If we take the simplest case first, that of minimum sunspots depending upon minimum solar activity, we then get chiefly a well-developed equatorial elongation with a marked absence of irregular streamers in mid-latitudes. In support of this, I append two drawings (Figs. 22, 23) made at the eclipses which took place in the years 1867 and 1879, years of sunspot minimum. In the second drawing, made by Prof. Newcomb, the disc is shown by which he eclipsed the brighter lower reaches of the corona, so as to give his eye the best chance of seeing feeble extensions. The pole supporting the disc was vertical, but it is shown slantwise in the illustration because it is most important to show the sun's axis upright.

At the minimum period not only are these extensions best seen, but the exquisite structure near the sun's poles is very strikingly revealed.

At and near the maximum all this is changed, and we get streamers and their separating rifts very irregularly distributed. In 1896 these irregular streamers form striking objects, and we know from the sunspot observations that the atmosphere was more than usually disturbed—more than might have been anticipated, seeing that the maximum was due to occur in 1892.

I am glad to find in the valuable collection of Russian memoirs to which I have referred an important paper by M. Hansky, calling attention to the way in which the form of the corona varies with the sunspot period. His studies entirely confirm all I have written on this subject.

NORMAN LOCKYER.

THE INTERNATIONAL CONGRESS OF MATHEMATICIANS.

ON August 9, 10 and 11, the first International Congress of Mathematicians met in Zürich. By the morning of the 9th there were in attendance 200 members from all parts, viz.: from Switzerland, 53; Germany, 40; France, 25; Italy, 19; Russia, 18; Austria Hungary, 16; United States, 7; Sweden, 6; Denmark, 4; Belgium, England, Holland, 3 each; Greece, Portugal, Spain, 1 each. The gathering, while fairly representative of the different branches of pure mathematics, did not adequately represent applied mathematics. The meeting of the British Association in Toronto was doubtless responsible for the absence of many English mathematicians, who might otherwise have been at Zürich; but, even making allowance for this, the presence of *three* representatives of English mathematics can hardly be regarded as a sufficient recognition of the importance of the congress.

The regulations, while prescribing French and German as the official languages, make provision also for the use of English and Italian; and it is expressly laid down that in the appointment of the committee these languages shall be represented. This body, elected at the first general meeting, was therefore composed as follows:—President, Prof. Geiser; Secretaries, MM. Franel and Rudio; Hon. Secretaries, MM. Borel, Pierpoint, Volterra, E. v. Weber; Members, MM. Brioschi, Hobson, Klein, Mertens, Mittag-Leffler, Picard, Poincaré (absent), H. Weber. The principal office of the committee was to formulate the objects and methods of the series of congresses, and to give a preliminary consideration to certain matters with respect to which action must be taken by the next congress, to be held in Paris in 1900. Among these matters those of most pressing importance are the adoption of some scheme of classification of the various branches of mathematics, and the undertaking of some bibliographical work. As to the organisation of the series of congresses, it is decided that these shall meet in different countries at intervals of from three to five years. The advisability of giving continuity to the series by the establishment of some permanent central body is affirmed, but action is deferred; this question will doubtless be ripe for discussion in 1900. The most tangible object of the congress is the encouragement of the production of detailed reports on different branches of mathematics, for which so admirable a model is afforded by the Brill-Noether "Bericht über Funktionen-theorie," published three years ago. Doubtless, too, the preparation of such reports will be materially assisted, and their international character secured, by the furthering of personal relations among mathematicians of different countries, which is laid down as one object of the congress. The arrangements so admirably planned and carried out by the Zürich committee of organisation gave all possible facilities for social intercourse, beginning with an informal gathering on the evening preceding the actual meetings, and including afternoon excursions on the lake and to the top of the Uetliberg.

Tuesday was given up to the reading of papers, for which five Sections were organised, each with President, Vice-President, and Secretary. (1) "Arithmetic and Algebra," Mertens, Peano, Amberg; (2) "Analysis and Theory of Functions," Picard, Brioschi, Jaccottet; (3) "Geometry," Reye, Segre, Künzler; (4) "Mechanics and Mathematical Physics," Jung, Joukowsky, Flatt; (5) "History and Bibliography," Moritz Cantor, Laisant, Schoute. The sittings of these Sections were arranged to begin at different hours of the morning and afternoon, to meet the natural desire of members to hear as many as possible of the leaders in different subjects. Papers of special interest were those of Brioschi, "Sur une classe d'équations du cinquième degré"; Picard, "Sur les fonctions de plusieurs variables"; Reye, "Neue Eigenschaften des Strahlenkomplexes zweiten Grades"; H. Weber, "Ueber die Genera in algebraischen Zahlkörpern"; Zeuthen, "Isaac Barrow et la méthode inverse des tangentes." Moreover, addresses were delivered at the opening and closing general meetings on Monday and Wednesday mornings by Poincaré, "Sur les rapports de l'analyse pure et de la physique mathématique"; Hurwitz, "Entwicklung der allgemeinen Theorie der analytischen Funktionen in neuerer Zeit"; Peano, "Logica Matematica"; Klein, "Zur Frage des höheren mathematischen Unterrichtes." In the much-regretted absence of M. Poincaré, his address was read by M. Franel.

Among the mathematicians present, in addition to those already named, were MM. Brill, Noether, G. Cantor, Dyck, Gordan, Korteweg, Larmor, F. Meyer, Osgood, Vassilief, Veronese, Enriques, Eneström.

THE BRITISH ASSOCIATION.

THE Toronto meeting of the British Association opened on Wednesday in last week, and came to an end yesterday, as we went to press. The reports which have reached us show that the meeting has been a successful one throughout, both from a social and also from a scientific point of view. As in 1884, when the Association met in Montreal, Canadians have shown by the enthusiastic reception given to the members that they value agencies which exist for the diffusion of knowledge and culture. The many papers read before the Sections by no means represent the whole result of such a gathering. The Dominion has been bound closer to the mother country, the interests of science have been brought before the notice of the public, and scientific knowledge will be advanced by the opportunity which the meeting has given for the exchange of ideas. The Montreal meeting of the Association was not only of value in assisting scientific education and research in Canada, but our Transatlantic contemporary—*Science*—acknowledges that it gave a considerable impulse to science throughout America, the result being that in the thirteen years which have elapsed since the meeting took place, America has come to the front as a nation which fosters scientific work, and a country which contributes a very large share to the world's wealth of natural knowledge.

Twelve hundred members and associates attended the meeting at Toronto, about four hundred being from Great Britain. The proceedings were opened on Wednesday, August 18, by a meeting of the General Committee, when the annual report of the Council for presentation to the Association was read. From the *Times* we learn that the report states that the Council has nominated Sir Donald Smith, Mr. A. S. Hardy, Premier of Ontario, and the Mayor of Toronto as Vice-Presidents of the Association, and regret is expressed at the decision of Mr. Vernon Harcourt to retire from the General Secretaryship of the Association. A warm tribute is paid to him for the invaluable services he has rendered in that capacity during the past fourteen years, and Prof. Roberts-Austen is recommended for appointment as his successor. Mention is made of the fact that the Imperial Government, at the request of the Association, has appointed a committee to report upon the desirability of establishing a National Physical Laboratory. The report states further that the Trustees of the British Museum have under consideration the Association's suggestion for the establishment of a Bureau of Ethnology of Greater Britain in connection with the Museum. The Corporation of Glasgow has forwarded an invitation for the annual meeting of the Association to be held in that city in 1901.

The annual report of the Treasurer shows the finances of the Association to be in a flourishing condition, the balance in hand being 1396*l*. The amount to be granted for various scientific investigations is about 1200*l*.

The sectional work began on Thursday morning, and the presidents of the various sections, with the exception of Anthropology and Botany, delivered their addresses. Two of the sectional addresses were printed in full in last week's NATURE, and also the President's address. We print this week the complete addresses delivered before the sections of Geology, Zoology, and Mechanical Science, and shall follow these with others. We have arranged for our usual reports of the work of the sections, and shall insert them as soon as possible after they are received.

On Friday last there was a special Convocation of Toronto University, under the presidency of the Vice-Chancellor, the Hon. William Mulock, to confer the honorary degree of Doctor of Laws upon Lord Kelvin, Lord Lister, Sir John Evans, and Mr. Hardy (Premier of Ontario). The University has also conferred the same honorary degree upon Lord Rayleigh, Prof. Wolcott Gibbs, and Sir Wilfrid Laurier. At a Convocation of Trinity University on Tuesday, the honorary degree of D.C.L. was conferred upon Lord Kelvin, Lord Lister, Sir John Evans, Mr. Bryce, and Sir George Robertson.

The feature of Friday last was a brilliant discourse delivered by Prof. Roberts-Austen, C.B., F.R.S., on "Canada's Metals." At the close of the lecture, which was illustrated by numerous striking experiments, a vote of thanks was proposed by Dr. George Dawson, and seconded by Sir Charles Fremantle.

A lecture to operatives was given by Dr. Henry O. Forbes on Saturday, upon the subject of "British New Guinea: the Country and its People, with some Account of the Problems which the Region offers to the Naturalist and the G ographer." The Mayor of Toronto, Mr. Shaw, presided, and a large audience attended. On Monday, Prof. John Milne, F.R.S., discoursed upon "Earthquakes and Volcanoes," the President, Sir John Evans, being in the chair.

SECTION C.

GEOLOGY.

OPENING ADDRESS BY G. M. DAWSON, C.M.G., LL.D., F.R.S., F.G.S., PRESIDENT OF THE SECTION.

THE nature and relations of the more ancient rocks of North America are problems particularly Canadian, for these rocks in their typical and most easily read development either constitute or border upon the continental Protaxis of the North. The questions involved are, however, at the same time, perhaps more intimately connected with a certain class of world-wide geological phenomena than any of those relating to later formations, in which a greater degree of differentiation occurred as time advanced. A reasonably satisfactory classification of the crystalline rocks beneath those designated as Palæozoic was first worked out in the Canadian region by Logan and his colleagues, a classification of which the validity was soon after generally recognised. The greatest known connected area of such rocks is embraced within the borders of Canada, and, if I mistake not, the further understanding of the origin and character of these rocks is likely to depend very largely upon work now in progress, or remaining to be accomplished here.

This being the case, it seems very appropriate to direct such remarks as I may be privileged to make on the present occasion chiefly to these more ancient rocks, and the subject is one which cannot fail to present itself in concrete form to the visiting members of this Section. Personally I cannot claim to have engaged in extended or close investigations of these rocks, and there is little absolutely new in what I can say in respect to them; but work of the kind is still actively in progress by members of the staff of the Geological Survey, and the classification and discrimination of these older terranes present themselves to us daily as important subjects of consideration in connection with the mapping of vast areas; so that, if still admittedly imperfect in many respects, our knowledge of them must be appraised, and, at least provisionally, employed in a practical way in order to admit of the progress of the surveys in hand.

Although it is intended to speak chiefly of the distinctively pre-Cambrian rocks of Canada, and more particularly of the crystalline schists, it will be necessary also to allude to others, in regard to the systematic position of which differences of opinion exist. Of the Cambrian itself, as distinguished by organic remains, little need be said, but it is essential to keep in touch with the palæontologically established landmarks on this side, if for no other reason than to enable us to realise in some measure the vast lapse of time, constituting probably one of the most important breaks in geological history, by which the Cambrian and its allied rocks are separated from those of the Huronian and Laurentian systems.

In attempting to review so wide a subject and one upon which so much has already been written, the chief difficulty is to determine how much may be legitimately eliminated while still retaining the important features. This must be largely a matter of individual judgment, and I can only hope to present what appear to me to be the essential points, with special reference to the geology of Canada. The useful object of any such review is, of course, to bring out what may now actually be regarded as established respecting these older rocks, and in what direction the most hopeful outlook exists for improving our knowledge of them. For this purpose, the best mode of approaching the subject, in the first place, and up to a certain point, is the historical one, and it will thus be desirable to recapitulate briefly the first steps made in the classification of the crystalline schists in Canada. This is the more appropriate, because of the substantial accuracy of these first observations, and the fact that they have since been largely buried out of sight by a copious controversial literature of later growth.

Soon after the Geological Survey of Canada was begun, now more than fifty years ago, Logan (who in the earlier years of the work may almost be said to have alone constituted the staff) found himself confronted with the great areas of crystalline rocks forming the continental Protaxis. The existing geological edifice has been so largely the result of the past half-century of work, that it is not now easy to realise the elementary condition in which its foundations lay at that time. It was then but ten years since Sedgwick and Murchison had given form to their discoveries in regard to the Cambrian and Silurian, and a still shorter time since the definite publication of the classification of the Cambrian and the appearance of the "Silurian System," while Hall, Emmons and others, working upon these lines, were actively engaged in building up a similar classification of the Palæozoic rocks of the Eastern States of the American Union. The Silurian and Cambrian had, in fact, but just been reclaimed from what Murchison speaks of as the "vast unclassified heaps of greywacke" or "transition limestones."

It would have been quite appropriate at this date to relegate all underlying and more or less completely crystalline rocks to the "Primary," or "Primitive," or "Azoic," but such a solution fortunately did not recommend itself to Logan.

It was along the Ottawa Valley, in 1845, that the rocks subsequently classed under the Laurentian and Huronian systems were first examined in some detail. In that year Logan met with and accurately described, severally, rocks which we now refer to (1) The Fundamental Gneiss; (2) The Grenville Series; and (3) The Huronian. He speaks of the rocks of the first class as being in the main syenitic gneisses "of a highly crystalline quality, belonging to the order which, in the nomenclature of Lyell, is called metamorphic instead of primary, as possessing an aspect inducing a theoretic belief that they may be ancient sedimentary formations in an altered condition." In what we now call the Grenville Series, he describes the association of crystalline limestones and interbedded gneisses, adding that it appeared to be expedient to consider this mass as a separate metamorphic group, supposed to be newer than the last. Of the Huronian, the relations were at that time left undetermined, although it is observed that its beds hold pebbles of the underlying rocks, here the Fundamental Gneiss.

The following season was spent by Logan, and by his assistant Murray, on the north shore of Lake Superior, Thunder Bay and its vicinity being one of the regions especially examined. Without enumerating particular localities, it may be stated that Logan there grouped the rocks met with as follows, beginning with the lowest; the column added on the left giving the present nomenclature of the several series defined:—

Laurentian ...	... { 1. Granite and syenite. 2. Gneiss.
Huronian ...	... 3. Chloritic and partly talcose and conglomerate slates [schists].
Animikie ...	... 4. Bluish slates or shales interstratified with trap.
Keweenawan	... 5. Sandstones, limestones, indurated marls and conglomerates, interstratified with trap.

It is not distinctly stated that No. 3 rests unconformably on the older rocks, but the observation that granitic boulders were found in it, leads to the belief that such unconformity was assumed. Murray, however, supposed the junction as seen on the Kaministiquia to be conformable, and unites the first three subdivisions, as above given, in one series.

Logan further states, still referring to the same region, that the "chloritic slates [schists] at the summit of the older rocks on which the volcanic formations rest unconformably, bear a strong resemblance to those met with on the upper part of Lake Temiscaming on the Ottawa, and it appears probable that they will be found to be identical."

It will thus be observed that the progress in classification made, up to this date at least, entirely accords with the results of the latest investigations. The identity of the rocks placed third in the table with those of the Upper Ottawa was more than conjectured, and the existence of a great stratigraphical break at the base of what is now known as the Animikie was clearly recognised. The several formations were merely described. No specific names were given to them at this time by Logan, and it is further stated that the age of the highest formations (Animikie and Keweenawan) was in doubt, although some reason was found to support Houghton's (then State Geologist of Michigan) view (or what was believed to be his view), that these formations are lower than the Potsdam, or "lowest fossiliferous formation."

In 1847 and 1848, investigations were continued along the north shore of Lake Huron, of which the characteristic rocks are, it is stated, believed to form a single system. They are described as in part sedimentary (quartzites, conglomerates, &c.), and in part igneous (greenstones), the latter being both interposed between the sedimentary beds and intrusive. The "slates" are particularly characterised by Murray as often chloritic, epidotic, and micaceous, and would now, of course, be more specifically termed schists.

