X.

IN SELF-DEFENCE.

Present State of Scientific Studies.

IT has been remarked by many observers that in all branches of physical as well as historical learning there is at the present moment a strongly pronounced tendency towards special researches. No one can hold his own among his fellow-workers who cannot point to some discovery, however small, to some observation, to some decipherings, to some edition of a text hitherto unpublished, or, at least, to some conjectural readings which are, in the true sense of the word, his property. A man must now have served from the ranks before he is admitted to act as a general, and not even Darwin or Mommsen would have commanded general attention for their theories on the ancient history of Rome, or on the primitive development of animal life, unless they had been known for years as sturdy workers in their respective quarries.

On the whole, I believe that this state of public opinion has produced a salutary effect, but it has also its dangers. An army that means conquest, cannot always depend on its scouts and pioneers, nor

must it be broken up altogether into single detachments of tirailleurs. From time to time, it has to make a combined movement in advance, and for that purpose it wants commanders who know the general outlines of the battle-field, and are familiar with the work that can best be done by each branch of the service.

Evolutionism.

If we look upon scholars, historians, students of physical science, and abstract philosophers, as so many branches of the great army of knowledge which has been fighting its way for centuries for the conquest of truth, it might be said, if we may follow up our comparison a little further, that the light cavalry of physical science had lately made a quick movement in advance, and detached itself too much from the support of the infantry and heavy artillery. The charge was made against the old impregnable fortress, the Origin of Life, and to judge from the victorious hurrahs of the assaulting squadron, we might have thought that a breach had at last been effected, and that the keys to the long hidden secrets of creation and development had been surrendered. As the general commanding this attack, we all recognise Mr. Darwin, supported by a brilliant staff of dashing officers, and if ever general was well chosen for victory, it was the author of the 'Origin of Species.'

There was indeed for a time a sanguine hope, shared by many a brave soldier, that the old warfare of the world would, in our time, be crowned with success, that we should know at last what we are, whence we came, and whither we go; that, beginning with the simplest elementary substances,

we should be able to follow the process of combination and division, leading by numberless and imperceptible changes from the lowest Bathybios to the highest Hypsibios, and that we should succeed in establishing by incontrovertible facts what old sages had but guessed, viz. that there is nowhere anything hard and specific in nature, but all is flowing and growing, without an efficient cause or a determining purpose, under the sway of circumstances only, or of a self-created environment. Πάντα ῥεῖ.

But that hope is no longer so loudly and confidently expressed as it was some years ago. For a time all seemed clear and simple. We began with Protoplasm, which anybody might see at the bottom of the sea, developing into Moneres, and we ended with the bimanous mammal called *Homo*, whether *sapiens* or *insipiens*, everything between the two being matter of imperceptible development.

Difficulties in Evolutionism.

The difficulties began where they generally begin, at the beginning and at the end. *Protoplasm* was a name that produced at first a soothing effect on the inquisitive mind, but when it was asked, whence that power of development, possessed by the protoplasm which begins as a Moneres and ends as Homo, but entirely absent in other protoplasm, which resists all mechanical manipulation, and never enters upon organic growth, it was seen that the problem of development had not been solved, but only shifted, and that, instead of simple Protoplasm, very peculiar kinds of Protoplasm were required, which under circumstances might become and re-

main a Moneres, and under circumstances might become and remain *Homo* for ever. That which determined Protoplasm to enter upon its marvellous career, the first κινοῦν ἀκίνητόν, remained as unknown as ever. It was open to call it an internal and unconscious, or an external and conscious power, or both together: physical, metaphysical, and religious mythology were left as free as ever. The best proof of this we find in the fact that Mr. Darwin himself retained his belief in a personal Creator, while Haeckel denies all necessity of admitting a conscious agent; and von Hartmann[1] sees in what is called the philosophy of evolutionism the strongest confirmation of idealism, 'all development being in truth but the realisation of the unconscious reason of the creative idea.'

Glottology and Evolutionism.

While the difficulty at the beginning consists in this that, after all, nothing can be developed except what was enveloped, the difficulty at the end is this that something is supposed to be developed that was not enveloped. It was here where I thought it became my duty to draw Mr. Darwin's attention to difficulties which he had not suspected at all, or which, at all events, he had allowed himself to undervalue. Mr. Darwin had tried to prove that there was nothing to prevent us from admitting a possible transition from the brute to man, as far as their physical structure was concerned, and it was natural that he should wish to believe that the same applied

[1] See a very remarkable article by von Hartmann on Haeckel, in the 'Deutsche Rundschau,' July, 1875.

to their mental capacities. Now, whatever difference of opinion there might be among philosophers as to the classification and naming of these capacities, and as to any rudimentary traces of them to be discovered in animals, there had always been a universal consent that language was a distinguishing characteristic of man. Without inquiring what was implied by language, so much was certain that language was something tangible, present in every man, absent in every brute. Nothing, therefore, was more natural than that Mr. Darwin should wish to show that this was an error: that language was nothing specific in man, but had its antecedents, however imperfect, in the signs of communication among animals. Influenced, no doubt, by the works of some of his friends and relatives on the origin of language, he thought that it had been proved that our words could be derived *directly* from imitative and interjectional sounds. If the Science of Language has proved anything, it has proved that this is not the case. We know that, with certain exceptions, about which there can be little controversy, all our words are derived from roots, and that every one of these roots is the expression of a general concept. 'Without roots, no language; without concepts, no roots,' these are the two pillars on which our philosophy of language stands, and with which it falls.

Mr. Wedgwood's Dictionary.

Any word taken from Mr. Wedgwood's Dictionary will show the difference between those who derive words *directly* from imitative and interjectional

sounds, and those who do not. For instance, s.v. *to plunge*, we read:

'Fr. *plonger*, Du. *plotsen*, *plonssen*, *plonzen*, to fall into the water, —Kil.; *plotsen*, also to fall suddenly on the ground. The origin, like that of *plump*, is a representation of the noise made by the fall. Swiss *bluntschen*, the sound of a thick, heavy body falling into the water. Under *plump* we read, 'that the radical image is the sound made by a compact body falling into the water, or of a mass of wet falling to the ground. *He smit den sten in't water, plump! seg dat*, He threw the stone into the water; it cried plump! *Plumpen*, to make the noise represented by plump, to fall with such a noise, etc., etc., etc.'

All this sounds extremely plausible, and to a man not specially conversant with linguistic studies, far more plausible than the real etymology of the word. To plunge is, no doubt, as Mr. Wedgwood says, the French *plonger*, but the French *plonger* is *plumbicare*, while in Italian *piombare* is *cadere a piombo*, to fall straight like the plummet. To plunge, therefore, has nothing to do with the splashing sound of heavy bodies falling into the water, but with the concept of straightness, here symbolised by the plummet.

This case, however, would only show the disregard of historical facts with which the onomatopoeic school has been so frequently and so justly charged. But as we cannot trace *plumbum*, or μόλυβος, or Old Slav. *olovo* with any certainty to a root such as *mal*, to be soft, let us take another word, such as *feather*. Here, again, we find that Mr. Wedgwood connects it with such words as Bav. *fledern*, Du. *vlederen*, to flap, flutter, the loss of the *l* being explained by such words as to splutter and to sputter. We have first to note the disregard of historical facts, for *feather* is O. H. G. *fedara*, Sk.

pat-tra, Gr. πτερόν for πετερον, all derived from a root *pat*, to fly, from which we have also *penna*, old *pesna*, πέτ-ομαι, *peto*, *impetus*, etc. The root *pat* expresses violent motion, and it is specialised into upward motion, πέτομαι, I fly; downward motion, Sk. patati, he falls; and onward motion, as in Latin *peto*, *impetus*, etc. Feather, therefore, as derived from this root, was conceived as the instrument of flying, and was never intended to imitate the noise of Du. *vlederen*, to flutter, and to flap.

My Lectures on Mr. Darwin's Philosophy of Language.

As this want of historical treatment among onomatopoeic philologists has frequently been dwelt on by myself and others, these instances may suffice to mark the difference between the school so ably and powerfully represented by Mr. Wedgwood, and the school of Bopp, to which I and most comparative philologists belong. It was in the name of that school that I ventured to address my protest to the school of evolutionists, reminding them of difficulties, which they had either ignored altogether, or, at all events, greatly undervalued, and putting our case before them in such a form that even philosophers, not conversant with the special researches of philologists, might gain a clear insight into the present state of our science, and form their opinion accordingly.

In doing this I thought I was simply performing a duty which, in the present state of divided and subdivided labour, has to be performed, if we wish to prevent a useless waste of life. However different

our pursuits may be, we all belong, as I said before, to the same army, we all have the same interests at heart, we are bound together by what the French would call the strongest of all solidarities, the love of truth. If I had thought only of my own fellow-labourers in the field of the Science of Language, I should not have considered that there was any necessity for the three Lectures which I delivered in 1873 at the Royal Institution. In my first course of Lectures on the Science of Language (1861), delivered before Evolutionism had assumed its present dimensions, I had already expressed my conviction that language is the one great barrier between the brute and man.

'Man speaks,' I said, 'and no brute has ever uttered a word. Language is something more palpable than a fold of the brain or an angle of the skull. It admits of no cavilling, and no process of natural selection will ever distil significant words out of the notes of birds or the cries of beasts.'

No scholar, as far as I know, has ever controverted any of these statements. But when Evolutionism became, as it fully deserved, the absorbing interest of all students of nature, when it was supposed that, if a Moneres could develop into a Man, Bow-wow and Pooh-pooh might well have developed by imperceptible degrees into Greek and Latin, I thought it was time to state the case for the Science of Language and its bearing on some of the problems of Evolutionism more fully, and I gladly accepted the invitation to lecture once more on this subject at the Royal Institution in 1873. My object was no more than a statement of facts, showing that the results of the Science of Language did not at present tally with the results of Evolutionism, that words could no

longer be derived directly from imitative and interjectional sounds, that between these sounds and the first beginnings of language, in the technical sense of the word, a barrier had been discovered, represented by what we call Roots, and that, as far as we know, no attempt, not even the faintest, has ever been made by any animal, except Man, to approach or to cross that barrier. I went one step further. I showed that Roots were with man the embodiments of general concepts, and that the only way in which man realised general concepts was by means of those roots, and words derived from roots. I therefore argued as follows: We do not know anything and cannot possibly know anything of the mind of animals: therefore, the proper attitude of the philosopher with regard to the mental capacities of animals is one of complete neutrality. For all we know, the mental capacities of animals may be of a higher order than our own, as their sensuous capacities certainly are in many cases. All this, however, is guesswork; one thing only is certain. If we are right that man realises his conceptual thought by means of words, derived from roots, and that no animal possesses words derived from roots, it follows, not indeed, that animals have no conceptual thought (in saying this, I went too far), but that their conceptual thought is different in its realised shape from our own.

From public and private discussions which followed the delivery of my lectures at the Royal Institution (an abstract of them was published in 'Fraser's Magazine,' and republished, I believe, in America), it became clear to me that the object which I had in view, had been fully attained. General attention had been

roused to the fact that at all events the Science of Language had something to say in the matter of Evolutionism, and I know that those whom it most concerned, were turning their thoughts in good earnest to the difficulties which I had pointed out. I wanted no more, and I thought it best to let the matter ferment for a time.

Mr. George Darwin's Article in the Contemporary Review.

But what was my surprise when I found that a gentleman who had acquired considerable notoriety, not indeed by any special and original researches in Comparative Philology, but by his repeated attempts at vilifying the works of other scholars, Professor Whitney, had sent a paper to Mr. Darwin, intended to throw discredit on the statements which I had recommended to his serious consideration. I did not know of that paper till an abstract of it appeared in the 'Contemporary Review,' signed George Darwin, and written with the avowed purpose of discrediting the statements which I had made in my Lecture at the Royal Institution. If Professor Whitney's appeal had been addressed to scholars only, I should gladly have left them to judge for themselves. But as Mr. Darwin, jun. was prevailed upon to stand sponsor to Professor Whitney's last production, and to lend to it, if not the weight, at least the lustre of his name, I could not, without appearing uncourteous, let it pass in silence. I am not one of those who believe that truth is much advanced by public controversy, and I have carefully eschewed it during the whole of my literary career. But if I had left Professor Whitney's

assertions unanswered, I could hardly have complained, if Mr. Darwin, sen., and the many excellent *savants* who share his views, had imagined that I had represented the difficulties which the students of language feel with regard to animals developing a language, in a false light; that in fact, instead of wishing to assist, I had tried to impede the onward march of our brave army. I have that faith in οἱ περὶ Darwin, that I believe they want honest advice, from whatever quarter it may come, and I therefore was persuaded to deviate for once from my usual course, and, by answering *seriatim* every objection raised by Professor Whitney, to show that my advice had been tendered *bona fide*, that I had not spoken in the character of a special pleader, but simply and solely as a man of truth.

My Answer to Mr. Darwin.

My 'Answer to Mr. Darwin' appeared in the 'Contemporary Review' of November, 1874, and if it had only elicited the letter which I received from Mr. Darwin, sen., I should have been amply repaid for the trouble I had taken in the matter.

