CHAPTER XLVI

1860–93

SCIENCE

THE Duke's great interest in science brought him into correspondence with many of the eminent scientific men of his time. Some of these letters, relating as they do to isolated scientific facts, are now of little general interest; but the following extracts, which are grouped according to subject-matter and date, have been selected, as illustrating both the wideness of the Duke's range of scientific thought, and the thorough methods he employed in pursuing his investigations and inquiries after truth. The extracts are mostly taken from his correspondence with Sir Charles Lyell, described by the Duke as being at that time 'the great law-giver in the philosophy of geology'; Sir Richard Owen, the distinguished anatomist; Sir William Flower, the successor to Sir Richard Owen in the superintendence of the Natural History Departments of the British Museum; Sir John Murray, of the *Challenger* expedition; Mr. Herbert Spencer; Professor Tyndall; and Lord Kelvin.

ORGANIC EVOLUTION.

Organic evolution, to the consideration of which the Duke devoted much thought and study, and on which he wrote a treatise, published in 1898, is the subject of the following correspondence:

To Sir Charles Lyell (*February 29th*, 1860).

'I wish I had been able to talk over Darwin's book.* It is a most delightful one, suggesting endless subjects for discussion and inquiry. I think he fails fundamentally in these two cardinal points: First, in showing that in the existing or contemporary world breeding does effect any changes such as tend to the formation of new species. Second, he fails to show that in the past worlds there is any proof or clear evidence of such gradations of change as his theory requires.

'I am thoroughly dissatisfied, too, with the explanations by which the latter difficulty is met. Most ingenious argument is expended in trying to show how it was that such gradations should have been lost; but I wonder his result did not suggest the over-ingenuity of those arguments, when it is summed up in the assertion that "Nature has, as if on purpose, concealed her periods of transition."

'As regards the effects of breeding, I think the facts he gives in respect to pigeons tell more against than for his theory.

'Does he not tell us that by crossing a pure white and a jet black in two generations the progeny reverted to the well-known original type of the blue rock, with its double black bar on the wing coverts. What a wonderful fact! How many generations off from the original wild stock were these reverted birds?

'Then, there is another fact which I think he omits. Pigeons have been bred, he tells us, for some 3,000 years. Yet how little, how infinitesimal, has been the change in the more essential habits and instincts which specifically distinguish the wild stock from other Columbidæ! Take, for instance, its non-arboreal habit. Pigeons are now everywhere, in woody countries, surrounded by trees; yet who ever sees

* 'The Origin of Species.'

a tame pigeon light on a tree? Did you ever see one so light? I never did but once, and that under very peculiar circumstances.

'There are some chapters in Darwin's book which are invaluable, on the migration of species, natural selection, etc., and the whole is so full of curious information that, whether his theory is right or wrong, it is a perfect storehouse of knowledge.

'Of course, Darwin applies his theory to man among other Mammalia. But the record of geology is confessedly *more* complete in the later times, during which the species *Homo* must have been in course of "selection," from among his Quadrumanous progenitors.

'My belief is that all the yet ascertained facts are against such theories of development.

'I should vastly like to see some of Mr. Darwin's stores, illustrative of his curious information and experiments.'

From Sir Charles Lyell (*March 1st*, 1861).

'I am very much delighted with your address, both that part which treats of the flints and that on the more difficult and delicate question of Darwin's "Origin of Species," which you have entered on without timidity, and yet in such a way as no reasonable man can object to, least of all Darwin himself. The difficulties have nowhere been more clearly or candidly stated. . . .

'I could write on for ever, but must conclude. I am glad you paid a parting compliment to Darwin's book, which has done so much to promote science. The subject can never go back to where it was before he wrote.'

To Sir Charles Lyell (*March 2nd*, 1861).

'I am very glad you approve of what I have said—at least, as regards its pertinence on the Darwinian theory.

'I cannot see how single centres of "creation" or "development" can be reconciled with his view of the mode in which new species take their origin; but this is a point of great importance, which requires to be carefully thought out.

'The truth is that, even on the supposition that some pre-existing species has always been employed, so to speak, as a means of introducing derivative forms, we require, for the law under which such derivation arises, some suggestion or clue which Darwin fails to supply.'

From Sir Charles Lyell (January 25th, 1865).

'MY DEAR DUKE OF ARGYLL,

'I have to thank you for a separate copy of your most interesting address to the Royal Society, Edinburgh, which I read at Berlin, to which place the *Proceedings* were forwarded to me. I have never seen so clear a definition of the various senses in which the term "law" is used by scientific writers; and I think you have done a real service to the scientific and theological public by showing, what I am convinced is most true, that there is no tendency to materialism in the reasoning or speculations of modern naturalists and physicists, but quite the contrary. Your objection that Darwin has in some parts of his book made natural selection do more in the way of originating or creating than is admissible, or even consistent, with his own explanation of natural selection, was felt strongly by me, and at page 469 of my "Antiquity of Man" I said: "If we confound variation or natural selection with such creational laws, we deify secondary causes, or immeasurably exaggerate their influence." You have put it much better, but I felt as strongly that Darwin is inclined to believe that he has made a greater step in the direction of discovering and originating cause or law than he has really made.'

To Sir Charles Lyell (*January* 31*st*, 1865).

'I do not agree with you when you say that when use is discovered in any form of which we knew only the beauty before, a rebuke is administered to the idea that beauty is an object in the works of Nature. Would it be a rebuke to the idea of the carving on the handle of a war-club being for ornament, that we should discover the said carving to be also intended to give a better hold to the hand of the savage? Surely not. I have no doubt whatever that most of the beautiful forms in Nature are married to use in some way or another, just as in human art we combine ornament with adaptation to use.

'For example, some of the most beautiful surface ornaments on shells are simply the lines of the shell's annual growth. In like manner in the vegetable world some of the most beautiful lines are lines connected with structure and growth.

'I believe ornament to be pursued in Nature very much on the same principle on which man pursues it in his own works, and no amount of connection between use and beauty would affect my view of it.

'Have you ever thought of the extreme intricacy of the arrangement by which any given ornament is effected in the case of birds' feathers? For example, a bar of white on a bird's wing can be made out only by a great number of separate feathers being partly white in such degree and at such a point of their length as to fit the pattern when the bird's wing is extended or folded. Then, again, how curious the number of filaments in each feather, which must be coloured differently in different parts of its length to fit the corresponding differences of the other filaments, so that the whole shall produce a given effect. The "eye" of an Argus pheasant's wing or of a peacock's train is made up out of single filaments so coloured that, when lying in contact, the "eye" results.'

To Sir Charles Lyell (*March 22nd*, 1865).

'I had previously heard of a case where pigeons had been tempted and seduced (by corrupting arts and influences being brought to bear upon them) so far to forget themselves and what was due to the memory of their ancestors as to settle on trees.

'But the fact remains that this is a rare exception, and that the specific instinct of the rock-dove remains a characteristic of all its descendants of every variety of shape and form.

'I saw in the *Field* the other day the account of a woodcock perching in a spruce-fir. But these accidental instances of misconduct do not affect the character of the species.

'It will be observed, too, that in the case of pigeons settling on trees they always settle on the large boughs, which are sometimes as broad and almost as steady as the ridges of rock which are their real specific resting-places; whereas all the true arboreal doves light habitually on mere twigs, as other perching birds do. I often see the ringdove sitting on the top shoot of a fir.

'The most abandoned character among tame pigeons would never do this.'

To Sir Charles Lyell (*May 1st*, 1867).

'The general tenor of your letter supposes an antagonism on my part to the "natural selection" doctrine, which I do not entertain, so far as regards the preservation and extinction of species once "born" or once "created." My point is that natural selection can in no way and in no degree account for the particular direction which variations take, that direction being a determinate one, so that the new forms are "correlated" with external conditions, with a view to their success and establishment in Nature.

'I specially point out that this argument, though most important in the philosophy of the subject, is in no way necessarily antagonistic to Darwin's theory,

though it shows it to be incomplete, and I go so far as to say that my conclusion is one which Darwin may very possibly be willing to accept.

'But I stick to one point which you dispute—viz., that in order to the establishment of a new form, and the starting of it on its way, there must be a corresponding change in both the sexes. Considering the close "correlation" of the sexes, I have no doubt that this is always the case.'

From Sir Charles Lyell (19*th September*, 1868).

'I have just read with great interest your spirited and clearly-written article in reply to Wallace on nidification. If I did not feel sure that portions of it will be embodied in some of your future works, I should grudge its being placed in a periodical just struggling into existence, though it may perhaps be most usefully published in the same journal as the paper which it controverts. . . .

'I cannot believe that Darwin or Wallace can mean to dispense with that mind, of which you speak, as directing the forces of Nature. They, in fact, admit that we know nothing of the power which gives rise to variation in form, colour, structure, or instinct.'

On March 10th, 1875, the Duke delivered a lecture on 'Anthropomorphism in Theology' to a Presbyterian college in London, which he mentions in a letter to Professor Tyndall on April 23rd, 1875:

'I have sent you a copy of a lecture lately read by me before the young men connected with a college in London. I hope you will find nothing in it inconsistent with the sincere respect I entertain for your love of all discoverable truth. I have taken no part in the outcry about your Belfast address, because I thought it greatly misunderstood, and that its tendency is rather to spiritualize matter than to material-

ize thought. But I need not say that by this route we may come round very much to the same goal, and I think we must always keep separate in language the two most separate things which can be conceived in thought.'

With Sir William Flower the Duke had many interesting discussions, especially on the question of the 'prospective character of rudimentary organs,' a subject which deeply interested him.

To Professor Flower (*June* 11*th*, 1883).

'I am very glad if my questions have directed your attention with definite results to the curious problem as to the prospective or retrospective character of rudimentary organs in the Cetacea as well as in other animals.

'I am not sure that I quite understand your argument; but it will be best understood by seeing specimens, and I should be very glad some of these days to attend at the Museum and see any that you could show me.

'In the processes of ordinary generation it is quite clear that the future organs must be in the germ, and must in time have incipient parts, whether they are visible or not. Transmutation involves the supposition that the whole line of future development must be similarly present in all germs, potentially at least, with beginnings of actual structure visible at certain times. *A priori*, therefore, one would expect such structures to appear in any complete series of organisms.

'If they do not appear, I suppose we must take refuge in that convenient "bolt-hole" the "imperfection of the record."

'That all limbs should begin with integumentary foldings, unsupported by any internal structure, seems very strange.

To Professor Flower (*June 30th*, 1883).

'I have read with great care your most interesting lecture, so far as published.

'I see that the principle for which I am looking as probably to be found in biology is virtually involved in a fact which has long been recognised in comparative anatomy, and which you specially dwell upon as exemplified in the whales, that fact being this—that in all cases of highly-specialized organs they are nothing more than an abnormal development of rudimentary structures, always to be found in the generalized forms.

'Thus you trace the baleen, which is a most peculiar specialization, to a development of certain "papillæ" which are to be found in the palate structure of all the mammalia.

