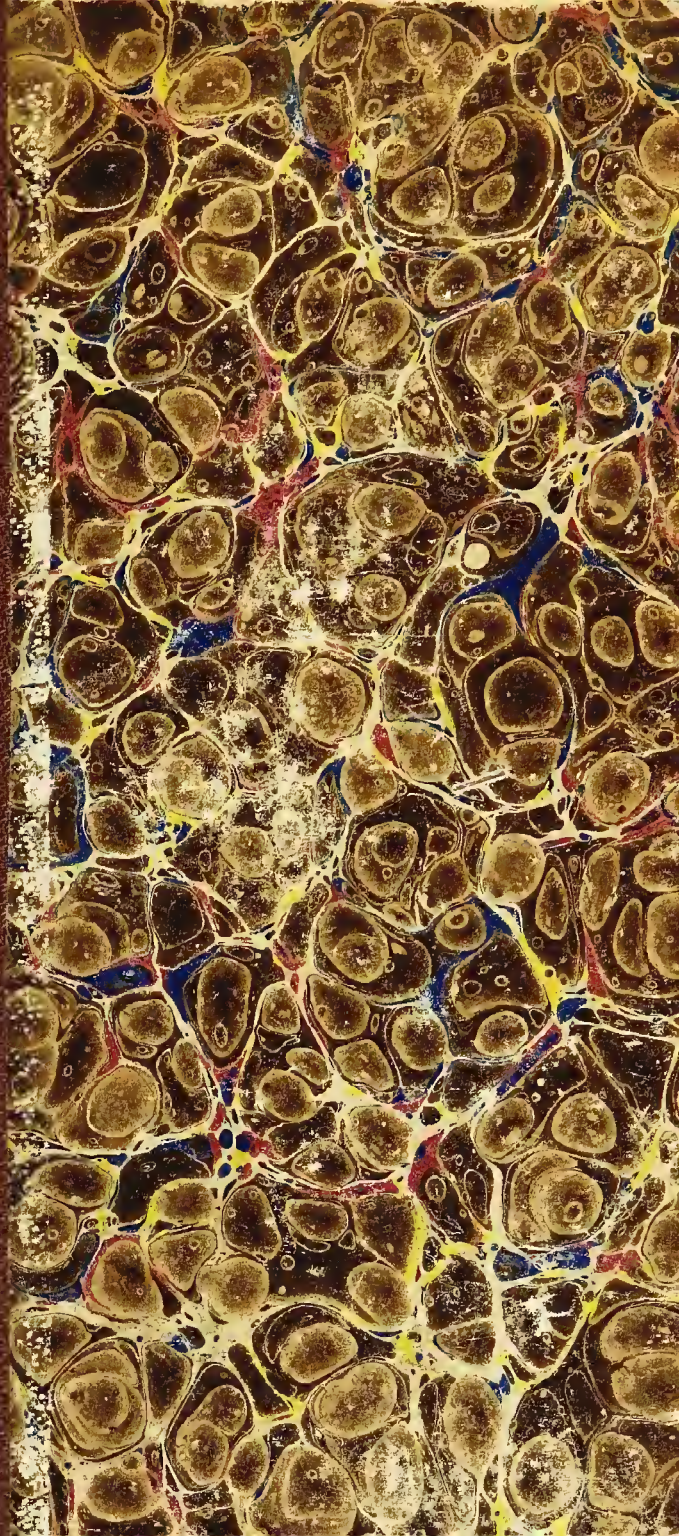


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MAX MÜLLER

AND THE

SCIENCE OF LANGUAGE

MAX MÜLLER

AND THE

Science of Language:

A CRITICISM,

BY

WILLIAM DWIGHT WHITNEY,

Professor in Yale University.

NEW YORK :

D. APPLETON AND COMPANY,

1892.

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PREFATORY NOTE.

IN preparing this criticism of the last revised edition of the work which is its subject, the writer has not refrained from discussing anew matters already treated by him in criticisms of the original editions of the same work. Such may be found in the *North American Review*, vol. c. (April, 1865), p. 565 ff.; and vol. cxiii. (October, 1871), p. 430 ff.; also in two or three other places referred to in foot-notes below.

YALE UNIVERSITY,
NEW HAVEN, CONN., Feb., 1892.

MAX MÜLLER AND THE SCIENCE OF LANGUAGE.

THERE has appeared recently a new and revised edition of Müller's well-known work on Language,* followed by the usual array of commendatory notices. Hence it may well be submitted once more to a critical examination, in order to the determination of its true place in the past history and present apparatus of the science. For many it has been their first introduction to linguistic study; and doubtless to a large proportion of English-speaking readers, especially, it is still the principal and most authoritative text-book of that study, as regards both methods and results. A work holding such a position calls for careful criticism, that it may not be trusted where it is untrustworthy, and so do harm to the science which it was intended to help.

THE REVISION OF THE WORK.

The revision undergone by the volumes has been indeed no merely nominal one. They have been changed from lectures into a series of chapters, with effacement of the more personal cast belonging to the lecture-form. Very numerous additions have been made, of every extent, from a line or two, or a brief note, to passages covering many pages; on the other

* The Science of Language, founded on lectures delivered at the Royal Institution in 1861 and 1863, by F. Max Müller. . . In two volumes, 12mo. pp. xlv, 582; viii, 744. New York: Charles Scribner's Sons. 1891.

hand, though in far from corresponding measure, there are not a few omissions ; passages have been recast, with more or less alteration, consisting chiefly in expansion ; and the arrangement of material has been sometimes changed, by transfer from one place to another. Enough has been done, certainly, to make the work in its present shape an authoritative record of its author's latest and most matured opinions.

And yet, if any one looks here for any serious modification of what had been already given, he will find himself disappointed. In all essential respects, the chapters are still what the lectures were. The additions, for example, are mostly of material quite accordant in kind with what was there before. Among them we may note, as of conspicuous extent and importance, a protracted account (i. 163-195) of the history of the Sanskrit language and of European acquaintance with it : a matter having, to be sure, no more than an indirect bearing upon the science of language, yet of like character with much that was in the volumes before, and in itself interesting enough ; a story (i. 303-306) of the life and labors of the noted philologist Pott, of which the same may be said ; a succinct description (ii. 113-125) of Melville Bell's analysis of the spoken alphabet, very much in place ; a revised and altered account (ii. 251-281), with considerable additions, of the phenomena comprehended under the name of Grimm's Law ; in a note (ii. 507-511), a long letter from Mr. Gladstone, explaining and defending his view of the relation of Greek mythology to Hebrew traditions ; a quantity of

additional material (ii. 670-674) respecting the superstitions connected with *barnacle*—and so on, and so on. The recastings and expansions are also for the most part of the same general nature: at the most, they concern matters of detail in the comparative treatment of languages (one or two of these we shall have occasion to notice later); with the great questions which underlie the study, and of which the science of language is made up, they have next to nothing to do. As to these questions, and their mode of discussion, our author's views seem to remain what they were thirty years ago; he has gained no new light upon them from the criticisms that have been made upon his work, nor from studying the discordant views of others, nor from his own review and comparison and coördination of his old opinions. And this, although a lack of cohesion and of system has been conspicuous among the faults found with his presentation of his subject. It might seem as if, having written with such popular effusion and miscellaneousness about so many matters bearing upon language, he would have seized gladly the opportunity of a final revision to ponder and recombine it all, establishing in his own mind a certain connected and clearly held body of opinions which should yield one another mutual support. An improved consistency, even at the cost of some attractive disquisition, might well have been one of the objects aimed at. Of course, it was for him to decide what he would and what he would not undertake to do; it is not for his readers to prescribe the scope and limits of his task. But it is ours, now that the work is done, to criticise it; to ex-

amine and report whether his achievement is worthy of his reputation ; whether he has duly felt and responded to the responsibility resting on one whom so many have taken for their guide, and almost for their prophet.

To this end, we may begin by taking up and analyzing a passage in the second volume (ii. 84-85), which will lead us as well as any other into the very heart of our author's philosophy, and method of discussing linguistic questions. It is found in his chapter on "Language and Reason," and has for its special object to justify him in "dissecting the dead body of language"—that is, in giving an account of the spoken alphabet and of the processes of utterance ; and it is headed—

"THE SOUND OF WORDS HAS NO INDEPENDENT EXISTENCE."

The author's intention is, as he states at the beginning, "to show clearly that reason cannot become real without speech ;" and this, strangely enough, is to be accomplished by proving that "articulate sounds, or what we may call the body of language, exist nowhere, have no independent reality." And this, in its turn, is to be inferred from the facts that we say *experiment* (of which the etymology is given in all detail), and not *esperiment* or *exporiment* or the like, and that English speakers say *châraacter*, with accent on the first syllable, while Germans say *charákker*, on the second, and Frenchmen *caractère*, on the third. But the logical coherence of this "less metaphysical but more convincing" argument, as he calls it, is as obscure even as any metaphysician could demand.

“*Chárákter*,” it is added in explanation, “has a meaning in English, but none in German or French,” and so with the other two forms respectively; and “articulate sound without meaning is even more unreal than inarticulate sound.” That is to say, *experiment* and *esperiment*, and, in English, *charákteer* and *caractère*, do not exist; and that proves the “body of language” to “have no independent existence”! In plainer English, there is not to be found anywhere in existence (i. e. in use, for existence in such matters is use, and, as our author himself teaches, use gives existence to even *charákteer* and *caractère* in other languages) a body of words or word-like combinations of articulate sounds apart from those which, endowed with meanings, form a part of one or another language. And (the argument goes on), if not now, then there never can have been found any such; and hence, since they did not exist, our conceptions could never have gotten them from anywhere; and consequently our conceptions, which really, as we must confess, now have words representing them, must have been born in combination with these words, and so could not have existed without them! His own way of putting the argument is this: “I think it follows that this so-called body of language [i. e. uttered expression] could never have been taken up anywhere by itself, and added to our conceptions from without; from which it would follow again that our conceptions, which are now always clothed in the garment of language, could never have existed in a naked state.” He adds complacently: “this would be perfectly correct

reasoning, if applied to anything else ; nor do I see that it can be objected to as bearing on thought and language." Is that so? let us try it on "garments" of another class. In the community X is worn a dress of one particular fashion, in Y of another, in Z of yet another. X's dress is non-existent in Y or Z ; no one would put it on there ; and a dress without a person in it is even more unreal than uncut cloth. If, then, these garments exist nowhere laid up for possible future use (this part of the comparison, it must be confessed, does not fit so well in these days of great clothing-warehouses as in the good old time, when every man was measured for his own coat : and yet, after all, what but such warehouses are the Greek and Latin dictionaries, in which men of science, from metaphysicians to bacteriologists, go and rummage when they wish to get their new conceptions so clad as to be presentable in society ?), it follows that they cannot have been brought from any where and put upon the people that wear them ; and hence, again, that people were born with them on, and could never have existed in a naked state. And we may add in our turn that, with a true view of what expression is to thought, this argument is no mere joke, but precisely as good as our author's. To clinch the latter, he appends a pair of illustrations : "If we never find skins except as the teguments of animals, we may safely conclude that animals cannot exist without skins [so all the earlier editions read, as the connection demands]. If color cannot exist by itself, it follows that neither can anything that is colored exist without color." Now of the second illus-

tration the conclusion is unquestionable, as much so as that nothing that is shaped can exist without shape, nor anything that is numbered without number. It does not, however, follow from the premise stated, but from this very different premise : that nothing is found by us to exist, or conceivable as existing, without (in the light) possessing color, as without shape or number. Our author's argument, as he uses it, would have been just as good in this form : if the color red cannot exist by itself, it follows that nothing can exist without being red. That this is a *non-sequitur* would probably be perceptible even to him ; but it is not really more plainly so than his other illustration concerning skins, the quality of which he might have tested by varying it thus : if we never find horns and tails except as the appendages of animals, we may safely conclude that animals cannot exist without horns and tails. In fact, this test of the quality of the reasoning involved was applied to it, some time since ;* and, for once, he appears to have seen the point, and been led, not to omit the unfortunate sentence altogether, but to change slightly its concluding words, which in this finally revised text read " we may safely conclude that skins cannot exist without animals." This is a very harmless statement ; though " if we found " and " we might conclude " would be yet safer, because it would probably puzzle our author to show that apples and potatoes, for example, are not as properly said to have " skins " as animals. But the alteration, though it has spoiled the sentence as a

* North American Review, vol. cxiii., Oct., 1871, pp. 439-40.

quotation for the chapter on fallacies in a treatise of logic, has at the same time destroyed its value as a part of the author's argument; it goes now to show only that words cannot exist without conceptions, not that conceptions cannot exist without words, and is quite out of connection with what precedes and follows. It is not possible to support the doctrine that conceptions and words are one and indivisible by pointing out that the skins of animals cannot exist without the animals to which they belong.

