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ORDINARY MEETING.*

THE PRESIDENT (SIR GEORGE G. STOKES, BART., F.R.S.), IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed.

The PRESIDENT.—I regret to say that the Author of this Paper has been unable to leave Edinburgh University, as he had hoped, so as to have been present here this evening; he has therefore asked his friend, Mr. G. G. Chisholm, to read the Paper for him.

THE GLACIAL PERIOD AND THE EARTH-MOVEMENT HYPOTHESIS. By Professor JAMES GEIKIE, LL.D., D.C.L., F.R.S., etc.

PERHAPS no portion of the geological record has been more assiduously studied during the last quarter of a century than its closing chapters. We are now in possession of manifold data concerning the interpretation of which there seems to be general agreement. But while that is the case, there remain, nevertheless, certain facts or groups of facts which are variously accounted for. Nor have all the phenomena of the Pleistocene period received equal attention from those who have recently speculated and generalised on the subject of Pleistocene climate and geography. Yet, we may be sure, geologists are not likely to arrive at any safe conclusion as to the conditions that obtained in Pleistocene times, unless the evidence be candidly considered in all its bearings. No interpretation of that evidence which does not recognise every outstanding group of facts can be expected to endure. It may be possible to frame a plausible theory to account for some particular conspicuous phenomena, but should that theory leave

* 9th Meeting, 27th Session.

unexplained a residuum of less conspicuous, but nevertheless well-proved facts, then, however strongly it may be fortified, it must assuredly fall.

As already remarked, there are many phenomena in the interpretation of which geologists are generally agreed. It is, for example, no longer disputed that in Pleistocene times vast sheets of ice—continental *mers de glace*—covered broad areas in Europe and North America, and that extensive snow-fields and large local glaciers existed in many mountain-regions where snow-fields and glaciers are now unknown, or only meagrely developed. As Professor Penck and others have shown, the line of perennial snow during the glacial period must have been depressed in Central Europe for 3,000 or 3,500 feet—a depression which would correspond approximately to a general lowering of the mean annual temperature of about 10° or 11° F.* This, as Penck points out, would bring the climate of Northern Norway down to Southern Germany, and the climate of Sweden to Austria and Moravia, while that of the Alps would be met with over the Mediterranean. It is particularly worthy of notice that the lowering of the temperature was not confined to North-Western and Central Europe, but was general over the whole continent. The Scoto-Scandinavian inland-ice covered many thousands of square miles in the Northern and North-Western portion of the continent; in the Alps and other mountains of Middle Europe great snow-fields and glaciers existed; while further south, as in the Sierra Nevada, Corsica, the Apennines, the Despoto Dagh, etc., only a few isolated local glaciers appeared. Still further south and south-east, as in North Africa and Syria, rainy or pluvial conditions seem to have been contemporaneous with the glacial period of Europe. Thus, it is highly probable—one might almost say certain—that precipitation over the whole continent was greater than now. The geographical distribution of glacial, fluvio-glacial, and other Pleistocene deposits leads, in fine, to the conclusion that in glacial times a wholesale displacement of climatic zones took place. This is most clearly indicated by the Pleistocene system of Europe and Asia, but it is hardly less marked in the corresponding deposits of North America.

It is further to be observed that the glacial conditions of

* According to Dr. Brückner the general lowering of temperature may not have exceeded $5\frac{1}{2}$° to 7° F. *Verhandl. d.* 73 *Jahresversam. d. Schweizerisch. Naturforschen. Ges. in Davos*, 1890.

the Pleistocene period were simply an exaggeration of those now existing. The great inland-ice of Northern Europe is represented to-day by the snow-fields and glaciers of Norway, while the glaciers of the Alps and other mountain-regions are the descendants of those of Pleistocene times. During the glacial period precipitation and accumulation of snow diminished from west to east, and the same is the case at present, for the snow-fields and glaciers of the Western Alps are on a larger scale than those that appear in the eastern portion of the chain. Again, while Norway has its glaciers, in the Urals there is none. Even during the climax of the glacial period the Ural Mountains nourished only a few small local glaciers. We note further that mountains which in our day do not reach the snow-line supported in glacial times relatively small snow-fields and glaciers. The contemporaneous phenomena of North America tell a similar tale. The north-eastern section of that continent was mantled with an immense ice-sheet, while in the far west only gigantic local glaciers existed. To-day the same contrast presents itself; in the north-east we have Greenland drowned in ice, but the loftier mountain-regions of the far North-West, although lying in the same latitude, support only local ice-flows. Were the climatic conditions of the glacial period to return, ice-sheets and glaciers would again extend over the same areas formerly occupied by them. This marked accord between the physical conditions of the Ice Age and those of the present, so far as the ratió of precipitation is concerned, cannot be too strongly emphasized. The old snow-fields, *mers de glace*, and local glaciers accumulated within those areas of northern and temperate latitudes where now-a-days snow and rain are precipitated most copiously; while traces of glaciation are either wholly wanting or very meagrely present in those northern and temperate latitudes which are even now notable for their dryness. It is needless to say that any theory that attempts to account for the glacial climate has these salient facts to reckon with.

The question of the origin of that climate has been greatly complicated by the rapidly increasing evidence which proves that the Ice Age was interrupted by one or more stages during which temperate conditions prevailed. So long as geologists had only one glacial epoch to account for they had less difficulty in suggesting feasible explanations. It was hard or even impossible, however, to reconcile such explanations with the occurrence of interglacial deposits. One is not sur-

prised, therefore, that for some time the evidence of climatic changes during the Ice Age should have been received with considerable doubt. That day of doubt, however, has now well-nigh passed, and geologists generally admit that there have been at least two glacial epochs, separated the one from the other by one well-marked interglacial stage. Indeed, as I shall presently point out, strong evidence has been adduced to show that three or even more glacial epochs, with intervening temperate stages, supervened during the Pleistocene period.

I have said that at least one interglacial epoch is generally admitted by geologists. But I may note here that attempts have often been made to explain away the evidence. It has been again and again suggested, for example, that the interglacial beds indicate no more than local retreats and readvances of ice-sheet and glacier, between the morainic accumulations of which the beds in question appear. This is so very obvious an explanation that it has doubtless occurred to every one who has ever had occasion to give the matter even the slightest consideration. I suppose no one who has been fortunate enough to discover an interglacial deposit has not tried first to account for its presence in this easy way. Nor is it improbable that certain beds containing arctic forms of life, and occupying an interglacial position, are to be thus explained. But there remain a large number of cases which refuse to be thus interpreted—interglacial deposits, which, according to those who have studied them on the spot, are eloquent of very considerable climatic changes. Geologists sometimes forget that in every region where glacial accumulations are well developed, good observers had recognised an upper and a lower series of "drift deposits," long before the idea of two separate glacial epochs had presented itself. Thus, in North Germany, so clearly is the upper differentiated from the lower "diluvium" that the two series had been noted and mapped as separate accumulations for years before geologists had formulated the theory of successive ice-epochs.* The division of the German "diluvium" into an upper and a lower series is as firmly established as any other well-marked division in historical geology. The stratigraphical evidence has been much strengthened, however, by the discovery between upper and lower boulder-clays of true interglacial

* Wahnschaffe: *Forschungen zur deutschen Landes- und Volkskunde von Dr. A. Kirchhoff.* Bd. vi, Heft i.

beds, containing lignite, peat, diatomaceous earth, and marine, brackish, and freshwater molluscs, fish, etc., and now and again bones of Pleistocene mammals.* A similar strongly-marked division characterises the glacial accumulations of Sweden, as has been clearly shown by De Geer,† who thinks that the older and younger epochs of glaciation were separated by a protracted period of interglacial conditions. In short, evidence of a break in the glacial succession has been traced at intervals across the whole width of the continent, from the borders of the North Sea to Central Russia. M. Krischtafowitsch has recently detected in the neighbourhood of Moscow‡ certain fossiliferous interglacial beds, the flora and fauna of which indicate a warmer or moister climate than the present. The interglacial stage, he says, must have been of long duration, and separated in Russia as in Western Europe two distinct epochs of glaciation.

