

# SCIENCE

EDITORIAL COMMITTEE: S. NEWCOMB, Mathematics; R. S. WOODWARD, Mechanics; E. C. PICKERING, Astronomy; T. C. MENDENHALL, Physics; R. H. THURSTON, Engineering; IRA REMSEN, Chemistry; J. LE CONTE, Geology; W. M. DAVIS, Physiography; O. C. MARSH, Paleontology; W. K. BROOKS, C. HART MERRIAM, Zoology; S. H. SCUDDER, Entomology; N. L. BRITTON, Botany; HENRY F. OSBORN, General Biology; H. P. BOWDITCH, Physiology; J. S. BILLINGS, Hygiene; J. MCKEEN CATTELL, Psychology; DANIEL G. BRINTON, J. W. POWELL, Anthropology.

FRIDAY, OCTOBER 30, 1896.

## CONTENTS:

*British Association for the Advancement of Science:—*

*Address by the President before the Zoological Section:* E. B. POULTON .....625  
*Section H.—Anthropology:* A. C. HADDON .....637

*The International Psychological Congress:* SHEPHERD IVORY FRANZ.....640

*The Princeton Sesquicentennial.....647*

*Current Notes on Anthropology:—*

*The Black Race of Susa; The Earliest Relics of Man in France; The Man in the Moon:* D. G. BRINTON .....649

*Current Notes on Meteorology:—*

*Atmospheric Electricity and Telephones; Weather Forecasts Several Days in Advance; The First Daily Weather Map:* R. DEC. WARD.....650

*Notes on Inorganic Chemistry;* J. L. H.....651

*Astronomical Notes:* H. J. ....652

*Scientific Notes and News.....652*

*University and Educational News. ....655*

*Discussion and Correspondence:—*

*Halsted on the Straight:* GEORGE BRUCE HALSTED. *The Curve-tracing Top:* JAS. E. TALMAGE. *Geology in the Colleges of the United States:* L. W. CHANEY, JR.....656

*Scientific Literature:—*

*Bendire's Life Histories of North American Birds:* C. H. M. *Smith's Economic Entomology for the Farmer and Fruit Grower:* L. O. HOWARD.....657

*Societies and Academies:—*

*The New York Academy of Science:—Biological Section:* CHARLES F. BRISTOL. *Annual Meeting of the New York Section of the American Chemical Society:* DURAND WOODMAN. *The Academy of Science of St. Louis:* WILLIAM TRELEASE .....659

*Scientific Journals:—*

*The American Geologist.....660*

MSS. intended for publication and books, etc., intended or review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

## BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.\*

### ADDRESS TO THE ZOOLOGICAL SECTION BY THE PRESIDENT OF THE SECTION.

A VERY brief study of the proceedings of this Section in bygone years will show that Presidents have exercised a very wide choice in the selection of subjects. At the last meeting of the Association in this city, in 1870, the Biological Section had as its President the late Prof. Rolleston, a man whose remarkable personality made a deep impression upon all who came under his influence, as I have the strongest reason for remembering, inasmuch as he was my first teacher in zoology, and I attended his lectures when but little over seventeen. His address was most characteristic, glancing over a great variety of subjects, literary as well as scientific, and abounding in quotations from several languages, living and dead. A very different style of address was that delivered by the distinguished zoologist who presided over the meeting. Prof. Huxley took as his subject 'The History of the Rise and Progress of a Single Biological Doctrine.'

Of these two types I selected the latter as my example, and especially desired to attempt the discussion, however inadequate, of some difficulty which confronts the zoologist at the very outset, when he begins to reason from the facts around him

\* Liverpool, 1896.

—a difficulty which is equally obvious and of equal moment to the highly-trained investigator and the man who is keenly interested in the results obtained by others, but cannot himself lay claim to the position and authority of a skilled observer; to the naturalist, and to the one who follows some other branch of knowledge but is interested in the progress of a sister science.

Two such difficulties were alluded to by Lord Salisbury in his interesting presidential address to the British Association at Oxford in 1894, when he spoke of 'two of the strongest objections to the Darwinian explanation' of evolution—viz, the theory of natural selection—as appearing 'still to retain all their force.' The first of these objections was the insufficiency of the time during which the earth has been in a habitable state, as calculated by Lord Kelvin and Prof. Tait, 100 million years being conceded by the former, but only 10 million by the latter. Lord Salisbury quite rightly stated that for the evolution of the organic world as we know it by the slow process of natural selection at least many hundred million years are required; whereas, "if the mathematicians are right, the biologists cannot have what they demand. \* \* \* The jelly-fish would have been dissipated in steam long before he had had a chance of displaying the advantageous variation which was to make him the ancestor of the human race."