'I need not say that this, so far as it goes, agrees with my idea that on the evolution hypothesis we ought to find structures *on the way* to functional importance, as well as structures *on the way* to final disappearance and extinction. Of course, papillæ are mere germs, but they are germs with a "potential" value, and are, as it were, the roots of growths which could not have arisen without the previous establishment of the roots.'

To Professor Flower (*July 8th*, 1883).

'Your second half is to me even more interesting than your first half (lectures on the whales). I see that the toothed whales have the least or smallest remnants of the quadrupedal limbs, whilst the whalebone whales have the largest and most distinct remains of those limbs.

'I suppose that on the theory of loss by atrophy and disuse this fact would point to the whalebone whales being the nearest to the parent stock—the youngest, because the least aberrant from the original

mammalian type—so far as limbs are concerned. If so, how does this doctrine apply to the appearance and development of whalebone as opposed to teeth? Clearly, as regards them, the whalebone whales are the most aberrant, the most differentiated from the original type, whilst as regards the limbs, they are the least differentiated, the least aberrant.

'You showed me some whale skeletons in which the whalebone was combined with teeth in full functional use, some in which the whalebone was quite subordinate as regards use. Are we to conclude that these are on the road to be full whalebone whales, or are these again cases of aborted and dying out remnants of growths which were once more fully developed?

'As regards functional use, I can't believe that small fringes of whalebone are at all required for the capture of ordinary fish-prey. Whalebone is a marvellous adaptation for the capture of minute organisms, but for this alone; and, therefore, the half-whalebone whales look very much like creatures having a special development beginning before its utility, or at least its necessity, has actually arisen.

'Until we can come to some conclusion on these questions, we may be quite sure, indeed, of the general fact of evolution, but we can know nothing of the tracks which it has followed.

'In marine animals, if anywhere, the record may be comparatively complete, and in the whales we may possibly recognise the line which development has followed.'

The Duke always rendered justice to the great knowledge and ability of Mr. Charles Darwin, although there was a wide divergence of opinion between them; and Mr. Darwin expressed to a friend his sense of 'the courtesy and deference' with which he was received by the Duke at Argyll Lodge.

The following letters from the Duke to Professor (now Sir George) Darwin contain allusions to his father, Mr. Charles Darwin:

To Professor Darwin (*January 17th*, 1888).

'Many years ago I recollect being struck, and at the moment puzzled, by a passage in one of your father's books in which he said that the teleological or "creation" theory "*would explain anything.*"

'At first sight this seemed a strange objection, but on farther thought I soon saw that your father was quite right in stating it as an objection, because an explanation which will cover everything in general can hardly be an explanation of anything in particular.

'Subsequently, it has appeared to me that the theory of "natural selection" was so vague and metaphorical that it is itself open to precisely the same objection. There is no phenomenon in biology to which the formula may not be made to apply with, perhaps, a little stretching.

'Now, both my papers in the *Nineteenth Century* were written before your father's Life was published.

'In the last of these I have referred to the "explain anything" argument, admitting its force, but pointing out its equal applicability to metaphors which do not represent the physical causes.

'When, therefore, I came to read the Life, I was amused and interested to see that some nameless friend had brought this objection before your father, who calls it "rather a queer objection." He must have forgotten his own old observation to the like effect. I do not think he saw the exact point of it. He says, "I quite agree with it."

'As the point has not been, I think, much noticed, if at all, I was rather curious to know who the objector was.'

To Professor Darwin (a few days later).

'What I mean by "explaining anything" is this: that, for example, in the case objected to by your father, "provision of Nature" will "explain anything." It explains "why," but not "how." It has no bearing on physical causation.

'In like manner, "natural selection" will explain anything, because, as Herbert Spencer now admits, it does not represent any true physical causation, or, at least, deals with it on assumptions which can be applied to anything.

'It applies to all improvements in human machines quite as well as to animal mechanism.

'And as regards animals, it can be applied to every kind of variation equally. If, for example, among the snipes, one bird's bill bends down and another bends up, and a third is twisted sideways (as actually happens), we can account for each equally well by assuming some unknown special use as determining both the origin and the preservation of the special form.

'I don't know any monstrosity of form, if actually propagated, which might not equally well be explained by the same assumptions.'

The point alluded to in this letter is dwelt upon in the Duke's treatise on 'Organic Evolution Cross-examined' (p. 88), as follows:

'The truth is that the phrase "natural selection," and the group of ideas which hide under it, is so elastic that there is nothing in heaven or on earth that by a little ingenuity may not be brought under its pretended explanation. Darwin in 1859-1860 wondered "how variously" his phrase had been "misunderstood." The explanation is simple: it was because of those vague and loose analogies which are so often captivating. It is the same now, after thirty-six

years of copious argument and exposition. Darwin ridiculed the idea which some entertained that natural selection "was set up as an active power of deity"; yet this is the very conception of it which is at this moment set up by one of the most faithful worshippers in the Darwinian cult. Professor Poulton, of Oxford, gives to natural selection the title of a "motive power" first discovered by Darwin. This development is perfectly intelligible. Nature is the old traditional refuge for all who will not see the work of creative mind. Everything that is, everything that happens, is and happens naturally. Nature personified does, and is, our all in all. She is the universal agent, and at the same time the universal product. What she does she may easily be conceived as choosing to do, or selecting to be done, out of countless alternatives before her. Then, we have only to shut our eyes, blindly or conveniently, to the absolute difference between the idea of merely selecting out of already existing things, and of selecting by prevision out of conceivable things yet to be—we have only to cherish or even to tolerate this gross confusion of thought, and then we can cram into our theories of natural selection the very highest exercises of mind and will. Let us carry out consistently the analogy of thought involved in the agency of a human breeder; let us emancipate this conception from the narrow limits of operation within which we know it to be humanly confined; let us conceive a strictly homologous agency in Nature which has power not merely to select among organs already so developed as to be fit for use, but to select and direct beforehand the development of organs through many embryonic stages of existence, during which no use is possible; let us conceive, in short, an agency in Nature which keeps, as it were, a book in which "all our members are written, which in continuance are fashioned, when as yet there are none of them": then the phrase and the theory of natural selection may be accepted as at least some-

thing of an approach to an explanation of the wonderful facts of biological development.'

The assumption of the Darwinian school of thought that all structures which we now find to be imperfect or functionless are remains of structures once in full use, but now in course of gradual effacement, was challenged by the Duke, who considered that they might be in many cases rudimentary organs in process of development for future use. On this point he wrote two articles, which appeared in the *Nineteenth Century* of March and April, 1897. These articles touched on a difference of opinion between Lord Salisbury and Mr. Herbert Spencer, and they contain a clear exposition of the Duke's views on evolution.

In connection with the question of rudimentary organs, Professor Cossar Ewart's researches on the subject of the electric organs of the skate excited the interest of the Duke, who wrote to him (October 4th, 1888):

'I suppose the result to be that the electric organs of fish obey the general law that all highly-specialized organs are not new inventions, so to speak, of Nature, but simply normal elementary structures, specially developed for some special functions, or, in other words, are apparatuses made out of common materials for an uncommon purpose. This is a most interesting generalization, and seems to me to help greatly in reconciling the facts of development with the idea of creation. . . .

'This I apprehend to be the general result, with this further interest—that the change from the ordinary motor apparatus to the highly-specialized electric apparatus is a change gradually made, so that the electric apparatus can be detected "on the rise," being made while as yet its utility lies wholly in

the future. This is fatal to the Darwinian idea of "selection" as the physical cause of such structures.'

To Professor Cossar Ewart (*January 22nd*, 1889).

'I have read with great interest your paper on the rays—the last sent to me. You have got hold of a point having the highest interest in biology and philosophy, and I hope you will work it thoroughly. It is not improbable that these organs will be found to exist more widely than is now suspected.

'I was telling an old fisherman of mine here about the skate, when he told me that he now thinks that the common skate does give a slight shock when handled by the tail. But he confessed that he was hardly able to distinguish between such a very slight shock and the mere concussion produced by flapping and wriggling. The dislike of all fishermen to handle the skate is notorious. He reminded me that, as several species were *unarmed* as to spines, it could not be dread of them that caused the reluctance to handle the fish.

'You observe in your paper that if the organ could be traced to heredity, all difficulty would be removed. But I don't admit this, because that would only remove the difficulty a few generations farther back to the *first* Placoid that began to institute these organs.

'The explanation you suggest—that all muscular action involves electric discharges, and that these organs are merely a specialization of this fact—is, I have no doubt, the true explanation.

'Evolution absolutely demands the assumption that all highly-specialized organs must begin *in germ*, or potentially—that is, before *use* is possible.

'Hence "natural selection" can never explain the origin of anything. Electric organs are no exception really, but their rarity strikes us, and exhibits very

clearly the fallacy of putting actual use forward as a physical cause of the organ that is to be used.

'I hope you will prosecute the subject thoroughly, and describe the organs of the Nile Siluroids. The variety of parts in different fish which are converted into batteries is a point of special interest in the general argument.'

The correspondence between the Duke and Mr. Herbert Spencer shows that, although on some important subjects their views were widely opposed, their public controversy on these points did not affect the Duke's admiration of Mr. Herbert Spencer as a philosopher. In a letter to Mr. Spencer (March 4th, 1893) he wrote:

'I always read your books and papers with the greatest interest, often with partial, and sometimes with entire, agreement. . . .

'I differ from you in thinking that the admission of the hereditary transmission of "acquired characters" can lift natural selection out of the difficulties and insufficiencies which you specify as affecting it when that transmission is not allowed. In my opinion that metaphor "natural selection," as used by the Darwinian school, labours under inherent incompetencies to account for, or "explain," the phenomena of Nature, which are not at all remedied by the mere admission of the power of "acquired characters." But I am all the more grateful to you for "showing up" these incompetencies in any form or under any conditions.

'Meanwhile, we can all desire to ascertain *facts*.'

To Mr. Herbert Spencer (*December 7th*, 1893).

'You keep the philosophic tone and temper more perfectly than any writer I know.

'It so happens that at the present moment—whenever I can get a moment from politics—I am engaged in a close examination of your "Biology," and I think the tone I refer to is very conspicuous there.'

From Mr. Herbert Spencer (*January* 11*th*, 1897).

'DEAR DUKE OF ARGYLL,

'That much would have been added to my gratification had the list of names in the *Times** been reinforced by the name of one so distinguished in various spheres, it is needless for me to say; but its absence is more than compensated for by the expression of regret which you have been so kind as to send me. As being joined with the expression of partial disagreement, this is more to be valued than did entire agreement prompt it. This manifestation of sympathy between those whose opinions are in considerable degrees at variance is a favourable trait in our times, amid many traits which are unfavourable.'

To Mr. Herbert Spencer (*September* 26*th*, 1898).

'It is very kind of you to send a copy of your last volume to such a heretic as I am. I shall read it with great interest. There is always in your writings *much* that I agree with, and often I feel as if—behind a screen of highly-specialized phraseology—there was a great deal more of the same coincidence of conceptions.'

