JOURNAL OF

THE TRANSACTIONS

OF

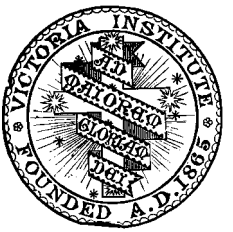
The Victoria Institute,

OR,

Philosophical Society of Great Britain.

EDITED BY THE HONORARY SECRETARY.

VOL. IX.



LONDON:

(Published for the Institute)

HARDWICKE & BOGUE, 192, PICCADILLY, W.

1876.

"Lord Shaftesbury's Glasgow Address."	*From the Author.*
"Commencement of the Second Christian Epoch."	*Ditto*
"Christian Dogmatics." By Van Oostersee.	*Messrs. Hodder.*
"The Miracle Recorded in Joshua." By late Rev. E. Biley.	*Messrs. Hatchard.*
"Newton's Principia."	*J. W. Lea, Esq.*
"Pratt's Mechanical Philosophy."	*Ditto*
"Sir W. Hamilton's Philosophy."	*Ditto*
"Records of the Past," 2 vols.	*Messrs. Bagster.*
"World Scientifically Considered." By C. Thompson.	*From the Author.*

The following paper was then read by the Author :—

ON THE BEARING OF CERTAIN PALÆONTOLOGICAL FACTS UPON THE DARWINIAN THEORY OF THE ORIGIN OF SPECIES, AND ON THE GENERAL DOCTRINE OF EVOLUTION.

By H. Alleyne Nicholson, M.D., D.Sc., M.A., F.R.S.E., Professor of Biology in the Durham University College of Physical Science, Newcastle-on-Tyne.

NO science, probably, will ultimately have more to say in the proof or disproof of the general doctrine of evolution as applied to the kingdoms of organic nature, than Palæontology. I do not, however, in the present communication, propose to discuss at length this wide question. I propose, rather, to take a more limited field, and to examine shortly the bearing of certain portions of palæontological evidence upon the Darwinian theory of the origin of species,—a theory which is only *one* method of explaining *how* evolution may have taken place, and which is totally independent of the general doctrine of evolution. In carrying out the object which I have in view on this occasion, I shall, for the most part, follow Mr. Darwin through his celebrated chapter on "The Imperfection of the Geological Record," in which he fairly states the chief objections which he conceives to be capable of being brought forward out of geology and palæontology against his theory, and in which he endeavours, with much ingenuity, to rebut these objections.

Before entering, however, upon the proper subject of my paper, it may be as well to indicate the general conclusions to which we might be led, as regards this subject, from a study of palæontology or zoology; since there seems, in the minds of some, both of those who are in favour of evolution and of those who are opposed to it, to exist some confusion on this point :—

First, then, we might be led by a study of the facts of the

case to the belief that no form or kind of evolution of living beings ever has taken place, or ever will take place. This conclusion would relieve us from any necessity of discussing what is known as the "Darwinian Theory," since this presupposes evolution, and is directly based upon it.

Secondly, we might be led to believe that evolution had been the general and universal agent in the production of all the different forms of animal and vegetable life, which have existed in past time, or which exist at the present day. This conclusion would still leave us under the necessity of discussing the Darwinian hypothesis, since this might be false, even if the general doctrine of evolution were true.

Thirdly, we might be led to the conclusion that certain forms of animal and vegetable life had been derived from other pre-existent forms, but that certain other forms had not been so derived. Now, I would here observe that there would be nothing unphilosophical in such a conclusion, supposing it were warranted by the facts. If there are facts which would go to prove that certain animals and plants have been derived from certain other animals and plants, we are warranted in adopting a derivative theory of origin for these animals and plants, but we are not warranted in doing more than this. Every naturalist will admit that the cases in which any direct probability of descent can be established, are limited, and comparatively few in number. The want of philosophy, therefore, if there be any, is on the side of those who, taking what at best has but been established as a probability in a certain number of cases, insist that we must manufacture out of this probability a general law to apply to *all* cases. In other words, it is directly asserted, or tacitly assumed, that if we admit that certain forms of animal and vegetable life (whether we choose to call these varieties or species) have been derived from other pre-existent forms, we must further admit that *all* forms of animal and vegetable life have been similarly derived from a single pre-existent form, that in turn, being evolved from inorganic material. I here protest most strongly against this assertion or assumption. It is an absurdity to maintain that evolution is either wholly true or not true at all; that we must either apply the doctrine to everything or to nothing. It is absurd to maintain that the admission that certain animals and plants have been derived from certain other different animals and plants, carries with it, of logical necessity, the further admission that *all* animals and plants have been similarly derived. Suppose we find that, as a general rule, bodies contract when heat is abstracted from them, are we therefore compelled to

admit that *all* bodies act in the same manner under similar circumstances? If we were so compelled, we know that we should be wrong, and that we should ultimately find our law confronted with certain bodies which do *not* contract on cooling. Similarly, we are not compelled by any necessity of the case either to apply the doctrine of evolution to all animals and plants alike, or to deny its existence and operation altogether. On the contrary, we are perfectly at liberty if we choose, and the facts will bear us out, to believe that some sort or kind of evolution has taken place, and that *some* animals and plants have been produced out of other pre-existent forms, whilst others have been differently produced, and owe their peculiarities to some other cause. It is perfectly open to us, to put the case in a concrete form, to believe that certain groups of allied species have been evolved each from a common ancestor; but we may at the same time consistently believe that the origin and production of these ancestral types has been conditioned and controlled by some totally different law. There are plenty of instances, in point of fact, in which one law continues to act regularlywithin certain limits, and then has its operation superseded by some higher law.

In the same way, with regard to the Darwinian hypothesis, it cannot reasonably be maintained, that we are either bound to suppose that all varieties of animal and vegetable life have been produced by the action of natural and sexual selection, or that we are shut up, as our only alternative, to the denial that natural selection is a *vera causa* at all. It is impossible to doubt the operation of "natural selection" within certain limits; but the question remains as to what these limits are; and we are certainly not justified in concluding that because it operates in certain cases, therefore all the peculiarities of the structure of living beings can be explained as due to this, alone or combined with "sexual selection."

Lastly, we have one extremely important consideration to bear in mind, and that is that very different meanings may be attached to the term "evolution." Supposing ground should appear for believing that certain forms of life have been evolved from other different forms, we have to admit the partial operation of "evolution" in its real and strict sense; but it still remains to gauge the quality and significance of this process, as well as to assign the causes by which it was brought about. To some minds, "evolution" appears to convey little or no notion of definite law and order, and the whole process appears to present itself as a kind of chance-medley operation, one species becoming converted into another, not along certain

fixed and unalterable lines, but solely according to chance variations in its environment and surroundings, or in its internal structure. On the other hand, there are other minds to which "evolution," in so far as we may believe it to have occurred at all, presents itself as a perfectly orderly and definitely regulated process, as much a part and parcel of the Divine order, and as thoroughly conformable to it, as any other conceivable mode of creation. On this view, certain types of life have been so endowed as to give rise to certain other related types by "evolution,"—the evolution not taking place, or capable of taking place, in any or every direction, but following a certain definite and necessary line. This is the "genetheonomy" of Mr. Davidson and Professor King, the "evolution of species effected mainly through the operation of Divine laws, and not by purposeless or accidental modifications." For my own part, if we substitute, in the above quotation, the word "wholly" for "mainly," I see no difficulty in accepting evolution as an agent in the production of species. It will be observed that this leaves open the question as to *how far* evolution has thus operated, and also as to *how* its operation has been effected, whether by "natural selection," or in some entirely different manner.

Having now cleared the ground by these preliminary considerations, I shall pass on to discuss the method in which Mr. Darwin has treated the difficulties which palæontology offers to the acceptance of his theory of the evolution of species by natural selection, as expounded in the chapter of the "Origin of Species," entitled "The Imperfection of the Geological Record." And I may here remark, that though I have come to the conclusion that Mr. Darwin has failed to remove these difficulties entirely, or even to materially lessen their weight, he has exhibited conspicuous fairness in the manner in which he has stated them, and that his arguments embrace much of the highest value, quite apart from the special conclusions which may be drawn from them. The subject may be considered under the following heads.