No mere temporary retreat and re-advance of the ice-front can account for these phenomena. The occurrence of remains of the great pachyderms at Rixdorf, near Berlin, and the character of the flora met with in the interglacial beds of North Germany and Russia are incompatible with glacial conditions in the low grounds of Northern Europe. The interglacial beds, described by Dr. C. Weber§ as occurring near Grünenthal, in Holstein, are among the more recent discoveries of this kind. These deposits rest upon boulder-clay, and are overlaid by another sheet of the same character, and belong, according to Weber, to "that great interglacial period which preceded the last ice-sheet of Northern Europe." The section shows 8 feet of peat resting on freshwater clay, 2 feet thick, which is underlaid by some 10 feet of

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* For interglacial beds of N. Germany see Helland: *Zeitschr. d. deutsch. geol. Ges.* xxxi, 879; Penck: *Ibid.* xxxi, 157; *Länderkunde von Europa* (Das deutsche Reich) 1887, 512; Dames: *Samml. gemeinverständl. wissensch. Vorträge, von Virchow u. Holtzendorff:* xx Ser. 479 Heft; Schröder: *Jahrb. d. k. geol. Landesanst. f.* 1885, p. 219. For further references see Wahnschaffe, *op. cit.* I have not thought it worth while in this paper to refer to the interglacial deposits of our own islands. A general account of them will be found in my *Great Ice Age*, and *Prehistoric Europe.* The interglacial phenomena of the continent seem to be less known here than they ought to be.

† *Zeitschrift d. deutsch geolog. Gesellschaft*, Bd. xxxvii, p. 177.

‡ *Anzeichen einer interglazidren Epoche in Central-Russland*, Moskau, 1891.

§ *Neues Jahrbuch f. Mineralogie, Geologie, u. Palæontologie*, 1891, Bd. ii, pp. 62, 228; 1892, Bd. i, p. 114.

"coral sand," with bryozoa. The flora and fauna have a distinctly temperate facies. It is no wonder, then, that continental geologists are generally inclined to admit that North Germany and the contiguous countries have been invaded at least twice by the ice-sheets of two separate and distinct glacial epochs. This is not all, however. While every observer acknowledges that the "diluvium" is properly divided into an upper and a lower series, there are some geologists who have described the occurrence of three, and even more boulder-clays—the one clearly differentiated from the other, and traceable over wide areas. Is each of these to be considered the product of an independent ice-sheet, or do they only indicate more or less extensive oscillations of the ice-front? The boulder-clays are parted from each other by thick beds of sand and clay, in some of which fossils have occasionally been detected. It is quite possible that such stratified beds were deposited during a temporary retreat of the ice-front, which when it re-advanced covered them up with its bottom-moraine. On the other hand, the phenomena are equally explicable on the assumption that each boulder-clay represents a separate epoch of glaciation. Until the stratified beds have yielded more abundant traces of the life of the period, our judgment as to the conditions implied by them must be suspended. It is worthy of note in this connection, however, that in North America the existence of one prolonged interglacial epoch has been well established, while distinct evidence is forthcoming of what Chamberlin terms "stages of deglaciation and re-advancing ice."*

When we turn to the Alpine lands, we find that there also the occurrence of former interglacial conditions has been recognised. The interglacial deposits, as described by Heer and others, are well known. These form as definite a geological horizon as the similar fossiliferous zone in the "diluvium" of Northern Germany. The lignites, as Heer pointed out, represent a long period of time, and this is still further illustrated by the fact that considerable fluviatile erosion supervened between the close of the first, and the advent of the later glacial epoch. No mere temporary retreat and re-advance of the ice will account for the phenomena. Let us for a moment consider the conditions under which the accumulations in question were laid down.

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* *Sixth Annual Report, U.S. Geol. Survey*, 1884–85, p. 315.

The glacial deposits underlying the lignite beds, contain, amongst other erratics, boulders which have come from the upper valley of the Rhine. This means, of course, that the ancient glacier of the Rhine succeeded in reaching the Lake of Zurich; and it is well known that it extended at the same time to Lake Constance. That glacier, therefore exceeded 60 miles in length. One cannot doubt that the climatic conditions implied by this great extension were excessive, and quite incompatible with the appearance in the low grounds of Switzerland of such a flora as that of the lignites. The organic remains of the lignite beds indicate a climate certainly not less temperate than that which at present characterises the district round the Lake of Zurich. We may safely infer, therefore, that during interglacial times the glaciers of the Alps were not more extensively developed than at present. Again, as the lignites are overlaid by glacial deposits, it is obvious that the Rhine glacier once more reached Lake Zurich—in other words there was a return of the excessive climate that induced the first great advance of that and other Swiss glaciers. That these advances were really due to extreme climatic conditions is shown by the fact that it was only under such conditions that the Scandinavian flora could have invaded the low grounds of Europe, and entered Switzerland. It is impossible, therefore, that the interglacial flora could have flourished in Switzerland, while the immigration of northern plants was taking place.

Lignites of the same age as those of Dürnten and Utznach occur in many places both on the north and south sides of the Alpine chain. At Imberg, near Sonthofen, in Bavaria, for example, they are described by Penck* as being underlaid and overlaid by thick glacial accumulations. The deposits in question form a terrace along the flanks of the hills, at a height of 700 feet above the Iller. The flora of the lignite has not yet been fully studied, but it is composed chiefly of conifers, which must have grown near where their remains now occur—that is at 3,000 feet, or thereabout, above the sea. It is incredible that coniferous forests could have flourished at that elevation during a glacial epoch. A lowering of the mean annual temperature by 3° C. only would render the growth of trees at that height almost impossible,

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* *Die Vergletscherung der deutschen Alpen*, 1882, p. 256.

and certainly would be insufficient to cause the glaciers of Algau to descend to the foot of the mountains, as we know they did—a distance of at least 24 miles. The Imberg lignites, therefore, are evidence of a climate not less temperate than the present. More than this, there is clear proof that the interglacial stage was long continued, for during that epoch the Iller had time to effect very considerable erosion. The succession of changes shown by the sections near Sonthofen are as follows:—

1. The Iller Valley is filled with glacier-ice which flows out upon the low grounds at the base of the Alps.
2. The glacier retreats and great sheets of shingle and gravel are spread over the valley.
3. Coniferous forests now grow over the surface of the gravels; and as the lignite formed of their remains attains a thickness of 10 feet in all, it obviously points to the lapse of some considerable time.
4. Eventually the forests decay, and their débris is buried under new accumulations of shingle and gravel.
5. The Iller cuts its way down through all the deposits to depths of 680 to 720 feet.
6. A glacier again descends and fills the valley, but does not flow so far as that of the earlier glacial stage.

In this section, as in those at Dürnten and Utznach, we have conclusive evidence of two glacial epochs, sharply marked off the one from the other. Nor does that evidence stand alone, for at various points between Lake Geneva and the lower valley of the Inn similar interglacial deposits occur. Sometimes these appear at the foot of the mountains, as at Mörschweil on Lake Constance, sometimes just within the mountain area, as at Imberg, sometimes far in the heart of the Alpine lands, as at Innsbruck. Professor Penck has further shown, and his observations have been confirmed by Brückner, Blaas, and Böhm, that massive sheets of fluviatile gravel are frequently met with throughout the valleys of the Alps, occupying interglacial positions. These gravels are exactly comparable to the interglacial gravels of the Sonthofen sections. And it has been demonstrated that they occur on two horizons, separated the one from the other by characteristic groundmoraine or boulder-clay. The lower gravels rest on groundmoraine, and the upper gravels are overlaid by sheets of the same kind of glacial detritus. In short, three separate and distinct groundmoraines are recognised. The gravels, one cannot doubt, are simply the

torrential and fluviatile deposits laid down before advancing and retreating glaciers; and it is especially to be noted that each sheet of gravel, after its accumulation, was much denuded and cut through by river-action. In a word, as Penck and others have shown, the valleys of Upper Bavaria have been occupied by glaciers at three successive epochs—each separated from the other by a period during which much river-gravel was deposited and great erosion of the valley-bottoms was effected.

