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690TH ORDINARY GENERAL MEETING,

HELD IN COMMITTEE ROOM B, THE CENTRAL HALL, WESTMINSTER, S.W.1, ON MONDAY, MAY 31ST, 1926, AT 4.30 P.M.

ALFRED W. OKE, ESQ., B.A., LL.M., F.G.S., IN THE CHAIR.

The Minutes of the previous Meeting were read, confirmed, and signed.

The CHAIRMAN then introduced Major Lewis M. Davies, R.A., F.G.S., to read his paper on "Evolution."

EVOLUTION.

By MAJOR L. M. DAVIES, R.A., F.G.S.

WE have among us here, in London, the tomb of an "Unknown Warrior." That warrior is, like all the rest of us, regarded as being a product of evolution. In other words, he is supposed to have been connected by a vast and unbroken chain of genetic connections with some of the lowest—and possibly the first—forms of life that ever appeared upon this planet; forms of life which, more primitive than any known to science to-day, were themselves derived by some spontaneous process from inanimate materials.

That is how I would briefly define the doctrine of evolution,* which I propose to discuss this evening; so I wish to ask all present to note the primary fact about this doctrine that it is,

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* Commonly called "Organic" evolution.

essentially, a doctrine of continuous and unbroken *genetic connections*. Nor is this doctrine limited to the question of our own origin. Far from it. We are told, on the contrary, that every living creature round us to-day—whether animal or vegetable—has similarly arisen from primeval protoplasm. People may indeed quarrel as to the method, or causes, of evolution, but they practically all agree as to its fact; all forms of life, from the eagle to the whale, the oak tree, the humming-bird, the elephant, the bee, the daisy, and the ostrich, etc., are regarded as the tips of the twigs of a tree, which, however far separated they may seem to be, can yet be traced back, through various groups of branches, to one common trunk from which all have sprung. Even so, we are told, can science trace back all terrestrial forms of life, through interminable ancestries, to one common origin.

Now if this doctrine of genetic connections and a common origin were put before us in the name of Philosophy, few objections could be opposed to it. It has all the merits of a finished creed. As a concept of nature it is unified, simple, and most comprehensive. It co-ordinates any number of facts in a most attractive manner, and strongly appeals—as we are repeatedly assured—to the "modern mind." Now that is a very strong argument in its favour as a philosophy; for every philosophy finds its basis, as Le Roy assures us, in a "frame of mind."* Our own generation, then, having acquired a frame of mind very different from that of our ancestors of a century ago—in that we are attracted by uniformitarian ideas as strongly as they were appealed to by catastrophic ones—evolution as a creed is undoubtedly better suited to ourselves than it was to them. Consequently it "does not pay," as they say, to oppose evolution nowadays. Those who write against it, however ably, never find their works received with the same favour as those which are written in its support. Thus probably everyone has heard of Mr. H. G. Wells' writings on the subject, although Mr. Wells is no research worker, has no first-hand knowledge of the subject, and so has none but borrowed ideas to pass on to others; but how many have heard of the works of a leading zoologist like Fleischmann, a scientist of European reputation, who flatly denied that evolution could be regarded as scientifically established? It is significant that no one ever attempted the task of directly opposing Fleischmann; but he was thoroughly

* *A New Philosophy: Henri Bergson*, p. 12.

abused instead, and soon forgotten. When men of science find that the open expression of serious doubts upon the subject are treated after this fashion, it is natural that they incline to keep them to themselves. Nobody likes to be boycotted and reviled. Although, therefore, the great majority of scientific workers certainly do accept belief in evolution, we have no reason to suppose that they all do so, even if we seldom hear of their openly opposing it. There are reasons for their keeping their doubts to themselves. When Canon Barnes,* for instance, was loudly proclaiming that no man of science had openly come forward to oppose his declaration that man was certainly descended from an ape, few seem to have noticed that a sarcastic letter from a man of science did appear in *The Times*, remarking that the worthy clergyman seemed to be very sure of himself, and inviting him to name the actual species of ape from which man was descended. That was probably about as far as a professional scientist could safely go, in opposing evolution, if he did not wish to call down a storm of abuse upon his head from qualified and unqualified critics alike; yet the hostility of the writer was none the less patent for all that, and his question was—scientifically—a most pertinent one. It was a question which could not be answered, and Canon Barnes never attempted to answer it.

Why, then, is evolution regarded as science and not philosophy?† All the real arguments in its favour are essentially philosophic and not scientific ones. When we are told—as we are at once, whenever we begin to question the scientific foundations of evolutionary belief—that evolution is a more "unifying concept" than creation, that it suits the "modern mind," that it appeals to known rather than to unknown causes for its

* Now Bishop of Birmingham.

† I should, perhaps, define my use of the terms "science" and "philosophy." I define a "philosophy," therefore, as being "a method, of explaining and co-ordinating facts, which suits a certain type of mind"; while "science" refers to "knowledge derived from the objective examination and verification of facts, and the study of their *necessary* implications."

(I see that *Cassell's Dictionary*—the only one by me at the moment—defines a philosophy as "a particular system upon which natural effects are explained"; and science as "truth or knowledge ascertained by observation, experiment, or induction." There is not much apparent difference between these definitions and my own, so I hope I am using terms in a fairly normal way.)

effects, etc.—all these are purely philosophical considerations. They simply amount to this: that we have laid down certain conditions to which a philosophy of nature must conform in order to please ourselves,* and evolution alone conforms to those conditions.

There is no doubt that a "unified concept" of nature is more pleasing to our minds than any other; but the actual truth of such a concept remains to be established quite apart from any question of our tastes. To say that evolution suits the "modern mind," which will no longer tolerate suggestions of creative interventions, merely amounts to saying that we intend to believe what we like. And the appeal to "known causes" is also peculiarly ineffective, however attractive it may be to some minds; for, as the late Duke of Argyll pointed out, the great objection to all modern theories of transformism is simply this, that they "ascribe to known causes unknown effects."†

Natural Selection, for instance, is a "known cause"; but in trying to argue that it might have produced such a structure as the eye, Mr. Darwin ascribed to that known cause a wholly "unknown effect." Although he wrote with all the dignity and polish of his superior education, Mr. Darwin often contributed no more to actual science than Uncle Remus did when he suggested that guinea-fowl were spotted because a cow once dipped her tail into some milk and splashed it over their ancestors. For the splashing of milk is also a "known cause"; although Uncle Remus ascribed an "unknown effect" to that cause when he suggested that the splashing might leave permanent marks on the birds, which would be inherited by their offspring. In other words, the determination to appeal to known causes does not necessarily lift the philosopher into the ranks of scientist, but has a dangerous tendency to reduce him to the level of the fable-monger. There was often no fundamental difference in principle between Mr. Darwin's reasonings and those of Uncle Remus.

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* J. S. Mill tells us that it is the aim of the Physical Philosopher to determine "what are the fewest and simplest assumptions, which being granted, the whole existing order of nature would result" (*Logic*, 3rd ed., vol. i., p. 327). That is all very well for the philosopher, and suits our bias for unification; but the scientist has to deal in facts, not assumptions, and see what he can actually prove.

† *Primeval Man*, p. 44.

Nothing has struck me more forcibly than this, that belief in evolution generally appears to be quite independent of scientific evidence. Whether he knows it or not, the average evolutionist is—so far as his belief in evolution goes—not a scientist but a philosopher. Whenever he is driven off the particular facts to which he appeals, he invariably falls back upon philosophic considerations. The wholly natural and unconscious way, too, in which he does this, shows where the foundations of his belief really lie.

It is worth remembering, therefore, that the more cautious evolutionists have often shown their own appreciation of the fact that belief in evolution is, after all, a philosophic rather than a scientific matter. Thus Dennert, when reviewing Fleischmann's works, frankly admits that his own continued belief in evolution "involves a creed," and so must be regarded as a philosophy.* A first-rate modern palæontologist like C. Depéret renders a warm tribute to certain of the older palæontologists, who believed in separate creations to their dying day: in criticizing the beliefs of these men, Depéret does not attempt to show that the same were opposed to the facts, but merely questions their merits as a "philosophy."† In Depéret's view, then, evolution is not so much science as a superior philosophy to that of creation. Messrs. Thomson and Geddes, in their attractive little work on "Evolution," trace the views of Darwin and Lamarck back to their origin not in scientific facts but in the popular social doctrines of their day, which they *read into* the facts. Thus, the writings of each of these leaders in evolutionary thought were, according to Messrs. Thomson and Geddes, "The philosophic epic of a great nation at its epoch."‡ Now that is all very fine, but it says very little for evolution as a matter of demonstrative fact. We do not believe that the world is round because Galileo wrote a philosophic epic about it, based upon contemporary social doctrines, but because he proved it by unquestionable facts of permanent value.

But, some will say, is not evolution also proved by the direct evidence of fossil successions, and by the indirect evidence of

* *Am Sterbelager des Darwinismus*, Eng. ed., pp. 131, 142, etc.

† *Transformations of the Animal World*, pp. 121–2.

‡ *Evolution*, p. xii; cf. p. 215. Similarly, Depéret comments (p. 60) on the "bursts of social philosophy which mark nearly every page" of Haeckel's *History of Creation*. There is a closer connection between social philosophies and evolutionary beliefs than most people realize.

many converging lines of testimony drawn from the facts of embryology, comparative anatomy, and geographical distribution ?

