has found that in the latter county the lower zones are apparently absent, and the remaining portion, representing, probably, the lower part of the zone of *Bel. Brunsvicensis,* is characterized by the presence, among the marine fossils, of plant remains, chiefly fragments of a Wealden fern, *Weichselia* (*Mantelli*?), and by other indications of fluviatile influence, suggesting the beginning of a lateral change into Wealden conditions.[1]

With the well-recognized gradual development of fresh-water conditions in the Purbeck beds of the Wealden area towards the close of the Jurassic period, and somewhat similar indications of the reversal of this process in the top of the Weald Clay during the later stages of the Lower Cretaceous, and with the above-mentioned evidence for a lateral passage of part of the Lower Cretaceous marine sediments of the North of England into estuarine deposits further south, there seems every reason to believe that in the fresh-water or estuarine strata of the English Wealden the whole of the time-interval between the Portlandian and Aptien stages is represented, and that it would be equally erroneous to classify the series entirely with the Jurassic system or entirely with the Cretaceous if the hitherto recognized boundary of these systems in the equivalent marine deposits of other areas is to be maintained.

The deposits classed as Wealden in Belgium, Germany, and France appear to be much more restricted in vertical range than the English series, and to represent different parts of the period in different places, but nowhere to imply the same long continuance of fresh-water conditions in a single area.

NOTICES OF MEMOIRS.

I.—British Association for the Advancement of Science. Seventieth Annual Meeting, held at Bradford, September 5–12, 1900.

List of Papers read in Section C (Geology).

Professor W. J. Sollas, F.R.S., President.

President's Address. (See p. 449.)

Professor W. B. Scott.—Notes on the Geology and Palæontology of Patagonia. (See p. 470.)

Professor J. Joly, F.R.S.—On the Viscous Softening of Rock-forming Minerals at Temperatures below their Normal Melting Points.

——— On the Geological Age of the Earth, as indicated by the Sodium-contents of the Sea.

——— Some Experiments on Denudation by Solution in Fresh and Salt Water.

——— On the Inner Mechanism of Marine Sedimentation.

Vaughan Cornish.—On Tidal Ripplemarks above Low-water Mark.

Dr. H. Woodward, F.R.S.—Remarks on a Table of Strata. (p. 474.)

Professor J. Milne, F.R.S.—Report of the Committee for Seismological Observations.

[1] See Survey Mem. "Borders of the Wash," o.s., sheet 69, pp. 21–25.

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

C. Reid, F.R.S. — Geological Notes on the Upway Disturbance (Appendix to the above Report).
S. W. Cuttriss.—The Caves and Potholes of Ingleborough and the District.
Rev. W. Lower Carter. — Report on the Underground Waters of North-West Yorkshire—the Sources of the Aire.
A. R. Dwerryhouse.—Report of the Committee on the Movement of Underground Waters of Craven. I. The Ingleborough District.
E. Greenly.—On Ancient Land-surfaces of Anglesey and Carnarvonshire.
——— On the Form of some Rock-bosses in Anglesey.
Dr. G. Abbott.—The Concretionary Types in the Magnesian Limestone of Durham.
Prof. T. Groom.—Pebbles of the Hollybush Conglomerate. (p. 471.)
——— On the Igneous Rocks associated with the Cambrian System of Malvern. (See p. 473.)
The President.—On a Concealed Coalfield beneath the London Basin.
R. H. Tiddeman.—On the Formation of Reef Knolls.
J. Lomas.—On the Construction and Uses of Strike Maps.
W. Gibson.—On Rapid Changes in the Thickness and Character of the Coal-measures of Staffordshire.
A. Smith Woodward.—Report of the Committee on the Registration of Type-specimens.
Rev. J. F. Blake.—Suggestions in regard to the Registration of Type Fossils. (See p. 471.)
C. B. Wedd.—The Outcrop of the Corallian Limestone of Elsworth and St. Ives.
H. Bolton.—Report of the Committee on Caves at Uphill, near Weston-super-Mare.
R. Lloyd Praeger.—Report of the Committee for the Exploration of Irish Caves.
Joint discussion with Section K on the Conditions during the Growth of the Forests of the Coal-measures. The discussion was opened by Mr. A. Strahan and Mr. J. E. Marr, F.R.S., on behalf of Section C, and by Mr. R. Kidston and Mr. A. C. Seward, F.R.S., on behalf of Section K.
Dr. E. D. Wellburn.—On the Fossil Fishes of the Yorkshire Coalfield.
——— On the Fish Fauna of the Millstone Grits of Great Britain.
J. J. H. Teall, F.R.S., Pres. G.S.—On the Plutonic Complex of Cnoc na Sroine, and its bearing on current Hypotheses as to the Genesis of Igneous Rocks.
Professor K. Busz.—On a Granophyre Dyke intrusive in the Gabbro of Ardnamurchan, Scotland. (See p. 436.)
Professor H. A. Miers, F.R.S.—Report of the Committee on the Present State of our Knowledge of the Structure of Crystals.
Dr. Wheelton Hind.—Report of the Committee on Life Zones in British Carboniferous Rocks.
Miss Igerna B. J. Sollas, B.Sc.—On *Naiadites* from the Upper Rhætic of Redland, Bristol.
F. W. Harmer.—The Influence of the Winds upon Climate during

past epochs: a Meteorological Explanation of some Geological Problems.

Dr. J. Monckman.—Notes on some recent Excavations in the Glacial Drift in Bradford.

J. E. Wilson.—On a Glacial Extra-Morainic Lake occupying the Valley of the Bradford Beck.

A. Jowett and H. B. Muff.—Notes on the Glaciation of the Keighley and Bradford Districts.

J. W. Stather.—The Source and Distribution of the Far-travelled Boulders of East Yorkshire.

———— On the Glacial Phenomena of the North-East Corner of the Yorkshire Wolds.

R. H. Tiddeman.—Raised Beaches of Gower, South Wales, and their relation to the Glacial Deposits. (See p. 441.)

Professor P. F. Kendall.—Report of the Committee on the Erratic Blocks of the British Isles.

Professor A. P. Coleman. — On a Ferriferous Horizon in the Huronian north of Lake Superior.

———— Final Report of the Committee on the Pleistocene Beds of Canada.

J. R. Dakyns.—Notes on the Glacial Geology of Snowdon.

R. D. Oldham.—Beach Formation in Thirlmere Reservoir. (See p. 473.)

———— Basal (Carboniferous) Conglomerate of Ullswater and its Mode of Formation.

Professor W. W. Watts.—Report of the Committee for the Collection and Preservation of Geological Photographs.

W. H. Crofts.—New Dock-sections at Hull.

A. C. Seward, F.R.S.—On the Jurassic Flora of the Yorkshire Coast.

G. W. Lamplugh.—The Age of the English Wealden Strata. (p. 443.)

P. M. C. Kermode.—Report of the Committee on the Irish Elk in the Isle of Man.

There was an excellent exhibition of geological specimens in the Museum adjoining the Section Room.

Papers read in other Sections bearing upon Geology.

Section A (Mathematical and Physical Science).

J. W. Gifford.—A Quartz-Calcite Symmetrical Doublet.

Professor A. W. Bickerton.—Cosmic Evolution.

Section B (Chemistry).

H. M. Dawson, D.Sc.—On the Influence of Pressure on the Formation of Oceanic Salt Deposits.

W. Ackroyd.—On a Limiting Standard of Acidity for Moorland Waters.

Section D (Zoology, including Physiology).

President's Address (Dr. R. H. Traquair, M.D., LL.D., F.R.S., F.G.S.).—The Bearings of Fossil Ichthyology on the Problem of Evolution. (See p. 463.)

Index Animalium.—Report by Committee: Dr. H. Woodward, F.R.S. Chairman; C. Davies Sherborn, Reporter. (See p. 473.)

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

Plankton Investigation.

Professor W. B. Scott.—The Miocene Fauna of Patagonia.

J. Stanley Gardiner.—Investigations upon the Coral Reefs of the Indian Region (Report).

SECTION E (GEOGRAPHY).

Vaughan Cornish.—On Snow Ripples and *Sastrugi.*

J. E. Marr, F.R.S.—The Origin of Moels, and their subsequent Dissection.

Dr. H. R. Mill.—The Treatment of Regional Geography.

E. G. Ravenstein.—Foreign and Colonial Surveys.

J. Milne, F.R.S.—Large Earthquakes recorded in 1899.

Report of Committee on Physical and Chemical Constants of Sea Water.

R. T. Günther.—On the possibility of obtaining more Reliable Measurements of the Changes of the Land-level of the Phlegræan Fields.

SECTION F (ECONOMIC SCIENCE AND STATISTICS).

Professor W. Saunders, LL.D.—Results of Experimental Work in Agriculture in Canada under Government Organization.

SECTION G (MECHANICAL SCIENCE).

J. Watson.—Water Supply, with a Description of the Bradford Waterworks.

J. H. Glass.—The Coal and Iron Ore Fields of Shansi and Honan, and Railway Construction in China.

SECTION H (ANTHROPOLOGY).

President's Address (*Professor John Rhys, M.A.*).—The Prehistoric Ethnology of the British Islands.

J. Paxton Moir.—Stone Implements of the Natives of Tasmania.

Professor E. B. Tylor, F.R.S.—The Stone Age in Tasmania as related to the History of Civilization.

