

From *The Canadian Naturalist*, 1873.

ANNUAL ADDRESS OF THE PRESIDENT OF THE
NATURAL HISTORY SOCIETY OF MONTREAL,
PRINCIPAL DAWSON, L.L.D., F.R.S., *May*, 1872.

As the Society has done me the honor to elect me twice in succession to the office of President, and as my address of last year was occupied almost entirely with local details, I may be permitted on the present occasion to direct your attention in the first place to some general topics of scientific interest, and merely to notice our own more special work in the end of this address. From the many subjects to which your attention and that of kindred Societies has been called in the past year, I may select the following as deserving our attention :—(1) The present aspect of inquiries as to the introduction of genera and species in geological time. (2) The growth of our knowledge of the Primordial and Laurentian rocks and their fossils. (3) The questions relating to the so-called Glacial Period.

There can be no doubt that the theory of evolution, more especially that phase of it which is advocated by Darwin, has greatly extended its influence, especially among young English and American naturalists, within the few past years. We now constantly see reference made to these theories, as if they were established principles, applicable without question to the explanation of observed facts, while classifications notoriously based on these views, and in themselves untrue to nature, have gained

currency in popular articles and even in text-books. In this way young people are being trained to be evolutionists without being aware of it, and will come to regard nature wholly through this medium. So strong is this tendency, more especially in England, that there is reason to fear that natural history will be prostituted to the service of a shallow philosophy, and that our old Baconian mode of viewing nature will be quite reversed, so that instead of studying facts in order to arrive at general principles, we shall return to the mediæval plan of setting up dogmas based on authority only, or on metaphysical considerations of the most flimsy character, and forcibly twisting nature into conformity with their requirements. Thus "advanced" views in science lend themselves to the destruction of science, and to a return to semi-barbarism.

In these circumstances, the only resource of the true naturalist is an appeal to the careful study of groups of animals and plants in their succession in geological time. I have, myself, endeavoured to apply this test in my recent report on the Devonian and Silurian flora of Canada, and have shown that the succession of Devonian and Carboniferous plants does not seem explicable on the theory of derivation. Still more recently, in a memoir on the Post-pliocene deposits of Canada, now in course of publication in the *Canadian Naturalist*, I have by a close and detailed comparison of the numerous species of shells found embedded in our clays and gravels, with those living in the Gulf of St. Lawrence and on the coasts of Labrador and Greenland, shown, that it is impossible to suppose that any changes of the nature of evolution were in progress; but on the contrary, that all these species have remained the same, even in their varietal changes, from the post-pliocene period until now. Thus the inference is that these species must have been introduced in some abrupt manner, and that their variations have been within narrow limits and not progressive. This is the more remarkable, since great changes of level and of climate have occurred, and many species have been obliged to change their geographical distribution, but have not been forced to vary more widely than in the Post-pliocene period itself.

Facts of this kind will attract little attention in comparison with the bold and attractive speculations of men who can launch their opinions from the vantage ground of London journals; but their gradual accumulation must some day sweep away the fabric

of evolution, and restore our English science to the domain of common sense and sound induction. Fortunately also, there are workers in this field beyond the limits of the English-speaking world. As an eminent example, we may refer to Joachim Barrande, the illustrious palæontologist of Bohemia, and the greatest authority on the wonderful fauna of his own primordial rocks. In his recent memoir on those ancient and curious crustaceans, the Trilobites, published in advance of the supplement to vol. 1st of the Silurian system of Bohemia, he deals a most damaging blow at the theory of evolution, showing conclusively that no such progressive development is reconcileable with the facts presented by the primordial fauna. The Trilobites are very well adapted to such an investigation. They constitute a well marked group of animals trenchantly separated from all others. They extend through the whole enormous length of the Palæozoic period, and are represented by numerous genera and species. They ceased altogether at an early period of the earth's geological history, so that their account with nature has been closed, and we are in a condition to sum it up and strike the balance of profit and loss. Barrande, in an elaborate essay of 282 pages, brings to bear on the history of these creatures, his whole vast stores of information, in a manner most conclusive in its refutation of theories of progressive development.