The second objection was that "we cannot demonstrate the process of natural selection in detail; we cannot even, with more or less ease, imagine it." "In natural selection who is to supply the breeder's place?" "There would be nothing but mere chance to secure that the advantageously varied bridegroom at one end of the wood should meet the bride, who by a happy contingency had been advantageously varied in the same direction at the same time at the other end of the wood. It would be a

mere chance if they ever knew of each other's existence; a still more unlikely chance that they should resist on both sides all temptations to a less advantageous alliance. But unless they did so the new breed would never even begin, let alone the question of its perpetuation after it had begun."

Prof. Huxley, in seconding the vote of thanks to the President, said that he could imagine that certain parts of the address might raise a very good discussion in one of the Sections, and I have little doubt that he referred to these criticisms and to this Section. When I had to face the duty of preparing this address I could find no subjects better than those provided by Lord Salisbury.

At first the second objection seemed to offer the more attractive subject. It was clear that the theory of natural selection as held by Darwin was misconceived by the speaker, and that the criticism was ill-aimed. Darwin and Wallace, from the very first, considered that the minute differences which separate individuals were of far more importance than the large single variations which occasionally arise—Lord Salisbury's advantageously varied bride and bridegroom at opposite ends of the wood. In fact, after Fleeming Jenkins' criticisms in the *North British Review* for June 1867, Darwin abandoned these large single variations altogether. Thus he wrote in a letter to Wallace (February 2, 1869): "I always thought individual differences more important; but I was blind, and thought single variations might be preserved much oftener than I now see is possible or probable. I mentioned this in my former note merely because I believed that you had come to a similar conclusion, and I like much to be in accord with you."\* Hence we may infer that the other great discoverer of natural selection had come to the same conclusion at an even earlier date. But this fact re-

\* *Life and Letters*, Vol. III.

moves the whole point from the criticism I have just quoted. According to the Darwin-Wallace theory of natural selection, individuals sufficiently advantageously varied to become the material for a fresh advance when an advance became necessary, and at other times sufficient to maintain the ground previously gained—such individuals existed not only at the opposite ends of the wood, but were common enough in every colony within its confines. The mere fact that an individual had been able to reach the condition of a possible bride or bridegroom would count for much. Few will dispute that such individuals “have already successfully run the gauntlet of by far the greatest dangers which beset the higher animal [and, it may be added, the lower animals also]—the dangers of youth. Natural selection has already pronounced a satisfactory verdict upon the vast majority of animals which have reached maturity.”\*

But the criticism retains much force when applied to another theory of evolution by the selection of large and conspicuous variations—a theory which certain writers have all along sought to add to or substitute for that of Darwin. Thus Huxley from the very first considered that Darwin had burdened himself unnecessarily in rejecting *per saltum* evolution so unreservedly.† And recently this view has been revived by Bateson’s work on variation and by the writings of Francis Galton. I had at first intended to attempt a discussion of this view, together with Lord Salisbury’s and other objections which may be urged against it; but the more the two were considered, the more pressing became the claims of the criticism alluded to at first—the argument that the history of our planet does not allow sufficient time for a process which all its advocates admit to be extremely slow in

its operation. I select this subject because of its transcendent importance in relation to organic evolution, and because I hope to show that the naturalist has something of weight to contribute to the controversy which has been waged intermittently ever since Lord Kelvin’s paper ‘On Geological Time’\* appeared in 1868. It has been urged by the great worker and teacher who occupied the Presidential Chair of this Association when it last met in this city that biologists have no right to take part in this discussion. In the Anniversary Address to the Geological Society in 1869 Huxley said: “Biology takes her time from geology. \* \* \* If the geological clock is wrong, all the naturalist will have to do is to modify his notions of the rapidity of change accordingly.” This contention is obviously true as regards the time which has elapsed since the earliest fossiliferous rocks were laid down. For the duration of the three great periods we must look to the geologist; but the question as to whether the whole of organic evolution is comprised within these limits, or, if not, what proportion of it is so contained, is a question for the naturalist. The naturalist alone can tell the geologist whether his estimate is sufficient, or whether it must be multiplied by a small or by some unknown but certainly high figure, in order to account for the evolution of the earliest forms of life known in the rocks. This, I submit, is a most important contribution to the discussion.

Before proceeding further it is right to point out that obviously these arguments will have no weight with those who do not believe that evolution is a reality. But although the causes of evolution are greatly debated, it may be assumed that there is

\* *Trans. Geol. Soc. Glasgow*, Vol. III. See also ‘On the Age of the Sun’s Heat,’ Macmillan, March, 1862: reprinted as Appendix to Thomson and Tait, *Natural Philosophy*, Vol. I, part 2, second edition, and ‘On the Secular Cooling of the Earth,’ *Royal Society of Edinburgh*, 1862.

\* Poulton, *Colours of Animals*, p. 308.

† See his letter to Darwin, November 23, 1859: *Life and Letters*, Vol. II.

no perceptible difference of opinion as to evolution itself, and this common ground will bear the weight of all the zoological arguments we shall consider to-day.