On the Italian side of the Alps, similar evidence of climatic changes is forthcoming. The lignites and lacustrine strata of Val Gandino, and of Val Borlezza, as I have elsewhere shown,* are clearly of interglacial age. From these deposits many organic remains have been obtained—amongst the animals being *Rhinoceros hemitœchus* and *R. leptorhinus*. According to Sordelli, the plants indicate a climate as genial as that of the plains of Lombardy and Venetia, and warmer therefore than that of the upland valleys in which the interglacial beds occur. Professor Penck informs me that some time ago he detected evidence in the district of Lake Garda of three successive glacial epochs—the evidence being of the same character as that recognised in the valleys of the Bavarian Alps.

In the glaciated districts of France similar phenomena are met with. Thus in Cantal, according to M. Rames,† the glacial deposits belong to two separate epochs. The older morainic accumulations are scattered over the surface of the plateau of Archæan schistose rocks, and extend up the slopes of the great volcanic cone of that region to heights of 2,300 to 3,300 feet. One of the features of these accumulations are the innumerable gigantic erratics, known to the country folk as *cimetière des enragés*. Sheets of fluvio-glacial gravel are also associated with the moraines, and it is worthy of note, that both have the aspect of considerable age—they have evidently been subjected to much denudation. In the valleys of the same region occurs a younger series of glacial deposits, consisting of conspicuous lateral and terminal moraines, which, unlike the older accumulations, have a very fresh and well-preserved appearance. With them, as with the older moraines, fluvio-glacial gravels are associated. M. Rames shows that the interval that supervened between

* *Prehistoric Europe*, p. 303.
† *Bull. Soc. Géol. de France*, 1884.

the formation of the two series of glacial deposits must have been prolonged, for the valleys during that interval were in some places eroded to a depth of 900 feet. Not only was the volcanic *massif* deeply incised, but even the old plateau of crystalline rocks on which the volcanic cone reposes suffered extensive denudation in interglacial times. M. Rames further recognises that the second glacial epoch was marked by two advances of the valley-glaciers, separated by a marked episode of fusion, the evidence for which is conspicuous in the valley of the Cère.

The glacial and interglacial phenomena of Auvergne are quite analogous to those of Cantal. Dr. Julien has described the morainic accumulations of a large glacier that flowed from Mont Dore. After that glacier had retreated a prolonged period of erosion followed, when the morainic deposits were deeply trenched, and the underlying rocks cut into. In the valleys and hollows thus excavated freshwater beds occur, containing the relics of an abundant flora, together with the remains of elephant (*E. meridionalis*), rhinoceros (*R. leptorhinus*), hippopotamus, horse, cave-bear, hyæna, etc.—a fauna comparable to that of the Italian interglacial deposits. After the deposition of the freshwater beds, glaciers again descended the valleys and covered the beds in question with their moraines.*

According to the researches of Martins, Collomb, Garrigou, Piette, and Penck, there is clear evidence in the Pyrenees of two periods of glaciation, separated by an interval of much erosion and valley-excavation. Penck, indeed, has shown that the valleys of the Pyrenees have been occupied at three successive epochs by glaciers—each epoch being represented by its series of moraines and by terraces of fluvio-glacial detritus, which occur at successively lower levels.

I have referred in some detail to these discoveries of interglacial phenomena because they so strongly corroborate the conclusions arrived at a number of years ago by glacialists in our own country. Many additional examples might be cited from other parts of Europe, but those already given may serve to show that at least one epoch of interglacial conditions supervened during the Pleistocene period. Before leaving this part of my subject, however, I may point out the significant fact that long before much was known of

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* *Des Phénomènes glaciaires dans le Plateau Central de France, etc.*

glaciation, and certainly before the periodicity of ice-epochs had been recognised, Collomb had detected in the Vosges conspicuous evidence of two successive glaciations.*

Having shown that alike in the regions formerly occupied by the great northern ice-sheet, and in the Alpine lands of Central and Southern Europe, alternations of cold and genial conditions characterised the so-called glacial period, we may now glance at the evidence supplied by those Pleistocene deposits that lie outside of the glaciated areas. Of these we have a typical example in the river-accumulations of the Rhine valley between Bâle and Bingen. Here and there these deposits have yielded remains of extinct and no longer indigenous mammals and relics of Palæolithic man—one of the most interesting deposits from which mammalian remains have been obtained, being the Sands of Mosbach, between Wiesbaden and Mayence. The fauna in question is characteristically Pleistocene, nor can it be doubted that the Mosbach Sands belong to the same geological horizon as the similar fluviatile deposits of the Seine, the Thames, and other river-valleys in Western Europe. Dr. Kinkelin has shown,† and with him Dr. Schumacher agrees,‡ that the Mosbach deposits are of interglacial age; while Dr. Pohlig has no hesitation in assigning them to the same horizon.§ It is true there are no glacial accumulations in the region where they occur, but they rest upon a series of unfossiliferous gravels which are recognised as the equivalents of the fluvio-glacial and glacial deposits of the Vosges, the Black Forest, the Alps, etc. These gravels are traced at intervals up to considerable heights above the Rhine, and contain numerous erratics, some of which are several feet in diameter, while a large proportion are not at all waterworn, but rough and sharply angular. The blocks have unquestionably been transported by river-ice, and imply therefore cold climatic conditions. The overlying Mosbach Sands have yielded not only *Elephas antiquus* and *Hippopotamus major*, but the reindeer, the mammoth, and the marmot—two strongly contrasted faunas, betokening climatic

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* *Preuves de l'existence d'anciens glaciers dans les vallées des Vosges*, 1847, p. 141.

† Kinkelin: *Bericht über die Senckenberg. naturf. Ges. in Frankfurt a. M.*, 1889.

‡ Schumacher: *Mittheilungen d. Commission für d. geolog. Landes-Untersuch. v. Elsass-Lothringen*, Bd. ii, 1890, p. 184.

§ *Zeitschr. d. deutsch. geolog. Ges.*, 1887, p. 806.

changes similar to those that marked the accumulation of the river-deposits of the Thames, the Seine, etc. Of younger date than the Mosbach Sands is another series of unfossiliferous gravels, which, like the older series, are charged with ice-floated erratics. The beds at Mosbach are thus shown to be of interglacial age: they occupy the same geological horizon as the interglacial beds of Switzerland and other glaciated tracts in Central and Northern Europe.

To this position must likewise be assigned the Pleistocene river-alluvia of other districts. There is no other horizon, indeed, on which these can be placed. That they are not of postglacial age is shown by the fact that in many places the angular gravels and flood-loams of the glacial period overlie them. And that they cannot all belong to preglacial times is proved by the frequent occurrence underneath them of glacial or fluvio-glacial accumulations. It is quite possible, of course, that here and there in the valleys of Western and Southern Europe some of the Pleistocene alluvia may be of preglacial age. But in the main these alluvia must be regarded as the equivalents of the glacial and interglacial deposits of northern and alpine districts. This will appear a reasonable conclusion when we bear in mind that long before the Pliocene period came to a close the climate of Europe had begun to deteriorate. In England, as we know, glacial conditions supervened almost at the advent of the Pleistocene period. And the same was the case in the alpine lands of the south. Again, in the glaciated areas of north and south alike, the closing stage of the Pleistocene was characterized by cold climatic conditions. And thus in those regions the glacial and interglacial epochs were co-extensive with that period. It follows, therefore, that the Pleistocene deposits of extra-glacial areas must be the equivalents of the glacial and interglacial accumulations elsewhere. If we refused to admit this we should be puzzled indeed to tell what the rivers of Western and Southern Europe were doing throughout the long-continued glacial period. There is no escape from the conclusion that the Pleistocene river-alluvia and cave-accumulations must be assigned to the same general horizon as the glacial and interglacial deposits. This is now admitted by continental palæontologists who find in the character of Pleistocene organic remains abundant proof that the old river-alluvia and cave-accumulations were laid down under changing climatic conditions. Did neither glacial nor interglacial deposits