It produced, however, a still more important result, for it elicited from the American assailant a hasty rejoinder, which opened the eyes even of his best friends to the utter weakness of his case. Professor Whitney, himself, had evidently not expected that I should notice his assault. He had challenged me so often before, and I had never answered him. Why, then, should I have replied now? My answer is, because, for the first time, his charges had been countersigned by another.

I had not even read his books before, and he blames

me severely for that neglect, bluntly asking me, why I had not read them. That is indeed a question extremely difficult to answer without appearing to be rude. However, I may say this, that to know what books one must read, and what books one may safely leave unread, is an art which, in these days of literary fertility, every student has to learn. We know on the whole what each scholar is doing, we know those who are engaged in special and original work, and we are in duty bound to read whatever they write. This, in the present state of Comparative Philology, when independent work is being done in every country of Europe, is as much as any man can do, nay, often more than I feel able to do. But then, on the other hand, we claim the liberty of leaving uncut other books in our science, which, however entertaining they may be in other respects, are not likely to contain any new facts. In doing this, we run a risk, but we cannot help it.

And let me ask Professor Whitney, if by chance he had opened a book and alighted on the following passage, would he have read much more?

'Take as instances *home* and *homely*, *scarce* and *scarcely*, *direct* and *directly*, *lust* and *lusty*, *naught* and *naughty*, *clerk* and *clergy*, a *forge* and a *forgery*, *candid* and *candidate*, *hospital* and *hospitality*, *idiom* and *idiocy*, *light*, *alight*, and *delight*, etc.'

Is there any philologist, comparative or otherwise, who does not know that *light*, the Gothic *liuhath*, is connected with the Latin *lucere;* that to *delight* is connected with Latin *delector*, Old French *deleiter*, and with Latin *de-lic-ere;* while to *alight* is of Teutonic origin, and connected with Gothic *leihts*, Latin *levis*, Sanskrit *laghus?*

But then, Professor Whitney continues, when at

last he had forced me to read some of his writings, why did I not read them carefully? Why did I read Mr. Darwin's article in the 'Contemporary Review' only, and not his own in an American journal?

Now here I feel somewhat guilty: still I can offer some excuse. I did not read Professor Whitney's reply in the American original, first, because I could not get it in time; secondly, because I only felt bound to answer the arguments which Mr. Darwin had adopted as his own. Looking at the original article afterwards, I found that I had not been entirely wrong. I see that Mr. Darwin has used a very wise discretion in his selection, and I may now tell Professor Whitney that he ought really to be extremely grateful that nothing except what Mr. Darwin had approved of, was placed before the English readers of the 'Contemporary Review,' and therefore answered by me in the same journal.

The Phenician Alphabet.

Other charges, however, of neglect and carelessness on my part in reading Professor Whitney's writings, I can meet by a direct negative. Among the more glaring mistakes of his lectures which I had pointed out, was this, that fifteen years after Rougé's discovery, Professor Whitney still speaks of 'the Phenician alphabet as the ultimate source of the world's alphabets.' Professor Whitney answers: 'If Professor Müller had read my twelfth lecture he would have found the derivative nature of the Phenician alphabet fully discussed.' When I read this, I felt a pang, for it was quite true that I had not read that lecture. I saw a note to it, in which Professor

Whitney states that the sketch of the history of writing contained in it was based on Steinthal's admirable essay on the 'Development of Writing,' and being acquainted with that, I thought I could dispense with lecture No. 12. However, as I thought it strange that there should be so glaring a contradiction between two lectures of the same course, that in one the Phenician alphabet should be represented as the ultimate source, in another as a derivative alphabet, I set to work and read lecture No. 12. Will it be believed that there is not one word in it about Rougé's discovery, published, as I said, fifteen years ago, that the old explanation that *Aleph* stood for an ox, *Beth* for a house, *Gimel* for camel, *Daleth* for door, is simply repeated, and that similarities are detected between the forms of the letters and the figures of the objects whose names they bear? Therefore of two things one, either Professor Whitney was totally ignorant of what has been published on this subject during the last fifteen years by Rougé, father and son, by Brugsch, Lenormant and others, or he thought he might safely charge me with having misrepresented him, because neither I nor any one else was likely to read lecture No. 12.

After this instance of what Professor Whitney considers permissible, I need hardly say more; but having been cited by him before a tribunal which hardly knows me, to substantiate what I had asserted in my 'Answer to Mr. Darwin,' it may be better to go manfully through a most distasteful task, to answer *seriatim* point after point, and thus to leave on record one of the most extraordinary cases of what I can only call Literary Daltonism.

Like and Unlike.

I am accused by Professor Whitney of having read his lectures carelessly, because I had only been struck by what seemed to me repetitions from my own writings, without observing the deeper difference between his lectures and my own. He therefore advises me to read his lectures again. I am afraid I cannot do that, nor do I see any necessity for it, because though I was certainly staggered by a number of coincidences between his lectures and my own, I was perfectly aware that they differed from each other more than I cared to say. I imagined I had conveyed this as clearly as I could, without saying anything offensive, by observing that in many places his arguments seemed to me like an *inverted fugue* on a *motive* taken from my lectures. But if I was not sufficiently outspoken on that point, I am quite willing to make amends for it now.

An Inverted Fugue.

I must give one instance at least of what I mean by an *inverted fugue*.

I had laid great stress on the fact that, though we are accustomed to speak of language as a thing by itself, language after all is not something independent and substantial, but, in the first instance, an act, and to be studied as such. Thus I said (p. 44):

'To speak of language as a thing by itself, as living a life of its own, as growing to maturity, producing offspring, and dying away, is sheer mythology.'

Again (p. 51):

'Language exists in man, it lives in being spoken, it dies with each word that is pronounced, and is no longer heard.'

When I came to Professor Whitney's Second Lecture, and read (p. 35):

'Language has, in fact, no existence save in the minds and mouths of those who use it,'

I felt pleasantly reminded of what I knew I had said somewhere. But what was my surprise, when a few lines further on I read:

'This truth is sometimes explicitly denied, and the opposite doctrine is set up, that language has a life and growth independent of its speakers, with which men cannot interfere. A recent popular writer (Professor Max Müller) asserts that, "although there is a continuous change in language, it is not in the power of man either to produce or to prevent it. We might think as well of changing the laws which control the circulation of our blood, or of adding an inch to our height, as of altering the laws of speech, or inventing new words according to our own pleasure."'

How is one to fight against such attacks? The very words which Professor Whitney had paraphrased before, only substituting 'skull' for 'height,' and by which I had tried to prove 'that languages are not the artful creations of individuals,' are turned against me to show that, because I denied to any *single* individual the power of changing language *ad libitum*, I had set up the opposite doctrine, viz. that language has a life and growth independent of its speakers.

Does Professor Whitney believe that any attentive reader can be taken in by such artifices? Suppose I had said that in a well-organised republic no individual can change the laws according to his pleasure, would it follow that I held the opposite doctrine, that laws have a life and growth independent of the lawgiver? The simile is weak, because an individual may, under very peculiar circumstances, change a law according to his pleasure; but weak as it is, I hope

it will convince Professor Whitney that Formal Logic is not altogether a useless study to a Professor of Linguistics. I only wonder what Professor Whitney would have said if he had been able to find in my Lectures a definition of language (p. 46), worthy of Friedrich Schlegel, viz.:

'Language, like an organic body, is no mere aggregate of similar particles; it is a complex of related and mutually helpful parts.'

And again:

'The rise, development, decline, and extinction of language are like the birth, increase, decay, and death of a living creature.'

In these poetical utterances of Professor Whitney's we have an outbreak of philological mythology of a very serious nature, and this many years after I had uttered my warning that 'to speak of language as a thing by itself, as living a life of its own, as growing to maturity, producing offspring, and dying away, is sheer mythology' (I. p. 44).

Repetitions and Variations.

It is, no doubt, quite natural that in reading Professor Whitney's lectures I should have been struck more forcibly than others by coincidences, which have reference not only to general arguments, but even to modes of expression and illustrations. I had pointed out some of these verbal or slightly disguised coincidences in my first article, but I could add many more. As we open the book, it begins by stating that the Science of Language is a modern science, that its growth was analogous to that of other sciences, that from a mere collection of facts it advanced to classification, and from thence to inductive reasoning on language. We are told that ancient nations considered the languages of their neighbours as merely

barbarous, that Christianity changed that view, that a study of Greek, Latin, and Hebrew widened the horizon of scholars, and that at present no dialect, however rude, is without importance to the students of the Science of Language. Next comes the importance of the discovery of Sanskrit, and a challenge for a place among the recognised sciences in favour of our new science.

Now I ask any one who may have read my Lectures, whether it was not very natural that I should be struck with a certain similarity between my old course of lectures on the Science of Language, and the lectures delivered soon after on the Science of Language at Washington? But I was not blind to the differences, and I never wished to claim as my own what was original in the American book.

For instance, when the American Professor says that one of the most important problems is to find out 'How we learn English,' I said at once, 'That's his ane;' and when after leading us from mother to grandmother, and greatgrandmother, he ends with Adam, and says:

'It is only the first man before whom every beast of the field and every fowl of the air must present itself, to see what he will call it; and whatever he calls any living creature, that is the name thereof, not to himself alone, but to his family and descendants, who are content to style each as their father had done before them,'

I said again, 'That's his ane.'

When afterwards we read about the large and small number of words used by different ranks and classes, and by different writers, when we come to the changes in English, the phonetic changes, to phonetics in general, to changes of meaning, etc., few, I think, will fail to perceive what I naturally perceived most

strongly, 'the leaves of memory rustling in the dark.' I perceived even such accidental reminiscences as,

Old Prussian leaving behind a brief catechism (p. 215), and,

Old Prussian leaving behind an old catechism (p. 227);

Frisian having a literature of its own (p. 211), and the

Frisians having a literature of their own (p. 203),

though, of course, no other reader could possibly perceive such unimportant coincidences. These, no doubt, were mere accidents; but when we consider that there is perhaps no science which admits of more varied illustration than the Science of Language, then to find page after page the same instances which one had collected oneself, certainly left the impression that the soil from which these American lectures sprang, was chiefly alluvial. Of course, as Professor Whitney has acknowledged his indebtedness to me for these illustrations, I have no complaint to make, I only protest against his ingratitude in representing such illustrations as mere by-work. For the purpose of teaching and placing a difficult subject into its proper light, illustrations, I think, are hardly less important than arguments. In order to show, for instance, in what sense Chinese may be called a *parler enfantin*, I had said:

'If a child says *up*, that *up* is to his mind, noun, verb, adjective, all in one. It means, I want to get up on my mother's lap.'

What has Professor Whitney to say on the same subject?

'It is thus that, even at present, children begin to talk; a radical word or two means in their mouths a whole sentence; *up* signifies "take me up into your lap."'

Enough of this, if not too much. Perhaps a thousand years hence, if any of our books survive so long, the question whether my Lectures were written by myself, or by an American scholar settled in Germany, may exercise the critical acumen of the philologists of the future.

Lectures Printed in England also.

But I see there is one more charge of carelessness brought against me, and as I promised to answer every one, I must at least mention it.

'He has not even observed that my Lectures are printed and published in England, and not only in America.'

Why I ought to have observed this, I do not understand. Would it have served as an advertisement? Should I have said that the author resided in Canada to secure his book against the imminent danger of piracy in England? Or does Professor Whitney suspect here too, one of those sinister influences which he thought had interfered with the sale of his books in England? However, whatever sin of omission I have committed, I am quite willing to apologise, in order to proceed to graver matters.

The Science of Language as one of the Physical Sciences.

I stand charged next not only with having read Professor Whitney's writings in too cursory a manner, but with actually having misrepresented his views on the question, so often discussed of late, whether the Science of Language should be reckoned one of the historical or one of the physical sciences. Let us look at the facts:

I had tried to show in my very first Lecture in what sense the Science of Language might properly be called a physical, and in what sense it might be called an historical science. I had given full weight to the arguments on either side, because I felt that, owing to the twofold nature of man, much might be said with perfect truth for one or the other view. When I look back on what I wrote many years ago, after having carefully weighed all that has been written on the subject during the last fifteen years, I am glad to find that I can repeat every word I then wrote, without a single change or qualification.

'The process,' I said (p. 43), 'through which language is settled and unsettled, combines in one the two opposite elements of necessity and freewill. Though the individual seems to be the prime mover 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. He can do nothing by himself, and the first impulse to a new formation in language, *though given by an individual*, is mostly, if not always, given without premeditation, nay, unconsciously. The individual, as such, is powerless, and the results apparently produced by him, depend on laws beyond his control, and on the co-operation of all those who form together with him one class, one body, or one organic whole. But though it is easy to show that language cannot be changed or moulded by the taste, the fancy, or genius of man, it is nevertheless through the instrumentality of man alone that language can be changed.'

