PRESIDENTIAL ADDRESS OF SIR J. WILLIAM DAWSON

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SOME RECENT DISCUSSIONS IN GEOLOGY

Annual Address by the President, **Sir J. William Dawson**

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INTRODUCTION.

Our science has been characterized as one whose goal today is its starting point tomorrow. Nothing, therefore, can be more suitable to an occasion of this kind than a glance at some of those questions at present most actively discussed, and on which we have within the last few months been reading the arguments and conclusions of some of our ablest workers. We may even venture to make some modest suggestions as to the manner of possible settlement of these questions, and this aid in clearing the way for those advances which in the near future must leave our present standpoint far in the rear. Such a review must necessarily be discursive and fragmentary—a sort of conglomerate in its material, but some consistency may be given to it by regarding its several topics in their relation to the foundation and building up of our continents, one of the great leading points of geologic investigation.

PRE-CAMBRIAN ROCKS.

Beginning with those ancient Archean or Eozoic formations, which are the foundation-stones of the earth, and in nearly every part of the world may be seen to underlie the other members of our geologic column, we have recently learned from Sir Archibald Geikie* that the great controversies which have raged as to these rocks in the west highlands of Scotland ever since the order assigned to them by Murchison was called in question by my friend and fellow-student, Professor Nicol, of Aberdeen, has been finally settled. On comparing his arrangement with American facts, and especially those displayed in the unequaled exposure of these rocks in Canada, it would appear that the following correlations may be stated:

The older gneissic group of the west Highlands of Scotland does not contain the whole of the Laurentian of Logan, the Lewisian of Murchison, but only or mainly the lower part of it, the Ottawa group of the Canadian survey. A certain limited track at Loch Maree not improbably represents the Upper Laurentian or Grenville series, and this certainly occurs in the western islands. I use the term Upper Laurentian in the sense recently given to it by Dr Adams;[†] the original Upper Laurentian apparently consisting, in what was regarded as its typical area, mainly of igneous products. It is to be observed, however, in this connection that over large areas in the west the Upper Laurentian is absent, or has been removed, or is replaced by rocks of somewhat different character from those of the east.

I take this opportunity to object to the term "Archean or Basement Complex" applied by some geologists to these formations. Every geologic formation is complex, especially when disturbed and invaded by igneous rocks, but none is more simple than the Lower Laurentian, as it consists almost entirely of orthoclase gneiss; and even the igneous masses and veins have been introduced so quietly and with so little of the violence of modern vulcansim that it is not easy to separate them from the old beds with which they are so intimately united. I may add that it seems likely that the Lower Laurentian is the oldest formation we shall ever know, and that its peculiar characteristics depend on its constituting the earliest deposits from water on the thin crust of a lately incandescent globe.

The Torridon sandstones and the associated beds of Geikie seem in mineral character and in association with

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* Journal of Geology, vol. 1, number 4, 1893.

the Laurentian, and in the few fossils which they contain, to be equivalent to the Huronian of Logan. The Dalradian, at least in Ireland, would seem to be of similar character and age.

The Uriconian and Longmyndian of Geikie probably include the equivalents of our Kewenian, and the same may perhaps be said of the Dalradian of Scotland. Some portions of these rocks may, however, be the same with what in Canada has been called by Matthew "Basal Cambrian."

It is evident that in Scotland, as in North America, the Laurentian rock have been elevated into land before the deposition of the Huronian, and that the latter and the Kewenian are coarse littoral deposits clinging to the Laurentian shores, protected in part from lateral crushing by their hard Laurentian base, and represented at a greater distance from the old land by formations which have sometimes received different names, and which are usually in a state of greater alteration and compression.

It may be remarked here that in Canada, though the Laurentian beds are much folded and contorted, they are comparatively little affected by faults or overthrusts and the succession is often extremely clear, while the outcrops of individual beds can be traced over great distances. This applies especially to the Upper or Grenville series, holding the great limestones and beds of graphite and magnetite and the serpentinous limestone containing eozoon.[*sic*]

The simple arrangement of the infra-Cambrian rocks as Kewenian, Huronian and Upper and Lower Laurentian is further vindicated by Walcott's section in the Colorado canyon, which shows them not only superimposed but unconformable. The lowest member is a granite rock probably equivalent to the fundamental gneiss. Walcott has found in the upper part of the infra-Cambrian an obscure discina-like or steno-theca-like shell and a fragment resembling the cheek of a small trilobite. Still lower are the stromatoporoid masses of supposed *Cryptozoum*. Some specimens of this, recently sliced, show distinct traces of structure similar to that of Hall's typical species of *Cryptozoum*.