exist the relics of the Pleistocene flora and fauna met with in extra-glacial regions would yet lead us to the conclusion that after the close of the Pliocene period, extremely cold and very genial climates alternated up to the dawn of the present. Thus during one stage of the Pleistocene "clement winters and cool summers permitted the wide diffusion and intimate association of plants which have now a very different range. Temperate and southern species like the ash, the poplar, the sycamore, the fig-tree, the judas-tree, etc., overspread all the low grounds of France as far north at least as Paris. It was under such conditions that the elephants, rhinoceroses, and hippopotamuses, and the vast herds of temperate cervine and bovine species ranged over Europe, from the shores of the Mediterranean up to the latitude of Yorkshire, and probably even further north still, and from the borders of Asia to the Western Ocean. Despite the presence of numerous fierce carnivora—lions, hyænas, tigers, and others—Europe at that time, with its shady forests, its laurel-margined streams, its broad and deep-flowing rivers, a country in every way suited to the needs of a race of hunters and fishers—must have been no unpleasant habitation for Palæolithic man." But during another stage of the Pleistocene period, the climate of our continent presented the strongest contrast to those genial conditions. At that time "the dwarf birch of the Scottish Highlands, and the Arctic willow, with their northern congeners, grew upon the low grounds of Middle Europe. Arctic animals, such as the musk-sheep and the reindeer lived then, all the year round, in the south of France; the mammoth ranged into Spain and Italy; the glutton descended to the shores of the Mediterranean; the marmot came down to the low grounds at the foot of the Apennines; and the lagomys inhabited the low-lying maritime districts of Corsica and Sardinia. The land- and freshwater molluscs of many Pleistocene deposits tell a similar tale: high alpine, boreal, and hyperborean forms are characteristic of these deposits in Central Europe; even in the southern regions of our continent the shells testify to a former colder and wetter climate. It was during the climax of these conditions that the caves of Aquitaine were occupied by those artistic men, who appear to have delighted in carving and engraving."* Such, in brief, is the testimony of the Pleistocene flora and fauna of extra-glacial regions. It is from the deposits in those regions,

* *Prehistoric Europe*, p. 67.

therefore, that we derive our fullest knowledge of the life of the period. But a comparison of their organic remains with those that occur in the glacial and interglacial deposits of alpine and northern lands shows us that the Pleistocene accumulations of glacial and extra-glacial countries are contemporaneous—for there is not a single life-form obtained from interglacial beds which does not also occur in the deposits of extra-glacial regions. The converse is not true—nor is that to be wondered at, for interglacial deposits have only been sparingly preserved. In regions liable to glaciation such superficial accumulations must frequently have been ploughed up and incorporated with groundmoraine. It was only in the extra-glacial tracts that alluvia of interglacial age were at all likely to be preserved in any abundance. To fully appreciate the climatic conditions of the Pleistocene period, therefore, it is necessary to combine the evidence derived from the glaciated areas with that obtained from the lands that lay beyond the reach of the ice-plough. The one is the complement of the other, and this being so, it is obvious that any attempted explanation of the origin of the glacial period which does not fully realise the importance of the interglacial phase of that period cannot be accepted.

But if the climatic changes of Pleistocene times are the most important phenomena which the geologist, who essays to trace the history of that period is called upon to consider, he cannot ignore the evidence of contemporaneous geographical mutations. These are so generally admitted, however, that it is only necessary here to state the well-known fact that everywhere throughout the maritime tracts of the glaciated lands of Europe and North America, frequent changes in the relative level of land and sea took place during Pleistocene and postglacial times.

I must now very briefly review the evidence bearing on the climatic conditions of postglacial times. And first, let it be noted that the closing stage of the Pleistocene period was one of cold conditions, accompanied in North-Western Europe by partial depression of the land below its present level. This is shown by the late-glacial marine deposits of Central Scotland and the coast-lands of Scandinavia. The historical records of the succeeding postglacial period are furnished chiefly by raised beaches, river- and lake-alluvia, calcareous tufas, and peat-bogs. An examination of these has shown that the climate, at first cold, gradually became less ungenial, so that the Arctic-alpine flora and northern

fauna were eventually supplanted in our latitude by those temperate forms which, as a group, still occupy this region. The amelioration of the climate was accompanied by striking geographical changes, the British Islands becoming united with themselves and the opposite coasts of the continent. The genial character of the climate at this time is shown by the great development of forests, the remains of which occur under our oldest peat-bogs. Not only did trees then grow at greater altitudes in these regions than is at present the case, but forests ranged much further north, and flourished in lands where they cannot now exist. In Orkney and Shetland, in the far north of Norway, and even in the Færöe Islands and in Iceland relics of this old forest-epoch are met with. In connection with these facts reference may be made to the evidence obtained from certain raised beaches on both sides of the N. Atlantic, and from recent dredgings in the intervening sea. The occurrence of isolated colonies of southern molluscs in our northern seas, and the appearance in raised beaches of many forms which are now confined to the waters of more southern latitudes, seem to show that in early postglacial times the seas of these northern latitudes were warmer than now. And it is quite certain that the southern forms referred to are not the relics of any preglacial or interglacial immigration. They could only have entered our northern seas after the close of the glacial period, and their evidence taken in connection with that furnished by the buried trees of our peat-bogs, leads to the conclusion that a genial climate supervened after the cold of the last glacial epoch and of earliest postglacial times had passed away.

To this genial stage succeeded an epoch of cold humid conditions, accompanied by geographical changes which resulted in the insulation of Britain and Ireland—the sea encroaching to some extent on what are now our maritime regions. The climate was less favourable to the growth of forests, which began to decay and to become buried under wide-spread accumulations of growing peat. At this time glaciers re-appeared in the glens of the Scottish Highlands, and here and there descended to the sea, as in Arran, Sutherland, and Ross. The evidence for these is quite conspicuous, for the moraines are found resting on the surface of postglacial beaches. Thus my friend Mr. L. Hinxman, of the Geological Survey, tells me that at the foot of Glen Thraill well-formed moraines are seen in section reposing on beach-deposits at the distance of about three-quarters of a

mile above the head of Loch Torridon.* The evidence of this recrudescence of glacial conditions in postglacial times is not confined to Scotland. I believe it will yet be recognised in many other mountain-regions; but already Prof. Penck has detected it in the valleys of the Pyrenees.† Dr. Kerner has also described similar phenomena in the valley of the Stubai near Innsbruck, while Professor Brückner has obtained like evidence in the Salzach region.‡

I have elsewhere traced the history of the succeeding stages of the postglacial period, and brought forward evidence of similar but less strongly-marked climatic changes having followed upon those just referred to, and my conclusions, I may add, have been supported by the independent researches of Professor Blytt in Norway. But these later changes need not be considered here, and I shall leave them out of account in the discussion that follows. It is sufficient for my present purpose to confine attention to the well-proved conclusion that in early postglacial times genial climatic conditions obtained, and that these were followed by cold and humid conditions, during the prevalence of which considerable local glaciers re-appeared in certain mountain-valleys.§

We speak of Pleistocene or glacial and of postglacial periods as if the one were more or less sharply marked off from the other. Of course, that is not the case, and in point of fact it would be for many reasons preferable to include them under some general term. Taken together they form one tolerably well-defined cycle of time, characterised above all by its remarkable climatic changes—by alternations of cold and genial conditions, that were most strongly contrasted in the earlier stages of the period. It is further worthy of note that various oscillations of the sea-level appear to have taken place again and again both in the earlier and later stages of the cycle.

We may now proceed to inquire whether the phenomena

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* For Scottish postglacial glaciers see J. Geikie: *Scottish Naturalist*, Jan., 1880; *Prehistoric Europe*, pp. 386, 407; Penck: *Deutsche geographische Blätter*, Bd. VI, p. 323; *Verhandlung d. Ges. f. Erdkunde, Berlin*, 1884, Heft i.

† Die Eiszeit in den Pyrenäen: *Mittheil. d. Vereins f. Erdkunde*, Leipzig, 1883.

‡ Eiszeit-studien in den sudöstlichen Alpen: *X. Jahresbericht d. geograph. Ges. v. Bern*, 1891.