D. G. Hogarth.—The Cave of Psychro in Crete.

A. M. Bell.—On the occurrence of Flint Implements of Palæolithic type on an old Land-surface in Oxfordshire, near Wolvercote and Pear-tree Hill, together with a few Implements of various Plateau types.

A. C. Haddon, Sc.D., F.R.S.—Relics of the Stone Age in Borneo.

SECTION K (BOTANY).

Dr. D. H. Scott, F.R.S.—On the presence of seed-like organs in certain Palæozoic Lycopods.

——— The Primary Structure of certain Palæozoic Stems referred to *Araucarioxylon.*

A. C. Seward, F.R.S., and Elizabeth Dale.—On the Structure of *Dipteris conjugata*, Rein, with notes on the geological history of the Dipteridinæ.

Professor Bower, F.R.S.—Illustrations of Sand-binding Plants.

W. C. Worsdell.—The Origin of Modern Cycads.

II.—British Association for the Advancement of Science. Bradford, 1900.

Address to the Geological Section, by Professor W. J. Sollas, D.Sc., LL.D., F.R.S., President of the Section. (Slightly abridged.)

Evolutional Geology.

THE close of one century, the dawn of another, may naturally suggest some brief retrospective glance over the path along which our science has advanced, and some general survey of its present position from which we may gather hope of its future progress; but other connection with geology the beginnings and endings of centuries have none. The great periods of movement have hitherto begun, as it were, in the early twilight hours, long before the dawn. Thus the first step forward, since which there has been no retreat, was taken by Steno in the year 1669; more than a century elapsed before James Hutton (1785) gave fresh energy and better direction to the faltering steps of the young science; while it was less than a century later (1863) when Lord Kelvin brought to its aid the powers of the higher mathematics and instructed it in the teachings of modern physics. From Steno onward the spirit of geology was catastrophic; from Hutton onward it grew increasingly uniformitarian; from the time of Darwin and Kelvin it has become evolutional. The ambiguity of the word 'uniformitarian' has led to a good deal of fruitless logomachy, against which it may be as well at once to guard by indicating the sense in which it is used here. In one way we are all uniformitarians, i.e. we accept the doctrine of the "uniform action of natural causes," but, as applied to geology, uniformity means more than this. Defined in the briefest fashion it is the geology of Lyell. Hutton had given us a "Theory of the Earth," in its main outlines still faithful and true, and this Lyell spent his life in illustrating and advocating; but, as so commonly happens, the zeal of the disciple outran the wisdom of the master, and mere opinions were insisted on as necessary dogma. What did it matter if Hutton, as a result of his inquiries into terrestrial history, had declared that he found no vestige of a beginning, no prospect of an end? It would have been marvellous if he had! Consider that when Hutton's "Theory" was published William Smith's famous discovery had not been made, and that nothing was then known of the orderly succession of forms of life, which it is one of the triumphs of geology to have revealed; consider, too, the existing state of physics at the time, and that the modern theories of energy had still to be formulated; consider also that spectroscopy had not yet lent its aid to astronomy, and the consequent ignorance of the nature of nebulæ: and then, if you will, cast a stone at Hutton. . . . .

Our science has become evolutional, and in the transformation has grown more comprehensive: her petty parochial days are done, she is drawing her provinces closer around her, and is fusing them together into a united and single commonwealth—the science of the earth.

Not merely the earth's crust, but the whole of earth-knowledge is the subject of our research. To know all that can be known about our planet, this, and nothing less than this, is its aim and scope. From the morphological side geology inquires not only into the existing form and structure of the earth, but also into the series of successive morphological states through which it has passed in a long and changeful development. Our science inquires also into the distribution of the earth in time and space; on the physiological side it studies the movements and activities of our planet; and not content with all this, it extends its researches into ætiology and endeavours to arrive at a science of causation. In these pursuits geology calls all the other sciences to her aid. In our commonwealth there are no outlanders; if an eminent physicist enter our territory we do not begin at once to prepare for war, because the very fact of his undertaking a geological inquiry of itself confers upon him all the duties and privileges of citizenship. A physicist studying geology is by definition a geologist. Our only regret is, not that physicists occasionally invade our borders, but that they do not visit us oftener and make closer acquaintance with us.

Early History of the Earth: First Critical Period.

If I am bold enough to assert that cosmogony is no longer alien to geology, I may proceed further, and taking advantage of my temerity pass on to speak of things once not permitted to us. I propose therefore to offer some short account of the early stages in the history of the earth. Into its nebular origin we need not inquire—that is a subject for astronomers. We are content to accept the infant earth from their hands as a molten globe ready made, its birth from a gaseous nebula duly certified. If we ask, as a matter of curiosity, what was the origin of the nebula, I fear even astronomers cannot tell us. There is an hypothesis which refers it to the clashing of meteorites, but in the form in which this is usually presented it does not help us much. Such meteorites as have been observed to penetrate our atmosphere and to fall on to the surface of the earth prove on examination to have had an eventful history of their own, of which not the least important chapter was a passage through a molten state; they would thus appear to be the products rather than the progenitors of a nebula.

We commence our history, then, with a rapidly rotating molten planet, not impossibly already solidified about the centre, and surrounded by an atmosphere of great depth, the larger part of which was contributed by the water of our present oceans, then existing in a state of gas. This atmosphere, which exerted a pressure of something like 5,000 lb. to the square inch, must have played a very important part in the evolution of our planet. The molten exterior absorbed it to an extent which depended on the pressure, and which may some day be learnt from experiment. Under the influence of the rapid rotation of the earth the atmosphere would be much deeper in equatorial than polar regions, so that in the latter the loss of heat by radiation would be in excess. This might of itself lead to convectional currents in the molten ocean. The effect on the atmosphere is very difficult to trace, but it is obvious that if a high-pressure area originated over some cooler region of the ocean, the winds blowing out of it would drive before them the cooler superficial layers of molten material, and as these were replaced by hotter lava streaming from below, the tendency would be to convert the high- into a low-pressure area, and to reverse the direction of the winds. Conversely, under a low-pressure area, the in-blowing winds would drive in the cooler superficial layers of molten matter that had been swept away from the anticyclones. If the difference in pressure under the cyclonic and anticyclonic areas were considerable, some of the gas absorbed under the anticyclones might escape beneath the cyclones, and in a later stage of cooling might give rise to vast floating islands of scoria. Such islands might be the first foreshadowings of the future continents. Whatever the ultimate effect of the reaction of the winds on the currents of the molten ocean, it is probable that some kind of circulation was set up in the latter. The universal molten ocean was by no means homogeneous: it was constantly undergoing changes in composition as it reacted chemically with the internal metallic nucleus: its currents would streak the different portions out in directions which in the northern hemisphere would run from N.E. to S.W., and thus the differences which distinguish particular petrological regions of our planet may have commenced their existence at a very early stage. Is it possible that as our knowledge extends we shall be able by a study of the distribution of igneous rocks and minerals to draw some conclusions as to the direction of these hypothetical lava currents? Our planet was profoundly disturbed by tides, produced by the sun, for as yet there was no moon; and it has been suggested that one of its tidal waves rose to a height so great as to sever its connection with the earth and to fly off as the infant moon. This event may be regarded as marking the first critical period, or catastrophe if we please, in the history of our planet. The career of our satellite, after its escape from the earth, is not known till it attained a distance of nine terrestrial radii; after this its progress can be clearly followed. At the eventful time of parturition the earth was rotating, with a period of from two to four hours, about an axis inclined at some 11° or 12° to the ecliptic. The time which has elapsed since the moon occupied a position nine terrestrial radii distant from the earth is at least fifty-six to fifty-seven millions of years, but may have been much more. Professor Darwin's story of the moon is certainly one of the most beautiful contributions ever made by astronomy to geology, and we shall all concur with him when he says, "A theory reposing on *veræ causæ*, which brings into quantitative correlation the length of the present day and month, the obliquity of the ecliptic, and the inclination and eccentricity of the lunar orbit, must, I think, have strong claims to acceptance."

The majority of geologists have long hankered after a metallic nucleus for the earth, composed chiefly, by analogy with meteorites, of iron. Lord Kelvin has admitted the probable existence of some such nucleus, and lately Professor Wiechert has furnished us with arguments—"powerful" arguments Professor Darwin terms them—in support of its existence. . . . .

The outer envelope of the earth which was drawn off to form the moon was, as we have seen, charged with steam and other gases under a pressure of 5,000 lb. to the square inch; but as the satellite wandered away from the parent planet this pressure continuously diminished. Under these circumstances the moon would become as explosive as a charged bomb, steam would burst forth from numberless volcanoes, and while the face of the moon might thus have acquired its existing features the ejected material might possibly have been shot so far away from its origin as to have acquired an independent orbit. If so, we may ask whether it may not be possible that the meteorites, which sometimes descend upon our planet, are but portions of its own envelope returning to it. The facts that the average specific gravity of those meteorites which have been seen to fall is not much above 3·2, and that they have passed through a state of fusion, are consistent with this suggestion.

Second Critical Period: "Consistentior Status."

