One of its main contentions, however, is this—that hardly any of the colleges at Oxford do much, and that none do more than they are obliged, to encourage natural science by means of their endowments.

I think that if I may be allowed briefly to state what the college which I know best, and the only one for which I have a right to speak, is doing in this matter, it will enable your readers to see that this contention is not universally applicable, and that there are at least some exceptions with regard to which the writer seems very imperfectly informed.

Magdalen College is spending at this moment in the direct endowment of natural science through professorships, fellowships, scholarships, and exhibitions, over £3500 a year, besides maintaining a laboratory of its own, and subsidising in other ways the teaching of natural science both in the University and within its own walls.

We support four professors of natural science. It may be said that we are obliged by statute to do so. That is true, but we were not bound to establish these professorships as rapidly as we have done, and we have been obliged at times to suspend fellowships in order to do so. We have, besides our four professor-fellows, three other fellows on our Governing Body voluntarily elected by the college for natural science.

The writer of the article complains that so few colleges have even a single tutor in natural science. More than twenty-five years ago we started a tutor, and for the last dozen years we have had a lecturer as well in natural science upon our regular staff.

We are not absolutely bound to offer any scholarships for natural science. We have always offered one a year ever since our demyships were thrown open, and we have frequently elected two and sometimes three demies in natural science in the same year, and often exhibitioners as well. Of the two senior demies, which are all we can at present afford ourselves, one was elected for natural science.

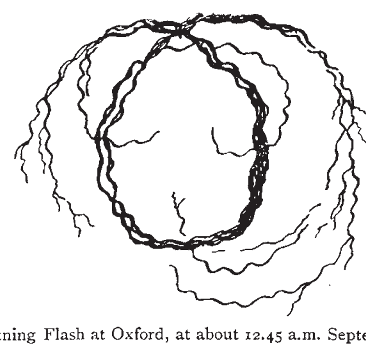
I believe that several other colleges at Oxford could point to facts analogously ignored or underrated by the writer of the article. What I have stated will at any rate, I think, show that my college, which was barely alluded to by him, has not been backward to recognise the claims or encourage by endowment the study of the natural sciences at Oxford.

T. HERBERT WARREN.

Magdalen College Oxford, September 17.

A Remarkable Lightning Flash.

ONE of the flashes of lightning during the heavy storm of September 8-9 at Oxford, was of so unusual a form that I venture to send a sketch of it to NATURE. Although a good many of the discharges struck downwards to earth, a considerable number passed horizontally from cloud to cloud, and most of these were very evidently branched at both ends. There had been some six or seven of this character in rapid succession in a cloud opposite the window at which I was sitting, and after a



Lightning Flash at Oxford, at about 12.45 a.m. September 9.

pause of two or three minutes I saw the appearance I have tried to represent. From the red glare by which it was surrounded, it was evidently within the cloud, but it was so dazzlingly bright that the after-image remained visible long enough for me to trace the convolutions and sketch them from memory. The main body of the flash made one complete loop, and the two ends, which were much branched, nearly completed a second turn. It appeared almost due north, about 35° above the horizon, and might have been comprised within a circle of about 5° in diameter. Evidently the path of the flash was an irregular spiral, and, with the exception of the branched ends, it looked exactly like the discharge of a large induction coil, seen end on. I much regretted not being provided with a camera.

GEORGE J. BURCH.

21 Norham Road, Oxford, September 9.

A Peculiarity in Perch.

I VENTURE to bring the following observation before the readers of NATURE, because I believe it to be uncommon, and that it will be a matter of interest to naturalists. My brother, whilst fishing in a pond in East Lancashire, caught twelve perch, the smallest weighing 3 ozs. and the largest 10 ozs., and eight of them exhibited a very marked peculiarity.

On the left side of the fish the cover of the gill was very small, being only less than half the natural size, and as a consequence a large portion of the gill was exposed. The largest fish presented this appearance.

The remaining four had covers to their gills, perfectly normal and similar on both sides.

The peculiarities about this malformation are that it apparently is confined to the gill-cover on the left side of the head, the one on the right side being perfectly normal; and is only to be found in certain of the fish in the pond.

It may possibly be the result of a disease; but if this is the case, the fact of it affecting always the same gill-cover appears somewhat remarkable, and to my mind is more than a coincidence. Besides every part of the fish, including the gill-cover itself, appears to be perfectly healthy.

The water has no predominant feature, and gives on analysis results similar to any common spring-water. I have known at rare intervals water containing iron to be discharged into the pond, but this has been almost immediately noticed and prevented.

As I have been unable to find an account of any disease exhibiting such a characteristic as above described, I have come to the conclusion that it is a very peculiar malformation of the cover of the gill. I should be glad to have some further information respecting this phenomenon if any reader of NATURE is in a position to give it.

R. J. FLINTOFF.

The Siemens Gas and Coke Fire.

I HAVE had a Siemens gas and coke fire in my study for fifteen years. There was much trouble in getting it put in properly, and Sir W. Siemens kindly advised me about it. It burns very little gas, and the coke is cheap. The gas is only used to kindle or liven up the coke. Everybody admires the beautiful fire it makes, and there is no smell and no smoke. The coke requires to be broken to the size of a small apple, and it is needful to clear out the bottom of the fire. I do this with an iron shovel, and thus remove the ash which, without this removal, would choke the fire. It is the neglect of this essential process which makes the Siemens fire sometimes a failure. Mine is, in all respects, a brilliant success.

P. W. CLAYDEN.

13 Tavistock Square, September 18.

P.S.—I read of the grate in NATURE in 1880, saw it at the Smoke Abatement Exhibition in 1881, and adopted it at once.

THE LIVERPOOL MEETING OF THE BRITISH ASSOCIATION.

V.

LIVERPOOL, *Wednesday.*

THE dominant note throughout this meeting has been "Listerism"—the germ theory, the application of biology to medicine. The reception given to the President by the people of Liverpool, especially by the medical profession, has been splendid and enthusiastic. The Philharmonic Hall was crowded to the doors by an attentive and appreciative audience on the occasion of the Presidential Address; and the vote of thanks was most appropriately and happily proposed by the Lord Mayor (Lord Derby), and seconded by Sir William Turner, an early friend and colleague of the President.

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This is a large meeting, the total number will probably nearly reach 3200, and this will place it as one of the few largest meetings of the Association; it has certainly been one of the busiest and liveliest. The hospitality of Lord Derby, both at the Town Hall and at Knowsley, and the banquets given by the Medical Institution and the American Chamber of Commerce, have been much appreciated.

Notwithstanding the unsettled weather, the garden parties have been largely attended, and have constituted a most agreeable and welcome method of meeting the members of other Sections.

St. George's Hall has been much admired as an unequalled reception-room; and with its new decoration, its beautiful tiled pavement, the electric light, the grand organ, and the crowd of people constantly passing to and fro, has presented a gay and lively scene.

At the Sectional meetings, although there has been nothing sensational, there is much evidence of solid work, and many interesting discussions, such as that on the Röntgen rays, in Section A, where Lenard is a notable figure, and the joint discussions of Sections D and I, on the origin of Vertebrates, following Prof. Gaskell's interesting address.

Among some of the other more interesting events in the Sections which seem to be attracting public attention were Prof. Ramsay on Helium and Argon, Prof. Dewar's account of liquid air, in Section B, the series of arctic papers, including Sir Martin Conway's lecture on Spitzbergen, and Mr. Scott Keltie's account of Nansen, in the Geographical Section, and the discussion, in Section H, on the Mediterranean race and the origin of Mycænean culture. Other attractive items before the Anthropological Section were the question of the age of the Dolmens, opinion being divided as to whether they belong to the Bronze or the Neolithic period, and the discussion on the femur of *Pithecanthropus*, a comparison of this celebrated bone with the femora of savage races showing that all its special characters are already known in human femora. The Section celebrated in an interesting manner the centenary of the Swede, Retzius the elder, the originator of modern methods of craniology.

One characteristic of the Sectional meetings has certainly been the extreme fulness of the programme, the result being that some Sections have had to meet early and continue sitting late; most of them held meetings on the Saturday, and several will have to continue their work well into the Wednesday forenoon.

Mention need not be made of the other scientific communications, as the usual special account of the work of the Sections will appear in future issues of NATURE.

The two conversaziones were brilliant functions, and the impression amongst the visitors seemed to be that the public rooms in Liverpool were very fine in dimensions and decoration. At the first soirée—that given by the Lord Mayor—there were no adventitious attractions beyond the stately reception and the pleasant meeting of friends. At the local committee's soirée in the Museum Library and Art Galleries, where ample accommodation is available, there were short lectures, demonstrations, and various exhibits which attracted much interest. Perhaps one of the finest exhibits was the great collection of models of ships lent by the Cunard, White Star, and other great ocean lines.

Between forty and fifty foreigners have been present, amongst the more notable figures being the botanists, Chodat, Pfitzer and Magnus; the physicists, Lenard and Kohlrausch; the archæologist, Montelius; and the zoologists, Hjort, Delage, and Minot. We have also had de Candolle, Le Conte, Dupuy, Walther, and Count Pfeil.

The Loan Collection in the new Museum of Zoology at University College seems to have been much appreciated. The exhibits in the collection chiefly illustrate papers read before Sections C, D, and H, and several of the Sections have adjourned in the afternoons to the Museum for special demonstrations.

On Saturday, notwithstanding the unsettled weather, all the excursions arranged were successfully carried out, including the dredging expedition in the Lancashire Sea Fisheries steamer, in which a number of the foreigners took part. The applications for the Thursday excursions are sufficiently numerous. The Isle of Man seems to be the favourite one, and as this is to be an expedition of considerable scientific interest, with a carefully-arranged programme, including nearly all the objects worthy of special attention in the island, a further report on the results of the excursion will be given in a future number of NATURE.

On Association Sunday, the usual arrangements were made, and several selected preachers dealt with the inter-relations of religion and science and other subjects which were supposed to be appropriate to the occasion. Amongst those who preached were Dean Farrar, Mr. Lund, Canon Diggle, Dr. Klein, and Prof. Ryle, of Cambridge. Many of the members of the Association seemed to prefer short trips in the neighbourhood of Liverpool, or to take advantage of the pleasant hospitality that was offered by some of the owners of large houses and gardens in the afternoon and evening.

At the meetings of the General Committee, several important matters have been decided. The date of the commencement of the meeting at Toronto next year has been fixed for August 18. The President-elect is Sir John Evans, K.C.B. The list of vice-Presidents and the local officers have also been fixed upon.

The Secretary of the Toronto Committee made a preliminary statement as to the facilities offered by the great steam ship companies in crossing the Atlantic, and the Committee have already distributed to members of the Association a most attractive preliminary programme in the form of a richly-illustrated pamphlet of seventy pages. Further details in regard to the arrangements for crossing the Atlantic, and also for travelling to America, are promised shortly.

With regard to the meeting in 1898, it seemed likely at one time that there would be competition between Glasgow and Bristol. A distinguished deputation from each City Council attended this meeting; but at the last moment the Lord Provost of Glasgow gracefully withdrew his claim in favour of Bristol, which had already made considerable preparations, and had been first in the field; consequently Bristol, on the motion of Sir. F. Bramwell, seconded by Prof. Ramsay, was unanimously fixed upon as the place of meeting for 1898. It was further resolved that the meeting in 1899 be held at Dover, in conjunction with the meeting of the French Association at Boulogne, on the other side of the Channel.

At the meetings of the Committee of Recommendations, the following Committees of the Association with grants of money were reappointed :—

Synopsis of Grants of Money appropriated to Scientific Purposes by the General Committee at the Liverpool Meeting, September, 1896. *The names of the Members entitled to call on the General Treasurer for the respective Grants are prefixed.*

Mathematics and Physics.

*Foster, Prof. Carey.—Electrical Standards (and unexpended balance)	£5
*Symons, Mr. G. J.—Photographs of Meteorological Phenomena	10
*Rayleigh, Lord.—Mathematical Tables	25
*Symons, Mr. G. J.—Seismological Observations ...	100
*Atkinson, Dr. E.—Abstracts of Physical Papers ...	100
*Harley, Rev. R.—Calculation of certain Integrals (partly renewed)	20
*Stokes, Sir G. G.—Solar Radiation	10
*Shaw, Mr. W. N.—Electrolysis and Electro-Chemistry...	50

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Chemistry.	
*Roscoe, Sir H. E.—Wave-length Tables of the Spectra of the Elements	£10
*Reynolds, Prof. J. Emerson.—Electrolytic Quantitative Analysis	10
*Bell, Sir J. Lowthian.—Chemical Constituents of Coal	10
*Tilden, Prof. W. A.—Isomeric Naphthaline Derivatives	50
Geology.	
*Hull, Prof. E.—Erratic Blocks	10
*Bonney, Prof. T. G.—Investigation of a Coral Reef by Boring and Sounding (renewed)	40
*Seeley, Prof. H. G.—Examination of Locality where the Cetiosaurus in the Oxford Museum was found (unexpended balance in hand)	
Flower, Sir W. H.—Fauna of Singapore Caves (unexpended balance)	40
*Geikie, Prof. J.—Photographs of Geological Interest	15
Dawkins, Prof. W. Boyd.—Remains of the Irish Elk in the Isle of Man	15
*Marr, Mr. J. E.—Life Zones in British Carboniferous Rocks	15
Zoology.	
*Herdman, Prof.—Table at the Zoological Station, Naples	100
*Bourne, Mr. G. C.—Table at the Biological Laboratory, Plymouth	40
Flower, Sir W. H.—Zoological Bibliography and Publication	5
Flower, Sir W. H.—Index Generum et Specierum	100
Sclater, Dr. P. L.—Zoology and Botany of the West India Islands	40
Newton, Prof.—To Work out Details of Observations on the Migration of Birds	40
Geography.	
*Ravenstein, Mr. E. G.—Climatology of Tropical Africa	20
Economic Science and Statistics.	
——— State Monopolies in other Countries	15
Price, Mr. L. L.—Future Dealings in Raw Produce	10
Mechanical Science.	
*Preece, Mr. W. H.—Small Screw Gauge	10
Anthropology.	
*Tylor, Prof. E. B.—North-Western Tribes of Canada	75
*Munro, Dr. R.—Lake Village of Glastonbury	30
*Brabrook, Mr. E. W.—Ethnographical Survey (renewed)	40
*Galton, Sir Douglas.—Mental and Physical Condition of Children	10
*Hartland, Mr. E. S.—Linguistic and Anthropological Characteristics of the North Dravidians	5
Evans, Mr. A. J.—Silchester Excavation	20
Physiology.	
Gaskell, Dr.—Investigations of Changes in Active Nerve Cells and their Peripheral Extensions	190
*McKendrick, Prof. J. G.—Physiological Applications of the Phonograph	15
*Herdman, Prof. W. A.—Oysters under normal and abnormal environments	30
Schäfer, Prof.—Physiological Effects of Peptone and its Precursors	20
Botany.	
Farmer, Prof. J. B.—Fertilisation in Phæophyceæ	20
Corresponding Societies.	
*Meldola, Prof. R.—Preparation of Report	25
	£1355

* Re-appointed.

At the final meeting, held on Wednesday, the usual votes of thanks and concluding speeches were made, and the general impression was expressed that the success of the meeting was characterised by the magnificence of the meeting-rooms, the hospitality of the people of Liverpool, and the general liveliness of the proceedings. The British Association certainly seems, from the evidence at this, its latest meeting, to be very full of life and vigour.

W. A. HERDMAN.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY J. E. MARR, M.A., F.R.S., SEC. G.S., PRESIDENT OF THE SECTION.

THE feelings of one who, being but little versed in the economic applications of his science, is called upon to address a meeting of the Association held in a large industrial centre, might, under ordinary circumstances, be of no very pleasant character; but I take courage when I remember that those connected with my native county, in which we are now gathered, have taken prominent part in advancing branches of our science which are not directly concerned with industrial affairs. I am reminded, for instance, that one amongst you, himself a busy professional man, has in his book on "The Origin of Mountain Ranges" given to the world a theoretical work of the highest value; that, on the opposite side of the county, those who are responsible for the formation and management of that excellent educational institution, the Ancoats Museum, have wisely recognised the value of some knowledge of geology as a means of quickening our appreciation of the beauties of nature; and that one who has done solid service to geology by his teachings, who has kept before us the relationship of our science to that which is beautiful—I refer to the distinguished author of "Modern Painters"—has chosen the northern part of the county for his home, and has illustrated his teaching afresh by reference to the rocks of the lovely district around him. Nor can I help referring to one who has recently passed away—the late Sir Joseph Prestwich—the last link between the pioneers of our science and the geologists of the present day, who, though born in London, was of Lancashire family, and whom we may surely therefore claim as one of Lancashire's worthies. With these evidences of the catholicity of taste on the part of geologists connected with the county, I feel free to choose my own subject for this address, and, my time being occupied to a large extent with academic work, I may be pardoned for treating that subject in academic fashion. As I have paid considerable attention to the branch of the science which bears the somewhat uncouth designation of stratigraphical geology, I propose to take the present state of our knowledge of this branch as my theme.