A clear statement of what the Duke refers to as the 'coincidence of conceptions' between himself and Mr.

* An address of congratulation had been presented to Mr. Herbert Spencer, signed by a number of eminent men, in recognition of the successful completion of his 'System of Synthetic Philosophy.'

Spencer is given in 'Organic Evolution Cross-examined' (p. 114):

'Let us, however, provisionally at least, accept the belief that organic life was first called into existence in the form of some three or four or five germs, each being the progenitor of one of the great leading types of the animal creation in respect to peculiarities of structure—one for the Vertebrata, one for the Mollusca, one for the Crustacea, one for the Radiata, and one for the Insecta. Let us assume, farther, on the same footing, that from each of these germs all the modifications belonging to each class have been developed by what we call the processes of ordinary generation. Then it follows that, as all these modifications have undoubtedly taken definite directions from invisible beginnings to the latest results and complexities of structure, the original germs must have been so constituted as to contain these complexities, potentially, within themselves. This conclusion is not in the least affected by any influence we may attribute to external surroundings. The Darwinian school in all its branches invariably dwells on external conditions as physical causes. But it is obvious that these can never act upon an organic mechanism except through and by means of a responsive power in that mechanism itself to follow the direction given to it, whether from what we call inside or outside things.

'This is no transcendental imagination, as some might think it. It is a conclusion securely founded on the most certain facts of embryology. It is the great peculiarity of organic development or growth that it always follows a determinate course to an equally determinate end. Each separate organ begins to appear before it can be actually used. It is always built up gradually for the discharge of functions which are yet lying in the future. In all organic growths the future dominates the present. All that goes on at any given time in such growths has exclusive reference

to something else that has yet to be done, in some other time which is yet to come. On this cardinal fact or law in biology there ought to be no dispute with Mr. Spencer. Numberless writers before him have, indeed, implied it in their descriptions of embryological phenomena and of the later growth of adapted organs. But, so far as I know, no writer before Mr. Spencer has perceived so clearly its universal truth, or has raised it to the rank of a fundamental principle of philosophy. This he has done in his "Principles of Biology," pointing out that it constitutes the main difference between the organic and the inorganic world. Crystals grow, but when they have been formed there is an end of the operation. They have no future. But the growth of a living organ is always premonitory of, and preparative for, the future discharge of some functional activity. As Mr. Spencer expresses it, "changes in inorganic things have no apparent relations to future external events which are sure or likely to take place. In vital changes, however, such relations are manifest."* This is an excellent generalization. It only needs that the word "relations" be translated from the abstract into the concrete. The kind of relation which is "manifest" is the relation of a previous preparation for an intended use. Unfortunately, Mr. Spencer is perpetually escaping or departing from the consequences of his own "manifest relations." In a subsequent passage of the same work he says,† "Everywhere structures in great measure determine functions." This is exactly the reverse of the manifest truth—that the future functions determine the antecedent growth of structure. This escape from his own doctrine on the fundamental distinction between the organic and the inorganic world is an escape entirely governed by his avowed aim to avoid language having teleological

* Spencer's 'Principles of Biology,' vol. i., ch. v., p. 73.

† *Ibid.*, vol. ii., ch. i., p. 4.

implications. But surely it is bad philosophy to avoid any fitting words because of implications which are manifestly true, and are an essential part of their descriptive power.

'If, therefore, we are to accept the hypothesis that all vertebrate animals, whether living or extinct, have been the offspring, by ordinary generation, of one single germ, originally created, then that original germ must have contained within itself certain innate properties of development along definite lines of growth, the issues of which have been forearranged and predetermined from the first. I have elsewhere* shown how this conception permeates, involuntarily, all the language of descriptive science when specialists take it in hand to express and explain the facts of biology to others. Huxley habitually uses the word "plan" as applicable to the mechanism of all organic frames.

'This is a theory of creation, by whatever other name men may choose to deceive themselves by calling it. It is a theory of development, too, of course, but of the development of a purpose. It is a theory of evolution also, but of evolution in its relation to an involution first. Nothing can come out that has not first been put in. It is not less a theory of creation which, whether true or not, gets rid absolutely of the elements of chance so valued by Darwin's more fanatical followers, and of the mere mechanical necessity which seems to be favoured by Mr. Spencer.'

Miscellaneous Zoology.

The following letter to Professor Owen is an evidence of the thorough manner in which the Duke investigated every subject on which he might be called upon to express an opinion :

* 'Philosophy of Belief,' ch. iii.

To Professor Owen (*July 4th*, 1854).

'Discussions have repeatedly arisen in Parliament in reference to the prohibition of *dog-carts*, upon the point whether or not dogs are physically unfitted for use as draught animals. Perhaps this question cannot be answered in the abstract, as local conditions, and the great power of adaptability found in breeding for a specific purpose, may enable dogs to be used, as in Siberia and other Northern regions, for purposes of draught, without doing great violence to the physical capabilities of the animal.

'The question must probably be answered, therefore, with special reference to the kind of use to which dogs are put, as draught animals, on the hard, dry roads of England.

'I am putting to you a *leading question* to a certain extent—that is to say, I have the strongest impression on my mind that the physical organization of the dog does not point it as intended for this sort of use, and that the circumstances and conditions under which it can be so used are the exception and not the rule.

'I should very much like to have your opinion upon this matter. Next Monday there is to be another discussion on the subject in the House of Lords, and a division probably on a proposed prohibition of dog-carts.'

From Professor Owen (*March 6th*, 1865), *acknowledging an article by the Duke on the Flight of Birds.*

'My dear Duke,

'I have had very great pleasure in the perusal of the enclosed; it is the best account of flight with which I am acquainted. The very few impulses to a marginal note have been made with misgiving and a query. With many thanks,

'Most faithfully your Grace's,

'Rd. Owen.'

To Sir Richard Owen (*March 4th*, 1885).

'Many thanks for your letter, from which I gather that you consider the Australian "dingo" to have been an immigrant along with the featherless biped, and not an indigenous mammal in Australia.

'My reason for asking is that I have had a letter from an Australian Bishop (not indigenous, clearly), finding fault with my statement that there was no native dog, but only a horrible caricature of our dear carnivore, alluding to the "tiger wolf."

'The non-indigenous Bishop thinks that I have forgotten the "dingo"; but as that was clearly non-indigenous, I was right enough.'

On the movement of diatoms, which he had been studying with the microscope, the Duke wrote to Sir John Murray (March 15th, 1887):

'It is not like mere ciliary movement. It is thoroughly under apparent control, with many incidents truly animal in their character. I saw one form fairly kick at some adherent dirt, with signs of impatience and irritation quite Gladstonian. The means of movement are inscrutable. I have watched them in a fine light, and with so high a power as to show all the flutings, etc., and *flagella* of adjacent organisms. Yet I see no signs of cilia or of currents in the water. Pure volition!'

Inveraray is situated on the shores of Loch Fyne, which is noted for the extraordinary abundance of herring to be found in its waters. The movements of these fish are sudden, swift, and capricious; the shoals appear and disappear in what seems to be an almost wholly arbitrary manner. These mysterious migrations greatly interested the Duke, and he mentions the subject in a letter to Sir John Murray (November 17th, 1891):

'I was away all last week. Before I left there was some play of herring fry in the loch—indeed, a good deal—but no big fish. When I came back last Saturday I heard that the big herring were in the deep. One boat got a good many near Strone Point. That night lots of boats came up from Skipness, Carradale, etc., and on Sunday there was a fleet at the pier. I went out last night in the launch, and saw them setting the drift-nets all over the loch. Trawlers got none, but drifters got a good fishing. To-night we counted one hundred and fifty boats between this and Kenmore. They say they feel them in the deep. How the deuce did they come? It was a glorious sight this evening at sunset. All down the loch the boats in groups all along the shore and in mid loch. Wouldn't you like to come to see the fleet and expiscate the facts?'

To Sir John Murray (*November* 10*th*, 1893).

'Have you been dredging at all this year? These Loch Fyne herrings are a profound mystery. For the last three weeks there seemed to be none here; no boats; reports of their being down at Otter. Suddenly this afternoon (a lovely one) a whole fleet appeared, and their sails and their smoke and their oars in the still water were too beautiful. No artist has ever been able to represent such a scene. I was driving along the road, and stopped the carriage to ask what it was all about. "Plenty of herring!" one boat shouted out. But they did not know exactly where. They had been "felt" by one or two crews accidentally, and some very fine fish were at breakfast this morning; but the fleet was "at sea" as to where to catch them, so they were watching all the bays and creeks.

'They seem to move about with great speed, or else to "lie low," and then suddenly "rise."'

'*April* 10*th*, 1896.

'MY DEAR MURRAY,

'You may commit any manner of poaching outrage you like on my salmon waters. I want much to know about kelts: what they do, where they go, and what they eat in the sea. I think one case is known of a salmon caught by a herring-net some forty miles off the land on the East Coast.

'The flat-fish caught in great numbers by lines in the Sound of Iona is a largish fish, of a yellow-brown, with very large spots of an orange-red. They are good when very fresh, but the flesh is very soft, and the bottom they affect is pure sand.

'I have caught in Loch Fyne very large flounders with similar large spots, but the ground colour is much darker, perhaps from assimilation to a darker habitat.

'I wish I could dredge with you. I should like much to see the fauna well searched. The large flounders are common near the head of the loch, in the shallower water of that region.

'You know that salmon don't ascend our rivers till late—the middle of June or so.'

From the Duke's letters to Mr. Harvie Brown on the subject of ornithology a few passages are quoted:

To Mr. Harvie Brown (*March 2nd*, 1888).

'I have never seen any notice of the peculiar habits of the heron at the pairing season. All birds, as you know, have some peculiarities of manner at that season. Storks and cranes seem (at the Zoological Gardens) to dance and caper on their long legs. The herons fly round in circles with a soaring flight, frequently stretching out their necks at nearly full length, which they never do in ordinary flight. The balance of the bird seems to require the long neck to be folded. But at this season, when love-making, they extend the neck and float about in the air in wheeling circles, round and above the trees where they are to nest.

'Yesterday morning, which was fine and sunny, there were nine herons floating and soaring in wide circles above the castle here, and in front of the steep wooded hill on which they build.

'The roller was first seen by me here on October 3rd, 1887, in the wild ground of the deer park. Its habits were markedly peculiar. After lighting on a tree it sat quite still, like a shrike, and then darted suddenly to the ground, returning to its perch like a fly-catcher.

'When it flew over my head, I saw that the flight was also very peculiar. Its wings were much longer than our jay, the primaries well separated at the tips, but not forming a very round-ended wing, like a jay or a crow. Its flight was flappy, but very strong, and it had the habit of descending before rising to a perch as jays do.

'It remained for more than a week in the same locality, and on one occasion was seen close to a keeper's cottage feeding with the poultry. It was very wary, and the keepers could not get at it.