1. *The Nature of Extinct Intermediate Varieties.*—Mr. Darwin commences by pointing out that at the present day, supposing his theory to be true, we should not expect to find any forms *directly* intermediate between two given species, or that, at any rate, the existence of such forms must be very rare and exceptional. What we should look for are "forms intermediate between each species and a common but unknown progenitor." It is clear, however, that as regards extinct species, we have a right to look for such *directly* intermediate

forms, if Mr. Darwin's theory be correct; because in many cases we should have the actual common progenitor and the resulting species. If, for example, we suppose that the two living species the Horse and the Ass are descended by evolution from a common progenitor, it may be, and is doubtless, true that we should find no links directly uniting the one with the other. But, looking into "the dark backward and abysm of time," we may perchance find this common progenitor, and then the element "unknown" is eliminated, and we may reasonably ask for the *directly* intermediate forms which unite *each* species with the now known progenitor. In the present instance, most evolutionists would admit Hipparion to be the required common progenitor. No directly intermediate links, however, have yet been discovered between Hipparion and Equus. Or, if, in order to evade this difficulty, it were supposed that Equus and Hipparion constituted two distinct and diverging lines of descent from a still older common progenitor, such as Anchitherium, it would still remain to find directly intermediate forms between each of these and the latter; and no such transitional links have as yet been discovered. The general view, no doubt, is to regard Anchitherium as being the at present oldest known common progenitor of the Horse and Ass, and to consider that Hipparion is the required directly intermediate form, or rather one of such forms. This view, however, disregards the fact that the requirements of the case necessitate the bringing forward of directly intermediate forms between two existing species and the nearest common progenitor that can be found. If Equus has been developed from Anchitherium, and Hipparion has constituted an intermediate stage between the two, then Hipparion is the nearest common progenitor at present known of the existing species of Equus, and we have the right to expect the production of forms directly intermediate between them. Similarly, we should expect to find forms directly intermediate between Hipparion and Anchitherium. In neither case, however, are any such intermediate links at present known.* It

* The new and remarkable forms of Equidæ discovered by Leidy and Marsh in the Tertiary formations of North America, do not supply the desired links between Hipparion and Equus, or between Hipparion and Anchitherium. Thus Orohippus, though closely related to Anchitherium, has four digits in the manus and no antorbital fossa. Miohippus may be regarded as linking Orohippus to Anchitherium, since it has only three digits to the manus, but it also has no antorbital fossa; whilst Pliohippus, though resembling Equus in its digits, differs in the important characters of possessing a large antorbital fossa and an additional upper præmolar. Hence all these forms, though perhaps indicating the occurrence of some kind of evolution, are so distinct and isolated in their characters that

is clear, that though the earlier life-periods of the earth's history may be for ever hidden from us, the period of which we have actual record is sufficiently long to make it certain that we must have in that period many common progenitors of existing species, or of species which came into existence in the later epochs of Geology. We should, therefore, expect to meet, as palæontologists, with numerous *directly intermediate types;* and the very general absence of such appears to me to be, to begin with, a very serious obstacle in the way of the Darwinian hypothesis.

2. *The Lapse of Geological Time.*—The argument under this head I may pass over without discussion. As a field-geologist, I am fully prepared to admit the vastness of geological time; but I do not see that we have at present any sufficiently definite data by which we can estimate whether this time has, or has not, been sufficient to allow of the production of all living and extinct species of animals and plants by the action of natural selection. Geological time, as asserted by Mr. Darwin, is no doubt commonly underestimated; but we cannot at present even approximately determine how long a period has elapsed since the first introduction of living beings upon the globe, and we have not the smallest means of calculating how long a period would be required for the origin of species on Darwinian principles. It seems futile, therefore, to attempt to draw any conclusion from the comparison of two unknown quantities. Sir William Thomson's conclusions, if proved, would undoubtedly seriously affect the position of the Darwinian theory, but it cannot be said that they are certain, and it seems better at present to regard our knowledge as insufficient for the formation of any definite opinion on this subject.

3. *The Poorness of our Palæontological Collections.*—The next section of Mr. Darwin's argument deals with the poverty of our best palæontological collections, which he evidently regards as so great as to render all negative evidence, founded on the absence of certain forms of life, as of no value when opposed to his theoretical views. Unquestionably if we were to take our entire palæontological collections and compare them with the vast number of animals and plants, which we may infer from various considerations to have existed in past time, but of which we have now no traces, Mr. Darwin is

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they are necessarily regarded as distinct *genera.* Hence, they do not lead us any nearer to the graduated series of transitional forms, which will have to be found before we can positively assert that Equus is a lineal descendant of Hipparion, and the latter of Anchitherium.

justified in the view he has taken, at any rate within certain limits. On the other hand, it may reasonably be maintained that this poverty of our collections is greatly reduced when we take certain groups of animals, or take the entire faunæ of certain formations. It may reasonably be maintained that the known collections, for example, of Silurian and Devonian fossils are not so fragmentary as to vitiate all the negative evidence drawn from them. In North America, at any rate, where the Devonian follows the Upper Silurian conformably and without any palæontological break of a marked kind, and where both sets of rocks are richly fossiliferous, it cannot be said that the poverty of our collections is such that no value can be attached to the absence of intermediate forms between the species of successive formations. If the Brachiopoda of these formations alone be taken, there are many species of which many thousands of perfect specimens have been collected; and if evolution can ever be proved by palæontology, we might fairly expect the proof here. Similarly, our collections of the fossils of various of the Secondary formations, as regards the marine animals, are sufficiently complete to render any negative evidence drawn from them of very decided value. Upon the whole, therefore, whilst the fragmentary nature of our palæontological collections must be fully admitted, it remains certain that as regards the marine faunæ of certain formations, and as regards certain groups of marine animals, this imperfection of our collections is not so great but that we may attach considerable importance to any negative evidence that they may afford.

4. *The Vastness of Unrepresented Time.*—Every modern geologist, probably, admits that the great geological formations are separated by vast lapses of time, more or less completely unrepresented by any accumulation of sediment. It is also universally admitted that all unconformities, whether between two formations, or as occurring in the limits of a single formation, similarly mark intervals of time not represented in the area where the want of conformity occurs by any stratified deposits. Every want of conformity, therefore, undoubtedly marks a time in which great biological changes may have taken place without our having any record of them now preserved to us; and it may be, as believed by some, that the periods unrepresented by any fossiliferous sediments are actually much longer than those of which we have material record in the form of strata charged with the remains of extinct animals. It is certain, therefore, that we have here a very marked cause of the imperfection of the palæontological record;

and the evolutionist may reasonably claim that many of the proofs of his theory have been in this way destroyed.

This argument, however, cannot be sustained with any confidence, when we come to look at the successive and conformable strata of a single group of beds. Doubtless, the lines between successive strata do mark periods of time in which no sediment was being accumulated, but we have no proof that these unrepresented periods were of any very great duration. When we find, as we often do, two successive and closely-related beds in which the fossil remains are partially alike and partially unlike, it is begging the entire question to assert that the line dividing the two beds *must* represent a long period of time *because* of the unlikeness of the organic remains of the two. Until we can indicate with some preciseness the sequence of phenomena indicated by the sudden appearance of new forms of life in time, we have no right to assume that two successive beds are separated by a wide interval, simply because the upper bed contains one or more new and peculiar forms of life.