It would be impossible here to give an adequate summary of his facts and reasoning. A mere example must suffice. In the earlier part of the memoir he takes up the modifications of the head, the thorax and the pygidium or tail piece of the Trilobites in geological time, showing that numerous and remarkable as these modifications are, in structure, in form and in ornamentation, no law of development can be traced in them. For example, in the number of segments or joints of the thorax, we find some Trilobites with only one to four segments, others with as many as fourteen to twenty-six, while a great many species have medium or intervening numbers. Now in the early primordial fauna the prevalent Trilobites are at the extremes, some with very few segments, as *Agnostus*, others with very many, as *Paradoxides*. The genera with the medium segments are more characteristic of the later faunas. There is thus no progression. If the evolutionist holds that the few-jointed forms are embryonic or more like to the young of the others, then on his theory they should have precedence, but they are contemporary with forms

having the greatest number of joints, and Barrande shows that these last cannot be held to be less perfect than those with the medium numbers. Further, as Barrande well shows, on the principle of survival of the fittest, the species with the medium number of joints are best fitted for the struggle of existence. But in that case the primordial Trilobites made a great mistake in passing at once from the few to the many segmented stage or *vice-versa*, and omitting the really profitable condition which lay between. In subsequent times they were thus obliged to undergo a retrograde evolution, in order to repair the error caused by the want of foresight or precipitation of their earlier days. But like other cases of late repentance, theirs seems not to have quite repaired the evils incurred; for it was after they had fully attained the golden mean that they failed in the struggle, and finally became extinct. "Thus the infallibility which these theories attribute to all the acts of matter organizing itself, is gravely compromised," and this attribute would appear not to reside in the trilobed tail, any more than according to some in the triple crown.

In the same manner, the palæontologist of Bohemia passes in review all the parts of the Trilobites, the succession of their species and genera in time, the parallel between them and the Cephalopods, and the relations of all this to the primordial fauna generally. Everywhere he meets with the same result; namely, that the appearance of new forms is sudden and unaccountable, and that there is no indication of a regular progression by derivation. He closes with the following somewhat satirical comparison, of which I give a free translation: "In the case of the planet Neptune, it appears that the theory of astronomy was wonderfully borne out by the actual facts as observed. This theory, therefore, is in harmony with the reality. On the contrary, we have seen that observation flatly contradicts all the indications of the theories of derivation with reference to the composition and first phases of the primordial fauna. In truth, the special study of each of the zoological elements of that fauna has shown that the anticipations of the theory are in complete discordance with the observed facts. These discordances are so complete and so marked that it almost seems as if they had been contrived on purpose to contradict all that these theories teach of the first appearance and primitive evolution of the forms of animal life."

This testimony is the more valuable, inasmuch as the annulose animals generally, and the Trilobites in particular, have recently been a favorite field for the speculations of our English evolutionists. The usual *argumentum ad ignorantiam* deduced from the imperfection of the geological record, will not avail against the facts cited by Barrande, unless it could be proved that we know the Trilobites only in the last stages of their decadence and that they existed as long before the Primordial, as this is before the Permian. Even this supposition, extravagant as it appears, would by no means remove all the difficulties.