Writing in 1849 ("Report on the North Shore of Lake Huron"), however, and later, in a communication presented to this Association in 1851, Logan, although still recognising the manifest unconformity at the base of the Animikie, speaks collectively of the "Copper-bearing Rocks" of Lake Superior and Huron, including under this general term what are now known as the Huronian, Animikie, and Keweenawan series, and adds that it is "highly probable" that all these are approximately equivalent to each other, and to the Cambrian of the British Islands.

In the Report for 1852-53 (published 1854), the name Laurentian was adopted for what had been previously designated merely as the "metamorphic series," and in the geological sketch printed in Paris in connection with the Exhibition of 1855 (which follows next in order of publication), this system is stated to consist almost exclusively of much altered and disturbed sedimentary beds. It is also, however, made to include some recognised intrusives, such as granite and syenites, forming parts of the mass, as well as the Labradorite rocks, which were afterwards for a time named Upper Laurentian, and to which further allusion will be made in the sequel. The name Laurentian is here, therefore, first employed exactly in the sense of the term "Basement Complex," introduced long afterwards, but under the distinct idea that most of the rocks are altered sediments, from which certain intrusive masses were not clearly separable.

In the same publication, the overlying series of Lakes Huron and Superior, including the Huronian proper, the Animikie and the Keweenawan, were collectively spoken of as the "Huronian or Cambrian system." These rocks are described as lying discordantly on the Laurentian, and as intervening between it and the lowest known fossiliferous strata. There being no other recognised place for such rocks in the scheme of the day, they are consequently supposed to represent the Lower Cambrian of Sedgwick.

It is unnecessary to follow in order the investigations carried on for a number of subsequent years, but reference may now be made to the "Geology of Canada," of 1863, in which all previous results of the Survey to that date were collected and systematised. In this volume, after stating that Hall's nomenclature of the Palæozoic rocks in the State of New York had been adopted unchanged for the adjacent Canadian territory, "in the interests of unity of plan for future researches," Logan writes:—"To the Azoic rocks no local names have yet been applied in any part of America except in Canada," and adds:—"The names of the Laurentian and Huronian systems or series, which we have been accustomed to apply to them, are allowed to remain unchanged, particularly as they have been recognised abroad, and have been made by other geologists a standard of comparison both in America and Europe."

In Chapter V. of this volume the "Upper Copper-bearing Rocks of Lake Superior" are separately treated, and are recognised as comprising two groups which are stated to overlie the Huronian unconformably. These groups are those now known as the Animikie and Keweenawan.

There can be no doubt about the classification intended at this time, and the rocks are correctly laid down on the atlas prepared to accompany the volume, but in consequence of an unfortunate error in the geographical description of the distribution of the Huronian about Thunder Bay, that arose in 1846 and was repeated in 1863, several later investigators have been led to regard the rocks of the "Upper Copper-bearing Series" as those of Logan's typical Huronian, and to suppose that when examining these rocks they were dealing with those intended to be classed as Huronian. Irving, Winchell and others have adopted this mistaken view, which it is particularly necessary to refer to here, as it has been the chief cause of all subsequent misapprehension in regard to the "Original Huronian."[1]

[1] As already stated, the relations of the principal rock-series of the vicinity of Thunder Bay had been correctly outlined in 1846, although the series had not at that time been named. The Kaministiquia River section had been examined by Murray, who also correctly described the distribution of the series there, stating that the "granite, syenite, gneiss, micaceous and chloritic schist" (Laurentian and Huronian) find their southern limit on a line running from the falls on that river to the "head of Thunder Bay," while the "Upper Slates (Animikie) rest upon them and occupy the country between such a line and Lake Superior" ("Report of Progress," 1846-47, p. 51). In combining his own results with those of Murray, Logan describes the southern line of the granite, gneiss, and chloritic slates as "commencing in the vicinity of Fort William," or at the *mouth* of the Kaministiquia, although the falls, at which this line had been determined by Murray, are some twenty miles up the river. Proceeding (*op. cit.* p. 25) to describe the extent of the "superior trappean formations" (Animikie and Keweenawan), he then reverts to the line previously stated, making these rocks to terminate locally where

The temporary grouping of the Huronian proper with the "Upper Copper-bearing Series" (Animikie and Keweenawan), on the grounds already explained, as "Huronian or Cambrian," together with the employment (proper enough at the date) of the term "slates" for rocks that would now be named schists, further assisted in giving colour to the erroneous view just referred to.

In a second geological sketch of Canada, printed in Paris at the time of the International Exhibition of 1867, the same classification is maintained, but to it is added the Upper Laurentian or Labradorian. This sketch was actually written by Hunt, but it was an official publication correctly representing the views held at that time, and may be accepted as Logan's last word on the subject. As thus defined and established, he left the Laurentian and Huronian systems.

In so far as the stratigraphical relations of the Laurentian, Huronian, and "Upper Copper-bearing Series" are concerned (leaving out of consideration the Labradorian), it is thus manifest that the conclusions originally formed from actual study on the ground were those finally held by Logan. The reference for a time of the Huronian proper and the "Upper Copper-bearing Series" together to the Lower Cambrian, meant only that, as then understood, there was no other systematic position recognised to which they could be assigned. That a great unconformity existed between these two systems was never doubted, but for some years Logan was not prepared to take the bold position of constituting a separate Huronian system beneath the lowest Cambrian; he was, on the contrary, anxious, if possible, to bring the Canadian section within the lines established in the classic region studied by Sedgwick and Murchison. The introduction of new systematic terms was at that time considered somewhat seriously. When eventually compelled to take this step (in 1857), he confined the name Huronian to rocks antedating the great break at the base of the "Upper Copper-bearing Series" (Animikie), embracing those first seen by him on the Upper Ottawa and on Lake Huron, with their representatives elsewhere, under this new system.

In so far as nomenclature goes, Logan thus certainly modified his original application of the name Huronian; it was not, however, as has been contended, to create an "extended Huronian," but on the contrary to restrict the name to rocks beneath the great unconformity at the base of the Animikie. The change was necessitated by the progress of investigation and by the recognition of an upper division of the "Azoic," beneath anything that could legitimately be classed as Cambrian. It was made by the author himself, and involved no departure from the law of priority or from any other acknowledged rule. In finally eliminating these upper rocks from his Huronian system, he was no doubt influenced by Whitney's criticisms of 1857 (*Am. Journ. Sci.*, vol. xxiii., May 1857), which were in part correct, although largely devoted to the very conservative contention that all stratified rocks below the great break were inseparable, and should be included in an "Azoic System." This influence may be traced in an important paper, of but three pages, communicated to the American Association for the Advancement of Science a few months later than the date of that above referred to, in which, while the name Huronian is reaffirmed for the rocks of Lake Huron and Lake Temiscaming, which are taken as typical of the system, nothing further is said of those now known as Animikie and Keweenawan.

In the summary volume of 1863, to which allusion has already been made, the existence of an Upper Laurentian, Labradorian or Norian Series was first tentatively indicated in a supplementary chapter. It is unnecessary to follow here the history of the rocks so classed, for the supposed series has not stood the test of later discussion and research, due chiefly to Selwyn and Adams. The apparently stratified rocks often included in it are now understood to be foliated eruptives. The recognition achieved by this and by other more or less hypothetical series about this time may be traced to the brilliant chemico-geological theories advanced by Hunt, previous to the general acceptance of modern petrographical methods.

In a similar manner, and very justly so, Logan, as a field geologist, was influenced by the views held by Lyell in the early editions of his "Principles," to accept without reservation the foliation of crystalline rocks as indicative of original bedding. This was, at the time of his early researches and thereafter for many years, the accepted view, although Dana, in a paper read before the American Association for the Advancement of Science in 1843, had already held that the schistose structure of gneiss and mica-slate was insufficient evidence of sedimentary origin; and Darwin, a few years later, had published his "Geological Observations," including a remarkable chapter on cleavage and foliation, in which he advocated a similar view. No such doctrine, however, achieved general recognition until long afterwards, while that class of facts remaining to be determined chiefly by the microscope, which may be included under the term "dynamic metamorphism," were wholly unknown and unforeseen.

In admitting that chemical, metamorphic, and uniformitarian hypotheses were thus given, in turn, undue weight, it is not to be assumed that the advances made under these hypotheses have been entirely lost; it has been necessary only to retreat in part in each instance, in order to fall again into the more direct road.

In late years, modern microscopical and chemical methods of research have been applied to the ancient crystalline schists of Canada—the older work has been brought under review, and new districts have been entered upon with improved weapons. Here, as in other parts of the world, investigations of the kind are still in active progress; finality has not been reached on many points, but the explanation of others has been found. One advance which deserves special mention is the recognition of the fact that a great part of the Huronian is essentially composed of contemporaneous volcanic material, effusive or fragmental. This was first clearly stated by Canadian geologists, but has only become generally admitted by degrees, in opposition to prevalent theories of metamorphism and cosmic chemistry.

The first opportunity of studying these Archæan rocks in detail, under the new conditions, fell to Dr. A. C. Lawson, then on the Staff of the Canadian Survey, in the vicinity of the Lake of the Woods and elsewhere to the west of Lake Superior. In that part of the Protaxis, the Laurentian appears to be represented only by the Fundamental Gneiss, and the Huronian, by a series to which a local name (Keewatin) was appropriately given,[1] but which is now known to differ in no essential respect from many other developments of the same system. The Huronian stands generally in compressed folds, and along the line of junction the gneisses are related to it in the manner of an eruptive, penetrating its mass and containing detached fragments from it. The same or very similar relations have since been found to occur in many other places.

Arguing from observations of the kind last mentioned, it was too hastily assumed by some geologists that the Laurentian as a whole is essentially igneous, and later in date than the Huronian. The conditions are, however, not such as to admit of an unqualified belief of this kind, even in regard to the Fundamental Gneiss. We may go so far as to assume that these rocks (occupying as they do much the larger part of the entire Protaxis) constitute a great "batholitic" mass of material at one time wholly fluent; but even on this hypothesis some primitive floor must have existed upon which the Huronian and the similarly circumstanced Grenville Series were laid down, and no such enormous substitution can have obtained as to result in the replacement of the whole of this floor by exotic material.[2] It seems much more probable that but limited tracts of the Fundamental Gneiss have passed into a fluent condition when at great depths in the earth's crust, and various arguments may be adduced in favour of a belief that the observed lines of contact might be those along which such fusion would be most likely to occur.[3] Moreover, the Huronian in many and widely separated localities is found to contain water-rounded fragments of syenitic,

he had said the older rocks began. In recasting the earlier observations for the volume of 1863 (no further work having meanwhile been done at this place), Logan is thus naturally led to state that the Huronian (*i.e.* the "Chloritic Slates") *occupies the coast east of the Kaministiquia*, whereas this coast, for ten or eleven miles, is actually occupied by Animikie rocks. Subsequent investigators, inspecting this coast-line with the volume of 1863 as a guide, very naturally thus assumed that they were examining Logan's "typical Huronian," or a part of it. It is in consequence only of a too consistent adhesion to this misunderstanding, that it has been found necessary to speak of an "Upper Huronian," and refer to an "inter-Huronian" unconformity. The so-called Upper Huronian is no part of the system as understood by the Canadian Survey. One cannot fail to note, in reading much that has been written on this subject, that the importance of the great unconformity at the base of the Animikie was realised only after a new classification had been adopted, in which it had practically been ignored.

[1] In the Archæan, local names are particularly useful, inasmuch as correlation must proceed on lithological and stratigraphical data, more or less uncertain when extended to wide areas, even in the case of the older and more homogeneous strata of the earth's crust.

[2] For analogous phenomena of much later date geologically, see Annual Report Geological Survey of Canada, 1886, p. 11 B.

[3] Hypotheses on this subject are well summarised by Van Hise. Annual Report U.S. Geol. Survey, 1894–95, p. 749

granitic and gneissic rocks, forming conglomerates, which may often be observed to pass into schists, but still plainly indicate that, in these places at least, materials not unlike those of the Fundamental Gneiss and its associates were at the surface and subject to denudation. Such materials cannot be regarded as parts of any primeval superficial crust of the earth in an original condition. They represent crystalline rocks formed at great depths, and under conditious similar, at least, to those under which the Fundamental Gneiss was produced. They imply a great pre-Huronian denudation, and show that the Huronian must have been deposited unconformably either upon the Fundamental Gneiss itself, or upon rocks occupying its position and very similar to it in character. There can be no reasonable doubt that the mass of what now constitutes the Fundamental Gneiss originally existed as the floor upon which the Huronian was deposited.

The name Archæan has been adopted and employed by the Geological Survey of Canada in the sense in which it was introduced (in 1874), and consistently maintained by Dana—*i.e.* to include all rocks below the great hiatus of which evidence was first found in the Lake Superior region. The author of the name never assented to its restricted application as proposed by Irving and followed by Van Hise and others, and as a synonym for the Fundamental Gneiss or "Basement Complex" it is not only unnecessary but is scarcely etymologically correct, if we admit that a part of the "Complex" is of comparatively late date.

We have reached a point at which we may ask what is now our conception of these Archæan rocks in Canada, and more particularly in the great Protaxis, as resulting from the most recent investigations of a critical kind. The reply may be given briefly from the latest reports of those still at work on the problems involved as follows:—

The *Laurentian* comprises (1) the Fundamental Gneiss or Lower Laurentian (also referred to as the Ottawa Gneiss or Trembling Mountain Gneiss in older Reports), and (2) the Grenville Series. An important part of the gneisses of the Grenville Series has been shown by chemical analysis to be identical in composition with ordinary Palæozoic argillites, and they are interbedded with quartzites and massive limestones, also evidently of aqueous origin, and in some places abounding in graphite. These beds are, however, closely associated with other gneisses in which orthoclase largely preponderates that have the composition of igneous rocks. The Fundamental Gneiss consists chiefly, if not exclusively, of rocks of the last-named class, the banding of foliation of which, though now generally parallel to that of the Grenville Series, has probably been produced mainly or entirely by movements induced by pressure, in a mass originally differing more or less in composition in its different parts. These two series are sometimes separable on the ground locally, but with difficulty; in other places they cannot be clearly defined (*cf.* Adams, Annual Report Geological Survey of Canada, 1895).

The Upper Laurentian, Labradorian, Norian or Anorthosite group, maintained for a number of years on the evidence already mentioned, is found to consist essentially of intrusive rocks, often foliated by pressure, later in age than the Grenville Series, but in all probability pre-Palæozoic.

The *Huronian* comprises felspathic sandstone or greywacke more or less tufaceous in origin, quartzites and arkoses passing into quartzose conglomerates and breccia conglomerates, often with large fragments of many different varieties of granite, syenite, &c., diorite, diabase, limestones, and shales or slates changing to phyllites in contact with the numerous associated igneous masses. Over wide areas altered greenstones and their associated tuffs preponderate, often with micaceous, chloritic, sericitic and other schists, many of which are of pyroclastic origin, although some may represent ordinary aqueous deposits, and all have been much affected by subsequent dynamic metamorphism.

The Huronian rocks have not yet been found in distinct relation to those of the Grenville Series, but are generally in contact with the Fundamental Gneiss, in the manner previously alluded to. Where not composed of volcanic material it appears to be largely of a littoral character, while the Grenville Series seems rather to indicate oceanic conditions.

No reference has so far been made to the development of Archæan rocks, known as the "Hastings series." The rocks thus named occupy considerable tracts to the south of the Ottawa River, west of the City of Ottawa. They were originally classed by Logan and Murray with the Grenville Series of the Laurentian, although Murray soon after insisted on their peculiar features, and they came to be recognised by the above geographical name during subsequent discussions as to their systematic position, by the authors above referred to, and by Hunt, Vennor, and Macfarlane. These rocks are particularly alluded to now, because later work seems to show that both the Grenville Series and the Huronian are represented in the district—in so far, at least, as lithological characters may be depended on. They include a preponderance of thinly-bedded limestones and dolomites, finer in grain and usually less altered than those of the typical Grenville Series, associated with conglomerates, breccias and slates still retaining complete evidence of their clastic origin.