Of the four great divisions of geology, petrology may be claimed as being largely of German origin, the great impetus to its study having been given by Werner and his teachings. Palæontology may be as justly claimed by the French nation, Cuvier having been to so great an extent responsible for placing it upon a scientific basis. Physical geology we may partly regard as our own, the principles laid down by Hutton and supported by Playfair having received illustration from a host of British writers, amongst whom may be mentioned Jukes, Ramsay, and the brothers Geikie; but the grand principles of physical geology have been so largely illustrated by the magnificent and simple features displayed on the other side of the Atlantic, that we may well refer to our American brethren as leaders in this branch of study. The fourth branch, stratigraphical geology, is essentially British as regards origin, and, as every one is aware, its scientific principles were established by William Smith, who was not only the father of English geology, but of stratigraphical geology in general.

Few will deny that stratigraphical geology is the highest branch of the science, for, as has been well said, it "gathers up the sum of all that is made known by the other departments of the science, and makes it subservient to the interpretation of the geological history of the earth." The object of the stratigraphical geologist is to obtain information concerning all physical, climatic, and biological events which have occurred during each period of the past, and to arrange them in chronological order, so as to write a connected history of the earth. If all of this information were at our disposal, we could write a complete earth-history, and the task of the geologist would be ended. As it is, we have barely crossed the threshold of discovery, and the "imperfection of the geological record," like the "glorious uncertainty" of our national game, gives geology one of its great charms. Before passing on to consider more particularly the present state of the subject of our study, a few remarks upon this imperfection of the geological record may not be out of place, seeing that the term has been used by so many modern writers, and its exact signification occasionally misunderstood. The imperfection of the palæontological record is usually understood by the term when used, and it will be considered here as an illustration of the incompleteness of our

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knowledge of earth-history; but it must be remembered that the imperfection of the physical record is equally striking, as will be insisted on more fully in the sequel.

Specially prominent amongst the points upon which we are ignorant stands the nature of the Precambrian faunas. The extraordinary complexity of the earliest known Cambrian fauna has long been a matter for surprise, and the recent discoveries in connection with the *Olenellus* fauna do not diminish the feeling.[1] After commenting upon the varied nature of the earliest known fauna, the late Prof. Huxley, in his Address to the Geological Society in 1862, stated that "any admissible hypothesis of progressive modification must be compatible with persistence without progression, through indefinite periods. . . . Should such an hypothesis eventually be proved to be true, . . . the conclusion will inevitably present itself, that the Palæozoic, Mesozoic, and 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." Whether or not this estimate is correct, all geologists will agree that a vast period of time must have elapsed before the Cambrian period, and yet our ignorance of faunas existing prior to the time when the *Olenellus* fauna occupied the Cambrian seas is almost complete. True, many Precambrian fossils have been described at various times, but, in the opinion of many competent judges, the organic nature of each one of these requires confirmation. I need not, however, enlarge upon this matter, for I am glad to say we have amongst us a geologist who will at a later stage read a paper before this Section upon the subject of Precambrian fossils, and there is no one better able, owing to his intimate acquaintance with the actual relics, to present fairly and impartially the arguments which have been advanced in favour of the organic origin of the objects which have been appealed to as evidences of organisms of Precambrian age than our revered co-worker from Canada, Sir J. William Dawson. We may look forward with confidence to the future discovery of many faunas older than those of which we now possess certain knowledge, but until these are discovered, the palæontological record must be admitted to be in a remarkably incomplete condition. In the meantime, a study of the recent advance of our knowledge of early life is significant of the mode in which still earlier faunas will probably be brought to light. In 1845, Dr. E. Emmons described a fossil, now known to be an *Olenellus*, though at that time the earliest fauna was supposed to be one containing a much later group of organisms, and it was not until Nathorst and Brögger established the position of the *Olenellus* zone that the existence of a fauna earlier than that of which *Paradoxides* was a member was admitted; and, indeed, the *Paradoxides* fauna itself was proved to be earlier than that containing *Olenus*, long after these two genera had been made familiar to palæontologists, the Swedish palæontologist, Augelin, having referred the *Paradoxides* fauna to a period earlier than that of the one with *Olenus*. It is quite possible, therefore, that fossils are actually preserved in our museums at the present moment, which have been extracted from rocks deposited before the period of formation of the *Olenellus* beds, though their age has not been determined. The *Olenellus* horizon now furnishes us with a datum-line from which we can work backwards, and it is quite possible that the *Neobolus* beds of the Salt Range,[2] which underlie beds holding *Olenellus*, really do contain, as has been maintained, a fauna of date anterior to the formation of the *Olenellus* beds; and the same may be the case with the beds containing the *Protolenus* fauna in Canada,[3] for this fauna is very different from any known in the *Olenellus* beds, or at a higher horizon, though Mr. G. F. Matthew, to whom geologists owe a great debt for his admirable descriptions of the early fossils of the Canadian rocks, speaks very cautiously of the age of the beds containing *Protolenus* and its associates. Notwithstanding our ignorance of Precambrian faunas, valuable work has recently been done in proving the existence of important groups of stratified rocks deposited previously to the formation of the beds containing the earliest known Cambrian fossils; I may refer especially to the proofs of the Precambrian age of the Torridon sandstone of north-west Scotland, lately furnished by the officers of the Geological Survey, and their discovery that the maximum thickness of these strata is over 10,000 feet.[1] Amongst the sediments of this important system, more than one fauna may be discovered, even if most of the strata were accumulated with rapidity, and all geologists must hope that the officers of the Survey—who, following Nicol, Lapworth, and others, have done so much to elucidate the geological structure of the Scottish Highlands—may obtain the legitimate reward of their labours, and definitely prove the occurrence of rich faunas of Precambrian age in the rocks of that region.

But, although we may look forward hopefully to the time when we may lessen the imperfection of the records of early life upon the globe, even the most hopeful cannot expect that record to be rendered perfect, or that it will make any near approach to perfection. The posterior segments of the remarkable trilobite *Mesonacis vermontana* are of a much more delicate character than the anterior ones, and the resemblance of the spine on the fifteenth "body-segment" of this species to the terminal spine of *Olenellus* proper, suggests that in the latter sub-genus posterior segments of a purely membranous character may have existed, devoid of hard parts. If this be so, the entire outer covering of the trilobites, at a period not very remote from the end of Precambrian times, may have been membranous, and the same thing may have occurred with the structures analogous to the hard parts of organisms of other groups. Indeed, with our present views as to development, we can scarcely suppose that organisms acquired hard parts at a very early period of their existence, and fauna after fauna may have occupied the globe, and disappeared, leaving no trace of its existence, in which case we are not likely ever to obtain definite knowledge of the characters of our earliest faunas, and the biologist must not look to the geologist for direct information concerning the dawn of life upon the earth.

Proceeding now to a consideration of the faunas of the rocks formed after Precambrian times, a rough test of the imperfection of the record may be made by examining the gaps which occur in the vertical distribution of forms of life. If our knowledge of ancient faunas were very incomplete, we ought to meet with many cases of recurrence of forms after their apparent disappearance from intervening strata of considerable thickness, and many such cases have actually been described by that eminent palæontologist, M. Barrande, amongst the Palæozoic rocks of Bohemia, though even these are gradually being reduced in number owing to recent discoveries; indeed, in the case of the marine faunas, marked cases of recurrence are comparatively rare, and the occurrence of each form is generally fairly unbroken from its first appearance to its final extinction, thus showing that the imperfection of the record is by no means so marked as might be supposed. Fresh-water and terrestrial forms naturally furnish a large percentage of cases of recurrence, owing to the comparative rarity with which deposits containing such organisms are preserved amongst the strata.

A brief consideration of the main reasons for the present imperfection of our knowledge of the faunas of rocks formed subsequently to Precambrian times may be useful, and suggestive of lines along which future work may be carried out. That detailed work in tracts of country which are yet unexplored, or have been but imperfectly examined by the geologist, will add largely to our stock of information, needs only to be mentioned; the probable importance of work of this kind in the future may be inferred from a consideration of the great increase of our knowledge of the Permo-Carboniferous faunas, as the result of recent labours in remote regions. It is specially desirable that the ancient faunas and floras of tropical regions should be more fully made known, as a study of these will probably throw considerable light upon the influence of climate upon the geographical distribution of organisms in past times. The old floras and faunas of Arctic regions are becoming fairly well known, thanks to the zeal with which the Arctic regions have been explored. But, confining our attention to the geology of our own country, much remains to be done even here, and local observers especially have opportunities of adding largely to our stock of knowledge, a task they have performed so well in the past. To give examples of the value of such work, our knowledge of the fauna of the Cambrian rocks of Britain is largely due to the present President of the Geological Society, when resident at St. David's, whilst

[1] Dr. C. D. Walcott, in his monograph on "The Fauna of the Lower Cambrian or Olenellus Zone" (Washington 1890), records the following great groups as represented in the Olenellus beds of America:—Spongiæ, Hydrozoa, Actinozoa, Echinodermata, Annelida? (trails, burrows, and tracks), Brachiopoda, Lamellibranchiata, Gasteropoda, Pteropoda, Crustacea, and Trilobita. Others are known as occurring in beds of the same age in the Old World.

[2] See F. Noetling, "On the Cambrian Formation of the Eastern Salt Range." *Records Geol. Survey, India*, vol. xxvii. p. 71.

[3] G. F. Matthew, "The Protolenus Fauna." *Trans. New York Acad. of Science*, 1895, vol. xiv. p. 101.

[1] Sir A. Geikie, "Annual Report of the Geological Survey [United Kingdom] . . . for the year ending December 31, 1893." (London, 1894.)

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the magnificent fauna of the Wenlock limestone would have been far less perfectly known than it is, if it were not for the collections of men like the late Colonel Fletcher and the late Dr. Grindrod. Again, the existence of the rich fauna of the Cambridge Greensand would have been unsuspected had not the bed known by that name been worked for the phosphatic nodules which it contains.

It is very desirable that large collections of varieties of species should be made, for in this matter the record is very imperfect. There has been, and, I fear, is still, a tendency to reject specimens when their characters do not conform with those given in specific descriptions, and thus much valuable material is lost. Local observers should be specially careful to search for varieties, which may be very abundant in places where the conditions were favourable for their production, though rare or unknown elsewhere. Thus, I find the late Mr. W. Keeping remarking that "it is noteworthy that at Upware, and indeed all other places known to me, the species of *Brachiopoda* [of the *Neocomian* beds] maintain much more distinctness and isolation from one another than at Brickhill."[1] The latter place appears to be one where conditions were exceptionally favourable in *Neocomian* times for the production of intermediate forms.

A mere knowledge of varieties is, however, of no great use to the collector without a general acquaintance with the morphology of the organisms whose remains he extracts from the earth's strata, and one who has this can do signal service to the science. It is specially important that local observers should be willing to devote themselves to the study of particular groups of organisms, and to collect large suites of specimens of the group they have chosen for study. With a group like the graptolites, for instance, the specimens which are apparently best preserved are often of little value from a morphological point of view, and fragments frequently furnish more information than more complete specimens. These fragments seldom find their way to our museums, and accordingly we may examine a large suite of graptolites in those museums without finding any examples showing particular structures of importance, such as the sac-like bodies carried by many of these creatures. As an illustration of the value of work done by one who has made a special study of a particular group of organisms, I may refer to the remarkable success achieved by the late Mr. Norman Glass in developing the calcareous supports of the brachial processes of Brachiopods. Work of this character will greatly reduce the imperfection of the record from the biologists' point of view.

The importance of detailed work leads one to comment upon the general methods of research which have been largely adopted in the case of the stratified rocks. The principle that strata are identifiable by their included organisms is the basis of modern work, as it was of that which was achieved by the father of English Geology, and the identification of strata in this manner has of recent years been carried out in very great detail, notwithstanding the attempt on the part of some well-known writers to show that correlation of strata in great detail is impossible. The objection to this detailed work is mainly founded upon the fact that it must take time for an organism or group of organisms to migrate from one area to another, and therefore it was stated that they cannot have lived contemporaneously in two remote areas. But the force of this objection is practically done away with if it can be shown that the time taken for migration is exceedingly short as compared with the time of duration of an organism or group of organisms upon the earth, and this has been shown in the only possible way—namely, by accumulating a very great amount of evidence as the result of observation. The eminent writers referred to above, who were not trained geologists, never properly grasped the vast periods of time which must have elapsed during the occurrence of the events which it is the geologist's province to study. An historian would speak of events which began at noon on a certain day and ended at midnight at the close of that day as contemporaneous with events which commenced and ended five minutes later, and this is quite on a par with what the geologist does when correlating strata. Nevertheless, there are many people who still view the task of correlating minute subdivisions of stratified systems with one another, with a certain amount of suspicion, if not with positive antipathy; but the work must be done for all that. Brilliant generalisations are attractive as well as valuable, but the steady accumulation of facts is as necessary for the advancement of the science as it was in the days when the Geological Society was founded, and its members applied themselves "to multiply and record observations, and patiently to await the result at some future period." I have already suggested a resemblance between geology and cricket, and I may be permitted to point out that just as in the game the free-hitter wins the applause, though the patient "stone-waller" often wins the match, so, in the science, the man apt at brilliant generalisations gains the approval of the general public, but the patient recorder of apparently insignificant details adds matter of permanent value to the stores of our knowledge. In the case of stratigraphical geology, if we were compelled to be content with correlation of systems only, and were unable to ascertain which of the smaller series and stages were contemporaneous, but could only speak of these as "homotaxial," we should be in much the same position as the would-be antiquary who was content to consider objects fashioned by the Romans as contemporaneous with those of mediæval times. Under such circumstances geology would indeed be an uncertain science, and we should labour in the field, knowing that a satisfactory earth-history would never be written. Let us hope that a brighter future is in store for us, and let me urge my countrymen to continue to study the minute subdivisions of the strata, lest they be left behind by the geologists of other countries, to whom the necessity for this kind of study is apparent, and who are carrying it on with great success.

The value of detailed work on the part of the stratigraphical geologist is best grasped if we consider the recent advance that has been made in our science owing to the more or less exhaustive survey of the strata of various areas, and the application of the results obtained to the elucidation of earth's history. A review of this nature will enable us not only to see what has been done, but also to detect lines of inquiry which it will be useful to pursue in the future; but it is obvious that the subject is so wide that little more can be attempted than to touch lightly upon some of the more prominent questions. A work might well be written treating of the matters which I propose to notice. We have all read our "Principles of Geology," or "The Modern Changes of the Earth and its Inhabitants considered as illustrative of Geology," to quote the alternative title; some day we may have a book written about the ancient changes of the earth and its inhabitants considered as illustrative of geography.

Commencing with a glance at the light thrown on inorganic changes by a detailed examination of the strata, I may briefly allude to advances which have recently been made in the study of denudation. The minor faults, which can only be detected when the small subdivisions of rock-groups are followed out carefully on the ground, have been shown to be of great importance in defining the direction in which the agents of denudation have operated, as demonstrated by Prof. W. C. Brögger, for instance, in the case of the Christiania Fjord (*Nyt. Mag. for Naturvidensk.*, vol. xxx. (1886), p. 79); and I have recently endeavoured to prove that certain valleys in the English Lake District have been determined by shattered belts of country, the existence of which is shown by following thin bands of strata along their outcrop. The importance of the study of the strata in connection with the genesis and subsequent changes of river-systems is admirably brought out in Prof. W. M. Davis's paper on "The Development of certain English Rivers (*Geograph. Journ.*, vol. v. (1895) p. 127), a paper which should be read by all physical geologists; it is, indeed, a starting-point of kindred work which remains especially for local observers to accomplish. Study of this kind not only adds to our knowledge of the work of geological agencies, but helps to diminish the imperfection of the record, for the nature of river-systems, when rightly understood, enables us to detect the former presence of deposits over areas from which they have long since been removed by denudation.