The solidification of the earth probably became completed soon after the birth of the moon. The temperature of its surface at the time of consolidation was about 1170° C., and it was therefore still surrounded by its primitive deep atmosphere of steam and other gases. This was the second critical period in the history of the earth, the stage of the "consistentior status," the date of which Lord Kelvin would rather know than that of the Norman Conquest, though he thinks it lies between twenty and forty millions of years ago, probably nearer twenty than forty.

Now that the crust was solid there was less reason why movements of the atmosphere should be unsteady, and definite regions of high and low pressure might have been established. Under the high-pressure areas the surface of the crust would be depressed; correspondingly, under the low-pressure areas it would be raised; and thus from the first the surface of the solid earth might be dimpled and embossed.

Third Critical Period: Origin of the Oceans.

The cooling of the earth would continuously progress, till the temperature of the surface fell to 370° C., when that part of the atmosphere which consisted of steam would begin to liquefy; then the dimples on the surface would soon become filled with superheated water, and the pools so formed would expand and deepen till they formed the oceans. This is the third critical stage in the history of the earth, dating, according to Professor Joly, from between eighty and ninety millions of years ago. With the growth of the oceans the distinction between land and sea arose—in what precise manner we may proceed to inquire. . . . .

The ocean when first formed would consist of highly heated water, and this, as is well known, is an energetic chemical reagent when brought into contact with silicates like those which formed the primitive crust. As a result of its action saline solutions and chemical deposits would be formed; the latter, however, would probably be of no great thickness, for the time occupied by the ocean in cooling to a temperature not far removed from the present would probably be included within a few hundreds of years.

The Stratified Series.

The course of events now becomes somewhat obscure, but sooner or later the familiar processes of denudation and deposition started into activity, and have continued acting uninterruptedly ever since. The total maximum thickness of the sedimentary deposits, so far as I can discover, appears to amount to no less than 50 miles, made up as follows:—

	Feet.	
Recent and Pleistocene ...	4,000	Man.
Pliocene	5,000	Pithecanthropus.
Miocene	9,000	
Oligocene	12,000	
Eocene	12,000	Eutheria.
Cretaceous	14,000	
Jurassic	8,000	
Trias	13,000	Mammals.
Permian	12,000	Reptiles.
Carboniferous	24,000	Amphibia.
Devonian	22,000	Fish.
Silurian	15,000	
Ordovician	17,000	
Cambrian	16,000	Invertebrata.
Keweenawan	50,000	
Penokee	14,000	
Huronian	18,000	

Geologists, impressed with the tardy pace at which sediments appear to be accumulating at the present day, could not contemplate this colossal pile of strata without feeling that it spoke of an almost inconceivably long lapse of time. They were led to compare its duration with the distances which intervene between the heavenly bodies; but while some chose the distance of the nearest fixed star as their unit, others were content to measure the years in terms of miles from the sun.

Evolution of Organisms.

The stratified rocks were eloquent of time, and not to the geologist alone, they appealed with equal force to the biologist. Accepting Darwin's explanation of the origin of species, the present rate at which form flows to form seemed so slow as almost to amount to immutability. How vast, then, must have been the period during which by slow degrees and innumerable stages the protozoon was transformed into the man! And if we turn to the stratified column what do we find? Man, it is true, at the summit, the oldest fossiliferous rocks 34 miles lower down, and the fossils they contain already representing most of the great classes of the Invertebrata, including Crustacea and Worms. Thus the evolution of the Vertebrata alone is known to have occupied a period represented by

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

a thickness of 34 miles of sediment. How much greater, then, must have been the interval required for the elaboration of the whole organic world! . . . .

Geologic Periods of Time.

Before proceeding to the discussion of estimates of time drawn from a study of stratified rocks, let us first consider those which have been already suggested by other data. These are as follows:—(1) Time which has elapsed since the separation of the earth and moon, fifty-six millions of years, minimum estimate by Professor G. H. Darwin. (2) Since the "consistentior status," twenty to forty millions (Lord Kelvin). (3) Since the condensation of the oceans, eighty to ninety millions, maximum estimate by Professor J. Joly.

It may be at once observed that these estimates, although independent, are all of the same order of magnitude, and so far confirmatory of each other. Nor are they opposed to conclusions drawn from a study of stratified rocks; thus Sir Archibald Geikie, in his Address to this Section last year, affirmed that, so far as these were concerned, 100 millions of years might suffice for their formation. There is, then, very little to quarrel about, and our task is reduced to an attempt, by a little stretching and a little paring, to bring these various estimates into closer harmony. . . .

A review of the facts before us seems to render some reduction in Dr. Joly's estimate imperative. A precise assessment is impossible, but I should be inclined myself to take off some ten or thirty millions of years.

We may next take the evidence of the stratified rocks. Their total maximum thickness is, as we have seen, 265,000 feet, and consequently, if they accumulated at the rate of one foot in a century, as evidence seems to suggest, more than twenty-six millions of years must have elapsed during their formation.

Obscure Chapter in the Earth's History.

Before discussing the validity of the argument on which this last result depends, let us consider how far it harmonizes with previous ones. It is consistent with Lord Kelvin's and Professor Darwin's, but how does it accord with Professor Joly's? Supposing we reduce his estimate to fifty-five millions: what was the earth doing during the interval between the period of fifty-five millions of years ago and that of only 26½ millions ago, when, it is presumed, sedimentary rocks commenced to be formed? Hitherto we have been able to reason on probabilities; now we enter the dreary region of possibilities, and open that obscure chapter in the history of the earth previously hinted at. For there are many possible answers to this question. In the first place, the evidence of the stratified rocks may have been wrongly interpreted, and two or three times the amount of time we have demanded may have been consumed in their formation. This is a very obvious possibility, yet again our estimate concerning these rocks may be correct, but we may have erroneously omitted to take into account certain portions of the

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

Archæan complex, which may represent primitive sedimentary rocks, formed under exceptional conditions, and subsequently transformed under the influence of the internal heat of the earth. This, I think, would be Professor Bonney's view, Finally, Lord Kelvin has argued that the life of the sun as a luminous star is even more briefly limited than that of our oceans. In such a case, if our oceans were formed fifty-five millions of years ago, it is possible that after a short existence as almost boiling water they grew colder and colder, till they became covered with thick ice, and moved only in obedience to the tides. The earth, frozen and dark, except for the red glow of her volcanoes, waited the coming of the sun, and it was not till his growing splendour had banished the long night that the cheerful sound of running waters was heard again in our midst. Then the work of denudation and deposition seriously recommenced, not to cease till the life of the sun is spent. Thus the thickness of the stratified series may be a measure rather of the duration of sunlight than of the period which has elapsed since the first formation of the ocean. It may have been so—we cannot tell —but it may be fairly urged that we know less of the origin, history, and constitution of the sun than of the earth itself, and that, for aught we can say to the contrary, the sun may have been shining on the just-formed ocean as cheerfully as he shines to-day.

Time required for the Evolution of the Living World.

But, it will be asked, how far does a period of twenty-six millions satisfy the demands of biology? Speaking only for myself, although I am aware that eminent biologists are not wanting who share this opinion, I answer, "amply." But it will be exclaimed, "surely there are 'comparisons in things.'" Look at Egypt, where more than 4,000 years since the same species of man and animals lived and flourished as to-day. Examine the frescoes and study the living procession of familiar forms they so faithfully portray, and then tell us, how comes it about that from changes so slow as to be inappreciable in the lapse of forty centuries you propose to build up the whole organic world in the course of a mere twenty-six millions of years? To all which we might reply that even changeless Egypt presents us with at least one change — the features of the ruling race are to-day not quite the same as those of the Pharaohs. But, putting this on one side, the admitted constancy in some few common forms proves very little, for, so long as the environment remains the same natural selection will conserve the type, and, so far as we are able to judge, conditions in Egypt have remained remarkably constant for a long period.

Change the conditions, and the resulting modification of the species becomes manifest enough; and in this connection it is only necessary to recall the remarkable mutations observed and recorded by Professor Weldon in the case of the crabs in Plymouth Harbour. In response to increasing turbidity of the sea-water these crabs have undergone or are undergoing a change in the relative dimensions of the carapace, which is persistent, in one direction, and rapid enough to be determined by measurements made at intervals of a few years.

Again, animals do not all change their characters at the same rate: some are stable, in spite of changing conditions, and these have been cited to prove that none of the periods we look upon as probable, not twenty-five, not a hundred millions of years, scarce any period short of eternity, is sufficient to account for the evolution of the living world. If the little tongue-shell, *Lingula*, has endured with next to no perceptible change from the Cambrian down to the present day, how long, it is sometimes inquired, would it require for the evolution of the rest of the animal kingdom? The reply is simple: the cases are dissimilar, and the same record which assures us of the persistency of the *Lingula* tells us in language equally emphatic of the course of evolution which has led from the lower organisms upwards to man. In recent and Pleistocene deposits the relics of man are plentiful: in the latest Pliocene they have disappeared, and we encounter the remarkable form *Pithecanthropus*; as we descend into the Tertiary systems the higher mammals are met with, always sinking lower and lower in the scale of organization as they occur deeper in the series, till in the Mesozoic deposits they have entirely disappeared, and their place is taken by the lower mammals, a feeble folk, offering little promise of the future they were to inherit. Still lower, and even these are gone; and in the Permian we encounter reptiles and the ancestors of reptiles, probably ancestors of mammals too; then into the Carboniferous, where we find amphibians, but no true reptiles; and next into the Devonian, where fish predominate, after making their earliest appearance at the close of the Silurian times; thence downwards, and the vertebrata are no more found—we trace the evolution of the invertebrata alone. Thus the orderly procession of organic forms follows in precisely the true phylogenetic sequence: invertebrata first, then vertebrates, at first fish, then amphibia, next reptiles, soon after mammals, of the lowlier kinds first, of the higher later, and these in increasing complexity of structure till we finally arrive at man himself. While the living world was thus unfolding into new and nobler forms, the immutable *Lingula* simply perpetuated its kind. To select it, or other species equally sluggish, as the sole measure of the rate of biologic change, would seem as strange a proceeding as to confound the swiftness of a river with the stagnation of the pools that lie beside its banks. It is occasionally objected that the story we have drawn from the palæontological record is mere myth or is founded only on negative evidence. Cavils of this kind prove a double misapprehension, partly as to the facts, partly as to the value of negative evidence, which may be as good in its way as any other kind of evidence.