Well, I would reply, I do not think so ; and the writers I have quoted apparently do not think so either, or they would hardly refer to evolution as a philosophy when all is said and done. The fact is that there is no real " convergence " of evidence in favour of evolution. A number of different supposed " lines of evidence " are indeed commonly quoted as if they bore undoubted testimony to descent, but one need never go beyond the writings of evolutionists themselves in order to reject every one of them in turn ; for every single one has been emphatically repudiated, as evidence for descent, by leading evolutionists themselves. It would be easy to multiply quotations to this effect if space permitted me to do so ; but it does not. It must be sufficient to point out that while the testimony of " Rudiments " has been appealed to with the utmost confidence by scientists like Haeckel, and ex-priests like Mr. Joseph McCabe, it has always been regarded as far too uncertain to be trusted by more able thinkers like Huxley and P. C. Mitchell. The supposed evidence of embryology was also regarded askance by Huxley, and has been rejected altogether by experts in embryology like Sedgwick and Ballantyne, T. H. Morgan who called it " *in principle false,*" and Carl Vogt who denounced it as " absolutely and radically false." And so we might go on through the list. What I particularly wish to point out here, however, is the fact that Huxley himself rejected, either as double-edged or as inconclusive, all lines of evidence save that of palæontology. Any other kind of evidence, said he, " remains mere secondary evidence. It may remove dissent, but it does not compel assent. Primary and direct evidence in favour of evolution can be furnished only by palæontology."*

* Address to celebrate " The Coming of Age of the *Origin of Species,*" 1880. The same thing has been said by others ; especially (it is important to note) by men whose studies lie more particularly among living structures, and who thus only turn to palæontology because definite proofs of evolution cannot be obtained elsewhere. Thus, Prof. G. H. Parker of Harvard University, writing on " Zoological Progress " in *The American Naturalist* for Feb. 1908, says : " It is plain that the history of the animal kingdom is to be sought for not through ingenious speculations on the recent group of animals, but by persistent and patient exploration of the fossil-bearing rocks " (p. 121). Similarly, as Dennert points out, the embryologist Hertwig " makes not the least mention " of the evidence of embryology, but " evidently regards as the sole really empirically and inductively serviceable proof of Descent, that which is drawn from palæontology " (*op. cit.*, p. 140).

This, then, narrows down the issue; and here I would join issue, for the purposes of a short paper.

So I would ask: Is it the case, as Huxley supposed, that palæontology can give decisive evidence in favour of evolution? Let us remember, what we remarked before, that the doctrine of evolution, or descent, is essentially one of uninterrupted *genetic connections*. Is it the case that palæontology can establish the *fact* of such connections? It is a question, you see, of the quality of the evidence. Is such evidence, in the nature of things, capable of establishing what it is called on to establish?

Huxley evidently thought that it was, but he never explained why he thought so; indeed one might quote his own remarks elsewhere, in order to show the limitations of that very sort of evidence. A fossil, said he, which takes an intermediate place between other forms, simply affords presumptive evidence in favour of evolution, since it indicates a possible route along which evolution may have travelled. Presumptive evidence, then, was the most that Huxley himself, in his more critical moments, felt that he could secure by finding intermediate forms in palæontology. He could not prove the fact of descent, but only point to a possible route for descent.

This is worth noting, for it leads us to ask: How is the fact of descent to be established?

Let us consider for a moment how it is established in actual practice. Let us return to the "Unknown Warrior," to whom we referred at the beginning of this paper. Why is he "Unknown"? I presume that his "identity disc" was lost. This so-called "disc" is a small plate of metal or other suitable material on which is stamped the regimental number, name, and unit of the soldier; and it is carried on his person into action, in order to facilitate his identification should the subsequent operations leave his body as one of countless dead upon the field. This small piece of actual *historic* testimony is thus regarded as likely to afford, through the regimental records, better evidence as to the dead man's relationships than could be obtained by any other means.

Note this. Genetic connections are, in practice, decided by *historic* evidence, and that alone. It is a significant fact that no one ever dreams of appealing to even the most august assemblage of palæontologists to decide who a dead man's father was. We sometimes find corpses exposed in a mortuary for recognition;

but we never find them being sent to the South Kensington Museum for that purpose. Historic evidence by the humblest person who actually knew the deceased in life, is worth more than all the help that the greatest palæontologist in the world could give in deciding the parentage of a dead man. In other words although descent is, essentially, a doctrine of unlimited genetic connections, there is no method known to science whereby even one single step in descent can be established apart from historic testimony.

Nor would anyone, I believe, admit this fact more frankly than the present Keeper of Geology at South Kensington. Firmly as he himself believes in evolution, Dr. Bather clearly stated his opinion, before the British Association in 1920, that palæontological succession is, in itself, no proof of descent. "The palæontologist" he pointed out, unlike the zoologist, "cannot assist at even a single birth."* In other words, no one ever actually saw one fossil "ancestor" being born of another. The element of historic testimony is thus wholly wanting in palæontology, and the clearer thinkers realize that there is nothing adequate to take its place.

Dr. Bather himself is inclined to supplement the evidence of succession by considerations drawn from the ontogeny of the forms; but this at once raises the whole question of the soundness of appealing from ontogeny to phylogeny. Many expert embryologists, as we have seen, have utterly rejected the theory of recapitulation; and it is interesting to find such an authority on embryology as Hertwig turning altogether from speculations based upon ontogeny to the facts of palæontological succession, while a most eminent palæontologist like Dr. Bather deliberately rejects succession as such, and makes his own essential appeal to ontogeny.

Nor is this abnormal. The most diverse ideas meet us, when we compare the efforts of various evolutionists to justify their creed. In palæontology itself, the existence of the most widely conflicting views is notorious. "What one writer," says Dr. Scott, "postulates as almost axiomatic, another will reject as impossible and absurd."† The whole trouble is, in my opinion,

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* *Advancement of Science*, 1920; "Geology," p. 7.

† Article on "The Palæontological Record," in *Darwin and Modern Science*, p. 189.

that people have accepted as science a belief which lies outside the limits of scientifically demonstrable fact; and so they cannot agree in their attempts to set it upon a scientifically satisfactory basis. The flaws which escape the notice of one man are only too patent to another. Messrs. Darwin and St. George Mivart were both firmly convinced of the fact of descent; but while Darwin regarded the existence of structural homologies as "utterly inexplicable," except upon a basis of common descent,* Mr. Mivart showed that an appeal to homologists, if consistently applied, must end in a *reductio ad absurdum*.† Modern evolutionists, therefore, have much to say about "convergence" and "homeoplasy," etc., in order to explain away those cases of structural homologies which cannot possibly be due to common descent. One might well ask, though, whether such facile jugglery is really admissible in science. One is tempted to sympathise with the blunt language of Carl Vogt—himself an enthusiastic evolutionist—who denounced the whole appeal to homologies as a "dogma." One thing is certain, he tells us: "the dogma, 'like formation, like descent,' on which all our phylogenetic studies rest, cannot pretend to universal validity. The *Onchidium* with the eyes of a vertebrate is no offspring of a vertebrate, nor the vertebrate of an *Onchidium*."‡ It is a singular fact that even Haeckel, that arch constructor of fossil "pedigrees," both knew and admitted, in his less elated moments, that there could be no actual proof that animals were genetically connected along the lines proposed by himself. Neither fossil successions themselves, nor any considerations drawn from the facts of their ontogeny, their vestigial structures, and whatnot, could ever amount to an actual proof of their genetic connections. "All ideas we can possibly form," said he, "about the stem history of any organism, even after the most critical investigation, are and must remain hypotheses" (*The Story of Our Ancestors*, p. 6). "It is self-evident" he elsewhere remarks, "that our genealogical history is and ever will be a fabric of hypotheses" (*Systematic Phylogeny*, vol. I, Preface, p. vi).

It is worth remembering this. On the testimony of Haeckel

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* *Descent of Man*, 2nd ed., reprint, 1906, p. 35.

† *Lessons from Nature*, p. 176.

‡ *Die Natur*, March, 1889. Similarly Prof. Otto declares that: "Homology of organs is no proof of their hereditary affiliation" (*Naturalism and Religion*, p. 123).

himself, evolution is and must ever remain a "fabric of hypotheses."

What then, I may be asked, is my own attitude to the subject as a working geologist? I reply that I hold to the Baconian definition of a true scientist.

"If any human being desire," said Bacon, "to attain to clear and demonstrative knowledge instead of attractive and probable theory, we invite him as a true son of science to join our ranks" (*Novum Organum*).

Now the best that an honest man who knows the facts can say about evolution is, that it is "an attractive and probable theory." But that is precisely what Bacon refused to regard as science. It was only the man who turned his attention from just such things as that, to matters of demonstrative fact, whom Bacon was prepared to recognize as a true son of science.