An intimate acquaintance with the lithological characters of the strata of a district affords valuable information in connection with the subject of glacial denudation. The direction of glacial transport over the British Isles has been largely inferred from a study of the distribution of boulders of igneous rock, whilst those of sedimentary rock have been less carefully observed. The importance of the latter is well shown by the work which has been done in Northern Europe in tracing the Scandinavian boulders to their sources, a task which could not have been performed successfully if the Scandinavian strata had not been

[1] W. Keeping, Sedgwick Essay: "The Fossils and Palæontological Affinities of the Neocomian Deposits of Upware and Brickhill." (Cambridge, 1883.)

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studied in great detail.[1] I shall presently have more to say with regard to work connected with the lithological characters of the sediments. Whilst mentioning glacial denudation, let me allude to a piece of work which should be done in great detail, though it is not, strictly speaking, connected with stratigraphy, namely, the mapping of the rocks around asserted "rock-basins." I can find no actual proof of the occurrence of such basins in Britain, and it is very desirable that the solid rocks and the drift should be carefully inserted on large-scale maps, not only all around the shores of several lakes, but also between the lakes and the sea, in order to ascertain whether the lakes are really held in rock-basins. Until this work is done, however probable the occurrence of rock-basins in Britain may be considered to be, their actual existence cannot be expected to be proved.

When referring to the subject of denudation, mention was made a moment ago of the study of the lithological character of the sediments. Admirable work in this direction was carried out years ago by one who may be said to have largely changed the direction of advance of geology in this country owing to his researches "On the Microscopical Structure of Crystals, indicating the Origin of Minerals and Rocks." I refer, of course, to Dr. H. C. Sorby. But since our attention has been so largely directed to petrology, the study of the igneous and metamorphic rocks has been most zealously pursued, whilst that of the sediments has been singularly little heeded, with few exceptions, prominent amongst which is the work of Mr. Maynard Hutchings, the results of which have been recently published in the *Geological Magazine*, though we must all hope that the details which have hitherto been supplied to us, valuable as they are, are only a foretaste of what is to follow from the pen of this able observer. Descriptions of the lithological changes which occur in a vertical series of sediments, as well as of those which are observed when any particular band is traced laterally, will no doubt throw light upon a number of interesting questions.

Careful work amongst the ancient sediments, especially those which are of organic origin, has strikingly illustrated the general identity of characters, and therefore of methods of formation, of deposits laid down on the sea-floors of past times and those which are at present in course of construction. Globigerine-oozes have been detected at various horizons and in many countries. Prof. H. Alleyne Nicholson (Nicholson and Lydekker, "Manual of Palæontology," chap. ii.) has described a pteropod-ooze of Devonian age in the Hamilton Limestone of Canada, which is largely composed of the tests of *Styliola*; and to Dr. G. J. Hinde we owe the discovery of a large number of radiolarian cherts of Palæozoic and Neozoic ages in various parts of the globe. The extreme thinness of many argillaceous deposits, which are represented elsewhere by hundreds of feet of strata, suggests that some of them, at any rate, may be analogous to the deep-sea clays of modern oceans, though in the case of deposits of this nature we must depend to a large extent upon negative evidence. The uniformity of character of thin marine deposits over wide areas is in itself evidence of their formation at some distance from the land; but although the proofs of origin of ancient sediments far from coast-lines may be looked upon as permanently established, the evidence for their deposition at great depths below the ocean's surface might be advantageously increased in the case of many of them. The fairly modern sediments, containing genera which are still in existence, are more likely to furnish satisfactory proofs of a deep-sea origin than are more ancient deposits. Thus the existence of *Archæopneustes* and *Cystechinus* in the oceanic series of Barbadoes, as described by Dr. Gregory, furnishes strong proofs of the deep-sea character of the deposits, whilst the only actual argument in favour of the deep-sea character of certain Palæozoic sediments has been put forward by Prof. Suess, who notes the similarity of certain structures of creatures in ancient rocks to those possessed by modern deep-sea crustacea, especially the co-existence of trilobites which are blind with those which have enormously developed eyes.

A question which has been very prominently brought to the fore in recent years is that of the mode of formation of certain coral-reefs. The theory of Charles Darwin, lately so widely accepted as an explanation of the mode of formation of barrier-reefs and atolls, has been, as is well known, criticised by Dr. Murray, with the result that a large number of valuable observations have been recently made on modern reefs, especially by biologists, as a contribution to the study of reef formation. Nor have geologists been inactive. Dr. E. Mojsisovics and Prof. Dupont, to mention two prominent observers, have described knoll-like masses of limestone more or less analogous, as regards structure, to modern coral-reefs. They consider that these have been formed by corals, and indeed Dupont maintains that the atoll-shape is still recognisable in ancient Devonian coral-reefs in Belgium.[1] I would observe that all cases of "knoll-reefs" of this character have been described in districts which furnish proofs of having been subjected to considerable orogenic disturbance, subsequent to the formation of the rocks composing the knoll-shaped masses, whilst in areas which have not been affected by violent earth-foldings, the reef-building corals, so far as I have been able to ascertain, give rise to sheet-like masses, such as should be produced according to Dr. Murray's theory. I would mention especially the reefs of the Corallian Rocks of England, and also some admirable examples seen amongst the Carboniferous Limestone strata of the great western escarpment of the Pennine chain which faces the Eden valley in the neighbourhood of Melmerby in Cumberland. Considering the number of dissected coral-reefs which exist amongst the strata of the earth's crust, and the striking way in which their structure is often displayed, it is rather remarkable that comparatively little attention has been paid to them by geologists in general, when the subject has been so prominently brought before the scientific world, for we must surely admit that we are much more likely to gain important information, shedding light upon the methods of reef-formation, by a study of such dissected reefs, than by making a few bore-holes on some special coral island. I would specially recommend geologists to make a detailed study of the British coral-reefs of Silurian, Devonian, Carboniferous, and Jurassic ages.

Turning now to organic deposits of vegetable origin, we must, as the result of detailed work, be prepared to admit the inapplicability of any one theory of the formation of coal seams. The "growth-in-place" theory may be considered fairly well established for some coals, such as the spore-coals, whilst the "drift" theory furnishes an equally satisfactory explanation of the formation of cannel-coal. It is now clear that the application of the general term *coal* to a number of materials of diverse nature, and probably of diverse origin, was largely responsible for the dragging-out of a controversy, in which the champions of either side endeavoured to explain the origin of all coal in one particular way.

The stratigraphical geologist, attempting to restore the physical geography of former periods, naturally pays much attention to the positions of ancient coast-lines; indeed, all teachers find it impossible to give an intelligible account of the stratified rocks without some reference to the distribution of land and sea at the time of their formation. The general position of land-masses at various times has been ascertained in several parts of the world, but much more information must be gathered together before our restorations of ancient sea-margins approximate to the truth. The carboniferous rocks of Britain have been specially studied with reference to the distribution of land and water during the period of their accumulation, and yet we find that owing to the erroneous identification of certain rocks of Devonshire as grits or sandstones, which Dr. Hinde has shown to be radiolarian cherts, land was supposed to lie at no great distance south of this region in Lower Carboniferous times, whereas the probabilities are in favour of the existence of an open ocean at a considerable distance from any land in that direction. This case furnishes us with an excellent warning against generalisation upon insufficient data.

As a result of detailed study of the strata, the effects of earth-movements have been largely made known to us, especially of those comparatively local disturbances spoken of as orogenic which are mainly connected with mountain-building, whilst information concerning the more widely spread epeirogenic movements is also furnished by a study of the stratified rocks. The structure of the Alps, of the North-West Highlands of Scotland, and of the uplifted tracts of North America is now familiar to geologists, whilst the study of comparatively recent sediments has proved the existence of widespread and extensive movements in times which are geologically modern; for instance,

[1] It is desirable that the boulders of sedimentary rock imbedded in the drifts of East Anglia should be carefully examined and fossils collected from them. The calcareous strata associated with the Alum Shales of Scandinavia and the strata of the Orthoceras-Limestone of that region may be expected to be represented amongst the boulders.

[1] Similar knoll-like masses have been described in this country by Mr. R. H. Tiddeman, as occurring in the Craven district of Yorkshire, but he does not attribute their formation to coral growth to any great extent.

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the deep-water deposits of late Tertiary age found in the West Indies indicate the occurrence of considerable uplift in that region. But a great amount of work yet remains to be done in this connection, especially concerning horizontal distortion of masses of the earth's crust, owing to more rapid horizontal advance of one portion than of another, during periods of movement. Not until we gather together a large amount of information derived from actual inspection of the rocks shall we be able to frame satisfactory theories of earth-movement, and in the meantime we are largely dependent upon the speculations of the physicist, often founded upon very imperfect data, on which is built an imposing superstructure of mathematical reasoning. We have been told that our continents and ocean-basins have been to a great extent permanent as regards position through long geological ages; we now reply by pointing to deep-sea sediments of nearly all geological periods, which have been uplifted from the ocean-abysses to form portions of our continents; and as the result of study of the distribution of fossil organisms, we can point almost as confidently to the sites of old continents now sunk down into the ocean depths. It seems clear that our knowledge of the causes of earth-movements is still in its infancy, and that we must be content to wait awhile, until we have further information at our disposal.

Recent work has proved the intimate connection betwixt earth-movement and the emission and intrusion of igneous rocks, and the study of igneous rocks has advanced beyond the petrographical stage; the rocks are now made to contribute their share towards the history of different geological periods. The part which volcanic action has played in the actual formation of the earth's crust is well exemplified in Sir Archibald Geikie's Presidential Addresses to the Geological Society, wherein he treats of the former volcanic history of the British Isles. (*Quart. Journ. Geol. Soc.*, vols. xlvii. and xlviii.) The way in which extruded material contributes to the formation of sedimentary masses has, perhaps, not been fully grasped by many writers, who frequently seem to assume that deposition is a measure of denudation, and *vice versâ*, whereas deposition is only a measure of denudation, and of the material which has been ejected in a fragmental condition from the earth's interior, which in some places forms a very considerable percentage of the total amount of sediment.

The intruded rocks also throw much light on past earth-history, and I cannot give a better illustration of the valuable information which they may furnish to the stratigraphical geologist when rightly studied, than by referring to the excellent and suggestive work by my colleague, Mr. Alfred Harker, on the Bala Volcanic Rocks of Carnarvonshire. (*Sedgwick Essay* for 1888: Camb. Univ. Press, 1889.)

Perhaps the most striking instance of the effect which detailed stratigraphical work has produced on geological thought is supplied by the study of the crystalline schists. Our knowledge of the great bulk of the rocks which enter into the formation of a schistose complex is not very great, but the mode of production of many of them is now well known, and the crude speculations of some of the early geologists are now making way for theories founded on careful and minute observations in the field as well as in the laboratory. Recent work amongst the crystalline schists shows, furthermore, how careful we should be not to assume that because we have got at the truth, we have therefore ascertained the whole truth. We all remember how potent a factor dynamic metamorphism was supposed to be, owing to discoveries made in the greatly disturbed rocks of Scotland and Switzerland; and the action of heat was almost ignored by some writers, except as a minor factor, in the production of metamorphic change. The latest studies amongst the foliated rocks tend to show that heat does play a most important part in the manufacture of schists. The detailed work of Mr. George Barrow, in North-East Forfarshire (*Quart. Journ. Geol. Soc.*, vol. xlix. (1893) p. 330) has already thrown a flood of light upon the origin of certain schists, and their connection with igneous rocks, and geologists will look forward with eagerness to further studies of the puzzling Highland rocks by this keen observer.

The subject of former climatic conditions is one in which the geologist has very largely depended upon followers of other branches of science for light, and yet it is one peculiarly within the domain of the stratigraphical geologist; and information which has already been furnished concerning former climatic conditions, as the result of careful study of the strata, is probably only an earnest of what is to follow when the specialist in climatology pays attention to the records of the rocks, and avoids the theories elaborated in the student's sanctum. The recognition of an Ice Age in Pleistocene times at once proved the fallacy of the supposition that there has been a gradual fall in temperature throughout geological ages without any subsequent rise, and accordingly most theories which have been put forward to account for former climatic change have been advanced with special reference to the Glacial period or periods, although there are many other interesting matters connected with climate with which the geologist has to deal. Nevertheless, the occurrence of glacial periods is a matter of very great interest, and one which has deservedly received much attention, though the extremely plausible hypothesis of Croll, and the clear manner in which it has been presented to general readers, tended to throw other views into the shade, until quite recently, when this hypothesis has been controverted from the point of view of the physicist. In the meantime considerable advance has been made in our actual knowledge, and this year, probably for the first time, and as the result of the masterly *résumé* of Prof. Edgworth David ("Evidences of Glacial Action in Australia in Permo-Carboniferous Time," *Quart. Journ. Geol. Soc.*, vol. lii. p. 289), the bulk of British geologists are prepared to admit that there has been more than one glacial period, and that the evidence of glacial conditions in the southern hemisphere in Permo-Carboniferous times is established. Croll's hypothesis of course requires the recurrence of glacial periods, but leaving out of account arguments not of a geological character, which have been advanced against this hypothesis, the objection raised by Messrs. Gray and Kendall ("The Cause of an Ice Age," *Brit. Assoc. Rep.* (1892), p. 708), that in the case of the Pleistocene Ice Age "the cold conditions came on with extreme slowness, the refrigerations being progressive from the Eocene period to the climax," seems to me to be a fatal one. At the same time, rather than asking with the above writers "the aid of astronomers and physicists in the solution of" this problem, I would direct the attention of stratigraphical geologists to it, believing that, by steady accumulation of facts, they are more likely than any one else to furnish the true clue to the solution of the glacial problem.

I have elsewhere called attention to marked changes in the faunas of the sedimentary rocks when passing from lower to higher levels, without the evidence of any apparent physical break, or any apparent change in the physical conditions, so far as can be judged from the lithological characters of the strata, and have suggested that such sudden faunistic variations may be due to climate. I refer to the matter as one which may well occupy the attention of local observers.

One of the most interesting points connected with climatic conditions is that of the former general lateral distribution of organisms, and its dependence upon the distribution of climatic zones. The well-known work of the late Dr. Neumayr ("Ueber klimatische Zonen während der Jura-und Kreidezeit," *Denkschr. der math.-naturwissen. Classe der k. k. Akad. der Wissenschaften*, vol. xlvii. Vienna, 1883) has, in the opinion of many geologists, established the existence of climatic zones whose boundaries ran practically parallel with the equator in Jurassic and Cretaceous times, and the possible existence of similar climatic zones in Palæozoic times has been elsewhere suggested; but it is very desirable that much more work should be done upon this subject, and it can only be carried out by paying close attention to the vertical and lateral distribution of organisms in the stratified rocks.

So far we have chiefly considered the importance of stratigraphical geology in connection with the inorganic side of nature. We now come to the bearing of detailed stratigraphical work upon questions concerning the life of the globe, and here the evidence furnished by the geologist particularly appeals to the general educated public as well as to students of other sciences.

Attention has just been directed to the probable importance of former climatic changes in determining the distribution of organisms, but the whole subject of the geographical distribution of organisms during former geological periods, though it has already received a considerable amount of attention, will doubtless have much further light thrown upon it as the result of careful observations carried out amongst the stratified rocks.

So long ago as 1853, Pictet laid it down as a palæontological law that "the geographical distribution of species found in the strata was more extended than the range of species of existing faunas." One would naturally expect that at a time when the diversity of animal organisation was not so great as it now is, the species, having fewer enemies with which to cope, and on the whole not too complex organisations to be affected by out-

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ward circumstances, would spread further laterally than they now do; but as we know that in earliest Cambrian times the diversity of organisation was very considerable, it is doubtful whether any appreciable difference would be exerted upon lateral distribution then and now, owing to this cause. At the time at which Pictet wrote, the rich fauna of the deeper parts of the oceans, with its many widely distributed forms of life, was unknown, and the range in space of early organisms must have then struck every one who thought upon the subject as being greater than that of the shallow-water organisms of existing seas, which were alone known. It is by no means clear, however, with our present knowledge, that Pictet's supposed law holds good, and it will require a considerable amount of work before it can be shown to be even apparently true. Our lists of the fossils of different areas are not sufficiently complete to allow us to generalise with safety, but a comparison of the faunas of Australia and Britain indicates a larger percentage of forms common to the two areas, as we examine higher groups of the geological column. If this indication be fully borne out by further work, it will not prove the actual truth of the law, for the apparent wider distribution of ancient forms of life might be due to the greater probability of elevation of ancient deep-sea sediments than of more modern ones which have not been subjected to so many elevatory movements. Still, if the law be apparently true, it is a matter of some importance to geologists; and I have touched upon the matter here in order once again to emphasise the possibility of correlating comparatively small thicknesses of strata in distant regions by their included organisms.