It is in this Hastings region that careful investigation and mapping are now in progress by several members of the Canadian Survey, with the prospect of arriving at definite results respecting the relations of the Grenville Series and the Huronian. It is too early to forecast what these results may be, for the question is one which must be approached with an open mind; but the work already completed by Messrs. Adams, Barlow, and Ells, appears to sustain the suggestion that both series occur, and to indicate that they may there be so intimately connected as to render their separation difficult. It must be borne in mind that, although the relations of the Grenville Series and those of the recognised Huronian to the Fundamental Gneiss are very similar, they characterise distinct tracts, to which the Hastings district is to some extent geographically intermediate, although most closely connected in this respect with the Grenville region.

Reverting to the original classification of the Archæan of the Canadian Survey, as developed in the field by Logan and his assistants, we may now inquire—In how far does this agree with the results of later work above outlined? In the main, this classification still stands substantially unaltered, as the result of all honest work carefully and skilfully executed must. The nomenclature adopted is still applicable, although some of our conceptions in regard to the rocks included under it have necessarily undergone more or less change.

The Laurentian is still appropriately made to include both the Fundamental Gneiss and the Grenville Series; although at first both were supposed to represent "metamorphic" rocks, it was even then admitted (1855) that these embraced some plutonic masses practically inseparable from them. Later investigations have increased the importance of such plutonic constituents, while at the same time demonstrating the originally supposed sedimentary origin of the characteristic elements of the Grenville Series; but the admission of so large a plutonic factor necessarily invalidates in great measure the estimates of thickness based upon the older reasoning, under which any parallelism of structure was accepted as evidence of original bedding.

Whatever views may be held as to the propriety of including rocks of the two classes under a single name, the necessity of so doing remains, because of the practical impossibility of separating them over any considerable area for the purpose of delineation on the map. No advance in knowledge is marked in substituting for Laurentian, with its original concept of a stratified time-series, such a name as "Basement Complex." It may, indeed, yet prove that the homogeneity of the Laurentian is greater than is at present supposed, for a mass of strata that included ordinary sediments, arkoses, and contemporaneous volcanic deposits of certain kinds, in which the arkose and volcanic constituents preponderated in the lower beds, might, under metamorphism at great depths, produce just such a combination as that of the Grenville Series and the Fundamental Gneiss, the latter representing an aggregate result of the alteration of that part composed chiefly of volcanic material or of arkose—in fact, under the conditions assumed, the lower mass could not now well exist under any other form than that actually found in the Fundamental Gneiss. In his address at the Nottingham Meeting of this Association, Teall has clearly pointed out that, in such cases, the chemical test must necessarily fail, and that the character and association of the rocks themselves must be given a greater weight.

The Huronian proper, under whatever local names it may be classed, still remains a readily separable series of rocks, with peculiar characters, and economically important because of the occurrence in it of valuable minerals.

The subsequently outlined Labradorian has been eliminated as a member of the time-series, and the rocks of the so-called "Hastings Group" remain yet in a doubtful position, but with the promise that they may afford a clue to the true relations of the Grenville Series of the eastern and the Huronian of the western province of the Protaxis.

To what extent the above subdivisions of the Archæan may be legitimately employed in other parts of the continent, more or less remote from the Protaxis, remains largely a question for future investigation. In the southern part of New Brunswick, however, the resemblance of the Archæan to that of the typical region is so close that there can be little risk of error in applying the same classificatory names to it. The Fundamental Gneiss is there in contact with a series comprising crystalline limestones, quartzites, and gneissic rocks, precisely resembling those of the Grenville Series. Later than this is a great mass of more or less highly altered rocks, chiefly of volcanic origin, comprising felsites, diorites, agglomerates, and schists of various kinds, like those of the typical Huronian. The existence of this upper group correlatively with that representing the Grenville Series, constitutes an argument, so far as it goes, for the separateness of these two formations in the general time-scale. All these Archæan rocks of New Brunswick are distinctly unconformable beneath fossiliferous beds regarded by Matthew as older than Cambrian.

In the Cordilleran region of Canada, again, a terrane is found lying unconformably beneath the lowest rocks possibly referable to the Cambrian, evidently Archæan, and with a very close general resemblance to the Grenville Series. To this the local name Shuswap Series has been applied, and a thickness of at least 5000 feet has been determined for it in one locality. It consists of coarsely crystalline marbles, sometimes spangled with graphite and mica, quartzites, gneisses, often highly calcareous or quartzose, mica schists, and hornblendic gneisses. With these is a much greater mass of gneissic and granitoid rocks, like those of the Fundamental Gneiss of the Protaxis, and the resemblance extends to the manner of association of the two terranes, of which, however, the petrographical details remain to be worked out (*cf.* Annual Report Geological Survey of Canada, 1888–89, p. 29 B.).

While it is true that a resemblance in lithological character, like that existing between the Grenville and Shuswap Series, far remote from each other geographically, may mean only that rocks of like composition have been subjected to a similar metamorphism, both the series referred to are separated above by an unconformity from the lowest beds of the Palæozoic, and there is thus sufficient evidence to indicate at least a probability of their proximate identity in the time-scale. In Scotland, an analogous series, and one apparently similarly circumstanced, seems to occur in the rocks of Gairloch and Loch Carron (*cf.* Geikie, "Ancient Volcanoes of Great Britain," vol. i. p. 115).

Particular attention has been directed throughout to the southern part of the continental Protaxis in Canada. In this region it happened that the Archæan rocks and those resting upon them were originally studied under exceptionally favourable conditions, for ever since the great revolution which succeeded Huronian time, the region is one which has remained almost stable. Selwyn and N. H. Winchell have particularly insisted on the importance of the stratigraphical break which here defines the Archæan above. It is not everywhere so well marked, for in the Appalachian province and in the country to the south of the great lakes, in Wisconsin and Michigan, repeated subsequent earth-movements have flexed and broken the older strata against the base of the table-land of the Protaxis. It is not from these districts, subjected to more recent and frequent disturbance, that the ruling facts of an earlier time may be most easily ascertained. Much careful and conscientious work has been devoted to them, but it is largely, I believe, because of the attempt to apply, for purposes of general classification, the still unsettled and ever-changing hypotheses derived from such more complicated tracts that so much confusion has been introduced in regard to the Archæan and early Palæozoic rocks.

If the unconformity closing Archæan time in the vicinity of the Great Lakes had been observed only in that region, it might be regarded as a relatively local phenomenon; but subsequent observations, and more particularly those of the last few years, due to Bell, McConnell, Tyrrell, and Low, show that rocks evidently representing the Animikie and Keweenawan, and practically identical with those of Lake Superior in general lithological character, recur in many places almost throughout the whole vast area of the Protaxis, on both sides of Hudson Bay, and northward to the Arctic Ocean, resting upon the Archæan rocks always in complete discordance, and lying generally at low angles of inclination, although often affected by great faults. The surface upon which these rocks have been deposited is that of a denudation-plane of flowing outline, not differing in any essential respect from that characterising parts of the same great plateau where there is no evidence to show that any deposition of strata has occurred since Archæan time Mr. Low, indeed, finds reason to believe that even the great valleys by which the Archæan plateau of Labrador is trenched had been cut out before the general subsidence which enabled the laying down of Animikie rocks upon this plateau to begin. The area over which these observations extend, thus in itself enables us to affirm that the unconformity existing between the Animikie or Keweenawan (as the case may be) and the Archæan is of the first order (*cf.* Selwyn, *Science*, February 9, 1883). It may be compared with that now known to occur between the Torridonian of Scotland and the underlying rocks there, and is evidenced by similar facts.

If the structural aspects of the Archæan rocks of the Protaxis are considered, the importance of this gap becomes still more apparent. We find long bands of strata referable to the Huronian and Grenville Series, occupying synclinal troughs, more or less parallel to each other and to the foliation of the Fundamental Gneiss, the strata, as well as the foliation, being in most cases at high angles, vertical, or even reversed. This structure is precisely that which would be discovered if a great mountain system, like that of the Alps, were to be truncated on a plane sufficiently low. Analogy thus leads to the belief that the Protaxis was originally, as Dana has suggested, a region of Appalachian folding, differing only from more modern examples of mountain regions of the same kind in its excessive width, which is so great as to render it difficult to conceive that crustal movements of sufficient magnitude to produce it could have occurred at any one period. It is thus, perhaps, more probable that successive and nearly parallel flexures of the kind, separated by long intervals of rest, piled range upon range against the central mass of the protaxial buttress subsequent to the Huronian period. In any case, the rugged mountain region brought into existence when the corrugation still evidenced by its remaining base occurred, was subsequently reduced by denudation to the condition of an undulating table-land such as has been named a "peneplain" by W. M. Davis—a surface approximating to a base-level of erosion. All this was accomplished after the close of the Huronian period, and before that time at which the first beds of the Animikie were laid down correlatively with a great subsidence. It would be difficult to deny that the time thus occupied may not have been equal in duration to that represented by the whole of the Palæozoic.

If we approach this ruling unconformity from above, in the region of the Protaxis, we find the Animikie and Keweenawan rocks uncrystalline, except when of volcanic origin, and resembling in their aspect the older Palæozoic sediments, but practically without characteristic organic remains so far as known. In order to bring ourselves into relation with the ascertained palæontological sequence, it is necessary to go further afield, and in so doing we lose touch, more or less completely, with the stable conditions of the Archæan platform, and are forced to apply indirectly such facts as it may be possible to ascertain in regions which have suffered more recent and complicated disturbance. It is thus not surprising that the taxonomic position of the Animikie and Keweenawan have been the subject of much controversy. It is not germane to the present discussion to enter at any length into this question, nor into the value of the unconformity which appears to exist between these two series. They have been classed collectively by Selwyn, N. H. Winchell, and others as Lower Cambrian, and are provisionally mapped as such by the Canadian Survey. It is believed to be more in accordance with the general principles of geological induction to refer these rocks above the great unconformity to the Cambrian, for the time being at least, than to unite them with the Huronian under any general term, or to erect a new system in which to place them. In so doing it has been assumed that the Cambrian is the lowest system of the Palæozoic, but of late years the position has been taken by good authorities that the true base of the Cambrian is to be found at the *Olenellus* zone; and while it appears very probable that, when fossils are found in the Animikie, they may be referable to this zone, the adoption of such an apparently arbitrary line certainly, for the time, must be considered as placing the Cambrian reference of the beds in question in doubt; but it does not interfere with a belief that if they should be found to be lower than Cambrian as thus defined, they may at least be considered as still in all probability Palæozoic.

The definition of the horizon of *Olenellus* as that of the base of the Cambrian is a question almost entirely palæontological, into which it is not proposed here to enter, further than to point out that it is only partially justified by what is known of North American geology. In the Atlantic province, and in the Appalachian region, there appears to be a very general physical break at about this stage, which it seems likely may correspond with the great unconformity at the base of the Animikie; but in the Rocky Mountain or Cordilleran region the *Olenellus* zone has been found high up in a series of conformable and similar sediments, coinciding with no break, and from these lower sediments some organic forms have been already recovered, but not such as to indicate any great diversity in fauna from that of the recognised Cambrian. Similarly, in one part of eastern Canada, Matthew has lately described a fauna contained in what he names the Etcheminian group, regarded by him as earlier than the *Olenellus* zone, but still Palæozoic. Recent discoveries of a like kind have been made in other parts of the world, as in the Salt Range of India. These facts have only last year been particularly referred to by Mr. Marr in his address to the Section.

The general tendency of our advance in knowledge appears, in fact, to be in the direction of extending the range of the Palæozoic downward, whether under the old name Cambrian, or under some other name applied to a new system defined, or likely to be defined, by a characteristic fauna; and under Cambrian or such new system, if it be admitted, it is altogether probable that the Animikie and Keweenawan rocks must eventually be included.

In other words, the somewhat arbitrary and artificial definition of the *Olenellus* zone as the base of the Cambrian, seems to be not only not of world-wide application, but not even of general applicability to North America; while, as a base for the Palæozoic Æon, it is of still more doubtful value. In the Cambrian period, as well as in much later geological times, the American continent does not admit of treatment as a single province, but is to be regarded rather as a continental barrier between two great oceanic depressions, each more or less completely different and self-contained in conditions and history—that of the Atlantic and that of the Pacific. On the Atlantic side the *Olenellus* zone is a fairly well-marked base for the Cambrian; on that of the Pacific it is found naturally to succeed a great consecutive and conformable series of sediments, of which the more ancient fauna is now only beginning to be known.

In thus rapidly tracing out what appears to me to be the leading thread of the history of the pre-Cambrian rocks of Canada, and in endeavouring to indicate the present condition of their classification, and to vindicate the substantial accuracy of the successive steps taken in its elaboration, many names and alternative systems of arrangement proposed at different times, by more or less competent authorities, have been passed without mention. This has been done either because such names and classifications appear now to be unnecessary or unfounded, or because they relate to more or less local subdivisions of the ruling systems which it is not possible to consider in so brief a review. This has been particularly the case in regard to the much-disputed region to the south of Lake Superior, out of which, however, after some decades of complicated and warring nomenclature, a classification, trending back substantially to that originally established and here advocated, is being evolved (albeit under strange names) by the close and skilful stratigraphical work in progress there.

It has also been my object, in so far as possible, by omitting special reference to divergent views, to avoid a controversial attitude, particularly in respect to matters which are still in the arena of active discussion, and in regard to which many points remain admittedly subject to modification or change of statement. But in conclusion, and from the point of view of Canadian geology, it is necessary to refer—even at the risk of appearing controversial—to the comparatively recent attempt to introduce an "Algonkian System," under which it is proposed to include all recognisable sedimentary formations below the *Olenellus* zone, assumed for this purpose to be the base of the Cambrian. If in what has already been said I have been able correctly to represent the main facts of the case—and it has been my endeavour to do so—it must be obvious that the adoption of such a "system" is a retrograde step, wholly opposed, not only to the historical basis of progress in classification, but also to the natural conditions upon which any taxonomic scheme should be based. It not only detaches from the Palæozoic great masses of conformable and fossiliferous strata beneath an arbitrary plane, but it unites these, under a common systematic name, with other vast series of rocks, now generally in a crystalline condition, and includes, as a mere interlude, what, in the region of the Protaxis at least, is one of the greatest gaps known to geological history. In this region it is made to contain the Keweenawan, the Animikie, the Huronian, and the Grenville Series, and that without in the least degree removing the difficulty found in defining the base of the last-mentioned series. It thus practically expunges the result of much good work, conducted along legitimate lines of advance during many previous years, with only the more than doubtful advantage of enabling the grouping together of many widely separated terranes in other districts where the relations have not been even proximately ascertained. It is in effect, to my mind, to constitute for geology what was known to the scholastic theologians of a former age as a *limbo*, appropriate as the abode of unjudged souls and unbaptised infants, that might well in this case be characterised as "a limbo large and broad."

It is not intended to deny that there may be ample room for the introduction of a new system, or perhaps, indeed, of an entire Geological Æon, between the Huronian, as we know it in Canada, and the lowest beds which may reasonably be considered as attaching to the Cambrian, or even to the Palæozoic as a whole. On the contrary, what has already been said will, I think, show that in the region of the Protaxis we might very reasonably speak of an "Algonkian hiatus," if we elect so to call it. Elsewhere it will undoubtedly be possible, sooner or later, to designate series of rocks laid down during the time represented only by orogenic movements and vast denudation in the province here more particularly referred to; but before any general systematic name is applied to such terranes they should be defined, and that in such a way as to exclude systems already established as the result of honest work.

It seems very likely, for instance, that the Grand Cañon Series, as last delimited by Walcott, separated by unconformities from the Tonto Cambrian above and the probably Archæan rocks below, may be referable to such an intermediate system; but here it may be noted, in passing, that the attempt to apply the new term "Algonkian" in this particular Western region, has led to the inclusion under that name of a great unconformity below the Grand Cañon Series, much resembling the post-Huronian break in the Lake Superior district.

For such unclassed rocks, wholly or in large part of sedimentary origin, the Canadian Survey has simply employed the term pre-Cambrian, involving for certain regions a frank confession of ignorance beyond a certain point. Indefinite as such a term is, it is believed to be more philosophical than to make an appearance of knowledge not borne out in fact, by the application of any systematic name not properly defined.