From long acquaintance with these rocks I conclude that the fourfold arrangement of Lower Laurentian, Upper Laurentian, Huronian and Kewenian will include them all, and that the name Algonkian, recently proposed, is merely provisional and equivalent to pre-Cambrian, which has been used to include rocks of uncertain classification in the base of or older than Paleozoic.

MOUNTAIN-MAKING.

It is an easy transition from the old crystalline rocks to the mountain masses which so largely consist of them, and our knowledge of the foldings, crumplings and overthrusts of the older rocks certainly gives much help in the comprehension of mountain-making. Yet we must not forget that all mountains are not made up of old rocks folded and pushed over or under each other. Mountains of great magnitude, like Etna, Vesuvius, and the cone of Cotopaxi, are built up of materials ejected from below in the manner of mole-hills or the dump of a mine. As I do not like the modern method of inventing grandiloquent names for structural features, I shall call this class "dump mountains." The most curious thing about them from our present point of view is the fact that they do not crush down the crust under them as sedimentary deposits would, and this, as any one can easily understand, depends on the circumstance that the very existence of such mountains is an effect of the upward pressure of the matter beneath them. It may be said that such mountains are modern; but it is true that some very old elevations are remnants of the denudation of ancient piled-up cones.

Another class of mountains, which may be named "blister mountains," is produced by the gentle swelling up of the crust without any folding. Such mountains are the Catskills, the western Sierra, some mountains of old red sandstone in Scotland, and high chain of Lebanon, which at its summit, 10,000 feet above the sea, presents horizontal beds of limestone falling away in mural precipices. Such mountains, unless supported merely by the heating and expansion of matter below, must be sustained by the horizontal injection of mobile matter beneath them. Hence the elevation of these mountains may imply much movement of softened rock beneath the crust, of a kind altogether distinct from lateral pressure at the surface.

The greater and more typical ranges of mountains, however, like the Alps and the Appalachians, are mountains of crumpling, showing evidence of enormous lateral pressure proceeding from the adjoining sea basins, and to this, it is now almost universally admitted, their elevation must be in great part due. We must note here, however, that in all great mountain ranges all these kinds of elevation are observed, for mountain-making on the great scale has implied not only plication but the elevation of plateaus and tablelands and volcanic ejections as well.

Two momentous questions arise here: Whence the pressure; and why has it acted along certain determinate lines?

The last of these questions comes first in order of time, for it seems established, and in this country has been well illustrated by Hall, Dana and Rogers, that the main lines of folding occur where the thickest sediments have been deposited along the borders of the oceans, and where, consequently, the lower parts of such sediments have been pushed by subsidence far down toward the heated interior of the earth. Again, whatever reasons may be urged against such a conclusion, it is evident that the crust underlying the oceans is the strongest of all, and that it must have been the pushing or resisting agent. The mountain regions of western America have, according to the Geological Survey of Canada, been pushed eastward by the Pacific area more than two degrees of longitude, and Claypole affirms that the sediments of the Appalachians have been reduced to one-third of their original breadth by the pressure of the Atlantic basin.

All this is explicable at once on the old contraction theory, so ably expounded in this country by Le Conte. The thick resisting ocean basins have settled downward toward the center of the earth; they have at the same time cause the mobile matter beneath them to ooze out in volcanic ejections or to slide laterally under the lighter parts of the continents. They have thus exerted a great lateral pressure on their sides, much as the thick coating of ice on one of our northern lakes casts up ridges on its margin. It is objected to this that the earth is a rigid mass, and that the zone of lateral pressure by contraction is very superficial; but rigidity is a relative term—everything can be made to submit to adequate pressure; and however physical demonstration may establish the solidity of the earth, we may say as did Galileo, though in a somewhat different connection, we are sure, nevertheless, that it moves; and the sediments that make up the mountains are the thinnest possible veneer, the mere coat of a varnish on an artificial globe, which can scarcely be laid on so evenly that it will not have inequalities greater than our mountains.