Mention of Pictet's laws, one of which states that fossil animals were constructed upon the same plan as existing ones, leads me to remark upon the frequent assumption that certain fossils are closely related to living groups, when the resemblances between the hard parts of the living and extinct forms are only of the most general character. There is a natural tendency to compare a fossil with its nearest living ally, but the comparison has probably been often pushed too far, with the result that biologists have frequently been led to look for the ancestors of one living group exclusively amongst forms of life which are closely related to those of another living group. The result of detailed work is to bring out more and more prominently the very important differences between some ancient forms and any living creature, and to throw doubts on certain comparisons; thus I find several of the well-known fossils of the Old Red Sandstone, formerly referred without hesitation to the fishes, are now doubtfully placed in that class.

The importance of detailed observation in the field is becoming every day more apparent, and the specialist who remains in his museum examining the collections amassed by the labours of others, and never notes the mode of occurrence of fossils in the strata, will perhaps soon be extinct, himself an illustration of the principle of the survival of the fittest. In the first place, such a worker can never grasp the true significance of the changes wrought on fossil relics after they have become entombed in the strata, especially amongst those rocks which have been subjected to profound earth-movements; and it is to be feared that many "species" are still retained in our fossil lists, whose supposed specific characters are due to distortion by pressure. But a point of greater importance is, that one who confines his attention to museums, cannot, unless the information supplied to him be very full, distinguish the differences between fossils which are variations from a contemporaneous dominant form, such as "sports," and those which have been termed "mutations," which existed at a later period than the forms which they resemble. The value of the latter to those who are attempting to work out phylogenies is obvious, and their nature can only be determined as the result of very laborious and accurate field-work; but such labour in such a cause is well worth performing. The student of phylogeny has had sufficient warning of the dangers which beset his path, from an inspection of the various phylogenetic trees, constructed mainly after study of existing beings only, so

> ". . . like the borealis race,
> That flit ere you can point their place,"

but recent researches amongst various groups of fossil organisms have further illustrated the danger of theorising upon insufficient data, especially suggestive being the discovery of closely similar forms which were formerly considered to be much more nearly related than now proves to be the case; thus Dr. Mojsisovics (*Abhandl. der k. k. geol. Reichsanst.*, vol. vi., 1893) has shown that Ammonites once referred to the same species are specifically distinct, though their hard parts have acquired similar structures, sometimes contemporaneously, sometimes at different times, and Mr. S. S. Buckman (*Quart. Journ. Geol. Soc.*, vol. li. p. 456, 1895) has observed the same thing, which he speaks of as "heterogenetic homœomorphy" in the case of certain brachiopods, whilst Prof. H. A. Nicholson and I (*Geol. Mag.*, December 4, 1895, vol. ii. p. 531) have given reasons for supposing that such heterogenetic homœomorphy, in the case of the graptolites, has sometimes caused the inclusion in one genus of forms which have arisen from two distinct genera. As the result of careful work, dangers of the nature here suggested will be avoided, and our chances of indicating lines of descent correctly will be much increased. It must be remembered that however plausible the lines of descent indicated by students of recent forms may be, the actual links in the chains can only be discovered by examination of the rocks, and it is greatly to be desired that more of our geologists, who have had a thorough training in the field, should receive in addition one as thorough in the zoological laboratory. Shall I be forgiven if I venture on the opinion that a certain suspicion which some of my zoological fellow countrymen have of geological methods, is due to their comparative ignorance of palæontology, and that it is as important for them to obtain some knowledge of the principles of geology as it is for the stratigraphical palæontologist to study the soft parts of creatures whose relatives he finds in the stratified rocks?

The main lines along which the organisms of some of the larger groups have been developed, have already been indicated by several palæontologists, and detailed work has been carried out in several cases. As examples, let me allude to the trilobites, of which a satisfactory natural classification was outlined by the great Barrande in those volumes of his monumental work which deal with the fossils of this order, whilst further indication of their natural inter-relationships has been furnished by Messrs. C. D. Walcott, G. F. Matthew, and others; to the graptolites, whose relationships have been largely worked out by Prof. C. Lapworth, *facile princeps* amongst students of the *Graptolitoidea*, to whom we look for a full account of the phylogeny of the group; to the brachiopods, which have been so ably treated by Dr. C. E. Beecher ("Development of the Brachiopoda," *Amer. Journ. Sci.*, ser. iii. vol. xli. (1891) p. 343, and vol. xliv. (1892) p. 133), largely from a study of recent forms, but also after careful study of those preserved in the fossil state; and to the echinids and lamellibranchs, whose history is being extensively elucidated by Dr. R. T. Jackson ("Phylogeny of the Pelecypoda," *Mem. Boston Soc. Nat. Hist.*, vol. iv. (1890) p. 277; and "Studies of Palæechinoidea," *Bull. Geol. Soc. Amer.*, vol. vii. (1896) p. 171), by methods somewhat similar to those pursued by Dr. Beecher. I might give other instances,[1] but have chosen some striking ones, four of which especially illustrate the great advances which are being made in the study of the palæontology of the invertebrates by our American brethren.

I have occupied the main part of my address with reasons for the need of conducting stratigraphical work with minute accuracy. Many of you may suppose that the necessity for working in this way is so obvious that it is a work of supererogation to insist upon it at great length; but experience has taught me that many geologists consider that close attention to details is apt to deter workers from arriving at important generalisations, in the present state of our science. A review of the past history of the science shows that William Smith, and those who followed after him, obtained their most important results by steady application to details, and subsequent generalisation, whilst the work of those who theorise on insufficient data is apt to be of little avail, though often demanding attention on account of its very daring, and because of the power of some writers to place erroneous views in an attractive light, just as

> ". . . the sun can fling
> Colours as bright on exhalations bred
> By weedy pool or pestilential swamp,
> As on the rivulet, sparkling where it runs,
> Or the pellucid lake."

[1] *E.g.* The following papers treating of the Cephalopoda:—A. Hyatt, "Genesis of the Arietidæ," Smithsonian Contributions, vol. xxvi. (1889); M. Neumayr, Jura-Studien I., "Ueber Phylloceraten," *Jahrb. der k. k. Geol. Reichsanst.*, vol. xxi. (1871) p. 297; L. Würtenberger, "Studien über die Stammesgeschichte der Ammoniten," Leipzig, 1880; S. S. Buckman, "A Monograph of the Inferior Oolite Ammonites of the British Islands," 1887 (*Monogr. Palæontographical Soc.*).

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Nor is there any reason to suppose that it will be otherwise in the future, and I am not one of those who consider that the brilliant discoveries were the exclusive reward of the pioneers in our science, and that labourers of the present day must be contented with the gleanings of their harvest; on the contrary, the discoveries which await the geologist will probably be as striking as are those which he has made in the past. The onward march of science is a rhythmic movement, with now a period of steady labour, anon a more rapid advance in our knowledge. It would perhaps be going too far to say that, so far as our science is concerned, we are living in a period rather of the former than of the latter character, though no great geological discovery has recently affected human thought in the way in which it was affected by the proofs of the antiquity of man, and by the publication of "The Origin of Species." If, however, we are to some extent gathering materials, rather than drawing far-reaching conclusions from them, I believe this is largely due to the great expansion which our science has undergone in recent years. It has been said that geology is "not so much one science, as the application of all the physical sciences to the examination and description of the structure of the earth, the investigation of the agencies concerned in the production of that structure, and the history of their action"; and the application of other sciences to the elucidation of the history of our globe has been so greatly extended of recent years, that we are apt to lose sight of the fact that geology is in itself a science, and that it is the special province of the geologist to get his facts at first hand from examination of the earth. The spectroscope and the telescope tell the geologist much; but his proper instrument is the hammer, and the motto of every geologist should be that which has been adopted for the Geological Congress, *Mente et malleo*.

At the risk of being compared to a child playing with edged tools, I cannot help referring to the bearing of modern stratigraphical research on the suggested replacement of a school of uniformitarianism by one of evolution. The distinguished advocate of Evolutionism, who addressed the Geological Society in 1869 upon the modern schools of geological thought, spoke of the school of evolution as though it were midway between those of uniformitarianism and catastrophism, as indeed it is logically, though, considering the tenets of the upholders of catastrophism, as opposed to those of uniformitarianism, at the time of that address, there is no doubt that evolutionism was rather a modification of the uniformitarianism of the period than intermediate between it and catastrophism, which was then practically extinct, at any rate in Britain. One of my predecessors in this chair, speaking upon this subject, says that "the good old British ship 'Uniformity,' built by Hutton and refitted by Lyell, has won so many glorious victories in the past, and appears still to be in such excellent fighting trim, that I see no reason why she should haul down her colours, either to 'catastrophe' or 'evolution.'" It may be so; but I doubt the expediency of nailing those colours to the mast. That Lyell, in his great work, proved that the agents now in operation, working with the same activity as that which they exhibit at the present day, *might* produce the phenomena exhibited by the stratified rocks, seems to be generally admitted, but that is not the same thing as proving that they *did* so produce them. Such proof can only be acquired by that detailed examination of the strata which I have advocated in this address, and at the time that the last edition of the "Principles" appeared, our knowledge of the strata was far less complete than it has subsequently become. It appears to me that we should keep our eyes open to the possibility of many phenomena presented by rocks, even newer than the Archæan rocks, having been produced under different conditions from those now prevalent. The depths and salinity of the oceans, the heights and extent of continents, the conditions of volcanic action, and many other things may have been markedly different from what they are at present, and it is surely unphilosophical to assume conditions to have been generally similar to those of the present day, on the slender data at our disposal. Lastly, uniformitarianism, in its strictest sense, is opposed to rhythmic recurrence of events. "Rhythm is the rule with nature; she abhors uniformity more than she does a vacuum," wrote Prof. Tyndall, many years ago, and the remark is worth noting by geologists. Why have we no undoubted signs of glacial epochs amongst the strata from early Cambrian times to the Great Ice Period, except in Permo-Carboniferous times? Is there not an apparent if not a real absence of manifestation of volcanic activity over wide areas of the earth in Mesozoic times? Were not Devonian, Permo-Triassic, and Miocene times periods of mountain-building over exceptionally wide areas, whilst the intervening periods were rather marked by quiet depression and sedimentation? A study of the evidence available in connection with questions like these suggests rhythmic recurrence. Without any desire to advocate hasty departure from our present methods of research, I think it should be clearly recognised that evolution may have been an important factor in changing the conditions even of those times of which the geologist has more direct knowledge. In this, as in many other questions, it is best to preserve an open mind; indeed, I think that geologists will do well to rest satisfied without an explanation to many problems, amongst them the one just referred to; and that working hypotheses, though useful, are better retained in the manuscript notebooks of the workers than published in the *Transactions* of Learned Societies, whence they filter out into popular works, to the great delight of a sceptical public should they happen to be overthrown.

May I trespass upon your patience for one moment longer? As a teacher of geology, with many years' experience in and out of a large University, I have come to the conclusion that geology is becoming more generally recognised as a valuable instrument of education. The memory, the reasoning faculties, and the powers of observation are alike quickened. The work in the open air, which is inseparable from a right understanding of the science, keeps the body in healthy condition. But over and above these benefits, the communing with nature, often in her most impressive moods, and the insignificance of events in a man's lifetime, as compared with the ceaseless changes through the long æons which have gone before, so influence man's moral nature, that they drive out his meaner thoughts and make him "live in charity with all men."

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY PROF. E. B. POULTON, F.R.S., PRESIDENT OF THE SECTION.

A VERY brief study of the proceedings of this Section in byegone 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—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 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;

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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's 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." ("Life and Letters," vol. iii.) 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 removes 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 animals [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." (Poulton, "Colours of Animals," p. 308.)

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. (See his letter to Darwin, November 23, 1859; "Life and Letters," vol. ii.) 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"[1] 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 his 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 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 the 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, &c., 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. (Sixth ed., 1872, p. 286.)

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 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 Palæozoic, Mesozoic, and 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."

[1] *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.

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Herbert Spencer, in his article on "Illogical Geology" in the *Universal Review* for July 1859 (reprinted in his "Essays," 1868, vol. i., pp. 324–376), 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 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 were 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 palæontological 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 the moon exert upon the sea, causing it to act as a sort of break upon the solid earth." (Anniv. Address to Geol. Soc., 1869.)

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 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 1000 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 Prof. Perry said in a letter to Prof. Tait (NATURE, January 3, 1895): "I know that solid rock is not like cobbler's wax, but 1000 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 coast-lines 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 geologist 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 1000 million years in which to do the work.

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

The spectacle presented by the geologist and biolog st, deeply shocked at Lord Kelvin'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° 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 7000° 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 structure, 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 "sorest 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 calculations of the age of the earth, has lent the weight of his authority to a period of ten million years, or half of Lord Kelvin's minimum. But

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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 7000° 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 (Rev. M. H. Close, in R. Dublin Soc., February 1878), 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, favouring 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 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° F. for every 50 feet, in 100,000,000 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 10,000,000,000 years the gradient at the surface is still 1° F. for every 50 feet; but that we have to descend to a depth of 1500 miles before we find the initial temperature of 7000° 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 (NATURE, January 3, 1895): "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 4000 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." (Newcomb's "Popular Astronomy," p. 523). 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 15⅓ 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 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. (*British Association Reports*, 1886, pp. 514–518.)

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 already been 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 at 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 6800 years. But this also supplies a

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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 seventy-three 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 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 he 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 favouring 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 two 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 evidence 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 allowance of time much beyond the narrow limits which recent physical speculation would concede."

In his letter to Prof. Perry (NATURE, January 3, 1895), Lord Kelvin says:—

"So far as underground heat alone is concerned, you are quite right that my estimate was 100 million, and please remark (*P. L. and A.*, vol. ii. p. 87) 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 yellow 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 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

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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.

We now come to the strictly biological part of our subject—to the inquiry as to how much of the whole scheme of organic evolution has been worked out in the time during which the fossiliferous rocks were formed, and how far, therefore, the time required by the geologist is sufficient.

It is first necessary to consider Lord Kelvin's attempt to rescue us from the dilemma in which we were placed by the insufficiency of time for evolution—the suggestion that life may have reached the earth on a meteorite. According to this view, the evolution which took place elsewhere may have been merely completed, in a comparatively brief space of time, on our earth.

We know nothing of the origin of life here or elsewhere, and our only attitude towards this or any other hypothesis on the subject is that of the anxious inquirer for some particle of evidence. But a few brief considerations will show that no escape from the demands for time can be gained in this way.

Our argument does not deal with the time required for the origin of life, or for the development of the lowest beings with which we are acquainted from the first formed beings, of which we know nothing. Both these processes may have required an immensity of time; but as we know nothing whatever about them, and have as yet no prospect of acquiring any information, we are compelled to confine ourselves to as much of the process of evolution as we can infer from the structure of living and fossil forms—that is, as regards animals, to the development of the simplest into the most complex Protozoa, the evolution of the Metazoa from the Protozoa, and the branching of the former into its numerous Phyla, with all their Classes, Orders, Families, Genera, and Species. But we shall find that this is quite enough to necessitate a very large increase in the time estimated by the geologist.

The Protozoa, simple and complex, still exist upon the earth in countless species, together with the Metazoan Phyla. Descendants of forms which in their day constituted the beginning of that scheme of evolution which I have defined above, descendants, furthermore, of a large proportion of those forms which, age after age, constituted the shifting phases of its onward progress, still exist, and in a sufficiently unmodified condition to enable us to reconstruct, at any rate in mere outline, the history of the past. Innumerable details and many phases of supreme importance are still hidden from us, some of them perhaps never to be recovered. But this frank admission, and the eager and premature attempts to expound too much, to go further than the evidence permits, must not be allowed to throw an undeserved suspicion upon conclusions which are sound and well supported, upon the firm conviction of every zoologist that the general trend of evolution has been, as I have stated it, that each of the Metazoan Phyla originated, directly or indirectly, in the Protozoa.

The meteorite theory would, however, require that the process of evolution went backward on a scale as vast as that on which it went forward, that certain descendants of some central type, coming to the earth on a meteorite, gradually lost their Metazoan complexity and developed backward into the Protozoa, throwing off the lower Metazoan Phyla on the way, while certain other descendants evolved all the higher Metazoan groups. Such a process would shorten the period of evolution by half, but it need hardly be said that all available evidence is entirely against it.