Although it would be unsuitable, at the close of this address, to introduce the old controversy respecting the Cambrian and Silurian, it may be noted that the ethical conceptions and many of the principles involved in that discussion still apply with undiminished value, and much of its literature may be re-read to-day with advantage. More particularly I would allude to Sedgwick's inimitable and now classic introduction to McCoy's "Palæozoic Fossils," one passage in which, paraphrased only by the change of names involved in that and in the present discussion, may be read as follows:—"'*Est Jupiter quodcunque vides*' was once said by Dean Conybeare in mockery of the old despotic rule of the name Greywacké. A golden age of truth and reason, and slow but secure inductive logic, seemed to follow, but the jovial days of a new dynasty are to spring up, it seems, under a new name not less despotic than the one which had ruled before it. If all the [sedimentary] rocks below the [*Olenellus* zone] are to pass under one name, let us cling to the venerable name Greywacké. It can do no mischief while it describes things indefinite, simply because it is without meaning. But the name [Algonkian], if used in the same extended sense, is pregnant with mischief. It savours of a history that is fabulous; it leads us back to a false type; it unites together as one systems that nature has put asunder."

SECTION D.

ZOOLOGY.

OPENING ADDRESS BY PROF. L. C. MIALL, F.R.S., PRESIDENT OF THE SECTION.

IT has long been my conviction that we study animals too much as dead things. We name them, arrange them according to our notions of their likeness or unlikeness, and record their distribution. Then perhaps we are satisfied, forgetting that we could do as much with minerals or remarkable boulders. Of late years we have attempted something more; we now teach every student of Zoology to dissect animals and to attend to their development. This is, I believe, a solid and lasting improvement; we owe it largely to Huxley, though it is but a revival of the method of Döllinger, who may be judged by the eminence of his pupils and by the direct testimony of Baer to have been one of the very greatest of biological teachers. But the animals set before the young zoologist are all dead; it is much if they are not pickled as well. When he studies their development, he works chiefly or altogether upon continuous sections, embryos mounted in balsam, and wax models. He is rarely encouraged to observe live tadpoles or third-day chicks with beating hearts. As for what Gilbert White calls the *life and conversation of animals*, how they defend themselves, feed, and make love, this is commonly passed over as a matter of curious but not very important information; it is not reputed scientific, or at least not eminently scientific.

Why do we study animals at all? Some of us merely want to gain practical skill before attempting to master the structure of the human body; others hope to qualify themselves to answer the questions of geologists and farmers; a very few wish to satisfy their natural curiosity about the creatures which they find in the wood, the field, or the sea. But surely our chief reason for studying animals ought to be that we would know more of life, of the modes of growth of individuals and races, of the causes of decay and extinction, of the adaptation of living organisms to their surroundings. Some of us even aspire to know in outline the course of life upon the earth, and to learn, or, failing that, to conjecture, how life originated. Our own life is the thing of all others which interests us most deeply, but everything interests us which throws even a faint and reflected light upon human life. Perhaps the professor of Zoology is prudent in keeping so close as he does to the facts of structure, and in shunning the very attempt to interpret, but while he wins safety he loses his hold upon our attention. Morphology is very well; it may be exact; it may prevent or expose serious errors. But Morphology is not an end in itself. Like the systems of Zoology, or the records of distribution, it draws whatever interest it possesses from that life which creates organs and adaptations. To know more of life is an aim as nearly ultimate and self-explanatory as any purpose that man can entertain.

Can the study of life be made truly scientific? Is it not too vast, too inaccessible to human faculties? If we venture into this alluring field of inquiry, shall we gain results of permanent value, or shall we bring back nothing better than unverified speculations and curious but unrelated facts?

The scientific career of Charles Darwin is, I think, a sufficient answer to such doubts. I do not lay it down as an article of the scientific faith that Darwin's theories are to be taken as true; we shall refute any or all of them as soon as we know how; but it is a great thing that he raised so many questions which were well worth raising. He set all scientific minds fermenting, and not only Zoology and Botany, but Palæontology, History, and even Philology bear some mark of his activity. Whether his main conclusions are in the end received, modified, or rejected, the the effect of his work cannot be undone. Darwin was a bit of a sportsman and a good deal of a geologist; he was a fair anatomist and a working systematist; he keenly appreciated the value of exact knowledge of distribution. I hardly know of any aspect of natural history, except synonymy, of which he spoke with contempt. But he chiefly studied animals and plants as living beings. They were to him not so much objects to be stuck through with pins, or pickled, or dried, or labelled, as things to be watched in action. He studied their difficulties, and recorded their little triumphs of adaptation with an admiring smile. We owe as many discoveries to his sympathy with living nature as to his exactness or his candour, though these too were illustrious. It is not good to idolise even our greatest men, but we should try to profit by their example. I think that a young student, anxious to be useful but doubtful of his powers, may feel sure that he is not wasting his time if he is collecting or verifying facts which would have helped Darwin.

Zoologists may justify their favourite studies on the ground that to know the structure and activities of a variety of animals enlarges our sense of the possibilities of life. Surely it must be good for the student of Human Physiology, to take one specialist as an example of the rest, that he should know of many ways in which the same functions can be discharged. Let him learn that there are animals (star-fishes) whose nervous system lies on the outside of the body, and that in other animals it is generally to be found there during some stage of development; that there are animals whose circulation reverses its direction at frequent intervals either throughout life (Tunicata) or at a particular crisis (insects at the time of pupation); that there are animals with eyes on the back (Oncidium, Scorpion), on the shell (some Chitonidæ), on limbs or limb-like appendages, in the brain-cavity, or on the edge of a protective fold of skin; that there are not only eyes of many kinds with lenses, but eyes on the principle of the pin-hole camera without lens at all (Nautilus) and of every lower grade down to mere pigment-spots; that auditory organs may be borne upon the legs (insects) or the tail (Mysis); that they may be deeply sunk in the body, and yet have no inlet for the vibrations of the sonorous medium (many aquatic animals). It is well that he should know of animals with two tails (Cercaria of Gasterostomum) or with two bodies permanently united (Diplozoon); of animals developed within a larva which lives for a considerable time after the adult has detached itself (some star-fishes and Nemertines); of animals which lay two (Daphnia) or three kinds of eggs (Rotifera); of eggs which regularly produce two (Lumbricus trapezoides) or even eight embryos apiece (Praopus[1]); of males which live parasitically upon the female (Cirripedes), or even undergo their transformations, as many as eighteen at a time, in her gullet (Bonellia); of male animals which are mere bags of sperm-cells (some Rotifera, some Ixodes, parasitic Copepods) and of female animals which are mere bags of eggs (Sacculina, Entoconcha). The more the naturalist knows of such strange deviations from the familiar course of things, the better will he be prepared to reason about what he sees, and the safer will he be against the perversions of hasty conjecture.

If a wide knowledge of animals is a gain to Physiology and every other branch of Biology, what opportunities are lost by our ignorance of the early stages of so many animals! They are often as unlike to the adult in structure and function as if they belonged to different genera, or even to different families. Zoologists have made the wildest mistakes in classifying larvæ whose subsequent history was at the time unknown. The naturalist who devotes himself to life-histories shares the advantage of the naturalist who explores a new continent. A wealth of new forms is opened out before him. Though Swammerdam, Réaumur, De Geer, Vaughan Thompson, Johannes Müller and a crowd of less famous naturalists have gone before us, so much remains to be done that no zealous inquirer can fail to discover plenty of untouched subjects in any wood, thicket, brook or sea.

Whoever may attempt this kind of work will find many difficulties and many aids. He will of course find abundant exercise for all the anatomy and physiology that he can command. He will need the systems of descriptive Zoology, and will often be glad of the help of professed systematists. The work cannot be well done until it is exactly known what animal is being studied. For want of this knowledge, hardly attainable 150 years ago, Réaumur sometimes tells us curious things which we can neither verify nor correct; at times we really do not know what animal he had before him. The student of life-histories will find a use for physics and chemistry, if he is so lucky as to remember any. Skill in drawing is valuable, perhaps indispensable.

If by chance I should be addressing any young naturalist who thinks of attending to life-histories, I would beg him to study his animals alive and under natural conditions. To pop everything into alcohol and make out the names at home is the method of the collector, but life-histories are not studied in this way. It is often indispensable to isolate an animal, and for this purpose a very small habitation is sometimes to be preferred. The tea-cup aquarium, for instance, is often better than the tank. But we must also watch an animal's behaviour under altogether natural circumstances, and this is one among many reasons for choosing our subject from the animals which are

[1] Hermann von Jhering, *Sitz. Berl. Akad.*, 1885; *Biol. Centralbl.*, Bd. vi. pp. 532-539 (1886).

locally common. Let us be slow to enter into controversies. After they have been hotly pursued for some time, it generally turns out that the disputants have been using words in different senses. Discussion is excellent, controversy usually barren. Yet not always; the Darwinian controversy was heated, and nevertheless eminently productive; all turns upon the temper of the men concerned, and the solidity of the question at issue. One more hint to young students. Perhaps no one ever carried through a serious bit of work without in some stage or other longing to drop it. There comes a time when the first impulse is spent, and difficulties appear which escaped notice at first. Then most men lose hope. That is the time to show that we are a little better than most men. I remember as a young man drawing much comfort from the advice of a colleague, now an eminent chemist, to whom I had explained my difficulties and fears. All that he said was: "Keep at it," and I found that nothing more was wanted.

I greatly believe in the value of association. It is good that two men should look at every doubtful structure and criticise every interpretation. It is often good that two talents should enter into partnership, such as a talent for description and a talent for drawing. It is often good that an experienced investigator should choose the subject and direct the course of work, and that he should be helped by a junior, who can work, but cannot guide. It seems to me that friendly criticism before publication is often a means of preventing avoidable mistakes. I am sorry that there should be any kind of prejudice against co-operation, or that it should be taken to be a sign of weakness. There are, I believe, very few men who are so strong as not to be the better for help. One difficulty would be removed if known authors were more generous in acknowledging the help of their assistants. They ought not to be slow to admit a real helper to such honour as there may be in joint-authorship.

Among the most important helps to the student of life-histories must be mentioned the zoological stations now maintained by most of the great nations. The parent of all these, the great zoological station at Naples, celebrated its twenty-fifth anniversary last April, so that the whole movement belongs to our own generation. How would Spallanzani and Vaughan Thompson and Johannes Müller have rejoiced to see such facilities for the close investigation of the animal life of the sea! The English-speaking nations have taken their fair share of the splendid work done at Naples, and it is pleasant to remember that Darwin subscribed to the first fund, while the British Association, the University of Cambridge and the Smithsonian Institution have maintained their own tables at the station.[1] The material support thus given is small when compared with the subsidies of the German Government, and not worth mention beside the heroic sacrifices of the Director, Dr. Anton Dohrn, but as proofs of lively interest in a purely scientific enterprise they have their value. Marine stations have now multiplied to such a point that a bare enumeration of them would be tedious. Fresh-water biological stations are also growing in number. Forel set an excellent example by his investigation of the physical and biological phenomena of the Lake of Geneva. Dr. Anton Fritsch of Prag followed with his movable station. There is a well-equipped station at Plön among the lakes of Holstein, and a small one on the Müggelsee near Berlin. The active station of Illinois is known to me only by the excellent publications which it has begun to issue. France, Switzerland, Sweden and Finland all have their fresh-water biological stations, and I hope that England will not long remain indifferent to so promising a sphere of investigation.

Biological work may answer many useful purposes. It may be helpful to industry and public health. Of late years the entomologist has risen into sudden importance by the vigorous steps taken to discourage injurious insects. I have even known a zoological expert summoned before a court of law in order to say whether or not a sword-fish can sink a ship. I would not on any account run down the practical applications of Biology, but I believe that the first duty of the biologist is to make science, and that science is made by putting and answering questions. We are too easily drawn off from this, which is our main business, by self-imposed occupations, of which we can often say nothing better than that they do no harm except to the man who undertakes them. There are, for example, a good many lists of species which are compiled without any clear scientific object. We have a better prospect of working to good purpose when we try to answer definite questions. I propose to spend what time remains in putting and answering as well as I can a few of the questions which occur to any naturalist who occupies himself with life-histories. Even a partial answer—even a mistaken answer is better than the blank indifference of the collector, who records and records, but never thinks about his facts.

The first question that I will put is this:—Why do some animals undergo transformation while others do not? It has long been noticed[1] that as a rule fresh-water and terrestial animals do not go through transformation, while their marine allies do. Let us take half a dozen examples of each:—

Fluviatile or terrestrial. Without transformation.	Marine. With transformation.
Crayfish.	Crab.
Earthworm.	Polygordius.
Helix.	Doris, Æolis.
Cyclas.	Oyster.
Hydra.	Most Hydrozoa.
&c.	&c.

We get a glimmer of light upon this characteristic difference when we remark that in fresh-water and terrestrial species the eggs are often larger than in the allied marine forms. A large egg favours *embryonic* as opposed to *larval* development. An embryo which is formed within a large egg may feed long upon the food laid up for it, and continue its development to a late stage before hatching. But if there is little or no yolk in the egg, the embryo will turn out early to shift for itself. It will be born as a larva, provided with provisional organs suited to its small size and weakness. Large eggs are naturally fewer than small ones. Does the size depend on the number, or the number on the size? To answer in a word, I believe that the size generally depends on the number, and that the number is mainly determined by the risks to which the species are exposed. At least so many eggs will in general be produced as can maintain the numbers of the species in spite of losses, and there is some reason to believe that in fresh waters the risks are less than in the shallow seas or at the surface of the ocean.[2] In most parts of the world the fresh waters are of small size, and much cut up. Every river-basin forms a separate territory. Isolation, like every other kind of artificial restriction, discourages competition, and impedes the spread of successful competitors. In the shallow seas or at the surface of the ocean conquering forms have a free course; in lakes and rivers they are soon checked by physical barriers.

A large proportion of animals are armour-clad, and move about with some difficulty when they have attained their full size. The dispersal of the species is therefore in these cases effected by small and active larvæ. Marine animals (whether littoral or pelagic) commonly produce vast numbers of locomotive larvæ, which easily travel to a distance. Floating is easy, and swimming not very difficult. A very slightly built and immature larva can move about by cilia, or take advantage of currents, and a numerous brood may be dispersed far and wide while they are mere hollow sacs, without mouth, nerves or sense-organs. Afterwards they will settle down, and begin to feed. In fresh waters armour is as common, for all that I know, as in the sea, but locomotive larvæ are rare.[3] There is no space for effective migration. Even a heavy-armoured and slow-moving crustacean or pond-snail can cross a river or lake, and to save days or hours is unimportant. In rivers, as Sollas has pointed out, free-swimming larvæ would be subject to a special risk, that of being swept out to sea. This circumstance may have been influential, but the diminished motive for migration is probably more important. At least an occasional transport to a new area is indispensable to most fresh-water organisms, and very unexpected modes of

[1] To this list may now be added the University of Oxford.

[1] Darwin, "Origin of Species," chap. xiii.; Fritz Müller, "Für Darwin," chap. vii.

[2] Indications are given by the survival in fresh waters of declining groups, *e.g.* Ganoid Fishes, which, when dominant, maintained themselves in the sea; and by the not uncommon case of marine animals which enter rivers to spawn. I do not attempt to count among these indications the supposed geological antiquity of fluviatile as compared with marine animals. Some marine genera are extremely ancient (Lingula, Nucula, Trigonia, Nautilus); a perfectly fair comparison is almost impossible; and great persistence does not necessarily imply freedom from risks. In the Mollusca, which afford a good opportunity of testing the effect of habitat upon the number of the eggs, marine species seem to produce more eggs as a rule than fluviatile, and these many more than terrestrial species.

[3] Dreyssensia and Cordylophora are examples of animals which seem to have quite recently become adapted to fresh-water life, and have not yet lost their locomotive larvæ. Many instances could be quoted of marine forms which have become fluviatile. The converse is, I believe, comparatively rare.

dispersal are sometimes employed, not regularly in each generation, but at long intervals, as opportunity offers.