At the same time I see no reason why we should not avail ourselves of the expansion theory of T. Mellard Reade as well. The heated and swelling sediments may have thickened and twisted upward in aid of the lateral pressure caused by contraction. Nor need Dutton's theory of isostasy be left out, for the whole process of mountain-making seems to imply a certain flotation and pouring sideways of the potential liquidity beneath the crust, which is also evidenced by the volcanic ejections accompanying or consequent on the elevation, and which add to the product their injected masses and dikes, overflows of molten rock and ejections of fragmented material. The final result is that mountains can neither be built in a day nor by one cause only. When we have to fold great masses of rock into a third of their original width, to raise them thousands of feet into the air, and to sculpture the rude masses thus provided into grand and beautiful forms, we may well avail ourselves of all possible causes of elevation, as well as of those atmospheric and aqueous denuding agencies which give shape to the whole.

UNIFORMITARIANISM.

In connection with mountain-making, as well as other geologic changes, the well worn discussions as to uniformitarianism in geology have been refurbished, more especially in New England, where Teall, in his address as president of the geological section of the British Association insisted on the unity of origin of the older crystalline rocks with their more modern successors, and the veteran Prestwich* has made a strong protest against an exaggerated uniformitarianism as applied to the later formations. Here also we need to beware of that one-sidedness which has led to so much unnecessary controversy from the days of Werner and Hutton down to the present time.

We may be fully prepared to admit that, on the hypothesis of a cooling globe, there must have been certain primitive rocks deposited as the first products of the action of a heated ocean on a still hot crust, conditions which would not again occur except in limited and exceptional cases. On the other hand, we know that ever since land and water existed, there must have been a certain uniformity and continuity of erosion and deposition. We may also in all this expect a kind of development whereby old rocks area wasting away and are redeposited in somewhat different states, but we must at the same time make allowance for the differences provided by alternate elevation and subsidence and by the occasional introduction of igneous products. So guarded, we may hold with truth that there has been a substantial uniformity of the origin and character of rocks throughout geologic time, though in every succeeding age the continents and the rocks composing them are different from their condition in any previous period. There has thus been uniformity with change and progress, but while the laws of nature and the operations under them have been uniform in kind, we must beware of supposing that they have been uniform in rate. In short, slow and gradual actions inevitably produce catastrophes or critical periods, and these again prepare the way for the recurrence of times of dull uniformity and scarcely perceptible motion. Slow and secular accumulation of sediments on limited areas or expansion and contraction of rocks may produce sudden and violent movement of the crust, just as we have seen lately the accumulation one by one of sheets of paper at length involve in sudden and utter ruin in a great public building. A cliff long acted on by disintegrating atmospheric agencies at length falls instantly in a mass of fragments, and this prepares the way for new action of the atmosphere on the cliff in its protracted and infinitesimal way, and for the agency of the waters in removing the talus of fallen material.

The stupendous changes which we know our continents have experienced in the later Cenozoic periods and in times comparatively short, should warn us against exaggerated uniformitarianism, more especially when we find that this opposes invincible difficulties in the way of any rational explanation of such climatic changes as those of the Glacial period or of the great continental movements which have interfered with the continued development even of man himself. It is especially with reference to these that Prestwich truly says that—

"The forms of erosion, the modes of sedimentation and the methods of motion are the same in kind as they have ever been, but we can never admit that they have always been the same in degree. The physical laws are permanent; but the effects are conditional and changing, in accordance with the conditions under which the law is exhibited."

I fear that the unreasonable uniformitarianism of certain modern schools of geology is a product less of scientific observation and induction than of the influence of certain philosophical dogmas. Lyell, the great author of rational uniformitarianism in geology, well understood the fact that catastrophe and cataclysm have their place in the grand uniformity of nature, and that long continued uniformities must lead to critical periods. He was not an agnostic or a believer in a necessitarian evolution. He saw in nature adaptations and a grand plan of development, including all changes, whether sudden or gradual; and I may add that it was this which gave that charm and fascination to his teaching, which caused one of his contemporaries to compare the interest of the Principles of Geology to that of an exciting romance. Dead materialistic uniformitarianism, shout it ever become the universal doctrine of science, would provoke a reaction in the human mind which would be itself a cataclysm.