Accepting the same ideal, therefore, I decline to discuss questions of ætiology. I try to keep to the less pretentious, but safer, matters of actual and demonstrative fact. The exact characters of particular fossils, the nature of their associates, the nature of the sediments in which they are found, the precise localities in which they are found, and the successions of types to be found in those localities—these are matters of fact; and they are demonstrative facts, too, since any man may go to the places I name, and either confirm or correct my observations on the spot. There is quite enough here, in the way of true science, to keep a man occupied all his life—even if he has a whole life to give to the study. The arranging of fossils into hypothetical genetic trees, however, is, in my opinion, nothing but a dangerously attractive way of wasting time and piling up structures which others will probably have to demolish before long. Yet no one, I believe, could indulge in the pastime more easily than myself, if I thought it right to do so. Only the other day, when reading a paper on the succession of certain echinoid forms in the lower Tertiaries of India, I was asked by a friend of mine, one of the palæontologists present, whether I did not regard the various species I was describing, as being the members of a locally evolving group. The temptation to regard the modifications of type, found at different horizons, as evidence of progressive evolution through descent, was almost irresistible; and I am sure that nobody present would have objected had I yielded to the temptation. But alas for the demonstrative value of such ideas! All

that I could really claim was what I did claim—a local succession of types. That was demonstrable. But whether the types were successively derived from each other, or successively created, or were simply contemporaneous forms which succeeded each other locally on account of locally changing conditions, who could say? One might choose any theory that happened to jump to one's fancy, for none was demonstrable.

That is why a man like C. Depéret remarks of all supposed fossil ancestries that: "The genealogical trees we are able to draw up by relying upon morphology and on chronological series are subjective to the feelings of each observer" (*Transformations of the Animal World*, p. 114).

In other words, we may accept or reject them as we like, for there is no necessary truth in any of them. It is worth remembering that Depéret himself (one of our foremost authorities on mammal remains) will have nothing to do with the famous fossil "ancestries" of the horse, which Huxley long ago regarded as "demonstrative" of the truth of evolution. According to Depéret, these fascinating series of bones are nothing but "pretended pedigrees" and "deceitful delusions."* Depéret implores his fellow palæontologists to remember that evolution is still "*only a theory*, which requires to be proved," quoting the words of Zittel (another leading authority) to that effect.† How its truth is ever to be proved, in face of Depéret's own admissions, it is not easy to see.‡

The truth is, that, *historic* testimony being necessarily absent in palæontology, the very multiplication of specimens only leads to an *embarras de richesse*. Where you have only a single *Archæopteryx*, you feel certain that birds have descended from reptiles, and quote that particular link. There is no competition. Where you have an abundance of three-toed and other types of horse-like creatures, however, you begin to feel the oppression of rival claims, and wonder which to choose. So there are many supposed genealogies of the horse, and you only have to examine enough of them to find that the modern

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* *Op. cit.*, p. 105, etc.

† *Ibid.*, pp. 117–18. The italics are Depéret's.

‡ As Prof. Scott remarks: "From the very nature of the case, complete demonstration is impossible" (*The Theory of Evolution*, p. 168). From the very nature of the case, then, evolution must continue to remain "only a theory."

horse is the only animal common to the lot.* There is not a single supposed "ancestor" whose claims have not been flatly denied by the most excellent authorities.

And when you get an unlimited amount of evidence, as you do when you examine the members of a living species, you find that no man in his senses would attempt to name the actual father of the "Unknown Warrior" apart from historic testimony.

In other words, the more we get of the evidence, the more clearly we see that that sort of evidence cannot of itself do what we wish it to do. The essential factor is historic evidence, and that is missing.

Now the early believers in creation helped the evolutionists considerably at times—and the cause of real science not at all—by the assumptions they made. It was *assumed* by such men as Alcide d'Orbigny and Louis Agassiz, that a newly created form must always be specifically distinct from previously created ones. Hugh Miller also pleaded urgently for this idea. But it was not a Scriptural idea at all. In the book of Exodus we are told that Aaron stretched out his rod and smote the dust of Egypt, and it became lice in man and in beast (viii, 16-19). Now that was an act of special creation, and we are told that the Egyptians had there to recognize the actual hand of God. We have no reason to suppose that these "lice"—or whatever species the Hebrew word may imply—were different from already existing ones; and so we see that the Bible itself not only admits of identical species being created more than once, but it even allows of both creations of the same species being in existence together. Scripture thus refuses to limit itself in the way that Hugh Miller and others wished; and so how can science oppose it?

Neither the fact that individuals belong to the same species,

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* Sir J. W. Dawson's remarks on this subject are worth remembering. After pointing out that the modern horse has been traced back to *Palæotherium* in Europe, and to *Eohippus* in America—these being, as Depéret shows, two entirely different forms—he goes on to say: "Both genealogies can scarcely be true, and there is no actual proof of either. The existing American horses, which are of European origin, are, according to the theory, descendants of *Palæotherium*, not of *Eohippus*; but if we had not known this on historical evidence, there would have been nothing to prevent us from tracing them to the latter animal. This simple consideration alone is sufficient to show that such genealogies are not of the nature of scientific evidence" (*Modern Ideas of Evolution*, p. 119).

nor even the fact that they appear side by side in the same sediments as co-existing members of the same species, can serve to prove that those individuals were not separately created, if the Bible be true. Philosophy may oppose with its whole armoury of objections, but science is disarmed. Science can only point to sequences of forms, their similarities, and their differences. If a Creator exists, who can and does create a finished article in a moment exactly like other articles normally begotten, then nothing in science can witness against His doing so. Remember how Satan invited our Lord to command stones to be made into bread (Matt. iv, 3, 4): if the Bible is true, the invitation was a real one, and our Lord never rejected the suggestion as impossible but refused on quite different grounds, thus implying that the proposed action was a possible one. Yet Satan's words implied the power of our Lord to create instantly, out of inanimate materials, both the *matured* and the *cooked* products of animate life, of identical species with ordinary wheat. Suppose such a challenge accepted and the miracle effected—how could science deal with it ? Apart from historic testimony as to the origin of the bread, all that science could do would be to affirm its exact resemblance to other bread.

You see how powerless science is in this matter of creation *versus* continuity. We know that creative acts do not normally occur ; but we cannot say it is an admitted fact that " no one ever saw a special creation," for the book of Exodus declares that some people did witness a special creation. We may refuse to believe the testimony, but we cannot deny its existence.

Personally, I take the Bible very seriously indeed ; and so I accept the testimony.* I am, therefore, very cautious in dealing with fossil forms. Such forms may or may not be *genetically* connected, but I know that I could never actually prove such a connection ; and so I keep to the things that can

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* " It is self-evident " said Tyndall, " that, if there is a God, He is Almighty, and, therefore, can perform miracles." Even Huxley admitted the same thing. "It seems to me " said he, " that ' creation ' in the ordinary sense of the word is perfectly conceivable. . . . The so-called *à priori* arguments against Theism, and given a Deity, against creative acts, appear to me to be devoid of reasonable foundation " (*Life of Darwin*, vol. ii, p. 187). In other words, literal creation is possible so long as the existence of God Himself is possible. So, since science is powerless to testify against creative acts, we have nothing but human philosophy to oppose to the witness of Scripture.

be proved. I aim, that is, at "clear and demonstrative knowledge" *alone;* and I defy anyone to show that my practical geological work is hampered in any way whatever by my talking of the local successions of the forms I describe, instead of their lines of descent. In fact I sometimes draw up "trees" myself, in order to illustrate the successions and branching correspondences of the forms I study; but I decline to call these *genetic* trees. The "affiliations," etc., which they are meant to illustrate may, indeed, represent genetic connections in some cases, but it is quite possible that a number of them simply represent affiliations of ideas in the mind of a Creator. We can draw up exactly similar "trees" to illustrate the development of motor-bicycles from "bone-shakers," where genetic connections are out of the question and all "affiliations" lie in creative minds.

Let me remind you, therefore, that the fact of a succession of ever higher types being found in the rocks is no argument against creation. A hundred years ago, those whom we now call the "fathers of palæontology" were believers in creation almost to a man; yet, almost to a man, they believed in a succession of ever higher types, just as we do. What is more, they sometimes made just as shrewd guesses, as to what to expect in the rocks, as we do. Agassiz, we are told, was once asked to describe the sort of fish he would expect to find in beds of a particular horizon which had never, up to then, produced any fossil fish. He did so, and apparently outlined the very characters of a fossil which had—unknown to himself—just been found in those beds. One does not need to be an evolutionist in order to appreciate the idea of fossil successions from lower to higher types.*

This is all very hurried and sketchy, perhaps, but the subject is so vast that it is hard to choose what to mention and what to leave out. Geology is a fascinating subject; and to me it has become a matter of almost passionate interest to study and describe the forms I find in the rocks. At the same time, however, I am a Christian; and I most gravely suspect that doctrine of

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* I also, the other day, was able to state the probable horizon to which a new and rather abnormal species of echinoid, found in the museum at Calcutta, belonged. It subsequently proved to have come from that horizon. In another case, some foraminiferal forms, found at a lower horizon than any at which the genus had yet been represented, fully bore out my anticipations regarding the probable character of such early forms.

interminable genetic connections which saturates the descriptive works of most other palæontologists to-day. I know it to be utterly unprovable; and feel compelled to say so, when asked to give my opinion.

Further, I believe the doctrine to be mischievous. Recently, for instance, I read the following:—

"THE SECOND COMING. '*No Hope of Physical Manifestation,*' declares Dr. Major. *Evolution Faith.* 'The hope that Christ will reappear in a physical manifestation is not held nowadays by educated people.' So declared the Rev. H. D. A. Major, of Oxford, preaching the Advent sermon yesterday at St. George's Church, Stuyvesant Square. Such people, he said, based their hopes of human progress on their conception of evolution." (*The Daily Mirror* 1/12/1925, p. 2.)

Here, you see, we find a professing minister of the Gospel denying the literal Coming of our Lord, on the strength of his ideas about evolution. It is significant that, nearly 2,000 years ago, it was prophesied that:

"There shall come in the last days scoffers . . . saying, Where is the promise of His Coming? For since the fathers fell asleep, *all things continue as from the beginning of the creation*" (2 Pet. iii, 3, 4).