Early migration by land is nearly always out of the question. Walking, and still more flying, are difficult exercises, which call for muscles of complex arrangement and a hard skeleton. A very small animal, turned out to shift for itself on land, would in most cases perish without a struggle. There might be just a chance for it, if it could resist superficial drying, and were small enough to be blown about by the wind (Infusoria, Rotifera, and certain minute Crustacea), or if it were born in a wet pasture, like some parasitic worms.

We can define two policies between which a species can make its choice. It may produce a vast number of eggs, which will then be pretty sure to be small and ill-furnished with yolk. The young will hatch out early, long before their development is complete, and must migrate at once in search of food. They will, especially if the adult is slow-moving or sedentary, be furnished with simple and temporary organs of locomotion, and will generally be utterly unlike the parent. The majority will perish early, but one here and there will survive to carry on the race.

Or the parent may produce a few eggs at a time, stock them well with yolk, and perhaps watch over them, or even hatch them within her own body. The young will in such cases complete their development as embryos, and when hatched, will resemble the parent in everything but size.

Which policy is adopted will largely depend upon the number of the family and the capital at command. There are animals which are like well-to-do people, who provide their children with food, clothes, schooling, and pocket-money. Their fortunate offspring grow at ease, and are not driven to premature exercise of their limbs or wits. Others are like starving families, which send the children, long before their growth is completed, to hawk matches or newspapers in the streets.

In Biology we have no sooner laid down a principle than we begin to think of exceptions. The exceptions may be apparent only; they may, when fully understood, confirm instead of disturbing the general principle. But this rarely happens unless the principle is a sound one *Exceptio probat regulam;* it is the exception which tests the rule, to give a new application to an old maxim.

Parasites form one group of exceptions to our rule. Whether they pass their free stages in air, water or earth, whether their hosts are marine, fluviatile or terrestrial, they are subject to strange transformations, which may be repeated several times in the same life-history. The change from one host to another is often a crisis of difficulty; many fail to accomplish it; those which succeed do so by means of some highly peculiar organ or instinct, which may be dropped as quickly as it is assumed. The chances of failure often preponderate to such an extent that an enormous number of eggs must be liberated. Even a brief parasitism may produce a visible effect upon the life-history. The young Unio or Anodon attaches itself for a short time to some fish or tadpole. To this temporary parasitism is due, as I suppose, the great number of eggs produced, and a degree of metamorphosis, unusual in a fresh-water mollusk.

The Cephalopoda, which are wholly marine, and the Vertebrates, whatever their habitat, very rarely exhibit anything which can be called transformation. Some few cases of Vertebrate transformation will be discussed later. Cephalopods and Vertebrates are large, strong, quick-witted animals, able to move fast, and quite equal in many cases to the defence of themselves and their families. They often produce few young at a time, and take care of them (there are many examples to the contrary among Cephalopods and Fishes). They are generally able to dispense with armour, which would have indirectly favoured transformation.

Echinoderms, which are all marine, develop with metamorphosis. There is an interesting exception in the Echinoderms with marsupial development, which develop directly, and give an excellent illustration of the effect of parental care.

Insects, which as terrestrial animals should lay a few large eggs, and develop directly, furnish the most familiar and striking of all transformations. I have already discussed this case at greater length than is possible just now (NATURE, December 19, 1895). I have pointed out that the less specialised insect-larvæ, *e.g.* those of Orthoptera, make a close approach to some wingless adult insects, such as the Thysanura, as well as to certain Myriopods. Fritz Müller seems to me to be right in saying that the larvæ of non-metamorphic insects come nearer than any winged insect to primitive Tracheates. The transformation of the Bee, Moth, or Blow-fly is transacted after the stage in which the normal Tracheate structure is attained, and I look upon it as a peculiar *adult* transformation, having little in common with the transformations of Echinoderms, Mollusks, or Crustaceans.

In the same way I believe that some Amphibia have acquired an adult transformation. Frogs and toads, having already as tadpoles attained the full development of the more primitive Amphibia, change to lung-breathing, tailless, land-traversing animals, able to wander from the place of their birth, to seek out mates from other families, and to lay eggs in new sites.

Medusæ furnish a third example of adult transformation, which seems to find its explanation in the sedentary habit of the polyp, which probably nearly approaches the primitive adult stage. But here the case is further complicated, for the polyp still proceeds from a planula, which is eminently adapted for locomotion, though perhaps within a narrower range. We have two migratory stages in the life-history. Each has its own advantages and disadvantages. The planula, from its small size, is less liable to be devoured, or stranded, or dashed to pieces, but it cannot travel far; the medusa may cross wide seas, but it is easily captured and is often cast up upon a beach in countless multitudes.

Adult transformation may be recognised by its occurrence after the normal structure of the group has been acquired, and also by its special motive, which is egg-laying and all that pertains to it; the special motive of larval transformation is dispersal for food.

The reproduction of the common Eel has been a mystery ever since the days of Aristotle, though a small part of the story was made out even in ancient times. It was long ago ascertained that the Eel, which seeks its food in rivers, descends to the sea in autumn or early winter, and that it never spawns, nor even becomes mature in fresh waters. The Eels which descend to the sea never return, but young eels or Elvers come up from the sea in spring, millions at a time. The Elvers have been seen to travel along the bank of a river in a continuous band or eel-rope, which has been known to glide upwards for fifteen days together. It was, of course, concluded that spawning and early development took place in the sea during the interval between the autumn and spring migration, but no certain information came to hand till 1896. Meanwhile this gap in our knowledge was a perplexity, almost a reproach to zoologists. The partially-known migration of the Eel could not be harmonised with the ordinary rule of migratory fishes. We tried to explain the passage of marine fishes into rivers at spawning time by the supposition (a true supposition, as I think) that the river is less crowded than the shallow seas, and therefore a region in which competition is less severe. The river is to some migratory fishes what the tundras of Siberia are to some migratory birds, places comparatively free from dangerous enemies, and therefore fit for the rearing of the helpless young. But the Eel broke the rule, and cast doubt upon the explanation. The Salmon, Sturgeon and Lamprey feed and grow in the sea, and enter rivers to spawn. The Eel feeds and grows in rivers, but enters the sea to spawn. What possible explanation could meet cases thus diametrically opposite?

This was the state of matters when Grassi undertook to tell us that part of the history of the Eel which is transacted in the sea. When it leaves the river, it makes its way to very deep water, and there undergoes a change. The eyes enlarge, and become circular instead of elliptical; the pectoral fins and the border of the gill-cover turn black; the reproductive organs, only to be discovered by microscopic search before this time, enlarge. The Eels, thus altered in appearance and structure, lay their eggs in water of not less than 250 fathoms' depth. The upper limit of the spawning-ground is nearly three times as far from sea-level as the 100-fathom line which we arbitrarily quote as the point at which the deep sea begins. The eggs, which are large for a fish (2·7 mm. diam.), float but do not rise. The young which issue from them are quite unlike the Eels of our rivers; they are tape-like, transparent, colourless, devoid of red blood, and armed with peculiar teeth. A number of different kinds of such fishes had been previously known to the naturalist as Leptocephali. Günther had conjectured that they were abnormal larvæ, incapable of further development. Grassi has, however, succeeded in proving that one of these Leptocephali (*L. brevirostris*) is simply a larval Eel; others are larvæ of Congers and various Murænoid fishes. He has with infinite pains compared a number of Leptocephali, and coordinated their stages, making out some particularly important ones by the direct observation of live specimens.

You will not unnaturally ask how Grassi or anybody else can tell what goes on in the sea at a depth of over 250 fathoms. His inquiries were carried on at Messina, where the local circumstances are very fortunate. Strong currents now and then boil up in the narrow strait, sweeping to the surface eggs, larvæ, and a multitude of other objects which at ordinary seasons lie undisturbed in the tranquil depths. Further information has been got by dredging, and also by opening the body of a sun-fish (*Orthagoriscus mola*), which at certain times of the year is taken at the surface, and is always found to contain a number of Leptocephali. When a Leptocephalus has completed its first stage of growth, it ceases to feed, loses bulk, and develops pigment on the surface of the body. At the same time the larval teeth are cast, and the larval skeleton is replaced. Then the fish begins to feed again, comes to the surface, enters the mouth of a river, and, if caught, is immediately recognised as an Elver or young Eel. It is now a year old, and about two inches long.

This history suggests a question. Are the depths of the sea free from severe competition? The darkness, which must be nearly or altogether complete, excludes more than the bare possibility of vegetation. A scanty subsistence for animals is provided by the slowly-decomposing remains of surface-life. When the dredge is sunk so low, which does not often happen, it may bring up now and then a peculiar and specially modified inhabitant of the dark and silent abyss. There cannot, we should think, be more than the feeblest competition where living things are so few, and the mode of life so restricted. Going a step further, we might predict that deep-sea animals would lay few eggs at a time, and that these would develop directly—*i.e.* without transformation. The risk of general reasoning about the affairs of living things is so great that we shall hold our conjectures cheap unless they are confirmed by positive evidence. Happily this can be supplied. The voyage of the *Challenger* has yielded proof that the number of species diminishes with increasing depth, and that below 300 fathoms living things are few indeed.[1] Dr. John Murray gives us the result of careful elaboration of all the facts now accessible, and tells us that the majority of the abyssal species develop directly (NATURE, March 25, 1897).

We seem, therefore, to have some ground for believing that the depths of the sea resemble the fresh waters in being comparatively free from enemies dangerous to larvæ. The Eel finds a safe nursery in the depths, and visits them for the same reason that leads some other fishes to enter rivers. It may be that the depths of the sea are safer than rivers, in something like the same degree and for the same reasons that rivers are safer than shallow seas. But we must be careful not to go too fast. It may turn out that deep recesses in the shallower seas—holes of limited extent in the sea-bottom—enjoy an immunity from dangerous enemies not shared by the great and continuous ocean-floor.[2]

After this short review of the facts I come to the conclusion that the general rule which connects the presence or absence of transformation with habitat is well-founded, but that it is apt to be modified and even reversed by highly special circumstances. The effect of habitat may, for instance, be over-ruled by parasitism, parental care, a high degree of organisation, or even by a particular trick in egg-laying. The direct action of the medium is probably of little consequence. Thus the difference between fresh and salt water is chiefly important because it prevents most species from passing suddenly from one to the other. But the abyssal and the fluviatile faunas have much in common, as also have the littoral and the pelagic faunas. Relative density and continuity of population seem to be of vital importance, and it is chiefly these that act upon the life-history.

In Zoology, as in History, Biography, and many other studies, the most interesting part of the work is only to be enjoyed by those who look into the details. To learn merely from text-books is notoriously dull. The text-book has its uses, but, like other digests and abridgments, it can never inspire enthusiasm. It is the same with most lectures. Suppose that the subject is that well-worn topic, the Alternation of Generations. The name recalls to many of us some class-room of our youth, the crudely coloured pictures of unlikely animals which hung on the walls, and the dispirited class, trying to write down from the lecture the irreducible minimum which passes a candidate. The lecturer defines his terms and quotes his examples; we have Salpa, and Aurelia, and the Fern, and as many more as time allows. How can he expect to interest anybody in a featureless narrative, which gives no fact with its natural circumstances, but mashes the whole into pemmican? What student goes away with the thought that it would be good and pleasant to add to the heap of known facts? The heap seems needlessly big already. And yet every item in that dull mass was once deeply interesting, moving all naturalists and many who were not naturalists to wonder and delight. The Alternation of Generations worked upon men's minds in its day like Swammerdam's discovery of the butterfly within the caterpillar, or Trembley's discovery of the budding Hydra, which when cut in two made two new animals, or Bonnet's discovery that an Aphis could bring forth living young without having ever met another individual of its own species. All these wonders of nature have now been condensed into glue. But we can at any time rouse in the minds of our students some little of the old interest, if we will only tell the tale as it was told for the first time

Adalbert Chamisso, who was in his time court-page, soldier, painter, traveller, poet, novelist, and botanist, was the son of a French nobleman. When he was nine years old, he and all the rest of the family were driven out of France by the French Revolution. Chamisso was educated anyhow, and tried many occupations before he settled down to Botany and light literature. In 1815 he embarked with Eschscholtz on the Russian voyage round the world commanded by Kotzebue. The two naturalists (for Chamisso is careful to associate Eschscholtz with himself, and even to give him priority) discovered a highly curious fact concerning the Salpæ, gelatinous Tunicates which swim at the surface of the sea, sometimes in countless numbers. There are two forms in the same species, which differ in anatomical structure, but especially in this, that one is solitary, the other composite, consisting of many animals united into a chain which may be yards long. Chamisso and Eschscholtz ascertained that the solitary form produces the chain-form by internal budding, while the chain-form is made up of hermaphrodite animals which reproduce by fertilised eggs.[1] There is thus, to use Chamisso's own words, "an alternation of generations. . . . It is as if a caterpillar brought forth a butterfly, and then the butterfly a caterpillar." Here the phrase *bring forth* is applied to two very different processes, viz. sexual reproduction and budding. Chamisso's phrase, "alternation of generations," is not exact. Huxley would substitute *alternation of generation with gemmation*, and if for shortness we use the old term, it must be with this new meaning. Subsequent investigation, besides adding many anatomical details, has confirmed one interesting particular in Chamisso's account, viz that the embryo of Salpa is nourished by a vascular placenta.[2] The same voyage yielded also the discovery of Appendicularia, a permanent Tunicate tadpole, and the first tadpole found in any Tunicate.

Some ten years after the publication of Chamisso's alternation of generations in Salpa, a second example was found in a common jelly-fish (Aurelia). Not a few Hydrozoa had by this time been named, and shortly characterised. Some were polyps, resembling the Hydra of our ponds, but usually united into permanent colonies; others were medusæ, bell-shaped animals which swim free in the upper waters of the sea. It was already suspected that both polyps and medusæ had a common structural plan, and more than one naturalist had come very near to knowing that medusæ may be the sexual individuals of polyp-colonies.

This was the state of matters when an undergraduate in Theology of the University of Christiania, named Michael Sars, discovered and described two new polyps, to which he gave the names, now familiar to every zoologist, of Scyphistoma and Strobila. In the following year (1830) Sars settled at Kinn, near Bergen, as parish priest, and betook himself to the life-long study of the animals of the Norwegian seas. He soon found out that his Scyphistoma was merely an earlier stage of his Strobila. Scyphistoma has a Hydra-like body, less than half an inch long,

[1] *Challenger* Reports. "Summary of Scientific Results" (1895), pp. 1430–96.

[2] I am aware that other things affect the interests of animals, and indirectly determine their structure, besides danger from living enemies. So complicated a subject can only be discussed in a short space if large omissions are tolerated.

[1] Brooks maintains that the solitary Salpa, which is female, produces a chain of males by budding, and lays an egg in each. These eggs are fertilised while the chain is still immature, and develop into females (solitary Salpæ). The truth of this account must be determined by specialists.

[2] Cuvier had previously noted the fact.

and drawn out into a great number of immensely long tentacles. It buds laterally like a Hydra, sending out stolons or runners, which bear new polyps, and separate before long, the polyps becoming independent animals. In the midst of the tentacles of the scyphistoma is a prominence which bears the mouth. This grows upwards into a tall column, the strobila, which is supported below by the scyphistoma. When the strobila is well nourished it divides into transverse slices, which at length detach themselves, and swim away.[1] These are the Ephyræ, which had been found in the sea before Sars' time, and were then counted as a particular kind of adult medusæ. They are small, flat discs with eight lobes or arms, all notched at the extremity. A pile of ephyræ is produced by the transverse constriction and division of the strobila in a fashion which reminds us of the rapid production of the animals in a Noah's ark by the slicing of a piece of wood of suitable sectional figure. It was thus ascertained that the scyphistoma, strobila, and ephyra are successive stages of one animal, but for a time no one could say where the scyphistoma came from, nor what the ephyra turned to. At length Sars, aided by the anatomical researches of Ehrenberg and Siebold, was able to clear up the whole story. The ephyra is gradually converted by increase of size and change of form into an Aurelia, a common jelly-fish which swarms during the summer in European seas. The Aurelia is of two sexes, and the eggs of the female give rise to ciliated embryos, which had been seen before Sars' time, but wrongly interpreted as parasites or diminutive males. These ciliated embryos, called planulæ, swim about for a time, and then settle down as polyps (scyphistomata). There is thus a stage in which Aurelia divides without any true reproductive process, and another stage in which it produces fertile eggs. There is alternation of generations in Aurelia as well as in Salpa, and Sars was glad to fortify by a fresh example the observations of Chamisso, on which doubts had been cast.