§ For a full statement of the evidence see *Prehistoric Europe*, Chaps. xvi, xvii.

we have been considering can be accounted for by movements of the earth's crust—a view which has recently received considerable support, more especially in America. I need hardly say that the view in question is not a novelty. Many years ago, while our knowledge of Pleistocene phenomena was somewhat rudimentary, it was usual to infer that glaciation had been induced by elevation of the land. This did not seem an unreasonable conclusion, for above our heads, at a less or greater elevation, according to latitude, an Arctic climate prevails. One could not doubt, therefore, that if a land-surface were only sufficiently uplifted it would reach the snow-line, and become more or less extensively glaciated. But with the increase of our knowledge of Pleistocene and postglacial conditions, such a ready interpretation failed to satisfy, although not a few geologists have continued to defend the "earth-movement hypothesis," as accounting fairly well for the phenomena of the glacial period. By these staunch believers in the adequacy of that view, it has been pointed out that elevation might not only lift lands into the region of eternal snow, but, by converting large areas of the sea-bed into land, would greatly modify the direction of ocean-currents, and thus influence the climate. What might not be expected to happen were the Gulf Stream to be excluded from northern regions? What would be the fate of the temperate latitudes of North America and Europe were that genial ocean-river to be deflected into the Pacific across a submerged Isthmus of Panama? The possibility of such changes having supervened in Pleistocene times has often been present to my mind, but I long ago came to the conclusion that they could not account for the facts. Moreover, I have never been able to meet with any evidence in favour of the postulated "earth-movements." Having carefully studied all that has been advanced of late years in support of the hypothesis in question I find myself more than ever constrained to oppose it, not only because it is grounded on no basis of fact, but because it altogether fails to explain the conditions that obtained in Pleistocene and postglacial times.

There are various forms in which the hypothesis has appeared, and these I shall now consider seriatim, and with such brevity as may be. It has been maintained, for example, that at the advent of the glacial period vast areas of Northern and North-Western Europe, together with enormous regions in the corresponding latitudes of North America,

stood several thousand feet higher than at present. But when we ask what evidence can be adduced to prove this we get no satisfactory reply. We are simply informed that a glacial climate must have resulted from great elevation, and that the latter, therefore, must have taken place at the beginning of the glacial period. Some writers, however, have ventured to give reasons for their faith. Thus Mr. W. Upham, pointing to the evidence of the fiords of North America, and to the fact that drowned river-valleys have been traced outwards across the 100-fathom line of the marginal plateau to depths of over 3,500 feet, maintains that the whole continent north of the Gulf of Mexico stood at the commencement of the glacial period some 3,000 feet at least higher than now. Of course he cites the fiords of Europe as evidence of a similar great upheaval for the northern and north-western regions of our continent. Mr. Upham even favours the notion that during glacial times a land-connection probably existed between North America and Europe, by way of the British Islands, Iceland, and Greenland. When "this uplifting attained its maximum, and brought on the glacial period," he says, "North America and North-Western Europe stood 2,500 to 3,000 feet above their present height."*

That fiords are simply submerged land-valleys has long been recognised: that they have been formed mainly by the action of running water—just in the same way as the mountain-valleys of Norway and Scotland—has been the belief for many years of most students of physical geology. But it is hard to understand why they should have been cited by Mr. Upham in support of his contention, seeing that their evidence seems to militate strongly against the very hypothesis he strives to maintain. No one acquainted with the physical features and geological structure of Scotland and Norway can doubt that the valleys which terminate in fiords are of great geological antiquity. Their excavation by fluviatile action certainly dates back to a period long anterior to the advent of the Ice Age. And a like tale is told by the fiords and drowned valley-troughs of North America, which cannot be referred to so recent a period as post-Tertiary times, Those who are convinced that our continental areas have persisted throughout long æons of geological time, and that rivers frequently have survived great geological revolutions—cutting their way across mountain-elevations as fast as these

* *American Geologist*, vi, p. 327.

were uplifted—will readily believe that some of the submarine river-troughs of North America, such as that of the Hudson, may belong even to Secondary times.* It would be hard to say at what particular date the excavation of the Scottish highland valleys commenced—but it was probably during the later part of the Palæozoic era. The process has doubtless been retarded and accelerated frequently enough, during successive movements of depression and elevation, but it was practically completed before the beginning of Pleistocene times, and that is all that we may trouble about here. Precisely the same conclusion holds good for Norway: and such being the case it is obvious that the origin and age of the fiords have no bearing whatever on the problem of the glacial climate and its cause. In point of fact, the evidence, as already remarked, tells against the "earth-movement hypothesis" for it shows us that, during a period when Europe and North America stood several thousand feet higher, and extended much further seawards, rivers, and not glaciers, were the occupants of our mountain-valleys. It was not until all those valleys had come to assume much the appearance they now present that general glaciation supervened.

We are not without direct evidence, however, as to the geographical conditions that obtained in the ages that immediately preceded the Pleistocene period. The distribution of the Pliocene marine beds of Britain entitles us to assume that at the time of their accumulation our lands did not extend quite so far to the south and east as now. The absence of similar deposits from the coast-lands of North America is supposed to support the view of great continental elevation in pre-glacial times. All it seems to prove, however, is that in Pliocene times the North American continent was not less extensive than it is at present. It is even quite possible that in glacial times pre-existing Pliocene beds may have been ploughed out by the ice, just as seems to have been the case in the north-east of Scotland. But without going so far back as Pliocene times, we meet with evidence almost everywhere throughout the maritime regions of the glaciated areas of Europe and North America, to show that immediately before those tracts became swathed in ice the geographical conditions were much the same as at present. The shelly

* Professor Dana inclines to date the erosion of the Hudson Trough so far back as the Jura-Trias period. *American Journ. Science.* xl. (1890), 435.

boulder-clays in various parts of our islands, and the similar occurrence of marine and brackish-water shells in and underneath the "diluvium" of North Germany, etc., proves clearly enough that just before the coming-on of glacial conditions neither Britain nor the present maritime lands of the continent were far removed from the sea. It is true that the buried river-channels of Scotland indicate a preglacial elevation of some 200 or 300 feet above the existing sea-level, but it is quite certain that the Minch, St. George's Channel, the Irish Sea, the North Sea, and the Baltic, were all in existence at the commencement of the glacial period. And we are led to similar conclusions with regard to the geographical conditions of North America at that time, from the occurrence of marine shells in the boulder-clays of Canada and New England.

Thus there appears to be no evidence either direct or indirect in favour of the view that glacial conditions were superinduced by great continental elevation. But it may be argued that even although no evidence can be cited in proof of such elevation, still, if the glacial phenomena can be well explained by its means, we may be justified in admitting it as a working hypothesis. Movements of elevation and depression have frequently taken place—the Pleistocene marine deposits themselves testify to oscillations of the sea-level—and there can be no objection, therefore, to such postulations as are made by the hypothesis under review. All this is readily granted, but I deny that the conditions that obtained in Pleistocene times can be accounted for by elevation and depression. Let us see how the desiderated elevation of northern lands would work. Were North-Western Europe and the corresponding latitudes of North America to be upheaved for 3,000 feet, and a land-passage to obtain between the two continents by way of the Færöe Islands, Iceland, and Greenland, how would the climate be affected? It is obvious enough that under such changed conditions the elevated lands in higher latitudes might well be subjected to more or less extensive glaciation. Norway would become uninhabitable and glaciers might well appear in the mountain-valleys of Scotland. But it may be doubted whether the climate of France and Spain, or the corresponding latitudes of North America would be much affected. For were a land-passage to appear between Britain and Greenland no Arctic current would flow into the North Atlantic, while no portion of the Gulf-stream would be lost in Arctic seas. The North Atlantic

would then form a great gulf round which a warm ocean-current would circulate. The temperature of that sea, therefore, would be raised and the prevailing westerly and south-westerly winds of Europe would be warmer than now. However much such warm moist winds might increase the snowfall in North Britain and Scandinavia, we cannot suppose they could have much influence in Central and Southern Europe, and in North Africa; and still less could they affect the climate of Asia Minor and the mountainous regions of the far east, in most of which evidence of extensive glaciation occurs. And how, we may ask, could the postulated geographical changes bring about the glaciation of the mountainous tracts on the Pacific sea-board? In fine, we may conclude, that however much the geographical changes referred to might affect North-Western Europe and North-Eastern America, they are wholly insufficient to account for the glacial phenomena of other regions. The continuous research of recent years has shown that the lowering of temperature of glacial times was not limited to the lands which would be affected by any such elevation as that we are considering. A marked and general displacement of climatic zones took place over the whole continent of Europe; and similar changes supervened in North America and Asia. Are we then to suppose that all the lands within the Northern Hemisphere were extensively and contemporaneously upheaved?