It may be admitted, then, that, as regards the entire series of stratified deposits, so many gaps exist that the record of life is seriously mutilated; and hence, supposing evolution to be true, many of the proofs of its operation have doubtless never been preserved to us, whilst many others must have been destroyed by denudation. On the other hand, it is to be urged that no such objection can, in the present state of our knowledge, be brought against certain groups of fossiliferous deposits which we may take in certain known and explored regions. No such objection, for example, can be urged against a large portion of the palæozoic rocks of North America. Commencing with the Clinton formation, we may pass from the base of the Upper Silurian to the summit of the Devonian series, through a thickness of some thousands of feet of sediments, without meeting with a single unconformity or with any *general* palæontological break. The entire series admits of subdivision into a number of subordinate groups, each characterized by some peculiar fossils; so that we have a constant extinction of certain older types of life and a constant appearance of certain new forms. The fauna of each subordinate group is, however, constantly found to be closely related to that of the groups immediately above and below, and there is no positive evidence, either stratigraphical or palæontological, of any long interval of unrepresented time separating the successive groups. In other words, so far as all the positive evidence would show, we have here an area which remained

beneath the waters of a single ocean, and that an ocean richly tenanted by living beings, during the whole of the vast lapse of time between the commencement of the Upper Silurian and the close of the Devonian period; whilst there is no proof of any considerable pauses in the process of sedimentation during the same period. Here, therefore, if anywhere, we ought to find proofs of evolution, if such a process really has taken place; and I shall immediately proceed to examine shortly some of the evidence that we have on this head. In the meanwhile it may be noticed that there is another respect in which the Upper Silurian and Devonian rocks of North America are peculiarly fitted to throw light upon this inquiry. Mr. Darwin has pointed out that richly fossiliferous deposits have been formed mainly during periods in which the sea-bottom was undergoing subsidence; but he remarks that during subsidence few new varieties or species will be formed, owing to the deepening of the sea and the consequent decrease in the inhabited area and the number of inhabitants. The bearing of this remark upon Mr. Darwin's views is obvious; since sediments accumulated during subsidence, when few new species are formed, could not be expected to yield many, or any, intermediate forms. Under any circumstances, I should not attach as much weight to the latter half of the above observation as Mr. Darwin seems inclined to do; but, at any rate, it does not apply to the case I have chosen. There is good evidence that the Upper Silurian and Devonian rocks of North America were laid down in an area of almost continued subsidence; but there is also good ground for believing that the accumulation of sediment kept pace, approximately, with the rate of subsidence; so that the depth of the sea remained tolerably constant, and there was no marked decrease in the size of the inhabited area and the number of inhabitants. We have also evidence that during the greater part of this period the sea was sufficiently shallow to admit of the existence of a profuse and varied marine fauna; and there is ample proof of the continual introduction of new species and varieties.

5. *The Absence of Numerous Intermediate Varieties in any Single Formation.*—It is freely conceded that one of the greatest difficulties which Mr. Darwin's theory has to overcome, is found in the fact that we do not find in the limits of any single formation "closely graduated varieties between the allied species which lived at its commencement and at its close." The essence of this difficulty lies in the words "closely-graduated"; for we do find in any single formation certain intermediate forms, which may perhaps support a partial theory of evolution,

but which do not offer the evidence required by the Darwinian hypothesis. The following are the chief considerations brought forward by Mr. Darwin, to break the force of this objection, though he admits his inability to assign a due proportional weight to each.

a. It is maintained that each formation is probably "short, compared with the period requisite to change one species into another." Seeing that each formation is characterized by an assemblage of living beings peculiar to itself, that few species pass through an entire formation, and that each subdivision of a formation is generally recognizable by its own peculiar forms of life, I do not see how it is possible to maintain this assertion. It may be noted, also, that though the amount of time, as having elapsed since the introduction of life upon this planet, demanded by the Darwinian theory is notoriously enormous, one has little idea of its immensity till one comes to analyze such an argument as that given above. It is admitted that the length of time indicated by our entire series of stratified rocks, is vast almost beyond conception; but the entire series consists of only fourteen or fifteen great formations, and would, therefore, irrespective of the blanks between the formations, correspond, on the above view, with less than the combined life of fifteen successive species. When we reflect on the enormous number of living forms that have died out, and the enormous number of new forms that have come into being, we feel hopeless of forming even an approximate conception of the time which Mr. Darwin asks for the carrying out of his theory.

b. It is alleged, again, that the first appearance of a species in any formation, probably only indicates that it had then first immigrated into that area, and that it might have been in existence elsewhere for a long period of time. This may in some, perhaps in many, instances be true; but there can be very few cases capable of definite proof, and it must, therefore, be regarded as more or less of the nature of an assumption. It can hardly be asserted that in the long lapse of geological time we have not record of the first appearance of *many* species; and we can never know, in most instances, whether the first appearance of a species, as known to us, is actually its first appearance, or is only so for the area under examination. Little weight, therefore, can be attached to this argument.

c. In order to get a *perfect* gradation between two forms, we should require them to have lived in the same area for a long period, during which a thick and continuous series of

deposits were laid down; but these conditions are probably rarely carried out. This is unquestionably true, if we only knew *how* thick the formation would need to be. This we do not know, and therefore it will always be open for each observer to hold his own opinion on this point. Some will be of opinion that the uninterrupted deposition of fifty or a hundred feet of sediment would amply fulfil the above conditions. Mr. Darwin, on the other hand, believes that a whole formation would not be sufficient for this purpose; and there does not appear at present to be any means of coming to an agreement on this point.

d. That every formation has been more or less intermittent in its accumulation is unquestionably true, since the dividing-line between every stratum and the next undeniably marks a pause in the work of deposition. We have, however, no proof that these pauses have been always of even approximately the same length. Sometimes we have reason to believe that they have been very long; at other times there are grounds for thinking that they were comparatively very short. We can, therefore, come to no positive conclusion, as to the amount of time represented in this way, and can thus attach no definite value to any argument derived from this source.

e. The last of Mr. Darwin's arguments which I may notice is that we have no right to look in our geological formations for "an infinite number of those fine transitional forms, which, on our theory, have connected all the past and present species of the same group into one long and branching chain of life." On the contrary, we have only a right to look for a few of these transitional links, and such are actually found to exist in nature. To this it may be replied that whilst we have assuredly no right to ask for an *infinite* series of links, we have a right to ask for a much more perfect series of links than has as yet been brought to light. The transitional forms which are at present known to us,—and there are more of them than might be imagined,—might be sufficient to give an *à priori* probability to some theory of evolution; but they can hardly be said to be in any single instance sufficient to be accepted as proof of the special explanation of evolution advocated by Mr. Darwin.

6. *On the Succession of Life in a Series of Conformable Deposits.*—We have seen that Mr. Darwin admits that the absence of a series of graduated intermediate forms between the species at the commencement of any single formation and those which lived at its close, is a great stumbling-block in the way of his theory. Let us now see what we actually do find in such a case, having in the meanwhile regard wholly to the facts, and

disregarding all theories and all possible explanations of anything which may appear unintelligible. For reasons already stated; I shall select for this inquiry the Upper Silurian and Devonian rocks of North America as being peculiarly fitted for this purpose. We have here a series of distinct rock-groups, all of which are capable of being defined by their fossils, but which follow one another conformably, and which possess a sufficiency of identical or closely-allied fossils, in any two successive groups, to indicate that they constitute a single natural group of deposits, elsewhere represented by the Upper Silurian and Devonian. When most fully developed, the series consists of the following groups in ascending order:—

I. Upper Silurian.
1. Oneida Conglomerate.
2. Medina Sandstone.
3. Clinton group.
} Middle Silurian.
4. Niagara group.
5. Guelph Limestones and Onondaga Salt group.
6. Lower Helderberg.
a. Tentaculite Limestone and Water-lime group.
b. Lower Pentamerus Limestone.
c. Delthyris Shaly Limestone.
d. Encrinal Limestone.
e. Upper Pentamerus Limestone.

II. Devonian.
7. Oriskany Sandstone.
8. Corniferous group.
a. Cauda-galli grit.
b. Schoharie grit.
c. Upper Helderberg or Corniferous Limestone.
9. Hamilton group.
a. Marcellus Shale.
b. Hamilton group proper.
c. Genesee Slates.
10. Portage group.
11. Chemung group.
12. Catskill group (Carboniferous?)

The line of division between the Upper Silurian and Devonian is so little marked that the best authorities are still divided as to whether the Oriskany Sandstone should properly be regarded as the summit of the former or the base of the latter; and it may conveniently be regarded as constituting a bed of passage between the two. In what follows, several groups of the above list will not come into consideration at all, as not yielding

many organic remains, or such as can be made available for the present purpose.

For many reasons it is desirable to restrict our investigation as regards the succession of life in the above-mentioned deposits to a single group of organisms, and for this purpose none offers such facilities as that of the Brachiopoda. It will not be possible, indeed, to study even these in an exhaustive manner, and only the more striking facts brought to light by a consideration of their occurrence in these formations can here be discussed.