Leaving this subject, we may turn for a little to the growth of our knowledge of the older faunas of the earth. A few years ago, when the last edition of Dana's Manual was published, the Potsdam Sandstone formed the base of the Palæozoic series in America, though Barrande in Bohemia and Salter and Hicks in Wales had disclosed lower horizons of life in those regions: now, in America, Palæozoic life descends almost if not quite as low as that of Europe. The researches of Mr. Murray in Newfoundland, together with the study of the fossils by Mr. Billings, have revealed a lower Potsdam, while Messrs Hartt and Matthew by their praiseworthy explorations of the rich primordial fauna of St. John, have enabled us to establish the "Acadian Group" on the horizon of the lower slate group of Jukes in Newfoundland, of the gold-bearing rocks of Nova Scotia, and of the slates of Braintree in Massachusetts.* Mr. Billings, I have reason to believe, will shortly be able to lead us to still greater depths, and as he indicated at a recent meeting of this Society, to introduce us to the fossils of Sir William Logan's Huronian group. It is thus clear that the student of American geology has to add a new or rather very old chapter to his studies of the older rock formations. In connection with this subject, Dr. Sterry Hunt has raised some new and startling questions as to the classification of all the old Metamorphic rocks of Eastern America, and has excited not a little of that controversy, which, like competition in trade, is the life of scientific progress. Dr. Hunt naturally attaches a very great importance to the mineral character of the more crystalline sediments; and in regions where fossils are wanting, and stratigraphy is obscure, he does well to claim precedence for his own special department of chemical geology; though those of us who have been accustomed to regard mineral character, as an un-

* Menevian of Salter, Etage D of Barrande.

certain guide, and to place our reliance on superposition and fossils, will hesitate to give our adhesion to his views, except so far as they may be established by these other criteria, while at the same time we must admit that Dr. Hunt has by his own labours immensely increased the value and importance of chemistry as an element in geological reasonings. Nor can there be any doubt that the promulgation of Dr. Hunt's views, in his address to the American Association last year, has given a new impulse to the study of this subject; and in the coming summer many skilled observers will be engaged in putting to those ancient, crumpled and mysterious rocks, which underlie or are associated with the fossiliferous rocks of Eastern America, the question, to what extent they will respond to the claims made on their behalf by Dr. Hunt. More especially we may look for much from the researches of Sir William Logan, who, released from the details of the business of the Survey, has been for some time applying his unrivalled skill as a stratigraphical geologist to the further elucidation of the intricacies of the structure of the Eastern Townships of the Province of Quebec; and whose matured results, whether in strict accordance with those deduced from the previous work of the Survey, or modified by his later researches, will be of the utmost value with reference to the structure of the whole of Eastern America.

The recent discoveries in the fossils of the primordial rocks have re-opened those discussions as to the terms Cambrian and Silurian which raged some years ago, between the late lamented Sir Roderick Murchison and his contemporary and survivor the venerable Sedgwick. Dr. Hunt has ably reviewed the history of this subject in the pages of the *Canadian Naturalist*, with the view of enquiring as to the best nomenclature for the present; and arrives at conclusions in harmony with those maintained by Sedgwick many years ago. I confess that I have myself long felt that the nomenclature introduced by the great authority of Sir Roderick and the English Survey, and followed somewhat too slavishly on this side of the Atlantic, requires a reform, of which indeed Sir C. Lyell has to some extent set the example in the latest edition of his elements. When Sir Roderick Murchison was preparing the last edition of his "Siluria," I had some correspondence with him on the subject, and ventured to urge that he should himself revise the classification of that work, wishing at the same time to make similar changes in my "Acadian Geo-

logy," the second edition of which was then in the press. But Sir Roderick was naturally unwilling to change the boundaries of that Siluria which he had conquered and over which he had reigned, and I contented myself at the time with affirming that the Silurian system, as held by Sir Roderick, really consists of two groups, which should have distinct names; but the question of the names I left to others. Dr. Hunt has now the credit of raising the question in a practical form, and I agree with him that the term Silurian should be restricted to the Upper Silurian of Sir Roderick, which constitutes a distinct period of the earth's history, equivalent to the Devonian or the Carboniferous. The Lower Silurian is really another distinct group, but to avoid multiplication of names, and as it formed the battle-ground of the Silurian and Cambrian controversy, I concur in the view that it may well have the name *Siluro-Cambrian*, while the name Cambrian or Primordial will remain for those great and important fossiliferous deposits extending downward from the Potsdam in America and the Tremadoc in England, and constituting an imperishable monument to the labours of Sedgwick and Barrande.