COAL-MAKING.

Of all the accumulations formed in geologic time probably the most slowly produced are those of organic materials; yet, curiously enough, even in the present exaggerated uniformitarianism there has been a tendency here to return to exploded catastrophism. One can imagine some of those great beds of sandstone which occur in the Coal Measures, filled with trunks of trees piled in the most confused manner, to have been deposited by violent inundations; but when, after all that has been done to explain the origin of coal, we find some late writers returning to the old and exploded idea of the production of coal by driftage, we are tempted on the one hand to vexation, and on the other to laughter. In a very recent article in a well known journal I find in support of this

* Nineteenth Century, October, 1893.

theory the contention that underclays are not ancient soils, and the following sentences, alleged to be contradictory to each other, quoted from authorities on the subject. The first is as follows: “Underclays are old vegetable soils, and they were formed, not under water, but on dry land.” Now underclays are certainly vegetable soils, but they were not necessarily formed on dry land. They may be deposits from water, but may have been raised-up or filled-in to constitute soils. The second is: “Underclays are distinctly stratified, showing that they have been deposited under water.” This is true of some of them at least, but is no argument against their having become soils. The subsoils of many swaps and marshes is a deposit from water, but land vegetation grows upon it. The imperfection of such statements and the absurdity of placing them in contrast are sufficiently obvious, yet such objections have to be met in the interest of scientific geology. They must be met exactly as they were met by Logan so many years ago in his observations on the underclays of south Wales, which have been followed up by myself and others. We have shown, in the first place, that the lycopods, ferns and calamites growing on these underclays were really land plants; secondly, that their roots penetrated the subjacent buds in such a way as to show that they have grown upon them, and, lastly, that the coal itself, in all cases except that of the cannel coals, bears evidence of subaërial accumulation, while the erect trees associated with it show that they decayed and became hollow by atmospheric action. No doubt the underclays were usually swamp rather than upland soils, but the occurrence of remains of land animals in erect trees shows that in some cases the soil must have been elevated ten feet or more above water level when the coal vegetation was growing on it. I have myself studied and described these facts as evidenced in the case of eighty successive beds of coal admirably exposed in the cliffs of the south Joggins.

In connection with all this we have the accumulation of five thousand feet of sediments and organic beds, each of which must in turn have been a land or shallow water surface, and the subsidence thus indicated must have taken place by small downthrows, only sufficient to keep pace with the accumulation of deposits, and this for a great lapse of time. The coal-deposits of the great Carboniferous system thus mark a special state in the production of our continents, when they were less differentiated as to orography, and when a very uniform and equable climate extended over the northern hemisphere, accompanied by a very peculiar vegetation. Such conditions did not occur in combination and to a like extent in any succeeding period in earth’s history.

RELATION OF VEGETATION TO CONTINENTAL MOVEMENTS.

This special position of the great coal-formation leads to a consideration of the relation of vegetation and of fossil plants to the elevation and depression of our continents, to changes of climate, and to the determination of geological age, and of which we are reminded by Professor White’s discussions of these subjects, and those in the recently published essay of Seward, as well as the posthumous report of Lesquereux on the flora of the Dakota group. I have already referred to the special conditions of the later Paleozoic in these respects, and am inclined to attribute the great geographic uniformity of its vegetation principally to the then unfinished condition of our continents, affording less local difference of elevation and greater uniformity in the distribution of ocean currents, though the larger proportion of carbonic dioxide in the atmosphere may have been also a determining cause. Yet, while there was little climatal difference of flora, there was a continued change in time; so that wherever fossil plants occur, we can distinguish the vegetation of the Lower, Middle and Upper Devonian, of the Lower Carboniferous, of the Coal Formation, of the Upper Coal Formation, and the Permian. The great earth-movements of the Permian seem to have extinguished this flora by creating adverse climatic conditions, and in the Mesozoic age it was replaced by a new assemblage of plants, seemingly of southern origin, and adapted to an insular condition of our hemisphere. The later Cretaceous flora, with its wealth of modern exogenous genera, seems to have originated in the north and propagated itself southward, and the condition of things which led to a temperate flora in Greenland was connected with the occurrence of a great Mediterranean sea between the Rocky mountains and the Appalachians, which determined the equatorial current upward through the interior of the American continent and threw its full force on Greenland, then probably less elevated than now. The geographic conditions of these ages of the later Cretaceous and early Cenozoic, we are now able to some extent to trace, and find them to correspond with the climatal conditions indicated by the plants. On the other hand, the changing physical conditions were correlated with those changes in the vegetation which have enabled us to recognize so distinctly the lower, middle and later Cretaceous floras, and those of the early, middle and later Cenozoic.*