In the first place, it is most remarkable to observe how in the larger and most abundantly represented genera closely allied forms succeed each other as we proceed from the base of the series towards the summit. Commencing with a single type-form in one of the lower groups, we find the same form under a somewhat different guise appearing in one or more of the higher groups, and sometimes represented therein by several allied species. I shall give some of the more conspicuous examples of this, drawn from a study of the genera Orthis, Strophomena, and Spirifera.

If we commence, for example, with Orthis elegantula, Dalm., in the Clinton group, we have a well-known type nearly allied to certain Lower Silurian forms (such as O. testudinaria, Dalm.), and distinguished by its flattened dorsal and convex ventral valve, and by the fine radiating dichotomising striæ with which the surface is ornamented. In the Niagara group the species is continued in full force, and little or not at all changed; but in the Lower Helderberg the species has disappeared, and its place is taken by the closely allied Orthis plano-convexa, Hall, and O. subcarinata, Hall. In the Oriskany Sandstone no representative of the type has yet been detected, but in the Corniferous group we find Orthis peloris, Hall, and O. lenticularis, Hall, whilst the Hamilton group has yielded O. solitaria, Hall; all of these being close allies of one another, and of O. elegantula.

Another series may be taken, having as its type Orthis hybrida, Sow. This type commences in the Clinton group in the person of O. circulus, Hall, and is represented in the succeeding formation of the Niagara group by the type-form O. hybrida, distinguished by its nearly equally convex valves and fine radiating striæ. In the Lower Helderberg the type has a great development, being represented by O. oblata, Hall, O. discus, Hall, O. eminens, Hall, O. perelegans, Hall, O. concinna, Hall, and O. assimilis, Hall; all of these being closely related to O. hybrida and to one another. In the Oriskany

Sandstone we have but a single example of the group, viz. O. musculosa, Hall. In the Corniferous Limestone, however, a second great expansion of the type occurs, and we find no less than six species of the group, viz. O. alsus, Hall, O. mitis, Hall, O. Livia, Billings, O. Vanuxemi, Hall, O. Semele, Hall, and O. Cleobis, Hall; some of these being hardly separable from one another and from precedent forms. Lastly, in the Hamilton group, besides O. Vanuxemi, continued from the Corniferous, we have four fresh representatives of the type, viz. O. leucosia, Hall, O. cyclas, Hall, O. Penelope, Hall, and O. idoneus, Hall.

Or, again, we may take another series, which culminates in the well-known Orthis resupinata of the Carboniferous rocks. This series commences in a well-marked form with Orthis multistriata, Hall, of the Lower Helderberg: it is represented in the Corniferous Limestone by the very similar O. propinqua, Hall (so similar as to be almost undistinguishable); it is continued in the Hamilton group by O. Tulliensis, Vanuxem, and O. Iowensis, Hall; whilst it is represented in the Portage and Chemung groups by O. impressa, Hall.

Turning to the genus Strophomena, we find exactly the same phenomena. Thus, the large and important group of Strophomenoid shells typified in the Lower Silurian by S. alternata, Conrad, and a number of allied forms, continues to be represented in the Clinton by S. alternata, and, though without any conspicuous example in the Niagara group, is represented in the Lower Helderberg by the two well-marked forms, S. concava, Hall, and S. varistriata, Conrad. Later on, in the Corniferous and Hamilton groups, we find the type represented by a whole group of forms—S. inequistriata, Conrad, S. inequiradiata, Hall, S. Patersoni, Hall, S. textilis, Hall, and S. hemispherica, Hall; which Hall considers as distinct species, but which Mr. Billings regards as probably nothing more than varieties of one protean form, which is continued into the Chemung group by S. Cayuta, Hall.

Similarly, the S. Headleyana, Hall, S. punctulifera, Conrad, S. Leavenworthana, Hall, and S. cavumbona, Hall, all from the Lower Helderberg group, are hardly or not at all separable from the S. ampla of the Corniferous Limestone, a species which is also stated by Mr. Billings to occur in the intermediate formation of the Oriskany Sandstone.

Again, the Strophomena patenta, Hall, of the Clinton group, related perhaps in turn to S. pecten, Linn., of the older rocks, is represented in the Niagara formation by the nearly allied S. subplana, Conrad, which is followed in the Lower Helder-

berg by the closely allied S. radiata, Vanuxem, and S. Woolworthana, Hall.

Evidence of precisely similar import can be obtained from a consideration of the distribution of the species of the genus Spirifera within the same formations. The Spirifera crispa, Linn., of the Niagara formation, is succeeded in the Lower Helderberg by the closely allied, if not identical, S. cycloptera and S. Vanuxemi, Hall, which are followed in the Oriskany by S. tribulis, Hall, and in the Corniferous by S. duodenaria, Hall. Again, the Spirifera sulcata of the Niagara group is succeeded by the cognate S. perlamellosa, Hall, of the Lower Helderberg, in turn followed in the Corniferous group by S. raricosta, Conrad. Lastly, the Spirifera Niagarensis, Conrad, which, so far as I am aware, is the oldest example of the genus in the Silurian rocks of North America, is directly succeeded in the Lower Helderberg by the closely related S. macropleura, Conrad.

The question now arises—What is the significance of facts such as these,—facts which could be greatly multiplied, and which no competent authority would think of disputing? Are we to consider that the eighteen forms which group themselves round Orthis hybrida as a central type, and which are found in the successive formations from the Clinton to the Hamilton, are so many absolutely distinct species, in the old and strict acceptation of this term? Or, shall we simply expand our conceptions of what constitutes a species, extend the limits of the term, and consider that these allied forms are so many more or less distinct varieties of a single protean species? If the latter view were adopted, whilst to the working palæontologist these forms would remain as so many distinct species, and would properly and usefully be designated by so many distinct names, to the transcendental palæontologist they would become simply so many successive phases of one variable form.

It cannot be too strongly borne in mind, as very properly insisted on by Mr. Darwin, that naturalists have no golden rule for determining what are species and what are varieties. Such determinations rest upon the value which certain observers attach to certain characters; and this is especially true of fossils, where, in addition to the actual anatomical or structural characters, we have the additional element of *time* introduced. Specimens which would at once be admitted to be mere varieties if they occurred in a single stratum, are by many palæontologists unhesitatingly set down as distinct species, if they happen to occur in beds of even slightly different age. For the purposes of the stratigraphical geologist, this does not matter, and is, indeed, often useful, since if a certain bed can be

invariably recognized by the occurrence in it of a particular fossil, it does not matter whether this be a true species or a mere variety. Philosophically, however, the system is a bad one, and the specific status of a fossil should be determined independently of its stratigraphical position.

Upon the whole, the most feasible explanation of cases such as those above recounted, seems to be contained in the admission that certain species in certain zoological groups have a great range of variability; that these variable species appear under slightly different forms in each successive period or subperiod; and that, though we may, for the sake of convenient classification and description, call these by distinct titles, they have in reality all been derived from a single primitive type by some kind of evolution. As before remarked, however, this is simply giving a more ample latitude and a broader expansion to our conception of the term "species," and it does not carry with it philosophically the acceptance of the *general* doctrine of evolution. *A fortiori* it does not bind us to accept any particular explanation as to the manner in which these related forms have been produced.

So far as the Darwinian theory is concerned, the facts above recounted do not appear to be in any way specially favourable to it; since, though they apparently point to some kind of evolution having taken place upon a limited scale, they do not show any indications of the graduated series of intermediate forms which is required by the hypothesis of natural selection, and which upon this view must at one time necessarily have existed. If Spirifera crispa of the Niagara group, for example, was changed by natural selection into S. cycloptera of the Lower Helderberg, this into S. tribulis of the Oriskany, and this into S. duodenaria of the Corniferous, we ought to find a graduated series of intermediate forms directly connecting them; since no ground can be alleged why each of the intermediate forms of the series should not have had just as long an existence as the four types themselves, and should not, therefore, have had just as good a chance of being preserved as fossils. Nor do the arguments brought forward by Mr. Darwin appear to touch this case, since all the forms in question inhabited a single ocean, the bottom of which was regularly and slowly subsiding, and in which a series of sediments was being accumulated, so far as we can judge, almost continuously.