This is a most striking prophecy, for it puts the modern doctrine of continuity into the mouths of these latter-day scoffers. Our Lord's Coming was to be denied by them upon the strength of a belief that present-day processes could be traced back, without a break, to the very beginning of the creation.* God's interventions would thus be specifically ruled out, and creation itself be explained upon a basis of present-day processes.

In other words, St. Peter's latter-day scoffers at the Second Advent were to be nothing more nor less than modern uniformitarian evolutionists.

It is impossible, as Dr. Major shows, for such people to believe in the literal return of our Lord. Their doctrine of continuity forbids their crediting any abnormal event. Their hopes for the

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* There is no doubt about the accuracy of this rendering. The Greek work *arche*, meaning "beginning," is there; so the scoffers are not simply talking of events since the creation, but are including creation itself in their scheme of uniformity. Their creed is thus identical with that of the modern evolutionist.

future, therefore, are ruled by their conception of the past; and they ridicule those, who take the Bible more seriously, as being less educated than themselves.

Thus we see that a prophecy which remained unfulfilled for eighteen centuries, is now being fulfilled before our eyes. It is rather a striking fact.

II.

The above is the paper that I originally prepared, for reading before you this evening. I have been told, however, that it is rather shorter than the papers usually read before the Victoria Institute, and I have been kindly invited to make it longer. I propose, therefore, to add a few remarks regarding some other deficiencies in palæontological evidence, when regarded as proof of descent, since I may have given the impression that the absence of historic testimony is the only serious defect—which is by no means the case.

I do not propose to dwell, here, upon the question of the "imperfection of the geological record," in the sense discussed by Darwin. I could easily quote reliable authorities to show how imperfect that record still is; what great deficiencies still remain in the matter of intermediate links between different types of structure;* but I prefer to pass such facts by, for the time being, in order to deal with more fundamental defects in palæontological evidence. I have no doubt that many more intermediate links are destined to be found than we have yet discovered; I myself always look for such links in the rocks, and see no reason why they should not have existed upon any theory of origins.† What

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* Thus Depéret points out that "*the majority of the fundamental types of the animal kingdom come before us without any link between them from a palæontological point of view*" (*op. cit.*, p. 74; the italics are his own). "(We) have to confess" he adds later on, "that at the present day we are utterly unable to see and even to explain otherwise than by simple theoretical views the fundamental divergences which separate the orders, classes, and great ramifications of the animal kingdom" (p. 279).

† It is worth noting, however, that while the intermediate types which we find usually serve well enough to *fill gaps* they cannot so easily be fitted into *direct series*. Thus in the case of the echinoid succession referred to above, no actual species of a middle horizon could be placed in exact series between species belonging to the horizons above and below; for every species was, in some respects, individualistic, and specialized out of exact series. It was in the *general characters* of the species of each horizon

I would ask you more particularly to consider, therefore, in the short time left to us, is the matter of those deficiencies which are inherent in the very nature of palæontological study, and which are therefore less likely to be corrected in the course of time.

(*a*) In the first place, then, I would recall the fact that it is, in the very nature of things, only the harder parts of animals that are preserved in the rocks. Exceptions to this are so rare, and themselves so fragmentary in the information they give, that they can be disregarded for practical purposes. All that we can normally compare, in palæontology, is one skeleton (internal or external) with another. The softer parts of the animals concerned have to be judged of from the indications afforded by the harder parts ; and those indications may be far too few for our purpose. Thus Professor Flower pointed out, in his book on *The Horse*, that if we had only known of horses, quaggas, zebras, and asses, from such parts as might be preserved in a fossil state, we would never have guessed how widely they differed in other respects. So we see that, if we could only study our fossil animals in the flesh, we would probably have to separate, as distinct species, a great many forms which we now regard as identical. But how this complicates matters for the palæontologist ! For if practically identical bony structures can thus be

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that I claimed to find a progressive change ; and it was by examining the quality of those characters in the unknown species, that I determined the horizon to which it probably belonged. Similarly with some foraminifera on which I have lately been working : a recently described species from the upper Ranikot of India proves to be just what was required to fill a gap, in certain general respects, between the species locally found in the sediments above and below ; but it is so strangely specialized out of series in some respects peculiar to itself, that it might almost be placed in a different sub-genus from all the others, and certainly could not represent a link in a genetic sequence. This is a very general phenomenon in palæontology ; for it is notorious that while it is easy enough to fill many gaps after an approximate fashion, it is almost impossible anywhere to form what Wood-Jones calls a true "end-on" series. Thus when Cope, Adloff and others pointed out that man's evolutionary "pedigree" was being filled up with creatures which could not possibly be regarded as his actual ancestors, Professor G. Schwalbe could only defend the practice by saying that there was nothing else to fill it up with. He added that similar objections could be raised against every other creature's supposed genealogy ; in other words, not a single one will bear close examination. (*See* Schwalbe's article on "The Descent of Man," in *Darwin and Modern Science*, pp. 131–134.)

possessed by creatures belonging to very distinct species, how can the palæontologist ever be sure that the skeleton he puts into a particular series belonged to a creature which was otherwise suitable to go into that series ? It seems clear that he never can be sure ; and so we find that a man like Professor Wood-Jones, when referring to those who glibly talk of finding true links between men and apes, insists that such people should first " become thoroughly acquainted with, in order fully to appreciate, the great differences which exist between anthropoids and man in those regions of the body which can never become the object of their study in fossil fragments " (*The Problem of Man's Ancestry*, p. 46). No matter how perfect a skeletal link we may find, to fit between other skeletons, we can never prove that it belonged to a creature whose softer parts were equally in series.

Nor is it only when studying fossil Vertebrates that such facts are brought home to us. The same thing is found when we deal with other branches of the animal kingdom. Thus, if we turn to the great phylum of the Mollusca, we find that while zoologists, dealing with the living creature, have proposed taking the structure of the gills as the soundest basis for classifying the Lamellibranchia, this plan has to be rejected by palæontologists, since it concerns fleshly characters which cannot be seen in fossilized forms. Similarly, in the case of the Gastropoda, we find that the features which zoologists have found to afford the best basis for a natural classification, are ones which leave no mark upon the shell, and so cannot be judged of when dealing with fossils. Thus we are apt to find, when we compare fossil structures with living ones, that not only is a very great deal of the evidence missing, but it is often the most important part of all that is missing.

(*b*) What, too, can we generally assert in regard to the ontogeny of our fossil types ? Next to nothing in many respects. As with the adult forms, traces of embryonic phases are only preserved in the case of the harder structures concerned, such as the early whorls of foraminiferal or mulluscan shells, or the remains of animals that died when young, or that shed hard skins during the process of growth. What student of embryology would content himself with examining the bare skeleton or empty shell of a new type ? Yet the facts in regard to living forms show us that seemingly quite small differences in the details of ontogeny may be correlated with complete physiological

separation of types. Thus it seems that the most important difference between the rabbit and the hare lies in the fact that the former brings forth its young in a blind and naked state, while the latter does not; yet the most determined efforts to cross the types have completely failed to produce a hybrid race. Such minute details of embryology could never be preserved in a fossil state, however; so we again see how the very resemblances in things that could be preserved in such a state would only prove a trap to the investigator who tried to draw up a phyletic series.

(*c*) We must constantly remember, in this connection, that the "species" of the palæontologist are purely morphological ones. They cannot, under the circumstances, be anything else. And so we are bound to admit that they are extremely artificial, as compared with the better-known species of the zoologist, since they can take no count of those more subtle and apparently more fundamental affinities which are revealed by the power of creatures to combine to produce perfect offspring. As we have seen above, we find that some living animals, which seem to us very similar, are physiologically quite distinct; and yet it is equally true that others, which seem to us at least as distinct in form, are physiologically identical. Thus the great differences between the members of the dog tribe are notorious; and Darwin himself remarked that if the various breeds of pigeons were judged of on the same lines as creatures found in the wild state, they would be placed by ornithologists not only in separate species, but even in separate genera. Similarly Professor Bateson tells us that: "We may even be certain that numbers of excellent species recognized by entomologists and ornithologists, for example, would, if subjected to breeding tests, be immediately proved to be *analytical varieties*, differing from each other merely in the presence or absence of definite factors" (*Mendel's Principles of Heredity*, p. 284). So we see how the increasingly more exact study of living types warns us against regarding fossil series as representing anything better than provisional guesses as to real affinities. Those forms which we place specifically, and even generically, apart, may be (in a physiological sense) identical. Those which we regard as members of one and the same species, may be physiologically quite distinct. The very means for forming a sound judgment, as to the real affinities of types, do not exist when we deal with fossils.

(*d*) Take, again, another fact. Darwin insisted (and I think rightly) that, in palæontology, positive evidence could alone be trusted, since negative evidence was "worthless." He pointed this out when defending his theory against those who objected that the links required by it had never been found. Darwin held that the links might yet be found; and his contention was quite legitimate. Negative evidence is, I believe, of little account in palæontology. But, if Darwin had been more consistent, he would have seen that this very fact was double-edged, since it must cut at the roots of all attempts to prove evolution by appealing to fossil series, for *all such series are essentially founded upon negative evidence in palæontology.* They all tacitly assume, that is, that "younger" types did not exist contemporaneously with "older" ones, simply because they have never yet been found at similar levels. So we see that no man who really regards negative evidence as "worthless" in palæontology, can ever consistently appeal to a fossil series as proving descent. He knows that he could never be sure that any given form was younger than its supposed ancestor.