While we have no evidence of a tropical climate in the northern part of America in the Cretaceous or the Cenozoic periods, we have proof from fossil plants of the continuance for long periods of a temperate climate as far north as Greenland, and that this passed gradually into the cooler temperature of the Miocene and Pliocene. We can also correlate these climatal conditions on the one hand with known geographic changes, and on the other with the distribution of animals and plants. The validity of such deductions does not altogether depend on the accuracy of the reference of fossil species to existing genera or families. In many cases there can be little doubt as to this, as in the species of liriodendron, sassafras, platanus, sequoia and salisburia, and especially in the case of all those of which seed or fruit have been preserved; but even when the naming is inaccurate or when the number of species has been unduly multiplied, the deductions as to climate may hold good, though not perhaps to the extent of enabling us to fix a definite thermometric mean temperature.

As to geologic age, the primary requisite is that in some of the localities of plants in question their relative ages shall be determined by stratigraphic evidence; this being done in a few cases, it is not difficult to assign to their approximate position intermediate or allied subfloras. Plants treated this way as evidence of geologic age have the advantage of wide distribution over the surface of the land, of slow migration and of long endurance in time. As in the case of animal fossils, we have to allow for differences of station, for possible driftage and intermixture of species belonging to higher and lower lands, and for chances of deposition and preservation. We have also to consider that plants are more permanent and less changeable than the animal inhabitants of the land, and therefore better fitted to mark the longer ages of geologic time; but this is more than compensated by the closeness of their relations with alternate elevations and depressions of our continents and the climatal relations dependent on them. A single leaf of some plant of a temperate genus found in arctic regions may thus bear explicit testimony to the former geography of a whole continent, and the climatal phenomena dependent on it; and thus aid us in understanding and referring to it true causes even the great Glacial period itself.

GLACIAL PERIOD.

I have recently been so venturesome as to add to the many publications on this vexed subject a republication of my numerous papers on phenomena of the Glacial period in America; and I am aware that many of my friends in this Society will dissent very widely from the views therein expressed. They will see, however, that I adhere very strictly to the physical possibilities of ice, and to the doctrine of existing causes, and that I have endeavored to take into account changes of geographic forms, and of climate dependent on them, and of all the varieties of land and water-borne ice anywhere to be seen in the colder portions of the earth at present. It is, I am convinced, only by taking all of these into account that we can succeed in explaining the complicated phenomena of this remarkable age; and we must be prepared also to allow for the movements of elevation and depression which seem to have occurred in that unsettled period, and of which many are fitted to produce a minimum distribution of heat in the higher lands of our continents, while furnishing great oceanic areas for the supply of vapor. The accumulation of ice and snow and the production of great glaciers can occur only where there are not only large areas of abundant precipitation, but others of equally abundant evaporation. I would therefore ask the attention of my fellow-workers to the facts and conclusions presented in the volume referred to, and would explain that I have been induced by long and careful study of the phenomena, both ancient and modern, presented to observation in Canada to conclude that no one

* Trans. Royal Society of Canada, 1893. Paper on New Plants from Vancouver Island.