It appears, then, that even in cases such as the preceding, which at first sight appear to favour the Darwinian theory, we do not find the graduated series of intermediate forms required to prove the case. On the contrary, we find a series of forms

very closely allied to one another, the two extreme terms of the series being conspicuously different, and the intermediate forms more or less completely uniting them together; but, at the same time, all the members of the series so far distinct that a thoroughly competent and skilled palæontologist describes all of them, without hesitation, as distinct and separate species. This is not at all what is required for the proof of the Darwinian hypothesis, and Mr. Darwin is so fully alive to this that, as we have seen, he has devoted much ingenuity to an attempt to explain away the absence of the finely gradational forms, which upon his theory ought to be found within the limits of each great formation.

So far, therefore, as any actual proof of the Darwinian theory of the origin of species is concerned, I believe Palæontology to be at present absolutely silent. The facts of Palæontology point to the operation of some law of evolution, but they do not support the special views advanced by Mr. Darwin. Everywhere we meet with intermediate forms linking together different groups; but these forms are always distinct in themselves and distinct from the types they connect. When we look at the "intercalary" or "linear" types interposed between the great classes of the Reptiles and Birds, for example, Compsognathus, Ichthyornis, Odontopteryx, Archæopteryx, Pterodactylus, and the like, we have a series of distinct structural types, which may as a whole be placed between Reptilia and Aves, but which are quite distinct in themselves, and which are not connected either with one another, or with these two classes by any graduated series of transitional forms. Similarly, Hipparion may be a "linear type" between Anchitherium and Equus; and in so far as this is probable, it lends support to some theory of evolution; but it does not support the Darwinian theory, as we have discovered no intermediate forms uniting these very distinct types. The same may be said of all, or almost all, of the known "transitional forms," which have as yet been brought to light by the study of Palæontology.

In the particular department which we have been investigating, we have seen that great variability exists in certain groups, and that a reasonable probability has been established that certain related groups of Brachiopods have descended each from a single primordial type. In other words, we have seen it to be reasonably probable that certain species are endowed with such a plastic organization, that when the surrounding conditions change, or in consequence of some unknown and inherent law, they undergo modification, and appear in successive periods under forms so different, as to have been described

as distinct species. We have thus ground for believing that a considerable expansion may reasonably be given to the philosophical conception of what constitutes a species.

There is, however, another aspect of the question to consider. Side by side with the groups of allied species of Brachiopods, which we have seen to occur in the Upper Silurian and Devonian formations, we must place the comparatively rigid, inflexible, and persistent species, such as Strophomena rhomboidalis, Wahl., and Atrypa reticularis, Linn., of the same formations; and we have also to consider the new types which came into being during the same period, without our being able to show that they have descended from pre-existent allied forms. As regards the persistent types, the two first-mentioned are the most important. Strophomena rhomboidalis commences in the Lower Silurian, and continues through the whole of the Upper Silurian and Devonian, finally dying out in the Carboniferous rocks. Though slightly varying in shape and size in certain deposits, it remains practically the same throughout the whole of this enormous period. Atrypa reticularis, Linn., commences in the earlier portion of the Upper Silurian, and continues to be represented till close upon the termination of the Devonian period. Unlike the preceding, it is exceedingly variable in size and in other characters, and at least two so-called species, viz., Atrypa impressa, Hall, and A. spinosa vel aspera, appear to have been founded upon mere varieties of this mutable form. Other species could be mentioned which pass through several sub-groups without apparent alteration; but these two have by far the largest range. Atrypa reticularis affords us an instance of a species, which, though very variable, and constantly presenting slight modifications of different kinds, nevertheless retains its specific stamp for a very extended period, and under what must have been very variable conditions. On the other hand, we have in Strophomena rhomboidalis a specific type which endured similarly changing conditions. and which survived for an even more prolonged term, but which throughout its entire lease of life never exhibited any modifications of even varietal value.

As regards the appearance of new forms of the Brachiopoda during the period which we have had under consideration, it will be advisable to look to the genera and sub-genera rather than to the species. As each new genus and sub-genus in general contains more than one, and often many, species, the case is thus rendered quite sufficiently strong for our purpose; though it is to be remembered that many new species of the old genera are also constantly making their appearance in the

successive formations. It should also be added that what follows relates only to the North American area, and that some of the types which here appear for the first time in certain deposits, have elsewhere existed at an earlier period. It should further be said that all palæontologists would not recognize the validity of some of the generic or sub-generic forms here enumerated, whilst others of doubtful value have been omitted altogether.

In the Clinton and Niagara formations, which in this connection may be considered together, we find representatives of the following genera:—Lingula, Pholidops, Discina, Orthis, Orthisina, Nucleospira, Leptæna, Strophomena, Streptorhynchus, Chonetes, Spirifera, Atrypa, Athyris, Cyrtina, Rhynchonella, Pentamerus, Stricklandinia, Leptocœlia, Camerella, Zygospira, and Trematospira. Most of these are more or less largely represented in the Lower Silurian rocks; but Chonetes, Spirifera, Cyrtina, Pentamerus, Stricklandinia, Leptocœlia, Trematospira, and Pholidops represent types which appear now to have first made their appearance in this area. In Ohio, the genus Trimerella also makes its first appearance at this period. In the Lower Helderberg epoch, though Brachiopods are very abundant, and many new specific forms come into existence, it is chiefly the already existing genera that are represented, and the only new types that appear are Eatonia and Rensselaeria. In the Devonian rocks, on the other hand, not only are many of the older types largely represented, but we have a large number of new types coming into existence, and many of these have a very striking development during the period. To say nothing of older types, like Chonetes, which are now for the first time plentifully represented, the following new types now make their appearance:—Ambocœlia, Amphigenia, Camarophoria (?), Centronella, Crania, Cryptonella, Spirigera, Leiorhynchus, Tropidoleptus, Vitulina, Terebratula, Pentamerella, and Productella. Of these Terebratula and Productella are of especial importance as being the forerunners of two very important groups of the class.

We see from the above that though the Upper Silurian and Devonian rocks of North America were laid down as an approximately continuous series, and certainly on an ocean-floor which was not once laid dry during their accumulation; nevertheless, new forms of life were constantly being introduced into the area in some manner that cannot be explained; and in many cases the new forms belong to altogether new generic types, which have no near allies in the older strata. This fact, which is, of course, one not confined to the particular

case under illustration, is undoubtedly a serious difficulty in the way of the acceptance not only of the Darwinian theory, but of any doctrine of evolution. Any such doctrine, if it is to be applied *universally*, must stand by Mr. Wallace's law, that "every species has come into existence, coincident, both in space and time, with a pre-existing closely-allied species." This statement of the case, as I have elsewhere pointed out, is obviously too wide, since, even from the evolutionist's point of view, we must *somewhere* come to a point where the organisms (or organism) in existence had no pre-existent types. It is certain, however, that no doctrine of general evolution can afford to admit the sudden appearance of new specific or generic types in time. From all that palæontology teaches us, on the surface at any rate, such new types *have* constantly been coming into existence in past time, as we have just seen; and it is not easy to discover any satisfactory explanation of this troublesome fact. The most obvious way of evading the difficulty, and the one which Mr. Darwin has adopted, is to assert that what appears to us to be the first appearance of new generic or specific types is only due to the imperfect state of our knowledge, and that the said types were really in existence long before the period of the formation in which we first find them. In such cases as concern the first appearance of given types in given areas, and in which it can be shown that similar or nearly allied types have existed in other areas in *older* formations, there is a strong probability that this explanation is correct, and that what we call "first appearance" is merely an instance of "migration." When this assertion, however, is made as a *general* statement, applying to the *general* phenomenon of the sudden appearance of new specific and generic types throughout the entire series of the stratified rocks, then two things are clear.—Firstly, that such an assertion is *only* an assertion, which, even if probable, would ever remain unprovable; and secondly, that such an assertion is in the highest degree improbable, though its falsity likewise does not admit of positive proof. That in many cases, the points where we now note the first appearance of generic and specific types in the geological record, are not the actual points at which they were first introduced upon the scene, either as regards time or space, is likely enough. But, that this is true of *all* the new species and genera that have made their appearance upon the earth since the commencement of the Cambrian epoch, is not only an assumption, but it is one that can only be sustained by making other assumptions equally unsupported by definite proofs. And it may be noted here, that to derive any benefit from this argument, it is neces-

sary to suppose that we are ignorant of the first appearance of *all* those specific and generic types which make their appearance suddenly in the stratified series. In other words we must suppose that fully three-fourths of all the known species of fossils had been in existence an indefinite period before their first appearance in the rocks as known to us. I apprehend that every evolutionist will admit this, since the cases in which it can actually be shown that a fossil species came into existence "coincident, both in space and time, with a pre-existing closely-allied species," are, on the most liberal estimate, not more than one-fourth of the total number of those with which we are already acquainted. All the other species, of which this cannot be shown, must, in accordance with the above *dictum*, have been in existence prior to the period where they now first appear upon the scene.