'The great grey shrike has been seen by me twice here, and on the last occasion I got the bird, and it is now stuffed in the hall. On both occasions it came in November.

'The great spotted woodpecker has been shot here twice, once about fifty years ago, and again about fifteen years ago. I have both specimens.

'The osprey I have seen once. The black-throated diver comes sometimes to the small moor lochs on the hills.'

To Mr. Harvie Brown (*February 5th*, 1889).

'The story about my starlings is very simple. Having seen the bird in America, and having read accounts of its habits, it struck me that if any American bird could be introduced into Europe and established there, this would probably be a species likely to thrive. It is very pretty and very hardy in the New World.

'Accordingly, I asked a friend in New York to send me a small consignment of the red-shouldered starling alive. He did so three years ago. The birds arrived in good condition in October, but I thought it would be better to keep them till spring before turning them out, as migratory instincts might interfere if they were let out late in autumn. They throve quite well all through the winter, only two or three having died. In May about seven couple were turned out. They were seen about the place for a few weeks, and then disappeared. One or two were recorded as having been seen and shot in different parts of Scotland. My keepers think they saw a small party next autumn, and one or two were recorded that autumn as having been seen at one of the lighthouses in the South of Ireland, as if they were trying to migrate across the ocean. None have ever been seen since in this neighbourhood. As there is here a good deal of swampy meadowland, with bushes, in one of the glens, I thought they might have found a habitat such as, I believe, they principally affect in America.

'The attempt to acclimatize or naturalize the species has, so far, been a complete failure; but, as all new birds are speedily shot, or at least fired at, in this country, the failure is hardly surprising. I had hoped, too, that they might have consorted with the common starling, and thus escaped special notice, but the types are too distinct for this.'

To Mr. Harvie Brown (*March 8th*, 1889).

'I have not got your book on the capercailzie, and should be delighted to have it, if you are kind enough to send it to me. Some came here about fifteen years ago, and established themselves for a few years; but they have disappeared, several having been killed against the wire fences. None were shot.

'Squirrels were unknown here in my younger days, and I have no idea how they came. But they have

been established for at least twenty years, and became so numerous as nearly to ruin my fine silver firs, on which (alone, I think) they are most destructive. I have now shot them down to a small number, and the silver firs are already recovering. I was born and brought up at Ardencaple on the Clyde, and never saw a squirrel till I went to England. They are now common both at Arden Caple and at Rosneath.

'The same thing is true of the starling. There were none on the Clyde or here when I was a boy. Now they are abundant in both places. Here they have come within the last fifteen years, and are increasing.

'The yellow-hammer is decreasing, alas! rapidly. I hardly ever see them here now, and they used to be common.

'In 1841-1845 the swallow-tailed kite was abundant here. I have seen nine sailing round the castle. They have disappeared absolutely—all killed off by the keepers. No doubt they were very destructive.'

To Mr. Harvie Brown (*November* 30*th*, 1890).

'I have not seen the harrier for many years. They also have been destroyed. But about twenty years ago I found the nest and eggs on one of our moors here. The kingfisher comes at intervals. We had a couple here this last September. I saw them myself.

'The gray shrike I have seen twice, both times in November. The last one I "secured," and it is now stuffed in the hall here. This was about five years ago. . . .

'The black-throated diver I have seen several times, once on a moor loch, and last winter in Loch Fyne.

'I am distressed by the diminution in our cole-tits. They were numerous here till the severe winter of 1886, but they have never recovered it. The tree-creeper, also, is rarer than it was. The quail has been repeatedly shot in Kintyre, but not of late years.'

To Mr. Harvie Brown (*February 8th*, 1891).

'Two friends of mine have been shooting in my island of Tiree, and the account they give of the wild-fowl there is wonderful. On one fresh-water loch they counted one hundred and five wild swans, besides packs of fifteen and twenty on other lakelets. They killed three Gadwall ducks, but the geese were unapproachable. Snipes in enormous quantity. They killed six hundred and forty-one in seven days. Turnstones and stints were there, but only one greenshank. This bird is getting rarer.'

To Mr. Harvie Brown (*March 24th*, 1891).

'I was just about to write a line to you to tell you of the brambling still making an occasional appearance. I saw one yesterday—the latest in the year I have ever seen. But the severity of the weather both south and north of us keeps birds here which do not wish to face such conditions. The golden-eyes here show no signs of leaving us. . . .

'I saw a fine pair of buzzards yesterday wheeling round a ravine in which they used often to breed. I am desiring my keeper to let them alone.'

To Mr. Harvie Brown (*September 30th*, 1891).

'It may interest you to know that we are now honoured with the presence here of a large snowy owl. My keepers seem to have seen it for three or four days, but yesterday it flew across in front of the Duchess, who, with a party, all stopped to see the wonderful big white bird, which seemed to her as "big as a white heron."

'It is in my deer park, preying probably on rabbits, which abound this year. Late south-west gales of great violence have probably brought it from the Outer Hebrides.

'I have just heard that the stormy petrel was seen yesterday on the loch, the first I have ever heard of.'

Geology and Palæontology.

Geology was one of the Duke's favourite scientific studies. 'This,' he once wrote, 'is the particular branch of science with which I am best acquainted; none other affords such signal illustrations of that in which all science consists; its triumphs, its limitations, its temptations, and its weaknesses, have been all equally conspicuous.' His paper on the 'Leaf Beds in the Isle of Mull,' read before the Geological Society of London on January 8th, 1851, will, in the opinion of scientists, always be remembered by geologists as first establishing, by means of the leaves of tertiary plants found buried beneath basaltic rocks, the important fact that large volcanic outbursts have taken place in the British Isles so lately as in tertiary times.

From Professor Owen (January 26th, 1859), in reference to a lecture which the Duke had delivered at Glasgow.

'I everywhere hear your lecture spoken of as an extraordinary summary of the main points of actual geology in so limited a space.'

From Sir Charles Lyell (May, 1859).

'I have been reading your lecture on geology with much pleasure at spare moments during a tour of ten days in Holland. To have conveyed so much information accurately, without the use of technical language, is alone a great point gained.'

To Sir John Murray (February 10th, 1888).

'I think I told you of the working-man geologist who has turned up at Campbeltown. He has made an interesting discovery of organic remains in the old red limestone strata near Campbeltown. So far

as I know, this is the first time they have been detected in any old red strata in the West Highlands. I know the bed well. It is a well-marked one in the series between the Mull of Kintyre and the coal-basin to the north. It is a highly crystalline and even silicious limestone of a yellowish-white, much burnt for lime at several places. I have often looked for fossils, but its compact silicious fracture left little hope of it. But this man has found a weathered surface which reveals, by solution of the lime, the silicious casts of a richly fossiliferous rock. I am much pleased with this discovery, and I want you to tell me to whom I should send the specimen in Edinburgh as the best authority. I want to keep it as a Scottish discovery; but I don't know any expert in Scotland to be compared with Etheridge at the British Museum.

'I am sure that if the specimen were sliced the fossils would be seen, not only on the weathered surface, but all through the rock. I think I can now just detect and trace them in the fresh fractures, crystalline though they be. I have written to congratulate Gray, to encourage him, and have told him I would communicate it to the Royal Society of Edinburgh.'

To Sir John Murray (*September 26th*, 1888).

'I am much excited. I have just discovered organic remains in one of our quartzite beds here! Never was there such a proof that our eyes can only see what the mind prepares them to see. Here am I, having passed this rock by carriage on one side of it and by boat on another side of it, and I never saw what to-day I have seen at the first glance when I went prepared to see.

'On my late cruise I had been reading about the "fucoid" bed in the Sutherland quartzites. I had it in my head to look to see whether such obscure markings might not be detected in the very few beds of like material which exist here. Then, a road sur-

veyor had broken a fresh surface for "metal." I passed it to-day, sent my servant to bring a bit, and lo! I saw not only some obscure markings, but lots of very clear plant remains in red oxide of iron, "picked out" against the pure white silicious grains of the rock.

'I don't think I can be mistaken. The fossil consists of small ramifying tubes of oxide of iron and of seed-vessels on small stalks; the tubes, when broken, show a vascular and linear structure on the inside. The berries or capsules are unlike any plant I know—small oval bodies on stalks.'

These fossil forms were the subject of much discussion with several authorities. It was eventually decided that they were 'old annelid tubes sheared by rock movements.' Specimens of the fossils were afterwards placed in the Geological Museum in London. Another discovery was announced on January 10th, 1890:

'My dear Murray,

'I write a hasty line to tell you that to-day I have discovered a bed of schist full of well-preserved corals. They are very striking—pure white carbonate of lime, quite crystalline, flattened, but with the surface flutings not destroyed. The effect of the pure white plates interfoliated in blue mica schist is very beautiful.

'They are near the summit level of the pass over to Loch Awe, in a wee quarry opened for road metal, and the coach passes it every day, the wheels almost in the rubbish.

'It is the more curious as only yesterday I got from Geikie four specimens of Norwegian schist showing obscure casts of coral, and a letter advising me to look whether any holes in our rocks might not be corals. I saw white spots in driving past to-day to see the sunset on Cruachan, and on returning got out

and looked. I was astonished to see a whole slab of rock covered with interleaved, flattened, linear cakes of pure white marble. . . .

'What a mystery these rocks are! Some so metamorphosed, others so unaltered. This bed looks like squeezed mud, argillaceous, shiny, soapy, greeny. A little above it is a bed of limestone, blue, destitute of fossils, so far as I have seen yet. I now suspect it to be a triturated coral rock, metamorphosed by some agency, yet the next bed below in the series is what I have described.'

Geographical Discovery.

The great advance made in geographical research during the last fifty years of the nineteenth century opened up the African continent to European enterprise. First among the names of explorers in that region is that of David Livingstone, whose imperially-minded projects the Duke always advocated. From the heart of Africa, Livingstone wrote letters full of statesmanlike insight, describing the difficulties which he was so heroically surmounting, and urging the Duke to support his representations to the Government. Dr. Livingstone always showed grateful recognition of the support and encouragement he had received from Inveraray.

From Tette Dr. Livingstone wrote (March 5th, 1859):

'The renewal of the slave-trade, on the pretence of carrying out the French emigration scheme, has forced the conviction on my mind more strongly than ever that an English colony ought to be attempted in the interior of this country. You threw out this idea once when I had the honour of calling on you, and every day since then the scheme has grown in importance.'

As President of the Royal Scottish Geographical Society, the Duke had occasion to write officially to Mr. Henry Stanley on his return from Africa in April, 1890. In his letter the following passage occurs :

'We desire to congratulate you most heartily on your safe return from Central Africa. Personally, I have a special interest in your great success, for I cannot forget that you brought back to me the last letter I ever received from my dear friend David Livingstone, when, with equal courage and determination, you had reached and succoured him in the same regions.'