It was not long before the alternation of generations was recognised in Hydromedusæ also, and then the ordinary Hydrozoan colony was seen to consist of at least two kinds of polyps, one sexual, the other merely nutrient, both being formed by the budding of a single polyp. The sexual polyp, or medusa, either swims away or remains attached to the colony, producing at length fertilised eggs, which yield planulæ, and these in turn the polyps which found new colonies.

Those of us who are called upon to tell this story in our regular course of teaching should not forget to produce our scyphistoma, strobila and ephyra; the interest is greatly enhanced if they are shown alive. It is not hard to maintain a flourishing marine aquarium even in an inland town, and a scyphistoma may be kept alive in an aquarium for years, budding out its strobila every spring.

Alternation of generations, when first announced, was taken to be a thing mysterious and unique. Chamisso brought in the name, and explained that he meant by it a metamorphosis accomplished by successive generations, the form of the animal changing not in the course of an individual life, but from generation to generation (*forma per generationes, nequaquam in prole seu individuo, mutata*). Sars adopted Chamisso's name and definition. Steenstrup a little later collected and discussed all the examples which he could discover, throwing in a number which have had to be removed again, as not fairly comparable with the life-histories of Salpa and Aurelia. He emphasised the alternation of budding with egg-production, and the unlikeness in form of the asexual and sexual stages. Like Chamisso, he carefully distinguished between development with metamorphosis and alternation of generations. All three naturalists, Chamisso, Sars and Steenstrup, laid stress on this point. In an insect, they would have said, there is development with metamorphosis. The same animal passes from larva to pupa, and from pupa to imago. In Aurelia or Salpa, however, the animal which lays eggs is not the animal which buds, but its progeny. The cycle of the life-history includes two generations and many individuals.

This view has spread very widely, and if we were to judge by what is commonly taught, I think that we should recognise this as the doctrine now prevalent. It is however, in my opinion, far inferior as an explanation of the facts to that adopted by Leuckart, Carpenter and Huxley, who regard the whole cycle, from egg to egg, as one life-history. Huxley and Carpenter, differing in this from Leuckart, do not shrink from calling the whole product of the egg an animal, even though it consists of a multitude of creatures which move about and seek their food in complete independence of one another. Rather than ignore the unity of the life-history of Aurelia or Salpa, they would adopt the most paradoxical language. This attitude was forced upon them by the comparative method. They refused to study Aurelia, for example, as an animal apart; it had its near and its remoter relatives. Among these is the fresh-water Hydra, which develops without transformation, buds off other Hydras when food is plentiful, and at length becomes sexually mature. Budding is here a mere episode, which may be brought in or left out, according to circumstances. The same individual polyp which buds afterwards produces eggs. The life-history of Salpa cannot be traced with equal facility to a simple beginning, for it presents points of difficulty, on which the learned differ. In the Polychæt Worms, however, we find a beautiful gradation leading up to alternation of generations. We begin with gradual addition of new segments and increasing specialisation of the two ends of the body, the fore end becoming non-reproductive, and the hinder end reproductive. Then we reach a stage (Syllis) in which the reproductive half breaks off from the fore part, and forms (after separation) a new head, while the fore part adds new segments behind. In Autolytus the new head forms before separation, and many worms may cohere for a time, forming a long chain with heads at intervals. In Myrianida the worms break up first, and afterwards become sexually mature. We should gather from these cases that alternation of generations may arise by the introduction of a budding-stage into a development with transformation. The polyp or worm buds while young and lays eggs at a later time. The separation of the two processes of reproduction often becomes complete, each being restricted to its own place in the life-history. As a rule the worm or polyp will bud while its structure is uncomplicated by reproductive organs. It is easy to propagate some plants by cutting one of the leaves into sections, and making every section root itself, and grow into a new plant; but we can seldom do the same thing with a flower. There may therefore be a distinct advantage to particular animals and plants in dividing the life-history into two stages, an earlier budding, and a later egg-laying stage.

The advantage to be drawn from budding is easily seen in those animals which find it hard to gain access to a favourable site. Thus a Tænia[1] is very lucky when it establishes itself in the intestine. Once there, it goes on budding indefinitely. It is harder to trace the advantage in the case of many polyps, though some (Cunina, &c.) admit of the same explanation as Tænia. There are yet other cases (some Worms, Salpæ, &c.) in which our ignorance of the conditions of life renders a satisfactory explanation impossible at present.

The budded forms often differ in structure from the budding forms which produce them, and many writers and teachers make this difference part of the definition of alternation of generations. I think that Leuckart has suggested a probable explanation in his essay of 1851,[2] which is still thoroughly profitable reading. He attributes the peculiarities of the larva mainly to the circumstance that it is turned out at an early age to shift for itself. In the budded forms there is no such necessity. The parent has established itself on a good site which commands a sufficiency of food. Until it has done this, it does not bud at all. The young which it produces asexually need not disperse in infancy, at least until crowding sets in. The tradesman who has founded a business puts his elder boys into the shop; perhaps the younger ones may be obliged to try their luck in a distant town. The budded forms, reared at the cost of the parent, may therefore omit the early larval stages at least, and go on at once to a later or even to the final stage. Thus the head of Tænia, when it has fixed itself in the intestine, produces sexual segments; the redia of Distomum produces cercariæ or more rediæ, omitting the locomotive embryo; the scyphistoma produces ephyræ. The saving of time must often be great, and the days saved are days of harvest. Think how much a tree would lose if in the height of summer it were unable to bud, and could only

[1] Leuckart (*Zeits. f. wiss. Zool.*, Bd. iii. p. 181) remarks that elongate animals tend to divide transversely or to bud axially, while broad animals tend to divide longitudinally or to bud laterally. The question has been raised more than once whether the division of the strobila is not really a case of budding. Leuckart shows that budding and fission cannot be separated by any definition; they pass insensibly into one another ("Wagner's Handb. d. Physiol.," art. "Zeugung").

[1] This case is quoted by Leuckart.

[2] "Ueber Metamorphose, ungeschlechtliche Vermehrung, Generationswechsel," *Zeits. f. wiss. Zool.*, Bd. iii. Equally important is the same author's treatise, "Ueber den Polymorphismus der Individuen oder die Erscheinung der Arbeitstheilung in der Natur," Giessen, 1851.

propagate by seeds. If the budded forms are sexual, while the budding forms are not, there is an obvious explanation of the difference in form. Even where there is no such fundamental difference in function, the circumstances of early life are very different, and may well produce an unlikeness upon which Natural Selection may found a division of labour.

No one who tries to trace origins can rest satisfied with Steenstrup's account of alternation of generations. He makes no effort to show how it came about. Instead of considering alternation of generations as a peculiar case of development with metamorphosis, complicated by asexual reproduction,[1] he considers asexual reproduction as a peculiar case of alternation of generations.[2] He ignores all the facts which show that the alternation may have been gradually attained, an omission which is only excusable when we note that his treatise is dated 1842. He asserts dogmatically that there is no transition from metamorphosis to alternation of generations.

It is impossible to think much on this subject without falling into difficulties over the word *generation*. For my own part I believe that such words as *generation*, *individual*, *organ*, *larva*, *adult* cannot be used quite consistently in dealing with a long series of animals whose life-histories vary gradually and without end. Ordinary language, which was devised to meet the familiar and comparatively simple course of development of man and the domestic animals, is not always appropriate to lower forms, with complex and unusual histories. If we are resolved at all hazards to make our language precise and uniform, we either fall into contradictions, or else use words in unnatural senses.

Certain recent discussions render it necessary to point out that there can be no alternation of generations without increase by budding. If a single larva produces a single sexual animal, as when a pluteus changes to an Echinus, there is development with transformation, but not alternation of generations.

It is, I think, of importance to be able to resolve so peculiar a phenomenon as alternation of generations into processes which are known to occur separately, and which may have arisen imperceptibly, becoming gradually emphasised by the steady action of the conditions of life. Every startling novelty that can thus be explained extends the application of that principle which underlies the theory of Natural Selection—I mean the principle that a small force acting steadily through a long time may produce changes of almost any magnitude.

The Hydrozoa yield innumerable and varied examples of development with transformation and also of budding. They yield also the most admirable examples of division of labour. We have Hydrozoan colonies, such as a budding Hydra, in which all the members are pretty much alike, but we soon advance to differentiation of the feeding and the reproductive members. In the Siphonophora the colony becomes pelagic, and floats at the surface of the sea. Then the medusæ no longer break off and swim away, but are harnessed to the colony; and drag it along. The colony may contain feeding polyps, which procure and digest food for the rest; swimming bells, which are attached medusæ; perhaps a float, which is a peculiar kind of swimming bell; defensive polyps (which may be either batteries of nettling cells or covering organs); and reproductive individuals. As the individuals become subordinated to the colony, and lose essential parts of the primitive structure, they pass insensibly into organs.

The life-histories of Invertebrates abound in complications and paradoxes. Thus Eucharis, one of the Ctenophors, becomes sexually mature as a larva, but only in warm weather. This happens just after hatching, when the animal is of microscopic size. Then the sexual organs degenerate, the larva, which has already reproduced its kind, grows to full size, undergoes transformation, and at length becomes sexually mature a second time.[3] There is often a striking difference between the early stages of animals which are closely related, or a strong adaptive resemblance between animals which are of very remote blood-relationship. In the Hydrozoa similar polyps may produce very different medusæ, and dissimilar polyps medusæ that can hardly be distinguished. There are insects so like in their adult state that they can only be distinguished by minute characters, such as the form and arrangement of the hairs on the legs, and yet the

[1] This is a convenient short account of Alternation of Generations, but it will not apply to every case. In Hydra, for instance, there is an ill-defined alternation of generations, but no metamorphosis.
[2] *Cf.* Leuckart, *loc. cit.*, p. 183.
[3] Chun, *Die pelagische Thierwelt*, p. 62 (1887).

larvæ may be conspicuously different.[1] Annelids and Echinoderms yield fresh examples of the same thing. In Lepidoptera and Saw-flies the larvæ are very similar, but the winged insects quite different.[2] New stages may be added in one species, while closely allied species remain unaffected. In Cunina and the Diphyidæ we get combinations which strain the inventive powers of naturalists even to name. Natural Selection seems to act upon the various stages of certain life-histories almost as it acts upon species.

But the history is not always one of growing complexity. Sometimes, for example, a well-established medusa-stage is dropped. First it ceases to free itself, then the tentacles and marginal sense-organs disappear, then the mouth closes. In the fresh-water Cordylophora the medusa is replaced by a stalked sac filled with reproductive elements or embryos. The Lucernariæ present a single stage which seems to be polyp and medusa in one. Hydra has no medusa. It is not always clear whether such Hydrozoa as these are primitive or reduced. Even the hydroid polyp, the central stage in the normal Hydrozoan life-history, may be suppressed, and certain medusa in both of the chief groups develop direct from the egg or planula (Pelagia, Geryonia, Ægina, Oceania). There is no stage common to all Hydrozoa except the egg. The same thing may be said of the Tunicates.

The life-history of many Arthropods is to all appearance quite simple. There emerges from the egg a spider, scorpion, or centipede (in most Chilopoda) which merely grows bigger and bigger till it is adult. But if, as in most Crustacea, the circumstances of the species call for a migratory stage, such a stage will be added. In certain Decapod Crustacea (Penæus, Leucifer) a nauplius and as many as five other stages may intervene before the final or adult stage. Some of these larval stages are common to a great many Crustacea, but none, as we now think, belong to the original phylogeny. If a resting or a winged stage is wanted, it is supplied just as easily; witness the holometabolic insects. Here again, so far as we know, there is nothing absolutely new.[3] The stages which seem new are merely exaggerations for special purposes of sections of the life-history, which were originally marked out by nothing more important than a change of skin and a swelling out of the body. Let us not suppose for a moment that it is a law of insect-development that there should be larva, pupa, and imago, or that it is a law of Crustacean development that there should be six distinct stages between the egg and the adult. Any of these stages may be dropped, if it proves useless—either totally suppressed, or telescoped, so to speak, into the embryonic development. Lost stages are indicated by the embryonic moults of some centipedes and spiders, Limulus, many Crustacea, and Podura. The parthenogenetic reproduction of some immature insects, such as Miastor, shows a tendency to suppress later stages. Perhaps the wingless Thysanura are additional examples, but here, as in the case of Hydra and Lucernaria, we do not certainly know whether they are primitive or reduced. It seems to be easy to add new stages, when circumstances (and especially parasitism) call for them. Meloe, Sitaris, and Epicauta are well-known examples. In some Ephemeridæ the moults, which are potential stages, become very numerous, but as a curious exception to a very general rule, the last moult of all, which is usually so important, may be practically suppressed. The fly of an Ephemera may mate, lay eggs, and die, while still enveloped in its last larval skin.

Among the many cases of what one is inclined to call *rapid* adaptation to circumstances (the chief indications of *rapidity* being the very partial and isolated occurrence of remarkable adaptive characters) are those which Giard[4] has collected and compared, and which he refers to a process called by him Pœcilogony. A number of very different animals[5] produce according to habitat, or season, or some other condition closely related to nutrition, eggs of more than one sort, which differ in the quantity of nourishment which they contain and in the degree of transformation which the issuing larva is destined to undergo. The analogy with the summer and winter eggs of

[1] Some species of Chironomus are referred to.
[2] Baron Osten Sacken (*Berl. Entom. Zeits.*, Bd. xxxvii. p. 465) gives two cases of Diptera, in which "almost similar larvæ produce imagos belonging to different families."
[3] "Nirgends ist Neubildung, sondern nur Umbildung."—Baer.
[4] C. R. 1891, 1892.
[5] *E.g.* Crustacea (Palæmonetes, Alpheus), Insects (*Musca corvina*, some Lepidoptera and Diptera), an Ophiurid (Ophiothrix), a Compound Ascidian (Leptoclinus), &c

Daphnia, &c., cannot escape notice, and Giard connects with all these the pædogenesis of Miastor and Chironomus, and many cases of heterogony. For our immediate purpose it is sufficient to remark that the reproductive processes and the course of development are as liable to vary for motives of expediency as the form of a leg or fin. The supposed constancy (the *necessary* constancy according to some naturalists) of the embryonic stages throughout large groups, would not be hard to break down, if it were to be again asserted. Probably the doctrine is now totally abandoned; it belongs to that phase of zoological knowledge in which Meckel could declare that every higher animal passes in the course of its development through a series of stages which are typified by adult animals of lower grade, and when an extreme partisan, far inferior to Meckel both in experience and caution, could affirm that the human embryo omits no single lower stage.

The tadpole-larva, which is common in lower Vertebrates and their allies, shows the influence of adaptation as strongly as any larva that we know. We may describe the tadpole as a long-tailed Chordate, which breathes by gills and has a suctorial mouth-disc, at least during some part of its existence. It is a cheap form of larva, when reduced to its lowest terms, requiring neither hard skeleton, nor limbs, nor neck, yet it can move fast in water by means of its sculling tail. Such a tadpole appears in many life-histories, and plays many parts. The tadpole is the characteristic Tunicate larva, and in this group commonly ends by losing its tail, and becoming fixed for life. But Salpa, which is motile when adult, has lost its tadpole. Appendicularia has lost the normal adult stage if it ever had one, and its tadpole becomes sexually mature. The same thing seems to have happened to many Amphibia, whose tadpoles acquire legs, become sexually mature, and constitute the normal adult stage. The Lamprey, as Balfour and others have recognised, is another kind of sexually mature tadpole. Thus the tadpole may act as larva to a sea-squirt, fish (Acipenser, Lepidosteus, Amia), or frog; it may also constitute the only remaining stage in the free life-history.