We may now consider another form of the earth-movement hypothesis. It has frequently been suggested that our glacial phenomena may have been caused by the submergence of the Isthmus of Panama, and the deflection of the Equatorial Current into the Pacific. But it may be doubted whether a submergence of that Isthmus, unless very extensive indeed, would result in more than a partial escape of Atlantic water into the Pacific Basin. The Counter Current of the Pacific which now strikes against the Isthmus might even sweep into the Caribbean Sea, and join the Equatorial on its way to the Gulf of Mexico. But putting that consideration aside, what evidence have we that the Isthmus of Panama was submerged during the glacial epoch? None whatsoever, it may be replied. It is only a pious opinion. Considerable movements of elevation and depression of the islands in the Caribbean Sea would seem to have taken place at a comparatively recent date, but those movements may quite well belong to Pliocene times. Whether they be of Pliocene or Pleistocene age, however, no one has yet

proved that the Isthmus of Panama was sufficiently submerged, either at the one time or the other, to permit the escape of the Atlantic Equatorial into the Pacific Basin. But let it be supposed that the Isthmus has become so deeply submerged that the Equatorial Current is wholly deflected, and that no Gulf-stream issues through the Straits of Florida to temper the climate of higher latitudes. What would result from such an unhappy change? Can any one, conversant with the geographical distribution of the glacial phenomena, imagine that the conditions of the glacial period could be thus reproduced? Norway might indeed become a second South Greenland, and perennial snow and ice might appear in the mountainous tracts of the British Islands. The climate of Hudson's Bay and the surrounding lands might be experienced in the Baltic and its neighbourhood, and what are now the temperate latitudes of Europe, north of the 50th parallel, would possibly approach Siberia in character. But surely these changes are not comparable to the conditions of the glacial period. The absence of a Gulf-stream would not sensibly affect the climate of South-Eastern Europe and Asia, and could not have the smallest influence on that of the Pacific coast-lands of North America.

Yes, but if we conceive the submergence of the Isthmus of Panama to coincide with great elevation of Northern lands, would not such geographical conditions bring about a glacial epoch comparable to that of Pleistocene times? It is hard to see how they could. No doubt, the climate of all those regions that would be affected by the withdrawal of the Gulf-stream alone would become still more deteriorated if they stood some 3,000 feet higher than now. A vast area in the north-west of Europe would certainly be uninhabitable; but it is for the advocates of the "earth-movement hypothesis" to explain why those inhospitable regions should necessarily be covered with an ice-sheet. For the production of great snow-fields and continental ice-sheets, considerable precipitation, no less than a low temperature, is requisite. Under the conditions we have been imagining, however, precipitation would probably be much less than it is at present. But to whatever extent North-West Europe might be glaciated, it is obvious that the geographical revolutions referred to could have little influence on the climate of South-Eastern Europe, not to mention Central and Eastern Asia. Nor could they possibly influence the climate of the Pacific coast-lands of North America. And yet, as is well-

known, the climate of all those regions was more or less profoundly affected during the glacial period. To account for the wide-spread evidences of glaciation by means of elevation it would therefore seem necessary to infer that all the affected areas were in Pleistocene times uplifted *en masse* into the Arctic zone that stretches above our heads. Now it seems easier to believe that the snow-line was lowered by several thousand feet than that the continents were elevated to the same extent. Glaciation, as we have seen, was developed in the same directions and over the same areas as we should expect it to be were the snow-line to be generally depressed. To put it in another way, were the snow-line by some means or other to be lowered over Europe, Asia, and North America, then, with sufficient precipitation, great ice-fields and glaciers would re-appear in the very regions which they visited during Pleistocene times. Neither elevation nor depression of the land would be required to bring about such a result. Certain advocates of the earth-movement hypothesis, however, do not maintain that all the glaciated areas were uplifted at one and the same time. The glaciation of the Alps, they think, may have taken place earlier or later than that of North-Western Europe, while the ice-period of the Rocky Mountains may not have coincided with that of Eastern North America. It is not impossible, they suppose, that the glaciation of the Himalaya may have been caused by an uplifting of that great chain, quite independent of similar earth-movements in other places. It can be demonstrated, however, that the glaciation of the Alps and of Northern Europe were contemporaneous and the facts go far to prove that the glaciers of the Rocky Mountains and the inland-ice of North-East America likewise co-existed. At all events all the old glacial accumulations of our hemisphere are of Pleistocene age, and it is for the advocates of the hypothesis under review to prove that they are not really contemporaneous. Their doubts on the subject probably arise from the simple fact that they are well aware how highly improbable or even impossible it is that all those glaciated lands could have been pushed up within the snow-line at one and the same time.

Let me, however, advance to another objection. We know that the glacial period was interrupted by at least one interglacial epoch of temperate and even genial conditions. Two glacial epochs with one protracted interglacial epoch are now generally admitted. How do the supporters of the earth-

movement hypothesis explain this remarkable succession of climatic changes? Their views as to the cause of glacial conditions we have considered. If we can believe that the glacial phenomena were due to elevation of the land, then we need have no difficulty in understanding how glacial conditions would disappear when the continents again subsided to a lower level. Not only did North America and Europe lose all their early glacial elevation, but by a lucky coincidence the Isthmus of Panama re-appeared, and the Gulf-stream resumed its beneficent course into the North Atlantic. This we are to suppose was the cause of the interglacial epoch. But I would point out that the geographical conditions which are thus inferred to have brought about the disappearance of the glacial climate, and to have ushered in the interglacial epoch are precisely those that now obtain —and, nevertheless, we are not yet in the enjoyment of a climate like that of interglacial times. The strangely equable conditions that permitted the development of the remarkable Pleistocene flora and fauna are not experienced in the Europe of our day. And what about the second glacial epoch? Are we to suppose that once more the lands were greatly uplifted, and that convenient Isthmus of Panama again depressed? Did the Alps, the Pyrenees, and the Plateau of Central France—in all of which we have distinct evidence of at least two glacial epochs—did these heights, one may ask, rise up to bring about their earlier glaciation, sink down again to induce interglacial conditions, and once more become uplifted at the succeeding cold epoch, to subside eventually in order to cause a final retreat of their glaciers?

But the climatic changes to be accounted for were in all probability more numerous and complex than those just referred to. Competent observers have adduced unmistakeable evidence of three epochs of glaciation in the alpine lands of Europe. And we are not without distinct hints that similar changes have taken place in Northern and North-Western Europe. Nor in this connection can we ignore the evidence of several interglacial episodes which Mr. Chamberlin and others have detected in the glaciated tracts of North America. Even this is not all, for the upholders of the earth-movement hypothesis have still further to account for the climatic oscillations of postglacial times. If it be hard enough to allow the possibility of one great movement of elevation having affected so enormous an area of our

hemisphere, if we find it extremely difficult to believe either that one such wide-spread movement, or that a multitude of local movements, each more or less independent of the other, could have lifted the glaciated regions successively within reach of the snow-line—we shall yet find it impossible to admit that such remarkable upheavals could be repeated again and again.

We seem driven to conclude, therefore, that the earth-movement hypothesis fails to explain the phenomena of Pleistocene times. One cannot deny, indeed, that glaciation might be induced locally by elevation of the land. It is quite conceivable that mountains now below the limits of perennial snow might come to be ridged up to such an extent as to be capable of sustaining snow-fields and glaciers. And such local movements may possibly have happened here and there during the long-continued Pleistocene period. But the glacial phenomena of that period are on much too grand a scale, and far too widely distributed to be accounted for in that way. And if the occurrence of even one glacial epoch cannot be thus explained, we may leave the supporters of the earth-movement hypothesis to show us what light is thrown by their urim and thummim on the origin of succeeding interglacial and glacial climates.