Referring to Sir John Murray's scheme for the annexation of Christmas Island to the British Crown, and for the exploitation of its rich deposits of phosphates, the Duke wrote (February 26th, 1888) :

'I am much amused by your proposed alliance between science, commerce, and the Foreign Office. I will do all I can to help you. If the island is in the Indian Ocean, I suppose it would be considered ours. At least, I doubt whether we should "see with indifference" (to use the regular phrase) any other Power take possession of an island in the Bay of Bengal or anywhere off the Indian coast. . . .

'I should write to Lord Salisbury at once, advising him to make one of our ships take formal possession. As to giving a "concession" of it, I don't know how these matters are settled under our system. But the national possession is the first thing to be looked to. . . .

'Lyon Playfair is the man who turns his science to commercial use most of all the men I know. He would swoop down on your islet like an osprey !'

To Sir John Murray (a few days later).

'I wrote a private note to Lord Salisbury, telling him he had better annex Christmas Island, and he has set on the Admiralty.'

Raised Beaches: Glaciation.

From the Duke's Natural History Diary, 1876.

'I have to-day examined more carefully some of the raised beaches on the west side of Jura, near the mouth of Loch Tarbert. I found that the level of the first or lowest, at the point where we anchored, was about 50 feet above the level of the present sea, as measured by an aneroid constructed for such measurements. This beach is so perfect that the back curve of the wave, or, rather, back slope, is as perfect as if the surf had left it yesterday. The next beach above it was about 75 feet; and a third, very well marked, from its forming a sort of embankment across a natural hollow, was, as nearly as I could make out, about 125 feet. After returning to the yacht, and when trawling on the Tarbert Bank, it seemed to me, judging by the eye (which, however, is very deceptive as to levels), that several of the raised beaches further to the north along the same line of hills were decidedly higher in elevation; and I therefore believe my recollection is correct that Captain Bedford, the surveying officer, reported some of them as reaching the height of 160 feet. They are indeed most striking objects, and one speculates whether the elevation of the land which they indicate was general or local. It is clear that they are the marks of a sea which is retiring and not advancing, because a rising sea would have obliterated at each successive stage of submergence the terraces of rolled pebbles which had been made previously. I think it clear, too, that the elevation was by hitches. The pebbles at the 50-foot level are much more completely rolled than at the 75-foot level, and, again, the highest one of all, which I visited to-day, was more completely rolled than the 75-foot.

'The quartz strata in the island of Islay are tilted to nearly the perpendicular, whereas in Jura they are

much less steeply inclined, and dip to the north-east. It is evident that the Sound of Islay marks a line of structural disturbance, whatever agencies may have taken advantage of this line to deepen and widen the disconnection. I think I can trace on the Islay shore two series of quartzites with mica slate and limestone intercalated between them, as in the typical Sutherland section.

'*July 22nd.*—On the 17th we visited the island of Eilean an Naoimh, which is now identified with the *Insula Hiruba* of the Columban Age, and found it very remarkable, both archæologically and geologically. The island and the islets all round consist of stratified rocks highly inclined to the north-west, presenting a precipitous face to the opposite coast of Mull, and a sloping face towards Scarba on the east and south-east. The beds on the eastern shore, which must be the upper ones of the whole, are of a conglomerate unlike any other conglomerate I have seen. The pebbles are much more thinly distributed in the embedding paste, and they are of quite different materials from those of the neighbouring conglomerates near Oban.

'Archæologically, the most interesting remain is a beehive house, of which I have never before seen any specimen. The whole structure is shaped like a beehive, formed of slaty stones, each course or layer projecting slightly beyond the lower one, and thus gradually approximating along the lines of a dome-shaped roof. The entrance appears to have been like a covered drain entering the basement. Those who entered it must have had to creep on hands and knees. The orifice of this entrance is complete, being built of well-fitted flagstones, but I did not take the measurements. It reminds me very much of the mode of entrance provided in the winter houses of the Eskimo. It is built on a ledge among outcropping strata of the natural rock, and when complete must have been almost invisible from any distance. At

present it attracts attention from the sea because one side has fallen down, and the rest of the structure presents the aspects of a cave with its mouth fronting to the east.

'There are several other buildings, all apparently of a much later date, for all of them present the usual gable ends of an ordinary cottage. One of them, indeed, has one end only constructed with rectangular walls and the other end rounded. This cottage is also curious in that one-half of its interior is built up with stones to the level of a high platform—as high as a man's breast. This half is the one terminating in a rounded wall, and looks very much like a dormitory raised from the ground for the sake of dryness.'

To Lord Kelvin, then Sir William Thomson (*June 12th*, 1883).

'Have you any physical explanation of the process—of any conceivable process—by which the land can have been let down to the extent of at least 2,000 feet during the great glacial submersion? I believe you have arrived at the conclusion that the crust of the earth has a very high degree of rigidity, comparable to that of some of the metals. Of course, material of this degree of rigidity will bend under adequate stress; but what can have been the stress under which the crust first sunk and then rose again, so as to account for the glacial submersion? I assume the fact. It is impressed upon me by innumerable facts which seem to me not otherwise to be accounted for. Yet geologists and physicists seem to me all to shirk it as a fact to be accounted for.'

To Sir John Murray (*August 8th*, 1890).

'Have you ever seen the parallel roads of Glenroy? If you have not, I wish you would go and examine them. I am secretly convinced that the

accepted explanation of glacier—dammed lakes—is a false theory. At least, I have a very strong impression that it is so, and that the terraces, after all, are due to marine action during a re-elevation by hitches. It is purely the difficulty of realizing this cause that forces men to invent ice-barriers which assume ice-sheets on a scale that probably never existed. The difficulty that used to intimidate me is that such marine action ought to have left similar marks elsewhere. But I see my way round this objection. There are other phenomena elsewhere. The grand raised beaches on the west side of Jura are "parallel roads" in more senses than one, and no maniac even will pretend that glaciers dammed up the sea there.

'Forbye, such terraces are only formed and kept under very peculiar conditions, and it is no wonder that these are uncommon.'

To Sir John Murray (*October 2nd*, 1890).

'Many thanks for the Glenroy series. I have read Darwin's paper with astonishment. It seems to me admirable and unanswerable, and yet he abandoned it all, under the influence of the ice mania, without any attempt to answer his own former reasoning.

'I am now thoroughly convinced that the Glenroy roads are old sea-lochs, when Scotland "sat low in the water" of the glacial sea, and strong tides raced through the cross channels which divided the whole country into a group of islands.

'The argument, "Why should the sea mark Glenroy alone?" is answered by the abundant evidence that there are similar lines elsewhere in many places; but all are now equally explained, not only by ice-barriers, but by ice-walls, both sides, or one side, being assumed to have been the sides of ice-sheets!

'Then, Greenland is always quoted, when Greenland ice does not do the work assigned to the mythical ice-sheet.'

To Sir John Murray (*April 25th*, 1893).

'How long one may live in a country and not see all that is to be seen, if the mind is awakened to some one point! I have just awoke to the curiosity and significance of the extraordinary difference between the two parallel glens here of the Aray and the Shira. The glen of the Aray is one mass of glacial mounds and of erratics of all shapes and sizes. Glenshira is quite free of glacial mounds, and has only a few scattered erratics, just enough to show that they are not covered up and concealed in that way.

'Why this difference? In what do they differ to account for it? Glenshira opens from the higher mountain land of the two. There is no mountain mass above, or at the top of Glenaray anything like the mountain mass of Benbuy and its outlying ridges. Therefore, if the Glacial Age was marked by great local glaciers, Glenshira must have been occupied by a more powerful mass of ice than Glenaray. In fact, Benbuy is the typical local mountain for sending off a local glacier. Yet the glen has no glacial mounds at all, and the erratics on its flanks and sides are comparatively few.

'What, then, is the difference between the two glens? One difference is patent. Glenaray is open at the top towards the north-north-west. Glenshira is shut up or closed in that direction by a high screen of steep mountain ridges.

'In short, it is a glen sheltered from a north-north-west drift of heavy floating floes, which, in my opinion, was the agency during the "great submergence" which did most of our polishing, scraping, and scratching.

'Be it observed, however, that the materials in Glenaray are all local. I have only found one stone—a boulder—which is of Cruachan granite, and the peaks of Cruachan look right down the glen, but from across the deep hollow of Loch Awe. If a glacier had come

direct from Cruachan to the low col at the top of Glenaray, of course it would have carried lots of Cruachan granite to us. But there are no indications of this ; one small boulder is all I have seen from that quarter. The other boulders are angular lumps of all kinds, and are all fragments of the local walls, which are high and steep.

'Glenshira is low in the floor, the local lake once reaching up four miles, now only one mile from filling up. So here, again, we see that a local glacier will not dig out or excavate even very soft material, unless, indeed, the lake deposits have all been post-glacial, which is impossible.'

To Sir John Murray (*April 3rd*, 1895).

'You seem to have done full justice to my paper, although a dissenter.

'I am an obstinate man ! Each new discussion only confirms me, because of the (to me) weakness of the objections. How can any man maintain that marine deposits most always contain marine organisms ? Is it not notorious that, as a fact, they do not ? We have lots of admitted "raised beaches" all round the coast, consisting of sands and gravels, and not a single shell. Diatoms I don't know about. They have never been exhaustively searched for. But even as regards them, would you really give up the marine origin of raised beaches because no diatoms exist in them ? I don't believe you would. Why, then, use this argument as of any real weight when applied to higher levels ? I should like to impose on you the labour of searching with a microscope for diatoms in unquestioned beach gravels and sands.

'Then, as to floe-ice not scratching because a few individuals have not seen it doing so : what evidence is this against that of Arctic navigators, who tell us how they saw floes of great thickness piled up against each other and lifted over reefs in Smith's Sound,

whilst all the shore ice is armed with stones, frozen into their lower surfaces ? What more effective grinding machine could be devised ?

'Again, how can perched blocks on the tops of ridges be accounted for by "free sheets"? Nordenskiold says the idea is ridiculous as applied to the Greenland ice-sheet.

'In short, I feel to stand "four-square to all the winds that blow," or, at least, that have hitherto blown.'

This theory of the general submergence of the land since the time of the first appearance of man upon the earth is further developed by the Duke in an article entitled 'The Glacial Theory,' in the *Nineteenth Century* of 1894, and in a communication to the Royal Society of Edinburgh in 1895, under the title of 'Two Glens and the Agency of Glaciation.'

Physics and Chemistry.

To Professor Tyndall (*December* 31*st*, 1875).

'Will you forgive me bothering you on a question of definition, with regard to heat and light ?

'The definition of purely scientific ideas constitutes a sort of borderland between physics and metaphysics, which is of extreme interest and importance.

'Sir W. Grove has objected to calling by the name of light any rays (or undulations) which do not produce the sensation of light on the human retina. This, however, is a purely verbal question, and it seems to me that if the undulations which do not produce the sensation are precisely of the same general character and quality as those which do, differing only in period, it is the most true representation of the facts to call them by one general name.

'This, therefore, is not the kind of difficulty I feel in admitting the alleged identity of heat and light.