The only other assumption by means of which the meteorite hypothesis would serve to shorten the time is even more wild and improbable. Thus it might be supposed that the evolution which we believe to have taken place on this earth, really took place elsewhere—at any rate as regards all its main lines—and that samples of all the various phases, including the earliest and simplest, reached us by a regular meteoric service, which was established at some time after the completion of the scheme of organic evolution. Hence the evidences which we study would point to an evolution which occurred in some unknown world with an age which even Prof. Tait has no desire to limit.

If these wild assumptions be rejected, there remains the supposition that, if life was brought by a meteorite, it was life no higher than that of the simplest Protozoon—a supposition which leaves our argument intact. The alternative supposition, that one or more of the Metazoan Phyla were introduced in this way while the others were evolved from the terrestrial Protozoa, is hardly worth consideration. In the first place, some evidence of a part in a common scheme of evolution is to be found in every Phylum. In the second place, the gain would be small; the arbitrary assumption would only affect the evidence of the time required for evolution derived from the particular Phylum or Phyla of supposed meteoric origin.

The meteoric hypothesis, then, can only affect our argument by making the most improbable assumptions, for which, moreover, not a particle of evidence can be brought forward.

We are therefore free to follow the biological evidence fearlessly. It is necessary, in the first place, to expand somewhat the brief outline of the past history of the animal kingdom, which has already been given. Since the appearance of the "Origin of Species," the zoologist, in making his classifications, has attempted as far as possible to set forth a genealogical arrangement. Our purpose will be served by an account of the main outlines of a recent classification, which has been framed with a due consideration for all sides of zoological research, new and old, and which has met with general approval. Prof. Lankester divides the animal kingdom into two grades, the higher of which, the Enterozoa (Metazoa), were derived from the lower, the Plastidozoa (Protozoa). Each of these grades is again divided into two sub-grades, and each of these is again divided into Phyla, corresponding more or less to the older Sub-Kingdoms. Beginning from below, the most primitive animals in existence are found in the seven Phyla of the lower Protozoan sub-grade, the Gymnomyxa. Of these unfortunately only two, the Reticularia (Foraminifera) and Radiolaria, possess a structure which renders possible their preservation in the

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rocks. The lowest and simplest of these Gymnomyxa represent the starting-point of that scheme of organic evolution which we are considering to-day. The higher order of Protozoan life, the sub-grade Corticata, contains three Phyla, no one of which is available in the fossil state. They are, however, of great interest and importance to us as showing that the Protozoan type assumes a far higher organisation on its way to evolve the more advanced grade of animal life. The first-formed of these latter are contained in the two Phyla of the sub-grade Cœlentera, the Porifera or Sponges, and the Nematophora or Corals, Sea-Anemones, Hydrozoa and allied groups. Both of these Phyla are plentifully represented in the fossil state. It is considered certain that the latter of these, the Nematophora, gave rise to the higher sub-grade, the Cœlomata, or animals with a cœlom or body-cavity surrounding the digestive tract. This latter includes all the remaining species of animals in nine Phyla, five of which are found fossil—the Echinoderma, Gephyrea, Mollusca, Appendiculata, and Vertebrata.

Before proceeding further, I wish to lay emphasis on the immense evolutionary history which must have been passed through before the ancestor of one of the higher of these nine Phyla came into being. Let us consider one or two examples, since the establishment of this position is of the utmost importance for our argument. First, consider the past history of the Vertebrata—of the common ancestor of our Balanoglossus, Tunicates, Amphioxus, Lampreys, Fishes, Dipnoi, Amphibia, Reptiles, Birds, and Mammals. Although zoologists differ very widely in their opinions as to the affinities of this ancestral form, they all agree in maintaining that it did not arise direct from the Nematophora in the lower sub-grade of Metazoa, but that it was the product of a long history within the Cœlomate sub-grade. The question as to which of the other Cœlomate Phyla it was associated with will form the subject of one of our discussions at this meeting; and I will therefore say no more upon this period of its evolution, except to point out that the very question itself, "the ancestry of Vertebrates," only means a relatively small part of the evolutionary history of the Vertebrate ancestor within the Cœlomate group. For when we have decided the question of the other Cœlomate Phylum or Phyla to which the ancestral Vertebrate belonged, there remains of course the history of that Phylum or those Phyla earlier than the point at which the Vertebrate diverged, right back to the origin of the Cœlomata; while, beyond and below, the wide gulf between this and the Cœlentera had to be crossed, and then, probably after a long history as a Cœlenterate, the widest and most significant of all the morphological intervals—that between the lowest Metazoon and the highest Protozoon—was traversed. But this was by no means all. There remains the history within the higher Protozoan sub-grade, in the interval from this to the lower, and within the lower sub-grade itself, until we finally retrace our steps to the lowest and simplest forms. It is impossible to suppose that all this history of change can have been otherwise than immensely prolonged; for it will be shown below that all the available evidence warrants the belief that the changes during these earlier phases were at least as slow as those which occurred later.

If we take the history of another of the higher Phyla, the Appendiculata, we find that the evidence points in the same direction. The common ancestor of our Rotifera, earthworms, leeches, Peripatus, centipedes, insects, Crustacea, spiders and scorpions, and forms allied to all these, is generally admitted to have been Chætopod-like, and probably arose in relation to the beginnings of certain other Cœlomata Phyla, such as the Gephyrea and perhaps Mollusca. At the origin of the Cœlomate sub-grade, the common ancestor of all Cœlomate Phyla is reached, and its evolution has been already traced in the case of the Vertebrata.

What is likely to be the relation between the time required for the evolution of the ancestor of a Cœlomate Phylum and that required for the evolution, which subsequently occurred, within the Phylum itself? The answer to this question depends mainly upon the rate of evolution in the lower parts of the animal kingdom as compared with that in the higher. Contrary, perhaps, to anticipation, we find that all the evidences of rapid evolution are confined to the most advanced of the smaller groups within the highest Phyla, and especially to the higher classes of the Vertebrata. Such evidence as we have strongly indicates the most remarkable persistence of the lower animal types. Thus in the class Imperforata of the Reticularia (Foraminifera) one of our existing genera (*Saccamina*) occurs in the Carboniferous strata, another (*Trochammina*) in the Permian, while a single new genus (*Receptaculites*) occurs in the Silurian and Devonian. The evidence from the class Perforata is much stronger, the existing genera *Nodosaria*, *Dentalina*, *Textularia*, *Grammostomum*, *Valvulina*, and *Nummulina* all occurring in the Carboniferous, together with the new genera *Archædiscus* (?) and *Fusulina*.

I omit reference to the much-disputed Eozoon from the Laurentian rocks far below the horizon, which for the purpose of this address I am considering as the lowest fossiliferous stratum. We are looking forward to the new light which will be thrown upon this form in the communication of its veteran defender, Sir William Dawson, whom we are all glad to welcome.

Passing the Radiolaria, with delicate skeletons less suited for fossilisation, and largely pelagic and therefore less likely to reach the strata laid down along the fringes of the continental areas, the next Phylum which is found in a fossil state is that of the Porifera, including the sponges, and divided into two classes, the Calcispongiæ and Silicospongiæ. Although the fossilisation of sponges is in many cases very incomplete, distinctly recognisable traces can be made out in a large number of strata. From these we know that representatives of all the groups of both classes (except the Halisarcidæ, which have no hard parts) occurred in the Silurian, Devonian, and Carboniferous systems. The whole Phylum is an example of long persistence with extremely little change. And the same is true of the Nematophora: new groups indeed come in, sometimes extremely rich in species, such as the Palæozoic Rugose corals and Graptolites; but they existed side by side with representatives of existing groups, and they are not in themselves primitive or ancestral. A study of the immensely numerous fossil corals reveals no advance in organisation, while researches into the structure of existing Alcyonaria and Hydrocorallina have led to the interpretation of certain Palæozoic forms which were previously obscure, and the conclusion that they find their place close beside the living species.

All available evidence points to the extreme slowness of progressive evolutionary changes in the Cœlenterate Phyla, although the Protozoa, if we may judge by the Reticularia (Foraminifera), are even more conservative.

When we consider later on the five Cœlomate Phyla which occur fossil, we shall find that the progressive changes were slower and indeed hardly appreciable in the two lower and less complex Phyla, viz. the Echinoderma and Gephyrea, as compared with the Mollusca, Appendiculata, and Vertebrata.

Within these latter Phyla we have evidence for the evolution of higher groups presenting a more or less marked advance in organisation. And not only is the rate of development more rapid in the highest Phyla of the animal kingdom, but it appears to be most rapid when dealing with the highest animal tissue, the central nervous system. The chief, and doubtless the most significant, difference between the early Tertiary mammals and those which succeeded them, between the Secondary and Tertiary reptiles, between man and the mammals most nearly allied to him, is a difference in the size of the brain. In all these cases an enormous increase in this, the dominant tissue of the body, has taken place in a time which, geologically speaking, is very brief.

When speaking later on upon the evolution which has taken place within the Phyla, further details upon this subject will be given, although in this as in other cases, the time at our disposal demands that the exposition of evidence must largely yield to an exposition of the conclusions which follow from its study. And undoubtedly a study of all the available evidence points very strongly to the conclusion that in the lower grade, sub-grades, and Phyla of the animal kingdom, evolution has been extremely slow as compared with that in the higher. We do not know the reason. It may be that this remarkable persistence through the stratified series of deposits is due to an innate fixity of constitution which has rigidly limited the power of variation; or, more probably perhaps, that the lower members of the animal kingdom were, as they are now, more closely confined to particular environments, with particular sets of conditions, with which they had to cope, and, this being successfully accomplished, natural selection has done little more than keep up a standard of organisation which was sufficient for their needs; while the higher and more aggressive forms ranging over many environments, and always prone to encounter new sets of conditions, were compelled to undergo responsive changes or to succumb. But whatever be the cause, the fact remains, and is of the highest importance for our argument. When the ancestor

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of one of the higher Phyla was associated with the lower Phyla of the Cœlomate sub-grade, when further back it passed through a Cœlenterate, a higher Protozoan, and finally a lower Protozoan phase, we must believe that its evolution was probably very slow as compared with the rate which it subsequently attained. But this conclusion is of the utmost importance; for the history contained in the stratified rocks nowhere reveals to us the origin of a Phylum. And this is not mere negative evidence, but positive evidence of the most unmistakable character. All the five Cœlomate Phyla which occur fossil appear low down in the Palaeozoic rocks, in the Silurian or Cambrian strata, and they are represented by forms which are very far from being primitive, or, if primitive, are persistent types, such as Chiton, which are now living. Thus Vertebrata are represented by fishes, both sharks and ganoids; the Appendiculata by cockroaches, scorpions, Limulids, Trilobites, and many Crustacea; the Mollusca by Nautilus and numerous allied genera, by Dentalium, Chiton, Pteropods, and many Gastropods and Lamellibranchs; the Gephyrea by very numerous Brachiopods, and many Polyzoa; the Echinoderma by Crinoids, Cystoids, Blastoids, Asteroids, Ophiuroids, and Echinoids. It is just conceivable, although, as I believe, most improbable, that the Vertebrate Phylum originated at the time when the earliest known fossiliferous rocks were laid down. It must be remembered, however, that an enormous morphological interval separates the fishes which appear in the Silurian strata from the lower branches, grades, and classes of the Phylum in which Balanoglossus, the Ascidians, Amphioxus, and the Lampreys are placed. The earliest Vertebrates to appear are, in fact, very advanced members of the Phylum, and, from the point of view of anatomy, much nearer to man than to Amphioxus. If, however, we grant the improbable contention that so highly organised an animal as a shark could be evolved from the ancestral vertebrate in the period which intervened between the earliest Cambrian strata and the Upper Silurian, it is quite impossible to urge the same with regard to the other Phyla. It has been shown above that when these appear in the Cambrian and Silurian, they are flourishing in full force, while their numerous specialised forms are a positive proof of a long antecedent history within the limits of the Phylum.

If, however, we assume for a moment that the Phyla began in the Cambrian, the geologist's estimate must still be increased considerably, and perhaps doubled, in order to account for the evolution of the higher Phyla from forms as low as many which are now known upon the earth; unless, indeed, it is supposed, against the whole weight of all such evidence as is available, that the evolutionary history in these early times was comparatively rapid.

To recapitulate, if we represent the history of animal evolution by the form of a tree, we find that the following growth took place in some age antecedent to the earliest fossil records, before the establishment of the higher Phyla of the Animal Kingdom. The main trunk, representing the lower Protozoa divided, originating the higher Protozoa; the latter portion again divided, probably in a threefold manner, originating the two lowest Metazoan Phyla, constituting the Cœlentera. The branch representing the higher of these Phyla, the Nematophora, divided, originating the lower Cœlomate Phyla, which again branched and originated the higher Phyla. And, as has been shown above, the relatively ancestral line, at every stage of this complex history, after originating some higher line, itself continued down to the present day, throughout the whole series of fossiliferous rocks, with but little change in its general characters, and practically nothing in the way of progressive evolution. Evidences of marked advance are to be found alone in the most advanced groups of the latest highest products—the Phyla formed by the last of these divisions.

It may be asked how is it possible for the zoologist to feel so confident as to the past history of the various animal groups? I have already explained that he does not feel this confidence as regards the details of the histoyr, but as to its general lines. The evidence which leads to this conviction is based upon the fact that animal structure and mode of development can be, and have been, handed down from generation to generation from a period far more remote than that which is represented by the earliest fossils; that fundamental facts in structure and development may remain changeless amid endless changes of a more general character; that especially favourable conditions have preserved ancestral forms comparatively unchanged. Working upon this material, comparative anatomy and embryology can reconstruct for us the general aspects of a history which took place long before the Cambrian rocks were deposited. This line of reasoning may appear very speculative and unsound, and it may easily become so when pressed too far. But applied with due caution and reserve, it may be trusted to supply us with an immense amount of valuable information which cannot be obtained in any other way. Furthermore, it is capable of standing the very true and searching test supplied by the verification of predictions made on its authority. Many facts taken together lead the zoologist to believe that A was descended from C through B; but if this be true, B should possess certain characters which are not known to belong to it. Under the inspiration of hypothesis a more searching investigation is made, and the characters are found. Again, that relatively small amount of the whole scheme of animal evolution which is contained in the fossiliferous rocks has furnished abundant confirmation of the validity of the zoologist's method. The comparative anatomy of the higher Vertebrate Classes leads the zoologist to believe that the toothless beak and the fused caudal vertebræ of a bird were not ancestral characters, but were at some time derived from a condition more comfortable to the general plan of vertebrate construction, and especially to that of reptiles. Numerous secondary fossils prove to us that the birds of that time possessed teeth and separate caudal vertebræ, culminating in the long lizard-like tail of Archæopteryx.

Prediction and confirmation of this kind, both zoological and palæontological, have been going on ever since the historic point of view was adopted by the naturalist as the outcome of Darwin's teaching, and the zoologist may safely claim that his method, confirmed by palæontology so far as evidence is available, may be extended beyond the period in which such evidence is to be found.

And now our last endeavour must be to obtain some conception of the amount of evolution which has taken place within the higher Phyla of the Animal Kingdom during the period in which the fossiliferous rocks were deposited. The evidence must necessarily be considered very briefly, and we shall be compelled to omit the Vertebrata altogether.

The Phylum Appendiculata is divided by Lankester into three branches, the first containing the Rotifera, the second the Chætopoda, the third the Arthropoda. Of these the second is the oldest, and gave rise to the other two, or, at any rate, to the Arthropoda, with which we are alone concerned, inasmuch as the fossil records of the others are insufficient. The Arthropoda contain seven Classes, divided into two grades, according to the presence or absence of antennæ—the Ceratophora, containing the Peripatoidea, the Myriapoda, and the Hexapoda (or insects); the Acerata, containing the Crustacea, Arichnida, and two other classes (the Pantopoda and Tardigrada) which we need not consider. The first Class of the antenna-bearing group contains the single genus Peripatus—one of the most interesting and ancestral of animals, as proved by its structure and development, and by its immense geographical range. Ever since the researches of Moseley and Balfour, extended more recently by those of Sedgwick, it has been recognised as one of the most beautiful of the connecting links to be found amongst animals, uniting the antenna-bearing Arthropods, of which it is the oldest member, with the Chætopods. Peripatus is a magnificent example of the far-reaching conclusions of zoology, and of its superiority to palæontology as a guide in unravelling the tangled history of animal evolution. Peripatus is alive to-day, and can be studied in all the details of its structure and development; it is infinitely more ancestral, and tells of a far more remote past than any fossil Arthropod, although such fossils are well known in all the older of the Palæozoic rocks. And yet Peripatus is not known as a fossil. Peripatus has come down, with but little change, from a time, on a moderate estimate, at least twice as remote as the earliest known Cambrian fossil. The agencies which, it is believed, have crushed and heated the Archæan rocks so as to obliterate the traces of life which they contained were powerless to efface this ancient type, for, although the passing generations may have escaped record, the likeness of each was stamped on that which succeeded it, and has continued down to the present day. It is, of course, a perfectly trite and obvious conclusion, but not the less one to be wondered at, that the force of heredity should thus far outlast the ebb and flow of terrestrial change throughout the vast period over which the geologist is our guide.