Nor is this simply an academic point. The history of the subject shows how repeatedly we have had to antedate the first appearances of types in palæontology. Thus it was, for a long time, regarded as certain that the first fishes appeared in the upper Silurian. Yet fishes have now been found, and found in swarms, in certain Ordovician sediments; and it is regarded as extremely probable that they also existed in the Cambrian. That is only one general instance out of many that could be quoted of a similar nature. If we come down to particular genera or species, we find much the same thing happening on a smaller scale. Thus Depéret rejects the orthodox "genealogy" of the modern bear, on the grounds that what appear (so far as we can judge from fossil remains) to have been virtually true bears in all but size, are now known to have existed since the middle Miocene (*op. cit.*, p. 106). Similarly Sir Arthur Keith has devoted no less than three chapters of his book on the *Antiquity of Man* to proving that what seem to have been men of a perfectly "modern" type preceded, by two whole cultural stages, the Neanderthal remains associated with "Mousterian" implements. In short, there is no way known to science, whereby any given member of a fossil series can be definitely shown to be younger than its supposed ancestor. We may have reasons for regarding it as extremely probable that the youngest members of a series

covering a great geological range represent later species than the oldest members do, but there is not even a great probability when we deal with specimens taken from a limited range of horizons ;* and the antedating of a form even a short way, may mean a recasting of the whole previous series.

(*e*) The same sort of difficulty presents itself when we ask where a fossil species first appeared. Here, again, the palæontologist can only appeal to " negative " evidence if he tries to answer the question ; for all he can positively say is, where the earliest known representatives of the species have hitherto been found. He cannot show that still earlier ones will never be found somewhere else.

I am afraid that some palæontologists here follow Darwin's example, and remember the limitations of geology only when it suits them to do so. Thus when the evolutionist finds a new and highly specialized type appearing abruptly in a given area (e.g. *Conchidium knighti*, at the base of the English Silurian), he at once assumes—and quite legitimately—that this " cryptogenetic " form may have been evolved elsewhere, and its sudden local appearance may be due simply to its migration from the scene of its earlier history. The possibility, however, is double-edged ; for the form which the evolutionist (when all runs smoothly for his theory) claims as a member of a given sequence, may also have migrated from elsewhere, and so have nothing to do with those between which it is placed in a local series.† So

* Is *Nummulites lævigatus* younger or older than *Cerithium giganteum* ? In France, *C. giganteum* and *Orbitolites complanatus* appear together, as characteristic fossils of the uppermost beds of the " Glauconie grossière," three zones above the level at which *N. lævigatus* first appears. In India, on the other hand, the same two species are found together in the " Alveolina Limestone " of Vredenburg, at a level three zones below that at which *N. lævigatus* first appears.

It is true that these three species belong to entirely different groups of forms ; but the facts show, nevertheless, that a succession of types in one place may be entirely reversed in another. Local succession affords no criterion as to the relative dates of first appearances of species.

† Dr. Bather pointed this out when he remarked that if anyone, rightly guessing that the crown of England was normally hereditary, and finding evidence on coins that James the First succeeded Queen Elizabeth, concluded that James was therefore her son, he would be quite wrong Yet that is just the sort of mistake, said Dr. Bather, which palæontologists are always making to-day, in regard to local successions of fossil forms. " Descent " he tells us, " is not a corollary of succession."

here again we find that the evidence cannot be secured, which is required in order to place our views as to descent upon a scientifically sound basis. It is obviously impossible to prove that one species was derived from another, when we cannot even prove that it was, at the time of its birth, in the same country as its supposed parent.

It would be easy to continue, but enough has perhaps been said to show the sort of questions which a palæontologist would like to ask of his fossil forms, but which he knows they could never answer. So, dealing as he does with the most fragmentary evidence, every attempt he may make to form a genetic series of bones must, as Haeckel said, be nothing but a "fabric of hypotheses." Not only is there a complete absence of historic testimony as to the actual mode of origin of his forms, but the forms themselves remain practically unknown as regards their softer parts. Most of the details of their embryonic development are equally unknown; and it is also utterly impossible to recognize the physiological limits of each type. The *date* of first appearance, in any particular case, can never be finally known, but must always be assumed upon the strength of that "negative evidence" which Darwin declared to be "worthless"; and all opinions as to the equally important question of the *locality* of the first appearance of a type must also rest for ever upon the same basis of "negative" evidence.

Realizing the fundamental inability of fossil series, therefore, to establish the fact of genetic connections, I flatly refuse to regard such series as scientific evidence of descent. They may be taken as representing possibilities, or as illustrating certain views regarding descent; but they are in no sense a *proof* of descent, since they carry no guarantee whatever of direct genetic connections. Remember, too, that the specimens composing the usual fossil series are not even supposed to be father and son, but mere occasional individuals separated from each other by untold myriads of intermediate generations which are not represented at all, and most of which must be regarded as lost for ever. In other words, it is never the whole chain that the evolutionist shows us, even when he produces his most perfect series, but only half a dozen links or so out of many millions, the vast majority of which have to be left entirely to the imagination. It is philosophy, philosophy alone, which knits these few and widely scattered facts together into a scheme of universal and uninterrupted

genetic connections. So what can we say ? Descent may or may not be a fact; but there appear to be no scientific means of establishing it as a fact.

That belief in evolution has come to stay, nobody holds more firmly than I do. The same Book which so stikingly foretold its rise in the "last days," also foretold that it should be an increasingly popular belief, and prepare the way for certain definite events, most of which seem now to be taking actual shape.* I cannot think it probable that matters will be reversed when things have gone so far. That, however, is another story. What I wish to point out here is that, although the Bible foretold the rise of the modern doctrine of uniformity, it nowhere implied that the doctrine should be a true one. Quite the reverse.† And I have tried to show that, on examining the actual facts, there appears to be no reason why anyone who still likes to retain belief in literal creation, should feel debarred from doing so. Evolution is not science, and—on the testimony of Haeckel himself—it never will be science.

Discussion.

Lieut.-Col. Mackinlay said: Major Davies has given us a very valuable paper, evidently the result of a careful study of the subject. It is specially valuable because it points out the necessity of making

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* It seems, by putting the prophecies regarding the "last days" together, that belief in uniformity is to lead to rejection of belief in the Flood, to rejection of belief in the Second Advent, and to rejection of belief in the future Judgment and everlasting perdition of sinners. In spite of the fact that uniformitarians are to be "ever learning," they are never to come to a knowledge of the truth as it is in Christ Jesus. They are, on the contrary, deliberately to turn away their ears from that Truth, and be turned to fables. Evil men and seducers are to wax worse and worse, deceiving and being deceived. And, finally, the deification of humanity, which is one of the corollaries of uniformitarianism, is to come to a head in the acceptance and worship of a transcendent genius, a veritable Superman, who will give himself out to be actually a god.

This last event is the only one which can fairly be said to be still altogether in the future; and yet even it must now be allowed to be a reasonable prospect, logically following upon belief in uniformitarian postulates. Nothing could better lead up to the introduction of a Superman, than our present pseudo-scientific creed of evolution.

† It refers to it, indeed, as "the error of the wicked" and a "strong delusion" (A.V.).

sure of the ground before adopting views which, though they attract many minds, cannot be demonstrated as true. There are many who, desirous of hearing some new thing, need to be put on their guard before accepting a present-day theory simply because it is popular.

The Rev. A. H. FINN said: The chief aim, I take it, of the paper is to show that Evolution is not a scientific truth, but a philosophic theory for which no scientific evidence can be produced—a very valuable argument. I should like, however, to bring forward some other points.

Strictly speaking, Evolution should mean that whatever is evolved was already latent in whatever went before. "The world," says a convinced evolutionist, "and everything in it, including man, have come to be what they are in virtue of inherent powers and capacities, by processes that have been continuous and orderly through time." That would mean that the genius of Shakespeare and the intellect of Newton existed in embryo in protoplasm or even primeval slime.

Nowadays, however, many advocate a modification of the original theory called "emergent evolution," mentioned recently by Canon Storr in a paper read before the Institute. That appears to mean that while in general Evolution proceeded automatically, yet at certain crises some new factor was introduced. So Bishop Barnes has said: "When life emerged from non-living matter, or, again, when self-conscious mind grew in living things, God made something new. So, also, in creating the soul of man He made something new, definite, real, something different from any previous evolutionary product." That really amounts to a series of new creations, and is a serious departure from what is ordinarily meant by Evolution.

This is not the only instance of modern evolutionists departing from the original theory. On p. 216 allusion is made to Bishop Barnes' "declaration that man was certainly descended from an ape." Yet a few years ago he said, "Man . . . is the final product of a vast process by which all life has developed from primitive organisms. Biologically he is cousin, a hundred thousand or a million times removed, to the gorilla, and his ancestry goes back through amphibians to fishes," which is not quite the same as "descended from an ape."