Light, as I understand it, is an undulation in pure ether, and would be light to sensation if no other medium existed.

'But heat—that is to say, sensible heat, heat that is measurable by dilatation of substances, heat that does mechanical work—is not producible by the ethereal undulations until they come into contact with other substances, and until they set up in those substances corresponding tremors.

'But the undulations of the ether which excite or produce this motion in other substances can no more be called heat than the vibrations of a harp-string can be called sound where there is no atmosphere to convey sound.

'Now, is this a correct analogy? If light alone—the pure ethereal undulation, whether visible or not—were the sole factor in what we know as heat, then we could not have the most intense cold in interstellar space, as we know there is. I can understand saying that light is the cause, or an essential condition, of heat, but not that the two are identical.'

To Lord Kelvin (*February 9th*, 1882).

'My attention was drawn to the paragraph I enclose some months ago. Is there any truth in the statement that organic matter has been found in meteorites?

'I have been much puzzled about the strict accuracy of your recent statement about the sun as the ultimate source of all our terrestrial energy. Gravitation seems the ultimate source to which we can trace most forms of energy, possibly even those of heat and light. Of course, the sun is the body to which the earth gravitates, but, on the other hand, all terrestrial bodies gravitate to the earth's centre, and to this gravitating force almost all terrestrial energies are due.

'Even the energy of the solar heat lifting water in the form of vapour would be useless as a source of

energy were it not for the existence of terrestrial gravitation, which makes it fall again as rain. If we knew the physical cause of gravitation, we might identify a source of energy one step farther back; but until this cause is known it seems to me that gravitation is (to us) the ultimate source of energy in matter. Is this not true ?'

To this question Lord Kelvin replied: 'Yes, I believe it is.' He also answered in the affirmative the Duke's proposition regarding gravitation as the source to which most forms of energy, including heat and light, might be traced: 'Yes, and the sun owes his energy to mutual gravitation between portions of matter coming together to form his mass.' Referring to the energy of solar heat, Lord Kelvin noted: 'But mechanical work can be got from sun heat here without intervention of terrestrial gravity.'

To Professor Tyndall (*November* 17*th*, 1882).

'I found in London that you had been kind enough to send me a copy of your paper on the relations of vapours to radiant heat. I have read it with much interest, and I have just now also read your little paper in *Longman*, which touches on the same subject. I write now to thank you very much for your kindness in sending to me the first of these papers, and also to ask you a question which arises out of a paragraph in the second.

'It happens to touch a matter which it has fallen in my way to think about lately.

'I refer to the "atomic" theory of the constitution of matter. You say that this atomic theory is the direct outcome and result of the discovery of the law of multiple proportions in chemical combinations.

'Now, as a matter of fact, we know that the ancient philosophers had reached the conception of atoms as

the ultimate constituents of matter, although they knew nothing of the laws of chemical affinity.

'You must mean, then, that the atomic theory has been raised from a mere speculation to a consistent and strictly scientific conclusion by the discovery of the combining law of multiple proportions.

'The reason you give and vindicate is that nothing else but atomism would account for such breaches of the law of continuity as are involved in the numerical leaps made by chemical combinations.

'Now, this rather puzzles me. I think it clear that, granting or assuming the atoms, their existence does not in the least account for the multiple proportion in which they combine. Why ten rather than eleven atoms of any element should combine with certain others, is in no way accounted for by the mere fact that matter does consist of atoms.

'The converse proposition seems to me equally true: that, as atoms don't account for multiple proportions, so multiple proportions are conceivable without atoms. The idea that definiteness in quantity must involve definiteness in the number of indivisible atoms is an idea which does not carry conviction to my mind.

'Then, a doubt often occurs to me: Is the law of multiple proportions so certainly and definitely ascertained as to justify the theory?

'Of course, the facts on which the law of multiple proportions is founded are facts of measurement by weight and volume. But have we any instruments for measuring either, which could inform us if two or three half-atoms, or even dozens of whole atoms, less or more, were to be found in any given combination?

'I apprehend certainly not. The atoms are far too small to be detected in this way. Therefore, although you conclude that, when we roughly (very roughly) measure combined elements, we find that they jump from weight to weight by leaps of multiple proportion, it seems to me we never can be sure that the exactness of our measurements is sufficient to exclude the possi-

bility of fractional variations in such units as you are dealing with in atoms. Scientific heresy! I dare say you will exclaim.

'A vague belief in the perfection of numerical laws in Nature may carry us over this difficulty, and we may conclude that if, by all means of measurement known to us, definite numbers do prevail in combinations, then we may be certain that Nature is more perfect than our instruments, and the numerical relations which we can measure are really absolutely accurate along the whole way which we cannot measure.

'Well, this may be true; but it is an act of faith to believe in it absolutely.'

To Professor Tyndall (*April 8th*, 1887).

'I am very sorry to see the intimation of your retirement, and especially of its cause. I dare say the faculty and the opportunity of communicating knowledge *viva voce* must be a great pleasure; but rest is a great pleasure, too, after hard work, and I hope you may long enjoy it.

'There is always plenty to learn, even to the end, and we are in the full stream of discovery and of speculation just now. And yet, somehow, it never seems to come to much on the problems which are fundamental.

'I agree with you entirely in what you say about Ireland. I wish we could all retire to some high platform like Hindhead, and look down philosophically on the "madness of the people."'

To Professor Tyndall (*April 17th*, 1887).

'Reading your lecture has reminded me of a question which has often occurred to me. In one of your former lectures you speak of trying to form a distinct physical image of physical facts. There is no difficulty in doing this, as regards what is called a "wave" of sound, which, as I conceive it, is merely a series of con-

densations and corresponding rarefactions following each other like undulations. As regards any one given sound, this is an idea easily "imaged." The only difficulty as regards sound arises in my mind when we try to "image" a great many different sounds all running together at the same rate and at the same time. I can't "image" this. I suppose a distant analogy would be the case of small wavelets or ripples, running on the surface of large waves, which are quite common. But, then, they do not seem to go at the same rate, or to reach any given point at the same time. On the other hand, as regards sound, the notes of all the different instruments in a large orchestra reach the ear simultaneously, and also, to a fine ear, so separately as to be distinguishable.

'I cannot "image" this with any distinctness; can you? This, however, is not the question which I wished to ask you chiefly. There is another question, kindred, but distinct, which arises in respect to light. Of course, the same difficulty applies to this case as regards the multiplicity of motions which are conveyed simultaneously in the same medium, resulting, to my mind, in the same "unpicturability." But, besides this difficulty, which is the same in both cases, there is quite a separate difficulty to me connected with the conception of "vibrations transverse to the direction of the ray."

'This difficulty does not arise with sound. It is easily conceivable; indeed, it is the "natural" conception that vibrations or undulations should travel in the direction of the force which originates them.

'But, to my mind, there is a complete unpicturability in the alleged relation between the "ray" and the vibrations which are transverse. The only "image" I can form of that relation is the image of solid particles, of infinite number and smallness, shooting through the medium and causing radials or divergent vibrations in all directions round its own intrusive path. Now, this is an "image" which combines

the old " emission " theory of light with the undulatory theory. The " ray " would represent the " emitted " particle, whilst the undulations would represent the " row " (disturbance) set up all around its " path." This is the only " image " conceivable to me, and, of course, it is the image suggested in the language used. It is a physical image of what is called the " path," and it is quite easily conceivable that a particle of matter, however fine, pursuing a " path " through any medium with enormous velocity should, would, and must set up transverse vibrations.

' Of course, our power of " picturing " is dependent on the resources of sight, directly or indirectly. I don't know that any example is visible to us of any " thing " or any " motion " passing through a medium and setting up no vibrations except transverse to its own " path."

' If there is no such phenomenon visible, it would account for the unpicturability of the alleged action of light, which I assume to be fully proved.'

A note of January 29th, 1891, to Professor Tyndall contains the following remark :

' I wish Huxley would not write so offensively. I can understand the agnostic frame of mind perfectly, but I can't understand making it so aggressive. He writes as if every believer in Christianity were no better than the blackbeetle beneath his feet.'

About the year 1864 the Duke made the acquaintance of Professor Max Müller, whose attention had been attracted by an article on ' The Supernatural,' written by the Duke in the *Edinburgh Review*. A correspondence followed, which was continued throughout the period of a whole generation—from 1864 to 1898.

In a letter to the Professor, July 8th, 1864, the Duke criticised the naturalistic theory.

'Generally, if not always, I have observed that metaphysical writers speak of the supernatural as a *thing*, as if they knew exactly what the "natural" includes, and could, therefore, determine or define what lies beyond it. In this sense, the supernatural is not only unbelievable: it is inconceivable.'

To Professor Max Müller (*November* 11*th*, 1864).

'What is the ultimate derivation of the word "law" —*lex?* My attention has been drawn lately to the extreme vagueness of the senses in which this word "law" is used now in science. I wish to get at the *root idea*.'

Among many letters on technical points in philology, there are others of more general interest, from which the following extracts are taken:

To Professor Max Müller (*January* 25*th*, 1875).

'We are all deeply grieved by Kingsley's death. It is a great public misfortune, and an irreparable loss to all who knew him personally.

'I have been reading over again, with great interest, your lecture on Mr. Darwin's philosophy of language,* in consequence of having been engaged myself in writing on a kindred subject, and I find a great deal which I had not noticed before, so much does the mind bring with it in all reading. I have also been looking at your lectures on the science of religion, with reference also to the same subject, and I am tempted to put a question to you which I cannot clearly answer for myself after reading the latter lectures.

'You seem in some passages to imply that the earliest *historical* religion has been monotheistic, and

* *Contemporary Review*, November, 1874, p. 894; January, 1875, p. 305.

that polytheism has been a degradation of it; but in other passages you seem, on the other hand, to admit the theory of development, to the effect that monotheism has been a stage, though a very early stage, of religious growth.

'You reject altogether the idea of any primeval revelation.

'Now, there can be no question that, as far as history goes back, including the constructive history founded on the science of language, monotheism is the earliest belief we know of. I do not know what is the earliest date you would assign to *any* sacred writing, or whether any Vedic hymn is quite certainly much older than the Book of Job; but when you say that three thousand years before Agamemnon our Aryan forefathers worshipped a "Heavenly Father" (*Dyaus-pita*), you must refer to a time long antecedent to any existing writing (unless to some Egyptian hieroglyphic), and *that* worship was surely monotheistic in the highest and purest form.

'Of course, you may say, as Darwin says: "The very earliest historic man is a modern creature as compared with the really earliest progenitor of the race." But this is theory. It is not yet an ascertained fact, and so far as the direct evidence brought forward in your lectures is concerned, *monotheism is the earliest known worship of mankind.*

'What I want to know is whether this is your opinion or not. Do you know of any religious appellation earlier than that of the *Deus Pater* or *Dyaus-pita* of our earliest Aryan ancestors?'

To Professor Max Müller (*February 2nd*, 1875).