While we have no evidence of wide-spread elevation having coincided with glacial conditions, proofs of subsidence are almost everywhere associated with the glacial phenomena of the maritime districts of North America and Europe. Raised beaches and marine deposits are traced on the coasts of North America, from an elevation of 50 feet or so in Southern New England up to 75–100 ft. near Boston; of 200 ft. or thereabout in Maine; of 520 ft. at Montreal; of 1,500 ft. in Labrador; and of 1,000–2,000 ft. in Arctic regions. None of the raised beaches of glacial age met with in Europe reaches such an elevation as these last—the highest being met with in Norway at 580 ft. or thereabout. Marine shells occur in the glacial series of Scotland at a height of 500 ft., but the highest raised beach of the period does not exceed 100 ft. in elevation. It is doubtful if all those indications of submergence can be assigned to one and the same stage of the glacial period. So far as regards Scotland they certainly belong to separate stages. Thus the shell-beds at 500 ft. are of interglacial age—they rest upon and are covered by boulder-clay, while the 100 ft. beach pertains to the close of the last glacial epoch. But putting such considerations aside, it

must be admitted that considerable submergence of the land took place in glacial times. The advocates of the earth-movement hypothesis naturally attach much importance to this evidence. If it can be shown that the crust of the earth has been depressed in northern regions to depths of 1,500 to 2,000 ft. it is less hard to believe that at other times it may have been uplifted to as great an extent above its present level. We have seen, therefore, that they do not hesitate to infer that, in early glacial times, North America and the north-western regions of Europe, if not a still larger area of that continent, stood some 3,000 ft. or so higher, and that those regions subsequently became submerged to the depths indicated by the raised beaches. The amount of subsidence in New England must therefore have amounted, according to this view, to more than 3,000 ft., say 3,200 ft., in Canada to 3,500 ft., in Labrador and the far north to 4,500 or 5,000 ft. In North-West Europe likewise the earth-movement must have ranged between 3,500 and 3,600 ft. Fortunately for mankind, our continents, when re-elevation ensued, were not uplifted to the great height which they are supposed to have attained at the beginning of the glacial period.

The remarkable association of evidence of glaciation with proofs of submergence has long been noted by geologists, and various attempts have been made to show that the drowning of the lands may have been caused by the great ice-sheets. Thus Croll and others have maintained that vast accumulations of ice in northern latitudes would tend to displace the earth's centre of gravity, and thus cause the sea to rise on the glaciated hemisphere. This is probably a *vera causa*, but it is very doubtful if it can account for the extreme submergence indicated by the more elevated raised beaches. Again, it has been supposed that the attractive influence of the great ice-sheets would bring about a deformation of the sea-level, but, as Dr. Drygalski has shown, this cause is quite insufficient to account for the amount of submergence which is known to have taken place. But the view which has met with most acceptance is that advocated by Mr. Jamieson, who thinks that the earth's crust was simply pressed down under the weight of overlying ice-masses. Even those geologists who most distrust Sir William Thomson's conclusion that the earth is substantially solid may well hesitate before they admit the feasibility of Mr. Jamieson's hypothesis. Were the crust so readily deformed as he supposes, it is hard to understand how great mountain-chains can be supported

above the surrounding low grounds, or how, indeed, continents can rise above abysmal oceanic depressions. Professor George Darwin has lately shown that the prominent inequalities of the earth's surface could not be sustained unless the crust be as rigid as granite for a depth of 1,000 miles. "If the earth," he remarks, "be solid throughout, then at 1,000 miles from the surface the material must be as solid as granite. If it be fluid or gaseous inside, and the crust 1,000 miles thick, that crust must be stronger than granite, and if only 200 or 300 miles in thickness much stronger than granite. This conclusion is obviously strongly confirmatory of Sir William Thomson's view that the earth is solid throughout." Now if the crust have anything like the solidity attributed to it by Professor Darwin—if there be no liquid stratum underlying a relatively thin crust, Mr. Jamieson's hypothesis cannot be maintained. The connection between glaciation and submergence, if it be not a mere coincidence, still remains, therefore, to be explained. Recently, however, a new interpretation of the facts, which may possibly approve itself to physicists, has been advanced by Dr. Drygalski. This author is of opinion that a thick ice-sheet, by reducing the temperature of the underlying crust, would cause this to contract, and so bring about subsidence. The resulting depression of the surface would continue so long as the ice-sheet endured, but after it had disappeared free radiation of earth-heat would be resumed, the depressed isogeotherms would rise, and a general warming of the upper portion of the lithosphere would take place. But the space occupied by the depressed section, owing to the spheroidal form of the earth, would be smaller than that which it filled before sinking had commenced, and consequently, when the ice vanished, expansion of the crust would follow, and the land-surface would then rise again. But it might not be able to attain its former elevation, and it is quite conceivable that the amount of elevation might vary throughout the newly risen area. If this explanation should commend itself to physicists it would be welcomed by geologists, for it is more readily reconcilable with the facts than any other which has yet been advanced. Especially would it throw some light on that irregular deformation to which the region of the great lakes of North America seems to have been subjected in glacial times.

The advocates of the earth-movement hypothesis have gladly hailed Mr. Jamieson's view as being in perfect harmony with theirs. They are under the impression that it gets them

out of a difficulty. Having postulated an amount of elevation for which no evidence can be cited, but which they conceive necessary for the generation of great ice-sheets and glaciers, they next attribute the subsidence of the highly elevated continents to the weight of those ice-masses. The ice-sheets, in fact, are supposed to have brought about their own destruction. Thus the responsibility for the various earth-movements required by the hypothesis is partly shifted from Pluto's shoulders. We first have great continental uplifts induced by subterranean action; next, the lands sink down again under their load of snow and ice. Thus reduced in elevation they cease to favour the accumulation of snow and ice, whereupon the *mers de glace* melt away, and the overburdened crust, relieved of its load, again rises. It seems all very simple and plausible, but let us see what it involves. The thickness attained by the European ice-sheet in the basin of the North Sea probably did not exceed 3,500 ft. or 4,000 ft.; and if we take 3,000 feet as its average thickness throughout the whole area covered by it we shall certainly be over the mark. Now let it be remembered that at the beginning of the Ice Age Europe is supposed to have stood some 3,000 feet higher than at present, and to have subsequently become depressed for some 500 or 600 feet below the existing sea-level. In other words, we are asked to believe that an ice-sheet, not 3,000 feet thick, succeeded in pressing down the crust of the earth to the extent of 3,500 or 3,600 feet! The North American ice-sheet was considerably greater than ours, but even allowing it to have been three times thicker, we shall yet hardly be persuaded that it could possibly depress the crust for 3,000 to 5,000 feet. We may safely conclude, then, that if the raised beaches and marine beds of the Atlantic borders owe their origin to submergence caused by the weight of ice-sheets, the continents could not have been so highly elevated at the advent of glacial conditions. On the other hand, if we accept the hypothesis of former great elevation of the land, then we must infer that the subsidence indicated by the raised beaches cannot have resulted from the pressure of the ice-sheets.

There are many other objections to the earth-movement hypothesis which the limits of this paper forbid me entering upon. But those already indicated may suffice to show that the hypothesis is not only baseless but wholly fails to explain the facts, most of which, indeed, tell strongly against it. It accounts neither for the wide-spread phenomena of the Ice

Age, nor for the remarkable climatic conditions of interglacial times. Finally, it throws no light whatsoever on the fact that cold and genial climates alternated during the Pleistocene and postglacial periods.

The PRESIDENT.—I will now ask you to accord your thanks to Professor Geikie for his Paper, and also to Mr. Chisholm, who has so kindly read it in the Author's unavoidable absence. (Applause.) I now invite remarks on the Paper, and am glad to see that many geologists are present.