cause, however potent, can account for all of these phenomena, and that we must invoke the combined and successive action of glaciers, of icebergs, of field, floe and pan ice, and in short, all these glacial agencies that now operate in the north, and this in connection with great and unequal changes of level, producing elevation and submergence, the whole in such a way as to modify climate locally, and to some extent throughout the northern hemisphere. The problems presented to us by the Glacial period of the Pleistocene are thus very complex, and the great error here, as in so many other departments of geology, has been that of referring the effects of various causes and conditions, alternating through a considerable lapse of time, to one dominant cause without reference to others equally important. The time, however, is rapidly approaching when we shall no long speak of opposed glacier and iceberg theories or invoke incredible physical changes to account for imaginary phenomena. I need scarcely add that our views of this whole subject have been greatly modified by demonstrations that the close of the Glacial period dates only a few thousands of years before our own time, and that those astronomic theories, which demand a vastly greater time for their operations, are no longer tenable as the cause of a glacial period.

I may base some objections to the idea of a continental glacier as now held by many in this country on a suggestive paper by Dr Warren Upham* in the Bulletin of this Society, in which he institutes comparisons between Pleistocene and present ice-sheets. The present ice-sheets are stated to be four: 1. Antarctic or that which fringes the Antarctic continent and is probably better entitled to the name than any other, but which differs from the supposed ice-sheets of the Pleistocene in fronting on the sea and discharging all its product as floating ice. In this, however, it certainly resembles many of the local glaciers of the Pleistocene. 2. The great névé of Greenland, which however, discharges by local glaciers, opening on the sea. 3. The Malaspina glacier of Alaska, evidently a local glacier of no great magnitude, though presenting some exceptional features. 4. The Muir glacier of Alaska, also a local glacier, but perhaps, like the Malaspina, showing some features illustrative of local Pleistocene glaciers.

In the "conferences and comparisons," however, the facts detailed in the earlier part of the paper are placed in comparison with postulates respecting the Pleistocene which are incapable of proof: 1. It is taken for granted that the upper limits of glaciation in the mountain ranges of America indicate the thickness of a continental ice-sheet. They probably indicate only the upper limit of the abrasion of local glaciers. 2. Hence it is computed that the thickness of a continental glacier flowing radially outward in all directions from the Laurentian highlands of Canada amounted to two miles, and in connection with this it is stated that the maximum thickness of the great Cordilleran glacier of the west in the Pleistocene age has been estimated to be about 7,000 feet, and entirely different thing and referring to the maximum depth of a local glacier traversing deep valleys. 3. It is admitted that the assumed continental glacier could not move without an elevation of the Laurentian highlands to the height of several thousand feet, of which we have no evidence, for the cutting of the deep fiords referred to in this connection must have taken place in the time of Pliocene elevation of the continents before the Glacial period. 4. The upper and lower bowlder-drift, so different in their characters, are accounted for on the supposition that the former comes from material suspended on the ice at some height above its base, the other from that in the bottom of the ice. In like manner the widely distributed interglacial beds holding remains of land-plants of north temperate character are attributed to such small local occurrences of trees on or under moraines as appear in the Alaska glaciers. 5. The rapid disappearance of the ice is connected with a supposed subsidence of the land under its weight, though from other considerations we know that if this was dependent on such a cause it must have been going on from the first gathering of the ice, so that the required high land could not have existed. All the evidence, however, points to subsidence and elevation owing to other and purely terrestrial causes, and producing, not produced by, the glaciers of the Pleistocene.

The question of erosion by glaciers is still agitated. My own conclusions, formed from the study of the Savoy glaciers in 1865, is that glaciers are never important

* Comparison of Pleistocene and present Ice-sheets. Bull. Geol. Soc. of America, vol. 4, pp. 191–204.

eroding agents, that in valleys they protect the rock from the greater denuding action of streams, and that the mud and sand which they produce are derive not from the rocks in which they slide, but from the material that falls upon the glacier. The bottom rock is merely the nether millstone.

One of the most experienced of alpine geologists, Professor Bonney, in a paper read before the Royal Geographical Society,* discusses this question in detail and arrives at the same conclusion which I stated in 1866, namely, that glaciers are "agents of abrasion rather than erosion," and that in the latter their power is much inferior to that of fluviatile action. Nor are glaciers agents in the excavation of lake-basins, which are to be accounted for in other ways; and the great gorges and fiords which have been ascribed to them are due to aqueous erosion when the continents were at a higher level, before the glacial age.