'I cannot see that any science has as yet discovered any *proof* that religion began, with some semi-brutal man, in the shape of a "suspicion of something beyond what he saw." This seems to me as purely theoretical (derived from *a priori* ideas) in respect to the origin

of religions, as Darwin's theory is as to the origin of the human body.

'In fact, I am quite unable to understand why you oppose Darwinism so much as applied to *language*, when your view as to *religion* seems to me essentially Darwinian. If man was born of brutes by insensible gradations, it is quite natural to suppose that he can have had no primitive intuition of the existence of his Maker. But if he was in *any sense* a special creation, or if he was born "with a leap" from some lower form, I cannot see why he may not have had such an intuition, which is a primitive revelation.

'It is curious that those who cannot swallow the Darwinian theory as applied to man's body have all different and separate difficulties connected with each man's special study, and so in like manner, your special study being language, you dwell on *it*, although to me this is but the symbol of other differences. But if man's religion has grown up from the lowest beginnings, why not also everything else? Why not his power of forming concepts, and his associated power of expressing them by sounds which become "roots"?

'I confess I am wholly puzzled to know what your view of human origin is. There are but three conceivable modes of origin: (1) That of special creation, as Genesis is at least supposed to teach; (2) birth, *but at a leap*, Nature making a *saltus* in this case; and (3) development through births on the Darwinian theory.

'The last is the only one which stands in natural connection with the theory that man's religion has been a growth from the lowest and obscurest beginnings; but you seem to hold this theory as regards religion, and to deny it as regards language. I cannot see any likelihood in this.

'I am, I confess, not able to dismiss as completely as you do all idea of the *substantial* truth of the Mosaic representation of creation. I am quite ready to believe that the language is highly "metaphorical,"

or "accommodative," or "poetical," or whatever other word you like to apply. But I mean that the idea of man being created, or made, or born, *at first* with a childlike knowledge and intuition of the Godhead as his Maker and Father in heaven is, in my opinion, a natural and probable correlative of his special creation *in any shape or form*; and that those who deny this primeval intuition give up their belief in the only thing which makes it difficult to assent at once to Darwinism *pur et simple*. I could never care to fight against that conclusion for the sake of "language," or "concepts," or anything else, if it be admitted as regards the most fundamental of all concepts—that of a Supreme Being.'

To Professor Max Müller (*February* 18*th*, 1880).

'The only parts of your Hibbert Lectures in which I disagree with you are those parts in which you have condescended too far to the materialists, and those parts in which you tacitly assume that the idea of *personality* in superhuman agencies is necessarily a growth out of vague conceptions of the "infinite" and the "invisible."

'I hold, on the contrary, that the idea of *personality* is the most natural, and therefore the earliest, of all; and that, consequently, the idea of a God may well have been strictly primeval.

'The truth is, that what you call the "infinite" is meant to include an infinite *Being*. It is better to say so at once. Infinite space, infinite time, infinite numbers of any given unit, will never beget the idea of a God.

'Our own personality is the nearest, homeliest of all conceptions, and the transfer of it to other agencies than our own is probably strictly primeval.

'Of course, if you assume that man was evolved from a beast, then a transition stage must be assumed. But of that stage we can form no conception.

'We must begin with man developed, as regards his faculties, to the rank of man; and if we begin there, it is safe to assume that the idea of a God would be one of the very earliest intuitions.

'You escape no difficulty by calling this idea "the infinite." On the contrary, you aggravate every difficulty, and do not one bit conciliate the materialists.

'Of course, I agree with you about all that is involved in "simple" sensation as it exists in man. To show *that* was the work of Kant. But the effect of that work is to show that we have intuitions of many categories, and I see no use in even seeming to give up the ground he won.

'I have been writing on the subject, and am very anxious to know the earliest evidences on the nature of sacrifice. The *food-offering* is probably the earliest notion, all flowing from *personality* as founded on our own experience of it.

'But, of course, the earliest Vedic literature may be very far from primeval, although I am inclined to think the symptoms are those of a true childhood.'

From Professor Max Müller (*April* 14*th*, 1888).

'On philosophical questions I should like to write to you more fully than I can at present. It requires an effort to see the inseparableness of language and thought. It has taken me a whole life to perceive it. People imagine that I hold that language and thought are identical. There is no sense in that. No two things can be identical. But they can be inseparable, neither can exist without the other—that is what I mean. We imagine that we can think without words because we can distinguish between the sound and the meaning. So we can between an orange and its skin, but *in rerum natura* there is no skin without an orange, nor an orange without a skin. You were one of the few men in England who I thought would see what I meant. But it requires an effort,

and it is only a historical study of language in all its phases that has at last led me to the conviction that the Greeks were right, and that what really makes us men and distinguishes us from the animal is the logos—*i.e.*, the gathering, or, as Hobbes said, addition and subtraction.

'. . . I am in no hurry, but I feel perfectly certain that what I have put before the world is true, and will be accepted in due time.

'I know little about Hartmann's philosophy, but I believe he has considerable influence in Germany, though not among the professors.'

To Professor Max Müller (*November 29th*, 1888).

'"I hae ye noo, Harry!"

'Can you translate that into some identity of thought?

'It is the phrase said to have been constantly used by a dull friend of Harry Erskine, a great wit, when he (the dull friend) at last took in the drift of one of Harry's jokes, and exclaimed, perhaps half an hour after, "Oo, I hae ye noo, Harry!"

'So I am your dull friend, and your last letter has made me exclaim as above, because it supplied to me one link towards an understanding which I had not seen before.

'I could not make out why you attached so much value in philosophy to the tenet of "identity." I did not feel sure as to the use to which it would be put—as to the edifice to be raised on this "foundation-stone." But now your letter has explained it, because you go on to ask whether it is not time to be more careful as to *definitions* of language; and you ask whether the vague use of certain words, such as "Nature," "Natural Selection," "Home Rule," etc., has not done mischief enough in science, politics, and philosophy.

'"Hear, hear!" I exclaim in Parliamentary emotion.

I entirely agree in the fallacies promoted by, and often consisting in, the lax and confused use of words; and if your theory helps you to expose this source of all human error, I am glad of it.

'Only, please let me say that, so far as I understand it, your theory would not help *me* one bit in this great and most needful work. On the contrary, the fallacies hid under language seem to me to point, not to the identity of thought and language, but to their essential separability. Why is "Home Rule" a fallacy? Why is "Natural Selection" another fallacy? Because thought is infinitely more subtle than speech; because language is infinitely too blunt for the purposes of really accurate thought.

'If you mean no more than that words exercise a great power over thought, by means of their ambiguities, then I agree cordially; and not only agree generally, but agree specifically in the prodigious importance of verbal analysis as one of the most powerful instruments in the detection of errors and the discovery of truth.

'So much so that I have been resolving in my mind for some time on an article, to be called "The Weapon of Analysis," on this very subject. It is one which has long struck me as of immense importance, and in all my pursuits in politics and in science I have long used this "weapon" with great satisfaction to my own mind, if not to others.

'But, again, I venture to say that this weapon, and the need of using it, does not imply any identity, but, on the contrary, a constant separability between thought and language, and a constant difficulty in the way of making them really or accurately coincident.'

To Professor Max Müller (*December* 19*th*, 1888).

'I write one line to say that the impression left on my mind by the reports [of your lectures] is one which

I do not believe you intend to produce—namely, that you regard religion as a product of the human mind, having no definite relation with any external or objective *facts*.

'"Geology" is the *logos* of *facts* in the history of the globe, and the *logos* consists in reducing these facts into an intelligible order, in so far as it can be done.

'In like manner theology is the *logos* of religious facts and spiritual existences, and we can only talk of the *origin* of theology in the same sense in which we talk of the first efforts of men to ascertain and correlate facts in any science.

'Such, at least, is my philosophy; but if we are to consider religion as a mere product of the human mind or imagination in any other sense, then, in my opinion, no such thing as religion exists as a *logos*.

'My impression of your teaching may be quite wrong; but a few words would set it right, if the reports are at all correct.'

To Professor Max Müller (*January 27th*, 1889).

'Having now delivered my soul on the point on which I differ from you, I am about to deliver my other soul upon the more substantial issues, as I think, on which I agree with you. I have been asked to deliver an address to the students of the University of Edinburgh at the opening of a union which they have formed among themselves for mutual improvement and recreation—lectures and addresses among other things.

'I intend to address them on "The Love of Truth, and on some methods of attaining it."

'*The* method on which I mean to dwell is the analysis of words and phrases, showing how the mere analysis of what certain phrases mean, the mere noting of the ideas expressed therein, is often enough to overthrow no end of fallacies and to establish important truths.

'Now, may I ask you a question on what you call "roots"? You say that all roots express *acts* and not *things*, by which I understand you to mean that all the oldest words you can trace are words which signify some self-conscious acting of the individual person, and not mere external objects. Taking the word *wealth*, I see that Skeat, in his "Dictionary," says that the root is the same as *well* and *will*, the root idea being "that which we will to have," or (in other words) that which we desire to possess. *Well* is, then, the result of having what we *will* or wish.

'But now comes the "suffix" or "affix," which converts the *act* of willing or wishing into the "thing wished for." In the case of this word the change is effected by two letters, *th*, as in many other English words—*e.g.*, *strong*, *strength*, etc. Can it be affirmed in such cases that there ever was a time when there was no word for the external object wished for, although there was a word for the act of wishing?

'Or, is the idea of a verbal "root" consistent with admitting that the "root" never existed without its offshoots and simpler derivations? It seems to me that the abstract concept of "things wished for" is now, and must always have been, inseparable from the concept and consciousness of "willing to get the things wished for." But this is not, I think, inconsistent with the idea that the primary element or "root" is in the conscious act of willing. I take this word as an illustration, as it expresses primeval desires, actions, and, probably, words. I only want to be sure exactly of what you mean.

'That thought and language are identical, in the sense of words being the vesture, the embodiment, the record, and the history of thought, I see more and more; and I want to point out how the "weapon of analysis," applied to words and phrases, is a weapon as powerful in the discovery of intellectual truth as chemical analysis is in revealing the elementary constitution of matter.'

To Professor Max Müller (*March 3rd*, 1889).

'My conclusion is that we differ too little to make it worth while to carry on the controversy. You say that a true concept cannot be clear and definite until it has been first named. I say, on the contrary, that such a concept cannot be named until after it has been first mentally conceived. This seems a direct antithesis; and yet the practical conclusion we aim at is the same: that phrases are becoming increasingly deceptive, and that the analysis of words would clear up the thoughts of all of us immensely.

'You and I both agreed in this; and I think this practical conclusion looks more in the direction of, stands in closer relation to, my abstract proposition than to yours.

'If words are such pitfalls, if they are so deceptive, if they need to be analyzed and purified, and kept up to the mark of accurate thought, how can it be maintained that they are necessarily identical with thought? How can it be denied that, so far from being identical, they are very apt to become separate, even antagonistic, and full of deceptive power?