In some respects it is unfair to begin our review with a discussion of this passage, because it is perhaps (there are others that run it hard) the weakest in the two volumes. It is more than weak; it is inane (what a German would call *albern*). But it is one of the passages to which, in a finally and thoroughly revised edition, one who knows the work cannot help first turning, in order to see what the quality of the revision has been. Moreover, it is not in its character an exceptional case, but rather the acme of the author's method; this is the kind of ratiocination by which, on no small scale, he arrives at untenable conclusions from misunderstood or half-understood premises. Yet again, it concerns a matter, the relation of expression to thought, which is of the most central and fundamental importance in linguistic philosophy, respecting which if any one goes wrong he is liable to fail at every point. The false views here involved ramify very widely through our author's whole work, and it may be well for us to go on and follow them out as they show themselves elsewhere in his reasonings.

WHAT DOES "CONVENTIONAL," MEAN?

We may note first his assumption, clearly made in these pages, that, if words and ideas (we may well enough use these terms in their popular sense, instead of seeking others more exact) were not confessed to be identical, or at least eternally joined and inseparable, we should have to believe that an array of articulate signs was produced in advance, and then by deliberate and formal agreement added to or imposed upon a like array of ideas that had been waiting for signs. That is our author's view of what is involved in the doctrine that the signs which make up language are "conventional." Let us see how he expresses himself in regard to it throughout his two volumes.

At i. 29, speaking of certain erring modern philosophers, he describes them as holding "that the varieties of human speech arose from different nations agreeing on different sounds as the most appropriate signs of their different ideas;" and so it is not difficult for him to point out, a little later (i. 32), that "no one has yet explained how, without language, a discussion, however imperfect, on the merits of each word, such as must needs have preceded a mutual agreement, could have been carried on." Again, at i. 94: "when, as we are told by some persons, the first men, as yet speechless, came together in order to invent speech, and to discuss the most appropriate names that should be given to the perceptions of the senses and the abstractions of the mind." Yet again, at i. 329, the idea is repudiated of "a congress for settling the proper exponents of such relations as

nominative, genitive," etc. Once more, at ii. 432, he asserts that Locke "never questions for a moment the received theory that at some time or other in the history of the world men had accumulated a treasure of anonymous general conceptions, to which, when the time of intellectual and social intercourse had arrived, they skilfully attached those phonetic labels which we call words." And so in other passages which might be quoted, and to which nothing of a different character could be opposed; for though in general, where he formally adopts a wrong view of anything, our author yet shows here and there glimpses of the right view, with regard to this he is consistent from one end to the other of his work; "conventional" is everywhere regarded as necessarily implying a convention of people, gathered to discuss and decide on the words and forms by which conceptions should be represented.

Now it may be extremely convenient to dispose in this manner of a doctrine opposed to one's own; but what are we to think of the character of a work which seriously puts forward such an absurd caricature, and thinks to win by rebutting it? Did our author really believe that to be the meaning of the word "conventional" as used by any human being in reference to language? It would be more credible provided the term were so used in reference to anything else; but can he bring up a single connection in which that is the case? It is "conventional" with us to bow or shake hands on meeting a friend, instead of rubbing noses with him, or patting the pit of one's own stomach: was that settled at a "convention" of the

forefathers of our division of the race? It is "conventional" to wear a dress-coat, with other articles of clothing to match, at an evening entertainment; but he who transgressed the rule would not be excused on the plea that he could find no record of the vote by which it was carried in the original "convention." It has been, and is still in some measure, "conventional" to write "your obedient servant" at the end of a letter: when and where was the gathering of epistolographers that decided it? And so on, through the whole range of acts and habits called "conventional;" the word everywhere signifies neither more nor less than 'resting on a mutual understanding or a community of habit.' As applied to any word constituting a part of language, it means that that word, instead of being bound to its sense by an internal and necessary tie, is so only by an external one, a tie of mutual understanding and common usage, formed by acquired habit on the part of every user.

And this is the most fundamental fact in the study of language; its recognition would sweep away a great part of the difficulties in which our author involves himself throughout his linguistic investigations. It is also the most obvious fact, if one will but take up the study at the right end. Thus, for example, for the idea 'one' (which we may select as an example of a conception of the simplest and most absolute kind) there are as many different names as there are languages in the world (and these are innumerable): there is our *one* (i. e. *wun*), Germ. *ein*, Fr. *un*, Ital. *uno*, Gr. *eis*, Skt. *eka*. Each of these names answers its purpose precisely as well as any of the others, and

only a fool—or else an ignorant who did not know that any of them save his own was in existence—would think of maintaining that one or another was the real sign for the conception 'one,' the rest being shams or blunders. Moreover, no living being ever comes to use any one of the series except as he learns it from others. There was a time when our author, great and learned man as he now is, was taken on some one's knee and taught, not without difficulty, to say *eins, zwei, drei*, and so on, and to associate these signs with conceptions of number which his possession of human faculties gave him the ability to frame ; since that time he has learned many different but equivalent series, either from living teachers, or from books, or from both sources combined. It was within the reach, probably, of his present memory, and by a conscious effort, that he learned *one, two, three*, and yet more *eka, dva, tri* ; and it is only because the acquisition of the first series lies beyond his recollection that he apparently thinks it born into his mind along with the first and simplest distinctions of number, while confessing that the English and Sanskrit names were in fact obtained by an external process. If this is his opinion, it rests upon him to explain why all persons who develop the beginnings of speech in a community that says *eins, zwei, drei* develop this particular set of names and no other—and so with *one, two, three*, with *un, deux, trois*, and all the rest.

For that it does not depend in any measure upon grade of intellect, or upon bent of character, or upon endowment of soul, is too obvious to be questioned ; all in the one community, fools and wise, tender and

truculent, imaginative and prosaic, grow up to say *eins, zwei, drei*; all in the other, of like diversity of gifts, grow up to say *one, two, three*. Equally is it independent of race or blood, as is shown by the most numerous and varied proofs. Individuals, classes, races, especially in our modern mixed communities, speak languages which were strange to their ancestors. The children of Germans who go to live in England, as our author perhaps knows by experience, are apt to learn the English signs first, and if their blood is mixed, by having parents of two races, it does not in the least degree tend to give them that (according to him) impossibility, a mixed language.

If, instead of these simple notions of number (which, if any such thing were possible, would seem most likely of all to generate corresponding incorporations in speech), we took a highly intricate notion, the product of a somewhat advanced civilization, such as 'city,' we should find the same thing true: *city, stadt, ville, urbs, polis, nagara*, are only specimens (all from one family of related languages) of the utter diversity of nomenclature that would present itself: as many different names as there are human tongues, all equally good for their purpose, none possessed by a single individual that has not learned it, each used effectively, to mutual understanding, between persons who, having learned it, know in what sense it should be used. And so in all classes of words, and in all the words of each class.

Hence it follows that every language known to us is a body of conventional signs for ideas, which has to be learned by those whose purposes it is to serve,

which can be acquired and used by any human being, and which avails in intercourse among those who, having alike acquired it, can understand it from one another's lips and hands.

It ought not to be necessary to make such an elementary exposition as this; but unfortunately it is necessary, and in the highest degree, and it cannot be made too distinctly and impressively. The truths here brought out are even expressly denied by a few; they are misunderstood and misrepresented and ignored by many more (as by our author); and they are left out of sight in the linguistic reasonings of the majority. But they are the most fundamental and indispensable principles of linguistic science; fruitful investigation in that science is possible only in proportion as they are seen and acknowledged and laid at the basis of reasoning.

LINGUISTIC PHILOSOPHY OF ARTEMUS WARD AND HIS SCHOOL.

It is reported of Artemus Ward (on how good authority need not concern us) that he said it was all natural enough that people should have learned something about the size and distance and motions of the planets, but that what puzzled him entirely was how they ever found out their names. There are a plenty of scholars besides our author who are unconscious adherents in this matter of the Wardian philosophy, holding that things possess by nature names of their own which men have to find out, instead of having names given them by men. It is the old and famous question of *φύσει* or *θέσει*, and Artemus and his followers say *φύσει*.

MISPRONUNCIATION DOES NOT ANNIHILATE A WORD.

Even a small degree of appreciation of the true character of a word would have saved our author from the assertion made by him in the passage we began with examining (ii. 84), that, if of a given word we "change only the accent," we get what "is nothing; change one vowel or one consonant . . . and we have mere noises, what Heraclitus would call a mere *psophos*, but no words." This is not only wrong, but ludicrously wrong; it flies directly in the face of facts without number with which every one is familiar in every-day life. Some people, even among the best classes of speakers, say *álly* instead of *ally*, *haráss* instead of *harass*, without failing to be understood; no one is conscious of a blank, or a Heraclitean *psophos*, where words should have stood in their sentences. Many such people say *nefew* instead of *newew*, *eksert* instead of *egzert* (spelt *nephew*, *exert*), with a similar result. Men of more or less deficient education are all the time committing so-called errors of pronunciation, or even of significance, which do not prevent their sharing in the general benefits of language. Dialectic varieties of speech, of varying kind and degree—Scotch, Lancashire, Yankee, Southern negro, and so on—are yet, with more or less difficulty, intelligible to us, however irksome we may find them in the measure in which they are nowadays thrust before us in literature. The efforts of most young children to reproduce the signs which they are learning to associate with their conceptions are attended with a dreadful slaughter of accents and consonants and vowels, but no one thinks of calling their utterance "mere

noises." All these things are as familiar to our author as to everybody else ; he simply loses sight of them in the heat of his great argument to prove " that reason cannot become real without speech : " what have the humble facts of the every-day life of language to do with so grand and far-reaching a conclusion ? If he knew that words are only the signs of ideas, it would be plain to him that such signs, if seriously used with the intent to communicate, will bear a great deal of disguise before they lose their intelligibility, and that their intelligibility is their existence. That difference of *châra*cter and *charâ*cter and *caractère*, which now he deems so portentous, would appear to him not less insignificant than it does to the enlightened readers of his book ; it is nothing more than the varying habit of pronunciation by different members of the great European community of a sign obtained by them all from a common source for a certain common conception ; it has no more importance than the difference of fashion in the cut of a coat.

LISTS OF WORDS AND LISTS OF IDEAS WAITING FOR UNION.

Another thing that would become clear to our author is the true reason why there is not, and never was, even in the imaginations of those to whom he gratuitously ascribes the doctrine, a scheme of signs and a scheme of conceptions waiting to be matched with one another. Whenever a conception takes such distinctness of form as to call for expression, expression is at once found for it ; that is the simple way in which language grows now, and has grown ever since

we have known anything of it. Our author himself, when he thought he had recognized a force, or a combination of forces, in the development of language, which had escaped previous observation, found it a naked infant which needed to be "clothed in the garment of language," and he proceeded to throw about it the loose wrapper of *dialectic regeneration*; and he has doubtless done such things at various other times, before and since. He made the wrapper out of old cloth; for, in the present condition of language, at least, there is so much of the finished stuff lying ready to hand that one very rarely finds it necessary or expedient to go back to the raw material. If his analysis of forces is approved by his fellow students of language, this name will be accepted on his proposal, and he will prove himself to have performed what he declares an impossibility: he will have "taken up" a bit of "the so-called body of language," and added it to his conception from without. Grimm, when he had demonstrated certain processes and facts in language-history, called them *lautverschiebung* ('a shoving away of sound:' a very indefinite name) and *stark* and *schwach* inflection ('strong' and 'weak': utterly fanciful; he might just as suitably have named them 'black' and 'white'); and, as a result, all who deal with Germanic inflection are obliged, whether they will or no, to talk of *strong* and *weak* conjugation and declension. Let it not be pleaded that these are examples only of new applications of old names; if the conception designated is new, it has its body added from without, whether this be obtained from the previously existing stores of one's own language, or from

those of another, living or dead—and not less than if the nomenclator had made for it a sign which no human ear ever heard before: like, for example, Bulwer's *vril* (in *A Strange Story*).

THE INDIVIDUAL'S POWER OVER LANGUAGE.

