin Online

Société Hollandaise des Sciences à Harlem. Foreign Member, 1877.

Zeeuwsch Genootschap der Wetenschappen te Middelburg. Foreign Member, 1877.

ITALY.

Società Geografica Italiana (Florence). 1870.

Società Italiana di Antropologia e di Etnologia (Florence). Hon. Member, 1872.

Società dei Naturalisti in Modena. Hon. Member, 1875.

Academia de' Lincei di Roma. Foreign Member, 1875.

La Scuola Italica, Academia Pitagorica, Reale ed Imp. Società (Rome). 'Presidente Onorario degli Anziani Pitagorici,' 1880.

Royal Academy of Turin. 1873. *Bressa* Prize, 1879.

PORTUGAL.

Sociedade de Geographia de Lisboa (Lisbon). Corresponding Member, 1877.

RUSSIA.

Society of Naturalists of the Imperial Kazan University. Hon. Member, 1875.

Societas Cæsarea Naturæ Curiosorum (Moscow). Hon. Member, 1870.

Imperial Academy of Sciences (St. Petersburg). Corresponding Member, 1867.

SPAIN.

Institucion Libre de Enseñanza (Madrid). Hon. Professor, 1877.

SWEDEN.

Royal Swedish Acad. of Sciences (Stockholm). Foreign Member, 1865.

Royal Society of Sciences (Upsala). Fellow, 1860.

SWITZERLAND.

Société des Sciences Naturelles du Neufchâtel. Corresponding Member, 1863.

© The Complete Work of Charles Darwin Online

INDEX.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

LOGICAL DEPT. CA

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

ORTHOPTERA.

PENGELLY.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

LONDON: PRINTED BY WILLIAM CLOWES AND SONS, LIMITED, STAMFORD STREET AND CHARING CROSS.

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online

University of Cambridge
DEPARTMENT OF ZOOLOGY
Balfour & Newton Libraries

© The Complete Work of Charles Darwin Online

© The Complete Work of Charles Darwin Online