Geologists are not unaware of the pitfalls which beset negative evidence, and they do not conclude from the absence of fossils in the rocks which underlie the Cambrian that pre-Cambrian periods were devoid of life; on the contrary, they are fully persuaded that the seas of those times were teeming with a rich variety of invertebrate forms. How is it that, with the exception of some few species found in beds immediately underlying the Cambrian, these have left behind no vestige of their existence? The explanation does not lie in the nature of the sediments, which are not unfitted for the preservation of fossils, nor in the composition of the then existing sea-water, which may have contained quite as much calcium carbonate as occurs in our present oceans; and the only plausible supposition would appear to be that the organisms of that time had not passed beyond the stage now represented by the larvæ of existing invertebrata, and consequently were either unprovided with skeletons or at all events with skeletons durable enough for preservation. If so, the history of the earlier stages of the evolution of the invertebrata will receive no light from palæontology; and no direct answer can be expected to the question whether, eighteen or nineteen millions of years being taken as sufficient for the evolution of the vertebrata, the remaining available eight millions would provide for that of the invertebrate classes which are represented in the lowest Cambrian deposits. On *à priori* grounds there would appear to be no reason why it should not. If two millions of years afforded time enough for the conversion of fish into amphibians, a similar period should suffice for the evolution of trilobites from annelids, or of annelids from trochospheres. The step from gastrulas to trochospheres might be accomplished in another two millions, and two millions more would take us from gastrulas through morulas to protozoa.

As things stand, biologists can have nothing to say either for or against such a conclusion: they are not at present in a position to offer independent evidence; nor can they hope to be so until they have vastly extended those promising investigations which they are only now beginning to make into the rate of the variation of species.

Unexpected Absence of Thermal Metamorphosis in Ancient Rocks.

Two difficulties now remain for discussion: one based on theories of mountain chains, the other on the unaltered state of some ancient sediments. The latter may be taken first. Professor van Hise writes as follows regarding the pre-Cambrian rocks of the Lake Superior district: "The Penokee series furnishes an instructive lesson as to the depth to which rocks may be buried and yet remain but slightly affected by metamorphosis. The series itself is 14,000 feet thick. It was covered before being upturned with a great thickness of Keweenaw rock. This series at the Montreal River is estimated to be 50,000 feet thick. Adding to this the known thickness of the Penokee series, we have a thickness of 64,000 feet. . . . . The Penokee rocks were then buried to a great depth, the exact amount depending upon their horizon and upon the stage in Keweenaw time, when the tilting and erosion, which brought them to the surface, commenced.

"That the synclinal trough of Lake Superior began to form before the end of the Keweenaw period, and consequently that the Penokee rocks were not buried under the full succession, is more than probable. However, they must have been buried to a great depth—at least several miles—and thus subjected to high pressure and temperature, notwithstanding which they are comparatively unaltered."[1]

I select this example because it is one of the best instances of a difficulty that occurs more than once in considering the history of sedimentary rocks. On the supposition that the rate of increment of temperature with descent is 1° F. for every 84 feet, or 1° C. for every 150 feet, and that it was no greater during these early Penokee times, then at a depth of 50,000 feet the Penokee rocks would attain a temperature of nearly 333° C.; and since water begins to exert powerful chemical action at 180° C. they should, on the theory of a solid cooling globe, have suffered a metamorphosis sufficient to obscure their resemblance to sedimentary rocks. Either, then, the accepted rate of downward increase of temperature is erroneous, or the Penokee rocks were never depressed, in the place where they are exposed to observation, to a depth of 50,000 feet. Let us consider each alternative, and in the first place let us apply the rate of temperature increment determined by Professor Agassiz in this very Lake Superior district: it is 1° C. for every 402 feet, and twenty-five millions of years ago, or about the time when we may suppose the Penokee rocks were being formed, it would be 1° C. for every 305·5 feet, with a resulting temperature at a depth of 50,000 feet of 163° C. only. Thus the admission of a very low rate of temperature increment would meet the difficulty; but, on the other hand, it would involve a period of several hundreds of millions of years for the age of the "consistentior status," and thus greatly exceed Professor Joly's maximum estimate of the age of the oceans. We may therefore turn to the second alternative. As regards this, it is by no means certain that the exposed portion of the Penokee series ever was depressed 50,000 feet: the beds lie in a synclinal, the base of which indeed may have sunk to this extent, and entered a region of metamorphosis; but the only part of the system that lies exposed to view is the upturned margin of the synclinal, and as to this it would seem impossible to make any positive assertion as to the depth to which it may or may not have been depressed. To keep an open mind on the question seems our only course for the present, but difficulties like this offer a promising field for investigation.

The Formation of Mountain Ranges.

It is frequently alleged that mountain chains cannot be explained on the hypothesis of a solid earth cooling under the conditions and for the period we have supposed. This is a question well worthy of consideration, and we may first endeavour to picture to ourselves the conditions under which mountain chains arise. The floor of the ocean lies at an average depth of 2,000 fathoms below the land, and is maintained at a constant temperature, closely approaching 0° C., by the passage over it of cold water creeping from the polar regions. The average temperature of the surface of the land is above zero, but we can afford to disregard the difference in temperature between it

[1] Tenth Annual Report U.S. Geol. Survey, 1888–89, p. 457.

and the ocean floor, and may take them both at zero. Consider next the increase of temperature with descent, which occurs beneath the continents: at a depth of 13,000 feet, or at the same depth as the ocean floor, a temperature of 87° C. will be reached on the supposition that the rate of increase is 1° C. for 150 feet, while with the usually accepted rate of 1° C. for 108 feet it would be 120° C. But at this depth the ocean floor, which is on the same spherical surface, is at 0° C. Thus surfaces of equal temperature within the earth's crust will not be spherical, but will rise or fall beneath an imaginary spherical or spheroidal surface according as they occur beneath the continents or the oceans. No doubt at some depth within the earth the departure of isothermal surfaces from a spheroidal form will disappear; but considering the great breadth both of continents and oceans this depth must be considerable, possibly even forty or fifty miles. Thus the sub-continental excess of temperature may make itself felt in regions where the rocks still retain a high temperature, and are probably not far removed from the critical fusion-point. The effect will be to render the continents mobile as regards the ocean floor; or, *vice versâ*, the ocean floor will be stable compared with the continental masses. Next it may be observed that the continents pass into the bed of the ocean by a somewhat rapid flexure, and that it is over this area of flexure that the sediments denuded from the land are deposited. Under its load of sediment the sea-floor sinks down, subsiding slowly, at about the same rate as the thickness of sediment increases; and, whether as a consequence or a cause, or both, the flexure marking the boundary of land and sea becomes more pronounced. A compensating movement occurs within the earth's crust, and solid material may flow from under the subsiding area in the direction of least resistance, possibly towards the land. At length, when some thirty or forty thousand feet of sediment have accumulated in a basin-like form, or, according to our reckoning, after the lapse of three or four millions of years, the downward movement ceases, and the mass of sediment is subjected to powerful lateral compression, which, bringing its borders into closer proximity by some ten or thirty miles, causes it to rise in great folds high into the air as a mountain chain.

It is this last phase in the history of mountain-making which has given geologists more cause for painful thought than probably any other branch of their subject, not excluding even the age of the earth. It was at first imagined that during the flow of time the interior of the earth lost so much heat, and suffered so much contraction in consequence, that the exterior, in adapting itself to the shrunken body, was compelled to fit it like a wrinkled garment. This theory, indeed, enjoyed a happy existence till it fell into the hands of mathematicians, when it fared very badly, and now lies in a pitiable condition neglected by its friends.[1]

For it seemed proved to demonstration that the contraction consequent on cooling was wholly, even ridiculously, inadequate to

[1] With some exceptions, notably Mr. C. Davison, a consistent supporter of the theory of contraction.

explain the wrinkling. But when we summon up courage to inquire into the data on which the mathematical arguments are based, we find that they include several assumptions the truth of which is by no means self-evident. . . . .