Our author has an infinite deal of trouble—as, indeed, could not fail to be the case, considering his false view of what a word is—with the subject of the action of individuals on language. He began with declaring (i. 39) that, “although there is a continuous change in language, it is not in the power of man either to produce or to prevent it.” But later he appears to have thought better of this; he already saw that the theory of inherent forces in language itself, which by their own energy bring about its changes, is (i. 46) “sheer mythology,” and he was also apparently unwilling to suppose a constant miraculous energy of the Almighty working out the same results; and the only possible third alternative was human action in some way or of some kind. Accordingly, in one or another edition later than the first, he altered his text to read “not in the power of ANY man” etc.; and so it stands at present. The next sentence, however, still reads as at first: “we [not “one of us”] might think as well of changing the laws which control the circulation of our blood, or of adding one cubit to our stature, as of altering the laws of speech, or inventing new words according to our own pleasure.” Considering that, by changing “man” into “any man,” he had virtually acknowledged that “man” does after all have something to do with the growth

of language, and considering that he denies with such superfluous copiousness the combined action of men upon it in "conventions" held for the purpose, one might think that he would feel a special call to investigate further, and determine what the mode of action of individuals really is, and how far it goes; and it is possible that in that case he would have come upon the truth of the matter—that individuals initiate changes, and the community either accepts and uses them, making them language by its use, or rejects and annuls them by refusing to use them; and that a recognition of this simple process, the mutual action of individual and community, explains everything. This, however, he does not do; and his various expressions on the subject, though sometimes betraying an inkling of the truth, are confused, indistinct, and inconsistent. Let us look through a few of them.

If even so great a man as an emperor could not (i. 39) by a blunder change the gender of *schisma* from neuter to feminine, it was only because the established habit was otherwise, and the reason for a change was insufficient—not to mention that the emperor himself in his more instructed moments would not insist upon it. To found upon such incidents a dogma of the inaccessibility of all language to any change by an individual would be the height of absurdity. A little class of Latin neuters plural in *a* appear in French as feminines singular (examples are *arme, bible, merveille*), as the result of similar blunders committed by speakers far below the rank of emperor. Our author himself tells us (ii. 440, note) that *essentia* is a blundering formation, for which somebody is to be held respon-

sible ; but *essence, essential*, etc., have long been unimpeachable parts of European language ; all language is full of the results of such blunders. He further says that, if we ever come to say *very delighted*, “ it will not be by the will of any individual, nor by the mutual agreement of any large number of men, but rather in spite of the exertions of grammarians and academies.” In this, however, he is altogether mistaken ; it will be by the mutual agreement of large numbers of men, of numbers so large that the small minority of grammarians and academicians quite disappears in comparison with them. Here is to be seen again our author's unaccountable persuasion that “ mutual agreement” can signify only formal and ceremonious agreement to do a thing, in a “ convention” held for the purpose, instead of, as is usual in real life, tacit agreement in doing it. We are plainly told at i. 63 and ii. 39 how changes are brought about in certain languages by the conscious and intended action of individuals. At i. 65 (note) is quoted with approval the statement that in a small community a single eminent man may make great changes in a language. At ii. 193, it is “ conceded that single individuals or single families may sometimes influence the fates of a language. Personal defects in pronunciation, at first congenital, may spread by imitation.” How near our author comes here to the true principle—initiation by an individual, imitation by his fellows—if he would only open his eyes and see it ! Also at i. 41-42 this principle can be seen to be involved, though almost hopelessly mixed up with obscuring elements : “ though the individual seems to be [read instead “ is”] the prime

agent in producing new words and new grammatical forms, he is so only after his individuality has been merged in the common action of the family, tribe, or nation to which he belongs [that is, in plain English, it needs the common acceptance and use to make his product a part of the common language]. He can do nothing by himself [of course, and for the reason just stated]; and the first impulse to a new formation, though always given by an individual [a most important admission!], is mostly, if not always, given without premeditation—nay, unconsciously [thus, then, such a new formation, in spite of the categorical denials elsewhere, may perhaps sometimes be given with conscious premeditation!]. The individual, as such, is powerless [repetition of what was said in the preceding sentence], and the results apparently produced by him [no, only initiated; production by an individual is not even apparent] depend on laws beyond his control [that is stuff: read “on influences of whose action he is mostly unconscious”], and on the coöperation of all those who form together with him one class, one body, or one organic whole [i. e., in less turgid phrase, of his fellow-speakers].” And one may compare further ii. 219–220. There are individuals and individuals; and the amount of influence exerted by any single one will depend on his weight in the community, as well as on the special circumstances of each case—which, again, will resolve themselves mainly into consonance with the established habits and resulting preferences of the community; in all their general features they admit of approximate determination, although the free action of the

human will introduces an element of uncertainty which baffles complete analysis.

It is very difficult to strike the balance of such warring and wavering views; but on the whole we may perhaps most safely assume that our author really teaches that men as a race, and even individual men, can and do effect change in language. It is true that, when he comes to sum up his inquiries, under the heading "causes of change in language" (i. 46), he gives us nothing more definite than this: "though it may be quite true that language cannot be changed or molded by the taste, the fancy, or genius of any individual man, it is equally true that it is through the instrumentality of man alone that language can be changed." This both denies the admissions made elsewhere, and is a palpably fragmentary explanation, since we can not but inquire at once what is the real effective force that produces the changes, employing the "instrumentality of man" to work "through." The addition of the next sentence brings us no light: "if language grows, it can grow on one soil only, and that soil is man." A very little prosaic clearness of view and consistency of statement would be far more edifying than all this poetic fancy. A nearer approach to definiteness, perhaps, is found at i. 533, where we read that "what is antecedent to the production of roots is the work of nature; what follows after is the work of man." Here, to be sure, the production itself of roots seems to fall to the ground between two stools; but, even if we understand this as included with what is "antecedent to" it, as the work of nature, nearly the whole substance of lan-

guage is of human production, and its history a history of human activity. And yet our author declares the study of language to be a physical science !

WHETHER THE STUDY OF LANGUAGE IS A PHYSICAL SCIENCE.

He so declares it, too, in the first chapter of his first volume. It must have seemed strange to many readers that a question of this character should be thus settled off-hand at the start, as a kind of minor preliminary to the presentation of the study, and before the author had enlightened his public as to its subject and methods. If he had put it off to the end of his work, it is hardly credible that he would have come to the same conclusion ; but, as he has made no retraction even in this final edition, we must take it as a doctrine which at any rate he means to stand by. There is no reason why we should enter into a serious discussion of it ; for probably no student of language who has any claim to public attention agrees with him ; nor is there anything serious to be urged in his defense. Moreover, in the second volume (ii. 7), he seems half-inclined to withdraw the teaching of the first, acknowledging that it was meant to be taken in a Pickwickian sense ; yet not without a hearty slur at his critics, as a " certain class of scholars " who " ignore your definitions and then show that you have been quite wrong." The implication here (and also on p. viii of the préface to the present edition) is that our author had a right at his own good pleasure to lay down such a definition of a physical science as should make the name properly

applicable to the study of this particular one among the products of human capacities—acting, as human capacities always do act, under conditions and in methods which are withdrawn from the full light of consciousness. So he may prove that a whale is a fish, if you only allow him to define what a fish is. If he were minded to deny altogether the existence of a division line between physical and historical sciences, his position would be intelligible, and his reasons would have a *prima facie* claim to attention. But, acknowledging as he does (i. 22) the classification, and giving the rude but generally acceptable distinction, that “physical science deals with the works of God, historical science with the works of man” (he does, indeed, insert in this edition “it has been said,” as if he had his misgivings about it), that he should still reckon the study of language to the former class is simply unaccountable. Nor does he allege any reason for doing so, except, on the one hand, that its methods have a strong resemblance to those of the physical sciences (but this is true, in greater or less degree, of other historical sciences in their modern forms); and, on the other hand, that the science of language, though called sometimes “comparative philology,” is a very different thing from philology as commonly understood (e. g. classical philology), since it deals with languages themselves, and not with literary records as such. His argument may, without doing it any injustice, be succinctly stated thus: philology is, as all agree, a historical science; but the science of language is of a quite other character; therefore the science of language is a physical

science. The logic of this is not at all discordant with that of his reasonings in general, as we have seen and shall see abundantly. It does not occur to him that the same material may be studied from different points of view while the fundamental character of the study remains the same. He adds in this edition a comparison to illustrate the distinction as he views it (i. 22): "thus the science of optics, including all the laws of light and color, is a physical science, whereas the science of painting, with all its laws of manipulation and coloring, being that of a man-created art, is a purely historical science." Precisely so; and in like manner the science of acoustics, including all the laws of sound and hearing in all their modifications (everything in speech that can be imitated by a speaking-machine), is a physical science; but the science of articulate sounds, as made by men's wills and applied to the needs of human expression and intercourse, is a purely historical science. The comparison offered us is a totally false one; the analogue of the science of language is not the science of optics, but would be a science of artistic expression, dealing with all the instrumentalities and methods and history of the various kinds of art, as related to the art capacities and instincts of men; and no one would hesitate as to where that should be classed.

SCHLEICHER CALLED TO AID.

It was said above that hardly anyone whom the world has reason to listen to agrees with our author on the point now under discussion. There are, of course, exceptions. A notable one was the German philologist

Schleicher (died in 1868), who, in a pamphlet entitled *The Darwinian Theory and the Science of Language* (Weimar, 1863), expresses himself thus: "languages are natural organisms, which, without being determinable by the will of man, grew and developed themselves in accordance with fixed laws, and then again grow old and die out; to them, too, belongs that succession of phenomena which is wont to be termed life. *Glottik*, the science of language, is accordingly a natural science; its method is on the whole and in general the same with that of the other natural sciences." This is a thoroughly consistent paragraph; a dogmatic statement and a logical inference from it; the two hang together, and must be together accepted or rejected. Now it will hardly be credited, but is nevertheless true, that our author (i. 46) quotes in substance the first part of the paragraph, the dogmatic statement, as an example of "sheer mythology" in treating of language, and at another place (i. 28, note) gives the other part, the inference, as a support to his own view! It would not be easy to find a more remarkable example of trying to sit on two stools at once. If our author can accept Schleicher's theory of language, he has the right to take with it the resulting view of the character of its science; but if he (with good reason) rejects the former as totally wrong, then his own doctrine is condemned by its agreement with the latter.

THE IDENTITY OF LANGUAGE AND THOUGHT.

But there is an additional term to this comparison of views. In various passages of his second volume,

our author teaches that thought and speech are identical. Respecting this subject, to be sure, as respecting nearly every other, he expresses himself quite differently in different places. At ii. 70, he claims only "that thought, in one sense of the word, i. e. in the sense of reasoning, is impossible without language or without signs;" and in the next sentence he allows "the reality of thought or mental activity in animals;" while, a little further on (p. 71), he is willing to "concede to animals sensation, perception, memory, will, and judgment." If judgment, or the drawing of conclusions from premises, is to be conceded to animals, one wonders wherein can lie the immense difference between thought and reasoning. There seems to be here another instance of that liberty (or license) of definition on his part which, as we saw above, our author reproaches his critics with desiring improperly to abridge. "Reasoning is that kind of thought which is carried on only by means of language; hence, reasoning is impossible without language." That is at any rate logically immaculate; but one does not seem to get on very much by it in his knowledge of either thought or language. If we look further on for more light, we find (p. 82) that, "without words, not even such simple ideas as white or black can for a moment be realized." Is that possible? Can any one doubt that many even of the lower animals distinguish white and black, and know each when they see it, as clearly as we do ourselves? But perhaps this again is a matter of definition, and the trick is in the word *realized*: "realizing is that kind of comprehension which is given only by language; hence, to

realize white and black is impossible without language." The next page, instead of enlightening us, confuses us still more. We are there informed that "a child *knows* [the italics are the author's] as certainly before it can speak the difference between sweet and bitter (i. e. that sweet is not bitter) as it knows afterwards (when it comes to speak) that wormwood and sugarplums are not the same thing." It appears, then, that to know that sweet is not bitter is possible without language, but to know that wormwood is not sugarplums is only attainable by means of words; therein lies the difference between mere thought and reasoning; and that is the sort of power one acquires by learning to talk! Some one who happens to have a child not yet able to speak might do well to try the experiment of slipping into its mouth a bit of wormwood instead of a sugarplum; perhaps, after all, he would find that the child not only "knew" the difference, but "realized" it not less thoroughly than the man who can talk an hour without stopping. But our author goes directly on to explain further: "a child receives the sensation of sweetness, it enjoys it, it recollects it, it desires it again; but it does not know what sweet is; it is absorbed in its sensations, its pleasures, its recollections; it cannot look at them from above, it cannot reason on them, it cannot tell of them." Poor child! It is to be inferred, then, that our author, who has learned to speak, does know what sweet is? Physicists would be much obliged to him if he would kindly tell them. The general opinion is that sweetness is a subjective sensation, not further definable, caused in the nerves of gustation by

certain substances, which we therefore call sweet ; and that we apply the name not because we thereby know or realize any better what sweet is, but in order that we may "tell of it." A child, it is true, may be more "absorbed" in the enjoyment of sweets than a man, owing to the absence in its life of other absorbing interests that occupy the mind of a man ; but even the man is not ordinarily tempted to "reason on" the sensation. As to "looking at it from above," we may as well give that up, as too difficult a puzzle, suppressing at the same time the timid suspicion that the phrase is perhaps mere ornament, and does not mean anything. The real balance of result for the science of language which we arrive at is simply this : the man and the child alike do not know what sweet is ; but the man differs from the child in being able to tell of it : no great gain to boast of. Our author, however, derives from it and from his other reasonings here reported "the one and indivisible character of language and thought" (ii. 85), and "considers the identity of language and reason as one of the fundamental principles of our science" (ii. 86); just as in the first volume (i. 527) he maintained that "words without thought are dead sounds, thoughts without words are nothing ; to think is to speak low, to speak is to think aloud."