It will be of interest to consider first how the matter presented itself to naturalists before the beginning of this controversy on the age of this habitable earth. I will content myself with quotations from three great writers on biological problems—men of extremely different types of mind, who yet agreed in their conclusions on this subject.

In the original edition of the 'Origin of Species' (1859), Darwin, arguing from the presence of trilobites, Nautilus, Lingula, etc., in the earliest fossiliferous rocks, comes to the following conclusion (pages 306, 307): "Consequently, if my theory be true, it is indisputable that before the lowest Silurian stratum was deposited long periods elapsed, as long as, or probably far longer than, the whole interval from the Silurian age to the present day; and that during these vast yet quite unknown periods of time the world swarmed with living creatures."

The depth of his conviction in the validity of this conclusion is seen in the fact that the passage remains substantially the same in later editions, in which, however, Cambrian is substituted for Silurian, while the words 'yet quite unknown' are omitted, as a concession, no doubt, to Lord Kelvin's calculations, which he then proceeds to discuss, admitting as possible a more rapid change in organic life, induced by more violent physical changes.\*

We know, however, that such concessions troubled him much, and that he was really giving up what his judgment still approved. Thus he wrote to Wallace on April 14, 1869: "Thomson's views of the recent age of the world have been for some time one of my sorest troubles. \* \* \*" And again, on July 12, 1871, alluding to Mivart's criticisms, he

\* 6th ed., 1872, p. 286.

says: "I can say nothing more about missing links than what I have said. I should rely much on pre-Silurian times; but then comes Sir W. Thomson like an odious spectre."

Huxley's demands for time in order to account for pre-Cambrian evolution, as he conceived it, were far more extensive. Although in 1869 he bade the naturalist stand aside and take no part in the controversy, he had nevertheless spoken as a naturalist in 1862, when, at the close of another Anniversary address to the same Society, he argued from the prevalence of persistent types "that any admissible hypothesis of progressive modification must be compatible with persistence without progression through indefinite periods;" and then maintained that "should such an hypothesis eventually be proved to be true \* \* \* the conclusion will inevitably present itself that the Paleozoic, Mesozoic, Cainozoic faunæ and floræ, taken together, bear somewhat the same proportion to the whole series of living beings which have occupied this globe as the existing fauna and flora do to them."

Herbert Spencer, in his article on 'Illogical Geology' in the *Universal Review* for July, 1859,\* uses these words: "Only the last chapter of the earth's history has come down to us. The many previous chapters, stretching back to a time immeasurably remote, have been burnt, and with them all the records of life we may presume they contained." Indeed, so brief and unimportant does Herbert Spencer consider this last chapter to have been that he is puzzled to account for 'such evidences of progression as exist;' and finally concludes that they are of no significance in relation to the doctrine of evolution, but probably represent the succession of forms by which a newly upheaved land would be peopled. He argues that the earliest immigrants would be

\* Reprinted in his *Essays*, 1868, Vol. I., pp. 324-376.

the lower forms of animal and vegetable life, and that these would be followed by an irregular succession of higher and higher forms, which 'would thus simulate the succession presented by our own sedimentary series.'

We see, then, what these three great writers on evolution thought on this subject; they are all convinced that the time during which the geologists concluded that the fossiliferous rocks had been formed was utterly insufficient to account for organic evolution.

Our object to-day is first to consider the objections raised by physicists against the time demanded by the geologist, and still more against its multiplication by the student of organic evolution; secondly, to inquire whether the present state of paleontological and zoological knowledge increases or diminishes the weight of the threefold opinion quoted above—an opinion formed on far more slender evidence than that which is now available. And if we find this opinion sustained, it must be considered to have a very important bearing upon the controversy.

The arguments of the physicists are three:

First, the argument from the observed secular change in the length of the day, the most important element of which is due to tidal retardation. It has been known for a very long time that the tides are slowly increasing the length of our day. Huxley explains the reason with his usual lucidity: "That this must be so is obvious, if one considers, roughly, that the tides result from the pull which the sun and moon exert upon the sea, causing it to act as a sort of brake upon the solid earth."\*

A liquid earth takes a shape which follows from its rate of revolution, and from which, therefore, its rate of revolution can be calculated.

The liquid earth consolidated in the form

\* Anniv. Address to Geol. Soc., 1869.

it last assumed, and this shape has persisted until now and informs us of the rate of revolution at the time of consolidation. Comparing this with the present rate, and knowing the amount of lengthening in a given time due to tidal friction, we can calculate the date of consolidation as certainly less than 1,000 million years ago.

This argument is fallacious, as many mathematicians have shown. The present shape tells us nothing of the length of the day at the date of consolidation; for the earth, even when solid, will alter its form when exposed for a long time to the action of great forces. As Professor Perry said in a letter to Professor Tait:\* "I know that solid rock is not like cobbler's wax, but 1,000 million years is a very long time, and the forces are great." Furthermore, we know that the earth is always altering its shape and that whole coastlines are slowly rising or falling, and that this has been true, at any rate, during the formation of the stratified rocks.