Lastly, on this subject, very important facts have been ascertained by the Geological Survey of Canada and by the United States observers in Alaska, indicating that during the height of the Glacial period there was an open arctic basin in the north. This coincides with the fact stated by Professor Penhallow† and myself in a previous volume of the Bulletin of this Society, that in the Pleistocene period the flora must have maintained its ground farther north. In northern Europe, Nathorst and others have shown a southward movement of the Scandinavian flora, but this does not seem to have been general, and the recent work of Lange and Warming on the flora of Greenland proves that the persistence of the arctic flora in the north applies even to that country, whose conditions as to climate does not seem in the Pleistocene to have differed much from that of the present time. It is not impossible that, as Howorth has suggested, the north Polar regions are colder now than in the Pleistocene, that the cold of that period was thus more local than has been supposed, and that we may find that even the mammoth was able to hold his ground in the north throughout the great Ice-age.

* Geographical Journal, July, 1893. See also J.W. Spencer: Quart. Jour. Geol. Soc. of London, 1890, p. 523.

† On the Pleistocene Flora of Canada. Bull. Geol. Soc. of Am., vol. i, pp. 311–344.

Allow me further to say that these facts tend to confirm the conclusions already stated in this address, that we are to look, for causes of change of climate, rather to movements of elevation and subsidence of the continents than to any extra-mundane influences.

POST-PLEISTOCENE CONTINENTAL MOVEMENTS.

We come now to the last great vicissitude of our continents, one that is beginning to connect itself with the history of man himself. No geologic fact is more certain than the occurrence of a period of continental elevation after the great Pleistocene submergence, and that this period coincided with the spread of postglacial or palanthropic man over the continents of the northern hemisphere. It is equally certain that within a time measured by a few thousand years this continental period terminated, and the continents subsided to their present limited dimensions, permanently submerging some of the fairest portions of the former abodes of man and for a time inundating vast areas of land. It has, however, been a much debated question whether these great changes were gradual or sudden, and whether they were connected with the disappearance palanthropic man and his contemporaries. I have myself long maintained the conclusion that the human period is on good geologic evidence divided into two portions by great earth-movements, and that it is the historical traditions of these which constitute the foundation of that universal belief of a deluge which has fixe itself in the memories of ancient men in every part of the world.

The great English geologist, Prestwich, has recently given much attention to this subject, and in a memoir in the Transactions of the Royal Society of London* has adduced a mass of evidence on which he bases the conclusion that an important movement of subsidence and reëlevation occurred at the end of the Glacial or post-Glacial period, and was of the character of a somewhat sudden inundation destructive of man and animals. The deposits produced by the recession of the waters of this inundation he designates

* Vol. 184, 1893, p. 903.

"rubble-drift," a formation which overlies the glacial deposits and indicates a movement of water distinct from anything belonging to glacial phenomena or to ordinary river inundations. He includes with this the deposits known as "head" in England and also the loess of the plains and tablelands of Europe and the material found in certain caves and fissures. He might have added some of the gravels and superficial deposits of Egypt and Syria, and modern deposits extending far east into central and northern Asia. Thus we now have geologic facts which show that man has been a witness of one great continental submergence, which must have intervened between the close of the Glacial period and the present time. These facts at once establish a remarkable correlation between the results of geologic investigation and the historic deluge, and expose the fallacies of those theories which assume an uninterrupted progress of man from his first appearance until the present day. A curious confirmation of this has recently been furnished by the excavations of Nuesch in a rock shelter near Schaffhausen*, which show an overlying deposit with Neolithic implements and bones of recent animals, a bed of rubble and loam destitute of human remains, and below this a bed containing bone implements, worked flints and traces of cookery of the palanthropic period. The whole rests on a bed of rolled pebbles supposed to be the upper part of the glacial deposits. This shows an interval between the palanthropic and neanthropic periods, and also the post-glacial date of man in Switzerland. It corresponds with a great number of other facts.