Probably the number of those who hold this doctrine, of the identity of thought or reason with language, is as small as the number of those who hold that the study of language is a physical science. But also, beyond doubt, the number of those who hold the two doctrines together is not greater than unity, be-

ing limited to our author himself. It takes a mind very peculiarly constituted to contain them both without being disturbed by their repugnance. Nor is it every one who could manage to be so far wrong in both the mutually-destructive parts of one theory.

The erroneous view that thought and language are identical is arrived at by three stages :

1. Language is of immense importance to thought.

This is incontestably true ; a great deal of our actual thinking is of a kind and degree that could not be carried on without the aid of the symbols called words, any more than intricate mathematical processes, even the multiplication of larger numbers, can be performed without figures, or than many processes of mechanical art without tools.

2. No thought is possible without language.

This is simply an exaggeration : as if one were to hold that figures are necessary in order to show how one and one make two, or how twice two are four ; or that the bare hands are unable to accomplish anything.

3. Thought and language are identical.

This is the merest confusion and absurdity, like maintaining the identity of processes of mathematical reasoning with mathematical signs, or of the hands with tools. With which of the words *one*, *ein*, *eka*, etc. (to recur to a former illustration : p. 11), is the conception 'one' identical? or is 'city' identical with *city*, or *urbs*, or *nagara*, or any other of its thousand signs? The infinite diversity of language is of itself a sufficient refutation of its identity, or even of its close and inseparable union, with thought,

which in its lower and simpler processes is substantially one and the same in all men.

LANGUAGE AND ETHNOLOGY.

One of the most important bearings of a sound science of language is upon ethnology. Having no clear and defensible view of language as related to race, our author can, of course, make no useful contribution to the discussion of the relation of these two branches of study; he rather complains that they "have both suffered most seriously from being mixed up together" (i. 458), without telling us how or why; or he withdraws himself behind the plea that "the Science of Language has nothing to do with skulls" (ii. 296): that is, that we are debarred even the attempt to coördinate the two classes of evidence, linguistic and physical, so as to make language furnish its part toward the unraveling of the early history of mankind. If we once see distinctly and hold firmly the truth (as stated above) that a language is a body of external signs for conceptions, which has to be learned by every human being that uses it, and which can be learned by a man of any race; and, further, that circumstances are fully capable of leading not only individuals, but large bodies of individuals, even whole communities, to learn languages unknown to their ancestors—then we have established a principle which governs all the relations of linguistic and physical evidence; and this, while of necessity limiting narrowly the value and reach of linguistic evidence, yet leaves it so much importance as should satisfy every linguist.

LANGUAGE AS BARRIER BETWEEN MAN AND THE LOWER ANIMALS.

Again, it is only a distinct understanding of the same fundamental truths as to the nature of language and its relation to thought that can give any value at all to statements made respecting its bearing on the distinction between man and brute. To declare, as our author does in various places (i. 12, 480, etc.), that language is *the* barrier, or the one impassable barrier, or the like, is unscientific and worthless. Language is not the only barrier, for there are a plenty of other differences which are not a whit less characteristic. If he had said "it is *a* barrier, and the one which, as a professed student of language, I most notice and appreciate," he would have taken up a position capable of defense. As it is, he might precisely as well have said "the Ural mountains are the barrier between Great Britain and Kamchatka." Even as *a* barrier, as one among many, it calls for a better linguistic philosophy than our author's to show that the difference is one of kind, and not of degree merely. Since many of the lower animals possess means of communicating with one another, and some of them to a noteworthy extent, it is incumbent on a student of language who meddles with the subject at all to show what, if anything, there is in the mode of communication possessed by men that is distinctive, that is different in principle from the methods of other animals. This our author does not do, and, holding his opinions, is quite unable to do; and yet nothing is more easy, if one knows what human speech is. In the complete conventionality of our spoken signs for

thought is found the (so far as we know or have any reason to suspect at present) full and sufficient distinction ; in the fact that no sign in any human language depends for its value upon an internal and immediately or instinctively apprehensible connection between sign and sense ; that every such sign has to be learned and associated with its sense by every one who understands and uses it. This element is what has to be sought after in the mutual communication of animals, if any parallel is to be drawn between that and ours. If, for example, the chatterings of a certain species of monkeys are at once intelligible to all the animals of that species, wherever born and however reared, and yet more if they are intelligible also to other species, they are not analogous with men's speech ; to be this, they must, however strong their appeal to monkeys of the given species from a certain locality, leave entirely unaffected those from another and distant locality, or individuals brought up away from the society of their fellows. It is precisely among the lowest and least cultivated races of men, as our author abundantly points out (chap. ii. of his first volume), that dialectic diversity is greatest, so that on a petty Polynesian island, peopled by tribes undeniably of the same stock, each tribe may be found to have a different speech, unintelligible to the others : and this by the universal and normally-acting laws of growth of human language. It is only communication, not kindred, that makes and keeps mutual intelligibility among human beings. Any means of communication that is immediately intelligible to a whole species, or to more than one, does not

correspond with language, but with those natural and instinctive signs whose use we must necessarily assume to have preceded the historical development of language and led the way to it, and which could, even were every now existent dialect wiped out, still be made available for a certain restricted amount of mutual intelligence—probably a greater amount than we now realize ; since the use of language, as a vastly more perfect instrumentality, has dulled both our power to produce and our quickness to understand such signs : indeed, we may perhaps even suppose, in accordance with modern views of heredity, that those powers have undergone actual degradation and effacement by long disuse. We ought also to hold our minds open to the possibility that traces of conventional intercourse, improbable as it now appears, may yet be found among some of the lower animals ; and certainly we need not be in the smallest degree afraid of such a discovery ; it is for men like our author, who appear able to see only one “barrier,” to dread even its smallest reduction, as threatening a removal. To us it is far enough from Great Britain to Kamchatka, though the Urals between were razed to the level of the steppe.

If we wish to put the absence of language among the lower animals in the class of deficiencies to which it belongs, and whose analogies explain its character, we must use the term “institution.” Every human language is an institution, a part of the civilization of the race that has produced it, slowly wrought out and shaped, like the race's other institutions, in the struggle of man with his surroundings, and handed down

by tradition, not by inheritance, from one generation to another, and even sometimes, like other institutions, from one race to another ; the linguistic philosophy of no one who does not see and acknowledge this has a really solid foundation. The lower animals, whatever their capacities, in some respects surpassing ours, may be, have no progressive civilization, no growing institutions ; and, with the rest, they have no language.

THE ORIGIN OF LANGUAGE.

There is yet another subject, one of the most interesting in the science, and, if taken up aright, neither mysterious nor obscure, namely the origin of language, as to which our author's fundamental failure to see what a word is in relation to its idea renders him incapable of saying anything worthy of serious attention. It is a subject upon which his views have undergone some degree of modification, though hardly of clarification. In his first edition he appeared to put forward as his own the so-called "ding-dong" theory : namely, that in man's "primitive and perfect [!] state" an idea that struck him rang by his tongue, and so furnished him a "phonetic type" of the idea ; and that, when its necessary work had been accomplished, and man provided with a set of "roots" for language to grow from, this instinctive capacity disappeared again. In later editions we learned that we had been mistaken ; that this account was meant only as a quotation from a certain German authority (whose name, however, had not been even mentioned in the Index to the volume) ; and in the

present work it is indeed, though retained, sufficiently hedged about with cautions against our attributing to it any substantial value. The simpler way might have been to omit it, and to put in its place a definite account of the author's own view ; but this would not have accorded, apparently, with the plan of his revision. So, also, in the final preface to the first volume (p. x), he makes an admiring and approving reference to Noiré's utterly fantastic theory, called by him "synergastic," that the first roots arose from the involuntary common exclamations of a troop of men engaged in performing a work together, like sailors pulling on a rope ; but he neither describes nor defends this theory in the chapter devoted to the subject. So much at least is clear, that to him "roots" are the initial elements of speech. In regard to this, doubtless no sound philologist will disagree with him ; for roots are simply words in which no grammatical character, whether of part of speech or of form of inflection, is discernible ; and all such distinctive characters are demonstrably the product of later growth in language, and do not belong to its primitive stage. That, however, in any given language, or even body of related languages, we can point out the body of roots which antedated the development of grammatical distinctions is by no means to be believed. Bodies of roots as arrived at by the sole process within our reach, namely by analysis of existing forms, would be only in part coincident even in the various tongues of our own family. Our author repeatedly insists that no new roots have ever been made since the original root-making period ; that is doubtless not true in any

sense ; but it is especially untrue in the lower and practical sense, since the forces of linguistic change are all the time reducing to the semblance of roots elements that are really derivative or compound : such are, for example, English *preach* and *cost* (from *præ-dicare* and *con-stare*), or French *périr* and *cueillir* (from *per-ire* and *con-legere*), or Latin *debere* and *posse* (from *de-habere* and *potis-esse*). Of these, to be sure, we happen to have recorded information that enables us to trace the genealogy ; but we are totally ignorant how many and which, if not all, of what we call Indo-European roots are really of the same secondary character. Our author points out (ii. 445), with good reason, that the period from now to the time of the Vedas is as nothing compared with that through which a word like the pronoun *aham* ' I ' comes down to us ; it may be added that the distance of time from us to the birth of *aham* is as nothing compared with that which would take us back to the actual beginnings of human speech. As to the phonetic form of these beginnings we know nothing, nor can hope ever to know anything ; as to their kind, we can draw confident conclusions from our knowledge of the character and the history of language within historic times.

In regard to the origination of the primeval stock of roots our author, as we have already seen, is partly inconsistent and partly non-committal. At one time he calls them the offspring of an instinct which died in giving them birth ; at another (e. g. ii. 375) he requires us to accept them simply as ultimate facts. At any rate, their recognition is, he says, opposed to the interjectional and onomatopoeic theories (i. 526) ; and

yet (i. 536) he is not unwilling to call the mental instinct which produced all that is material in language either interjectional, or onomatopoeic, or mimetic. Still later, however, in his *Lectures on Mr. Darwin's Philosophy of Language* (1873; since only a magazine reprint is accessible, a more definite reference cannot be given*), he says that roots "represent the nuclei formed in the chaos of interjectional or imitative sounds;" and again, that "interjections and imitations are the only possible materials out of which human languages could be formed." If he really reached this conclusion, and in such a way as to hold it firmly, why should it not appear in this final revision of his text-book on the science? And if (i. 528, note) his *Science of Thought* has yet later and more matured conclusions, why should he not report them also here, at least in brief, instead of only serving up to us once more his crude and superseded notions of thirty years ago?