This argument is dead and gone. We are, indeed, tempted to wonder that the physicist, who was looking about for arguments by which to revise what he conceived to be the hasty conclusions of the geologists as to the age of the earth, should have exposed himself to such an obvious retort in basing his own conclusions as to its age on the assumption that the earth, which we know to be always changing in shape, has been unable to alter its equatorial radius by a few miles under the action of tremendous forces constantly tending to alter it, and having 1,000 million years in which to do the work.

With this flaw in the case it is hardly necessary to insist on our great uncertainty as to the rate at which the tides are lengthening the day.

The spectacle presented by the geologist and biologist, deeply shocked at Lord Kel-

\* *Nature*, January 3, 1895.

vin's extreme uniformitarianism in the domain of astronomy and cosmic physics, is altogether too comforting to be passed by without remark; but, in thus indulging in a friendly *tu quoque*, I am quite sure that I am speaking for every member of this Section in saying that we are in no way behind the members of Section A in our pride and admiration at the noble work which he has done for science, and we are glad to take this opportunity of congratulating him on the half century of work and teaching—both equally fruitful—which has reached its completion in the present year.

The second argument is based upon the cooling of the earth, and this is the one brought forward and explained by Lord Salisbury in his Presidential Address. It has been the argument on which, perhaps, the chief reliance has been placed, and of which the data—so it was believed—were the least open to doubt.

On the Sunday during the meeting of the British Association at Leeds (1890) I went for a walk with Prof. Perry and asked him to explain the physical reasons for limiting the age of the earth to a period which the students of other sciences considered to be very inadequate. He gave me an account of the data on which Lord Kelvin relied in constructing this second argument, and expressed the strong opinion that they were perfectly sound, while, as for the mathematics, it might be taken for granted, he said, that they were entirely correct. He did not attach much weight to the other arguments, which he regarded as merely offering support to the second.

This little piece of personal history is of interest, inasmuch as Prof. Perry has now provided us with a satisfactory answer to the line of reasoning which so fully satisfied him in 1890. And he was led to a critical examination of the subject by the attitude taken up by Lord Salisbury in 1894. Prof. Perry was not present at the meeting, but

when he read the President's address, and saw how other conclusions were ruled out of court, how the only theory of evolution which commands anything approaching universal assent was set on one side because of certain assumptions as to the way in which the earth was believed to have cooled, he was seized with a desire to sift these assumptions and to inquire whether they would bear the weight of such far-reaching conclusions. Before giving the results of his examination, it is necessary to give a brief account of the argument on which so much has been built.

Lord Kelvin assumed that the earth is a homogeneous mass of rock similar to that with which we are familiar on the surface. Assuming, further, that the temperature increases, on the average,  $1^{\circ}$  F. for every 50 feet of depth near the surface everywhere, he concluded that the earth would have occupied not less than twenty, nor more than four hundred million, years in reaching its present condition from the time when it first began to consolidate and possessed a uniform temperature of  $7,000^{\circ}$  F.

If, in the statement of the argument, we substitute for the assumption of a homogeneous earth an earth which conducts heat better internally than it does towards the surface, Prof. Perry, whose calculations have been verified by Mr. O. Heaviside, finds that the time of cooling has to be lengthened to an extent which depends upon the value assigned to the internal conducting power. If, for instance, we assume that the deeper part of the earth conducts ten times as well as the outer part, Lord Kelvin's age would require to be multiplied by fifty-six. Even if the conductivity be the same throughout, the increase of density in the deeper part, by augmenting the capacity for heat of unit volume, implies a longer age than that conceded by Lord Kelvin. If the interior of the earth be fluid or contain fluid in a honeycomb struc-

ture, the rate at which heat can travel would be immensely increased by convection currents, and the age would have to be correspondingly lengthened. If, furthermore, such conditions, although not obtaining now, did obtain in past times, they will have operated in the same direction.

Prof. Tait, in his letter to Prof. Perry (published in *Nature* of January 3, 1895), takes the entirely indefensible position that the latter is bound to prove the higher internal conductivity. The obligation is all on the other side, and rests with those who have pressed their conclusions hard and carried them far. These conclusions have been, as Darwin found them, one of our 'sores of troubles;' but when it is admitted that there is just as much to be said for another set of assumptions leading to entirely different conclusions our troubles are at an end, and we cease to be terrified by an array of symbols, however unintelligible to us. It would seem that Prof. Tait, without, as far as I can learn, publishing any independent calculation of the age of the earth, has lent the weight of his authority to a period of 10 million years, or half of Lord Kelvin's minimum. But in making this suggestion he apparently feels neither interest nor responsibility in establishing the data of the calculations which he borrowed to obtain therefrom a very different result from that obtained by their author.