The types of species and genera, to say nothing of those of families and orders, which make their appearance in the Cambrian period, are so numerous that we are compelled by this argument to assume that they themselves must have been in existence for an indefinitely long period before the commencement of the Cambrian; whilst the types from which they were derived must have flourished in ages so immeasurably earlier that the very imagination is left powerless. Indubitably, there is every reason to believe that the great pile of Laurentian sediments was once fossiliferous, and that the Laurentian period was anything but "azoic." Upon strict Darwinian principles, however, the Laurentian period, long as it must have been, is altogether inadequate for the development of all the forms of life which make their first nominal appearance in the Cambrian. We are, therefore, compelled to assume the former existence of vast Pre-Laurentian deposits, the memorials of an ancient period rich in life, which must have been destroyed by subsequent denudation. No one dare assert that such deposits *may* not have existed; but as we have absolutely no proof of such a thing, their character and contents can hardly be brought forward as factors in a scientific argument. Mr. Darwin, therefore, candidly admits that "the case at present must remain inexplicable."

In the case which we have been considering, the argument employed by Mr. Darwin, though not demanding such extensive hypotheses, is equally incapable of proof, and must, in my opinion, be equally rejected. We find, for example, in the Devonian rocks of North America, amongst many others, the entirely new Brachiopodous type, Productella, represented by twenty-one known species, all, of course, equally new. Upon

the above-mentioned argument we have to assume that this is *not* the first appearance of Productella, but that the genus, or sub-genus, had already been in existence elsewhere for an unknown but certainly long period, and had only at this time migrated into the American area. If this hypothesis were to be accepted, it would doubtless remove, at any rate, some of the difficulties of the case, but it would not remove all, and there is neither proof nor probability in its favour. If Productella had been in existence elsewhere in Pre-Devonian times, it is almost, if not quite, inconceivable that no remains of the genus should ever have been found in the Silurian deposits of other areas,—deposits which have a very wide extension in space, which are enormously rich in Brachiopoda, and which have been most diligently searched and examined for fossils. Even supposing that at some future time Productella *should* be found to have existed during the Silurian period, the difficulty by this would only be removed a step further back. We should still have to believe that *this* was not really the first appearance of the genus, and we should still have to inquire why no remains of the genus had been disentombed from the Cambrian deposits.

When I consider the vast number of cases precisely similar in all essential respects to the above, and when I reflect on the great extent of uncertain and unexplorable ground traversed by the above hypothetical explanation of the facts, I feel compelled to reject this argument altogether, so far as its general application is concerned. The continued introduction of fresh types of life, which we know to have gone on since the first appearance of organized beings upon the globe, still remains, in my opinion, unexplained. It may be that when we know the law under which it has occurred, we shall find that it has been in accordance with the Darwinian theory of the origin of species. In the meanwhile there is nothing to lead us to suppose that this will be the case, and it appears to me to constitute one of the greatest difficulties which this and other kindred theories have to surmount, before they can place themselves upon a thoroughly satisfactory basis.

7. *General Conclusions.*—As the result of the inquiry in which we have been engaged,—an inquiry necessarily extremely limited in its range and scope,—the following conclusions may be drawn with more or less confidence. And it may be added, that though I have only here treated of a single comparatively small group of rocks, I know nothing in the entire range of palæontology which would at present confirm with any certainty *more* than is contained in these conclusions, so far as these are

of a positive nature. In so far as they are negative, there is doubtless room for much divergence of opinion :—

1. The common phenomenon of closely-allied forms directly succeeding one another in time renders it a reasonable supposition that in certain zoological groups many forms so distinct as to have been described by competent observers as distinct *species*, may have descended from a single primitive ancestral type.

2. The evidence at present in our hands is opposed to the view that this production of groups of allied forms from as many primitive types has been effected solely or mainly by "natural selection"; though it is probable that this agency may have played a subordinate part in the process.

3. New types of life are constantly making their appearance, without, so far as we know, being preceded by any closely-allied types; and we have, therefore, no positive ground for believing that the origin of such types is due to evolution from pre-existent forms.

4. Variability—even in the most variable groups—has never been shown to be indefinite; but, on the contrary, appears to be confined within certain fixed limits for each species; in some cases wide, in others very narrow. Palæontology shows no instances in which we can positively assert that the variability has been unlimited; and though we meet with types connected by intermediate links, we have also to account for the existence of a vast number of isolated forms, which, so far as our present knowledges goes, stand alone, and are not intimately related to other forms.

5. Even where we find types which may be regarded as strictly transitional or intermediate (as Hipparion in its relation to Anchitherium on the one hand, and Equus on the other hand), we nevertheless are confronted with forms which are in themselves quite distinct, and which could not be confounded with the forms which they serve to connect.

6. We cannot fairly have recourse to the "imperfection of the record," as satisfactorily explaining the absence of the numerous intermediate types required by the Darwinian theory. Such imperfection admittedly exists, and is in some instances almost hopelessly great. On the other hand, we have had in other instances a fairly *complete* series of successive forms preserved to us. This is the case with the Brachiopoda and Cephalopoda, for example, and it is by these and similarly well-preserved groups that any theory of the origin of species will have to be tested.

7. The examination of such tolerably complete groups affords

support to the belief that evolution has operated within certain limits, and has been one of the causes which has led to the production of new forms. Even in the best-preserved groups, however, we meet constantly with isolated types, and we are incessantly met with the sudden appearance of new types. An excellent example of this is to be found in the sudden appearance of new species of Ammonites in the Liassic rocks, and their very definite range and complete limitation to known zones. The study of such groups would, therefore, lead us to reject any exclusive doctrine of evolution.

8. Whilst certain types of life exhibit a striking variability, others exhibit an equally striking persistence and immobility. This would go far to prove that *changes in external conditions have little to do with the origin of variations;* since some forms appear to vary even under approximately constant conditions, whilst others remain unchanged even when submitted to the most varying surroundings.

9. In some instances, it can even be shown that entire groups of species have existed without change through periods which we may justly estimate as exceedingly long. Thus, Principal Dawson affirms that of more than two hundred species of fossils, chiefly Mollusca, from the Post-Pliocene deposits of Canada, no one form can be shown to have varied materially, during the long period which separates the oldest boulder-clay from the present time, and in spite of notable climatal and geographical changes.

10. Upon the whole, we may conclude that palæontology, in its present stage of development, offers no strong support, or is directly opposed, to the special theory of the Origin of Species advocated by Mr. Darwin. On the other hand, many known palæontological facts would lead us to infer that, in certain cases and within certain limits, new forms have been produced by the modification of pre-existent types. Palæontology, therefore, would appear to support, at any rate, a partial doctrine of evolution.

11. It remains for future consideration, whether evolution—in so far as it has operated at all—has not been effected by means of inherent tendencies impressed upon living beings by the Creator. On this view, evolution is not a mere disorderly and fortuitous process, by which a given animal or plant is produced out of a different one by the operation of chance and accidental surroundings; but it becomes an orderly process, by which *certain forms of life have from the beginning been impressed with the inherent power of developing in certain fixed*

directions, and thus of giving rise to a definite series of specific types.

12. It further remains for future consideration, whether this orderly process of evolution has always been effected in a *gradual* manner, and whether it has not been occasionally effected by changes taking place *suddenly* and *per saltum*.

13. Finally, it remains to consider within what limits evolution has operated, and what supplementary causes may be found to have acted in the production of new forms of life. Or, rather, it remains to consider whether evolution is a main, or only a subsidiary agency in the production of new species.

The CHAIRMAN.—I am sure the meeting will pass a cordial vote of thanks to Dr. Nicholson for his paper. It is now open for any of those present to make observations thereon.