The essential determining element in any real theory of the beginnings of speech must be the same recognition of the true nature of speech which we have already found efficient above in deciding other questions: namely, that words are only external signs for conceptions. The plain and undeniable existing result of the unnumbered centuries of linguistic growth has been to provide all the different races of men with bodies of such signs, all conventional, all requiring to be learned, all diverse. And through the whole known history of language the thing striven after has

* See *North American Review*, vol cxix. (July, 1874), p. 61 ff.

been the same : to obtain, always from the most convenient source, signs which should be associated with whatever new ideas were conceived, and should express these by representation. There is no reason for our assuming anything else to have been the case from the first ; though the circumstances were very different, the intent was the same ; the beginnings of speech were the successful results of the attempt to arrive at mutually intelligible signs. That is what takes place nowadays between two human beings who have not learned one another's speech and who desire intercourse ; that would, if living languages were to disappear suddenly, be the way in which a start was made toward the recovery of the lost possession. The exigencies of primitive human intercourse called out the first signs ; the possibilities of mutual intelligence suggested them. The *primum cognitum* had nothing whatever to do with the matter ; it was determined by a combination of the *primum designandum* and the *primum designabile* (if we may coin the term). Whatever was most plainly suggestive of an intended meaning was put to use ; and it is simply because such suggestiveness is found especially in imitative utterances, whether interjectional or onomatopoeic, that these must be acknowledged as the initiators of speech. If we seek an instructive parallel, we may find it in the beginnings of writing, which, as no one questions, were the depictions, the recognizable imitations, of visible objects—and for the same reason, because they were mutually intelligible signs. Or, again, in the written language of mathematics, which has become a great system, fearfully and wonderfully made to the

apprehension of those who are not familiar with it, and wholly independent of ordinary speech, but which has its starting-point in one, two, and three strokes, suggestive signs for 'one,' 'two,' and 'three.' At present, it is true, no suggestive character is to be traced in the signs of speech—or, if in exceptional cases discernible, it is unessential, decorative rather than significant; but that is because, after a certain amount of use, each sign came to be habitually employed, no longer because of its suggestiveness, but because it had been used and understood before. So we no longer see the two and three strokes in our figures 2 and 3, although they are really there, disguised from view. So we no longer discover pictures in our own alphabetic characters, nor in the cuneiform signs, nor in the Chinese mode of writing, while yet not questioning their hidden presence there; out of hieroglyphical signs, which have their original suggestiveness, grow everywhere hieratical and demotical signs, in which this is lost, and may grow alphabetic signs, involving an altogether different principle. In this sense (though one not recognized by him), it is strictly true, as claimed by our author (i. 507), that language begins where imitation ends: that is, human speech, as a system of conventional signs, began its history when the signs lost their pictorial or depictive character, and were used because others had used them before. That out of a very small array of such signs a whole rich language could with the process of time be developed no one can deny—least of all our author, who in his chapter on "the root *mar*" (ii. 408 ff.) has shown how from a single starting-point

(and one which he himself acknowledges may have been originally imitative : p. 408) a whole scheme of diverse senses and applications, of every class, could arise.

DIALECTS.

That a word is the conventional sign for a thought, not its depiction or inseparable reflection, is a fact that underlies also of necessity the constant change of every language ; and the latter is unintelligible without its full and formal recognition. That this change is the cause, and the only cause, of the divarication of any given language with the course of time into dialects is one of the truths without which the theory of dialects cannot possibly be understood. Another is that communication, intercourse, is the counteracting and regulating force which prevents indefinite divarication, maintaining a degree of unity of speech within the limits of a community. Of these fundamental truths our author has no clear vision, but only occasional and imperfect glimpses, and hence all that he says of dialects is obscure and full of inconsistencies. It is altogether doubtful even how he would define a dialect as distinguished from a language ; he often expresses himself as if the two names belonged to different things, instead of to the same thing under different aspects. A dialect is simply a related language. The two differ only as a brother, a cousin, an uncle, differs from a man. Every dialect is a language, as fully entitled to the name as any other. Every language is also a dialect, provided it have any relatives, descendants with it from the same more original speech ;

and, as a matter of fact, no language known to us fails to have such relatives ; all languages are found to occur in groups having, in very varying degrees, that kind of resemblance which comes from sharing a common tradition ; proved products of the divergent alteration of a single tongue. One dialect in a group, or more than one, may be more conspicuous and important than the rest, or all may be alike obscure : this has nothing to say against the double character of every one, as dialect and at the same time language. The world of speech, as of men, is made up of all sorts of individuals.

Our author rightly says (i. 61) that "literary languages, such as Sanskrit, Greek, and Latin, are the royal heads in the history of language" ; and a considerable part of his lucubrations on the subject is open to but little criticism. But, here as everywhere else, he has no such definite conceptions as he can hold fast to ; and another considerable part is of a very contrary character. On the page already quoted from (i. 61), he thinks it may be shown *à priori* "that dialects must have existed before uniform literary languages." Considering how late phenomena even the earliest literary languages are, could it possibly have entered into the mind of any one to believe otherwise ? But what is a "*uniform literary language*" ? for we are immediately after told that, even under the sway of a literary language, "hardly any one even now speaks like everybody [he doubtless means "anybody"] else." And hence we are made to infer that "from its very first beginning language existed in the form of dialects." But that is by no means true, unless before

“the very first beginning” there existed a common language to divaricate from. Half the time our author seems to see no distinction between dialectic variety and non-dialectic diversity. If we bring together an English, a Chinese, a Zulu, and a red Indian person or family, we shall have difference of speech enough, but it will not be dialectic variety, but fundamental discordance. Dialects imply previous connection; and if (which we do not know) language did not come into being until after there were “clans and tribes” as well as “individuals and families” (i. 61), then there were as many diverse languages as centers where it arose, and, later, just so many groups of dialects descended from these—not “from,” but *after* the very first beginning of speech.

FAMILIES OF LANGUAGES.

Further on in the volume (i. 239), we are warned against “the common but altogether gratuitous supposition that the principle of genealogical classification must be applicable to all languages.” But why? Are not all existing and recorded tongues descended from other tongues; and is not dialectic divarication, even “from the very first beginning,” a necessary tendency of all human speech; and is it not therefore the first question we have to ask, respecting any human speech whose history we wish to investigate, with what others it is related? That and nothing else is what is meant by “the principle of genealogical classification;” and to deny its universal application is to deny altogether the scientific character of language-study.

As, however, we go on to examine our author's further reasonings upon this subject, we are led to suspect that, as often elsewhere, he perhaps does not mean precisely what he says, but only intends to intimate that, in the present condition of our knowledge, we are not able to carry out everywhere a satisfactory genealogical classification. For he points out (i. 240) the great importance, as signs of relationship, of "formal or grammatical elements" ("which," he sagely adds, "after they have been affected by phonetic change, can be kept up only by a continuous tradition"—as if any of the elements of speech ever were or could be kept up in any other way!); and, showing that some languages may lack these, he concludes (i. 241) that a "genealogical classification of such languages is, therefore, from the nature of the case, simply impossible, at least if such classification is chiefly to be based on grammatical or formal evidence." A most coherent argument, certainly, where a "simple natural impossibility" is tempered by an "at least if" and a "chiefly"! What he really means is rather this: "since grammatical or formal evidence is of the highest value and most convincing character as between dialects, we shall find it very difficult, and sometimes impossible, to make a complete genealogical classification where it is wanting;" and in this there is much truth. Elsewhere, however, he is far more liberal in regard to the admission of evidence; he allows (i. 400) a real relationship of all "Turanian" speech on the ground of "common words and common roots which have been discovered in the most distant branches," and even (i. 399)

points out that, instead of our finding here "the same family likeness which holds the Semitic or Aryan languages together, . . . it is the very absence of that family likeness which constitutes one of the distinguishing features of the Turanian dialects." If even the absence of family likeness can be relied on to prove family relationship, there is no reason, certainly, to deny the universal application of the principle of genealogical classification in any sense, or to despair of bringing all languages without exception together into families; many of them exhibit a great deal of that kind of evidence.

HOW MANY FAMILIES OF LANGUAGES?

On account, probably, of the general lack of evidence of both kinds, family likeness and the absence of family likeness, our author declares that there are not more than two or three families of language to be recognized in the world. After his denial of the possibility of general genealogical classification, indeed, no one can be surprised to find him (i. 385) maintaining that, "strictly speaking, the Aryan and Semitic are the only families of speech which fully deserve that title." A little further on (i. 396), however, he says of the "North-Turanian" languages that "they share not only common morphological features, but they are held together by a real genealogical relationship, though not a relationship so close as that which holds the Aryan or Semitic languages together." It seems, accordingly, that "real relationship" does not make a family, unless it reaches a certain undefined grade of closeness. Yet at i. 156 we are told of cer-

tain dialects "now classed as members of the Turanian family." Again, at i. 389, we read of the Bantu languages that "not only their strongly marked grammatical features [so they do not lack even this highest class of evidence!], but their common property in certain important words also leaves no doubt of their being descendants of one and the same family." Without some explanation, not vouchsafed, no one can be fairly expected to see what the distinction is between being a family and being the descendants of a family. Still earlier (i. 156), we are told of "one of the most brilliant discoveries in the history of the science of language, the establishment of the Malay and Polynesian family of speech." Amid all these uncertainties and discordances, the only thing we see clearly is that here is another subject respecting which our author does not know his own opinion well enough to adhere to it from chapter to chapter, even from page to page, of a finally revised work. There is, in fact, no discoverable reason for limiting the name "family" to Aryan and Semitic, except that these two are such extremely respectable old families, whose fame is spread through all the earth. And this our author elsewhere deems no sound reason, when he (rightly) teaches us that, from the point of view of the science of language, one dialect is as good as another, the uncultivated as the cultivated, the obscure as the noted, the recent as the ancient. It cannot be in addition because we know all about Aryan and Semitic, while the limits of other families are uncertain; for there are a plenty of uncertainties besetting even these two, as regards not only their own

limits, but their connection with other recognized families (if we may provisionally speak of others), and even with one another; many scholars maintain the original identity of Hamitic with Semitic; many more of Semitic with Aryan; and a trusted authority repeatedly quoted by our author (e. g. i. 475, note) makes not the least difficulty of connecting Chinese and Mongol with Aryan.

WHY ARYAN, SEMITIC, AND TURANIAN ARE THE ONLY
FAMILIES.

In a work (*Introduction to the Science of Religion*, London, 1873) of considerably later date than the original Lectures of which the present is the final edition, our author fully admits three real families instead of two, and says (p. 161) that "the reason why scholars have discovered no more than these two or three great families of speech is very simple. There were no more, and we cannot make more. Families of languages are very peculiar formations; they are, and they must be, the exception, not the rule, in the growth of language." And he goes on to point out the "possibility," but the absence of "necessity for human speech leaving its primitive stage of wild growth and wild decay," and attributes the cases of withdrawal which actually occurred to "a purely spontaneous act on the part of the ancestors of the Semitic, Aryan, and Turanian races"—with more, which must be seen in order to be credited.* Of all the theories with which our author's fertile brain has decked the outside of the

* It is discussed more fully in the *Transactions of the American Philological Association* for 1880, p. 100 ff.

science of language, this is doubtless the most original and surprising. The "conventions" by which he (with everybody else) denies that men, assembling, formed their conventional speech would be as nothing compared with the spontaneous movements here assumed for these three ancient races, meeting together and combining their dialects into a family, and resolving that they would never, never have anything more to do with wild growth and wild decay—only, alas, how vainly! since the records show that they have since gone on growing and decaying not less wildly than before. We ought not, indeed, to lay too much stress on the theory, since it may possibly belong only in the environment of the particular work from which we have taken it, and not among its author's general linguistic doctrines—though it has been quoted with acceptance by some of his followers. But, at any rate, it is not too late for students of language, by spontaneous acts of their own, to make up for neglect on the part of the ancient speakers of tongues outside the pale of the grand trio, by themselves gathering those tongues as now spoken into groups and branches and families. That is just what they are doing, as rapidly and thoroughly as the circumstances, often unfavorable, will allow; they are tracing out dialectic affinities, and classifying accordingly, making up just so many families as the facts seem to warrant, but acknowledging that much of their work is provisional only, and that, if even Aryan and Semitic are in some degree doubtful designations, other family names must long, or always, continue such. To work in this way is to take a part in the

progress of linguistic science ; to do otherwise is to stand apart from that progress.

THE OLD TURANIAN FAMILY ABANDONED.

Our author should have due credit for finally abandoning in this edition the old "Turanian" aggregation, which, as established by himself, and widely accepted on his authority, has for a generation been a stumbling-block in the way of science. He is still, however, apparently under the impression that his labors upon it were a part of the advance of knowledge ; that the setting up of a "Turanian family" to contain all the Asiatic languages which are neither Aryan nor Semitic was a step forward, and its present repudiation another. In this he is doubtless mistaken; the classification was always a groundless and unscientific one, a classification of ignorance, or a practical erection of the absence of family likeness into a family tie—a principle which, as seen above, he has not yet given up in theory. It was a step backward, in which our author dragged with him a great many weak or ill-informed followers ; and these, unfortunately, will be slow in retrieving it ; the name Turanian will probably long continue, as it has long been, one of the watchwords of sciolism.

DOES UNITY OF SPEECH PRECEDE DIVERSITY?