Prof. Perry's object was not to substitute a more correct age for that obtained by Lord Kelvin, but rather to show that the data from which the true age could be calculated are not really available. We obtain different results by making different assumptions, and there is no sufficient evidence for accepting one assumption rather than another. Nevertheless, there is some evidence which indicates that the interior of the earth in all probability conducts better than the surface. Its far higher density is consistent with the belief that it is rich

in metals, free or combined. Prof. Schuster concludes that the internal electric conductivity must be considerably greater than the external. Geologists have argued from the amount of folding to which the crust has been subjected that cooling must have taken place to a greater depth than 120 miles, as assumed in Lord Kelvin's argument. Prof. Perry's assumption would involve cooling to a much greater depth.

Prof. Perry's conclusion that the age of the habitable earth is lengthened by increased conductivity is the very reverse of that to which we should be led by a superficial examination of the case. Prof. Tait, indeed, in the letter to which I have already alluded, has said: "Why, then, drag in mathematics at all, since it is absolutely obvious that the better conductor the interior in comparison with the skin, the longer ago must it have been when the whole was at 7,000° F., the state of the skin being as at present?" Prof. Perry, in reply, pointed out that one mathematician who had refuted the tidal retardation argument\* had assumed that the conditions described by Prof. Tait would have involved a shorter period of time. And it is probable that Lord Kelvin thought the same; for he had assumed conditions which would give the result—so he believed at the time—most acceptable to the geologist and biologist. Prof. Perry's conclusion is very far from obvious, and without the mathematical reasoning would not be arrived at by the vast majority of thinking men.

The 'natural man' without mathematics would say, so far from this being 'absolutely obvious,' it is quite clear that increased conductivity, favoring escape of heat, would lead to more rapid cooling and would make Lord Kelvin's age even shorter.

The argument can, however, be put clearly without mathematics, and, with Prof. Perry's

\*Rev. M. H. Close in *E. Dublin Soc.*, February 1878.

help, I am able to state it in a few words. Lord Kelvin's assumption of an earth resembling the surface rock in its relations to heat leads to the present condition of things, namely, a surface gradient of  $1^{\circ}$  F. for every 50 feet, in 100 million years, more or less. Deeper than 150 miles he imagines that there has been almost no cooling. If, however, we take one of the cases put by Prof. Perry, and assume that below a depth of four miles there is ten times the conductivity, we find that after a period of 10 billion years the gradient at the surface is still  $1^{\circ}$  F. for every 50 feet; but that we have to descend to a depth of 1,500 miles before we find the initial temperature of  $7,000^{\circ}$  F. undiminished by cooling. In fact, the earth, as a whole, has cooled far more quickly than under Lord Kelvin's conditions, the greater conductivity enabling a far larger amount of the internal heat to escape; but in escaping it has kept up the temperature gradient at the surface.

Lord Kelvin, replying to Prof. Perry's criticisms, quite admits that the age at which he had arrived by the use of this argument may be insufficient. Thus, he says, in his letter\*: "I thought my range from 20 millions to 400 millions was probably wide enough, but it is quite possible that I should have put the superior limit a good deal higher, perhaps 4,000 instead of 400."

The third argument was suggested by Helmholtz, and depends on the life of the sun. If the energy of the sun is due only to the mutual gravitation of its parts, and if the sun is now of uniform density, "the amount of heat generated by his contraction to his present volume would have been sufficient to last 18 million years at his present rate of radiation."<sup>†</sup> Lord Kelvin rejects the assumption of uniform density, and is, in consequence of this change, able to offer

a much higher upward limit of 500 million years.

This argument also implies the strictest uniformitarianism as regards the sun. We know that other suns may suddenly gain a great accession of energy, so that their radiation is immensely increased. We only detect such changes when they are large and sudden, but they prepare us to believe that smaller accessions may be much more frequent, and perhaps a normal occurrence in the evolution of a sun. Such accessions may have followed from the convergence of a stream of meteors. Again, it is possible that the radiation of the sun may have been diminished and his energy conserved by a solar atmosphere.

Newcomb has objected to these two possible modes by which the life of the sun may have been greatly lengthened, that a lessening of the sun's heat by under a quarter would cause all the water on the earth to freeze, while an increase of much over half would probably boil it all away. But such changes in the amount of radiation received would follow from a greater distance from the sun of  $15\frac{1}{2}$  per cent., and a greater proximity to him of 18.4 per cent., respectively. Venus is inside the latter limit, and Mars outside the former, and yet it would be a very large assumption to conclude that all the water in the former is steam and all in the latter ice. Indeed, the existence of water and the melting of snow on Mars are considered to be thoroughly well authenticated. It is further possible that in a time of lessened solar radiation the earth may have possessed an atmosphere which would retain a larger proportion of the sun's heat; and the internal heat of the earth itself, great lakes of lava under a canopy of cloud, for example, may have played an important part in supplying warmth.