Now I ask any reader of Mr. Whitney's Lectures, whether he has found in them anything in addition to what I had said on this subject, anything materially or even in form, differing from it. He speaks indeed of the actual additions made by individuals to language, but he treats them, as I did, as rare

exceptions (p. 32), and I cannot help thinking that when he wrote (p. 52):

'Languages are almost as little the work of man as is the form of his skull, the outlines of his face, the construction of his arm and hand,'

he was simply paraphrasing what I had said, though, as will be seen, far more cautiously than my American colleague, because my remarks referred to the laws of language only, not to language as a whole (p. 40):

'We might think as well of changing the laws which control the circulation of our blood, or of adding an inch to our height, as of altering the laws of speech, and inventing new words, *according to our own pleasure.*'

I cannot hope to convince Mr. Whitney, for after I had tried to explain to him, why I considered the question whether the Science of Language is to be classed as a physical or an historical science, as chiefly a question of technical definition, he replies:

'That I should probably consider it as more than a matter of terminology or technical definition whether our science is an historical science, *because men make language*, or a physical science, *because men do not make language.*'

Everybody will see that to attempt a serious argument on such conditions, is simply impossible.

If Professor Whitney can produce one single passage in all my writings, where I said that *men do not make language*, I promise to write no more on language at all. I see now that it is Schleicher who, according to Professor Whitney, at least, held these crude views, who called languages natural organisms, which, without being determinable by the will of man, arose, grew, and developed themselves, in accordance with fixed laws, and then again grow old and die out; who ascribed to language that suc-

cession of phenomena which is wont to be termed life, and who accordingly classed *Glottik*, the Science of Language, as a natural science. These are the very opinions which, with the exception of the last, are combated in my writings.

I understood perfectly well what Mr. Whitney meant, when he, like nearly all scholars before him, claimed the Science of Language as an historical or a moral science. Man is an amphibious creature, and all the sciences concerning man, will be more or less amphibious sciences. I did not rush into print, because he took the opposite side to the one I had taken. On the contrary, having myself laid great stress on the fact that language was not to be treated as an artful creation of the individual, I was glad that the artistic element in language, such as it is, should have found so eloquent an advocate. But I confess, I was disappointed when I saw that, with the exception of a few purely sentimental protests, there was nothing in Mr. Whitney's treatment of the subject that differed from my own. I proved this, if not to his satisfaction, at least to that of others, by giving *verbatim* extracts from his Lectures, and what is the consequence? As he can no longer deny his own words, he uses the only defence which remained, he now accuses me of garbling quotations and thus misrepresenting him. This, of course, may be said of all quotations, short of reprinting a whole chapter. Yet to my mind the charge is so serious, that I feel in duty bound to repel it, not by words, but by facts.

This is the way in which Professor Whitney tries to escape from the net in which he had entangled himself. In his reply to my argument he says:

'He chooses even more than once a sentence, in order to prove that I maintain an opinion, directly from an argument in support of the opposite opinion; for instance, in quoting my words, "that languages are almost as little the work of man as is the form of his skull," he overlooks the preceding parts of the same sentence: "*as opposed* to the objects which he, the linguist, follows in his researches, and the results which he wishes to attain." The whole is a part of a section which is to prove that the absence of reflection and conscious intent, takes away from the facts of language the subjective character which would otherwise belong to them as products of the voluntary action.'

Very well. We now have what Professor Whitney says that he said. Let us now read what he really said (p. 51):

'The linguistic student feels that he is not dealing with *the artful creations of individuals*. So far as concerns the purposes for which he examines them, and the results he would derive from them, they are almost as little the work of man as is the form of his skull.'

To render 'so far as concerns the purposes' by 'Gegenüber den Zwecken, die er bei seinen Untersuchungen verfolgt,' is a strong measure. But even thus, the facts remain as I, not as he, had stated them. There was no garbling on my part, but something worse than garbling on his, and all this for no purpose whatever, except for one which I do not like to suggest. As a linguistic student Professor Whitney feels what I had felt, 'that we are not dealing with the artful creations of individuals.' What Professor Whitney may feel besides about language, does not concern us, but it does concern us, and it does still more concern him, that he should not endeavour to impart to scientific language that character which, as he admits, it has not, viz. that of being the very artful creation of an individual.

I am quite willing to admit, and I have done so before on several occasions, that I may have laid

too great stress on those characteristics of the Science of Language by which it belongs to the physical sciences. I have explained why I did so at the time. In fact these are not new questions. Because I had said, as Dr. Whewell had said before me,

'That there are several large provinces of speculation which concern subjects belonging to man's immaterial nature, and which are governed by the same laws as sciences altogether physical,'

it did not follow, as Professor Whitney seems to think, that I regarded language as something like a cow or a potato. I cannot defend myself against such puerilities.

In reviewing Schleicher's essay, 'On Darwinism tested by the Science of Language,' I had said:

'It is not very creditable to the students of the Science of Language that there should have been among them so much wrangling as to whether that science is to be treated as one of the natural or as one of the historical sciences. They, if any one, ought to have seen that they were playing with language, or rather that language was playing with them, and that unless a proper definition is first given of what is meant by nature and by natural science, the pleading for and against the admission of the Science of Language to the circle of the natural sciences, may be carried on *ad infinitum*. It is, of course, open to anybody so to define the meaning of nature as to exclude human nature, and so to narrow the sphere of the natural sciences, as to leave no place for the Science of Language. It is also possible so to interpret the meaning of growth that it becomes inapplicable alike to the gradual formation of the earth's crust, and to the slow accumulation of the *humus* of language. Let the definition of these terms be plainly laid down, and the controversy, if it will not cease at once, will at all events become more fruitful. It will then turn on the legitimate definition of such terms as nature and mind, necessity and free-will, and it will have to be determined by philosophers rather than by scholars. Unless appearances deceive us, it is not the tendency of modern philosophy to isolate human nature, and to separate it by impassable barriers from nature at large, but rather to discover

the bridges which lead from one bank to the other, and to lay bare the hidden foundations which, deep beneath the surface, connect the two opposite shores. It is, in fact, easy to see that the old mediaeval discussions on necessity and free-will are turning up again in our own time, though slightly disguised, in the discussions on the proper place which man holds in the realm of nature; nay, that the same antinomies have been at the root of the controversy from the days when Greek philosophers maintained that language existed φύσει or θέσει, to our own days, when scholars range themselves in two hostile camps, claiming for the Science of Language a place either among the physical or the historical branches of knowledge.'

And again:

'At all events we should never allow ourselves to forget that, if we speak of languages as natural productions, and of the Science of Language as one of the natural sciences, what we chiefly wish to say is, that languages are not produced by the free-will of individuals, and that, if they are works of art, they are works of what may be called a natural or unconscious art—an art in which the individual, though he is the agent, is not a free agent, but checked and governed from the very first breath of speech by the implied co-operation of those to whom his language is addressed, and without whose acceptance language, not being understood, would cease to be language.'

In the first lecture which I delivered at Strassburg, I dwelt on the same problem, and said:

'There is, no doubt, in language a transition from the material to the spiritual: the raw material of language belongs to nature, but the form of language, that which really makes language, belongs to the spirit. Were it possible to trace human language *directly* back to natural sounds, to interjections or imitations, the question whether the Science of Language belongs to the sphere of the natural or the historical sciences would at once be solved. But I doubt whether this crude view of the origin of language counts one single supporter in Germany. With one foot language stands, no doubt, in the realm of nature, but with the other in the realm of spirit. Some years ago, when I thought it necessary to bring out as clearly as possible the much neglected natural

element in language, I tried to explain in what sense the Science of Language had a right to be called the last and the highest of the natural sciences. But I need hardly say that I did not lose sight, therefore, of the intellectual and historical character of language; and I may here express my conviction that the Science of Language will yet enable us to withstand the extreme theories of the evolutionists, and to draw a hard and fast line between spirit and matter, between man and brute.'

Professor Whitney will see, therefore, that all that can be said, and be justly said, against treating the Science of Language as a purely physical science, was not so new to me as he expected; nay, his friends might possibly tell him that the *pro's* and *con's* of this question had been far more fully and fairly weighed before his own lectures were published than afterwards. A writer on this subject, if he wishes to win new laurels, must do more than furbish up old weapons, and fight against monsters which owe their existence to nothing but his own heated imagination.

Is Glottology a Science?

His knowledge of the German language ought to have kept Professor Whitney from an insinuation that I had claimed for Glottology a place among the physical sciences, because I feared that otherwise the title of 'science' would be altogether denied to my researches. Now whatever artificial restriction may have been forced on the term 'science' in English and American, the corresponding term in German, *Wissenschaft*, has, as yet, resisted all such violence, and it was as a German that I ventured to call *Sprachwissenschaft* by its right name in English, and did not hesitate to speak even of a

Science of Mythology, a Science of Religion, and a Science of Thought.

Finally, as to my wishing to smuggle in Glottology, and to secure for it at least some small corner in the circle of the Physical Sciences, I am afraid I cannot lay claim to such modesty. When at the meeting of the British Association at Oxford in 1847, Bunsen claimed the establishment of a separate section for Ethnology, he said:

'If man is the apex of creation, it seems right on the one side, that a historical inquiry into his origin and development should never be allowed to sever itself from the general body of natural science, and, in particular, from physiology. But on the other hand, if man is the apex of creation, if he is the end to which all organic formations tend from the very beginning; if man is at once the mystery and the key of natural science; if that is the only view of natural science worthy of our age, then ethnologic philology, once established on principles as clear as the physiological are, is *the highest branch* of that science for the advancement of which this Association is instituted. It is not an appendix to physiology or to anything else; but its object is, on the contrary, capable of becoming the end and goal of the labours and transactions of a scientific association.'

These words of my departed friend express better than anything which I can say, what I meant by claiming for the Science of Language and the Science of Man, a place among the physical sciences. By enlarging the definition of physical science so as to make it comprehend both Anthropology and Glottology, I thought I was claiming a wider scope and a higher dignity for physical science. The idea of calling language a vegetable, in order to smuggle it through the toll-bar of the physical sciences, certainly never entered my mind.

When one remembers how since 1847 man has

become the central point of the discussions of the British Association year after year, Bunsen's words sound almost prophetic, and it might have been guessed, even in America, that the friend and pupil of Bunsen was not likely to abate much in his claims for the recognition of the Science of Man, as the highest of all sciences.

Have I done? Yes, I believe I have answered all that required an answer in Mr. Darwin's article, in Professor Whitney's new attack in the *Contemporary Review*, and in his Lectures. But alas! there is still a page bristling with challenges.

Have I read not only his lectures, but all his controversial articles? No. Then I ought.

Have I quoted any passage from his writings to prove that the less he has thought on a subject, the louder he speaks? No. Then I ought.

Have I produced any proof that he wonders that no one answers his strictures? No. Then I ought.

He actually appeals to my honour. What can I do? I cannot say that I have since read all his controversial articles, but I have read a considerable number, and I frankly confess that on many points they have raised my opinion of Professor Whitney's acquirements. It is true, he is not an original worker, but he is a hard reader, and a very smart writer. The gall of bitterness that pervades all his writings, is certainly painful, but that concerns him far more than us.

Language and Thought Inseparable.

First then, I am asked to explain what I meant by saying that Professor Whitney speaks the

loudest on subjects on which he has thought the least. I could best explain my meaning, if I were to collect all that Professor Whitney has written on the relation of language to thought. He certainly grows most boisterous in these latitudes, and yet he evidently has never, as yet, read up that subject, nay, he seems convinced that what has been written on it by such dreamers as Locke, Schelling, Hegel, Humboldt, Schopenhauer, Mansel, and others, deserves no consideration whatever. To maintain, what every one of these philosophers maintains, that a conception cannot be entertained without the support of a word, would be, according to the Harvard Professor, the sheerest folly (p. 125),—'part of that superficial and unsound philosophy which confounds and identifies speech, thought, and reason' (p. 439).

I can quite enter into these feelings, for I can still remember the mental effort that is required in order to surrender our usual view of language, as a mere sign or instrument of thought, and to recognise in it the realisation of all conceptual thought. A mere dictionary would, no doubt, seem the best answer to those who hold that thought and language are inseparable, and to throw a stout Webster at our head might be considered by many as good a refutation of such sheer folly, as a slap in the face was supposed to be of Berkeley's idealism. However, Professor Whitney is an assiduous reader, and I do not at all despair that the time will come when he will see what these thinkers really mean by conceptual thought and by language, and I am quite prepared to hear him say that 'he had known all that long ago, that any child knew it, that it was mere *bathos*, and that it was only due to a want

of clear and definitive expression, or to a want of knowledge of English, excusable in a foreigner, if there had been so much darkening of counsel by words without thought.' I shall then be told that :

'I consulted excellent authorities, and I worked these up with a commendable degree of industry, but that I am wanting in the inner light . . . and have never gained a comprehension of the movements that go on in my own mind, without which real insight into the relation of language to thought is impossible' (p. 268).

Professor Prantl on the Reform of Logic.

In order to accelerate that event, may I advise Professor Whitney to read some articles lately published by Professor Prantl? Professor Prantl is *facile princeps* among German logicians, he is the author of the 'History of Logic,' and therefore perhaps even the American Professor will not consider him, as he does others who differ from him, as quite ignorant of the first rules of logic! At the meeting of the Royal Academy at Munich, March 6, 1875, Professor Prantl claimed permission, after having finished his 'History of Logic,' to lay some thoughts for the 'Reform of Logic,' before the members of that Academy, the very fundamental principle of that reform being

The essential unity of thought and language.