If, however, the older Palæozoic rocks tell us nothing of the origin of the antennæ-bearing Arthropods, what do they tell us of the history of the Myriapod and Hexapod Classes?

The Myriapods are well represented in Palæozoic strata, two species being found in the Devonian and no less than thirty-two

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in the Carboniferous. Although placed in an Order (Archipolypoda) separate from those of living Myriapods, these species are by no means primitive, and do not supply any information as to the steps by which the Class arose. The imperfection of the record is well seen in the traces of this Class; for between the Carboniferous rocks and the Oligocene there are no remains of undoubted Myriapods.

We now come to the consideration of insects, of which an adequate discussion would occupy a great deal too much of your time. An immense number of species are found in the Palæozoic rocks, and these are considered by Scudder, the great authority on fossil insects, to form an Order, the Palæodictyoptera, distinct from any of the existing Orders. The latter, he believes, were evolved from the former in Mesozoic times. These views do not appear to derive support from the wonderful discoveries of M. Brongniart[1] in the Upper Carboniferous of Commentry in the Department of Allier in Central France. Concerning this marvellous assemblage of species, arranged by their discoverer into 46 genera and 101 species, Scudder truly says:

"Our knowledge of Palæozoic insects will have been increased three or fourfold at a single stroke. . . . No former contribution in this field can in any way compare with it, nor even all former contributions taken together." (S. H. Scudder, *Am. Journ. Sci.*, vol. xlvii., February 1894, Art. viii.)

When we remember that the group of fossil insects, of which so much can be affirmed by so great an authority as Scudder, lived at one time and in a single locality, we cannot escape the conclusion that the insect fauna of the habitable earth during the whole Palæozoic period was of immense importance and variety. Our knowledge of this single group of species is largely due to the accident that coal-mining in Commentry is carried on in the open air.

Now, these abundant remains of insects, so far from upholding the view that the existing orders had not been developed in Palæozoic times, are all arranged by Brongniart in four out of the nine orders into which insects are usually divided, viz. the Orthoptera, Neuroptera, Thysanoptera, and Homoptera. The importance of the discovery is well seen in the Neuroptera, the whole known Palæozoic fauna of this order being divided into 45 genera and 99 species, of which 33 and 72 respectively have been found at Commentry.

Although the Carboniferous insects of Commentry are placed in new families, some of them come wonderfully near those into which existing insects are classified, and obviously form the precursors of these. This is true of the Blattidæ, Phasmidæ, Acridiidæ, and Locustidæ among the Orthoptera, the Perlidæ among the Neuroptera, and the Fulgoridæ among the Homoptera. The differences which separate these existing families from their Carboniferous ancestors are most interesting and instructive. Thus the Carboniferous cockroaches possessed ovipositors, and probably laid their eggs one at a time, while ours are either viviparous or lay their eggs in a capsule. The Protophasmidæ resemble living species in the form of the head, antennæ, legs, and body; but while our species are either wingless or, with the exception of the female Phyllidæ, have the anterior pair reduced to tegmina, useless for flight, those of Palæozoic times possessed four well-developed wings. The forms representing locusts and grasshoppers (Palæacrididæ) possessed long slender antennæ like the green grasshoppers (Locustididæ), from which the Acridiidæ are now distinguished by their short antennæ. The divergence and specialisation which is thus shown is amazingly small in amount. In the vast period between the Upper Carboniferous rocks and the present day the cockroaches have gained a rather different wing venation, and have succeeded in laying their eggs in a manner rather more specialised than that of insects in general; the stick insects and leaf insects have lost or reduced their wings, the grasshoppers have shortened their antennæ. These, however, are the insects which most closely resemble the existing species; let us turn to the forms which exhibit the greatest differences. Many species have retained in the adult state characters which are now confined to the larval stage of existence, such as the presence of tracheal gills on the sides of the abdomen. In some the two membranes of the wing were not firmly fixed together, so that the blood could circulate freely between them. On the other hand, they are not very firmly fixed together in existing insects. Another important point was the condition of the three thoracic segments, which were quite distinct and separate, instead of being fused as they are now in the imago stage. This external difference probably also extended to the nervous system, so that the thoracic ganglia were separate instead of concentrated. The most interesting distinction, however, was the possession by many species of a pair of prothoracic appendages much resembling miniature wings, and which especially suggest the appearance assumed by the anterior pair (tegmina) in existing Phasmidæ. There is some evidence in favour of the view that they were articulated, and they exhibit what appears to be a trace of venation. Brongniart concludes that in still earlier strata, insects with six wings will be discovered, or rather insects with six of the tracheal gills sufficiently developed to serve as parachutes. Of these, the two posterior pair developed into the wings as we know them, while the anterior pair degenerated, some of the Carboniferous insects presenting us with a stage in which degeneration had taken place, but was not complete.

[1] Charles Brongniart.—"Recherches pour servir à l'Histoire des Insectes fossiles des temps primaires, précédées d'une Etude sur la nervation des ailes des Insectes." 1894.

One very important character was, as I have already pointed out, the enormous size reached by insects in this distant period. This was true of the whole known fauna as compared with existing species, but it was especially the case with the Protodonata, some of these giant dragon-flies measuring over two feet in the expanse of the wings.

As regards the habits of life and metamorphoses, Brongniart concludes that some species of Protoephemeridæ, Protoperlidæ, &c., obtained their food in an aquatic larval stage, and did not require it when mature. He concludes that the Protodonata fed on other animals, like our dragon-flies; that the Palæacrididæ were herbivorous like our locusts and grasshoppers, the Protolocustidæ herbivorous and animal feeders like our green grasshoppers, the Palæoblattidæ omnivorous like our cockroaches. The Homoptera, too, had elongated sucking mouth-parts like the existing species. It is known that in Carboniferous times there was a lake with rivers entering it, at Commentry. From their great resemblance to living forms of known habits, it is probable that the majority of these insects lived near the water and their larvæ in it.

When we look at this most important piece of research as a whole, we cannot fail to be struck with the small advance in insect structure which has taken place since Carboniferous times. All the great questions of metamorphosis, and of the structures peculiar to insects, appear to have been very much in the position in which they are to-day. It is indeed probable enough that the orders which zoologists have always recognised as comparatively modern and specialised, such as the Lepidoptera, Coleoptera, and Hymenoptera, had not come into existence. But as regards the emergence of the Class from a single primitive group, as regards its approximation towards the Myriapods, which lived at the same time, and of both towards their ancestor Peripatus, we learn absolutely nothing. All we can say is that there is evidence for the evolution of the most modern and specialised members of the Class, and some slight evolution in the rest. Such evolution is of importance as giving us some vague conception of the rate at which the process travels in this division of the Arthropoda. If we look upon development as a series of paths which, by successively uniting, at length meet in a common point, then some conception of the position of that distant centre may be gained by measuring the angle of divergence and finding the number of unions which occur in a given length. In this case, the amount of approximation and union shown in the interval between the Carboniferous period and the present day is relatively so small that it would require to be multiplied many times before we could expect the lines to meet in the common point, the ancestor of insects, to say nothing of the far more distant past in which the Tracheate Arthropods met in an ancestor presenting many resemblances to Peripatus. But it must not be forgotten that all this vast undefined period is required for the history of one of the two grades of one of the three branches of the whole Phylum.

Turning now to the brief consideration of the second grade of Arthropods, distinguished from the first grade by the absence of antennæ, the Trilobites are probably the nearest approach to an ancestral form met with in the fossil state. Now that the possession of true antennæ is certain, it is reasonable to suppose that the Trilobites represent an early Class of the Aceratous branch which had not yet become Aceratous. They are thus of the deepest interest in helping us to understand the origin of the antennaless branch, not by the ancestral absence, but by the loss of true antennæ which formerly existed in the group. But the Trilobites did not themselves originate the other Classes, at any

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rate during Palæozoic times. They represent a large and dominant Class, presenting more of the characters of the common ancestor than the other Classes; but the latter had diverged and had become distinct long before the earliest fossiliferous rocks; for we find well-marked representatives of the Crustacea in Cambrian, and of the Arachnida in Silurian strata. The Trilobites, moreover, appear in the Cambrian with many distinct and very different forms, contained in upwards of forty genera, so that we are clearly very far from the origin of the group.

Of the lower group of Crustacea, the Entomostraca, the Cirripedes are represented by two genera in the Silurian, the Ostracodes by four genera in the Cambrian and over twenty in the Silurian; of these latter two genera, Cythere and Bairdia, continue right through the fossiliferous series and exist at the present day. Remains of Phyllopods are more scanty, but can be traced in the Devonian and Carboniferous rocks. The early appearance of the Cirripedes is of especial interest, inasmuch as the fixed condition of these forms in the mature state is certainly not primitive, and yet, nevertheless, appears in the earliest representatives.

The higher group, the Malacostraca, are represented by many genera of Phyllocarida in the Silurian and Devonian, and two in the Cambrian. These also afford a good example of the imperfection of the record, inasmuch as no traces of the group are to be found between the Carboniferous and our existing fauna in which it is represented by the genus Nebalia. The Phyllocarida are recognised as the ancestors of the higher Malacostraca, and yet these latter already existed—in small numbers, it is true—side by side with the Phyllocarida in the Devonian. The evolution of the one into the other must have been much earlier. Here, as in the Arthropoda, we have evidence of progressive evolution among the highest groups of the Class, as we see in the comparatively late development of the Brachyura as compared with the Macrura. We find no trace of the origin of the Class, or of the larger groups into which it is divided, or, indeed, of the older among the small groupings into families and genera.[1]

Of the Arachnida, although some of the most wonderful examples of persistent types are to be found in this class, but little can be said. Merely to state the bare fact that three kinds of scorpion are found in the Silurian, two Pedipalpi, eight scorpions, and two spiders in the Carboniferous, is sufficient to show that the period computed by geologists must be immensely extended to account for the development of this Class alone, inasmuch as it existed in a highly specialised condition almost at the beginning of the fossiliferous series; while, as regards so extraordinarily complex an animal as a scorpion, nothing apparent in the way of progressive development has happened since. Prof. Lankester has, however, pointed out to me that the Silurian scorpions possessed heavier limbs than those of existing species, and this is a point in favour of their having been aquatic, like their near relation, Limulus. If so, it is probable that they possessed external gills, not yet inverted to form the lung-book. The Merostomata are of course a Palæozoic group, and reach their highest known development at their first appearance in the Silurian; since then they have done nothing but disappear gradually, leaving the single genus Limulus, unmodified since its first appearance in the Trias, to represent them. It is impossible to find clearer evidence of the decline rather than the rise of a group. No progressive development, but a gradual or rapid extinction, and consequent reduction in the number of genera and species, is a summary of the record of the fossiliferous rocks as regards this group and many others, such as the Trilobites, the Brachiopods, and the Nautilidæ. All these groups begin with many forms in the oldest fossiliferous rocks, and three of them have left genera practically unchanged from their first appearance to the present day. What must have been the time required to carry through the vast amount of structural change implied in the origin of these persistent types and the groups to which they belong—a period so extended that the interval between the oldest Palæozoic rocks and the present day supplies no measurable unit?

But I am digressing from the Appendiculate Phylum. We have seen that the fossil record is unusually complete as regards two Classes in each grade of the Arthropod branch, but that these Classes were well developed and flourishing in Palæozoic times. The only evidence of progressive evolution is in the development of the highest orders and families of the Classes. Of the origin of the Classes nothing is told, and we can hardly escape the conclusion that for the development of the Arthropod branches from a common Chætopod-like ancestor, and for the further development of the Classes of each branch, a period many times the length of the fossiliferous series is required, judging from the insignificant amount of development which has taken place during the formation of this series.

[1] For an account of the evolution of the Crustacea, see the Presidential Addresses to the Geological Society in 1895 and 1896 by Dr. Henry Woodward.

It is impossible to consider the other Cœlomate Phyla as I have done the Appendiculata. I can only briefly state the conclusions to which we are led.

As regards the Molluscan Phylum, the evidence is perhaps even stronger than in the Appendiculata. Representatives of the whole of the Classes are, it is believed, found in the Cambrian or Lower Silurian. The Pteropods are generally admitted to be a recent modification of the Gastropods, and yet, if the fossils described in the genera Conularia, Hyolithes, Pterotheca, &c., are true Pteropods, as they are supposed to be, they occur in the Cambrian and Silurian strata, while the group of Gastropods from which they almost certainly arose, the Bullidæ, are not known before the Trias. Furthermore, the forms which are clearly the oldest of the Pteropods—Limacina and Spiriales—are not known before the beginning of the Tertiary Period. Either there is a mistake in the identification of the Palæozoic fossils as Pteropods, or the record is even more incomplete than usual, and the most specialised of all Molluscan groups had been formed before the date of the earliest fossiliferous rocks. If this should hereafter be disproved, there can be no doubt about the early appearance of the Molluscan Classes, and that it is the irony of an incomplete record which places the Cephalopods and Gastropods in the Cambrian and the far more ancestral Chiton no lower than the Silurian. Throughout the fossiliferous series the older families of Gastropods and Lamellibranchs are followed by numerous other families, which were doubtless derived from them; new and higher groups of Cephalopods were developed, and, with the older groups, either persisted until the present time or became extinct. But in all this splitting up of the Classes into groups of not widely different morphological value, there is very little progressive modification, and, taking such changes in such a period as our unit for the determination of the time which was necessary for the origin of the Classes from a form like Chiton, we are led to the same conclusion as that which followed from the consideration of the Appendiculata, viz. that the fossiliferous series would have to be multiplied several times in order to provide it.

Of the Phylum Gephyrea, I will only mention the Brachiopods, which are found in immense profusion in the early Palæozoic rocks and which have occupied the subsequent time in becoming less dominant and important. So far from helping us to clear up the mystery which surrounds the origin of the Class, the earliest forms are quite as specialised as those living now, and, some of them (Lingula Discina) even generically identical. The demand for time to originate the group is quite as grasping as that of the others we have been considering.

All the Classes of Echinoderma, except the Holothurians, which do not possess a structure favourable for fossilisation, are found early in the Palæozoic rocks, and many of them in the Cambrian. Although these early forms are very different from those which succeeded them in the later geological periods, they do not possess a structure which can be recognised as in any way primitive or ancestral. The Echinoderma are the most distinct and separate of all the Cœlomate Phyla, and they were apparently equally distinct and separate at the beginning of the fossiliferous series.

In concluding this imperfect attempt to deal with a very vast subject in a very short time, I will remind you that we were led to conclude that the evolution of the ancestor of each of the higher animal Phyla, probably occupied a very long period, perhaps as long as that required for the evolution which subsequently occurred within the Phylum. But the consideration of the higher Phyla which occur fossil, except the Vertebrata, leads to the irresistible conclusion that the whole period in which the fossiliferous rocks were laid down must be multiplied several times for this later history alone. The period thus obtained requires to be again increased, and perhaps doubled, for the earlier history.

In the preparation of the latter part of this address I have largely consulted Zittel's great work. I wish also to express my thanks to my friend Prof. Lankester, whom I have consulted on many of the details, as well as the general plan which has been adopted.

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SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY SIR DOUGLAS FOX, VICE-PRESIDENT INSTITUTION OF CIVIL ENGINEERS, PRESIDENT OF THE SECTION.

IT is rather over a quarter of a century since the British Association last held its meeting in the hospitable city of Liverpool. The intervening period has been one of unparalleled progress, both generally and locally, in the many branches of knowledge and of practical application covered by Civil and Mechanical Engineering, and therefore rightly coming within the limits for discussion in the important Section of the Association in which we are specially interested.

During these twenty-five years the railway system of the British Isles, which saw one of its earliest developments in this neighbourhood, has extended from 15,376 miles, at a capital cost of 552,680,000*l.*, to 21,174 miles, at a capital cost of 1,001,000,000*l.* The railway system of the United States has more than trebled in the same period, and now represents a total mileage of 181,082, with a capital cost of 11,565,000,000 dollars.