We shall boldly assume that the contraction at some unknown depth in the interior of the earth is sufficient to afford the explanation we seek. The course of events may then proceed as follows. The contraction of the interior of the earth, consequent on its loss of heat, causes the crust to fall upon it in folds, which rise over the continents and sink under the oceans, and the flexure of the area of sedimentation is partly a consequence of this folding, partly of overloading. By the time a depression of some 30,000 or 40,000 feet has occurred along the ocean border the relation between continents and oceans has become unstable, and readjustment takes place, probably by a giving way of the continents, and chiefly along the zone of greatest weakness, i.e. the area of sedimentation, which thus becomes the zone of mountain-building. It may be observed that at great depths readjustment will be produced by a slow flowing of solid rock, and it is only comparatively near the surface, five or ten miles at the most below, that failure of support can lead to sudden fracture and collapse; hence the comparatively superficial origin of earthquakes.

Given a sufficiently large coefficient of expansion—and there is much to suggest its existence—and all the phenomena of mountain ranges become explicable: they begin to present an appearance that invites mathematical treatment; they inspire us with the hope that from a knowledge of the height and dimensions of a continent and its relations to the bordering ocean we may be able to predict when and where a mountain chain should arise, and the theory which explains them promises to guide us to an interpretation of those worldwide unconformities which Suess can only account for by a transgression of the sea. Finally, it relieves us of the difficulty presented by mountain formation in regard to the estimated duration of geological time.

Influence of Variations in the Eccentricity of the Earth's Orbit.

This may perhaps be the place to notice a highly interesting speculation which we owe to Professor Blytt, who has attempted to establish a connection between periods of readjustment of the earth's crust and variations in the eccentricity of the earth's orbit. Without entering into any discussion of Professor Blytt's methods, we may offer a comparison of his results with those that follow from our rough estimate of one foot of sediment accumulated in a century.

Table showing the time that has elapsed since the Beginning of the Systems in the first column, as reckoned from Thickness of Sediment in the second column, and by Professor Blytt in the third:—

	Years.	Years.
Eocene	4,200,000	3,250,000
Oligocene	3,000,000	1,810,000
Miocene	1,800,000	1,160,000
Pliocene	900,000	700,000
Pleistocene	400,000	350,000

It is now time to return to the task, too long postponed, of discussing the data from which we have been led to conclude that a probable rate at which sediments have accumulated in places where they attain their maximum thickness is one foot per century.

Rate of Deposition of Sediment.

We owe to Sir Archibald Geikie a most instructive method of estimating the existing rate at which our continents and islands are being washed into the sea by the action of rain and rivers: by this we find that the present land surface is being reduced in height to the extent of an average of $\frac{1}{2400}$ foot yearly.[1] If the material removed from the land were uniformly distributed over an area equal to that from which it had been derived, it would form a layer of rock $\frac{1}{2400}$ foot thick yearly, i.e., the rates of denudation and deposition would be identical. But the two areas, that of denudation and that of deposition, are seldom or never equal, the latter as a rule being much the smaller. Thus the area of that part of North America which drains into the Gulf of Mexico measures 1,800,000 square miles; the area over which its sediments are deposited is, so far as I can gather from Professor Agassiz' statements, less than 180,000 square miles, while Mr. McGee estimates it at only 100,000 square miles. Using the larger number, the area of deposition is found to measure one-tenth the area of denudation; the average rate of deposition will therefore be ten times as great as the rate of denudation, or $\frac{1}{240}$ foot may be supposed to be uniformly distributed over the area of sedimentation in the course of a year. But the thickness by which we have measured the strata of our geological systems is not an average but a maximum thickness; we have therefore to obtain an estimate of the maximum rate of deposition. If we assume the deposited sediments to be arranged somewhat after the fashion of a wedge with the thin end seawards, then twice the average would give us the maximum rate of deposition: this would be one foot in 120 years. But the sheets of deposited sediment are not merely thicker towards the land, thinner towards the sea, they also increase in thickness towards the rivers in which they have their source, so that a very obtuse-angled cone, or, better, the down-turned bowl of a spoon, would more nearly represent their form. This form tends to disappear under the action of waves and currents, but a limit is set to this disturbing influence by the subsidence which marks the region opposite the mouth of a large river. By this the strata are gradually let downwards, so that they come to assume the form of the bowl of a spoon turned upwards. Thus a further correction is necessary if we are to arrive at a fair estimate of the maximum rate of deposition. Considering the very rapid rate at which our ancient systems diminish in thickness when traced in all directions from the localities where they attain their maximum, it would appear that this correction must be a large one. If we reduce our already corrected estimate by one-fifth, we arrive at a rate of one foot of sediment deposited in a century. . . . .

[1] According to Professor Penck $\frac{1}{3600}$ foot.

It may be objected that in framing our estimate we have taken into account mechanical sediments only, and ignored others of equal importance, such as limestone and coal. With regard to limestone, its thickness in regions where systems attain their maximum may be taken as negligible; nor is the formation of limestone necessarily a slow process. The successful experiments of Dr. Allan, cited by Darwin, prove that reef-building corals may grow at the astonishing rate of six feet in height per annum.

In respect of coal there is much to suggest that its growth was rapid. The Carboniferous period well deserves its name, for never before, never since, have carbonaceous deposits accumulated to such a remarkable thickness or over such wide areas of the earth's surface. The explanation is doubtless partly to be found in favourable climatal conditions, but also, I think, in the youthful energy of a new and overmastering type of vegetation, which then for the first time acquired the dominion of the land. If we turn to our modern peat-bogs, the only carbonaceous growths available for comparison, we find from data given by Sir A. Geikie that a fairly average rate of increase is 6 feet in a century, which might perhaps correspond to one foot of coal in the same period.

The rate of deposition has been taken as uniform through the whole period of time recorded by stratified rocks; but lest it should be supposed that this involves a tacit admission of uniformity, I hasten to explain that in this matter we have no choice; we may feel convinced that the rate has varied from time to time, but in what direction, or to what extent, it is impossible to conjecture. That the sun was once much hotter is probable, but equally so that at an earlier period it was much colder; and even if in its youth all the activities of our planet were enhanced this fact might not affect the maximum thickness of deposits. An increase in the radiation of the sun, while it would stimulate all the powers of subaërial denudation, would also produce stronger winds and marine currents; stronger currents would also result from the greater magnitude and frequency of the tides, and thus while larger quantities of sediment might be delivered into the sea they would be distributed over wider areas, and the difference between the maximum and average thickness of deposits would consequently be diminished. Indications of such a wider distribution may perhaps be recognized in the Palæozoic systems. Thus we are compelled to treat our rate of deposition as uniform, notwithstanding the serious error this may involve. . . . .

If one foot in a century be a quantity so small as to disappoint the imagination of its accustomed exercise, let us turn to the Cambrian succession of Scandinavia, where all the zones recognized in the British series are represented by a column of sediment 290 feet in thickness. If 1,600,000 years be a correct estimate of the duration of Cambrian time, then each foot of the Scandinavian strata must have occupied 5,513 years in its formation. Are these figures sufficiently inconceivable?

In the succeeding system, that of the Ordovician, the maximum thickness is 17,000 feet. Its deposits are distributed over a wider

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

area than the Cambrian, but they also occupied longer time in their formation; hence the area from which they were derived need not necessarily have been larger than that of the preceding period.

Great changes in the geography of our area ushered in the Silurian system: its maximum thickness is found over the Lake district, and amounts to 15,000 feet; but in the little island of Gothland, where all the subdivisions of the system, from the Llandovery to the Upper Ludlow, occur in complete sequence, the thickness is only 208 feet. In Gothland, therefore, according to our computation, the rate of accumulation was one foot in 7,211 years.

With this example we must conclude, merely adding that the same story is told by other systems and other countries, and that, so far as my investigations have extended, I can find no evidence which would suggest an extension of the estimate I have proposed. It is but an estimate, and those who have made acquaintance with 'estimates' in the practical affairs of life will know how far this kind of computation may guide us to or from the truth.

III.—The Bearings of Fossil Ichthyology on the Problem of Evolution; being the Address to the Zoological Section. By Ramsay H. Traquair, M.D., LL.D., F.R.S., President of the Section. (Slightly abridged.)

I HAVE been told that an idea is prevalent in the minds of recent biologists that the results of Palæontology are so uncertain, so doubtful, and so imperfect, that they are scarcely worthy of serious attention being paid to them. The best answer I can make to such an opinion, if it really does exist, is to try to place before you some evidence that Palæontology is not mere fossil shell hunting, or the making up of long lists of names to help the geologists to settle their stratigraphical horizons, but may present us with abundance of matter of genuine biological interest.

Since the days of Darwin, there is one subject which more than all others engrosses the attention of scientific biologists. I mean the question of Evolution, or the Doctrine of Descent. From the nature of things it is clear that the voice of the palæontologist can only be heard on the morphological aspect of the question, but to many of us, including myself, the morphological argument is so convincing that we believe that even if the Darwinian theory were proved to-morrow to be utterly baseless, the Doctrine of Descent would not be in the slightest degree affected, but would continue to have as firm a hold on our minds as before.

Now as Palæontology takes us back, far back, into the life of the past, it might be reasonably expected that it would throw great light on the descent of animals, but the amount of its evidence is necessarily much diminished by two unfortunate circumstances. First, the terrible imperfection of the geological record, a fact so obvious to anyone having any acquaintance with geology that it need not be discussed here; and secondly, the circumstance that save in very exceptional cases only the hard parts of animals are preserved, and those too often in an extremely fragmentary and disjointed condition. But though we cannot expect that the palæontological record will ever be anything more than fragmentary, yet the constant occurrence of new and important discoveries leads us to entertain the hope that, in course of time, more and more of its pages will become disclosed to us.