Rev. G. HENSLOW.—I think we must all feel greatly indebted to Dr. Nicholson for this paper: in it he has distinctly pointed out a matter upon which I have reason to think that there is some confusion in the public mind. I mean the distinction between *Darwinism* and *Evolution;* the former, involving as it does the theory of natural selection, I do not hold; but there is a great difference between natural selection and evolution. Again and again have I stood up on behalf of evolution, but I have always felt that natural selection, pure and simple, would never be sufficient to account for it. I do not know, however, that I agree entirely with all Professor Nicholson's views. For instance, with reference to the poverty of our Palæontological collections; in my opinion, a strong point should be made in regard to the evidence that is wanting. Mr. Darwin speaks of the paucity of the geological record; but there is one thing that ought not to be forgotten:—When we examine certain strata and calculate their thickness, we get something obvious before us, but we are apt to forget, at the same time, that every one of those strata is just as much a measure of what is *lost*, as it is of what we have before us. When we consider the Laurentian strata, the question arises, where did they come from? If they are so many thousands of feet thick, there must have been so many other feet of thickness of primitive strata, about which we know nothing at all, and those primitive strata might have been full of life. For instance, take the sand upon the sea-shores of the south-east of England, where the chalk strata are to be found: the sand, of course, is formed from the wear and tear of the chalk-flints, which are derived from the denuded cliff; but if you take the sand of the sea-shore of Scarborough, this is not the first time it really has been sand: the sandy beach results from the denudation of the fresh-water strata which form the rocks round the coast; so that the same sand must have been used at least twice, if not many times over. Every stratum is

the measure, possibly, of several lost strata; and we do not know how many such there may have been. I was rather puzzled to understand one particular reference to the coming in of new forms; Dr. Nicholson said we seemed to be certain that sometimes we arrived at the first beginning of a new form: this idea rests solely upon negative evidence, unless he refers to some of the graduated forms; as, for instance, to one of those species of *Orthis* or *Spirifera* to which the paper refers: but when we suddenly come to a new species or genus, we have no ground whatever for assuming that it is the first, and the only explanation (unless we fancy it was created then and there, which we should hardly do) is that it must have *migrated*. I think the negative evidence is all in favour of migration, wherever we come across a permanent type for the first time; but so long as it is one of a graduated series, I think we might be justified in saying that it is probably its first commencement. With reference to the horse group of which Professor Nicholson spoke, in which there are not fine intermediate links, it must be borne in mind that evolutionists generally do not necessarily require such fine links, though Mr. Darwin's theory of natural selection does. Mr. Darwin requires extremely small variations, but the question really turns upon this:—how much difference is really required between one form and another? Mr. Darwin requires a succession of slight differences, and palæontology does not always give them: but may it not be true that some of the higher types of life are formed by "sports"—by slight leaps, as it were, instead of by minute gradations? I should like to ask Dr. Nicholson, as being a better palæontologist than I am myself, whether it may not turn out to be a law applying to the higher types, that the distances between them are rather greater than is the case in the lower; the *horizontal* modifications, for instance, being more numerous and more varied, in comparison with the *vertical* modifications. Take the Foraminifera among animals, or the agarics among vegetables, and there you have simple organisms, but there is an enormous amount of variety amongst them—perhaps hundreds of thousands: they are low types, on a common level, as it were, varying to a very great extent; and I would ask, would not Dr. Nicholson's experience support the idea that the lower the type, the greater the amount of horizontal modification; but that when you get to the higher forms the modifications come by jumps and leaps? I should like to know whether that idea has been found to be the case? It certainly would clear up the difficulty that Dr. Nicholson has pointed out;—that in the higher groups especially there are these breaks, and that you do not get a graduated series such as you find among the lower types.

Mr. J. E. Howard, F.R.S.—I think we are all indebted to Dr. Nicholson for this very able exposition of views, in which, for my own part, I entirely agree. So far as my knowledge extends, there is certainly a law of variability which prevails among some species very much more than among others, and which I have sometimes compared to the swing of a pendulum. If we

could suppose our lives limited to one swing of a pendulum, and ourselves occupied in observing the motion of the pendulum, we might naturally come to the conclusion that the law which carried the pendulum in that direction would carry it throughout the remainder of the circle, through our not being acquainted with another fact, namely, that when it had reached its full swing it would stop its forward motion and return. We find that some species are variable exactly in the way Dr. Nicholson has shown, and these variations are in some instances so great and so considerable, that we might suppose they would be carried on to the formation of a new species. We might come to that conclusion, but then, on further examination, we find that there is a retrocession, a counteracting law,—something which prevents that law of variability from going beyond a certain limit; as in the case (for instance) of pigeons and dogs, which, though they may be greatly varied in breed and kind, always remain pigeons or dogs. What Dr. Nicholson said about the Lingula recalled a circumstance to my mind. I was speaking to the captain of a Welsh slate-quarry about the underlying rocks, very low down in the Silurian measures, when he said, "These are what they call the Lingula rocks"; and he asked, "What is that word 'Lingula'"? I gave him my explanation of what the Lingula was, when he replied, "These cannot then be Lingula rocks, because they are azoic." I mention this to show how such matters are caught up by intelligent men, where you would scarcely expect that they would enter so fully into such questions. The Lingula, then, is one of the most remarkable instances of an unchangeable organization in which the law of variability seems to have no place, because the immense period of time which must have elapsed between the deposition of those rocks in which the Lingula occurs, coupled with the fact of the Lingula being unchanged down to the present time, certainly seems to be extremely inconsistent with any notion of the evolution of species such as is required by the system of Mr. Darwin. I would further observe, in reference to "natural selection," that we really ought to be furnished with a definition of the exact meaning of the term, for when we ask those that uphold the doctrine what natural selection really is, we can get no answer. What is the power that is called natural selection? Some use the phrase as a sort of modification of divine power, just as we use the word "nature"; but that, I submit, is not a philosophical way of using language. If by natural selection is meant chance, no possible lapse of time would be sufficient for one species to evolve itself into another; because chance operates as much in one direction as in another, and would never, by itself, evolve one species out of another. Natural selection is a power which I cannot at all conceive of, it seems to be continually watching the operations of chance, adopting those which are beneficial, and casting aside those which are injurious. This is the only explanation of it which I have met with; and I say again that no possible lapse of time—not even an eternity—would suffice to change one species into another by natural selection, unless you bring some modification of the

Divine or Creative power, apart from mere variation or chance, to bear upon it.

Mr. E. CHARLESWORTH (a visitor).—The problem of evolution, which has now for some years occupied so prominent a place in the mind of the intellectual world, is unquestionably one of those which may be worked out with the greatest benefit to human knowledge. To deal satisfactorily with evolution, or Darwinism, or natural selection, one of the things desirable would be that we should have spread out before us all the types of organic life that have ever existed, or that now exist. This is what we unquestionably want in order to deal with the problem in a satisfactory manner; but we cannot get such a map—we cannot see all these forms spread out; and the question therefore which we have to consider is: can we, upon the imperfect data that we have, deal with the problem in such a way as to make it of any practical utility? I maintain that we can. What is theory? Is it not one of the grandest incentives to observation? When a theory like that of evolution is put before the intellectual world, it sets men observing and thinking, and calls forth a vast amount of brain-power. All this being wisely directed, unquestionably tends to build up a great storehouse of human knowledge, even though ultimately the theory in question may wholly and entirely come to grief. Let me give you one illustration of this. Nearly a thousand years ago there was discovered one of the most lovely and exquisite forms to be found in the animal kingdom, popularly known as the "paper nautilus"; it was found in vast quantities on the shores of the Mediterranean, and other parts of the world, and that nautilus, when associated with animal life, had in it a cuttlefish. You could take up the shell, turn it topsy-turvy, and out dropped a cuttlefish. Then distinguished philosophers told us that cuttlefish dropped out of the shell when it was turned up. It could not possibly have made the shell, for it was a universal law throughout all the science of malacology, that where you had a shell made by an animal, that animal must have a muscular or organic attachment to the shell. We know that that is so in the case of the oyster,—that when you open an oyster you have to cut through the muscle. Then they said the beautiful and exquisite shell of the nautilus never could have been made by that hideous animal the cuttlefish, it must have been made by some other animal. For nearly a thousand years distinguished natural philosophers wrangled, fought, and quarrelled over this great problem, as to whether a cuttlefish did or did not make the shell of the paper nautilus. Look what an elaborate mass of reasoning has been accumulated around that insignificant matter. But what was the result of all this fighting? Why, that hundreds and thousands of naturalists set to work to study the habits of the cuttlefish, and although they did not solve the problem until very recently, they were led to make hosts of other most interesting discoveries, which are of the greatest possible advantage to the human race. This is the way in which I look at this problem of evolution. All the world is thinking and talking of it, and the brain-