The Forth and Brooklyn, amongst bridges, the Severn and St. Gothard, amongst tunnels, the gigantic works for the water-supply of towns, are some of the larger triumphs of the civil engineer; the substitution of steel for iron for so many purposes, the perfecting of the locomotive, of the marine engine, of hydraulic machinery, of gas and electric plant, those of the mechanical branch of the profession.

The city of Liverpool and its sister town of Birkenhead have witnessed wonderful changes during the period under review. Great and successful efforts have been made to improve the water-gate to the noble estuary, which forms the key to the city's greatness and prosperity; constant additions have been made to the docks, which are by far the finest and most extensive in the world. The docks on the two sides of the river have been amalgamated into one great trust. In order properly to serve the vast and growing passenger and goods traffic of the port, the great railway companies have expended vast sums on the connections with the dock lines and on the provision of station accommodation, and there have been introduced, in order to facilitate intercommunication, the Mersey Railway, crossing under the river, and carrying annually nearly 10 millions of passengers, and the Liverpool Overhead Railway, traversing for six miles the whole line of docks, and already showing a traffic of 7½ millions of passengers per annum. A very complete water-side station connected with the landing-stage has been lately opened by the Dock Board in connection with the London and North-Western Railway. In addition to this, the water-supply from Rivington and Vyrnwy has now been made one of the finest in the world.

The following comparative figures, kindly supplied by Mr. K. Miles Burton, may be of interest:—

	1871 (estimated).	1895 (estimated).
Population of Liverpool ...	493,405	641,000
Population of Birkenhead ...	65,971	109,000
	Acres.	Acres.
Area of docks, Liverpool, about	236	362½
Area of docks, Birkenhead, about	147	160
	383	552½
Number of steamers using the port	7,448	18,429
Average tonnage of six largest vessels entering the port ...	2,890	6,822

The following figures show the importance of the local railway traffic:—

Number of passenger stations within the boroughs ...	—	58
Number of goods stations ...	—	50
Number of passengers crossing the Mersey in the twelve months (Woodside Ferry)	—	7,143,088
Number of passengers crossing the Mersey in the twelve months (Mersey Railway)	—	6,976,299

To the hydraulic engineer there are few rivers of more interest, and presenting more complicated problems, than the Mersey and its neighbours, the Dee and the Ribble. They all possess vast areas of sand covered at high water, but laid dry as the tide falls, and in each case the maintenance of equilibrium between the silting and scouring forces is of the greatest importance to the welfare of the trading communities upon their banks. The enclosure of portions of the areas of the respective estuaries for the purposes of the reclamation of land, or for railway or canal embankments, may thus have far-reaching effects, diminishing the volume of the tidal flow and reducing the height of tide in the upper reaches of the rivers. Some idea of the magnitude of these considerations may be derived from the fact that a spring tide in the Mersey brings in through the narrows between Birkenhead and Liverpool 710 millions of cubic yards of water to form a scouring force upon the ebb. The tidal water is heavily laden with silt, which is deposited in the docks, and, at slack water, upon the sandbanks. The former is removed by dredging, and amounts to some 1,100,000 cubic yards per annum; the latter is gradually fretted down into the channels and carried out to sea before the ebb. Whilst a considerable portion of the narrows is kept scoured, in some places right down to the sandstone rock, there is a tendency, on the Liverpool side, near the landing-stage, to silt up, a difficulty counteracted, to some extent, by the extensive sluicing arrangements introduced by Mr. George Fosbery Lyster, the engineer of the Mersey Docks and Harbour Board.

Very extensive and interesting operations have been carried on by the Board in connection with the bar at the mouth of the river. Dredgers specially designed for the purpose have been employed for some six years, with the result that 15,142,600 tons of sand and other dredged matter have been removed, and the available depth of water at low-water increased from 11 to 24 feet in a channel 1500 feet in width.

Those who have made the transatlantic passage in former years can more readily appreciate the very great advantage accruing from this great improvement.

Formerly vessels arriving off the port on a low tide had to wait for some hours for the water-level to rise sufficiently to enable them to cross the bar; the result of a large vessel lying outside, rolling in the trough of the sea with her engines stopped, was that not infrequently this proved to be the worst part of the voyage between New York and Liverpool, and passengers who had escaped the malady of sea-sickness throughout the voyage were driven to their cabins and berths within three or four hours of landing.

Owing to the very successful dredging operations, ships of largest size can now enter or depart from the Mersey at any state of the tide, and they are also able to run alongside the landing-stage without the intervention of a tender.

Such vessels as the *Teutonic* or *Majestic*, of nearly 10,000 registered tonnage, 566 feet in length, 57 feet wide, and 37 feet deep; or the still larger vessels, the *Campania* or *Lucania*, of nearly 13,000 tons register, 601 feet in length, 65 feet in width, and 38 feet in depth, can be seen, on mail days, lying alongside.

Whilst the estuary of the Mersey presents a narrow entrance with a wide internal estuary, the Dee, owing to extensive reclamation of land in the upper reaches, has a wide external estuary leading to an embanked river of very limited width, up which the tide rushes with great velocity laden with silt, rising in some two hours, then, during a short time of slack water, depositing the silt, which is not removed by the ebb-tide, spread over some ten hours, and therefore having comparatively little velocity. In this case, also, the outer estuary shows a great tendency to silt up beyond the reach of any but the highest spring tides.

The reclamation of the Ribble has not yet proceeded so far as to so seriously affect the general conditions of the estuary; but here, also, there is a constant tendency in the channels to shift, and the erosion which takes place when a high tide and wind combine is very remarkable.

A most important improvement was introduced in 1886, by Mr. G. F. Lyster, when it was decided to raise the water-level in certain of the docks by pumping, the wharves being heightened in proportion, and half-tide basins, or locks, made use of to compensate for the difference of level.

The area of the docks so treated in Liverpool is 78 acres, whilst at Birkenhead the whole area of the docks on that side of the river, amounting to 160 acres, is so raised.

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The hydraulic power used in the docks is very large, the indicated horse-power of the engines amounting to 1673 in the case of Liverpool, and 874 in that of Birkenhead; whilst the Hydraulic Power Company are supplying some 1000 h.p. to railways and private firms.

The direct-acting hydraulic lifts of the Mersey Railway have now been at work for ten years, and through these, at St. James's Station, no less than 75,000,000 to 80,000,000 of passengers have passed with regularity and safety.

It is remarkable that, whilst Great Britain led the van in the introduction of steam locomotion, she has lagged in the rear as regards electric and other mechanical traction. This arose in the first instance from mistaken legislation, which strangled electrical enterprise, which is still much hampered by the reluctance of public authorities to permit the introduction of the necessary poles and wires into towns.

At the date of the latest published returns there were at work in the United States no less than 12,133 miles of electric, in addition to 599 miles of cable, tramway. Hardly a large village but has its installation, and vast have been the advantages derived from these facilities. In Brooklyn one company alone owns and works 260 miles of overhead trolley lines. With the exception of some small tramways at Portrush, Brighton, Blackpool, South Staffordshire, Hartlepool, &c., the only examples in this country of serious attempts to apply electro-motive force to the carriage of passengers are the City and South London Railway and the Liverpool Overhead Railway, the latter being the latest constructed, and having, therefore, benefited by the experience gained upon the London line.

This railway is over six miles long, a double line of the normal, or 4 ft. 8½ in. gauge, running on an iron viaduct for the whole length of the docks; the installation is placed for convenience of coal supply about one-third of the distance from the northern end. Particulars of this interesting work will be placed before the Section, but suffice it to say that a train service of three minutes each way is readily maintained, with trains carrying 112 passengers each, at an average speed of twelve miles per hour, including stoppages at fourteen intermediate stations. During the last year, as before stated, 7½ million passengers were carried, the cost of traction per train mile being 3·4*d.*

The Hartlepool Tramway is proving successful, overhead trollies and electric traction having taken the place of a horse tramroad, which was a failure from a traffic point of view.

Careful researches are being prosecuted, and experiments made, with the intention of reducing the excessive weight of storage batteries. If this can be effected, they should prove very efficient auxiliaries, especially where, in passing through towns, underground conductors are dangerous, and overhead wires objectionable.

In connection with electric traction, it is very important to reduce, if possible, the initial force required for starting from rest. Whether this will be best attained by the improvement of bearings and their better lubrication, or by the storage, for starting purposes, of a portion at least of the force absorbed by the brakes, remains to be seen, but it is a fruitful field for research and experiment.

In the United States there is a very general and rapid displacement of the cable tramways by the overhead wire electric system. The latter has many opponents, owing, probably, to causes which are preventible.

Many accidents were caused by the adoption of very high tension currents, which, on the breakage of a wire, were uncontrollable, producing lamentable results.

The overhead wires were placed in the middle of the street, causing interference with the passage of fire-escapes.

The speed of the cars was excessive, resulting in many persons being run over.

The cable system, therefore, found many advocates, but the result of experience is in favour of electrical traction under proper safeguards.

The cable system can only compete with the electric system when a three-minute or quicker service is possible, or, say, when the receipts average £20 per mile per day; it is impossible to make up lost time in running, and the cars cannot be "backed." If anything goes wrong with the cable the whole of the traffic is disorganised. The cost of installation is much greater than in the case of electricity, and extensions are difficult.

On the other hand, electricity lends itself to the demands of a growing district, and extensions are easily effected; it satisfies more easily the growing demands on the part of the public for luxury in service and car appointment. It is less expensive in installation, and works with greater economy. By placing the wire at the side of the street, and using a current of low voltage, the objections are greatly minimised, and the cars are much more easily controlled and manipulated. In cases of breakdown these are limited to the half-mile section, and do not completely disorganise the service. Electric cars have been worked successfully on gradients of 1 in 7.

The conduit slot system can be adopted with good results, provided care is taken in the design of the conduit, and allowance made for ample depth and clearance; a width of ⅝-inch is now proved to be sufficient. Where, however, there are frequent turnouts, junctions, and intersecting lines, the difficulties are great, and the cost excessive.

The following figures represent the cost of a tramway, on this system, in America.

Cost of track and conduit	£5600	(per mile of
Insulator, boxer, and double conductor .	480	single track)
Asphalte paving on 6 inches of concrete to 2 feet outside double track) . .	1500	,,
	£7580	

Complete cost of operating 4 miles of double track for 24 hours per day with 2½ minute service, 4·55*d.* per train mile (exclusive of interest, taxes, &c.).

One train consists of one motor car and one trailer.

The trains make a round trip of eight miles in one hour, with three minutes lay-off at each end.

The cost of keeping the slot clean comes to about 40*l.* per quarter, and the repairs to each plough conductor about 50*s.* per quarter.

Attempts have been made to obviate the necessity of the slot by what is known as the closed conduit; but at present the results are not encouraging.

The following figures will help to convey to the mind the great development which is taking place in America, as regards the earnings upon lines electrically equipped. They are derived from the Report of the State Board of Railroad Commissioners for Massachusetts.

	1888.	1894.	Increase per cent.
Net earnings per passenger carried ...	·48	·78	62·5
Net earning per car mile	2·78	4·83	73·56
Net earning per mile of road	£484	£762	57

In addition to the application of electricity for illuminating purposes, and for the driving of tram-cars and railways, it has also been applied successfully to the driving of machinery, cranes, lifts, tools, pumps, &c., in large factories and works. This has proved of the greatest convenience, abolishing as it does the shafting of factories, and applying to each machine the necessary power by its own separate motor; the economy resulting from this can hardly be over-estimated.

It is also successfully employed in the refining of copper, and in the manufacture of phosphorus, aluminium, and other metals, which, before its application, were beyond the reach of commercial application.

The extent of its development of chemical purposes in the future no one can foresee.

It is hardly necessary to call attention to the successful manner in which the Falls of Niagara, and the large Falls of Switzerland, and elsewhere, are being harnessed and controlled for the use of man, and in which horse-power by thousands is being obtained.

At Niagara, single units of electrical plant are installed equal to about 5000 horse-power output. The units are destined to be utilised for any of the purposes previously suggested, and it is computed that one horse-power can be obtained from the river, and sold for the entire year day and night continuously, for the sum of 3*l.* 2*s.* 6*d.* per annum.

Electric head lights are being adopted for locomotives in the United States.

The use of compressed air and compressed gas for tractive purposes is at present in an experimental stage in this country. The latter is claimed to be the cheapest for tramway purposes, the figures given being—

Single horse cars	5¾*d.*
Electrical cars, with overhead wires	4¼*d.*
Gas cars	3¼*d.*

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Combination steam and electric locomotives, gazoline, compressed air, and hot-water motors are all being tried in the United States, but definitive results are not yet published.

The first electric locomotive practically applied to hauling heavy trains was put into service on the Baltimore and Ohio Railway in 1895 to conduct the traffic through the Belt Line Tunnel.

It is stated that, not only was the guaranteed speed of 30 miles per hour attained, but, with the locomotive running light, it reached double that speed.

On the gradient of 8 per cent. a composite train of forty-four cars, loaded with coal and lumber, and three ordinary locomotives—weighing altogether over 1800 tons—was started easily and gradually to a speed of 12 miles an hour without slipping a wheel. The voltage was 625. The current recorded, was at starting, about 2200 ampères, and, when the train was up to speed, it settled down to about 1800 ampères. The drawbar pull was about 63,000 lbs.

The actual working expense of this locomotive is stated to be about the same as for an ordinary goods locomotive—viz. 23 cents per engine mile.

The rapid extension of tunnel construction for railway purposes, both in towns and elsewhere, is one of the remarkable features of the period under review, and has been greatly assisted by the use of shields, with and without compressed air. This brings into considerable importance the question of mechanical ventilation. Amongst English tunnels, ventilation by fan has been applied to those under the Severn and the Mersey. The machinery for the latter is, probably, the most complete and most scientific application up to the present time.

There are five ventilating fans, two of which are 40 feet in diameter, and 12 feet wide on the blades; two of 30 feet, and 10 feet wide; and one quick-running fan of 16 feet in diameter, all of which were ably installed by Messrs. Walker Brothers of Wigan. They are arranged, when in full work, to throw 800,000 cubic feet of air per minute, and to empty the tunnel between Woodside and St. James's Street in eight minutes; but, unfortunately, it is found necessary, for financial reasons, not to work the machinery to it's full capacity.

The intended extension of electrical underground railways will render it necessary for those still employing steam traction either to ventilate by machinery or to substitute electro-motive force.

Great improvements have been lately made in the details of mechanical ventilators, especially by the introduction of anti-vibration shutters, and the driving by belts or ropes instead of direct from the engine. The duties now usually required for mining purposes are about 300,000 cubic feet of air per minute with a water-gauge of about 4 inches; but one installation is in hand for 500,000 cubic feet of air per minute, with a water-gauge of 6 inches. Water-gauge up to 10 inches can now be obtained with fans of 15 feet diameter only.

An interesting installation has been made at the Pracchia Tunnel on the Florence and Bologna Railway.

The length of the tunnel is 1900 metres, or about 2060 yards; it is for a single line, and is on a gradient of 1 in 40. When the wind was blowing in at the lower end, the steam and smoke of an ascending train travelled concurrently with the train, thus producing a state of affairs almost unimaginable except to those engaged in working the traffic.

Owing to the height of the Apennines above the tunnel, ventilating shafts are impracticable; but it occurred to Signor Saccardo that, by blowing air by means of a fan into the mouth of the tunnel, through the annular space which exists between the inside of the tunnel arch and the outside of the traffic gauge, a sufficient current might be produced to greatly ameliorate the state of things.

The results have been most satisfactory, the tunnel, which was formerly almost dangerous, under certain conditions of weather, being now kept cool and fresh, with but a small expenditure of power.

In an age when, fortunately, more attention is paid than formerly to the well-being of the men, the precautions necessary to be observed in driving long tunnels, and especially in the use of compressed air, are receiving the consideration of engineers. In the case of the intended Simplon Tunnel, which will pierce the Alps at a point requiring a length of no less than 12½ miles, a foreign commission of engineers was entrusted by the Federal Government of Switzerland with an investigation of this amongst other questions.

During the construction of the St. Gothard Tunnel, which is about ten miles in length, the difficulties encountered were, of necessity, very great; the question of ventilation was not fully understood, nor was sanitary science sufficiently advanced to induce those engaged in the work to give it much attention. The results were lamentable, upwards of 600 men having lost their lives, chiefly from an insidious internal malady not then understood. But the great financial success of this international tunnel has been so marked as to justify the proposed construction of a still longer tunnel under the Simplon.

The arrangements which are to be adopted for securing the health of the *employés* are admirable, and will surely not only result in reducing the death-rate to a minimum, but also tend to shorten the time necessary for the execution of the undertaking to one-half.