The lower and smaller animals seem to show beyond others the prevalence of adaptive features. They offer visible contrivances of infinite variety, while they are remarkable for the readiness with which new stages are assumed or old ones dropped, and for their Protean changes of forms, which are so bewildering that many Worms, for instance, cannot as yet be placed at all, while many larvæ give no clue to their parentage. These lower and smaller animals show beyond others a tendency to multiply rapidly, and to break away from one another in an early stage. The tendency is so strong in the microscopic Protozoa that it enters into the definition of the group. Fission, budding, alternation of generations, and spore-formation (as in Gregarina) are ultimately due to the same tendency.

Weak animals are almost inevitably driven to scatter, and to make up by their insignificance, their invisibility, and their powers of evasion for the lack of power to resist. It is a great thing to a Hydrozoan colony that if one polyp is bitten off, others remain, that no enemy can possibly devour all the medusæ liberated from one colony, or all the planulæ liberated from one medusa. Low organisation gives very special facilities for extreme division. There are animals and plants which multiply greatly as a consequence of being torn to pieces or chopped small. (Chigoe, some Fungi, &c.)

Small animals are usually short-lived. Many complete their life-history in a few weeks. Those which last for so long as a year are often driven, like annual plants, to adapt every detail of their existence to the changing seasons. The naturalist who explores the surface waters of the sea with a tow-net soon learns that the time of year determines the presence or absence of particular larvæ. It is probably as important to an Aurelia as to a butterfly that it should tide over the storms of winter by means of a sedentary and well-protected stage. Any one who keeps scyphistoma in an aquarium will remark how small it is, how it creeps into crevices or the hollows of dead shells. But when the depth of winter is past, it pushes out its strobila, which in spring liberates ephyræ. These rapidly enlarge, and by August have grown from microscopic discs to jelly-fishes a foot across.

The intelligence of many small animals is very low. They go on doing the thing that they have been used to do, the thing that has commended itself to the experience of many generations. They are governed by routine, by that inherited and unconscious power of response to external stimulus, which we call instinct. But there are some notable exceptions. Of all small animals, insects seem to show the greatest flexibility of intelligence.

There is one large group of animals which is in striking contrast to nearly all the rest. Vertebrates, and especially the higher Vertebrates, are usually big and strong. They rely upon skill, courage, or some other product of high organisations, rather than upon numbers and fertility. Vertebrates swallow many other animals, together with their living parasites, but are rarely swallowed alive or fresh by Invertebrates. This fact of nature has led to many consequences, among others to this, that many parasites which pass their earlier stages in the bodies of Invertebrates only attain sexual maturity in a Vertebrate host. The complexity of the structure of a Vertebrate precludes the possibility of multiplication by breaking-up or budding, and they multiply only by egg-laying or strictly analogous processes. The higher Vertebrates live so long that the accidents of a particular year or a particular season are not of vital importance. Hence seasonal transformation is almost unknown; the quadruped or bird may choose the warm months for rearing the family, or celebrate the pairing season by getting a new suit of feathers, or grow a thicker coat against the cold of winter, but that is all. No Vertebrates perish regularly at the approach of winter, leaving only batches of eggs to renew the species in spring, nor is their structure profoundly modified by the events of the calendar (the frog is a partial exception). One minor cause of transformation, which affects the life-history of many polyps, worms and insects, is thus removed. Vertebrates often take care of their young, and the higher Vertebrates bring forth few at a time. For this reason among others they rarely afford examples of free larvæ. Such Vertebrate larvæ as we do find conform to the Vertebrate type. It is often impossible to predict what adult will develop from an Invertebrate larva, but no one could hesitate to rank an Ammocoetes, a Leptocephalus, or a tadpole among the Vertebrates.

It accords with this strength and mastery that Vertebrates, and especially the higher Vertebrates, should be more stable, more conservative, less experimental than other animals. They retain ancient structures long after they have ceased to be useful. The gill-clefts, gill-arches, and branchial circulation are good examples. Though not functional in Sauropsida and Mammalia, they never fail to appear in the course of the development. Yet the Sauropsida and the Mammalia are positively known to go back to the earliest secondary and late palæozoic times. Ever since the beginning of the secondary period at least, every reptile, bird, and mammal has continued to pass through a stage which seems obviously piscine, and of which no plausible explanation has ever been offered, except that remote progenitors of these animals were fishes. Could not Natural Selection, one is tempted to ask, have straightened the course of development during lapses of time so vast, and have found out less roundabout ways of shaping the tongue-bone and the ossicles of the ear? Either it costs nothing at all to pursue the old route, or it costs nothing which a higher Vertebrate will ever miss. The second alternative seems to me the more likely. The Sauropsida and Mammalia, in comparison with other animals, are particularly well off, and like wealthy housekeepers, they do not care what becomes of the scraps. It is, I fancy, different with many fishes, which show, by their numerous eggs, the occasional presence of peculiar immature stages, and some other slight hints, that their life is a hard one.

The presence in the developing reptile, bird, or mammal of piscine structure which are no longer useful has been ascribed to a principle called Recapitulation, and Haeckel lays it down as a fundamental biogenetical law that the development of the individual is an abbreviated recapitulation of the development of the race. If I had time to discuss the Recapitulation Theory, I should begin by granting much that the Recapitulationist demands—for instance, that certain facts in the development of animals have an historical significance, and cannot be explained by mere adaptation to present circumstances; further, that adaptations tend to be inherited at corresponding phases both in the ontogeny and the phylogeny. I am on my guard when he talks of *laws*, for the term is misleading, and ascribes to what is a mere general statement of observed facts the force of a command. The so-called laws of nature (a phrase to be avoided) may indeed enable us to predict what will happen in a new case, but only when the conditions are uniform and simple—a thing which is common in Physics, but very rarely in Biology. I diverge from him when he says that "each animal is compelled to discover its parentage in its own development," that "every animal in its own development repeats this history, and climbs up its own genealogical tree." When he declares that "the proof of the theory depends chiefly on its universal applicability to all animals, whether high or low in the zoological scale, and to all their parts and organs,"[1] I feel persuaded that, if this is really so, the Recapitulation Theory will never be proved at all. The development, so far as it has yet been traced, of a Hydra, Peripatus, Beetle, Pond-mussel, Squid, Amphioxus, Chick or Mammal tells us very little indeed of the history of the races to which they belong. Development tells us something, I admit, and that something is welcome, but it gives no answer at all to most of the questions that we put. The development of a Mammal, for instance, brings to light what I take to be clear proof of a piscine stage; but the stage or stages immediately previous can only be vaguely described as Vertebrate, and when we go back further still, all resemblance to particular adult animals is lost. The best facts of the Recapitulationist are striking and valuable, but they are much rarer than the thorough-going Recapitulationist admits; he has picked out all the big strawberries, and put them at the top of the basket. I admit no sort of necessity for the recapitulation of the events of the phylogeny in the development of the individual. Whenever any biologist brings the word *must* into his statement of the operations of living nature, I look out to see whether he will not shortly fall into trouble.

This hasty review of animal transformations reminds me how great is the part of adaptation in nature. To many naturalists the study of adaptations is the popular and superficial side of things; that which they take to be truly scientific is some kind of index-making. But we should recognise that comparatively modern adaptations may be of vital importance to the species, and particularly luminous to the student because at times they show us nature at work.

I am accustomed to refer such adaptations to the process of Natural Selection, though if any one claimed to explain them by another process, I should, for present purposes, cheerfully adopt a more neutral phrase. There are, I believe, no limits to be assigned to the action of Natural Selection upon living plants and animals. Natural Selection can act upon the egg, the embryo, the larva, and the resting pupa, as well as upon the adult capable of propagation. It can even influence the race through individuals which are not in the line of descent at all, such as adults past bearing or the neuters of a colony. The distinction between historical and adaptive, palingenetic and cœnogenetic, is relative only, a difference not of kind but of degree. All features are adaptive, but they may be adapted to a past rather than to a present state of things; they may be ancient, and deeply impressed upon the organisation of the class.

In Biology facts without thought are nothing; thought without facts is nothing; thought applied to concrete facts may come to something when time has sorted out what is true from what is merely plausible. The Reports of this Association will be preserved here and there in great libraries till a date when the biological speculations of 1897 are as extinct as the Ptolemaic Astronomy. If many years hence some one should turn over the old volumes, and light upon this long-forgotten address, I hope that he will give me credit for having seen what was coming. Except where the urgent need of brevity has for the moment been too much for scientific caution, I trust that he will find nothing that is dogmatic or over-confident in my remarks.

SECTION G.

MECHANICAL SCIENCE.

OPENING ADDRESS BY G. F. DEACON, M.INST.C.E., PRESIDENT OF THE SECTION.

IN this ever-memorable year of the Victorian Age, it is not unnatural that any one called to fill the chair I occupy to-day should experience a sense of oppression, when contemplating the fruits of mechanical science during the last sixty years, and the tremendous vista, fading in the distance to a dream, of the fruits it is destined to produce before such another period shall have passed away.

There would be no possibility, in the time at my disposal, even if I were qualified to attempt it, of adequately reviewing the past; and however fascinating the thought may be, it would ill become my office to venture far along the vista before us, lest a too airy imagination should break the bonds of that knowledge and that truth to which she must ever remain, in our rightful speculations, a helpful, if not always an obedient, handmaiden.

In the year 1831, two places, the one ancient and memorable, the other young, but destined to become memorable, bore the name of York. At the first of these, amid relics of ancient Rome and lasting memorials of the better phases of Britain's mediæval history, were met together in that year the earliest members of the British Association. And as the sun at noon-day shone on that ancient York, it rose upon the other York—a little town, scarcely more than a village, of 1700 people, fast springing from a plain on the shores of Ontario, where the wigwam of the Chippewa had lately been; and between the two two lay the Atlantic and a distance of 4000 miles.

Sixty-six years later, the British Association meets in that other York, distinguished under the name of Toronto, and grown into a noble city. Painfully, in stage coaches, must many of the founders of this Association have travelled to that ancient York; peacefully and amid all comfort and luxury have we from the mother country reached, at her invitation, this great city—chiefest, in its people, its commerce, and its University, of the cities of Western Canada.

Neither at the meeting in York of 1831, nor elsewhere, until many years later, was there any expectation of the possibility of these things. Six years later, about the beginning of that glorious reign of which the sixty-first year is now passing—although two or three vessels had already crossed the Atlantic under steam, it was still seriously doubted whether, without the aid of a Government subsidy of considerable amount, a line of steamers, even for the New York service, could be permanently maintained. It was not, indeed, until 1838 that the *Great Western* inaugurated the attempt on a commercial basis, and she performed in fifteen days the voyage which is now regularly performed with complete commercial success in five.

Would not the suggestion of such a change, of such a spanning of great distances, of such a consequent growth of prosperity and of culture, within the reign of a princess then approaching womanhood, have been received as the wildest of forecasts by the British Association of 1831?

Yet this is but one of a multitude of results, no less startling, which the same agencies have brought about. We are now holding the second meeting of the Association in Canada, and at the first such meeting, held thirteen years ago in Montreal, some hundreds of miles nearer home, Sir Frederick Bramwell told you from this chair, in his own inimitable way, the causes of so great a change, and he pointed out to you, as I venture to point out again, that the visible instruments of that change have been forged by the men who are, or were, or ought to be, the members of Section G. To such encouragement as Section G has given is largely due the progress and triumph of applied mechanics as the natural outcome of theoretical investigation and physical research. Finally, and with no reserve in the minds of reasonable men, the old fallacy of a discord between theory and practice has been swept away. For centuries that fallacy held apart, as it were, the oxygen and the nitrogen of that atmosphere in which alone the new life could exist. It limited the philosopher who examined the laws of nature almost entirely to the study of phenomena external to the earth on which he dwelt, and it stamped the practical man as a lower being, the possessor of certain necessary knowledge, having no relation to the studies of the schoolmen, and which it would be beneath their dignity to pursue. And notwithstanding the great names which have stood out in opposition to these views, the popular idea of discord between theory and practice took long to die, and only within the Victorian Age has the complete truth been generally recognised, that if one fails to account for the result of any physical combination, the cause is to be found not in any discord with theory, but in the fact that the observer has failed to discover the whole of the theory.

We English-speaking people, alone, I believe, among civilised nations, use this word, *theory*, with unpardonable looseness—as almost synonymous in effect with *hypothesis*, and the result is fruitful of error. Until the truth of any hypothesis

[1] The quotations are from the late Prof. A. Milnes Marshall's Address to Section D, British Association Report, 1890, which states the Recapitulationist case with great knowledge and skill.

is placed beyond all manner of doubt it is not, and should never be called, the theory.

Within these walls, the *genius loci* impels me to thoughts which have not often entered into discussions of Section G; and, perhaps, if this address were to be discussed, I should choose subjects and premises, the proof of which, to the satisfaction of others than myself, it would probably be less difficult to maintain. In this University of Toronto under whose *ægis* all that was best in the older schools of thought is cultivated by the side of those practical applications of science which in bygone days were distinguished as the unworthy uses of philosophy, one's thoughts insensibly turn to the marvellous change in the opportunities afforded for acquiring a knowledge of applied science—for becoming, in short, an engineer.

It is not proposed to discuss the progress and prosperity which mechanical science has brought about in the Victorian Era, much less that which the succeeding years will yield; but I venture to think that a proper subject for consideration from this chair, if not for discussion in this Section, is to be found in any unnecessary waste of energy which may occur in the process of mental development of the men who are to succeed us in the great work to which we devote our lives. Obviously it is to the interests of our calling, and consequently of the nation at large, that such waste should be reduced to a minimum, and therefore I make no apology for mentioning certain points in which its presence is particularly striking. There may be waste of potential, as well as of actual energy, and if we fail to expend energy on certain subjects because our time is occupied with others which are less useful, it is waste of energy only differing in degree from its expenditure on useless subjects. There is assuredly no lack of potential energy in the coming race. In spite of any training, whether well or ill directed, a large proportion will become actual and useful energy; but guidance and direction being given, the mode of that guidance and direction should be the one best calculated to secure the highest possible proportion of useful effect.

If we look back at the greatest names among the engineers and inventors of the latter part of the eighteenth century and the first half of this, we find that the majority were brought up in pursuits quite distinct from the work of their after lives, and by which they have become so familiar to us. There were scarcely any means whatever, beyond the original thought and dogged perseverance of the worker, by which those men could attain the knowledge they used with such effect. Men of no less exceptional parts are among us now, but the whole environment of their early work has changed. We have given to the exceptional man a starting-point of knowledge which, wisely used, lifts him as high above our heads as of old, but we have given to the average man a comparatively easy means of attaining the same knowledge. We cannot ensure the wise use of that knowledge, but we can at least endeavour to impart it in such a manner that the sense of right proportion shall be acquired and maintained. We have made it more difficult to distinguish between the exceptional and the commonplace—between the gold and the silver, if not between the silver and the brass; let us be careful, so far as early guidance can control it, that the knowledge imparted to the average mind gives to that mind a fair start concerning the relations, undivided and indivisible, between true theory and sound practice.

Having myself passed as an ordinary apprentice through workshops of mechanical engineering in the old days when working hours were longer than they now are—from six in the morning till six in the evening, and that, too, on the banks of the Clyde, where no special indulgence was given to what was sometimes called the "gentleman apprentice," and feeling convinced, as I still do, of the immense and permanent advantage derived from that experience, I shall not be judged to underrate its value in the case of others who have yet to choose the details of the career by which they expect to gain a place in the profession or business of an engineer.

On the other hand, as a student thirty-four years ago under the late Prof. Macquorn Rankine and the present Lord Kelvin, I shall not be prone to under-estimate the advantages of academical training in its proper application to the profession to which I am proud to belong.

In the pursuit of that profession it has fallen to my lot to observe the training as engineers of many younger men—men of variously constituted minds, but one and all bent on learning some portion of "the art of directing the great sources of power in nature for the use and convenience of man," words wisely chosen, sixty-nine years ago, and set out as the object of the profession in the Royal Charter of the Institution of Civil Engineers. It is a noble object, this *direction of the great forces of nature for the use and convenience of man*; it is an ambitious object, and one which I venture to think demands for its right performance the best energies of well-balanced minds working upon a store of knowledge which nothing but years of untiring study and observation can give. Yet there is no hesitation shown to enter the lists. The number of candidates is appalling. In the old country, at least, there certainly is not work for all, but when one points this out, anxious parents only reply that the difficulty is as great in connection with any other profession. Whether this be so or not I cannot judge, but I am persuaded that of those who do enter the business or profession of the engineer, the enormous majority are not born engineers, and cannot, in the nature of things, hope for success unless they take advantage of the best facilities open to them—the *best facilities*; here is the difficulty: from the multitude of facilities how are we to choose?