Again we have a greater age if there was more energy available than in Helmholtz's hypothesis. Lord Kelvin maintains that

\**Nature*, January 3, 1895.

<sup>†</sup>Newcomb's *Popular Astronomy*, p. 523.



this is improbable because of the slow rotation of the sun, but Perry has given reasons for an opposite conclusion.

The collapse of the first argument of tidal retardation, and of the second of the cooling of the earth, warn us to beware of a conclusion founded on the assumption that the sun's energy depends, and has ever depended, on a single source of which we know the beginning and the end. It may be safely maintained that such a conclusion has not that degree of certainty which justifies the followers of one science in assuming that the conclusion of other sciences must be wrong, and in disregarding the evidence brought forward by workers in other lines of research.

We must freely admit that this third argument has not yet fully shared the fate of the two other lines of reasoning. Indeed, Prof. George Darwin, although attacking these latter, agrees with Lord Kelvin in regarding 500 million years as the maximum life of the sun.\*

We may observe, too, that 500 million years is by no means to be despised; a great deal may happen in such a period of time. Although I should be very sorry to say that it is sufficient, it is a very different offer from Prof. Tait's 10 million.

In drawing up this account of the physical arguments, I owe almost everything to Prof. Perry for his articles in *Nature* (January 3 and April 18, 1895), and his kindness in explaining any difficulties that arose. I have thought it right to enter into these arguments in some detail and to consume a considerable proportion of our time in their discussion. This was imperatively necessary, because they claimed to stand as barriers across our path, and, so long as they were admitted to be impassable, any further progress was out of the question. What I hope has been an unbiassed examination has shown that, as barriers, they are

more imposing than effective; and we are free to proceed and to look for the conclusions warranted by our own evidence. In this matter we are at one with the geologists; for, as has been already pointed out, we rely on them for an estimate of the time occupied by the deposition of the stratified rocks, while they rely on us for a conclusion as to how far this period is sufficient for the whole of organic evolution.

First, then, we must briefly consider the geological argument, and I cannot do better than take the case as put by Sir Archibald Geikie in his Presidential Address to this Association in Edinburgh in 1892.

Arguing from the amount of material removed from the land by denuding agencies, and carried down to the sea by rivers, he showed that the time required to reduce the height of the land by one foot varies, according to the activity of the agencies at work, from 730 years to 6,800 years. But this also supplies a measure of the rate of deposition of rock; for the same material is laid down elsewhere, and would, of course, add the same height of one foot to some other area equal in size to that from which it was removed.

The next datum to be obtained is the total thickness of the stratified rocks from the Cambrian system to the present day. "On a reasonable computation these stratified masses, where most fully developed, attain a united thickness of not less than 100,000 feet. If they were all laid down at the most rapid recorded rate of denudation they would require a period of 73 millions of years for their completion. If they were laid down at the slowest rate they would demand a period of not less than 680 millions."

The argument that geological agencies acted much more vigorously in past times he entirely refuted by pointing to the character of the deposits of which the stratified series is composed. "We can see no proof

\* British Association Reports 1886, pp. 514-518.

whatever, nor even any evidence which suggests, that on the whole the rate of waste and sedimentation was more rapid during Mesozoic and Palæozoic time than it is to-day. Had there been any marked difference in this rate from ancient to modern times, it would be incredible that no clear proof of it should have been recorded in the crust of the earth."

It may, therefore, be inferred that the rate of deposition was no nearer the more rapid than the slower of the rates recorded above, and, if so, the stratified rocks would have been laid down in about 400 million years.

There are other arguments favoring the uniformity of conditions throughout the time during which the stratified rocks were laid down, in addition to those which are purely geological and depend upon the character of the rocks themselves. Although more biological than geological, these arguments are best considered here.

The geological agency to which attention is chiefly directed by those who desire to hurry up the phenomena of rock formation is that of the tides. But it seems certain that the tides were not sufficiently higher in Silurian times to prevent the deposition of certain beds of great thickness under conditions as tranquil as any of which we have evidence in the case of a formation extending over a large area. From the character of the organic remains it is known that these beds were laid down in the sea, and there are the strongest grounds for believing that they were accumulated along shores and in fairly shallow water. The remains of extremely delicate organisms are found in immense numbers and over a very large area. The recent discovery, in the Silurian system of America, of trilobites, with their long delicate antennæ perfectly preserved, proves that in one locality (Rome, New York State) the tranquillity of deposition was quite as profound as in

any locality yet discovered on this side of the Atlantic.

There are, then, among the older Palæozoic rocks a set of deposits than which we can imagine none better calculated to test the force of the tides; and we find that they supply evidence for exceptional tranquillity of conditions over a long period of time.

There is other evidence of the permanence, throughout the time during which the stratified rocks were deposited, of conditions not very dissimilar from those which obtain to-day. Thus the attachments of marine organisms, which are permanently rooted to the bottom or on the shores, did not differ in strength from those which we now find—an indication that the strains due to the movements of the sea did not greatly differ in the past.