The quantity of air to be forced into the workings will be twenty times greater than in previous works. Special arrangements are devised for reducing the temperature of the air by many degrees, suitable houses are to be provided for the men, with excellent arrangements for enabling them to change their mining clothes, wet with the water of the tunnel, before coming in contact with the Alpine cold; every man will have a bath on leaving; his wet clothes will be taken care of by a custodian, and dried ready for his return to work; suitable meals of wholesome food will be provided, and he will be compelled to rest for half-an-hour on emerging from the tunnel, in pleasant rooms furnished with books and papers. This may appear to some as excessive care; but kind and humane treatment of men results, not only in benefit to them, but also in substantial gain to those employing them, and the endeavour of our own authorities, and of Parliament, to secure for our own workpeople the necessary protection for their lives and limbs in carrying out hazardous trades and employments, is worthy of admiration.

The great improvements in sub-aqueous tunnelling can be clearly recognised from the fact that the Thames Tunnel cost 1150*l.* per lineal yard, whilst the Blackwall Tunnel, consisting of iron lined with concrete, and of twenty-five feet internal diameter, has, by means of Greathead's shield and grouting machine, been driven from shaft to shaft a distance of 754 yards for 375*l.* per yard.

Tunnels have now been successfully constructed through the most difficult strata, such as water-bearing silt, sand, and gravel, and, by the use of grouting under pressure, subsidence can almost entirely be avoided, thus rendering the piercing of the substrata of towns, underneath property without damaging it, a simple operation; and opening up to practical consideration many most important lines of communication hitherto considered out of the question.

On the other hand, very little improvement has taken place in the mode of constructing tunnels in ordinary ground, since the early days of railways. The engineers and contractors of those days adopted systems of timbering and construction which have not been surpassed. The modern engineer is, however, greatly assisted by the possibility of using Brindle bricks of great strength to resist pressure, combined with quick-setting Portland cement, by the great improvements which have taken place in pumping machinery, and by the use of the electric light during construction.

A question which is forcing itself upon the somewhat unwilling attention of our great railway companies, in consequence of the continual great increase of the population of our cities, is the pressing necessity for a substantial increase in the size of the terminal stations in the great centres of population.

Many of our large terminal stations are not of sufficient capacity to be worked properly, either with regard to the welfare of the staff, or to the convenience of the travelling public.

Speak to station-masters and inspectors on duty, when the holiday season is on, and they will tell you of the great physical strain that is produced upon them and their subordinates, in endeavouring to cope with the difficulty.

This, if nothing else, is a justification for the enterprise of the Manchester, Sheffield and Lincolnshire Railway Company in providing an entirely new terminus for London.

It is thirty years since the last, that of St. Pancras, was added, and during that period the population of London has increased by no less than two millions.

The discussion, both in and out of Parliament, of the proposals for light railways has developed a considerable amount of interest in the question. Experience only can prove whether they will fulfil the popular expectations. If the intended branch lines are to be of the standard gauge, with such gradients, and curves as

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will render them suitable for the ordinary rolling-stock, they will, in many cases, not be constructed at such low mileage costs as to be likely to be remunerative at rates that would attract agricultural traffic. The public roads of this country (very different from the wide and level military roads of Northern Italy and other parts of the continent) do not usually present facilities for their utilisation, and, once admitted, the necessity for expropriating private property, the time-honoured questions of frontage severances and interference with amenities will force their way to the front, fencing will be necessary, and, even if level crossings be allowed at public roads, special precautions will have to be taken.

Much must then depend upon the regulations insisted upon by the Board of Trade. If, in consideration of a reduction in speed, relaxation of existing safeguards are permitted, much may, no doubt, be effected by way of feeders to existing main lines.

If, on the other hand, the branches are of narrower gauge, separate equipment will be necessary, and transhipment at junctions will involve both expense and delay. It is very doubtful whether the British farmer would benefit much from short railways of other than standard gauge. He must keep horses for other purposes, and he will probably still prefer to utilise them for carting his produce to the nearest railway station of the main line, or to the market town.

The powers granted by the Light Railways Act, in the hands of the able Commissioners appointed under the Act, cannot, however, fail to be a public boon.

Special Acts of Parliament will be unnecessary, facilities will be granted, procedure simplified, some Government aid rendered, and probably the heavy burden of a Parliamentary deposit will be removed.

It would seem quite probable, that motor cars may offer one practical solution of the problem how best to place the farms of the country in commercial touch with the trunk railways, seaports, and market towns. They could use existing roads, could run to the farmyard or field, and receive or deliver produce at first hand.

Such means of locomotion were frequently proposed towards the end of the last century, and in the early part of the present one, and it was not until the year 1840, that the victory of the railway over steam upon common roads was assured, the tractive force required being then shown to be relatively as 1 to 7.

The passing of the Act of 1896, superseding those of 1861 and 1865, will undoubtedly mark the commencement of a new era in mechanical road traction. The cars, at present constructed chiefly by German and French engineers, are certainly of crude design, and leave much to be desired. They are ugly in appearance, noisy, difficult to steer, and vibrate very much with the revolutions of their engines, rising as they do to 400 per minute; those driven by oil give out offensive odours, and cannot be readily started, so that the engine runs on during short stops. There would seem to be arising here an even more important opening for the skill of our mechanical engineers than in the case of bicycles, in which wonderful industry the early steps appear also to have been foreign.

It is claimed for a motor car that it costs no more than carriage, horse, and harness, that the repairs are about the same, and that, whilst a horse, travelling 20 miles per day, represents for fodder a cost of *2d.* per mile, a motor car of $2\frac{1}{2}$ horse-power will run the same distance at $\frac{1}{2}$*d.* per mile.

The highway authorities should certainly welcome the new comer, for it is estimated that two-thirds of the present wear and tear of roads is caused by horses, and one-third only by wheels.

Perhaps no invention has had so widespreading an influence on the construction of railways as the adoption of the Bessemer process for the manufacture of steel rails. This has substituted a homogeneous crystalline structure, of great strength and uniformity, for the iron rails of former years, built up by bundles of bars, and therefore liable to lamination and defective welds. The price has been reduced from the 13*l.* per ton, which iron rails once reached, to 3*l.* 15*s.* as a minimum for steel. There are, however, not infrequently occurring, in the experience of railway companies, the cracking, and even fracture of steel rails, and the Government has lately appointed a Board of Trade Committee for the investigation, incidentally of this subject, but specially of the important question of the effect of fatigue upon the crystallisation, structure, and strength of the rail. Experience proves, at any rate, that it is of great importance to remove an ample length of crop end, as fractures more frequently take place near the ends, aided by the weakening caused by bolt holes. Frequent examination by tapping, as in the case of tyres, seems, at present, the most effective safeguard.

It is open to serious question, whether the great rigidity of the permanent way of the leading railways of this country is an advantage. Certainly the noise is very great, more so than in other countries, and this points to severe shocks, heavy wear and tear of rails and tyres, and—especially when two heavy locomotives are run with the same train—liability to fracture. Whilst the tendency in this country, and in the United States, has been to gradually increase the weight of rails from 40 lbs. up to 100 lbs. per lineal yard, there are engineers who think that to decrease the rigidity of rail and fishplate, and weight of chair, and to increase the sleepers, so as to arrive as nearly as possible at a continuous bearing, would result in softness and smoothness of running.

The average and maximum speeds now attained by express trains would appear to have reached the limit of safety, at any rate under the existing conditions of junctions, cross-over roads, and other interferences with the continuity of the rail. If higher speeds are to be sought, it would seem to be necessary to have isolated trunk lines, specially arranged in all their details, free from sharp curve and severe gradient, and probably worked electrically, although a speed of 100 miles per hour is claimed to have been reached by a steam locomotive in the United States.

The grain trade of the port of Liverpool has assumed very large proportions, and the system of storage in large silos has been adopted, with great advantage, both as regards capital, outlay, and the cost of working, per ton of grain.

The Liverpool Grain Storage Warehouses at Bootle will be open to members of the Association, and there can be seen the latest development of the mechanical unloading, storing and distribution of grain in bulk; the capacity is large, being—

Warehouse No. 1,	56,000 tons	}	
,, ,, 2,	30,000 ,,	}	or 4,240,000 bushels;
Quay Stores	20,000 ,,	}	

thus constituting this granary as one of the largest, if not the largest, in the world.

The question of the pressure of grain is a very difficult one, and, in constructing the brick silos, which are 12 feet across at the top, by nearly 80 feet in depth, large allowance has been made both for ordinary pressure, and for possible swelling of the grain.

The grain is unloaded by elevators, and then transported on bands, the result being its cooling and cleansing, as well as its storage and distribution.

The question of the early adoption in England of the metric system is of importance not only to the engineering profession, but also to the country at large. The recommendation of the recent Royal Commission, appointed for the consideration of the subject, was, that it should be taught at once in all schools, and that, in two years' time, its adoption should be compulsory; but it is much to be regretted that, up to the present time, nothing has been done.

The slight and temporary inconvenience of having to learn the system is of no moment compared to the great assistance it would prove to the commercial and trading world; the simplification of calculations and of accounts would be hailed with delight by all so soon as they realised the advantages. England is suffering greatly in her trade with the continent for want of it.

Our foreign customers, who have now used it for many years, will not tolerate the inconvenience of the endless variety of weights and measures in use in England, and they consequently purchase their goods, to a great extent, from Germany, rather than use our antiquated English system. It is no exaggeration to say that, with their knowledge of the metric system, they regard ours as completely obsolete and unworkable, just in the same way as we should were we to buy our corn, our wine, our steel and iron, by the hin, the ephah, or the homer, or to compute our measurements by cubit, stadium, or parasang.

It behoves all who desire to see England regain her trade to use all their influence in favour of the adoption of this system, as its absence is, doubtless, one of the contributory causes for the loss that has taken, and is taking, place.

An important argument in favour of the metric system of

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weights and measures is that it is adopted all over the civilised world by physicists and chemists; and it may be stated with confidence, that the present international character of these sciences is largely due to this.

It is interesting also to notice, that the metric system is being gradually introduced into other branches of science. Anthropometric measurements made by the Committees of the British Association in this country and in Canada are invariably given in metres, and a comparison with measurements made in other countries can be at once made.

The period of twenty-five years under review has indeed witnessed great advances, both in scientific knowledge and practical application. This progress has led to powerful yet peaceful competition between the leading nations. Both from among our cousins of the United States, and from our nearer neighbours of Europe, have we, at this meeting, the pleasure of welcoming most respected representatives. But their presence, and the knowledge of the great discoveries made, and colossal works carried out, by them and their brother scientific men and engineers, must make us of Great Britain face with increased earnestness the problem of maintaining our national position, at any rate, in the forefront of all that tends towards the "utilisation of the great sources of power in nature for the use and convenience of man." Those English engineers who have been brought in contact with engineering thought and action in America and abroad have been impressed with the thoroughness of much of the work, the great power of organisation, and the careful reliance upon scientific principles constantly kept in view, and upon chemical and mechanical experiments, carried out often upon a much more elaborate scale than in this country. This is not the place from which to discuss the questions of bounties and tariffs, which have rendered possible powerful competition for the supply of machinery and railway plant from the continent to our own colonies; but there is certainly need for advance all along the line of mechanical science and practice, if we are to hold our own—need especially to study the mechanical requirements of the world, ever widening and advancing, and to be ready to meet them, by inventive faculty first, but also by rigid adherence to sound principles of construction, to the use of materials and workmanship of the highest class, to simplicity of design and detail, and to careful adaptation of our productions to the special circumstances of the various markets.

It is impossible to forecast in what direction the great advances since 1871 will be equalled and exceeded in the coming quarter of a century. Progress there will and must be, probably in increased ratio; and some, at the end of that period, may be able to look back upon our gathering here in Liverpool in 1896 as dealing with subjects then long since left behind in the race towards perfection.

The mechanical engineer may fairly hope for still greater results in the perfection of machinery, the reduction of friction, the economical use of fuel, the substitution of oil for coal as fuel in many cases, and the mechanical treatment of many processes still dependent upon the human hand.

The electrical engineer (hampered as he has been in this country by unwise and retrograde legislation) may surely look forward to a wonderful expansion in the use of that mysterious force, which he has already learned so wonderfully to control, especially in the direction of traction.

The civil engineer has still great channels to bridge or tunnel, vast communities to supply with water and illuminating power, and (most probably with the assistance of the electrician) far higher speeds of locomotion to attain. He has before him vast and ever-increasing problems for the sanitary benefit of the world, and it will be for him to deal from time to time with the amazing internal traffic of great cities. China lies before him, Japan welcomes all advance, and Africa is great with opportunities for the coming engineers.

Let us see to it, then, that our rising engineers are carefully educated and prepared for these responsibilities of the future, and that our scientific brethren may be ever ready to open up for them by their researches fresh vistas of possibilities, fresh discoveries of those wonderful powers and facts of nature which man to all time will never exhaust.

The Mechanical Section of the British Association has done good work in this direction in the past, and we may look forward with confidence to our younger brethren to maintain these traditions in the future.

THE IRON AND STEEL INSTITUTE.

THE Iron and Steel Institute, probably the most cosmopolitan of all our technical societies, has always been noted for taking its members far afield during the annual autumn excursions. The United States—from far north to the extreme south—Austria, Hungary, Germany, France, and Belgium have been among the countries visited, and now Spain may be included in the list. A very novel and somewhat ambitious programme had been arranged by the Executive for the 1896 meeting. It has long been thought desirable that members of the Institute should pay a formal visit to the great source of supply for the steel-workers' raw material situated in Northern Spain. It is from the Bilbao district that the greater part of the iron ore used by British steel-makers is obtained. What the modern steel trade of this country would have been had not the wonderful deposits of non-phosphoric ore of the Peninsula existed it is difficult to realise, but we may be sure that the industry would not have flourished in the way it has. We have, it is true, a limited and partial supply of hæmatite ore in this country; but it would not nearly have sufficed to satisfy the demands of the trade. The acid process of steel-making requires ores free from phosphorus, and though the basic process has been introduced with a view to eliminating phosphorus during the course of manufacture, it cannot be said to have rendered us independent of purer ores.

Nature seems to have designed the hills of Northern Spain especially for the use of the steel-maker. Happily for England, the communication between our country and Spain is of a very direct nature, and across the element which is peculiarly our own, the open sea. Next to having these pure rich ores within our own borders, they could hardly be placed more advantageously than they now are. Spain has not been in the past ambitious to institute a steel-making industry. She has been content to sell the valuable raw material to countries with a more advanced manufacturing organisation. A new spirit, however, has arisen of late, and the somewhat sorely-pressed steel-maker of to-day finds the prospect of another rival springing up at the seat of supply. That, however, is more of the future than the present, for the steel works of Spain now in operation are of comparatively small extent.

The iron mines of Northern Spain are not mines at all in the proper acceptation of the term, for they are open workings, in fact vast diggings or quarries. The mountains themselves are just heaps of iron ore, covered naturally with but a thin layer of earth. This is removed, and it only remains to break up the ore and load it into fitting receptacles, when it is conveyed down to the water's edge by its own gravity. It is difficult to conceive anything more favourable for the purposes of transport. Self-propulsion to the ship's hold, and then the cheapest of all artificial methods of carriage to the home port. Fortunately for us, in the struggle for the word's steel market, our coast-line is more accessible from Spain than that of our great rival, Germany. The Pyrenees offer a barrier to land carriage even if the French railways would frame rates that would allow competition with those wonderfully economical cargo boats, which are one of the greatest triumphs of our engineering industry. There are, however, compensations for our great competitor even in this. A patient and ingenious people, such as the Germans, finding they are blocked in one direction will try other measures. In the manufacture of acid steel Germany laboured under a disadvantage, for the reasons stated, but this led her steel-makers to put forth great efforts to perfect the basic process, by which they could utilise their own supply of native ore, too phosphoric for the manufacture of acid steel. Their labours have been crowned with almost unexampled success, for the development of the basic steel industry in Germany is one of the most creditable achievements in the history of industrial progress. It is true that the best steel is produced from non-phosphoric ores, but the German makers can manufacture excellent steel castings at a low price, and though these may not be equal to the best acid steel, they are commercially successful. After all steel-making is a trade, not simply a competition like prize-winning at an exhibition.

We have been led somewhat astray from our immediate subject by the economic problem suggested by the Bilbao trip of the Iron and Steel Institute, and will now return to our text. The Council, knowing the insufficiency of hotel accommodation for so large a number of persons

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