'Realised thought, or what others might call the realisation of the faculty of thought, exists therefore in language only, and *vice versâ*, every element of language contains thought. Every kind of priority of real thought before its expression in language, is to be denied, as well as any separate existence of thought' (p. 181).

'In one sense I should not deny that there is something in animals which in a very high degree of elevation is called language in man. In recognition of the distance produced by this high

degree of elevation, one can agree with Max Müller, that language is the true frontier between brute and man' (p. 168).

Or, if the Harvard Professor wants a more popular treatment of the subject, he might read Dr. Loewe's essay on 'the Simultaneity of the genesis of Speech and Thought,' also published this year. Dr. Loewe, too, avails himself gladly of the new results obtained by the Science of Language, and shows clearly that the origin of thought is the origin of language.

Every one who has to write on philosophical subjects in English, German, and French, or who has to superintend translations of what he has written into other languages, must know how difficult it is to guard always against being misunderstood, but a reader familiar with his subject at once makes allowance for this; he does not raise clouds of dust for nothing. Observe the difference between some criticisms passed on what I had said, by Dr. Loewe, and by others. I had said in my Lectures (ii. 76):

'It is possible, without language, to see, to perceive, to stare at, to dream about things; but, without words, not even such simple *ideas* as white or black can be for a moment realised.'

My German translator had rendered *ideas* by *Vorstellungen*, while I used the word in the sense of concept, *Begriff*. Dr. Loewe in commenting on this passage says:

'If M. M. maintains that *Vorstellungen*, such as white and black, cannot be realised for a moment without words, he is right, but only if by *Vorstellung* he means *Begriff*. And this is clearly his meaning, because shortly before he had insisted on the fact that it was conceptual thought which is impossible without words. Were we to take his words literally, then it would be wrong, for sensuous images (Sinnesbilder), such as white and black, do not require words for their realisation. One glance at the psychical life

of animals would suffice to prove that sensuous representation (Vorstellen) can be carried out without language, for it is equally certain that animals have sensuous images as that they have no words.'

This is the language of a well-schooled philosopher, who cares for truth and not for controversy, *à tout prix*. Let us contrast it for a moment with the language of Professor Whitney (p. 249):

'This may be taking a very high view of language; it certainly is taking a very low view of reason. If only that part of man's superior endowments which finds its manifestation in language is to receive the name of reason, what shall we style the rest? We had thought that the love and intelligence, the soul, that looks out of a child's eyes upon us to reward our care long before it begins to prattle, were also marks of reason, etc.'

This is a pretty domestic idyll, but the marvellous confusion between conceptual thought and the inarticulate signs of the affections, will, I fear, remind logicians of infantine prattle with no mark of reason about it, rather than of scientific argument.

It is quite clear, therefore, from this single specimen, that it would be impossible to argue with Professor Whitney on this subject. He returns to it again and again, his language grows stronger and stronger every time, yet all the time he speaks like a man whom nothing shall convince that the earth does move. He does not even know that he might have quoted very great authorities on his side of the question, only that they, knowing the bearings of the whole problem, speak of their antagonists with the respect due say by Nyâya to a Sânkhya philosopher, not with the contempt which a Brahman feels for a Mle*kkha*.

Grammatical Blunders.

But let us take a subject where, at all events, it is possible to argue with the Professor—I mean Sanskrit Grammar—and we shall see again that he is most apodictic when he is least informed. He has criticised the first volume of my translation of the Rig-veda. He dislikes it very much, and gives me very excellent advice as to what I ought to have done and what I ought not. He thinks I ought to have thought of the large public who want to know something of the Veda, and not of mere scholars. He thinks that the hymns addressed to the Dawn would have pleased the young ladies better than the hymns to the Stormgods, and he broadly hints that all the *pièces justificatives* which I give in my commentary are *de trop*. A translation, such as Langlois', would, no doubt, have pleased him best. I do not object to his views, and I hope that he or his friends may some day give us a translation of the Rig-veda, carried out in that spirit. I shall devote the remaining years of my life to carrying on what I ventured to call and still call the first *traduction raisonnée* of the Veda, on those principles which, after mature reflection, I adopted in the first volume, and which I still consider the only principles in accordance with the requirements of sound scholarship. The very reason why I chose the hymns to the Maruts was because I thought it was high time to put an end to the mere trifling with Vedic translation. They are, no doubt, the most difficult, the most rugged, and, it may be, the least attractive hymns, but they are on that very account an excellent introduction to a scholarlike study of the Veda. Mere guessing and skipping will

not avail us here. There is no royal road to the discovery of the meaning of difficult words in the Veda. We must trace words of doubtful meaning through every passage where they occur, and we must give an account of their meaning by translating every passage that can be translated, marking the rest as, for the present, untranslatable. Boehtlingk and Roth's excellent Dictionary is the first step in that direction, and a most important step. But in it the passages have only undergone their first sifting and classifying; they are not translated, nor are they given with perfect completeness. Now if one single passage is left out of consideration in establishing the meaning of a word, the whole work has to be done again. It is only by adopting my own tedious, it may be, but exhaustive method that a scholar may feel that whatever work he has done, it is done once for all.

On such questions, however, it is easy to write a great deal in general terms; though it is difficult to say anything on which all competent scholars are not by this time fully agreed. It is not for me to gainsay my American critic that my renderings into English, being those of a foreigner, are tame and spiritless, but I doubt, whether in a new edition I shall change my translation, 'the lights in heaven shine forth,' for what the American professor suggests: 'a sheen shines out in the sky,' or 'gleams glimmer in the sky.'

All this, however, anybody might have written after dinner. But once at least Professor Whitney, Professor of Sanskrit in Harvard, attempts to come to close quarters, and ventures on a remark on Sanskrit grammar. It is the only passage in all his writings, as far as I remember, where, instead

of indulging in mere sheet lightning, he comes down upon me with a crashing thunderbolt, and points out a real grammatical blunder. He says it is—

'An extremely violent and improbable grammatical process to render pari tasthushas, as if the reading were paritasthivâ*m*sas. The participial form tasthushas has no right to be anything but an accusative plural, or a genitive or ablative singular; let us have the authority for making a nominative plural of it, and treating pari as its prefix, and better authority than the mere dictum of a Hindu grammarian.'

Those who are acquainted with Vedic studies know that Professor Benfey has been for years preparing a grammar of the Vedic dialect, and, as there is plenty of work for all workers, I purposely left the grammatical questions to him, confining myself in my commentary to the most necessary grammatical remarks, and giving my chief attention to the meaning of words and the poetical conceptions of the ancient poets. If the use of the accusatival form tasthushas, with the sense of a nominative, had been confined to the Veda, or had never been remarked on before, I ought, no doubt, to have called attention to it. But similar anomalous forms occur in Epic literature also, and more than that, attention had but lately been called to them by a very eminent Dutch scholar, Dr. Kern, who, in his translation of the B*r*ihat-Sa*m*hitâ, remarks that the ungrammatical nom. plur. vidushas is by no means rare in the Mahâbhârata and kindred works. If Professor Whitney had only read as far as the eleventh hymn in the first book of the Rig-veda, he would have met there in abibhyushas an undoubted nom. plur. in ushas:

tvâ*m* devâ*h* ábibhyusha*h* tu*g*yámânâsa*h* âvishu*h*,
The gods, stirred up, came to thee, not fearing.

Now, I ask, was I so far wrong when I said that Professor Whitney speaks loudest when he knows least, and that in charging me, for once at least, with a tangible blunder, he only betrayed his ignorance of Sanskrit grammar? In former times a scholar, after such a misfortune, would have taken a vow of silence or gone into a monastery. What will Professor Whitney do? He will take a vow of speech, and rush into a North-American Review.

Hard and Soft.

There are other subjects to which Professor Whitney has of late paid much more attention than to Sanskrit Grammar, and we shall find that on them he argues in a much gentler tone.

It is well known that Professor Whitney held curious views about the relation of vowels to consonants, and I therefore was not surprised to hear from him that 'my view of the essential difference between vowels and consonants will not bear examination.' He mixes up what I call the substance (breath and voice) with the form (squeezes and checks), and forgets that *in rerum naturâ* there exist no consonants except as modifying the column of voice and breath, or as what Hindu grammarians call vyan*g*ana, i. e. determinants; and no vowels except as modified by consonants. In order to support the second part of this statement, viz. that it is impossible to pronounce an initial vowel without a slight, and to many hardly perceptible, initial noise, the *coup de la glotte*, I had appealed to musicians who know how difficult it is, in playing on the flute or on the violin, to weaken or to avoid certain noises

(*Ansatz*) arising from the first impulses imparted to the air, before it can produce really musical sensations. Professor Whitney, in quoting this paragraph, leaves out the sentence where I say that I want to explain the difficulty of pronouncing initial vowels without some *spiritus lenis*, and charges me with comparing all consonants with the unmusical noises of musical instruments. This was in 1866, whereas in 1854 I had said: 'If we regard the human voice as a continuous stream of air, emitted as breath from the lungs and changed by the vibration of the *chordae vocales* into vocal sound, as it leaves the larynx, this stream itself, as modified by certain positions of the mouth, would represent the vowels. In the consonants, on the contrary, we should have to recognise a number of stops opposing for a moment the free passage of this vocal air.' I ask any scholar or lawyer, what is one to do against such misrepresentations? How is one to qualify them, when to call them unintentional would be nearly as offensive as to call them intentional?

The greatest offence, however, which I have committed in his eyes is that I revived the old names of *hard* and *soft*, instead of *surd* and *sonant*. Now I thought that one could only revive what is dead, but I believe there is not a single scholar alive who does not use always or occasionally the terms *hard* and *soft*. Even Professor Whitney can only call these technical terms obsolescent; but he thinks my influence is so omnipotent that, if I had struck a stroke against these obsolescent terms, they would have been well nigh or quite finished. I cannot accept that compliment. I have tried my strokes against much more objectionable things than *hard* and *soft*,

and they have not yet vanished. I know of no living philologist who does not use the old terms *hard* and *soft*, though everybody knows that they are imperfect. I see that Professor Pott[1] in one passage where he uses *sonant* thinks it necessary to explain it by *soft*. Why, then, am I singled out as the great criminal? I do not object to the use of *surd* or *sonant*. I have used these terms from the very beginning of my literary career, and as Professor Whitney evidently doubts my word, I may refer him to my *Proposals*, submitted to the Alphabetic Conferences in 1854. He will find that as early as that date, I already used *sonant*, though, like Pott, I explained this new term by the more familiar *soft*. If he will appeal to Professor Lepsius, he will hear how, even at that time, I had translated for him the chapters of the Prâtisâkhyas, which explain the true structure of a physiological alphabet, and ascribe the distinction between k and g to the absence and presence of voice. I purposely avoided these new terms, because I doubted, and I still doubt, whether we should gain much by their adoption. I do not exactly share the misgivings that a *surd mute* might be mistaken for a *deaf and dumb* letter, but I think the name is awkward. *Voiced* and *voiceless* would seem much better renderings of the excellent Sanskrit terms ghoshavat and aghosha, in order to indicate that it is the presence and absence of the voice which causes their difference. Frequent changes in technical terms are much to be deprecated[2], particularly if the new terms are themselves imperfect.

[1] 'Etymologische Forschungen,' 1871, p. 78, tönende, *d. h.* weiche.

[2] See p. 365.

Every scholar knows by this time what is meant by *hard* and *soft*, viz. *voiceless* and *voiced*. The names *hard* and *soft*, though not perfect, have, like most imperfect names, some kind of excuse, as I tried to show by Czermak's experiments[1]. But while a good deal may be said for *soft* and *hard*, what excuse can be pleaded for such a term as *media*, meaning originally a letter between the *Psila* and the *Dasea*? Yet, would it be believed that this very term is used by Professor Whitney on the page following immediately after his puritanical sermon against my backslidings!

This gentle sermon, however, which Professor Whitney preaches at me, as if I were the Pope of Comparative Philologists, is nothing compared with what follows later. When he saw that the difference between *voiced* and *voiceless* letters was not so novel to me as he had imagined, that it was known to me even before I published the Prâtisâkhya,—nay, when I had told him that, to quote the words of Professor Brücke, the founder of scientific phonetics,

'The medias had been classed as *sonant* in all the systems elaborated by the students of language who have studied comparative phonology,'

he does not hesitate to write as follows:

'Professor Müller, like some other students of philology (who, except Professor Whitney himself?) finds himself unable longer to resist the force of the arguments against *hard* and *soft*, and is convinced that *surd* and *sonant* are the proper terms to use; but, instead of frankly abandoning the one, and accepting the other in their place, he would fain make his hearers believe that he has always held and taught as he now wishes he had done. It is either a case of disingenuousness or of remarkable self-deception: there appears to be no third alternative.'

[1] 'Lectures,' vol. ii. p. 139.