There remains one point still before leaving this subject. It is the gap between the fauna of the Primordial and that of the Laurentian—the latter still represented only by that Titan of foraminifers, *Eozoon Canadense*. Barrande refers to this gap in his memoir above mentioned; and I had hoped ere this time to have done something to bridge it over. I may here state in anticipation of the results of researches still incomplete, (1) That in rocks of Huronian age in Bavaria and probably also in Ontario, *Eozoon* has been found. (2) In the middle and Upper Cambrian we know as yet few limestones likely to contain such a fossil, but we have in Labrador species of *Archæocyathus*, one of which I have ascertained to be a calcareous chambered organism of the nature of a foraminifer; though there seems little doubt that others are, as Mr. Billings has shown, allied to sponges. (3) In the Cambro-Silurian, in the limestones of the Trenton group, animals of the type of *Eozoon* return in full force. The concentrically laminated fossils which sometimes form large masses in these limestones, and which are known as *Stromatopora*, are mostly of this nature, though it is true that fossils of the nature of corals have been included with them. In the Silurian proper, we have the similar if not identical forms known as

Coenostroma, and which according to Lindstrom, form masses in the shales and limestones of Gothland a yard or more in diameter. In all these fossils the skeleton consists of a series of calcareous layers connected with each other by pillars or wall-like processes. The layers are perforated with minute artifices, which are, however, less delicate and regular than in Eozoon, and have in the thickened parts of the walls, radiating tubes of the nature of the canals of Eozoon. (4) On a still higher horizon, that of the Devonian, these organisms abound, so that certain limestones of this age in Michigan contain, according to Winchell, masses sometimes twelve feet in length, and in one place constitute a bed of limestone twenty-five feet in thickness. A beautiful collection of these Devonian forms, recently shown to me by Mr. Rominger, of the State Survey of Michigan, who has worked out these fossils with great care, fully confirms their foraminiferal affinities, and also shows that in some respects, these Devonian forms are intermediate between the Eozoon of the Laurentian and the *Parkeria* and *Loftusia* of the Greensand and Eocene. We thus learn that these gigantic representatives of one of the lowest forms of animal life have extended from the Laurentian, through the Huronian, Cambrian and following formations, down nearly to the close of the Palæozoic. I have no doubt, that when these successive forms are studied more minutely, they will show like the Trilobites, indications rather of successive creations than of evolution, though in creatures of so low organization the differences must be less marked. The point I now wish to insist on, is their continuance, from the Laurentian down to a comparatively modern geological period.

For the third topic referred to at the beginning of this address, I have reserved little space. In the memoir in the Journal of the Natural History Society already referred to, I have re-asserted and supported by many additional proofs that theory of the combined action of Icebergs and Glaciers in the production of our Canadian Boulder-clay and other superficial deposits, which, fortified by the great names of Lyell and Murchison, I have for many years maintained, in opposition to the views of the extreme glacialists. It is matter of gratification to me to find, in connection with this, that researches in other regions are rapidly tending to overthrow extreme views on the subject, and to restore this department of geological dynamics more