Do not suppose that I think the training of the born engineer should not be controlled. He will stand head and shoulders above the rest of us whatever we may do with him; but in order that his exceptional parts may not wreck him as an engineer, and in order that his energies may be rightly directed at the start, he, too, should have the advantages of that systematic training which to his less gifted brethren is becoming more and more absolutely essential to success.

At the time I began practice the large majority of young engineers were left entirely to their own devices so far as the attainment of any scientific knowledge was concerned. As pupils or apprentices, articled or not, they entered an engineer's works or office; for a certain number of years they had the run of the place and some encouragement if they worked well, but it could not, in the nature of things, amount to much more. This was a very necessary, perhaps the most necessary, element of their training; but except to the few who were so constituted that with little or no guidance they could supplement their practical knowledge with the study of principles elsewhere, it was entirely ineffectual in the production of that well-balanced attitude of mind which any person who properly assumes the name of an engineer must hold towards every engineering problem, great or small, which he is called upon to solve. And so strongly have I felt this, that in the earlier days, when there were fewer schools of practical science, and when their utility was little understood, I required, wherever the matter was under my control, the insertion into the articles of apprenticeship of a clause by which, at some inconvenience to the office, the pupil was required to attend two sessions at the science classes of Glasgow University, or at some other approved school of practical science; and without this condition I declined to take the responsibility attaching to the introduction into the profession of men who, in their earlier careers, from no fault of their own, had not even acquired a knowledge of what there was to learn, much less of how to learn it.

More recently this course has generally become unnecessary; for in Westminster, at least, the young engineer rarely enters an office until he has acquired some knowledge of what he has to learn. He enters, in short, at a much more advanced age than formerly. When it is essential that he should be earning something soon after he comes of age, anything like a complete training is an impossibility; his work ceases to be general, and his practice is more or less confined in a much narrower sphere than need be the case if the pursuit of further knowledge continues to be his chief duty.

But whatever course his circumstances may permit him to adopt, the difficulty of gaining the required knowledge in the time available is a serious one. This is not the place to inquire whether public school education in the mother country is, or is not, the best for the general purposes of after life, or to discuss what improvements may be made in it; and of higher education in Canada I unfortunately know little or nothing. Personally I admit the possibility of improvement in the English system, and slowly but surely improvement is creeping in, as such changes rightly find their way into institutions which have done so much for Englishmen. In this particular I lean to the conservative side, and whatever our individual views may be concerning the time spent on the study of Latin and Greek, we should all probably agree that the school education of an engineer should be as thorough and liberal as for any other profession. But for the sake of a technical training to follow, this school education is often unduly curtailed, to the great after-grief, in very many cases, of the successful engineer, and not infrequently also of the less successful engineer who, in some phases of his professional career, has been only too keenly alive to the self-reproach and sense of inferiority which want of thoroughness or of time, or of both, at school has brought upon him.

But at some time the boy must leave school. Let us hope that he does not aspire "to control the great forces of nature"; but if he does we must make the best we can of him.

It is not desirable, at least so it appears to me, that even at this stage his training should be specialised in view of the particular branch of the profession or business he is likely to follow. The fundamental principles of any branch of mechanical engineering are broadly the fundamental principles of any branch of the profession. I hesitate to speak of civil engineering as if it were a separate branch, instead of being, as it really is, the generic name of the profession; but the training demanded for the various branches of civil engineering in its narrower sense is precisely the same as that required in its earlier stages for mechanical engineering pure and simple.

I shall make no attempt to review the large number of excellent courses which are now available for the teaching of applied science in relation to engineering. Experience of the results as judged by the students who have come directly under my notice, and examination of many calendars, has aroused various thoughts concerning them, and this thought is perhaps uppermost: *are we not in some cases attempting at too early a stage the teaching of subjects instead of principles?* attempting at too early a stage the teaching of subjects instead of principles? Complete subjects, I mean, including the practical working of details which will become the regular study of the student in the office or works of an engineer. It certainly seems to me to be so. I do not say that subject training of this kind at college may not be useful; but we have to consider whether it does not, for the sake of some little anticipation of his office work, divert the attention of the student from the better mastery of those principles which it is so essential for him to grasp at the earliest possible time, and which do not limit his choice in the battle of life to any branch whatever of the profession or business of an engineer, but which, on the contrary, qualify him better to pursue with success whatever branches his inclination or his opportunities or his means may suggest. Not one in a hundred of us can hope to emulate the careers of exceptional men in our profession, but it is sometimes useful to observe those careers, and whenever we do so we find the very reverse of specialisation. The minds of such men are impregnated with the fundamental principles which we may call the common law of our art; it has happened that their practice has been large in certain branches, and small or wanting in certain others; but in any it would have been equally successful. Of no class of men can it be said with greater truth than of engineers that their standard should be sound knowledge of the principles of many things and of the practice of a few.

There is some danger in the usual limitation of compulsory subjects in examinations for certificates and degrees. When an examination has to be passed subjects not made compulsory are too often entirely neglected, however important to the engineer they may be. A little learning is certainly not a dangerous thing if within its limits it is sound, and every engineer will in after life be grateful to those who in his student days insisted upon his acquiring some knowledge of the principles of such subjects as electricity and chemistry. At present it too often happens that, unless an engineering student is predestined to practise electrical work or some chemical industry, he begins life as an engineer with no knowledge of the principles of either the one or the other, and chiefly as a result of their neglect for the sake of certain subjects made compulsory for the test he has had to pass, which subjects too occasionally include the highly specialised favourites of a particular professor or verge too completely on perfected details which, I venture to think, cannot be rightly mastered in schools. It is natural and right that each professor of a principal subject should seek to make the best, from his own particular standpoint, of every student who attends his lectures or his laboratories; and the professor of a compulsory subject cannot be expected to encourage the inclusion, in a course already overcrowded, of secondary or collateral subjects which are dealt with by other professors; while, on the other hand, the professors of secondary subjects, such as electricity or chemistry, not unnaturally value chiefly the students who make those subjects their principal work.

For these reasons it appears to me that a certain very moderate standard in all such subjects should be made compulsory if a certificate of proficiency, whether by degree or otherwise, is to be given in engineering or even in physical science.

In the teaching of mathematics within the Victorian Age a considerable change has taken place, and I plead for still a little more change in the same direction where the training of the engineer is concerned. Mathematics, as taught in our public schools—let us say for the Cambridge University Tripos—may be all that is claimed for it as a mode of mental culture; but of kindred mental culture the engineer must necessarily have more than most men, and much might therefore be omitted which, to him at least, has only an abstract value, to the great advantage of his mastery over those branches which at once train his mind and give point and direct utility to his solutions.

In America I understand that a college course of engineering generally includes workshop practice designed to supersede the old system of apprenticeship to a mechanical engineer. This fact and other important differences between the English and American practice have only lately come to my knowledge, and before they did so the substance of this address had been written. It might, in some particulars, require modification as applied to Canada, but it remains the result of my observations concerning the conditions of engineering education which obtain in the mother country.

A few words now in relation to that physical and mental training gained laboriously, and somewhat wastefully as I think, at the joiner's bench, in the fitting and turning shops, the foundry and the forge, during the old course of mechanical engineering apprenticeship. I am convinced that the kind of knowledge which comes of thoughtful chipping and filing and turning and forging, though only applied to a few of the materials with which in after life the engineer has to deal, are quite as important as tables of density and strength to his future sense of rightness in constructive design. The use of such work is not merely to teach one the parts and combinations of any particular machine; in a still higher degree it is the insensible mastery of a much more subtle knowledge or mental power, the application of the senses of sight and touch and force, it may be of other senses also, to the determination of the nature of things. (I am not going to apologise for referring to the *sense of force*. The vexed question of its separate existence appears to me to have been settled fourteen years ago by Lord Kelvin in his address at Birmingham on "the six gateways of knowledge," and I may well leave it where he left it.) I should altogether fail to describe adequately what this mastery means. It appears to me to be inscrutable. The value and nature of the power can only be appreciated by those who have experienced it, and who have felt its defect in those of their assistants or in others who do not possess it.

But the great workshop training has still further advantages. The apprentice is surrounded by skilled workers from whose example, if he is wise, he learns a great deal; and apart from this it is no small profit to have rubbed against the British workman, to have discovered what manner of man he is, and to comprehend how little the world knows of his best parts. The whole time spent in large engineering works cannot, however, be equally beneficial; the apprentice must take the work as it comes; the most interesting or instructive portions cannot be reserved for him, and he often feels that some of his time is being well-nigh wasted.

A few years ago I should not have thought it practicable usefully to substitute for such a course anything that could be undertaken in a student's workshop, however organised; but the impossibility, in many cases, of including such experience without neglecting something equally important has led me to view with satisfaction the introduction of workshop training into certain schools of applied science in England. Such a change cannot, of course, carry with it all the advantages of experience in the great workshop and of contact with its workers, but those advantages which it does retain may be secured in a shorter time where there is no commercial interest to be served.

In Canada and the United States, as I have already said, the principle of the student's workshop has been carried considerably further. Compared with the old country, I believe the number of young assistant engineers who in proportion to the number of their chiefs can find employment in America is much greater, and that it would be practically impossible for the British system of pupilage to be generally employed. Here, therefore, the whole college training of an engineer is designed to fit him for immediate employment in some specific branch of the profession, and up to this point his training is, necessarily no doubt, more academic than in England, where the application of the principles he has acquired at college is still generally left for the office or works of the engineer. With this difference I am not at present concerned, but I desire to reiterate what I have already said to the effect that where, as in England, the student of engineering has the opportunity of continuing his training in the office or works, it is better that his limited college course should cover all that is possible of the principles of those sciences which may prove useful or necessary to him in after life, rather than that any of them should be omitted for the sake of anticipating the practical application of certain others.

The compulsory inclusion of the principles of all such subjects as chemistry, electricity, geology, and many others, in science courses intended for a future engineer is desirable not only because a fundamental knowledge of them leaves open a very much wider field from which the engineer may, as opportunity offers, increase his knowledge and practice in the future, but because many of such subjects are inseparable from an intelligent understanding of almost any great engineering work. "Nothing so difficult as a beginning" may be a proverb of rather too far-reaching a nature, but it contains the suggestion of a great truth, increasing in weight as we grow older, and the beginnings of such collateral sciences should therefore find a place in every engineering student's store of early knowledge.

But after all, when these things have been done in the best manner—when the scientific and practical training of the engineering student has been all that can be desired, it is a matter of general experience among engineers who have closely watched the rising generation, that the most successful men in after life are not produced exclusively from the ranks of those whose college course has been most successful. No doubt such men have on the average been nearer the top than the bottom, but it is an undoubted fact that when we class them according to their earlier successes or failures, we find the most remarkable disparities. We find many who in academic days gave but little promise, and we miss large numbers who promised great things. These facts are not confined to the profession of the engineer, but they seem to me to be accentuated in that profession. We shall no doubt be right in attributing the disparity to differences of mental temperament and of opportunity; but does it follow that there are no faculties which may be cultivated to reduce the effect of such differences? I venture to think there are. I will instance only one, but perhaps the most important of such faculties, and which in my experience among young engineers is exceptionally rare. I refer to the power of marshalling facts, and so thinking, or speaking, or writing of them that each maintains its due significance and value.

In the minds of many young engineers a mathematical training undoubtedly has the effect of making it extremely difficult to avoid spending an amount of time upon some issues out of all proportion to their importance; while other issues which do not readily lend themselves to mathematical treatment, but which are many times more important, are taken for granted upon utterly insufficient data, and chiefly because they cannot be treated by any process of calculation. I believe that nothing but well-directed observation and long experience can enable one to assign to each part of a large engineering problem its due importance; but much may be done in early training also, and I think ought to be done, to lead the mind in broader lines, to accustom it to look all round the problem, and to control the imagination or the natural predilection for one phase from disguising the real importance of others. In the practical design and execution of important works the man will sooner or later be recognised who has the power so to formulate his knowledge, and on the same principles has succeeded in so marshalling and expressing his thoughts, as to convey to those by whom he is employed just so much as may be necessary and proper for their use.

Such considerations are not, it is true, a branch of mechanical science, but being essentially important to the attainment of maximum usefulness in the application of any science to the various branches of engineering which are the chief ends and aims of mechanical science, they are, I think, worthy of mention from this chair.

In proportion as the engineer possesses and exercises such powers he will avoid those innumerable pitfalls to which imperfectly instructed ingenuity is so particularly liable, and to which the Patent Office is so sad a witness; and in the same proportion must always be the useful outcome of the great schools of science which have become so striking a feature of the later Victorian Age.

In relation to the results of applied science, I have spoken only of the steamship; add the telegraph, and I think we have the most important tools by which the present conditions of modern civilisation have been rendered possible. And more than this, I think we have, in the lessening of space, and the facility for intercourse they give, the chief secret of that marvellous development of the empire which this year has so pleasantly and so memorably signalised. Is "Our Lady of" the Sunshine and "the Snows" no nearer to the mother land than sixty years ago? Are the Australias—New Zealand—no nearer to both? Assuredly they are. Would British Africa, would the Indian Empire have been possible to Britain on the principles and the methods of Imperial Rome? Unquestionably not. Then let me say again that I claim for the objects and the work of Section G a magnificent record, an abiding power for the peace of the world, and for the unity and prosperity of the great empire to which we belong.

THE AMERICAN ASSOCIATION.

THE meeting of the American Association for the Advancement of Science, held at Detroit August 9-13, though the smallest since 1879, was, as many small meetings are, one of the most interesting and important.

Much disappointment was felt at the absence of President Wolcott Gibbs, owing to the condition of his health and his advanced age, which forbade him making so long a journey. In his absence Prof. Wm. J. McGee occupied the chair.

A feature of the meeting was the forecast of a jubilee celebration at Boston next year, for which Prof. Putnam, Secretary of the Association, has secured a wealth of invitations from all governmental, educational, and scientific organisations situated at Boston; and the election of Prof. Putnam himself, after a quarter of a century of service, to preside at the great Boston meeting, which promises to be a very great scientific gathering.

A memorial address on the life and work of the late president of the Association, Prof. Edward D. Cope, was delivered by Prof. Theodore Gill, and is printed in full in *Science*. The address concluded with the following reflections upon the place which Cope must be assigned in the history of science:—

"Among those that have cultivated the same branches of science that he did—the study of the recent as well as the extinct Vertebrates—three naturalists have acquired unusual celebrity. Those are Cuvier, Owen, and Huxley.

"Cuvier excelled all of his time in the extent of his knowledge of the anatomical structure of animals and appreciation of morphological details, and first systematically applied them to, and combined them with, the remains of extinct Vertebrates, especially the mammals and reptiles. He was the real founder of Vertebrate palæontology.

"Owen, a disciple of Cuvier, followed in his footsteps, and, with not unequal skill in reconstruction and with command of ampler materials, built largely on the structure that Cuvier had begun.

"Huxley covered as wide a field as Cuvier and Owen, and likewise combined knowledge of the details of structure of the recent forms with acquaintance with the ancient ones. His actual investigations were, however, less in amount than those of either of his predecessors. He excelled in logical and forcible presentation of facts.

"Cope covered a field as extensive as any of the three. His knowledge of structural details of all the classes of Vertebrates was probably more symmetrical than that of any of those with whom he is compared; his command of material was greater than that of any of the others; his industry was equal to Owen's; in the clearness of his conceptions he was equalled by Huxley alone; in the skill with which he weighed discovered facts, in the aptness of his presentation of those facts, and in the lucid methods by which the labour of the student was saved and the conception of the numerous propositions faciliated he was unequalled. His logical ability may have been less than that of Huxley and, possibly of Cuvier. He has been much blamed on account of the constant changes of his views and because he was inconsistent. Unquestionably he did change his views very often. Doubtless some of those changes were