I call this a gentle reproof, as coming from Professor Whitney; but I must say at the same time that I seldom saw greater daring displayed, regardless of all consequences. The American captain sitting on the safety-valve to keep his vessel from blowing up, is nothing in comparison with our American Professor. I have shown that in 1854 the terms *surd* and *sonant* were no novelty to me. But as Professor Whitney had not yet joined our ranks at that time, he might very properly plead ignorance of a paper which I myself have declared antiquated by what I had written afterwards on the same subject. But will it be believed that in the very same lecture which he is criticising, there occurs the following passage (ii. p. 57):

'What is it that changes k into g, t into d, p into b? B is called a media, a soft letter, a sonant, in opposition to P, which is called a tenuis, a hard letter, or a surd. But what is meant by these terms? A tenuis, we saw, was so called by the Greeks, in opposition to the aspirates, the Greek grammarians wishing to express that the aspirates had a rough or shaggy sound, whereas the tenues were bald, slight, or thin. This does not help us much. *Soft* and *hard* are terms which, no doubt, express an outward difference of p and b, but they do not explain the cause of that difference. *Surd* and *sonant* are apt to mislead; for if, according to the old system both p and b continue to be classed as mute, it is difficult to see how, taking words in their proper sense, a mute letter could be sonant. . . . . Both p and b are momentary negations of breath and voice; or, as the Hindu grammarians say, both are formed by complete contact. But b differs from p in so far as, in order to pronounce it, breath must have been changed by the glottis into voice, which voice, whether loud or whispered, partly precedes, partly follows the check.'

And again:

'But although the hardness and softness are secondary qualities

of *tenues* and *mediae*, of surd and sonant letters, the true physiological difference between p and b, t and d, k and g, is that in the former the glottis is wide open, in the latter narrowed, so as to produce either whispered or loud voice.'

In my introduction to the 'Outline Dictionary for Missionaries,' published in 1867, I wrote:

'Unfortunately, everybody is so familiar with his alphabet, that it takes some time to convince people that they know next to nothing about the true nature of their letters. Take even a scholar, and ask him what is T, and he may possibly say, a dental tenuis; ask him what is D, and he may reply, a dental media. But ask him what he really means by a tenuis or media, or what he considers the true difference between T and D, and he may probably say that T is hard and D is soft; or that T is sharp and D is flat; or, on the contrary, as some writers have actually maintained, that the sound of D requires a stronger impulse of the tongue than the sound of T; but we shall never get an answer that goes to the root of the matter, and lays hold of the mainspring and prime cause of all these secondary distinctions between T and D. If we consult Professor Helmholtz on the same subject, he tells us that "the series of so-called mediae, b, d, g, differs from that of the tenues, p, t, k, by this, that for the former the glottis is, at the time of consonantal opening, sufficiently narrowed to enable it to sound, or at least to produce the noise of the *vox clandestina*, or whisper, while it is wide open with tenues, and therefore unable to sound. Mediae are therefore accompanied by the tone of the voice, and this may even, where they begin a syllable, set in a moment before, and where they end a syllable, continue a moment after the opening of the mouth, because some air may be driven into the closed cavity of the mouth, and support the sound of the vocal chords of the larynx. Because of the narrowed glottis, the rush of the air is more moderate, the noise of the air less sharp than with the tenuis, so that a great mass of air may rush at once from the chest."

'This to many may seem strange and hardly intelligible. But if they find that, several centuries before our era, the Indian grammarians gave exactly the same definition of the difference between p, t, k, and b, d, g, such a coincidence may possibly startle them, and lead them to inquire for themselves into the

working of that wonderful instrument by which we produce the various sounds of our alphabet.'

If Professor Whitney asserts

'That I *repeatedly* will not allow that the sonant letters *are* intonated, but only that they *may be* intonated,'

I have no answer but a direct negative. For me to say so, would be to run counter to all my own teaching, and if there is anywhere a passage that would admit of such a construction, Professor Whitney knows perfectly well that this could be due to nothing but an accidental want of precision in expressing myself. I know of no such passage[1].

[1] Having still that kind of faith left, that a man could not wilfully say a thing which he knows to be untrue, I looked again at every passage where I have dwelt on the difference between soft and hard consonants, and I think I may have found the passage which Professor Whitney grasped at, when he thought that I knew nothing of the difference between voiced and voiceless letters, until he had enlightened me on the subject. Speaking of letters, not as things by themselves, but as acts, I sometimes speak of the process that produces the hard consonant first, and then go on to say that it can be voiced, and be made soft. Thus when speaking of s and z, I say, the former is completely surd, the latter capable of intonation, and the same expression occurs again. Could Professor Whitney have thought that I meant to say that z was only capable of intonation, but was not necessarily intonated? I believe he did, for it is with regard to s and z that, as I see, he says, 'it is a marvel to find men like Max Müller, in his last lectures about language, who still cling to the old view that a z, for instance, differs from s primarily by inferior force of utterance.' Now, I admit that my expression, 'capable of intonation,' might be misunderstood, and might have misled a mere tiro in these matters, who alighted on this passage, without reading anything before or after. But that a Professor in an American University could have taken my words in that sense is to me, I confess, a puzzle, call it intellectual or moral, as you like.

In order to leave no doubt as to the real distinction between k, t, p and g, d, b, I quoted, for the satisfaction of Sanskrit scholars, the technical terms by which native grammarians define so admirably the process of their formation, the vâhyaprayatna, viz. vivârasvâsâghoshâh, and samvâranâdaghoshâh. Would it be believed that Professor Whitney accuses me of having invented these long Sanskrit terms, and to have appended them superfluously and pedantically, as he says, to each list of synonyms? 'They are found in no Sanskrit grammarian,' he says. Here again I have no answer but a direct negative. They are found in the native commentary on Pânini's Grammar, in Boehtlingk's edition, p. 4, and fully explained in the Mahâbhâshya.

If one has again and again to answer the assertions of a critic by direct negatives, is it to be wondered at that one rather shrinks from such encounters? I have for the last twenty years discussed these phonetic problems with the most competent authorities. Not trusting to my own knowledge of physiology and acoustics, I submitted everything that I had written on the alphabet, before it was published, to the approval of such men as Helmholtz, Alexander Ellis, Professor Rolleston, and I hold their *vu et approuvé*. I had no desire, therefore, to discuss these questions anew with Professor Whitney, or to try to remove the erroneous views which, till lately, he entertained on the structure of a physiological alphabet. I believe Professor Whitney has still much to learn on this subject, and as I never ask anybody to read what I myself have written, still less to read it a second time, might I suggest to him to read at all events

the writings of Brücke, Helmholtz, Czermak, to say nothing of Wheatstone, Ellis, and Bell, before he again descends into this arena? If he had ever made an attempt to master that one short quotation from Brücke, which I gave on p. 159, or even that shorter one from Czermak, which I gave on p. 143:

'Die Reibungslaute zerfallen genau so wie die Verschlusslaute in *weiche* oder *tönende*, bei denen das Stimmritzengeräusch oder der laute Stimmton mitlautet, und in *harte* oder *tonlose*, bei denen der Kehlkopf absolut still ist,'

the theory which I followed in the classification both of the Checks and the Breathings would not have sounded so unintelligible to him as he says it did;—he would have received some rays of that inner light on phonetics which he misses in my Lectures, and would have seen that besides the disingenuousness or the self-deception which he imputes to me, in order to escape from the perplexity in which he found himself, there was after all a third alternative, though he denies it, viz. his being unwilling to confess his own *ὀψιμαθία*.

Fir, Oak, Beech.

I now proceed to the next charge. I am told that I am in honour bound to produce a passage where Professor Whitney expressed his dissatisfaction at not being answered, or, as I had ventured to express it, considering the general style of his criticism, when he is angry that those whom he abuses, do not abuse him in turn. He is evidently conscious that there is some slight foundation for what I had said, for he says

that if Steinthal thought he was angry, because 'he (Mr. William Dwight Whitney) and his school' had not been refuted, instead of philosophers of the last century, he was mistaken. Yet what can be the meaning of this sentence, that 'Professor Steinthal ought to have confronted *the living and aggressive* views of others,' i. e. of Mr. William Dwight Whitney and his school? (p. 365.)

However, I shall not appeal to that; I shall take a case which, in this tedious process of incrimination and recrimination, may perhaps revive for a moment the flagging interest of my readers.

I had in the second volume of my Lectures called attention to a curious parallelism in the changes of meaning in certain names of trees and in the changes of vegetation recorded in the strata of the earth. My facts were these. *Foraha* in Old High German, *Föhre* in modern German, *furh* in Anglo-Saxon, *fir* in English, signify the *pinus silvestris*. In the Lombard Laws the same word *fereha* means oak, and so does its corresponding word in Latin, *quercus*.

Secondly, φηγός in Greek means oak, the corresponding word in Latin *fagus*, and in Gothic *bôka*, means beech.

That is to say, in certain Aryan languages we find words meaning fir, assuming the meaning of oak; and words meaning oak, assuming the meaning of beech.

Now in the North of Europe geologists find that a vegetation of fir exists at the lowest depth of peat deposits; that this was succeeded by a vegetation of oak, and this by a vegetation of beech. Even in the lowest stratum a stone implement was found under a fir, showing the presence of human beings.

Putting these two sets of facts together, I said: Is it possible to explain the change of meaning in one word which meant fir and came to mean oak, and in another which meant oak and came to mean beech, by the change of vegetation which actually took place in early ages? I said it was an hypothesis, and an hypothesis only. I pointed out myself all that seemed doubtful in it, but I thought that the changes of meaning and the parallel changes of vegetation required an explanation, and until a better one could be given, I ventured to suggest that such changes of meaning were as the shadows cast on language by real, though prehistoric, events.

I asked for an impartial examination of the facts I had collected, and of the theory I had based on them. What do I receive from Professor Whitney? I must quote his *ipsissima verba*, to show the spirit that pervades his arguments:

'It will not be difficult,' he says, 'to gratify our author by refuting his hypothesis. Not the very slightest shade of plausibility, that we can discover, belongs to it. Besides the serious minor objections to which it is liable, it involves at least three impossible suppositions, either one of which ought to be enough to insure its rejection.

'In the first place it assumes that the indications afforded by the peat-bogs of Denmark are conclusive as regards the condition of Europe—of all that part of it, at least, which is occupied by the Germanic and Italic races; that, throughout this whole region, firs, oaks, and beeches have supplanted and succeeded each other, notwithstanding that we find all of them, or two of them, still growing peaceably together in many countries.'

Here Professor Whitney is, as usual, ploughing with my heifer. I said:

'I must leave it to the geologist and botanist to determine whether the changes of vegetation as described above, took place

in the same rotation over the whole of Europe, or in the North only.'

I had consulted several of my own geological friends, and they all told me that there was, as yet, no evidence in Central Europe and Italy of a succession of vegetation different from that in the North, and that, in the present state of geological science, they could say no more. In the absence of evidence to the contrary, I said, Let us wait and see; Professor Whitney says, Don't wait.

His second objection is his own, but hardly worthy of him.

'The hypothesis,' he says, 'assumes that the Germanic and Italic races, while they knew and named the fir-tree only, yet kept by them all the time, laid up in a napkin, the original term for oak, ready to be turned into an appellation for beech, when the oaks went out of fashion.'

This is not so. The Aryan nations formed many new words, when the necessity for them arose. There was no difficulty in framing ever so many names for the oak, and there can be little doubt that the name φηγός was derived from φάγω, the oak tree being called φηγός, because it supplied food or mast for the cattle. If there remained some consciousness of this meaning among the Greeks, and the Italians, and Germans, then the transference of the name from the oak to the beech would become still more easily intelligible, because both the beech-nuts and the acorns supplied the ordinary mast for cattle.

Professor Whitney probably had misgivings that these two objections were not likely to carry much weight, so he adds a third.

'The hypothesis,' he says, 'implies a method of transfer of names

from one object to another which is totally inadmissible; this, namely—that, as the forest of firs gave way to that of oaks, the meaning of fir in the word *quercus* gave way to that of oak; and in like manner in the other case. Now if the Latins had gone to sleep some fine night under the shade of their majestic oaks, and had waked in the morning to find themselves *patulae sub tegmine fagi*, they might naturally enough have been led, in their bewilderment, to give the old name to the new tree. But who does not see that, in the slow and gradual process by which, under the influence of a change of climatic conditions, one species of tree should come to prevail over another, the supplanter would not inherit the title of the supplanted, but would acquire one of its own, the two subsisting together during the period of the struggle, and that of the supplanted going out of use and memory as the species it designated disappeared?'

This objection was of course so obvious that I had thought it my duty to give a number of instances where old words have been transferred, not *per saltum*, but slowly and gradually, to new objects, such as *musket*, originally a dappled sparrow-hawk, afterwards a gun. Other instances might have been added, such as *θάπτω*, the Sanskrit *dah*, the latter meaning to burn, the former to bury. But the best illustrations are unintentionally offered by Professor Whitney himself. On p. 303 he alludes to the fact that the names *robin* and *blackbird* have been applied in America, for the sake of convenience, and under the government of old associations, to birds essentially unlike, or only superficially like, those to which they belong in the mother country. Of course, every Englishman who settled in America knew that the bird he called *robin* was not the old Robin Redbreast he knew in England. Yet the two names co-existed for a time in literature, nay, they may still be said to co-exist in their twofold application, though, from a strictly American point of view, the

supplanting American bird has inherited the title of the supplanted Cock-Robin of England.