'Meanwhile, my address to the students of the college in Edinburgh was enthusiastically received by them, though I saw it puzzled them. But it was not reported at all, or only in the barest abstract; so the chairman, Professor Campbell Fraser, of the Chair of Logic and Metaphysics, has asked me to reproduce and publish it; and as I spoke it and did not write it, this odious work of writing is now occupying me in all my spare time. I shall, of course, send you a copy when it is published.

'I find some very interesting *dicta* by Berkeley in his "Commonplace Book" on language. In one place he says that language is so pestilent a source of fallacy that if men could dispense with language altogether they would never mistake! In another place he speaks of language as indispensable for

thought. It would be easy to quote him on either side of our logomachy. But the drift of his analysis always is to show how deceptive words are, and he was started on his course of thought by the word "reality." What does it mean? What constitutes a real thing? My own result is that language is the coinage of thought, stamped with its "image and superscription," coined automatically, unconsciously, and absolutely needed as the "medium of exchange." But that it comes second, and not first.'

To Professor Max Müller (*April* 14*th*, 1892).

'Inveraray.

'I have just completed a rather careful reading of your Glasgow lectures, and am glad to recognise in them what seems to me a very substantial contribution to the great subject of natural theology. What I note as of primary importance is, first, that your view is definitely and distinctly that religion is not an *invention*, but a *discovery*; that it is not the development of an *imagination*, but the development of a *recognition*, so that the subject-matter of religion is *fact*. Secondly, I note your language about a *personal* God as essential, as much so as a *personal* soul; thirdly, that abstractions must be the abstracts of facts; and, lastly, that *philosophies* are not religions.

'I note these as leading thoughts of immense importance.

'But now, since you challenge and write criticisms either as to historical faith or as to logical conclusions, I wish to say frankly that, in my opinion, your treatment of what you call "physical miracles" is not logical.

'If there be a Supreme Being, we cannot logically confine His methods of operation so as to exclude what is usually called the "supernatural"—a word which I dislike, and which you rarely use, but which you do use a little. I entirely dissent from your

doctrine that it is no longer now a question of evidence, but that miracles are proved impossibilities.

'My own chapter on the supernatural in the "Reign of Law" still expresses my own view; and even Huxley admits that there is no *a priori* disproof possible.

'Of course, immense consequences follow from your rejection of all the "supernatural" elements in Christianity. In my opinion this does reduce it from a religion to a mere philosophy.

'Of course, I can't go into this subject now — it is an immense one; but I could not write on the subject at all without expressing my entire dissent from all this on logical grounds.'

To Professor Max Müller (*April 24th*, 1893).

'I have completed my first reading of your last book, and have been extremely interested, thinking that it adds some valuable reconstructive elements to natural religion. There are many parts of the book with which I find myself in special agreement, such as the passages about *types* in organic structure, on which subject I had written some passages only last week which are almost a transcript of your own.

'The main point on which I do not find myself in agreement is neither any fact to be disputed nor any deduction to be rejected. You challenge replies on either ground. But my feeling of dissent, or at least of great doubt, has reference to an ill-defined, but *felt*, atmosphere of an assumption or preconception which pervades many passages.

'You dissociate abstract conceptions from all objective facts more than I can understand, and more than I can admit to be truly philosophic.

'Thus, consider the general and abstract idea of Christ being the Incarnate Word. You dwell on this at great length as of the essence of Christianity, which, no doubt, it is. Yet your language leaves it doubtful

whether on this account you think it at all needful to accept as historically true any one fact of His recorded life. "Galilæan legends" is a phrase you use, without specifying which or what you mean; and your language gives the impression that, in your mind, you can divorce altogether the satisfaction you feel in the abstract idea, from belief in, say, the fact of the Resurrection.

'Now, this is a frame of mind I cannot even understand. Abstract ideas are poor stuff unless they are abstracted from real objective facts. In like manner, constantly you use the words "mythological forms" for all attempts to personify, or to embody, abstract conceptions. Of course, many such personifications are pure myth; but not *all* are myth, and my philosophy teaches me that all abstract ideas have some embodiment in objective facts. They are only "mythological" when those embodiments are fanciful and unreal. But you seem to treat all kinds and forms of embodiment as equally mythology.

'I am pleased and amused by one passage, in which your words imply that thought comes before words, and seeks in a vocable its own expression. This seems to me the order of Nature; and I know as a fact that I frequently can recall an idea, and even handle it in reasoning, while, nevertheless, its name has vanished from my memory.

'On the great leading idea of the book—the mischief of supposing that Christianity is to be defended by pretending that all its ideas are novelties in the world, and in the use you make of St. Paul in this connection—I heartily agree with you.

'Your love of the mystics amuses and interests me. It is correlated with the love of abstract conceptions which is common to all students of philosophy, but it has inspired some of the most striking passages of your book.'

Another subject which the Duke discussed with Professor Max Müller was the question of the antiquity

of man, considered in the light of the evidence gathered from the history of language.

To Professor Max Müller (*February 20th*, 1868).

'You say that it is certain that there was a time when the ancestors of the Greeks, Latins, Germans, Indians, etc., dwelt under a common roof, because they have all the same words to express the primary relationships of life and the most ancient of the domestic animals.

'But the question arises, Have we any measure of time to enable us to guess when this "common roof" was the home of the ancestors of all those races? Have we any link connecting time-relative with time-absolute?

'One step towards this would be to estimate how far back, *in years*, the diverse dialects of the Indo-Germanic language can be traced in well-marked separation from each other. The evidence of monuments, and of something like history, seems to go back as far as the twenty-eighth century B.C., on the most moderate computations of Egyptologists. But beyond this, all trace of time, measurable by years, seems to be lost. What is the farthest-back date to which you think we can reach by the evidence of language?

'The rate of growth of dialects in early stages of the world, when there were few arresting causes, must be as much matter for conjecture as the rate of growth in geological formations. But it may be possible to fix a *minimum*, if we know such a date as I have referred to.'

Professor Max Müller replied that 'every attempt at translating the periods of natural growth or structure into the language of definite solar chronology has proved a failure'; and the Duke, continuing the subject, wrote on February 25th, 1868:

'About 1200 B.C. we have the Sanskrit language, in the Veda, perfectly formed and reduced to writing. We also have evidence that the Greek language was already in a similar condition about the same period. Now, both these languages bear traces of having come from a common stock, different from either, and the oldest forms of Greek and Sanskrit show an increasing approximation.

'But the very fact of a language becoming a *written* one indicates a stage at which an arrest would be put upon the causes of change. Therefore, it is impossible to measure the rate of changes *before* a language became written by the rate of change *after* it became written. You raise a question of immense interest when you say that the history of language is only the history of decay. Do you mean to say that language is higher and more perfect as we go back in time? And, if so, in what sense was it higher? Or do you only mean that, though language becomes always more perfect in its adaptation to thought, its materials are the *detritus* of older and ruder forms of speech? "Decay" in this sense does not mean degeneracy, but only crumbling. The oldest forms may be the rudest, and the youngest may be the most perfect, although these last are made out of the "decay" of the first.

'What I always feel about time, measured geologically, is, that if the causes of change were more rapid in pre-Adamite time than now, all measure of time-absolute is lost. So, likewise, if, before language was reduced to writing, the causes of change were much more rapid in their operation, no man can say how great those changes may have been when men were nomads, diverging rapidly from each other in place and habits. No man can say what changes and developments of speech may have arisen in 3,000 years, under such conditions of the race.

'I hope you will give a lecture on this great subject of time-relative in its relations with time-absolute.'

In all his writings on scientific subjects, and in any part he took in controversies regarding disputed points, the Duke's chief aim invariably was to maintain that the most fearless investigation of every new discovery, brought to light in the progress of knowledge, should be welcomed. He held that no proved truth could touch the belief in the Divine Source of all truth, but that each one formed a new link in a chain reaching back to the great First Cause, without the recognition of Whose guiding Hand no theory could be constructed to explain the earliest dawn of life. In the early and mid-Victorian days, a wave of infidelity appeared to follow in the wake of scientific discovery. The new light thrown upon the forces in Nature had revealed a new earth, and with the old earth there had passed away, for many, the old heaven. To those who found that doubt was 'as lead upon the feet of their most anxious will'* the firm stand made for the faith by a man like the Duke, who had kept abreast of all intellectual progress, and in whose great abilities and powers of judgment men placed confidence, formed a rallying-point when they had lost the old landmarks, and were in danger of missing the path in the darkness of infidelity. Many letters addressed to him testify to the help he had afforded to others, by his counsel and by his writings. A number are from clergymen of the Church of England, who were, by their office, specially called upon to deal with the spiritual difficulties of the age, and who gladly availed themselves of the weapons provided by his reasoned arguments to resist attack. The ex-president of a secular society, who had been led to repudiate atheistical doctrines, wrote to the Duke stating that his long experience in

* George Eliot.

connection with this society had shown him how great an influence for good the Duke exercised over the working population, amongst whom a low-class infidelity had been spreading.

A letter from the Duke to Lord Bramwell on July 31st, 1863, refers to a theological discussion:

'You are a judge, and you are accustomed when on the bench to throw your mind into the judicial attitude, both as to facts and principles.

'I hope you will endeavour to deal with yourself in the same way when you are brought face to face with the problems of what you call theology.

'You need not quote to me a passage from Sir W. Maxwell, in which he refers (probably) to the doings of the Inquisition in Spain or in the Low Countries.

'There is an older author than Keir, who has put this matter into terser words—Lucretius: "Tantum religio potuit suadere malorum."

'The conclusion from this great fact seems, in your mind, to be this: "Religions or religious dogmas are the source of all evil."

'If you looked into the question judicially your conclusion would be very different. It would probably be something like this:

'"Men's conduct has in all ages been determined fundamentally by their beliefs. It has been bad in proportion as these beliefs have been false; it has been good in proportion as these beliefs have been true."

'Consequently, the line of Lucretius and the sentiment of Keir are equally true when made to face the other way: "Tantum religio potuit suadere bonorum."

'Just as false religion and false dogma have been the source of tremendous evils, so have true religion and true dogma been the source of all that is best and highest in human conduct and in human institutions.

'This is as much a *fact* as the converse proposition.

'It is the idlest of all occupations to rail against beliefs. They will exist, and they will exert their power. Even the purely negative belief that there is no true religion, and no knowledge respecting it, is a belief which will have its own tremendous power.

'I submit, therefore, that the duty of all men is, not to despise questions of belief, but to study them, and, as far as may be, to solve them.

'As a matter of fact, the fundamental institutions of our law are, in all their moral aspects, more or less directly moulded on Christian belief, and I have never yet seen any other foundation even suggested which has the same strength or the same truth.'

The following words, written by the Duke, allude to the recognition he received of the help afforded to others by his literary work:

'I have had letters from the most distant parts of the world—from the backwoods of America and the bush of Australia, from men whom I have never seen, nor can see, in this world, thanking me for having lifted from off their spirits that deadly nightmare of a rigid, fateful, and mechanical necessity seated on the throne of Nature.'