Evolution, it is often confidently asserted, is found in the history of mankind, rising gradually from rudest savagery to the present high condition. Is that fully justified by the evidence? Suppose a terrific convulsion were to wipe out completely the present human race, while burying and preserving their works. Suppose some 3,000 or 4,000 years hence another race of intelligent beings were to set about exploring and excavating. In Europe they would find traces of splendid buildings, steam-engines, ironclad vessels, rifles, cannon, and the like. In other parts, such as the Andaman Isles, they would find rude huts, canoes, bows, and arrows with points only hardened in the fire. They might easily jump to the conclusion that this latter state belonged to a much earlier era than the former, a state when men had not learned the use of metals. Yet we know that both states are co-existing at the present day. Is it not possible, then, that the various so-called ages—Stone, Bronze, and Iron—may have been to some extent contemporaneous? It is really an assumption that these were world-wide stages and successive.

When, too, it is taken for granted that the original state of mankind was only that of savages little higher than animals, has sufficient allowance been made for the possibility of degeneration? Compare the marvellous achievements of Ancient Egypt with the conditions that prevailed there in quite recent times. To my mind, the boasted Evolution of the human race is by no means conclusively proved.

The Rev. MORRIS MORRIS, M.Sc., said: I congratulate the author on taking an interest in Geology. So do I. I not only took final honours in the subject, but have also published researches. I also congratulate him upon being a Christian. So am I. In my opinion, the Gospel has been verified by experience, as well as anything can be, and, therefore, I would query any interpretation of events which wars against it.

But that is as far as I can go in congratulating the author, for he has confused two things which should always be carefully distinguished, namely, Evolution and the Doctrine of Descent. Evolution implies the Doctrine of Descent, but the Doctrine of Descent does not imply Evolution; and, in assuming that it does, he has committed the very common fallacy known as "undistributed

middle." All Yorkshiremen are Englishmen, but not all Englishmen are Yorkshiremen.

The author has covered so much ground that I cannot, in the space of five minutes, cope with all that he has said. I will, therefore, confine myself to his main point. He set out to overthrow the theory of Evolution; and there I am one with him, for Evolution is a false version of the Doctrine of Descent. But, instead of opposing Evolution, he has attacked the Doctrine of Descent, showing clearly that he does not recognize the difference between them.

The crux of the matter is not Descent, as the author supposes, but *Variation*. We infer Descent from direct evidence, and the evidence is overwhelming and incontestable; but we infer Variation from Descent. For many years naturalists have been absolutely unanimous in accepting variation during descent; but, unanimous as they are about the *fact* of Variation, they are equally unanimous in admitting that we know nothing whatsoever about its nature and cause. The author has made remarks reflecting on the integrity of naturalists. I was sorry to hear him speak so, for, in my experience, they are the truest of the true. When they know, they say so; and when they do not know they are equally candid. There is not one of them who claims to know anything about the nature and cause of Variation. It remains as great a mystery as ever.

And if we do not know what it is, what right has anyone to call it Evolution, as if we *do* know? It may have consisted in creative power. Take, for example, the differentia of Man. What caused it to appear in the first man? That is the question. There are *two* possibilities: either it was *evolved*, that is, produced by natural processes, or else it was not evolved, in which case it must have been *created* in his developing body before birth, and added to the qualities which he inherited.

You all know that Christ descended from ancestors. The New Testament commences with the descent of the last Adam from the first. Therefore, He must have inherited most of His qualities. But He also has qualities which were not inherited. These were added in His Mother's womb; and we call that act or addition the Incarnation. Is it not conceivable to you that something similar took

place in the *first* Adam ?—that, although he descended from an earlier species, something was created in him before birth and added to his inherited qualities ?

Do you say this is contrary to the Scriptures ? Nay, I have deprived you of the right to say that ; for in *New Light on Genesis* and in *Man Created during Descent,* I have shown that this is the *Scriptural* standpoint ; and I will challenge anyone to interpret Gen. i and ii from any other standpoint without violating the original and making it inconsistent with itself.

If the Doctrine of Descent offers two interpretations, according as Variation is considered to have been a natural process (Evolution during Descent) or a creative act (Creation during Descent), ought we not to distinguish them ? Otherwise we must either accept them both, or reject them both. In the one case, we must accept the false as well as the true, and in the other we must reject the true as well as the false ! Would it not be better to reject the false version (Evolution during Descent) and keep the true one (Creation during Descent) ?

Instead of doing this, the author has tried to overthrow Descent ! And, needless to say, he has failed miserably. If I had time, it would be easy to expose the falsity of his arguments and the irrelevancy of his quotations. From Huxley, Mitchell, Thomson and Geddes, for example, he has quoted passages which refer to one thing, and has applied them to something else. Naturalists do not rely on ontogeny alone for ascertaining stem-history, for it not only recapitulates stem-history, but contains secondary modifications as well. But although the law of recapitulation is not the *only* factor which determines the metamorphosis of embryos, yet it *is* a factor, and all naturalists, without exception, believe in it, including those mentioned by the author.

I cannot conclude without adverting to the author's extraordinary notion of what constitutes a scientific proof. It is absurd to sweep aside the testimony of the Geological record and say it has no bearing on Descent because no one was there to *see* any of those ancient forms giving birth to one another. The method of Science is *inductive.* It begins with a theory and then tests it by observation and experiment, to see whether it is true. The Geological record confirms the Doctrine of Descent in every particular.

Mr. W. E. Leslie said: The paper is of peculiar interest, since it is a philosophical criticism of the nature of scientific inference, written by a disciple of one of the physical sciences.

I fear Major Davies is somewhat in the position of a man who, desiring to saw a branch off a tree, omitted to notice that he was sitting upon the branch! His contention is that historical evidence for genetic sequences in Palæontology is absent. But how much historical evidence is there for his own science—Geology? Surely, instead of condemning the theory of genetic Evolution on the ground that it is supported by philosophical arguments, we should say it is supported by fallacious philosophic arguments.

I have greatly enjoyed the author's masterly handling of the geological part of his subject.

Written Communications.

Colonel H. Biddulph, C.M.G., D.S.O., R.E.: The note on p. 218, "There is a closer connection between social philosophies and evolutionary beliefs than most people realize," is worthy of considerable attention. If man and beast have a common origin, and progress is due to Evolution alone, then mankind is logically shut up to a strictly utilitarian philosophy, and can look forward merely to the goal of becoming ever more and more a highly specialized "scientific" animal—a truly appalling outlook! The denial of man's origin as being created in the image of God leads logically (and in practice eventually will do so) to the most terrible conclusions. In this connection it is worth noting what Disraeli said at Oxford, in 1864, on "Evolution":—

"What is the question now placed before society with a glib assurance the most astounding? The question is this: Is man an ape or an angel? I am on the side of the angels. I repudiate with indignation and abhorrence the contrary view. . . . The Church teaches us that man is made in the image of his Creator—a source of inspiration and of solace—a source from which only can flow every principle of morals and every Divine truth. . . . It is between these two contending interpretations of the nature of man and their consequences that Society will have to decide. Their rivalry is at the bottom of all human affairs. Upon our acceptance of that Divine interpretation . . . all sound and salutary

legislation depends. That truth is the only security for civilization, and the only guarantee of real progress." (*Life of Disraeli* (Buckle), vol. iv, p. 374.)

Professor A. RENDLE SHORT, M.D., B.S., B.Sc., F.R.C.S.: As one who, like the author, has spent very many happy hours geologizing in the field, as well as studying the theory of the science, I would like to express my great interest in his paper, and especially in the second instalment, which the Victoria Institute was well advised to ask for. I desire to corroborate and enforce most of what he says. Evolution is much more a philosophy than a deduction from scientific facts. It is only in the dogmatics of text-books that Embryology (the Recapitulation Theory) is relied upon to prove the doctrine of Evolution: "The Recapitulation Theory of Fritz Müller and Haeckel is chiefly conspicuous now as a skeleton on which to hang innumerable exceptions. . . . The Recapitulation Theory is mostly wrong" (Professor Kellogg in *Darwinism To-day*). As a simple example of this, let us consider the stages of development of a butterfly: first the egg, then the caterpillar, then the pupa, and finally the imago or perfect insect. Now it might be reasonable, perhaps, to conclude that in past geological ages the ancestor of the butterfly was a grub, but we cannot believe that for the next few thousands of years it was represented by the pupa, motionless, not reproducing itself, its interior nothing but a mass of creamy cells in which no organs can be distinguished!

I would like to confirm Major Davies' remarks as to the limitations of the palæontological evidence. I know some of the older formations better; he cites the newer ones, in the main, but the same lessons may be learned in the one and the other. What look like continuous series of fossils, as we pass from older strata to newer, are often met with, but there is usually something to show that they are not on the direct line. In the very great majority of fossils only the shell (in lamellibranchs, brachiopods, and gasteropods) is found, and the internal structure is lost to us; sometimes a specimen turns up that reveals enough of some internal structure to show that the fossil belongs to another genus altogether, in spite of a superficial outward resemblance.

It is always a pleasure to read a contribution from one who has a competent practical and theoretical knowledge of those sciences

that run alongside of the Biblical record, who is not thereby stumbled in his belief in the Word of God, and yet does not ride off into fantastic theories that can convince no one but himself.

The Lecturer's Reply.

The lecturer, in his reply, regretted that the chief opposition to his paper had taken so intangible a form. His principal critic, Mr. Morris, had apparently been so eager to tell us what he thought the paper should have discussed, that he took little notice of what it actually did discuss. The greater part of his five minutes' talk, therefore, calls for no remark. He had, however, made allegations which could not be passed over. Thus he accuses the author of confusing two things. The author did nothing of the sort. Mr. Morris himself, no doubt, uses the terms "Evolution" and "Descent" to express two different ideas; but the author does not. On the contrary, at the beginning of his paper, the author carefully defines what he means by "Evolution" and "Descent," and shows that he uses these two words as interchangeable terms, referring to the one question of "unbroken genetic connections." So far from confusing two things, therefore, the author uses *two words* to express *one* thing—and he kept to the subject of that one thing (the question of genetic connections) throughout his paper. Mr. Morris's talk about "Yorkshiremen" and "undistributed middles," therefore, is simply irrelevant.