power thus called forth will have similar results some day or other. As to the question of fact with regard to evolution and Darwinism, I am truly in a fog; it is a question of high interest, but it is also one in reference to which the data are so imperfect, that it is extremely difficult to make up one's mind. Let me put one case. There is a shell we often see in London which is known as the almond whelk, it lives in vast numbers in our own seas: there is also another kind of whelk which is found in vast numbers on the rocks of our shores, and is called the dog whelk. Scientifically, one is known as the *Fusus antiquus*, and the other as *Purpura lapillus*; and if you take the whole range of the conchological world, you perhaps could not pick out two shell-fish which could be more readily distinguished from each other; a child could see the difference. Go back to geological times, and go to the ancient formation called the Suffolk crag. In the seas which deposited that, there lived these almond whelks and these dog whelks; but if you take 50,000 of the fossil specimens, I would defy any one to separate them into their respective species; they merge the one into the other by the most minute gradations. "There," an evolutionist will tell you, "is a most magnificent instance of the way in which two types of form have diverged so widely that you cannot bring them together now, although in old times they did trench one upon the other, and were in fact one." Now, to take a fact on the other side, look at the Ammonites. The seas of the ancient world swarmed with countless millions of cuttlefish which had shells united to them organically; these fish were not like the cuttlefish of the paper nautilus, but were united to their shells by a muscular attachment. These Ammonites form one of the great wonders of palæontology, for they existed in countless myriads, not merely as individuals, but as different genera and species, all over the world. All over the world the life of these Ammonites ceased at the same time. I thank Dr. Nicholson for his most interesting paper, and as a visitor I would request him, in his reply, to say how the extinction of these extraordinary shell-fish would apply in reference to the doctrine of evolution. If evolution was going on there, what was evolved out of them, and where shall we find any trace of the species to which these extinguished species gave rise?

Rev. J. Sinclair.—If I rightly understand the views of Dr. Nicholson, I think they give a scientific basis for the definition of the word species,—that species would include every possible variation within a specific limit.

Dr. Nicholson.—With regard to the observations of Mr. Henslow, I would simply say, that I believe we have just grounds for thinking that we can know the first appearance of certain species. It is quite true that if you take every individual instance—if you take each separate fossil—and ask me as a palæontologist, "Are you quite certain that this made its first appearance where you first found it?" I should be bound to reply, "No: nobody can be certain"; but when you take a whole series, we must know the first appearance of a great many forms. Obviously, migration will not account

for many of the facts, although I hold migration myself, and always have done so. Take the Ammonites: we find them for the first time in the carboniferous beds; if I am told, "That is not their first appearance: it is a case of migration from some other area, as the Devonian"; or, "They occur in the Silurian beds of New Zealand," it is open to me to say that *that* is not their first appearance either, and that they have migrated from somewhere else. But it comes to an absolute certainty that, in the aggregate, we must know the first appearance of a great many forms, although we cannot make a positive assertion as to individual cases. As to the existence of pre-Laurentian rocks, that is a matter of opinion; such may have existed, but opinion is not yet definitely settled as to the existence of life, even in the Laurentian deposits; and as it is quite possible, and indeed probable, that these were all formed out of igneous rocks, we have no right to found any argument on the supposed existence of fossiliferous rocks prior to the Laurentian; there may have been such rocks, but we know nothing of them. As to the variation of the lower and higher types of life, I should be quite inclined to agree with Mr. Henslow, that there is a great difference, and I think it quite probable that in very many cases the variation is a quick one, and is effected *per saltum*: we know this is sometimes the case among living animals,—Japanned peacocks, for instance, have been produced by a sport.—This is more likely to occur in the case of the higher, than in the case of the lower animals. With regard to the extinction of Ammonites, that is a rather unfortunate instance to take, because there is such an enormous break in all parts of the world between the highest Cretaceous rocks and the lowest Tertiary rocks, that we do not know what became of those Cephalopods, nor of any Tetrabranchiates except the *Nautili*. It is almost certain that we shall find rocks, somewhere, intermediate in age between the lower Tertiaries and the upper Cretaceous, and there we may find Ammonites; but this is at present conjectural. Finally, with regard to the question as to the origin of species, I had hoped that I had distinctly expressed my opinion that evolution does occur, and that evolution is an operating cause in the modification and production of species. The remark that variation is bounded by definite limits is, all the same, quite true, though you admit evolution. When I say that variation is not indefinite, I am quite prepared to believe that the horse and the donkey have proceeded from a common ancestor, but that does not bind me to suppose that they have descended from an oyster (laughter). Variation must stop somewhere (cheers).

The meeting was then adjourned.

NOTE.

PRINCIPAL DAWSON, F.R.S., in his 1874 Annual Address, as President of the Natural History Society of Montreal, made some remarks, the insertion of which may not be deemed out of place at the close of this discussion. After alluding to the earlier elevation of that coast, he continued:—"We know that the eastern coast of America has in modern

times been gradually subsiding. Further, the remarkable submarine forests in the Bay of Fundy show that within a time not sufficient to produce the decay of pine-wood, this depression has taken place to the extent of at least 40 feet, and probably to 60 feet or more. We have thus direct geological evidence of a former higher condition of the land, which may, when at its maximum, have greatly exceeded that above indicated, since we cannot trace the submarine forests as far below the sea-level as they actually extend. The effect of such an elevation of the land would be not only a general shallowing of the water in the Bay of Fundy and the Acadian Bay, and an elevation of its temperature both by this and by the greater amount of neighbouring land, but, as Professor Verrill well states, it would also raise the banks off the Nova Scotia coast, and extending south from Newfoundland, so as to throw the Arctic current further from the shore and warm the water along the coasts of Nova Scotia and Northern New England. In these circumstances the marine animals of Southern New England might readily extend themselves all around the coasts of Nova Scotia and Cape Breton, and occupy the Acadian Bay. This modern subsidence of the land would produce a relapse toward the glacial age, the Arctic currents would be allowed to cleave more closely to the coast, and the inhabitants of the Acadian Bay would gradually become isolated, while the northern animals of Labrador would work their way southward. Various modern indications point to the same conclusions. Verrill has described little colonies of southern species still surviving on the coast of Maine. There are also dead shells of these species in mud-banks, in places where they are now extinct. He also states that the remains in shell-heaps left by the Indians indicate that even within the period of their occupancy some of these species existed in places where they are not now found. Willis has catalogued some of these species from the deep bays and inlets on the Atlantic coast of Nova Scotia, and has shown that some of them still exist on the Sable Island banks. Whiteaves finds in the Bradelle and Orphan bank littoral species remote from the present shores, and indicating a time when these banks were islands, which have been submerged by subsidence, aided, no doubt, by the action of the waves. It would thus appear that the colonization of the Acadian Bay with southern forms belongs to the modern period, but that it has already passed its culmination, and the recent subsidence of the coast has, no doubt, limited the range of these animals, and is probably still favouring the gradual inroads of the Arctic fauna from the north, which, should this subsidence go on, will creep slowly back to reoccupy the ground which it once held in the post-pliocene time.

"Such peculiarities of distribution serve to show the effects of even comparatively small changes of level upon climate and upon the distribution of life, and to confirm the same lesson of caution in our interpretation of local diversities of fossils, which geologists have been lately learning from the distribution of cold and warm currents in the Atlantic. Another lesson which they teach is the wonderful fixity of species. Continents rise and sink, climates change, islands are devoured by the sea or restored again from its depths; marine animals are locally exterminated and are enabled in the course of long ages to regain their lost abodes; yet they remain ever the same, and even in their varietal forms perfectly resemble those remote ancestors which are separated from them by a vast lapse of ages and by many physical revolutions. This truth, which I have already deduced from the post-pliocene fauna of the St. Lawrence Valley, is equally taught by the mollusks of the Acadian Bay, and by their Arctic relatives returning after long absence to claim their old homes."