Incomplete, however, as our knowledge of Evolution as derived from Palæontology must be, that is no reason why we should not appraise it at its proper value, and now and again stop for a moment to take stock of the material which has accumulated.

You are all already acquainted with the telling evidence in favour of Evolution furnished by the well-known series of Mammalian limbs, as well as of teeth, in which the progress, in the course of time, from the more general to the more special is so obvious that I cannot conceive of any unprejudiced person shutting his eyes to the inference that Descent with modification is the reason of these things being so. Suppose, then, that on this occasion we take up the palæontological evidence of Descent in the case of fishes. This I do the more readily because what original work I have been able to do has lain principally in the direction of fossil ichthyology; and again, because it does seem to me that it is in this department that one has most reason to complain of want of interest on the part of recent biologists, even, I may say, of some professed palæontologists themselves. I shall in the main limit myself to the consideration of Palæozoic forms.

Here I may begin by boldly affirming that I include the Marsipobranchii as fishes, in spite of the dictum of Cope that no animal can be a fish which does not possess a lower jaw and a shoulder-girdle. Why not? The position seems to me to be a merely arbitrary one; and it is, to say the least, not impossible that the modern Lampreys and Hags may be, as many believe, the degenerate descendants of originally gnathostomatous forms.

To the origin of the Vertebrata Palæontology gives us no clue, as the forerunners of the fishes must have been creatures which, like the lowest Chordata of the present day (Tunicata, *Balanoglossus*, *Amphioxus*), had no hard parts capable of preservation. And though I shall presently refer again to the subject, I may here affirm that, so far as I can read the record at least, it is impossible to derive from Palæontology any support to the view, recently revived, that the ancient fishes are in any way related to Crustacean or merostomatous ancestors.

What have we, then, to say concerning the most ancient fishes with which we are acquainted?

The idea that the minute bodies known as Conodonts, which occur from the Cambrian to the Carboniferous, are the teeth of fishes and possibly even of ancient Marsipobranchs may now be said to be given up. They are now accepted by the most reliable authorities as appertaining to Invertebrata such as Annelides and Gephyrea.

More recently, however, Rohon [1] has described from the Lower

[1] "Ueber untersilurische Fische": Mélanges Géol. et Paléont., vol. i (St. Petersburg, 1899), pp. 9–14.

Silurian of the neighbourhood of St. Petersburg small teeth (*Palæodus* and *Archodus*) associated with Conodonts, and which seem to be real fish teeth, but not of Selachians, as is shown by the presence of a pulp cavity surrounded by non-vascular dentine. It is impossible to say anything more of their affinities.

Obscure and fragmentary fish remains have been obtained by Walcott, and described by Jaekel, from rocks in Colorado supposed to be of Lower Silurian or Ordovician age.[1] But doubts have been thrown on their age, and the fossils themselves, which have, it must be owned, a very Devonian look about them, are so extremely fragmentary that they do not help us much in our present purpose.

It is not till we come to the Upper Silurian rocks that we begin to feel the ground securely under our feet, though we may be certain, from the degree of specialization of the forms which we there find, that fishes lived in the waters of the globe for long ages previously.

Characteristic of the 'Ludlow bone-bed' are certain minute scales on which Pander founded the family Cœlolepidæ, having a flat or sculptured crown, below which is a constricted 'neck,' and then a base usually perforated by an aperture leading into a central pulp cavity.

The genera *Thelodus, Cœlolepis,* and others were founded on these dermal bodies, and complete specimens of *Thelodus* have now been found in the Upper Silurian rocks of the South of Scotland, from which it is evident that the fish, though somewhat shark-like, can hardly be reckoned as a true Selachian.[2] *Thelodus Scoticus,* Traq., has a broad flattened anterior part corresponding to the head and forepart of the body, very bluntly rounded in front, and passing behind into right and left angular flap-like projections, which are sharply marked off from the narrow tail, which is furnished with a deeply cleft heterocercal caudal fin. Under the flap-like lateral projections are representatives of pectorals; no other fins are present, neither do we find any teeth or jaws, nor any trace of internal skeleton; and it is only a few days since Mr. Tait, collector to the Geological Survey of Scotland, pointed out to me in a recently acquired specimen a right and left dark spot at the outer margins of the head near the front, which spots may indicate the position of the eyes. A previously unknown genus, *Lanarkia,* Traq., also occurs in which the creature has the very same form, but instead of having the skin clothed with small shagreen-like scales, possesses in their place minute, sharp, conical, hollow spines, without base and open below. What we are to think of those two ancient forms, apparently so primitive and yet undoubtedly also to a great extent specialized, we shall presently discuss.

Let us now for a moment look at the genus *Drepanaspis,* Schlüter, from the Lower Devonian of Gmünden in Western Germany. We

[1] Bulletin Geol. Soc. America, vol. iii (1892), pp. 153–171.

[2] R. H. Traquair, "Report on Fossil Fishes collected by the Geological Survey in the Silurian Rocks of the South of Scotland": Trans. Roy. Soc. Edin., vol. xxxix (1899), pp. 827–864.

[3] R. H. Traquair: Geol. Mag., April, 1900.

have here a strange creature whose shape entirely reminds us of that of *Thelodus*, having the same flat broad anterior part, bluntly rounded in front and angulated behind, to which is appended a narrow tail ending in a heterocercal caudal fin, which is, however, scarcely bilobate. But here the dermal covering, instead of consisting of separate scales or spinelets, shows a close carapace of hard bony plates, of which two are especially large and prominent —the median dorsal and the median ventral—other large ones being placed around the margins, while the intervening space is occupied by a mosaic of small polygonal pieces. The position of the mouth, a transverse slit, is seen just at the anterior margin; it is bounded behind by a median mentum or chin-plate, but no jaws properly so called are visible, nor are there any teeth. Then on each margin near the front of the head is a small round pit, exactly in the position of the dark spot seen in some examples of *Thelodus*, which, if not an orbit, must indicate the position of some organ of sense. Again, the tail is covered with scales after the manner of a 'ganoid' fish, being rhombic on the sides, but assuming the form of long deeply imbricating fulcra on the dorsal and ventral margins. The position of the branchial opening, or openings, has not yet been definitely ascertained.

All these plates are closely covered with stellate tubercles, and we cannot escape from the conclusion that they are formed by the fusion of small shagreen bodies like those of *Thelodus*, and united to bony matter developed in a deeper layer of the skin.

If the angular lateral flaps of *Thelodus* represent pectoral fins, then we would have the exceedingly strange phenomenon of such structures becoming functionally useless by enclosure in hard unyielding plates, though still influencing the general outline of the fish. Be that as it may, can we doubt that in *Drepanaspis* we have a form derived by specialization from a Cœlolepid ancestor?

This *Drepanaspis* throws likewise a much desired light on the fragmentary Devonian remains known since Agassiz's time as *Psammosteus*. These consist of large plates and fragments of plates, composed of vaso-dentine, and sculptured externally by minute closely-set stellate tubercles, exactly resembling the scales of some species of *Thelodus*. These tubercles are also frequently arranged in small polygonal areas, reminding us exactly of the small polygonal plates of *Drepanaspis*, and, like them, often having a specially large tubercle in the centre. That *Psammosteus* had an ancestry similar to that of *Drepanaspis* can also hardly be doubted.

Finally, in the well-known *Pteraspis* of the Upper Silurian and Lower Devonian formations we have a creature which also has the head and anterior part of the body enveloped in a carapace, to which a tail covered with rhombic scales is appended behind, and, though the caudal fin has never been properly seen, such remains of it as have occurred distinctly indicate that it was heterocercal in its contour. The plates of the carapace have a striking resemblance in general arrangement to those of *Drepanaspis*, though the small polygonal pieces have disappeared, and there is a prominent pointed rostrum in front of the mouth; and it is to be noted that the small round apertures usually supposed to be orbits are in a position quite analogous to that of the sensory pits in *Drepanaspis*. The plates of the carapace of *Pteraspis* are not, however, tuberculated, but ornamented by fine close parallel ridges, the microscopic structure of which, along with their frequent lateral crenulation, leaves no doubt in our minds that they have been formed by the running together in lines of *Thelodus*-like shagreen grains. An aperture supposed to be branchial is seen on the plate forming the posterior angle of the carapace on each side.

Until these recent discoveries concerning the Cœlolepidæ and Drepanaspidæ, *Pteraspis* and its allies, *Cyathaspis*, and *Palæaspis* constituted the only family included in the order Heterostraci of the sub-class Ostracodermi, distinguished, as shown by Lankester, by the absence of bone lacunæ in the microscopic structure of their plates. It is now, however, clear that we can trace them back to an ancestral family in which the external dermal armature was still in the generalized form of separate shagreen grains or spinelets.