In parts of his discussions respecting dialects and families, our author appears distinctly to recognize the principle that dialectic resemblances point back to original unity of speech. Thus, at i. 33, he speaks of "an earlier language, the mother, if we may so call it, of the whole Indo-European or Aryan family of

speech ;” of “the original language of the Semitic race ;” and of the Ural-Altaiic languages, “all radii from one common center of speech ;” and again at i. 235, and in the second volume at p. 350, nothing could well be more explicit than his statements to a like effect. But at i. 247, where he comes to speak of the Germanic or Teutonic dialects, he surprises us by declaring that “there never was a common, uniform Teutonic language ; nor is there any evidence to show that there existed at any time a uniform High-German or a uniform Low-German language, from which all High-German and Low-German dialects are respectively derived ;” and then follows a page or two of rambling discussion, in which truisms and errors are intermingled, in a way that it would require many pages of comment to unravel and set right. Since the unity of Germanic speech rests on precisely the same basis of fact and inference as that of Indo-European or Aryan speech, we cannot but wonder to find the one denied and the other maintained between the same covers. In fact, the one is just as true, and needs to be taken with the same limitations, as the other. Insistence on the qualification “uniform,” which our author uses in connection with his Germanic statement, will not relieve him from the charge of inconsistency ; for, as he has recognized elsewhere (see above, p. 42), there is not now, and so of course there never was, a wholly uniform speech, spoken by a limited community, even a cultivated community, anywhere in the world ; hence the unity which is postulated at the point of divarication of a set of dialects is one that necessarily involves

a certain, not inconsiderable, percentage of variety. It is only that kind and degree of unity which is possible in human speech, a unity in comparison with the much greater variety which grows out of it.

But all this concerns chiefly our author's first volume. When we come to the second, in a long passage (ii. 181-5) which is now for the first time added, and must therefore be taken as representing his latest and ripest opinions, we learn that the original Aryan language itself is also a fiction (ii. 184): "it was formerly [i. e. in the preceding volume?] the fashion to speak of a Proto-Aryan language from which Sanskrit, Greek, Latin, Teutonic, Slavonic, and Celtic were all derived, just as French was derived from Latin, or English from Anglo-Saxon. That theory, however, has hardly held its own for a longer time than the theory which it was meant to replace, namely that all Aryan languages were derived from Sanskrit." If our author has really gained new light which overthrows the theory in question, making it a blunder comparable with the ignorant opinion, never held by any one of any degree of instruction, that Sanskrit was the mother-tongue of the Indo-European family, why, in this final revision, should not the statements in the first volume which taught the blunder have been rectified? We found there a seeming inconsistency of one kind; now we have one of another kind, and of a degree that may fairly be called a stultification. The theory has "held its own" in those earlier chapters even till now; and, thanks to our author's heedlessness, it bids fair to hold its own there still as long as this text-book of the Science of Language shall endure.

There would seem, at the very least, to be due us here some rendering of a reason for the abandonment of the theory elsewhere taught. Are the differences of Sanskrit, Greek, Latin, and the rest, eternal? Converging as they plainly do in the past, do they never meet or almost meet? Was there "from the very first beginning," as it is said elsewhere, no Proto-Aryan, but only a Proto-Sanskrit, a Proto-Greek, and so on? Then why not also a Proto-French and a Proto-English? What are we to put in place of our old belief, if now discarded? These are questions to which the succeeding paragraph seems to undertake to furnish a reply; and it is so curious and characteristic a specimen of our author's peculiar style of ratiocination that we cannot help giving it a little detailed examination.

WHY THERE WAS NO PROTO-ARYAN LANGUAGE.

He begins his paragraph of explanation thus: "And yet there was some truth in that theory, if only rightly understood." Its essential untruthfulness, then, is not to be proved, but only taken for granted; that is our first disillusionment. Still, if the single phase of truth which it presents is clearly set forth, according to the apparent promise of this sentence, we shall perhaps be able to infer wherein the remaining falsity consists. But we are again disappointed, for in what follows is made no attempt to let us "rightly understand" what the "some truth in that theory" is. He goes on, namely, thus: "To imagine that there was a settled Proto-Aryan language, as settled as Sanskrit, and that it became modified after-

wards, according to strict phonetic rules, is, no doubt, impossible." No one has ever ventured to imagine that the original Aryan tongue was "as settled as Sanskrit," or settled at all in any such sense; yet, on the other hand, every competent linguistic scholar will not only imagine, but confidently hold, that whatever phonetic modifications it afterward underwent were "according to strict phonetic rule," and could not have been otherwise. It is not every one who could combine into one sentence a truism and a false assumption, each of which has no bearing on the other, while neither has any bearing on the subject under discussion. The question whether a language must be "as settled as Sanskrit" in order to have its phonetic changes take place according to rule is a quite new one, which historical students of language have not yet taken up, and to which it will probably be found impossible to induce them to direct serious attention. On the contrary, since we find that phonetic changes do occur with a great degree of regularity through the whole body of Aryan languages, we shall be tempted, on our author's own principles, to deem it rather necessary than impossible to imagine a settled Proto-Aryan language. Further: "That process can be studied to great advantage in the transition of Sanskrit into Prakrit dialects." Doubtless; but why point it out here? Such an *obiter dictum* merely disturbs the strained attention with which we are trying to follow the argument. It proceeds: "But we have only to study languages before they are reduced to writing in order to see that the natural state of language is always dialectic."

That is unquestionably true—or after they are reduced to writing, either. The writing of both Sanskrit and Prakrit has not prevented the multifariousness of later Hindu dialects, nor the writing of Latin that of Romanic dialects, nor the more recent writing of Italian that of modern Italian dialects, nor the writing of several successive stages of German that of German dialects. Dialectic divarication is so necessary an accompaniment of the growth of language that no actual linguistic conditions have been found able to prevent it; only ideal conditions, which may be approached but probably never realized in the future, can put an end to it altogether. But we must go on and finish the sentence we began to quote above: “. . . is always dialectic, and dialectic, not in the sense in which Italian, Spanish, and French are dialects, derived from Latin, but as we often find in the smallest Polynesian island two or three dialects existing side by side, not one of which has a right to claim precedence before the others.” This is the end of the paragraph; and, instead of explaining anything, it raises a whole array of new difficulties. It appears to be claimed that language is “naturally” dialectic in the Polynesian sense, but unnaturally so in the Romanic sense. But wherein the difference lies is not made clear. Is it derivation from Latin? or being found on a small island? or existence side by side? or the absence of precedence in the one case but not in the other? Possibly, however, our author meant rather to say “not only in the one” sense, “but also” in the other; we ought in charity to allow him to emend to that effect, since it is impossible by any other means

to put even the smallest degree of sense into the statement. There would be left, then, the wrong and untenable doctrine that a body of dialects into the development of which has come as one element the influence of a written or cultivated, a "settled" language, are essentially different from others that have always lacked that element. Among the infinite variety of circumstances that influence the growth of dialects, the grade of civilization of their speakers is one, and a very important one; and if this reaches the point at which writing is developed, it attains a correspondingly increased importance. But its mode of action is solely by adding strength to the forces of communication, which make for unity of speech; there is no difference of principle; Romanic dialects and Polynesian dialects are products of the same tendencies, working in the same way; either group is enough to illustrate the truth that in the Proto-Aryan speech—unless it were, as no one supposes, an out-and-out original tongue, with absolutely nothing earlier behind it—there must have been dialectic differences, and yet a sufficient unity to make it one tongue, able to transmit a family likeness to its descendants. Dialects are always the result of divarication, and the simple reason why our historical researches never find language otherwise than in a condition of dialectic division is that the remotest period in language-history to which we are able to penetrate is still very far short of the beginning. Our author shows in many of his utterances that he does not see this, but imagines human speech to have actually sprung into being in the form of related dialects.

UNDIFFERENTIATED ALPHABETIC SOUNDS.

We discover in the sequel why it is that our author wants to set up a distinction between the "natural" dialects of unsettled languages and the unnatural and regularly modified dialects of settled or written languages. It is because of the prominent importance which he proceeds (ii. 185 ff.) to attribute to the occurrence of undifferentiated "letters" (read everywhere "sounds") in the former class, assuming it to be impossible in the latter. He quotes from sundry authorities statements as to the uncertain character of various consonantal sounds in Polynesian, American Indian, and other languages, and points out how differences might appear in the descendants of those languages which did not imply a transition from one consonant-sound to another, but only a diverse differentiation of a primitively indistinct sound. For example, in Hawaiian there is no real distinction of *t* and *k* (p. 187); hence, of two dialects descended from Hawaiian, if we were to find, in words apparently corresponding, a *t* prevailing in the one and a *k* in the other, we should not be justified in saying that either *k* or *t* was original, and that *k* had been changed to *t*, or *vice versa*. In this he is unquestionably right; and it is to be hoped that no historical student of language will be (if it is fair to suppose that any have been, which is doubtful) otherwise than duly mindful of the principle; if it be ignored, an occasional false etymology might be the result. But the danger also exists that an incautious student, thinking himself to have brought out a new principle like this, will let himself be run away with by it, and will apply it

where it has nothing to do. And to this danger our author has plainly succumbed. He assumes that in general, and on a very large scale, wherever among the Indo-European languages we find different consonantal sounds in two forms apparently of the same word, we are to ascribe the fact to a diverse differentiation of an originally indistinct consonant. In so doing he leaves out of sight, as if it did not exist, the overwhelming evidence we possess that the "Proto-Aryan" did distinguish guttural and labial and dental consonants, even to three varieties of each class, from one another. As a prevailing rule, the words which in one modern language of the family show a dental show it likewise in all the others; and so with guttural and with labial. That this would not be the case if they were results of the differentiation of a unitary sound is palpable. If there ever was a time in the history of our language when a Hawaiian indistinctness of consonantal utterance prevailed, it could only have been in the pre-Proto-Aryan period; and no sound scholar has paid or will pay any attention to our author's claims. Cases like Gr. *tettares*, Skt. *chatwar*, Lat. *quatuor* (p. 187; Germ. *fidvor* should have been added), our *four*, will continue to be treated, as they have been, as special exceptions, calling for separate investigation. In casting about for support, the author descends to such pleading as this (p. 188): "there is nothing to show that in *thermos* [=Skt. *gharma*] Greek ever had an initial guttural; and to say that Sanskrit *gh* became Greek *th* is in reality saying what is impossible. No Sanskrit letter can become a Greek letter"! This

is like making the fact that it is not scientifically exact to say "the sun rises" a reason for denying that night ever turns to day. What is next added is still more amusing: "in fact, no letter ever *becomes*; people pronounce letters; and they either pronounce them properly or improperly." Very true; but we need a definition of "properly." It might have become proper for the Greeks to say *th* where it had been proper for their ancestors to say *gh* in the word in question: just as it is proper for us to pronounce *laugh* with a final *f*, though evidence of all kinds proves that our ancestors pronounced instead a guttural spirant, not less distinctly, and with equal propriety. And if any one chose to say that in such words (numerous enough) a more original *gh* had "become" *f*, who would have the right to criticise him? Our author is welcome to "assume that in an earlier or, as it is now called, a pre-historic state of the Greek language the pronunciation [of *thermos*] fluctuated or hesitated between" guttural and dental, provided he will not insist on transferring this fluctuation back to the "Proto-Aryan" period, where all evidences show that it did not exist. Cannot such a fluctuation, followed by an out-and-out shift of pronunciation, arise in the course of linguistic history, instead of being necessarily primitive? and, if not, why not? We are taught further on (p. 198) that the French dialectic pronunciation of *mékier* and *moikié* for *métier* and *moitié*, and their like, is a real "becoming" of *t* to *k*; why not, then, as well in Doric *poka* for *pote*, and so on (p. 188, note), which he wants to make out an original phenomenon? This

whole chapter on "Phonetic Change" (ch. iv. of vol. ii.), which is in great part re-written, is one of the most remarkable examples of wrong or half-wrong premises and false conclusions from them that the literature of linguistics has to show.

GRIMM'S LAW.

But the height of the argumentation is not reached until the next chapter, on "Grimm's Law," where the whole extremely complicated set of phenomena popularly included under that name are also attributed to a sort of Proto-Aryan "fluctuation or hesitation" of utterance. But it is of a different order. Whereas we were taught in the preceding chapter that the Proto-Aryans did not well distinguish *k* and *t*, here we find that they not only made this distinction clearly, but (ii. 232) that "some of them, at all events, had elaborated a threefold, if not a fourfold, modification of the consonantal checks"—the Greeks, for example, a *t*, *th*, and *d*, and the Sanskrit-speakers a *t*, *th*, *d*, and *dh*—all "for the sake of distinguishing a number of roots which they required in their intellectual intercourse." The later recorded Greek language has no *dh*; hence, we are taught, "from the very beginning" *th* appeared in its place. The Romans, on the same evidence, "never" had either *th* or *dh*, and hence "it is clear that in their language the distinctions so carefully elaborated at first, and so successfully kept up in Sanskrit and Greek, would be lost." One gets hopelessly confused among these "from the very beginnings" and "nevers" and "at firsts." If a triple, or a quadruple, distinction was

really made "at first," and has only been "kept up" in Sanskrit and Greek, can the Romans "never" have had it, while yet they have "lost" the distinctions conveyed by it? And, if they have lost *th* and *dh*, might not Greek also have lost *dh* in the same way? On the next page (234) it is made yet clearer that the Romans, though they "never" had aspirates, had once distinguished the two roots *dā* 'give' and *dhā* 'put;' and we are even taught to admire their "good sense," in that, "when they felt that they could not efficiently keep the two roots apart," they frankly let one go, and used something else instead. This "feeling" and consequent action comes dangerously near, once more, to that sort of "convention" of which, as gratuitously ascribed by him to his opponents, our author in his first volume so strongly reprobated the assumption.