It is worth remembering, in this connection, that one of our leading geologists, Dr. Watts, has remarked that "The essence of Evolution is unbroken sequence" (*Geol. Mag.*, 1924, vol. lxi, p. 532). Thus we see that Dr. Watts himself does not use the term "Evolution" to refer to the *method* of Descent, as Mr. Morris would like to insist upon every one doing, but to refer to the *fact* of Descent, just as the author does. This is, indeed, the general practice, Mr. Morris's methods being peculiar to himself. It is also significant that Dr. Watts regards "unbroken sequence" as being the very "Essence" of Evolution; just as the author—perhaps a little more precisely—defines "unbroken genetic connections" as being the crux of the matter. (The "sequence" to which Dr. Watts refers

is, of course, one of *genetic connections;* otherwise there would be no Evolution.) If Mr. Morris, therefore, would have liked the author to discuss other things, Dr. Watts obviously would not. The author chose, for his subject of discussion, what Dr. Watts regards as being "the essence of Evolution."

The gravest feature about Mr. Morris's remarks, however, lay in his assertions that the author makes imputations against the integrity of men of science, and misquotes and misrepresents them. This is entirely false. The author does not discuss the integrity of his fellow-students of science, but their differences of opinion upon this question of Descent; differences which are notorious, despite the denials of Mr. Morris.

It was unfortunate that circumstances did not permit the author to produce all the actual works referred to, in order to show that he had *not* misquoted his authorities. This being out of the question, all the author could do was to point out that he had, in his paper, given references to show where most of the passages he referred to could be found in the original treatises. If these were insufficient, he would be only too glad to give further references; and he concluded by inviting his hearers to verify his quotations for themselves, and judge for themselves whether or not the contexts suited the uses he made of them.

Subsequent Communications.

From Mr. W. Hoste, B.A.: As Mr. Morris Morris challenged the relevancy and genuineness of Major Davies' quotations from Huxley and P. C. Mitchell, as also from Messrs. Thomson and Geddes (without, however, giving any instance), and since Major Davies appealed to his audience to verify his quotations, I have done so; and I find them, by actual scrutiny, to be verbally accurate and quite applicable. I also find, where the lecturer only professes to give a *résumé* that in each case this fairly represents and utilizes the thought of the writer referred to.

On p. 219, the lecturer asserts that the testimony of "Rudiments" "has always been regarded as far too uncertain to be trusted by more able thinkers like Huxley and P. C. Mitchell." In his article on "Evolution" in the *Encyclopædia Britannica* (ed. xi), Dr. Mitchell

urges caution "in endeavouring to support the doctrine of Evolution by them (*i.e.* "Rudiments"). For it is almost impossible to prove that any structure, however rudimentary, is useless . . . and if it is in the slightest degree useful, there is no reason why, on the hypothesis of direct creation, it should not have been created." In what way, then, does Major Davies misrepresent P. C. Mitchell? For nowhere else in his lecture does he refer to Dr. Mitchell.

In the same way, Huxley warns us that the facts of "dysteleology" (*i.e.* the study of the alleged purposelessness of certain living organisms) "cut both ways. If we are to assume, as evolutionists in general do, that useless organs atrophy, such cases as the existence of lateral rudiments of toes in the foot of a horse place us in a dilemma. For either these rudiments are of no use to the animal, in which case, considering that the horse has existed in its present form since the Pliocene epoch, they surely ought to have disappeared; or they are of some use to the animal, in which case they are of no use as arguments against Teleology" (reprinted, in "Critiques and Addresses," from *The Academy*, 1869).

Certainly, if an argument can confessedly be used equally for or against a thesis, Major Davies was accurate in his reference to Huxley and Mitchell, and was perfectly justified in his use of their testimony. He is no less so on p. 218, as regards Messrs. Thomson and Geddes. His comments on this page (the only place where he refers to those authors) are exactly borne out by a reference to their book on "Evolution," p. xi, where they say: "Yet it was essentially in the very opposite way" (*i.e.* not from scientific facts to theory) "that modern evolution doctrines really originated; *as a social theory*, that of progress, and the *generally diffused spirit* of the later eighteenth century, and the earlier nineteenth, has both consciously and unconsciously stimulated naturalist and physicist towards their evolutionary inquiries and doctrines. . . . Each of these two great advances of thought" (*i.e.* Doctrine of Evolution and Natural Selection) "is thus *the philosophic epic of a great nation* at its epoch; and Lamarck and Darwin are their representative prophets respectively." I fail to see any irrelevancy or misapplication of the admissions of these writers. The references are both accurate and apposite.

I think the above will suffice to show that every confidence may be

placed in the fairness of the lecturer's quotations and references throughout his paper.

I might, however, test one more reference on p. 218 ; this time to Depéret, a leading French palæontologist, in his *Transformations of the Animal World*, p. 122. "The hypothesis," he says, "not very tenable *from a philosophic point of view*" (my italics) "of successive creations has been maintained with real talent by the disciples of the Cuverian school." Does not this bear out the lecturer's contention as to the part which philosophic thought, as distinguished from scientific fact, has played in the sphere of evolutionary theories ?

As for Major Davies' references to Depéret and Von Zittel, on p. 224, and the footnote for the former on p. 224, I have verified each one, and found them to correspond in their context with the use the lecturer makes of them.

From the lecturer, Major DAVIES : I am very grateful to Mr. Hoste for adopting my suggestion, and consulting my authorities for himself. I am still more grateful to him for his kindness in reporting the result. Such charges as Mr. Morris apparently does not hesitate to bring against those whose conclusions differ from his own, are peculiarly difficult to deal with on the spot ; they require direct investigation of a kind which is not immediately possible.

Mr. Morris declared that, "if he had time," it would be easy for him to expose the falsity of my arguments and the irrelevancy of my quotations. Well, he has had plenty of time to do these things since my lecture, and I have repeatedly urged him to make good his claim. For reasons best known to himself, however, he has completely ignored all such invitations upon my part, however plainly worded. I may point out that it is not at all normal for one scientific worker to make sweeping charges against another without offering at least some attempt to justify them by quoting actual data.

Mr. Morris, then, can hardly complain if I treat him somewhat rigorously ; and now that I have seen his statements on paper, over his signature, I propose to deal with them as he should have dealt with mine—*ad rem.*

As I remarked at the beginning of my lecture, there are many people who attempt to obfuscate all free discussion of the question of Descent ; and it is as well, perhaps, to note how they do so.

So I would point out that Mr. Morris does not, from start to finish of his remarks, bring a single tangible argument to bear upon the actual issue of my paper. I had limited that issue to the basal problem (quite apart from any question-begging discussion as to the supposed *causes* of Evolution) of proving the actual fact of genetic connections; and I had shown the many patent difficulties that lie in the way of obtaining any such proof. Mr. Morris does not attempt to show how a single one of these difficulties is to be overcome. He simply says that I "fail miserably." This method of disposing, by bald assertion, of everything that calls for proof, is typical of the class which he represents.

Mr. Morris asserts that: "We infer Descent from direct evidence, and the evidence is overwhelming and incontestable." Unfortunately for Mr. Morris, the evidence notoriously is *not* direct, but circumstantial and indirect.* That is just why Dr. Bather remarks that no one ever saw the actual birth of a fossil ancestor.† *Historic* evidence would be *direct*; while "Geology," as Hugh Miller pointed out long ago, "only shows us things lying on top of things." To say that the evidence is "overwhelming" is purely subjective; it merely describes the effect which the supposed evidence, viewed as Mr. Morris views it, has upon Mr. Morris. To call the evidence "incontestable" is to beg the whole question.

Mr. Morris talks largely of the method of science being *inductive*; but seems to know little enough about it, nevertheless. A scientific

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* "By direct evidence is meant the statement of a person who saw, or otherwise observed with his senses, the fact in question. By indirect, or, as it is often called, circumstantial, evidence, is meant evidence of facts, from which the fact in question may be inferred or presumed" (*Man. Mil. Law*, p. 65).

Evolutionists often do (*see* pp. 218 and 219 of my lecture) refer to Palæontology as "direct" evidence *when compared with other* supposed lines of testimony to Descent. What they mean is, that it is more to the point than the others; and one can let it pass in that obviously loose sense. Mr. Morris, however, was not comparing lines of testimony, but claiming the evidence as direct in an absolute sense; which no one, realizing the circumstantial nature of *all* the evidence, could possibly have done.

† As Professor E. W. McBride told us the other day, in a paper at Oxford on "Evolution, a Vital Phenomenon," we "could never have direct evidence of Evolution, unless an angelic recorder had taken notes and those notes were available" (*Daily News*, August 27th, 1925).

"induction" does not, as he asserts, "begin with a theory." It begins with a hypothesis. If fresh facts bear it out, the hypothesis *then* rises to the dignity of a "theory." Even so, however, it remains far removed from Baconian *science*, or demonstrable fact.* Mr. Morris confuses hypotheses with theories, and theories with proved facts.