nearly to the domain of ordinary existing causes. Whymper, Bonney, and other Alpine explorers, have ably supported in England, the conclusion which after a visit to Switzerland in 1865, I ventured to affirm here, that the erosive power of glaciers is very inconsiderable. The recent German expeditions have done much to remove the prevailing belief that Greenland is a modern example of a continent covered with a universal glacier. Mr. Milne Home, Mr. McIntosh, and others, have ably combated the prevalent notions of a general glacier in England and Scotland. Mr. James Geikie, a leading advocate of land glaciers, has been compelled to admit that marine beds are interstratified with the true boulder-clay of Scotland, and consequently to demand a succession of elevations and depressions in order to give any colour to the theory of a general glacier. The idea of glacial action as means of accounting for the drifts of central Europe and of Brazil seems to be generally abandoned. Lastly, in a recent number of Silliman, Prof. Dana has admitted the necessity, in order to account for land glaciation of the hills of New England, of supposing a mountain range or table land of at least 6,000 feet in height, to have existed between the St. Lawrence and Hudson's Bay, while in addition to the imaginary N. W. & S. E. glacier, flowing from this immense and improbable mass, there must have been a transverse glacier running beneath it up the valley of the St. Lawrence. Such demands amount, in my judgment, to a virtual abandonment of the theory of even very large local glaciers in America in the Post-pliocene period. Thus there are cheering indications that the world-enveloping glacier, which has so long spread its icy pall over the geology of the later Tertiary periods, is fast melting away before the sunshine of truth.

With the exception of that which relates to the Post-pliocene, the geology of Canada has hitherto had to deal only with the more ancient formations. Now, however, there opens up to us a vast field of mesozoic geology in the far west. Already the exploring parties of the Geological Survey are bringing the first fruits of this harvest. The first report of the survey on British Columbia and Vancouver Island is not yet published, but Mr. Selwyn has given us a sketch of his work and that of his indefatigable assistant, Mr. Richardson, in a most interesting and important communication to this Society, a communication which we hail as an earnest of the great things to be expected

from the exploration of those great western territories of the Dominion, whose grand physical features of mountain and plain so excite the imagination, and whose structure and natural productions are so different from those of our eastern regions, and therefore so stimulating to our curiosity.

These explorations will, no doubt, serve not only to enrich the annals of science but also to disclose those sources of material wealth which will ere long attract large populations and capital to the Pacific Coast. In the meantime, perhaps, no features excite greater interest on the part of the geologist than the appearance of a comparatively highly altered condition in sediments of no great geological age, and the occurrence of coal in Vancouver Island, associated with animal fossils of Cretaceous date and with a flora composed of exogenous trees of very modern aspect.

In addition to the papers on which the above remarks have been based, we have had two interesting communications from Prof. Nicholson of Toronto, whom we welcome as a valuable addition to our band of workers. Dr. Hunt has contributed a paper on the structure of Mont Blanc; Mr. Billings has given us papers on the Fossils from the Huronian rocks, on the Taconic controversy, on the genus *Obolellina* and on new species of Palæozoic Fossils; Prof. Bailey has given us a paper on the previously little known geology of the Island of Grand Manan; and Mr. Matthew, one on the Surface Geology of New Brunswick. Dr. Anderson, of Quebec, has contributed a notice of a whale captured in the Gulf of St. Lawrence; Mr. Macfarlane has given us his views on the classification of crystalline rocks; Dr. Carpenter has directed our attention to the death-rate of Montreal; and Dr. Smallwood has reported on Meteorological Results for 1871.

I cannot conclude without referring to a new branch of scientific research undertaken by the Society in conjunction with the Department of Marine and Fisheries—that of dredging in the deeper and hitherto unexplored parts of the Gulf of St. Lawrence; and we have to congratulate ourselves on important scientific results obtained in a manner equally creditable to the Government, to the Society, and to its Scientific Curator, Mr. Whiteaves. A knowledge of the fauna of the Gulf has been obtained to a depth of 250 fathoms. Probably one hundred species have been added to the known inhabitants of our Canadian waters. Interesting facts have been obtained as to the distribution and food of fishes; and the attention of the Government of

the Dominion has been awakened to the value of researches of this kind. It is hoped that they will be renewed in the approaching summer with larger means and with apparatus for ascertaining more correctly the temperature and composition of the water at great depths.

In conclusion, we have much reason to be satisfied with the measure of success which has attended our work in the past year, and to take courage for the future.

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measure of success which has attended our work in these years
and to the committee for the future.

The work has been done in a very satisfactory manner and
it is to be hoped that the results will be of great value
in the future. The committee will be glad to receive any
information that may be of use to them.

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