We have evidence of a somewhat similar kind to prove uniformity in the movements of the air. The expanse of the wings of flying organisms certainly does not differ in a direction which indicates any greater violence in the atmospheric conditions. Before the birds had become dominant among the larger flying organisms their place was taken by the flying reptiles, the pterodactyls, and before the appearance of these we know that in Palæozoic times the insects were of immense size, a dragon-fly from the Carboniferous rocks of France being upwards of 2 feet in the expanse of its wings. As one group after another of widely dissimilar organisms gained control of the air, each was in turn enabled to increase to the size which was best suited to such an environment, but we find that the limits which obtain to-day were not widely different in the past. And this is evident, for the uniformity in the strains due to wind and storm no less than to those due to gravity. Furthermore, the condition of the earth's surface at present shows us how extremely sensitive the flying organism is to an increase in the former of these strains, when

it occurs in proximity to the sea. Thus it is well known that an unusually large proportion of the Madeiran beetles are wingless, while those which require the power of flight possess it in a stronger degree than on continental areas. This evolution in two directions is readily explained by the destruction, by drowning, of the winged individuals of the species which can manage to live without the power of flight, and of the less strongly winged individuals of those which need it. Species of the latter kind cannot live at all in the far more stormy Kerguelen Land, and the whole of the insect fauna is wingless.

The size and strength of the trunks of fossil trees afford, as Prof. George Darwin has pointed out, evidence of uniformity in the strains due to the condition of the atmosphere.

We can trace the prints of raindrops at various geological horizons, and in some cases found in this country it is even said that the eastern side of the depressions is the more deeply pitted, proving that the rain drove from the west, as the great majority of our storms do to-day.

When, therefore, we are accused of uniformitarianism, as if it were an entirely unproved assumption, we can at any rate point to a large body of positive evidence which supports our contention, and the absence of any evidence against it. Furthermore, the data on which we rely are likely to increase largely, as the result of future work.

After this interpolation, chiefly of biological argument in support of the geologist, I cannot do better than bring the geological evidence to a close in the words which conclude Sir Archibald Geikie's address: "After careful reflection on the subject, I affirm that the geological record furnishes a mass of evidence which no arguments drawn from other departments of Nature can explain away, and which, it seems to me, cannot be satisfactorily interpreted save with an al-

lowance of time much beyond the narrow limits which recent physical speculation would concede."

In his letter to Prof. Perry,\* Lord Kelvin says: "So far as underground heat alone is concerned, you are quite right that my estimate was 100 million, and please remark † that that is all Geikie wants; but I should be exceedingly frightened to meet him now with only 20 million in my mouth."

We have seen, however, that Geikie considered the rate of sedimentation to be, on the whole, uniform with that which now obtains, and this would demand a period of nearly 400 million years. He points out, furthermore, that the time must be greatly increased on account of the breaks and interruptions which occur in the series, so that we shall probably get as near an estimate as is possible from the data which are available by taking 450 million as the time during which the stratified rocks were formed. Before leaving this part of the subject, I cannot refrain from suggesting a line of enquiry which may very possibly furnish important data for checking the estimates at present formed by geologists, and which, if the mechanical difficulties can be overcome, is certain to lead to results of the greatest interest and importance. Ever since the epoch-making voyage of the 'Challenger' it has been known that the floor of the deep oceans outside the shallow shelf which fringes the continental areas is covered by a peculiar deposit formed entirely of meteoric and volcanic dust, the waste of floating pumice and the hard parts of animals living in the ocean. Of these latter only the most resistant can escape the powerful solvent agencies. Many observations prove that the accumulation of this deposit is extremely slow. One indication of this is especially convincing; the teeth of sharks and the most resistant part of the

\* *Nature*, January 3, 1895.

† *P. L. and A.*, Vol. II., p. 87.

skeleton—the ear bones—of whales are so thickly spread over the surface that they are continually brought up in the dredge, while sometimes a single haul will yield a large number of them. Imagine the countless generations of sharks and whales which must have succeeded each other in order that these insignificant portions of them should be so thickly spread over that vast area which forms the ocean floor. We have no reason to suppose that sharks and whales die more frequently in the deep ocean than in the shallow fringing seas; in fact, many observations point in the opposite direction, for wounded and dying whales often enter shallow creeks and inlets, and not uncommonly become stranded. And yet these remains of sharks and whales, although well known in the stratified rocks which were laid down in comparatively shallow water and near coasts, are only found in certain beds, and then in far less abundance than in the oceanic deposit. We can only explain this difference by supposing that the latter accumulate with such almost infinite slowness as compared with the continental deposits that these remains form an important and conspicuous constituent of the one, while they are merely found here and there when looked for embedded in the other. The rate of accumulation of all other constituents is so slow as to leave a layer of teeth and ear-bones uncovered, or covered by so thin a deposit that the dredge can collect them freely. Dr. John Murray calculates that only a few inches of this deposit have accumulated since the Tertiary period. These most interesting facts prove furthermore that the great ocean basins and continental areas have occupied the same relative positions since the formation of the first stratified rocks; for no oceanic deposits are found anywhere in the latter. We know the sources of the oceanic deposit, and it might be possible to form an estimate,