But the Ostracodermi are usually made to include two other groups or orders, namely the Osteostraci and the Asterolepida.[1]

The Osteostraci are distinguished from the Heterostraci by the possession of lacunæ in their bone structure, and by having the eyes in the middle of the head-shield instead of at the sides. *Cephalaspis*, which occurs from the Upper Silurian to the top of the Devonian, is the best known representative of this division. Instead of a carapace, we find a large head-shield of one piece, though its structure shows evidence of its having been originally composed of a mosaic of small polygonal plates, and it is also to be noted that the surface is ornamented by small tubercles, there frequently being one larger in size in the centre of each polygonal area. The posterior external angles of the shield project backwards in a right and left pointed process or *cornu*, scarcely developed in *C. Murchisoni*, internal to which, and also organically connected with the head-shield, is a rounded flap-like structure, which strongly reminds us of the lateral flaps of the Cœlolepidæ. The body is covered with scales, which on the sides are high and narrow; there is a small dorsal fin, and the caudal, though heterocercal, is not bilobate. It is scarcely necessary for me to add that we find just as little evidence of jaws or of teeth as in the case of the Heterostraci.

The association of the Heterostraci and Osteostraci in one sub-class of Ostracodermi has been strongly protested against by Professor Lankester and Dr. O. M. Reis, but here the Scottish Silurian strata come to the rescue with a form which I described last year under the name of *Ateleaspis tessellata*, and of which some more perfect examples than those at my disposal at that time have recently come to light through the labours of Mr. Tait, of the Geological Survey of Scotland.

[1] To these I myself recently added a fourth, the Anaspida, for the remarkable Upper Silurian family of Birkeniidæ, but as these throw no light as yet on the problem of Descent they may at present be only mentioned.

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

Here we have a creature whose general form reminds us strongly of *Thelodus*, but whose close affinity to *Cephalaspis* is absolutely plain, were it only on account of the indications of orbits on the top of the head.

The expanded anterior part which here represents the head-shield of *Cephalaspis* shows not the slightest trace of cornua, but forms posteriorly a gently rounded lobe on each side, clearly suggesting that the cornual flaps of *Cephalaspis* are homologous with and derivable from the lateral expanses in the Cœlolepidæ. This cephalic covering is composed of numerous small polygonal plates like those of which the head-shield in *Cephalaspis* no doubt originally consisted, and the minute tubercles which cover their outer surfaces also suggest that the superficial layer was formed by the fusion of Cœlolepid scales. The body is covered with rhombic scales, sculptured externally with tubercles and wavy transverse ridges, and arranged in lines having the same general direction as the scutes of *Cephalaspis*, from which we may infer that the latter originated from the fusion of scales of similar form. The fins are as in *Cephalaspis*, there being one small dorsal situated far back, and a heterocercal caudal, which is triangular in shape, and not deeply cleft into upper and lower lobes as in the Cœlolepidæ. Finally, the scales, on microscopic examination, show well-developed bone lacunæ in their internal structure.

That *Ateleaspis* belongs to the Osteostraci there is thus not the smallest doubt, but its general resemblance to the Cœlolepidæ in its contour anteriorly led me to regard it as an annectent form, and consequently to believe that there is after all a genuine genetic connection between the Heterostraci and the Osteostraci. And I have not seen reason to depart from that opinion even though *Ateleaspis* turns out to be still closer to *Cephalaspis* than was apparent in the original specimens.

If this be so, then *Cephalaspis*, as well as *Pteraspis* and its allies, is traceable to the Cœlolepidæ, shark-like creatures in which, as we have already seen, the dermal covering consists of small shagreen-like scales, or of minute hollow spines, and consequently all theories as to the arthropod origin of the Ostracodermi, so far as they are founded on the external configuration of the carapace in the more specialized forms, must fall to the ground. And from the close resemblance of these scales of *Thelodus* to Elasmobranch shagreen bodies—for forty-five years they had been, by most authors, actually referred to the Selachii—I concluded that the Cœlolepidæ owed their origin to some form of primitive Elasmobranchs. That is, however, not in accordance with the view of the late Professor Cope, that the Ostracodermi are more related to the Marsipobranchii, and that, from the apparent absence of lower jaw, they should be placed along with the last-named group in a class of Agnatha, altogether apart from the fishes proper. And Dr. Smith Woodward, who is inclined to favour Cope's theory, has expressed his view that the similarity of the Cœlolepid scales to Elasmobranch shagreen is no proof of an Elasmobranch derivation, but that such structures, representing the

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

simplest form of dermal hard parts, may have originated independently in far distant groups.[1] Knowing what we do of the occurrence of strange parallelisms in evolution, it would not be safe to deny such a possibility. But as to a Marsipobranch affinity, I would point out that the apparent want of lower jaw among the hard parts which nature has preserved for us is no proof of the absence of a Meckelian cartilage among the soft parts which are lost to us for ever; and also, as Professor Lankester has remarked, that there is no evidence whatever that any of the creatures classed together as Ostracodermi were monorhinal like the Lampreys. The only fossil vertebrate having a single median opening, presumably nasal, in the front of the head is *Palæospondylus,* but, whatever be the true affinities of this little creature, at present the subject of so much dispute, I think we may be very sure that it is not an Ostracoderm.

The Devonian 'Antiarcha' or Asterolepida, of which *Pterichthys* is the best known genus, are also usually placed in the Ostracodermi, with which they agree in the possession of a carapace of bony plates, in the absence of distinct lower jaw or teeth, in the non-preservation of internal skeleton, and in having a scaly tail furnished with a heterocercal caudal fin, and, as in the Cephalaspidæ, also with a small dorsal. But they have in addition a pair of singular jointed thoracic limbs, evidently organs of progression, which are totally unlike anything in the Osteostraci or in the Heterostraci, or indeed in any other group of fishes. These limbs are covered with bony plates and hollow inside, but though I once fancifully compared them in that respect with the limbs of insects, I must protest strongly against this expression of mine being quoted in favour of the arthropod theory of the derivation of the Vertebrata!

Nor do I think that there is any probability in the view published by Simroth nine years ago,[2] namely, that *Pterichthys* may have been a land animal which used its limbs for progression on dry ground, and that the origin of the heterocercal tail was the bending up of the extremity of the vertebral axis caused by its being dragged behind the creature in the act of walking. That view was promulgated before the discovery of the membranous expanse of the caudal fin in this genus.

But though the Asterolepida are apparently related to and inclusible in the Ostracodermi, the geological record is silent as to their immediate origin, no intermediate forms having been found connecting them more closely with either the Heterostraci or the Osteostraci. In the possession of bone lacunæ and of a dorsal fin they have a greater resemblance to the latter, but it may be looked upon as certain that they could have had no direct origin from that group.

As regards the Ostracodermi as a sub-class, they become extinct at the end of the Devonian epoch, and cannot be credited with any

[1] Geol. Mag., March, 1900.
[2] "Die Entstehung der Landthiere"; Leipzig, 1891.

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

share in the evolution of the fishes of more recent periods, not even if we restore the Coccosteans or Arthrodira to their fellowship. To the latter most enigmatical group, which I shall still continue to look upon as fishes, I shall make some reference further on.

(*To be continued in our next Number.*)

IV.—Notes on the Geology and Palæontology of Patagonia. By Professor W. B. Scott, Princeton University.[1]

FOR the past four years Princeton University has been conducting explorations in Patagonia under the direction of Mr. J. B. Hatcher. The large expenses of the undertaking have been defrayed by the generosity of friends in New York and Baltimore, and Mr. J. Pierpont Morgan has given the sum of £5,000 for the publication of the important results which Mr. Hatcher has obtained.

The oldest sedimentary formation observed is a marine Cretaceous found in the Cordillera; the Ammonites of this horizon have been studied by Mr. Stanton, and he reports that they indicate Gault age and show close relationship to the Uitenhage beds of South Africa.

The oldest marine Tertiaries are given in the section near the Straits of Magellan, and my assistant, Dr. Ortmann, informs me that the fossils point to a late Eocene or Oligocene age for these beds, which he has called the Magellanian beds. These are overlain by the great Patagonian formation, which is of wide extent, of marine origin, and richly fossiliferous. The 200 species of marine invertebrate fossils obtained from this horizon have been studied by Dr. Ortmann, and lead to some very interesting conclusions. In the first place they unequivocally demonstrate the Miocene age of the beds (not Cretaceous and oldest Eocene as Ameghino has maintained), and in the second place they display the closest resemblance to the Miocene of Australia and New Zealand, pointing to a shore connection with those countries in Miocene times. The Patagonian and supra-Patagonian stages are shown not to be distinguishable.

The Santa Cruz beds, a fresh-water and terrestrial formation, overlie and partially dovetail in with the Patagonian. They contain an incredibly abundant and varied mammalian fauna, of which a vast collection was brought home. This fauna has only a very remote connection with the Miocene mammals of the northern hemisphere, and strongly confirms Rütimeyer's contention of a southern centre of distribution. The presence of numerous carnivorous marsupials (there are no true Carnivora) is additional evidence of a connection, direct or indirect, with Australia. Unconformably overlying the Santa Cruz is another marine formation, discovered by Mr. Hatcher and by him named the Cape Fairweather beds. The fossils indicate the Pliocene age of these beds.

Mr. Hatcher's labours have thus resulted in proving that Patagonian geology is in complete accord with the system established for the northern hemisphere, and that it is not of such exceptional character as has been supposed.

[1] Read before the British Association, Section C (Geology), Bradford, Sept., 1900.

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

V.—The Pebbles of the Hollybush Conglomerate, and their bearing on Lower and Cambrian Palæogeography.[1] By Professor Theodore Groom, M.A., D.Sc.