But the same convention shows yet more strikingly further on. The sturdy Germanic tribes (p. 235) did not "submit to" the losses of their aspirates "without an effort to counteract them." Though "without real aspirates, yet, in taking possession of the phonetic inheritance of their Aryan . . . forefathers, they retained the consciousness of the threefold variety of their consonantal checks, and they tried to meet this threefold claim as best they could." And "hence," where Sanskrit had *dh* and Greek *th*, the Gothic (like Latin, Celtic, etc.) "preferred" a *d*, and High-German a *t*: that is, for example, the root *dhā* was in Gothic *dā*, and in High-German *tā*. But, not only did "none [of these] borrow from the other" (a thing which no one ever supposed or could suppose), but

also "none was before or after the other; all four . . . must be taken as dialectic varieties of one and the same type." This last is the cardinal feature of our author's system: the contemporaneousness of all dialectic forms; and he insists upon it wherever occasion offers: for example, at ii. 183, where we are taught that Lat. *tres* and Goth. *threis*, or *duo* and *twai*, or our *door*, Gr. *thyra*, and Lat. *foris*, "are parallel, not successive forms, and no one can say which was before or after the other." Accordingly, the four forms *dhā*, *thā*, *dā*, and *tā* of the root *dhā* were coexistent and equally primitive, and the root *dā* also must have had at the same time the same four forms: and how the Proto-Aryans managed to understand one another is a question crying for solution.

The "good sense" of the Romans, who, when they had lost the distinction of *dhā* from *dā*, let it go and said something else, saved them from endless throes which the more stubborn Germanic races had to suffer. There were also roots and words beginning "in all the Aryan languages" with *d*—and yet the root *dar* 'tear,' our author's chosen example, must, it would seem, have had the same four coexistent and not successive forms as *dhā*, among which speakers had only to select according to their dialectic preference. He goes on (p. 235) to explain: "What could the Goths . . . do? they had really robbed Peter to pay Paul, . . . had spent their sonant checks [as *d*] to supply the place of the aspirates [as *dh*]." Yet their *d*'s of the kind now used in *da* for *dhā* "could not be allowed to run together and be lost" by confusion with the other kind of *d*'s in *dar* 'tear' and its like.

Accordingly, "the Goths, guided by a wish to keep distinct what must be kept distinct, fixed" these latter *d*'s as *t*'s; and so they, as we do, said *tar* 'tear' instead of *dar*. And, having thus further robbed Paul to pay John, when "the same pressure" was "felt once more" in reference to the words that had an original (though coexistent with the other three varieties) *t* in them, they "were driven to adopt the only remaining expedient," and employed instead of *t* "the corresponding surd spirant . . . *th*." Surely, rather than go through all this, the Goths might well have submitted quietly to be conquered by the Romans, whose "good sense" had been shown in burying their dead consonants when they were dead, and making no further fuss about them.

If it appears impossible that the operations here described should have been carried on without a "convention," and considerable lively debate, the High-German performances still more evidently demand it. Let us read the whole story (p. 236): "The High-German tribes passed through nearly the same straits. What the Greeks took for surd aspirates [as *th*] they had taken for surd tenues [as *t*]. Having spent their . . . *t*'s, they were driven to adopt the spirants, . . . *s*'s, as the second variety; while, when the third variety came to be expressed, nothing remained but the mediæ [as *d*]." And that is why Sanskrit *tad*, our *that*, is in German *das*. With a bit of imagination, one can realize their perplexity; can see them scratch their heads and swear a little, vote that they will neither give up their

precious three-fold distinction nor retrace the first fatal step of change, and finally, under the guidance of some prehistoric Grimm or Müller, work their way out of the confusion.

This we are expected to receive as an explanation of the phenomena of the *Lautverschiebung*! There have been a great many explanations proposed before, and some of them of a very high degree of eminence in their particular provinces of absurdity; but it may fairly be questioned whether this is not at the head of them all; certainly, none proceeding from an equally noted source can rival it. We painfully miss, however, one feature (omitted in this edition) which its earlier versions contained—the comparison with the Isle of Man, which has three promontories, looking respectively at England, Scotland, and Ireland, like a Goth or a High-German at his *t*, *th*, and *d*; it was a comparison really admirably adapted to the character of the argument it was introduced to illustrate.

We might feel tempted to draw out a scheme of the linguistic principles involved in our author's reasonings, but it is safer to refrain; one would run the risk of dizziness, or even of a permanent obfuscation of the logical faculty. The grand central principle, perhaps, is what we may call the eternal generation of dialects; nothing in known languages (with certain exceptions, which our author does not give us the secret of) is so because it has "become" so in the course of linguistic development; it was there "from the very first."

PHONETIC DECAY AND DIALECTIC REGENERATION.

It is on the basis of such principles as are here involved that our author founds his distinction of "phonetic decay" and "dialectic regeneration" (i. 47 ff.), or of "phonetic change" and "dialectic growth" (ii. 183 ff.), on which he lays much stress. The distinction is of no value, and hence the nomenclature will never be accepted as part of the language of science. All languages are dialects, and hence whatever goes on in language, destructive or constructive, goes on in dialects; phonetic decay or change is not less dialectic than any thing else; it is linguistic, and growth is linguistic. Nor can any line of demarcation be drawn between phonetic change and growth; the one and the other are part of the same process. Some changes result more regeneratively than others; each change must be followed up along its own history, and estimated according to its own effect.

ON WORDS FOR 'FIR,' 'OAK,' AND 'BEECH.'

In the sequel of the discussions we have here been reviewing occurs the (as we may fairly call it) noted excursus "on words for fir, oak, and beech," which we cannot help being in no small degree astonished at finding at full length and unmodified in this finally revised edition. The repetition fairly challenges and provokes a repetition, as briefly as possible, of the exposure already made of its unsound and fantastic character.*

* See *Transactions of the American Philological Association* for 1876, p. 73 ff.

That Lat. *fagus*, Germ. *boka* signify 'beech,' while Gr. *phēgos* means 'oak', was an obvious and familiar fact; but that Lat. *quercus* 'oak' is the same with Germ. *foraha*, our 'fir,' was, it is believed, left for our author's ingenuity to bring to light. Here were two words that had curiously changed their meaning respectively from 'fir' to 'oak' and from 'oak' to 'beech' (or else the contrary); why had this taken place? Now our author chanced to read in Lyell that in Denmark, during the latest geological period, ending with the present, there had been a succession of forests, first fir, then oak, then beech: "the oak has in its turn been almost superseded by the common beech." He proceeded, accordingly, to connect the two phenomena, putting forward the latter as the cause of the former, and founding on the connection certain far-reaching conclusions as to the period of arrival in Europe of the tribes speaking languages of our family. This was all very ingenious and entertaining; but unfortunately it was only that; for the theory involves not less than three obviously fatal fallacies:

1. That the succession of forest-trees in a limited locality in northern Europe, one tree superseding another, and the beech being left as victor, has been true in general of Europe, including especially Italy, where the change of *quercus* from 'fir' to 'oak' is found.

This objection, as formerly made, our author* professed to understand as merely an echo of his own expressed desire for a scientific investigation of the facts; but in all these twenty-nine years he has not suc-

* "In Self-defense," in *Chips* etc., vol. iv.

ceeded in finding any testimony to convince him that in Italy all other trees have not been "superseded by the common beech." He has not even been willing to draw a philologist's natural conclusion from the circumstance that words for 'fir,' 'oak,' and 'beech,' all the three, are found in every European language.

2. That the supersession of the meaning 'fir' by that of 'oak,' for example, in a name can be in any way coördinated with the supersession of the one tree by the other in a district or region.

Such coördination is even absurdly impossible. The latter process must have been a very long-drawn one (Lyell tells of "at least 4000 years," with the possibility of "a number of centuries . . . four times as great"), and during many generations the new oaks must have been struggling on equal terms with the old firs; and meanwhile there must have been in use names for both trees, the old name for 'fir' dying out with the firs, and the new name for 'oak' surviving with the oaks. This is too clear to be disputed; nor does our author try to bring up anything against it.

3. That a people would have changed the sense of *quercus* in order to obtain a name for 'oak,' when, according to the terms of our author's theory itself, they had already the name *fagus* 'oak,' which they later proceeded to change to 'beech'—by the impossible process above described, when the beeches superseded the oaks—as they did not. To this also he has nothing to oppose.

Certainly, any man who can put one and one together to make two must see that our author's genial theory never had a foot to stand upon. The apparent

correspondence was simply an amusing coincidence, a proper subject for jocose exposition over a dinner-table, but not serious enough for a meeting of a philological society, even a local one. What shall we think of the would-be founder of a new science who gives it a dozen pages in his text-book, prints it in edition after edition, and saves every word of it in his final revision—and that, too, though it met long ago the thorough refutation which he himself, at the end of his exposition of it, professed himself ready to welcome? How easy it would have been for him, when he found himself incapable of answering the objections raised, to drop out the whole little discussion, which was never anything but an excrescence on his work!

THE SPOKEN ALPHABET.

There is also another section of the second volume which might to decided advantage have been omitted in this revised edition, namely that on the vocal organs and the modes of formation of the different alphabetic sounds (or, as he calls them, letters), with the cuts by which the latter are illustrated. There was more excuse for introducing this material in 1863, when comparatively little had been done in phonetics, and that little was comparatively inaccessible to English readers; but it is all at present so thoroughly antiquated that its reproduction is an error. Especially those impossible pictures of positions of utterance, with their exaggerated cavernous mouth-cavities, and their totally inaccurate tongue-postures, are nothing better than caricatures; one has only to compare them with other depictions of the same positions, such as Melville.

Bell's, to see how incapable they are of teaching anything. Whoever is to instruct others on phonetic subjects must as a first requisite have a pretty clear and correct idea, at least so far as concerns matters of prime importance, of what goes on in his own mouth in the processes of utterance; yet we find our author, after near thirty years of added experience and observation, still declaring (ii. 130), as he declared in 1863, that, "if I could trust my own ear, I should say that this vowel [i. e. the neutral vowel, that in *but*, *son*, *blood*, etc.] was always pronounced with non-sonant or whispered breath; that it is in fact a whispered, not a voiced, vowel." It is not too much to say of a person who is able to make that statement that nothing which he brings forward on phonetic subjects can be of any degree of authority, or of value save by accident. It is as if we had a mathematical treatise from one who in the course of it declared that he could never really convince himself that six and six do not make sixteen, or a zoölogical discussion from one who confessed that in his private opinion a bat is a species of butterfly. The whole phonetic exposition is, in fact, what might be expected from such an authority; truth, error, and absurdity are inextricably intermingled in it. Only one whose antecedent ignorance is great can learn anything from it; and even he, if he trusts it, will be taught more confusion and error than valuable knowledge.

VIVARASVASAGHOSHAI AND SAMVARANADAGHOSHAI.

At the end of this chapter, the author introduces a bit of blundering pedantry, to which we may direct a

moment's attention. In reviewing and summing up the classification of alphabetic sounds (ii. 163), he says of certain sounds : "these are called surd letters or non-sonant (psila, tenues, hard, sharp ; vivārasvāsāghoshāḥ ; . . . these are called sonant letters (mesa, mediæ, soft, blunt [he means "flat"]); samvāranādaghoshāḥ)." The addition of these long Sanskrit compounds to the two lists of synonyms it is impossible to regard as otherwise than pedantic, since they must be unknown and unintelligible to any one but a scholar specially versed in the Sanskrit phonetic literature (if not also to him); nothing that precedes has prepared the way for them, or furnished any ground for their citation; but, on the familiar principle of *omne ignotum pro magnifico*, they give to our author's closing page that show of profundity which belongs to obscurity, and help him to "split the ears of the groundlings." In his striving after this result, however, he has committed an oversight, and taken matter unsuited to his purpose; for the two compounds in question are not descriptive epithets at all; they are only fortuitous lists of characteristic qualities, put together as in Sanskrit one puts together into one word 'hands-and-feet,' or 'gods-and-men-and-demons,' or the like: the one means 'opening-and-breath-and-non-sonance,' the other 'closure-and-tone-and-sonance.' It is as if one were to give, as synonyms of *devil*, "Satan, Old Nick, Evil One; horns-and-hoofs-and-tail." The matter is recurred to here because, to a former criticism to the effect that no such "terms" as these were to be found "used by any Sanskrit grammarian," our author made

what doubtless seemed to most of his readers a triumphant answer, by merely pointing out where the compounds were to be read, without at all explaining what they were; possibly he again overlooked his mistake and simply repeated it, instead of ignoring and hiding it intentionally.* At any rate, the two false synonyms are still to be read in his pages.