I cannot doubt that evidences of the second continental period exist in America. Those which are afforded by the warm-water fauna of the southern bay of the Gulf of Saint Lawrence I pointed out many years ago. There are also superficial deposits which show depression since the glacial age, though I fear that many of them have been confounded with the ordinary drift, and I think the attention of geologists who study these more recent deposits should now be directed to the separation of rubble drift, head and loess from the beds properly belonging to the Glacial period, and to the bearing of these facts on questions as to the possible occurrence of man in America in the Palanthropic age.

PREGLACIAL MAN.

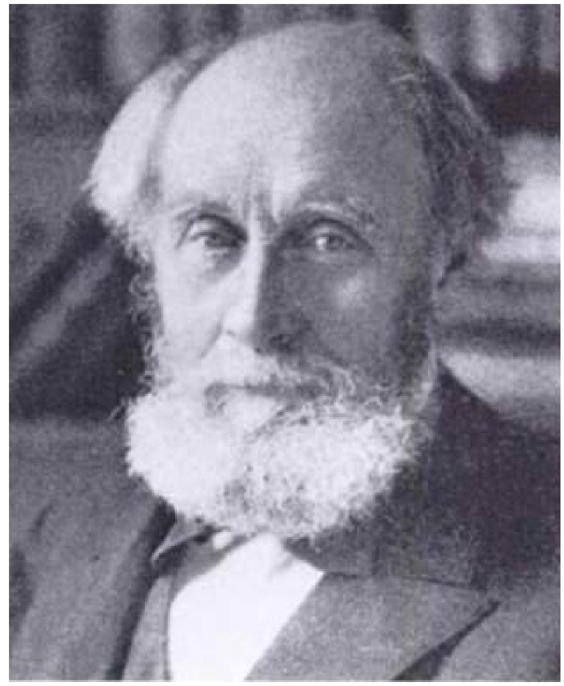
I confess that I have all along been skeptical on geologic grounds as to the numerous finds of paleolithic implements in the glacial gravels. The gravels themselves are probably in many instances postglacial, and it is doubtful if the implements belong to these gravels or are merely superficial. The observations of Mr W.H. Holmes, of the United States Geological Survey, seem now to have confirmed these doubts, very notably in the case of the celebrated Trenton implements. With the aid of a deep excavation made for a city sewer he has shown that the supposed implements do not belong to the undisturbed gravel, but merely to a talus of loose débris lying against it, and to which modern Indians resorted to find material for implements and left behind them rejected or unfinished pieces. The alleged discovery has, therefore, no geologic or anthropogenic significance. The same acute and industrious observer has inquired into a number of similar cases in different parts of the United States, and finds all liable to objections on the above grounds, except in a few cases, where the alleged implements are probably not artificial. These observations not only dispose, for the present at least, of paleolithic man in America, but they suggest the propriety of a revision of the whole doctrine of paleolithic and neolithic implements as held in Great Britain and elsewhere. Such distinctions are often founded on forms which may quite as well represent merely local or temporary exigencies or the débris of old workshops as any difference in time or culture. All this I reasoned out many years ago on the basis of American analogies, but the Lyellian doctrine of modern causes as explaining ancient facts seems as yet to have too little place in the science of anthropology.

This question, however, still remains whether there is any evidence of the occurrence of postglacial or palanthropic men in America, as distinguished from the modern American Indian, and, if so, whether any geologic evidence exists of his having shared in the diluvial catastrophe so destructive to his old-world conferes.

The collections now being accumulated by Putnam in the Peabody Museum at Cambridge, will do something toward settling these questions, if properly aided by the work of geologists in the field, and it would be a triumph for American science to remove them from the doubt and difficulty which now surrounds them; but the geologist, rather than the archeologist, must now assume the responsibility of establishing the true age and sequence of the deposits.

I began with the statement that our goal today will be our starting point tomorrow, and have endeavored to attract your attention to a few of the questions which are being agitated today. What tomorrow may bring forth it remains for my successors to tell. I may conclude with thanking you for the honor you have done me in placing me in this presidential chair, and by expressing my sincere good wishes for the prosperity and usefulness of the Geological Society of America.

Transcribed from the original GSA Bulletin *article for posting on the Web in April 2013.*



Sir J. William Dawson