within wide limits, of its rate of accumulation. If it were possible to ascertain its thickness by means of a boring, some conclusions as to the time which has elapsed during the lifetime of certain species—perhaps even the lifetime of the oceans themselves—might be arrived at. Lower down the remains of earlier species would probably be found. The depth of this deposit and its character at deeper levels are questions of overwhelming interest; and perhaps even more so is the question as to what lies beneath. Long before the ‘Challenger’ had proved the persistence of oceanic and continental areas, Darwin, with extraordinary foresight, and opposed by all other naturalists and geologists, including his revered teacher, Lyell, had come to the same conclusion. His reasoning on the subject is so convincing that it is remarkable that he made so few converts, and this is all the more surprising since the arguments were published in the ‘Origin of Species,’ which in other respects produced so profound an effect. In speculating as to the rocks in which the remains of the ancestors of the earliest known fossils may still exist, he suggested that, although the existing relationship between the positions of our present oceans and continental areas is of immense antiquity, there is no reason for the belief that it has persisted for an indefinite period, but that at some time long antecedent to the earliest known fossiliferous rocks “continents may have existed where oceans are now spread out; and clear and open oceans may have existed where our continents now stand.” Not the least interesting result would be the test of this hypothesis, which would probably be forthcoming as the result of boring into the floor of a deep ocean; for although, as Darwin pointed out, it is likely enough that such rocks would be highly metamorphosed, yet it might still be possible to ascertain whether they had at any

time formed part of a continental deposit, and perhaps to discover much more than this. Such an undertaking might be carried out in conjunction with other investigations of the highest interest, such as the attempt to obtain a record of the swing of a pendulum at the bottom of the ocean.

E. B. POULTON.

(*To be Concluded.*)

#### SECTION H. ANTHROPOLOGY.

THE Liverpool session of the Anthropological Section will long be remembered as one of exceptional interest. The President, Mr. Arthur J. Evans, keeper of the Ashmolean Museum at Oxford, had long previously arranged for a discussion on the origin of the Mediterranean race and culture, and numerous distinguished archæologists and anthropologists had been invited to attend and join in the discussion, among others may be mentioned Prof. Sergi, M. Salomon Reinach, Dr. P. Topinard and Prof. D. G. Brinton, but these four were at the last unfortunately unable to be present. In the course of his able address the President touched on many points that were coming on for discussion during the meeting, and he thus, as it were, struck the keynote of the proceedings. Taking it as a whole the meeting was distinctly archæological in character, and it will probably be found that the giving of a distinctive character to a meeting will ensure a higher average of excellence in the papers than if the communications offered are left to chance. There is more likelihood of a number of distinguished men interested in a comparatively limited subject gathering to meet with one another by prearrangement, than the same number of equally competent men in various departments of Anthropology; but at the same time no department of Anthropology should be entirely unrepresented.

The range of the subjects dealt with at

the meeting will be evident from the following summary, in which no attempt is made to retain the order in which the papers were read.

Mr. Seton Karr exhibited a selection of the paleolithic implements he discovered in Somaliland, these form a remarkable series taken in conjunction with the types from India and Western Europe, and suggest either the extension of an associated people or a migration. Recent numerous finds of flint implements in North Ireland appear to throw back the age of man in Ireland further than the typical Neolithic period which is the limit usually acknowledged, but it is not yet generally accepted that the striæ on some of these implements are really of glacial origin. Mr. W. J. Knowles brought forward evidence to show that at Whitepark Bay, County Antrim, Neolithic settlers carried away to sites among the sand hills, the weathered cores and flakes of palæolithic age from the raised beach and worked them up into fresh implements, which still show the older flaked surfaces; their newer surfaces, however, are still fresh. A lantern exhibition of photographs taken by Prof. W. A. Herdman, of the dolmens of Brittany, led to a discussion of their age. Most speakers dated them as being neolithic, but perhaps in some cases of later date, Prof. Boyd Dawkins, however, claimed them as belonging to the Bronze age. Mr. F. T. Elworthy recorded the very recent discovery of a cist burial in Somersetshire, of which he exhibited photographs. The man, judging from his skull, certainly belonged to the Roundbarrow or Bronze race, but the interment and the earthen vessel were more neolithic in character; perhaps he was a pioneer. The ancient forts or *brochs* of Scotland formed the subject of a paper by Miss Maclagan.

The occurrence of an European Copper age was more than once alluded to. Dr.