KING FROM SANSKRIT JANAKA.

There are other weak points in our author's Sanskrit as exhibited in these volumes, which may be passed without remark, inasmuch as it is fundamental and important principles, and not minor details, that are engaging our attention throughout this criticism. One etymology, however, calls for brief notice, because it involves more than a superficial error, and because it is calculated to mislead ethnological inquirers. It is (ii. 322-3) the explanation given of our word *king*, as by origin identical with the alleged Sanskrit *janaka*. The insuperable obstacle in the way of this etymology is that in earlier Sanskrit not only is there no such word as *janaka* "procreator, parent, then king" (p. 322), but even there is no such mode of formation as a word like this implies. In more modern or classical Sanskrit are found a number of *quasi*-primary suffixes, freely used to make derivatives directly from roots, which nevertheless the history of the language plainly shows to be composite, produced by the recent addition of a secondary suffix to a pri-

* See further *Proceedings of the American Oriental Society* for May, 1876 (*Journal*, vol. x., p. cxxviii).

mary ; conspicuous examples are the gerundive-making suffixes *tavya* and *anīya*, of which the former is *tu + ya*, and the latter *ana + īya*.* Of this kind is also the alleged primary suffix *aka* ; it is *a + ka* ; and a word like *janaka* (itself only a later Sanskrit word) is not produced by adding a primary and pre-historic *aka* to a root *jan*, but, in the traceably historic period of the Sanskrit itself, long subsequent to our first acquaintance with it, by adding the secondary adjective-suffix *ka* to the really old noun *jana*. If our author had taken the trouble to look at the indexes by final letters and elements to the Rig-Veda and the Atharva-Veda, he would have seen that they contain almost no words in *aka*, and none at all having the character postulated by him for *janaka* ; and this might have set him upon the track of the true history of the apparent suffix. His etymology of *king* is not simply false (any scholar, even the best, is liable to make a false etymology) ; it is also discreditable, since it involves such a palpable anachronism as no sound scholar should be guilty of. It is as if one were to take our suffix *ness* as unitary and primeval, and attempt to explain by it general Indo-European words.

THE TRANSLATION OF THE RIG-VEDA.

It may be well finally to glance at the laudatory notice of his own fragment of a translation of the Rig-Veda which our author has added (ii. 520) in this last revision of his book on language. He does not, indeed, repeat here his (considering his many pre-

* See further *Transactions of the American Philological Association* for 1884, p. 118 ff.

decessors) preposterous and offensive claim, made in the work itself, that this is the "first translation of the Rig-Veda;" but he claims it as a "specimen of . . . the only scientific method," and asserts that "whoever means to produce a really satisfactory translation will have to follow my method." The method consists in this: "we must collect all the passages in which the same word occurs—this I have done in my Index Verborum [he forgets to mention that it had been done before him, and on a much better and more helpful plan, by Grassmann]—and we must then try to discover a meaning that will fit all the passages in which the same word occurs." As if this were "my" method, and not rather the method, either implicitly or explicitly followed, of every serious translator! No man can be saved merely by the adoption of this universal method, but only by the degree of skill and insight with which it shall be applied. Our author appears, too, to have forgotten that in his volume itself the specific claim to being a "first translation" was founded rather on the translation's being accompanied by notes which should make it plain just why every word was translated as it was—whence the *exiguum* of twelve hymns in a whole volume of apparatus, or on the scale of seventy-five volumes for the entire Veda. But to make this claim was to claim an utter impossibility; its fulfilment would have called for a volume to a hymn instead. And it was pointed out at the time* that, in the very first verse of his translation, while he had squandered eleven

* See *North American Review*, vol. cxiii. (July, 1871), p. 177.

pages of useless notes on the adjective 'red' (after all, too, probably mistranslating it, by supplying the wrong noun for it to agree with), he had also assumed a grammatical solecism (the use as nom. pl. of a form which could properly be only accus. pl. or gen. or abl. sing.) without spending a word of explanation upon it, and had made thereby an unacceptable version. When this was brought to his attention, he asserted in his defense,* with no little heat and indignation, that (in this translation which was to be the "first" in virtue of accounting for everything) he had of set purpose left all grammatical points to be treated by Benfey, who was known to have "been for years preparing a grammar of the Vedic dialect." This plea can hardly have appeared to any sensible reader as aught but a subterfuge, since no mention had been anywhere made of Benfey, or of a plan of leaving out to others parts of the work which our author professed himself to have undertaken (he might, under such a system, have left 'red' to somebody's lexicon); and also since a Vedic grammar could, at the utmost, only have pointed out the solecism in question as not entirely without analogies elsewhere, and could not have required its assumption in this particular case. But how treacherous a support to lean on Benfey would have been will be best appreciated by noticing that he had already (in his *Orient und Occident*, vol. i., p. 13) translated the verse, and not in accordance with our author, but understanding the disputed form as normally used (as accus. pl.; the critic had preferred

* "In Self-defense," *Chips* etc., vol. iv.

to take it as abl. sing.)* Our author has since shown his hand as a translator of the Rig-Veda in his volumes of Upanishads in the *Sacred Books of the East*, where verses from it occur here and there; and they are rendered with an altogether reprehensible inaccuracy, going so far in one case (Vol. i., p. 229; Rig-Veda viii. 76. 12) as to treat as of the third person a first singular verb-form which has its subject *aham* 'I' expressed in full beside it. He has yet to show that he can translate the Rig-Veda in a manner corresponding with the present stage of advance of Vedic scholarship.

GENERAL CHARACTER OF THE WORK.

It would not (as was pointed out at the beginning) have been in the least worth while to submit this work in its renewed form, and with its renewed lease of life, to a renewed critical examination, but for its exceptional vogue and popularity. It is probably the work which is most read, and oftenest quoted in discussions, whether anthropological or ethnographical, into which language enters as an important element. If, therefore, its facts are untrustworthy and its reasonings wrong, its influence is in a high degree damaging to the study of language and of everything concerned with language. That such is indeed its character, that it is unsound in every part, most of all in its fundamental doctrines, but in varying degree from the bottom to the top, has been abundantly demonstrated

* This point was first brought out by the late Dr. Ezra Abbot, in the *Bibliotheca Sacra* (Andover, Mass.) for July, 1877, vol. xxxiv., p. 560.

in the foregoing pages, and might be further shown at indefinite length, since there is nothing in the work essentially different from what we have been reviewing. It is, in fact, no scientific work, and the name of "science" should not appear in connection with it; it ought to be called by some such name as "Facts and fancies in regard to language and other related subjects." Science implies some system of presentation, some consistency of views, some coherence of reasoning; and all these qualities are conspicuously wanting here. The book is not science, but literature. Taken as literature, it is of high rank, as the admiration of the public sufficiently testifies; its author has a special gift for interesting statement and illustration, for lending a charm to the subjects he discusses; and he carries captive the judgments of his hearers and of many of his readers. He is a born *littérateur*; as to what he is toward science, that is sufficiently shown by his ambition to see it governed by the authority of the philosophers; he has thought to stop Darwinism by quoting Kant against it. With what he is in other departments of anthropological study we have nothing to do here; as regards language, he lacks the clearness of apprehension which would enable him to maintain anything. To consistency, indeed, he would doubtless make no pretense; in his genial way he approaches a subject from one side, and presents one lively view of it; then he approaches it from another side, and presents another view; how the two views stand related to one another is no concern of his. But, more than that, he is most curiously deficient in the logical faculty; if he hap-

pens to hold a right view, or to state a true fact, the chance is small that he will give a right reason for the one or draw a true inference from the other ; one can never be certain that he will not declare two and two to make three and a half, or five, or even six or more ; and when he expressly sets out to place two things in their correct mutual relations, or to prove something by a process of argument, then he makes his most conspicuous failure, and produces what his readers can only smile at. His volumes are valuable to one who knows how to use them properly ; by the wide interest which they have excited, and the numerous readers they have found, they have done much to initiate a taste for language-study ; to many who were wholly ignorant of the subjects they dealt with they have taught a great deal, and sometimes given the desire to learn more ; the more than compensating disadvantage is that so many have accepted them as authoritative. No man can do that with impunity ; no investigator in any department of anthropology can use their views as the basis of his reasonings and arrive at any valuable results ; it would be easy to give striking examples of failures thus brought about. Their author must not be taken seriously by one who would learn from him ; the incitement he brings must be an incitement to challenge his facts, compare and correct his reasonings, and question and refute his conclusions. The more the work is handled as a student in logic handles a collection of fallacies gathered for him to correct, the more profit will it yield.

PERSONAL.

I have myself—to drop now the impersonal form of statement—been constrained from the first publication of the work to treat it in the way here described. Any one to whom the facts and principles of language-study were in some measure known must have felt sharply antagonized when, at the appearance of this new and popular manual, he found that study declared, on transparently false grounds, to be a physical science, and language an existence which man had no part in making and changing; dialectic growth misunderstood, families of language regarded as exceptional, and a “Turanian” barathrum arranged to catch all little-known varieties of speech; antecedent unity of dialect taught in one case and denied in another; a word held to be killed by the least mispronunciation; *conventional* explained to mean ‘voted by a convention;’ thought and its expression viewed as inseparable, and even identical; the origin of language seemingly ascribed to an instinctive ding-dong of the tongue—and so on; to complete the list would be almost to give a table of principal contents of the two volumes—and a style of discussion used throughout which indicated that the author was playing with his subject rather than investigating it seriously. Then and there, accordingly, began my opposition to the Müllerian school of linguistics; and he has never allowed me to relax it, much as I have desired and sought to find opportunity to agree with and commend his doctrines. It is questionable whether I should myself ever have written a work on the general subject of language if I had not been driven to it by what

seemed to me the necessity of counteracting, as far as possible, the influence of such erroneous views. That my criticisms, so far as he deigned to pay them any attention (and he abundantly showed and shows that he has not heeded them enough to derive any benefit from them), were agreeable to him I never imagined; nor did I suppose that he would fail to resent them as he was best able. To one living in such an atmosphere of adulation as has been his environment for the past thirty years (fit to sap the vigor of a stronger nature than his), and who has established so tyrannical a sway in British public opinion that even those most opposed to him hardly dare to raise a voice in public against him,* it may well enough have seemed that I was playing Mordecai to his Haman; and when at length, in 1875-6 (in the articles "My Answer to Mr. Darwin" and "In Self-defense," in *Chips* etc., vol. iv.), he turned upon me in a determined effort to crush me, I had no reason to be surprised. Nor should I have had any ground to complain of merely harsh and unsparing treatment; and I would have not only accepted with gladness, but also done public penance for, any injustice which he proved me to have committed by misrepresentation or unfounded criticism of his views. Nothing of this kind that was worthy of the name, however, was furnished, as may be suffi-

* It remains to be seen whether any literary paper in England will let its readers learn of Böhlingk's exposure (in *F. Max Müller als Mythendichter*, St. Petersburg, 1891) of his astounding misstatements respecting the Petersburg Academy and the edition of the Rig-Veda, as made in his recent volume, *Natural Religion*.

ciently gathered from the three specimens that are reported above (the words for 'fir,' 'oak,' and 'beech,' p. 65 ff. ; *vivārasvāsāghoshāh* etc., p. 69 ; and the first verse of the Rig-Veda translation, p. 73) ; and I was fully authorized to continue in the belief that he had no real defense to make. A virulent counter-attack, too, endeavoring to make points against me in return, was to be expected ; and I could only congratulate myself on the utterly trifling character (even granting their truth) of the pile of "principal bones of contention" between us, as he called them, which he managed to rake together out of nooks and corners, and solemnly proposed to allow an international tribunal to sit upon. What I had more right to object to was the uncandor and misrepresentation by which these "bones" were in good part gathered, and the attribution to me of the worst personal motives. Yet in this also I did not suffer alone ; Müller has never been known either as a fair fighter or as an accurate reporter of facts, and I could only look to receive from him the treatment which I saw others receive. And by the public at large I shall doubtless be finally judged according to the general character of what I have done and written. At any rate, now as heretofore, I rest my defense on not the just intent alone, but the real substantial justice of my criticisms ; if they are unfounded, I deserve reprehension for making them ; if they are right, then there is nothing, either in the degree of importance of the subjects to which they relate, or in the personality against whom they are directed, to call for their condemnation.