Now, I ask, was there anything in these three cheap objections that required an answer? Two of them I had myself fully considered, the third was so flimsy that I thought no one would have dwelt on it. Anyhow, I felt convinced that every reader was competent to judge between Professor Whitney and myself, and it certainly never entered my mind that I was in honour bound, either to strike out my chapter on the Words for *Fir*, *Oak*, and *Beech*, or to fight.

Was I then so far wrong when I said that Professor Whitney cannot understand how anybody could leave what he is pleased to call his arguments, unheeded? Does he not express his surprise that in every new edition I adhere to my views on *Fir*, *Oak*, and *Beech*, though he himself had told me that I was wrong, and when he calls my expressed desire for real criticism a mere 'rhetorical flourish,' is this, according to the opinion of American gentlemen, or is it not, abuse?

Epitheta ornantia.

Professor Whitney's ideas of what is real criticism, and what is mere banter, personal abuse, or rudeness are indeed strange. He does not seem to be aware that his name has become a byword, at least in Europe, and he defends himself against the charge of abusiveness with so much ardour that one sometimes feels doubtful whether it is all the mere rhetoric of a bad conscience, or a case of the most extraordinary self-deception. He declares in so many words that he was never personal (*Ich bestreite durchaus, dass was*

ich schrieb, im geringsten persönlich war), and he immediately goes on to say that 'Steinthal burst a two from anger and rancour, and his answer was a mere outpouring of abuse against his personality.'

Now I am the last person or personality in the world to approve of the tone of Steinthal's answer, and if Professor Whitney asks why I had quoted it several times in public, it was because I thought it ought to be a warning to others. I think that all who are interested in maintaining certain civilised usages even in the midst of war, ought to protest against such a return to primitive savagery, and I am glad to find that my friend, Mr. Matthew Arnold, one of the highest authorities on the rules of literary warfare, entertains the same opinion, and has quoted what I had quoted from Professor Steinthal's pamphlet, together with other specimens of theological rancour, as extreme cases of bad taste.

I frankly admit, however, that, when I said that Steinthal had defended himself with the same weapons with which his American antagonist attacked him, I said too much. Professor Whitney does not proceed to such extremities as Professor Steinthal. But giving him full credit so far, I still cannot help thinking that it was a fight with poisoned arrows on one side, with clubs on the other. As Professor Whitney calls for proofs, here they are :

Page 332. Why does he call Professor Steinthal, *Hajjim Steinthal?* Is that personal or not?

P. 335. 'Professor Steinthal startles and rebuffs a common-sense inquirer with a reply from a wholly different and unexpected point of view: as when you ask a physician, "Well, Doctor, how does your patient promise this morning?" and he answers, with a wise

look and an oracular shake of the head, "It is not given to humanity to look into futurity." The effect is not destitute of the element of *bathos*.' Is that personal?

P. 337. Steinthal's mode of arguing is 'more easy and convenient than fair and ingenuous.' Is that personal?

P. 338. 'A mere verbal quibble.'

P. 346. 'The eminent psychologist may show himself a mere blunderer.'

P. 356. 'To our unpsychological apprehension, there is something monstrous in the very suggestion that a word is an act of the mind.'

P. 357. 'Prodigious . . . Chaotic nebulosity . . . We should not have supposed any man, at this age of the world, capable of penning the sentences we have quoted.'

P. 359. 'We are heartily tired of these comparisons that go limping along on one foot, or even on hardly the decent stump of a foot.'

P. 363. 'Can there be more utter mockery than this? We ask for bread, and a stone is thrown us.'

P. 365. 'He does not take the slightest notice of the *living* and *aggressive* views of others.'

P. 366. 'All this, again, is in our opinion very verbiage, mere turbid talk.'

P. 367. 'The statement is either a truism or falsity.'

P. 372. 'We must pronounce Professor Steinthal's attempt . . . . a complete failure, a mere continuation of the same delusive reasonings by which he originally arrived at it.'

P. 374. 'We have found in his book nothing but mistaken facts and erroneous deductions.'

If that is the language in which Professor Whitney speaks of one whom he calls

'An eminent master in linguistic science, from whom he has derived great instruction and enlightenment,' and 'whose books he has constantly had upon his table,'

what can other poor mortals like myself expect? It is true he has avoided actionable expressions, while Professor Steinthal has not, at least, according to German and English law. But suppose that here-

after, when certain small animals have crossed what he calls 'the impervious distance,' and acquired the power of language, they were to say, 'We have only stung you, and you have killed us,' would they obtain much commiseration?

I had collected a number of *epitheta ornantia* which I had gathered at random from Mr. Whitney's writings, such as *worthless, futile, absurd, ridiculous, superficial, unsound, high-flown, pretentious, disingenuous, false,* and I claimed the honour of every one of them having been presented to me as well as to other scholars by our American assailant. Here, for the first time, Professor Whitney seems staggered at his own vocabulary. However, he is never at a loss how to escape. 'As the epithets are translated into German,' he says, 'he is quite unable to find the passages to which I may refer.' This is feeble. However, without taxing his memory further, he says that he feels certain it must be a mistake, because he never could have used such language. He never in his life said anything personal, but criticised opinions only. This is 'the language of simple-minded consciousness of rectitude.'

What can I do? Professor Whitney ought to know his own writings better than I do, and nothing remains to me, in order to repel the gravest of all accusations, but to publish in the smallest type the following Spicilegium. I must add that in order to do this work once for all, I have complied with Professor Whitney's request, and read nearly all the articles with which he has honoured every one of my writings, and in doing so I believe I have at last found the key to much that seemed to me before almost inexplicable.

Formerly I had simply acquiesced in the statement made by one of his best friends, Professor Weber[1], who, some ten years ago, when reproving Professor Whitney for the acrimony of his language, said:

'I believe I am not wrong when I trace it to two causes: first, Professor Whitney found himself forced to acknowledge as erroneous and to withdraw several of his former views and assertions, which he had defended with great assurance, and this disturbed his equanimity; secondly, and still more, there were the miserable political circumstances of North America, which could not but exercise an irritating and galling effect on so warm a patriot as Whitney, an effect which was transferred unconsciously to his literary criticisms and polemics, whenever he felt inclined to it.'

These two scholars were then discussing the question, whether the Nakshatras or the Lunar Zodiac of the Hindus, should be considered as the natural discovery of the Brahmans, or as derived by them, one knows not how, from China, from Chaldaea, or from some other unknown country. They both made great efforts, Professor Weber chiefly in Sanskrit, Professor Whitney in astronomy, in order to substantiate their respective opinions. Professor Weber showed that Professor Whitney was not very strong in Sanskrit, Professor Whitney retaliated by showing that Professor Weber, as a philologue, had attempted to prove that the precession of the equinox was from West to East, and not from East to West. All this, at the time, was amusing to bystanders, but by this time both combatants have probably found out, that the hypothesis of a foreign origin of the Nakshatras, whether Chinese or Babylonian, was uncalled for, or, at all events, is as uncertain to day as it was ten years ago. I myself, not being an astronomer, had

[1] 'Indische Studien,' X. 459.

been content to place the evidence from Sanskrit sources before a friend of mine, an excellent astronomer at Oxford, and after discussing the question again and again with him, had arrived at the conviction that there was no excuse for so violent a theory as postulating a foreign origin of the simple triseinadic division of the Nakshatra Zodiac. I quite admit that my practical knowledge of astronomy is very small[1], but I do believe that my astronomical ignorance was an advantage rather than

[1] When I saw how M. Biot, the great astronomer, treated Professor Weber *du haut en bas*, because in criticising Biot's opinion he had shown some ignorance of astronomy, I said, from a kind of fellow-feeling: 'Weber's Essays are very creditable to the author, and hardly deserved the withering contempt with which they were treated by Biot. I differ from nearly all the conclusions at which Professor Weber arrives, but I admire his great diligence in collecting the necessary evidence.' Upon this the American gentleman reads me the following lesson. First of all, I am told that my statement involves a gross error of fact; I ought to have said, Weber's Essay, not Essays, because one of them, and the most important, was not published till after Biot's death. I accept the reproof, but I believe all whom it concerned knew what Essay I meant. But secondly, I am told that the epithet *withering* is only used by Americans when they intend to imply that, in their opinion, the subject of the contempt is withered, or ought to be withered, by it. This may be so in American, but I totally deny that it is so in English. 'Withering contempt' in English means, as far as I know, a kind of silly and arrogant contempt, such, for instance, as Professor Whitney displays towards me and others, intended to annihilate us in the eyes of the public, but utterly harmless in its consequences. But let me ask the American critic what he meant when, speaking of Biot's treatment of Weber, he said, 'Biot thought that Weber's opinions had been *whiffed* away by him as if unworthy of serious consideration. Does *whiff away* in America mean more or less than *withering*? What Professor Whitney should have objected to was the adverb *hardly*: I wish I had said *vix, et ne vix quidem*.

a disadvantage to me in rightly understanding the first glimmerings of astronomical ideas among the Hindus. Be that as it may, I believe that at the present moment few scholars of repute doubt the native origin of the Nakshatras, and hardly one admits an early influence of Babylonian or Chinese science on India. I stated my case in the preface to the fourth volume of my edition of the Rig-Veda, and if anybody wishes to see what can be done by misrepresentation, let him read what is written there, and what Professor Whitney made of it in his articles in the 'Journal of the American Oriental Society.' His misunderstandings are so desperate that he himself at times feels uneasy, and admits that a more charitable interpretation of what I wanted to say would be possible. When I saw this style of arguing, the utter absence of any regard for what was, or what might charitably be supposed to have been, my meaning, I made up my mind once for all, that that American gentleman should never have an answer from me, and in spite of strong temptation I kept my resolve till now. A man who could say of Lassen that his statements were 'wholly and reprehensibly incorrect,' because he said that Colebrooke had shown that the Arabs received their lunar mansions from the Hindus, was not likely to show mercy to any other German professor.

I find, however, by reading one of his Essays, that there is a more special reason why, in his repeated onslaughts on me, both before and after the Rebellion, 'he thinks he may dispense with the ordinary courtesies of literary warfare.' I may tell it in his own words:

'Some one (I may add the name, now, it was the late Professor

Goldstücker) falls fiercely upon the work of a company of collaborators; they unite in its defence; thereupon the aggressor reviles them as a mutual-admiration society; and Müller repeats the accusation, giving it his own indorsement, and volunteering in addition that of another scholar.'

I might possibly represent the case in a different light, but I am willing to accept the *acte d'accusation*, as it comes from the hand of my accuser; nay more, I am quite ready to plead guilty to it. Only let me explain how I came to commit this great offence. What is here referred to must have happened more than ten years ago. Professor Goldstücker had criticised the Sanskrit Dictionary published by Professors Boehtlingk and Roth, and 'the company of collaborators' had united in its defence, only, as Professor Whitney is authorised to assure us, 'without any apparent or known concert.' Professor Goldstücker was an old friend of mine, to whom, in the beginning of my literary career at Berlin and in Paris, I was indebted for much personal kindness. He helped me when no one else did, and many a day, and many a night too, we had worked together at the same table, he encouraging me to persevere when I was on the point of giving up the study of Sanskrit altogether. When Professor Goldstücker came to England, he undertook a new edition of Wilson's 'Sanskrit Dictionary,' and he very soon became entangled in a controversy with 'the company of collaborators' of another Sanskrit dictionary, published at the expense of the Russian Academy. I do not defend him, far from it. He had a weakness very common among scholars;—he could not bear to see a work praised beyond its real merits, and he thought it was his duty to set everything right

that seemed to him wrong. He was very angry with me, because I would not join in his condemnation of the St. Petersburg dictionary. I could not do that, because, without being blind to its defects, I considered it a most valuable performance, highly creditable to all its collaborators; nay, I felt bound to say so publicly in England, because it was in England that this excellent work had been unduly condemned. This embittered my relations with Professor Goldstücker, and when the attacks by the company of collaborators on him grew thicker and thicker, while I was treated by them with the greatest civility, he persuaded himself that I had taken part against him, that I had in fact become a sleeping partner in what was then called the 'International Praise Insurance Society.' To show him once for all that this was not the case, and that I was perfectly independent of any company of collaborators, I wrote what I wrote at the time. Nor did I do so without having had placed before me several reviews, which certainly seemed to give to the old saying *laudari a viro laudato* a novel meaning. Having done what I thought I was bound to do for an old friend, I was perfectly prepared to take the consequences of what might seem a rash act, and when I was twitted with having done so anonymously, I, of course, thought it my duty to reprint the article, at the first opportunity, with my name. Now let it be borne in mind that one of the chief culprits, nay, as appeared afterwards, the most eager mischief-maker, was Professor Whitney himself, and let us now hear what he has to say. As if he himself were entirely unconcerned in the matter, instead of having been the chief culprit, he

speaks of 'cool effrontery;' 'magisterial assumption, towards a parcel of naughty boys caught in their naughtiness;' 'most discreditable;' 'the epithet outrageous is hardly too strong.' Here his breath fails him, and, fortunately for me, the climax ends. And this, we are asked to believe, is not loud and boisterous, but gentle and calm: it is in fact 'the language of simple-minded consciousness of rectitude'!