Because the facts of the geological record can—with some manipulation—be squared with Descent, Mr. Morris thinks that they prove it. But they can equally well—and with no more manipulation—be squared with belief in progressive creations.† They can, in fact, be squared with all sorts of different ideas—with a little manipulation. Palæontologists know this: their very disagreements show it. Hence Dr. Bather, when discussing the reasons for such disagreements, points out that "Descent is not a corollary of succession." Mr. Morris, however, brushes all such considerations aside.

Mr. Morris asserts that "The Geological Record confirms the Doctrine of Descent in every particular." This is simply misleading. The actual facts, as they stand, often do not suit Evolution at all. Pteropods appear long before the Opisthobranchiate Molluscs from which they are supposed to have taken their origin; the earliest Graptolites are more complex than their successors; the modern Monotremata are regarded as "representing" the ancestors of the ancient Marsupials; and so forth. The earliest forms of all the great types are far too high to satisfy the evolutionist; and the latter is also continually having to postulate fresh ancestries to account for the fresh forms which he finds—forms which, even when they serve to fill gaps, seldom go directly into series. Many, too, are the things which we are asked to believe in the cause of Descent. Thus

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* Thus Dr. Watts tells us, in the paper already referred to, that the "keynote" of the earth's history "is Evolution, the dream of philosophers from the earliest times, now passed from the realm of hypotheses into that of established theory." Note the successive stages: Philosophic dream—hypothesis—theory. And there the matter ends. Dr. Watts is too familiar with the realities of the subject to call Evolution an established *fact*.

† Whatever the palæontological facts might be, I would guarantee to raise suppositions to square them *either* with Evolution or with Creation. Both ideas are philosophies, and—in the nature of things—incapable of exact proof.

we are invited to agree that the pre-Cambrian rocks are too old and metamorphosed for us to expect them to contain any trace of those interminable ancestries which are required in order to explain the appearance of relatively high and well-differentiated forms in the early Cambrian. Yet, since we have actually found remains of fossil *jellyfish* in the Cambrian itself, it is hard to see why the record opens so suddenly. Discoveries of pre-Cambrian fossils are extremely rare*; yet, if a *jellyfish* could be preserved from the Cambrian onwards, it is hard to see why (if the required ancestries existed) the palæontological record should not go back at least as far beyond the Cambrian as the Cambrian is removed from ourselves. Sometimes, too, as in the case of the huge Cuddapah series of India, the pre-Cambrian beds (20,000 feet in thickness) are perfectly undisturbed, unaltered, and just of the sort to have preserved traces of life had any existed. Yet they are entirely barren, although the immediately succeeding formations contain abundant representatives of by no means the lowest types of life.† Such facts are notorious. The truth is that, so far from the Record confirming the Theory in every particular, we are always having to pull the

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* And among these earliest remains are Pteropods! The extremely early appearance of this type has been a sore trial to evolutionists; some of whom, like the eminent palæntologists Neumayer and Pelseneer, have strongly urged that, in spite of the structure of their shells, such pristine forms could not possibly have belonged to so highly specialized a family. Unfortunately for this idea, the more recent discovery of very perfect specimens, with distinct impressions of the Pteropoda, seems to put the matter beyond question. Fresh fossil evidence is not always of a sort to please the evolutionist.

† As Mr. Wadia, of the Geological Survey of India, remarks: "The entire series of Cuddapah rocks are totally unfossiliferous, no sign of life being met with in these vast piles of marine sediments. This looks quite inexplicable since not only are the rocks very well fitted to contain and preserve some relics of the seas in which they were formed, but also all mechanical disturbances, which usually obliterate such relics, are absent from them. . . . (In) formations immediately subsequent to the Cuddapahs . . . we find evidence of fossil organisms, which, though the earliest animals to be discovered, are by no means the simplest or the most primitive. The geological record is in many respects imperfect, but in none more imperfect than this—its failure to register the first beginnings of life" (*Geology of India*, pp. 72–3).

Theory this way and that in order to keep it fitted to the Record.*

Fortunately for the evolutionist, the Theory is so adaptable that an ingenious man can always raise suppositions to square it with any set of facts. Thus, when it became apparent that the great majority of the data of ontogeny are directly opposed to Recapitulation, the resourceful Haeckel, unwilling to give up the occasional coincidences as well, informed us that the embryo is subject to two influences—namely, *Palingenesis* which makes it repeat its ancestral history, and *Cœnogenesis* which makes it alter that history. So every happy coincidence is now credited, by people like Mr. Morris, to *Palingenesis*, and hailed as evidence for Evolution; while every discrepancy, however glaring, is waved aside as being caused by the wicked *Cœnogenesis*.†

The *naïveté* of this is delightful, and reminds one of how the small American boys, in *Tom Sawyer*, "proved" both the value of incantations and the devastating influence of witches. When they got what they wanted, after repeating an appropriate incantation, it showed the value of the incantation; and they had quite a list of such successes to enforce the point. When they failed, as they too often did, they could see how busy the witches were! Without in the least impugning the *integrity* of the more confident evolutionists, one can hardly help doubting the scientific value of their methods, which have so distinctly juvenile a flavour about them.‡

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* Thus, in the case of the early Pteropods, those malacologists who accept the facts (for a few still resist the evidence) surrender the orthodox derivation of Pteropods from Opisthobranchs, and postulate a common origin for both groups in those far pre-Cambrian days which have never yet produced any forms of life whatever. And yet some people claim that a single fossil found out of place would have destroyed the credit of Evolution! The history of the subject shows that, given a little practice, the follower of Darwin can always reconcile the facts to his creed, whether they stand on their heads or their heels.

† *Alias* "secondary modifications."

‡ These people seem to forget that, by admitting *Cœnogenesis*, they make it exceedingly difficult to prove that *Palingenesis* exists at all. But some evolutionists do see the difficulty. "Man," says Prof. Gamble, "is at no time a fish, an amphibian, or a reptile, as it is somewhat crudely put. . . . (The) older history like a papyrus has received alterations of a later date, and we

It is not true, as Mr. Morris asserts, that the authorities I quoted as attacking belief in Recapitulation themselves believed in Recapitulation. Sedgwick declares that Recapitulation and Evolution *cannot both be true*; Ballantyne concludes that ontogeny is *not* an epitomized phylogeny; and so forth. But even if such men did believe in Recapitulation, their sweeping admissions as to its falseness would reduce such continued belief to the level of a mere unreasoning vote, of no significance to any independent reasoner, and impressive only to counters of noses.

Too many people, however, think more about votes than reasons. It distresses them to admit that any votes whatever go in the wrong direction. They are therefore reduced to the strangest tactics when they find that some modern men of science do not even accept belief in Evolution itself. Time and again we see how the more extreme evolutionists first ignore, or decry, the testimony of experts like Fleischmann,* Reinke,† Meunier,‡ Wasmann,§ etc., and then talk as if scientific opponents of Evolution did not exist at all. Such tactics are more understandable than laudable. Mr. Morris himself coolly ignored the fact that, at the very meeting at which he spoke, both the author and the Chairman were Fellows of the Geological Society, and yet disbelieved in Evolution.

The remarks of Professor Rendle Short are in pleasing contrast to those of the critic last considered; and show how frankly one of the more serious students of science can admit the plain facts which less responsible people attempt to obscure. The ex-Hunterian Professor obviously does not see eye to eye with me in all things; but I am glad to find that he agrees with my main contention that belief in Evolution is much more a matter of philosophy than anything else.

Mr. Leslie's criticism is interesting. It is true, as he implies, that geologists do not usually, nowadays, analyse the potentialities of

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know not how much of the altered development to attribute to that added matter" (*The Animal World*, p. 232). The wicked *Cænogenesis*, that is, may be faking the very coincidences.

* Zoologist and comparative anatomist.

† Botanist.

‡ Geologist.

§ Entomologist.

their evidence. The older geologists, like De la Beche, used to do such things; some of the more senior thinkers, like Dr. Bather, still occasionally do so; but the younger ones seldom follow their example. It is generally taken for granted that things can be proved which are not strictly capable of proof. The fact of Descent, as Haeckel himself felt forced at times to admit, is one of them.

I cannot quite understand, however, why Mr. Leslie compares me to a man cutting off a branch upon which he is sitting; what he means by my "own science" of Geology, in this connection; and why he would have me talk of fallacious philosophies. My *own science* is Palæontology, which is a definite *subhead* of Geology. I was discussing that subhead alone, and showing its limitations. Mr. Leslie will also appreciate that, since I define a "philosophy" as a "method of explaining and co-ordinating facts, which suits a certain type of mind," I cannot decry Evolution so long as it continues to suit anybody at all; for it thereby fulfils its function. Mr. Leslie, obviously, does not use the term "philosophy" in quite the same sense as I do*; but he has a distinctly analytic brain, and I expect he will agree that, *under my own definitions of terms*, I can only talk of Evolution being a philosophy as opposed to science.† This is exactly what the clearer thinkers, among those who believe in Evolution, themselves admit it to be.

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* He would probably refer to things as "philosophic," which I would call "analytic."

† I do not wish to be misunderstood. While pointing out that my definitions of terms themselves prohibited my talking of inferior philosophies, I wish to keep it clear that I do *not* regard Evolution as an inferior philosophy in any sense of the words. I have no quarrel with those who accept it simply *as* a philosophy. My quarrel, from start to finish, is with the nonsense talked by those who would have us treat it not as a piece of philosophy but as science.