THE Malvern Hills are commonly supposed to have formed part of an old coast during the deposition of the Lower Palæozoic beds. A preliminary examination of the materials of the Hollybush Conglomerate by the author does not support this view.

The most abundant pebbles consist of quartz; these vary from a coarse mosaic of crystals to a fine quartz-schist. Most of the varieties are probably of metamorphic origin; some appear to be merely vein-quartz, and some represent the quartzose portions of granites and other rocks. Red granites and granophyres, often crushed, are tolerably abundant; these often contain microcline. Mica-schist and chlorite-schist occur rarely. Very abundant are different varieties of felsite. These appear to be mostly micro- or cryptocrystalline, and often micrographic, rhyolites, compact or porphyritic; sometimes banded, and occasionally spherulitic. Some of the varieties may represent crushed intrusive felsites. Far rarer than the rhyolites are microlithic andesites, or andesitic basalts. Other pebbles, and the grains of the groundmass, consist of materials derivable from the rocks mentioned above.

The resemblance of these materials to the rocks of the Malvern range is sufficiently close to prove the Pre-Cambrian age of the latter. But striking differences in microscopic structure and in the proportionate numbers of corresponding rocks in the two series, and the absence of any relation between the local nature of the conglomerate and that of the Archæan mass nearest to it, can hardly be explained except on the assumption that the range itself did not furnish the materials.

The stratigraphical relations of the conglomerate and Archæan mass, moreover, appear to indicate that the Malvern Hills — the southern portion at least—in Cambrian times formed part of an area of deposition, and not of denudation.

The author maintains, then, that the Malvern Hills did not form a coastline in Cambrian times, a conclusion which is in agreement with his former contention that they arose at a much later date.

VI.—Suggestions in regard to the Registration of Type Fossils.[1] By the Rev. J. F. Blake, M.A., F.G.S.

WHEREAS:

1. There is now in existence, and has been for some time, a Committee of the British Association "to consider the best methods for the registration of all type-specimens of fossils in the British Isles."

2. There is as yet in course of production no general register of such specimens.

3. The original types are in many, perhaps the majority of, cases either lost, inaccessible, or inadequately preserved or described.

[1] Read before the British Association, Section C (Geology), Bradford, Sept., 1900.

4. Many names in common use have a foreign origin, which have not been adopted after actual comparison with the original foreign types.

5. Palæontological nomenclature consequently still remains burdened with names of uncertain value.

It is therefore advisable that:

1. The above-named Committee recognize and register a new class of 'types,' which may be either original or adopted, but which satisfy certain conditions laid down to ensure their having a definite value.

2. A register be published annually of such types, so that an author in using a name may have the option of quoting this register, instead of the original author's name.

3. This register should give references (1) to the author or authors, and their publications thereupon, who have first satisfied the required conditions; (2) to the museum where the type is deposited.

4. The limitation of types, registered by the British Association, should have reference to the type-specimens, whatever their origin, which are deposited in museums within the United Kingdom (possibly to be enlarged at a future date to the British Empire).

5. The Committee should, from time to time, determine the conditions required for registration, but should be in no way responsible for the validity of the 'species' to which the type may be said to belong, nor for the name under which it is registered, which registration should apply to the 'specific' name only, and not be affected by its reference later to another genus; the only care of the Committee, beyond seeing that the required conditions are satisfied, being to secure that identical diagnoses are not registered under different names, and that the same name is not used at different times for different diagnoses.

The suggested conditions for registration are as follows:—

1. A single specimen must be selected as the type, but two or more co-types may be admitted, which are identical in all other respects save in the preservation of different necessary characters.

2. The exact horizon and locality of the specimen thus selected must be known.

3. All the commonly called 'specific' characters required, in the class to which it belongs, must be known by the type or by the co-types together, and also described, and also the generic ones when the genus is not obvious. [N.B.—The determination whether this condition is carried out in any particular case will rest with the member of the Committee charged with the class.]

4. All characters capable of numerical statement, including size, proportion of parts or lines, angle, etc., must be so given. [N.B.—Adequate figures may suffice for this.]

5. The type-specimen must be permanently placed in a public museum in the United Kingdom.

N.B.—It is not necessary that the type-specimen in the above sense should be the first anywhere described under the registered name, but only the first that satisfies the above conditions.

https://doi.org/10.1017/S0016756800183359 Published online by Cambridge University Press

It is suggested that registered types should be quoted as B 1, B 13—e.g. *Terebratula biplicata*, B 1, or *Phacops caudatus*, B 13—B standing for British, and the number for that of the year of the century. Specimens differing notably from the type, but included in the same species, might be quoted as (B 1).

VII.—On the Igneous Rocks associated with the Cambrian Beds of Malvern.[1] By Professor Theodore Groom, M.A., D.Sc.

The igneous rocks of the Cambrian beds of the Southern Malverns have commonly been regarded as of volcanic origin. The author, after a careful examination of the rocks under the microscope, and of the ground, concludes that the scoriæ and tuffs previously described are non-existent, and that the whole of the igneous rocks are probably intrusive. They consist of sills and small laccolites of basic and ultra-basic olivine diabase and olivine basalt, in which olivine is often extremely abundant, and of bosses and dykes of peculiar amphibole bearing andesites and andesitic basalts. Intrusion probably took place in Ordovician times.

VIII.—Beach Formation in the Thirlmere Reservoir. By R. D. Oldham, F.G.S., Geological Survey of India.[1]

Readers of Mr. Marr's book on the "Scientific Study of Scenery" will recall the comparison made by the author between the irregular and angular outline of the Thirlmere Lake Reservoir, due to the submergence of a land surface shaped by subaërial denudation, and the more gracefully curved outline of the natural lakes, where wind, waves, and streams have combined to round off the angularities by wearing away the prominences and filling up the re-entering angles. This reproach seems to the author to be somewhat exaggerated, as the shore-lines of the Cumberland Lakes have only been partially remodelled by wave action and delta formation, and the original outlines due to simple submergence are still to be seen. However this may be, the reproach, such as it is, is in process of removal. All along the shore of the Thirlmere Lake incipient beach erosion is to be seen, and towards the northern end of the lake, where the shores close in and are exposed to the force of the waves driven along the length of the lake by the prevailing southerly winds, typical beaches and beach-curves are being developed. [Lantern-slides showing the as yet incompleted transition from the irregular outlines produced by submergence to the regular curves of beach-formation were exhibited, and attention drawn to this interesting opportunity of witnessing the gradual formation and growth of a beach.]

IX.—Report of the Committee appointed to compile an Index Animalium: Dr. Henry Woodward (Chairman), Mr. W. E. Hoyle, Mr. R. MacLachlan, Dr. P. L. Sclater, Rev. T. R. R. Stebbing, and Mr. F. A. Bather (Secretary).[1]

The Committee has the honour to report that this work has made very satisfactory progress in the hands of Mr. C. Davies Sherborn, and that the literature down to the year 1800 has now

[1] Read before the British Association, Section C (Geology), Bradford, Sept., 1900.

been sought out and indexed. The manuscript of this portion will be ready for the printer in a few weeks, and the Committee is considering the best form of publication and estimating the cost. Meanwhile the indexing of literature after 1800 is being continued. At this stage the Committee would be glad to receive suggestions or offers of help for the publication of this great work, since the sums hitherto so generously awarded to it are only sufficient for the necessary current expenses, which continue as before. The Committee therefore earnestly requests its reappointment with a grant of £100.

The following report on dates of publication has been issued by Mr. Sherborn during the year: Lacepède's "Tableaux des Mammifères et des Oiseaux," *Natural Science*, December, 1899. The Committee desires to thank M. Gadeau de Kerville, Dr. Rudolph Burckhardt, of Basel, Mr. F. Justen (Dulau & Co.), and the Smithsonian Institution of Washington for assistance concerning rare books wanted by the compiler.

X.—Woodward's Table of British Stratigraphical Geology and Palæontology; showing the Economic Products of each Formation.[1]

DR. HENRY WOODWARD exhibited on behalf of Mr. Horace Woodward and himself a coloured Table of British Strata, showing the major and minor subdivisions of the strata, with their thicknesses, the characteristic fossils met with in each, also their economic products, and the places throughout the British Isles where the several formations are exposed at the surface and can be studied. The Table will be published by Messrs. Dulau & Co., 37, Soho Square, W.

REVIEWS.

Summary of Progress of the Geological Survey of the United Kingdom for 1899. pp. 214. (London: printed for H.M. Stationery Office, 1900. Price 1*s*.)

THIS, the third issue of the "Summary of Progress," has the merit of being a trifle shorter than the previous number; and contains, as an Appendix, a very useful "Catalogue of Types and Figured Specimens from the Eocene and Oligocene Series preserved in the Museum of Practical Geology," compiled by Mr. H. A. Allen. We hope that further instalments of this Catalogue will be published at as early a period as possible.

With regard to the "Summary" of the field-work, we venture to rejoice if it is a few pages shorter, because there is a tendency to print an excessive amount of detail, and thereby to obscure the character of the work done. Those interested in the progress of science want to know clearly and readily the advances which have been made, not simply the observations made, by officers of the

[1] Exhibited and explained before the British Association in Section C (Geology), at the Bradford Meeting, September, 1900.