These gentle onslaughts were written and published by Professor Whitney ten years ago. I happen to know that a kind of *colportage* was established to send his articles to gentlemen whom they would not otherwise have reached. I was told again and again that I ought to put an end to these manœuvres, and yet, during all these years, I thought I could perfectly well afford to take no notice of them. But when after such proceedings Professor Whitney turns round, and challenges me before a public which is not acquainted with these matters, to produce any of the *epitheta ornantia* I had mentioned as having been applied by him to me, to Renan, to Schleicher, to Oppert, to Bleek, nay, even to Bopp and Burnouf and Lassen, when with all 'the simple-minded consciousness of rectitude' he declares, that he was never personal, then I ask, Could I remain silent any longer?

How hard Professor Whitney is driven in order to fix any real blame on me, may be seen from what follows. The article in which the obnoxious passage which, I was told, deprived me of any claim to the amenities of literary intercourse occurs, had been reprinted in the 'Indische Studien,' before I reprinted it in the first volume of 'Chips.' In reprinting it

myself, I had rewritten parts of it, and had also made a few additions. In the 'Indische Studien,' on the contrary, it had been reprinted in its original form, and had besides been disfigured by several inaccuracies or misprints. Referring to these, I had said that it had been, as usual, very incorrectly reprinted. Let us hear what an American pleader can make out of this:

'In this he was too little mindful of the requirements of fair dealing; for he leaves any one who may take the trouble to turn to the "Indische Studien," and compare the version there given with that found among the "Chips," to infer that all the discordances he shall discover are attributable to Weber's incorrectness, whereas they are in fact mainly alterations which Müller has made in his own reprint; and the real inaccuracies are perfectly trivial in character and few in number—such printer's blunders as are rarely avoided by Germans who print English, or by English who print German. We should doubtless be doing Müller injustice if we maintained that he deliberately meant Weber to bear the odium of all the discrepancies which a comparer might find; but he is equally responsible for the result, if it is owing only to carelessness on his part.'

What will the intelligent gentlemen of the jury say to this? Because I complained of such blunders as altars being 'construed,' instead of 'constructed,' 'enlightoned' instead of 'enlightened,' 'gratulate' instead of 'congratulate,' and similar inaccuracies, occurring in an unauthorised reprint of my article, therefore I really wanted to throw the odium of what I had myself written in the original article, and what was, as far as the language was concerned, perfectly correct, on Professor Weber. Can forensic ingenuity go further? If America possesses many such powerful pleaders, we wonder how life can be secure.

Having thus ascertained whence *illae lacrumae*,

I must now produce a small bottle at least of the tears themselves which Professor Whitney has shed over me, and over men far better than myself, all of which, he says, were never meant to be personal, and most of which have evidently been quite dried up in his memory.

I begin with Bopp. 'Although his mode of working is wonderfully genial, his vision of great acuteness, and his instinct a generally trustworthy guide, he is liable to wander far from the safe track, and has done not a little labour over which a broad and heavy mantle of charity needs to be drawn' (I. 208).

M. Renan and myself have 'committed the very serious error of inverting the mutual relation of dialectic variety and uniformity of speech, thus turning topsy-turvy the whole history of linguistic development. . . It may seem hardly worth while to spend any effort in refuting an opinion of which the falsity will have been made apparent by the exposition already given' (p. 177).

In another place (p. 284) M. Renan is told that his objection to the doctrine of a primitive Indo-European monosyllabism is noticed, not for any cogency which it possesses, but only on account of the respectability of M. Renan.

Lassen and Burnouf, who thought that the geographical reminiscences in the first chapter of the Vendidad had a historical foundation, are told that their 'claim is baseless, and even preposterous' (p. 201). Yet what Professor Whitney's knowledge of Zend must be, we may judge from what he says of Burnouf's literary productions. 'It is well known,' he says, 'that the great French scholar produced *two or three bulky volumes* upon the Avesta.' I know of *one* bulky volume only, 'Commentaire sur la Yaçna,' tome i. Paris, 1833, but that may be due to my lamentable ignorance.

'Professor Oppert simply exposes himself in the somewhat ridiculous attitude of one who knocks down, with gestures of awe and fright, a tremendous man of straw of his own erecting (I. 218). His erroneous assumptions will be received with most derisive incredulity (I. 221); the incoherence and aimlessness of his reasonings (I. 223); an ill-considered tirade, a tissue of misrepresentations of linguistic science (I. 237). He cannot impose upon us by his authority, nor attract us by his eloquence: his present essay is as heavy in style, as loose and vague in expression, unsound in argument, arrogant in tone' (I. 238). The motive imputed to Professor Oppert in writing his Essay is that 'he is a Jew, and wanted to stand up for the Shemites.'

If Professor Oppert is put down as a Shemite, Dr. Bleek is sneered at as a German. 'His work is written with much apparent profundity, one of a class, not quite unknown in Germany, in which a minimum of valuable truth is wrapped up in a maximum of sonating phraseology' (I. 292). Poor Germany catches it again on page 315. 'Even, or especially in Germany,' we are told, 'many an able and acute scholar seems minded to indemnify himself for dry and tedious grubbings among the roots and forms of Comparative Philology by the most airy ventures in the way of constructing Spanish castles of linguistic science.'

In his last work Professor Whitney takes credit for having at last rescued the Science of Language from the incongruities and absurdities of European scholars.

Now on page 119 Professor Whitney very properly reproves another scholar, Professor Goldstücker, for having laughed at the *German* school of Vedic interpretation. 'He emphasises it,' he says, 'dwells upon it, reiterates it three or four times in a paragraph, as if there lay in the words themselves some potent argument. Any uninformed person would say, we are confident that he was making an unworthy appeal to English prejudice against foreign men and foreign ways.' Professor Whitney finishes up with charging Professor Goldstücker, who was himself a German—I beg my reader's pardon, but I am only quoting from a North American Review—with 'fouling his own nest.' Professor Whitney, I believe, studied in a German university. Did he never hear of a 'cute little bird, who does to the nest in which he was reared, what he says Professor Goldstücker did to his own?

Χαῖρέ μοι, ὦ Γώλδστυκρε, καὶ εἰν Ἀΐδαο δόμοισιν·
Πάντα γὰρ ἤδη τοι τελέω, τὰ πάροιθεν ὑπέστην.

Haeckel is called a headlong Darwinian (I. 293), Schleicher is infected with Darwinism (I. 294), 'he represents a false and hurtful tendency (I. 298), he is blind to the plainest truths, and employs a mode of reasoning in which there is neither logic nor common sense (I. 323). His essays are unsound, illogical, untrue; but there are still incautious sciolists by whom every error that has a great name attached to it is liable to be received as pure truth, and who are ever specially attracted by good hearty paradoxes' (I. 330).

I add a few more references to the *epitheta ornantia* which I was charged with having invented. 'Utter futility' (p. 36); 'meaningless and futile' (p. 152); 'headlong materialist' (p. 153); 'better humble and true (Whitney) than high-flown, pretentious, and false' (not-Whitney, p. 434); 'simply and solely nonsense' (I. 255); 'darkening of counsel by words without knowledge' (I. 255); 'rhetorical talk' (I. 273); 'flourish of trumpets, lamentable (not to say) ridiculous failure' (I. 277).

What a contrast between the rattling discharges of these *mitrailleuses* at the beginning of the war, and the whining and whimpering assurance now made by the American professor, that he never in his life said anything personal or offensive!

Why I ought not to have answered.

Having taken the trouble of collecting these spent balls from the various battlefields of the American general, I hope that even Professor Whitney will no

longer charge me with having spoken without book. As long as he cited me before the tribunal of scholars only, I should have considered it an insult to them to suppose that they could not, if they liked, form their own judgment. For fifteen years have I kept my fire, till, like a Chinese juggler, Professor Whitney must have imagined he had nearly finished my outline on the wall with the knives so skilfully aimed to miss me. But when he dragged me before a tribunal where my name was hardly known, when he thought that by catching the *aura popularis* of Darwinism, he could discredit me in the eyes of the leaders of that powerful army, when he actually got possession of the pen of the son, fondly trusting it would carry with it the weight of the father, then I thought I owed it to myself, and to the cause of truth and its progress, to meet his reckless charges by clear rebutting evidence. I did this in my 'Answer to Mr. Darwin,' and as I did it, I did it thoroughly, leaving no single charge unanswered, however trifling. At the same time, while showing the unreasonableness of his denunciations, I could not help pointing out some serious errors into which Professor Whitney had fallen. Some thrusts can only be parried by *a-tempo* thrusts.

Professor Whitney, like an experienced advocate, passes over in silence the most serious faults which I had pointed out in his 'Lectures,' and after he has attempted—with what success, let others judge—to clear himself from a few, he turns round, and thinks it best once for all to deny my competency to judge him. And why?

'I do not consider Professor Müller capable of judging me justly,' he says. And why? 'Because I have felt moved, on account of his extraordinary popularity and the exceptional importance attached

to his utterances, to criticise him more frequently than anybody else.'

Is not this the height of forensic ingenuity? Because A has criticised B, therefore B cannot criticise A justly. In that case A has indeed nothing to do but to criticise B C D to Z, and then no one in the world can criticise him justly. I have watched many controversies, I have observed many stratagems and bold movements to cover a retreat, but nothing to equal this. Professor Pott was very hard on Professor Curtius, but he did not screen himself by denying to his adversary the competency to criticise him in turn. What would Newman have said, if Kingsley had tried to shut him up with such a remark, a remark really worthy of one literary combatant only, the famous Pastor Goeze, the critic of Lessing?

What would even Professor Whitney think, if I were to say that, because I have criticised his 'Lectures,' he could not justly criticise my 'Sanskrit Grammar?' He might not think it good taste to publish an advertisement to dissuade students in America from using my grammar; he might think it unworthy of himself and dishonourable to institute comparisons, the object of which would be too transparent in the eyes even of his best friends in Germany. Mr. Whitney has lived too long in Germany not to know the saying, *Man merkt die Absicht und man wird verstimmt.* But should I ever say that he was incompetent to criticise my 'Sanskrit Grammar' justly? Certainly not. All that I might possibly venture to say is, that before Professor Whitney undertakes to criticise my own or any other Sanskrit grammar, he should look at § 84 of my grammar, and practise that very simple

rule, that if Visarga is preceded by *a*, and followed by *a*, the Visarga is dropt, *a* changed to *o*, and the initial vowel elided. If with this rule clearly impressed on his memory, he will look at his edition of the Atharva-Veda Prâtisâkhya, I. 33, then perhaps, instead of charging Hindu grammarians in his usual style with 'opinions obviously and grossly incorrect and hardly worth quoting,' he might discover that eke sp*r*ish*t*am could only have been meant in the MSS. for eke 'sp*r*ish*t*am, and that the proper translation was not that vowels are formed *by contact*, but that they are formed *without contact*. Instead of saying that none of the other Prâtisâkhyas favours this opinion, he would find the same statement in the Rig-Veda Prâtisâkhya, Sûtra 719, page cclxi of my edition, and he might perhaps say to himself, that before criticising Sanskrit grammars, it would be useful to learn at least the phonetic rules. I had pointed out this slip before, in the second edition of my 'Sanskrit Grammar;' but, as to judge from an article of his on the accent, Professor Whitney has not seen that second edition (1870), which contains the Appendix on the accent in Sanskrit, I beg leave to call his attention to it again.

Why I ought to be Grateful.

I am glad to say that we now come to a more amusing part of this controversy. After I had been told that because I was attacked first, therefore I was not able to criticise Professor Whitney's writings justly, I am next told that I ought to be very grateful for having been attacked, nay, I am told that, in

my heart of hearts, I am really very grateful indeed. I must quote this passage in full:

'During the last eight years I have repeatedly taken the opportunity accurately to examine and frankly to criticise the views of others and the arguments by which they were supported. I have done this more particularly against eminent and famous men whom the public has accustomed itself to regard as guides in matters referring to the Science of Language. What unknown and uncared for people say, is of no consequence whatever: but if Schleicher and Steinthal, Renan and Müller, teach what to me seems an error, and try to support it by proofs, then surely I am not only justified, but called upon to refute them, if I can. Among these students the last-named seems to be of different opinion. In his article, "My Reply to Mr. Darwin," published in the March number of the "Deutsche Rundschau," he thinks it necessary to read me a severe lecture on my presumption, although he also flatters me by the hint that my custom of criticising the most eminent men only is appreciated, and those whom I criticise feel honoured by it.'

I confess when I read this, I wished I had really paid such a pretty compliment to my kind critic, but, looking through my article from beginning to end, I find no hint anywhere that could bear so favourable an interpretation, unless it is where I speak of 'the noble army of his martyrs,' and of the untranslated remark of Phocion, which he may have taken for a compliment. In saying that it was acknowledged to be an honour to be attacked by him, Professor Whitney was, no doubt, thinking of the words of Ovid, *Summa petunt dextra fulmina missa Jovis*, and I am not going in future to deny him the title of the Jovial and Olympian critic, nor should I suggest to him to read the line in Ovid immediately preceding the one quoted. Against one thing only I must protest. Though the last named, I am surely not, as he boldly asserts, the only one of the four *sommités* struck by his Olympian thunderbolts, who

have humbly declined too frequent a repetition of his celestial favours. Schleicher, no doubt, was safe, for alas, he is dead! But Steinthal surely has uttered rather Promethean protests against the Olympian,

Οἶδ' ὅτι τραχὺς καὶ παρ' ἑαυτῷ
τὸ δίκαιον ἔχων Ζεύς· ἀλλ' ἔμπας
μαλακογνώμων
ἔσται ποθ', ὅταν ταύτῃ ῥαισθῇ·

and as to M. Renan, does his silence mean more than—

Ἐμοὶ δ' ἔλασσον Ζηνὸς ἢ μηδὲν μέλει?

I confess, then, frankly that, in my heart of hearts, I am not grateful for these cruel kindnesses, and if he says that the other Serene Highnesses have been less ungrateful than I am, I fear this is again one of his over-confident assertions. My publishers in America may be grateful to him, for I am told that, owing to Professor Whitney's articles, much more interest in my works has been excited in America than I could ever have expected. But I cannot help thinking that by the line of action he has followed, he has done infinite harm to the science which we both have at heart. In order to account somehow or other for his promiscuous onslaughts, he now tells Mr. Darwin and his friends that in the Science of Language all is chaos. That is not so, unless Mr. Whitney is here using chaos in a purely subjective sense. There are differences of opinion, as there are in every living and progressive science, but even those who differ most widely, perfectly understand and respect each other, because they know that, from the days of Plato and Aristotle, men who start from different points, arrive at

different conclusions, particularly when the highest problems in every science are under consideration. I do not agree with Professor Steinthal, but I understand him; I do not agree with Dr. Bleek, but I respect him; I differ most of all from Schleicher, but I think that an hour or two of private conversation, if it were possible still, would have brought us much nearer together. At all events, in reading any of their books, I feel interested, I breathe a new atmosphere, I get new ideas, I feel animated and invigorated. I have now read nearly all that Professor Whitney has written on the Science of Language, and I have not found one single new fact, one single result of independent research, nay, not even one single new etymology, that I could have added to my Collectanea. If I am wrong, let it be proved. That language is an institution, that language is an instrument, that we learn our language from our mothers, as they learned it from their mothers and so on till we come to Adam and Eve, that language is meant for communication, all this surely had been argued out before, and with arguments, when necessary, as strong as any adduced by Professor Whitney.

Professor Whitney may not be aware of this, or have forgotten it; but a fertile writer like him ought at all events to have a good memory. In his reply, p. 262, he tells us, for instance, as one of his latest discoveries, that in studying language, we ought to begin with modern languages, and that when we come to more ancient periods, we should always infer similar causes from similar effects, and never admit new forces or new processes, except when those which we know prove totally inefficient. In

my own Lectures I had laid it down as one of the fundamental principles of the Science of Language that 'what is real in modern formations must be admitted as possible in ancient formations, and that what has been found true on a small scale may be true on a larger scale.' I had devoted considerable space to the elucidation of this principle, and what did Professor Whitney write at that time (1865)?

'The conclusion sounds almost like a bathos: we should have called these, not fundamental principles, but obvious considerations, which hardly required any illustration' (p. 243).

Here is another instance of failure of memory. He assures us:

'That he would never venture to charge anybody with being influenced in his literary labours by personal vanity and a desire of notoriety, except perhaps after giving a long string of proofs—nay, not even then' (p. 274).

Yet it was he who said of (I. 131) the late Professor Goldstücker that—

'Mere denunciation of one's fellows and worship of Hindu predecessors do not make one a Vedic scholar,'

and that, after he had himself admitted that 'no one would be found to question his (Professor Goldstücker's) immense learning, his minute accuracy, and the sincerity and intensity of his convictions.'

By misunderstanding and sometimes, unless I am greatly mistaken, wilfully closing his eyes to the real views of other scholars, Professor Whitney has created for himself a rich material for the display of his forensic talents. Like the poor Hindu grammarian, we are first made to say the opposite of what we said, and are then brow-beaten as holding opinions 'obviously and grossly incorrect and hardly

worth quoting.' All this is clever, but is it right? Is it even wise?

Much of what I have here written sounds very harsh, I know; but what is one to do? I have that respect for language and for my friends, and, may I add, for myself, to avoid harsh and abusive words, as much as possible. I do not believe in the German saying, *Auf einen groben Klotz gehört ein grober Keil.* I have tried hard, throughout the whole of my literary career, and even in this 'Defence,' not to use the weapons that have been used against me during so many years of almost uninterrupted attacks. Much is allowed, however, in self-defence that would be blameable in an unprovoked attack, and if I have used here and there the cold steel, I trust that clean wounds, inflicted by a sharp sword, will heal sooner than gashes made with rude stones and unpolished flints.

Professor Whitney might still, I feel convinced, do some very useful work, as the apostle of the Science of Language in America, if only, instead of dealing in general theories, he would apply himself to a critical study of scientific facts, and if he would not consider it his peculiar calling to attack the personal character of other scholars. If he must needs criticise, would it be quite impossible for him, even in his character of Censor, to believe that other scholars are as honest as himself, as independent, as outspoken, as devoted at all hazards to the cause of truth? Does he really believe in his haste that all men who differ from him, or who tell him that he has misapprehended their teaching, are humbugs, pharisees, or liars? Professor Steinthal was a great friend of his, does he imagine that his violent resent-

ment was entirely unprovoked? I have had hundreds of reviews of my books, some written by men who knew more, some by men who knew less than myself. Both classes of reviews proved very useful, but, beyond correcting matters of fact, I never felt called upon to answer, or to enter into personal recriminations with any one of my reviewers. We should not forget that, after all, reviews are written by men, and that there are often very tangible reasons why the same book is fiercely praised and fiercely abused. No doubt, every writer who believes in the truth of his opinions, wishes to see them accepted as widely as possible; but reviews have never been the most powerful engines for the propaganda of truth, and no one who has once known what it is to feel oneself face to face with Truth, would for one moment compare the applause of the many with the silent approval of the still small voice of conscience within. Why do we write? Chiefly, I believe, because we think we have discovered facts unknown to others, or arrived at opinions opposed to those hitherto held. Knowing the effort one has made oneself in shaking off old opinions or accepting new facts, no student would expect that everybody else would at once follow his lead. Indeed, we wish to differ from certain authorities, we wish to be criticised by them; their opposition is far more important, far more useful, far more welcome to us, than their approval could ever be. It would be an impossible task were we to attempt to convert personally every writer who still differs from us. Besides, there is no wheat without bran, and nothing is more instructive than to watch how the millstones of public opinion slowly and noiselessly separate the one from the other. I have brought my harvest, such as it was,

to the mill: I do not cry out when I see it ground. From my peers I have received the highest rewards which a scholar can receive, rewards far, far above my deserts; the public at large has treated me no worse than others; and, if I have made some enemies, all I can say is, I do not envy the man who in his passage through life has made none.

Even now, though I am sorry for what Professor Whitney has done, I am not angry with him. He has great opportunities in America, but also great temptations. There is no part of the civilized world where a scholar might do more useful work than in America, by the bold and patient exploration of languages but little known, and rapidly disappearing. Professor Whitney may still do for the philology of his country what Dr. Bleek has done for the languages of Africa at the sacrifice of a life-long expatriation, alas! I have just time to add, at the sacrifice of his life.

But I admit that America has also its temptations. There are but few scholars there who could or would check Professor Whitney, even in his wildest moods of asseveration, and by his command of a number of American papers, he can easily secure to himself a temporary triumph. Yet, I believe, he would find a work, such as Bancroft's 'On the Native Races of the Pacific States of North America,' a far more useful contribution to our science, and a far more permanent monument of his life, than reviews and criticisms, however brilliant and popular.

It was because I thought Professor Whitney capable of rendering useful service to the Science of Language in America that I forbore so long, that I never for years noticed his intentional rudeness and arro-

gance, that I received him, when he called on me at Oxford, with perfect civility, that I assisted him when he wanted my help in procuring copies of MSS. at Oxford. I could well afford to forget what had happened, and I tried for many years to give him credit for honorable, though mistaken, motives in making himself the mouthpiece of what he calls the company of collaborators.

In fact, if he had arraigned me again and again before a tribunal of competent judges, I should gladly have left my peers to decide between me and my American traducer. But when he cleverly changed the venue and brought his case before a tribunal where forensic skill was far more likely to carry the day than complicated evidence that could be appreciated by a special jury only, then, at last, I had to break through my reserve. It was not exactly cowardice that had kept me so long from encountering the most skilful of American swordsmen, but when the duel was forced upon me, I determined it should be fought out once for all.

I might have said much more; in fact, I had written much more than what I here publish in self-defence, but I wished to confine my reply as much as possible to bare facts. Professor Whitney has still to learn, it seems, that in a duel, whether military or literary, it is the bullets which hit, not the smoke, or the report, however loud. I do not flatter myself that with regard to theories on the nature of language or the relation between language and thought, there ever will be perfect unanimity among scholars, but as to my bullets or my facts, I believe the case is different. I claim no infallibility, however, and would not accept the papal tiara among comparative philologists, even

though it was offered me in such tempting terms by the hands of Professor Whitney. In order, therefore, to satisfy Mr. Darwin, Professor Haeckel and others whose good opinion I highly value, because I know that they care for truth far more than for victory, I now appeal to Professor Whitney to choose from among his best friends three who are *Professores ordinarii* in any university of England, France, Germany, or Italy, and by their verdict I promise to abide. Let them decide the following points as to simple matters of fact, the principal bones of contention between Professor Whitney and myself:

1. Whether the Latin of the inscription on the Duilian Column represents the Latin as spoken in 263 B.C. (p. 446);
2. Whether Ahura-Mazda can be rendered by 'the mighty spirit' (p. 446);
3. Whether sarvanâma in Sanskrit means 'name for everything' (p. 446);
4. Whether Professor Whitney knew that the Phenician alphabet had by Rougé and others been traced back to an Egyptian source (pp. 446, 467, 485);
5. Whether Professor Whitney thought that the words *light*, *alight*, and *delight* could be traced to the same source (p. 484);
6. Whether in the passages pointed out on pp. 450–451, Professor Whitney contradicts himself or not;
7. Whether he has been able to produce any passage from my writings to substantiate the charge that in my Lectures I was impelled by an overmastering fear lest man should lose his proud position in the creation (p. 451);

8. Whether there are *verbatim* coincidences between my Lectures and those of Professor Whitney (pp. 441, 489, 491);
9. Whether I ever denied that language was made through the instrumentality of man (p. 487);
10. Whether I had or had not fully explained under what restrictions the Science of Language might be treated as one of the physical sciences, and whether Professor Whitney has added any new restrictions (pp. 438 seq., 492 seq.);
11. Whether Professor Whitney apprehended in what sense some of the greatest philosophers declared conceptual thought impossible without language (p. 501);
12. Whether the grammatical blunder, with regard to the Sanskrit pari tasthushas as a nominative plur., was mine or his (p. 506);
13. Whether I had not clearly defined the difference between hard and soft consonants long before Professor Whitney, and whether he has not misrepresented what I had written on the subject (p. 509).
14. Whether in saying that the soft consonants can be intonated, I could have meant that they may or may not be intonated (p. 515);
15. Whether I invented the terms vivârasvâsâghoshâh and samvâranâdaghoshâh, and whether they are to be found in no Sanskrit grammarian (p. 516);
16. Whether I was right in saying that Professor Whitney had complained about myself and others not noticing his attacks, and whether his remarks on my chapter on Fir, Oak, and Beech required being noticed (p. 518);

17. Whether I had invented the *Epitheta ornantia* applied by Professor Whitney to myself and other scholars, or whether they occur in his own writings (p. 522);
18. Whether E. Burnouf has written two or three bulky volumes on the Avesta, or only one (p. 533);
19. Whether Professor Whitney made a grammatical blunder in translating a passage of the Atharva-Veda Prâtisâkhya, and on the strength of it charged the Hindu grammarian with holding opinions 'obviously and grossly incorrect and hardly worth quoting' (p. 537);
20. Whether Professor Whitney has occasionally been forgetful (p. 541).

Surely there are among Professor Whitney's personal friends scholars who could say Yes or No to any of these twenty questions, and whose verdict would be accepted, and not by scholars only, as beyond suspicion. Anyhow, I can do no more for the sake of peace, and to put an end to the supposed state of chaos in the Science of Language, and I am willing to appear in person or by deputy before any such tribunal of competent judges.

I hope I have thus at last given Professor Whitney that satisfaction which he has claimed from me for so many years; and let me assure him that I part with him without any personal feeling of bitterness or hostility. I have grudged him no praise in former days, and whatever useful work we may receive from him in future, whether on the languages of India or of America, his books shall always receive at my hands the same justice as if they had been written by my best friend. I have never belonged to any

company of collaborators, and never shall; but whosoever serves in the noble army for the conquest of truth, be he private or general, will always find in me a faithful friend, and, if need be, a fearless defender. I gladly conclude with the words of old Fairfax (Bulk and Selvedge, 1674): 'I believe no man wishes with more earnestness than I do, that all men of learning and knowledge were men of kindness and sweetness, and that such as can outdo others would outlove them too; especially while self bewhispers us, that it stands us all in need to be forgiven as well as to forgive.'

THE MUMBLES, NEAR SWANSEA, WALES,
September, 1875.