Professor E. HULL, LL.D., F.R.S.—As I come within the category of geologists, and as this is a subject I have had before me for a good many years, especially in my official capacity on the Geological Survey, I am very pleased to take part in this discussion. We are certainly favoured this evening in having an elaborate Paper on the subject of which the Author may be considered the chief exponent amongst British geologists. Professor James Geikie has made the subject of glaciation his own, to a great extent, by the publication of his well-known work *The Great Ice Age*, and this Paper contains so much that is interesting—and that calls for discussion—a good deal of which I acknowledge was previously unknown to me, that I listened to it with great interest. He leaves us, however, very much in the position, as regards the question of the origin of the Great Ice Age, in which we were before the Paper was read. He combats a view, or an interpretation, of that cause which we must not forget was originated, or at any rate elaborately maintained, by so distinguished an observer and interpreter of natural phenomena as Sir Chas. Lyell; and of course when the Author combats a view which has been elaborately defended and maintained by so great an authority on Physical Geology and Geography of past times, as Lyell, we must feel that he is treading on very dangerous ground; and for my part I fully expected that if my old friend and brother colleague, Professor Geikie, endeavoured in this Paper to demolish what he calls "the Earth-movement hypothesis," he would have presented us with

something in its place which would have given us a more clear and adequate idea of the causes which brought about this remarkable epoch in the earth's history which immediately preceded, or was partly contemporaneous with, the appearance of man. The Author, however, has not done so, as he may have considered that this was not within the scope of his Essay. He endeavours to show that the Earth-movement hypothesis is untenable, but he does not give us anything in its place. The very distinguished physicist and astronomer, Sir Robert Ball, has within recent times given us from his (an astronomical) point of view, an hypothesis to account for this remarkable period, and, I supposed or hoped, that perhaps Professor Geikie would have discussed Sir Robert Ball's hypothesis. Again, we also know that there is Croll's hypothesis, also of an astronomical character, and as Dr. Croll was a fellow-countryman of Professor Geikie's, I had also supposed that he was prepared either to maintain or to argue against Dr. Croll's hypothesis. Under these circumstances I shall not, on the present occasion, attempt to offer to the Institute any hypothesis: it is not my province to do so, but I would point out one or two arguments in defence of the Earth-movement hypothesis.

I do not understand why it is that the Author supposes 3,000 feet as the necessary elevation of the earth's surface. He says, in order to bring about the glacial condition of the Great Ice Age, it was necessary that the Northern hemisphere should have been elevated 3,000 feet. It seems to me that this is carrying your demand for elevation very much beyond what is at all necessary. For my part, I think it could be very easily shown that an elevation of 1,000 feet would probably cause such a change in the climatic conditions of the Northern hemisphere that a very large amount of glaciation would take place amongst the mountainous regions of Europe and the British Islands, which would also have a very material effect on the climate of the adjoining lands to the southward. I do not see, therefore, that it is necessary to demand such an enormous general elevation as that of 3,000 feet.

Then, as to the movement of the earth's crust. We have in the British Islands the most clear evidence that the inter-glacial epoch, of which Professor Geikie speaks, was contemporaneous with a depression of the land surface, amounting to at least 1,300 feet, because beds of sand and gravel with marine shells have been found, both on the mountains of Ireland and of North Wales, at

an elevation of 1,300 feet above the present level of the sea, and in several other places at a lower level; and those shells are certainly referable to the inter-glacial stage. So that this one fact shows that the earth's crust is capable, in comparatively recent periods (speaking geologically of course), of undergoing considerable alternations of elevation. I will not go farther into this topic because I fear, if I did, I should occupy too long a time; but I should like to refer to one effect which the glacial epoch of the Northern hemisphere had upon the regions which Professor Geikie calls extra-glacial. He, in this Paper, has very clearly defined what were the limits of these great ice sheets in Europe and adjoining countries; but the point I wish to refer to is to show the effect which the glaciation of Northern Europe must have had on the regions immediately to the south of the great ice sheets. Now when travellers explore the central and northern parts of Africa, Arabia Petræa, the Great Arabian Desert and Palestine, they are struck by the fact that those regions which are extra-glacial, are traversed by magnificent valleys which were once, undoubtedly, the channels of considerable rivers. Along the bottoms of those valleys we have alluvial strata in great terraces, extending from side to side—perhaps two or three miles in width, with well-defined banks on either side; yet those valleys are now absolutely dry, or almost dry. The rivers are dispersed; and we ask ourselves—was there a time when these great river valleys, which, for example, traverse the Sinaitic Peninsula and Southern Palestine for many miles, were filled with streams? No geologist can hesitate as to the answer to that question. Every geologist will say, at once, "Yes, there must have been rivers occupying those channels." The interesting point connected with the subject is that we have to refer to this glacial period as affording us an explanation of the mode of formation of these great river valleys. We can quite understand that if the northern half of Europe and the Lebanon were covered with perennial snows and glaciers, the climate of the regions to the south of them would be very different to what it is at present. They would, in fact, have a climate similar to that of the British Islands at the present day. Instead of being absolutely rainless, or nearly so, they would have their proportionate rainfall, as is the case with our own country. Therefore, we have in the glacial period a very interesting explanation, as it seems to me, of the occurrence of these valleys

which are now dry, but which were formerly filled with streams. That is the special inference I wish to draw from this Paper; which exhibits a wide knowledge of the subject it treats of.

The Rev. W. B. Galloway, M.A., urged the greater probability, in his opinion, of the older views of Cuvier and Buckland, which accounted for the phenomena in question by a universal Deluge. He alluded to the mammoth found in the River Lena, as making against the long periods of time required by the glacial theory, and suggested that the deluge had been caused by a change in the earth's axis, mentioning that this appeared to have been the view of the great astronomer Halley, who read a paper on the subject in 1694, which appeared in the Transactions in 1724.

Professor J. Logan Lobley, F.G.S.—The Paper, so far as it goes, is noteworthy for its elaboration and clearness of expression and for the weighty argument that it brings against the Earth-movement theory to account for the climate of the Glacial Period; but I could have wished it had advanced some hypothesis to explain the cause of that very remarkable epoch. An elevation of 1,000 feet would, as Professor Hull suggests, doubtless cause a great alteration of climate, and might produce such masses of ice and snow as would account for much of the phenomena we observe; but I would point out that the depression which has been deduced from the presence at high levels of recent shells on Moel Tryfaen and other places, has been disputed. At a recent meeting of the Geological Society a paper was read, in which the occurrence of these shells was attributed to the elevating action of ice, so that we can scarcely accept the great depression and elevation in question as absolutely proved. Too much, I think, is made of the Gulf Stream and its effects on the climate of North-West Europe. I attribute our mild climate not so much to the action of the Gulf Stream as to the general flow from the south of warm water through the North Atlantic, and to the south-west winds that come over those warmer waters. To Professor James Geikie is due great credit for having investigated the phenomena produced by the Glacial Period, but it is evident that still further observations are required, before we can come to any satisfactory conclusion on this interesting subject.

Mr. G. G. Chisholm.—Professor Logan Lobley mentions one possible way of accounting for beds of mollusca at considerable elevation, and he implies that the mere fact of those beds of

mollusca existing at that elevation is no proof that the land was depressed to such an extent as to allow of their being deposited there, and refers to a suggestion that they may have been forced up by other action. I should say that much would depend upon the precise position in which the beds were found, and the indications afforded by the surrounding circumstances, as to the manner in which deposits were made, and I feel no doubt that Professor Geikie would hardly have spoken of such beds of mollusca being deposited by the sea instead of being pushed up by the ice, if he had not thought that the evidence was sufficient for their being deposited in that manner. As to the idea that mollusca or small boulders can be pushed up to considerable elevations by the means of ice, I have myself heard Professor Geikie point out instances of that kind, and he has cited examples of small boulders that must have travelled from all parts of the North of Scotland down the valleys and up the mountains, and so forth, under the action of ice; so I do not think that Professor Lobley's supposition of the possibility of mollusca beds being found in the position in which they are found, would have been absent from Professor Geikie's mind; only in regard to the particular mollusca beds to which he has referred I suppose his impression was that the evidence was not favourable to the idea of that mode of deposition. I will make one more remark as to Professor Lobley's observation concerning Professor Geikie's use of the term "Gulf Stream" as applied to the agency which undoubtedly moderates the climate of Western Europe. It is safe to say that in using that expression, Professor Geikie was quite aware of the fact that as a distinct marine river, the Gulf Stream can hardly be said to reach the shores of Western Europe at all. It is well known that as a marine river the Gulf Stream cannot be detected further north than between the latitudes 40° and 50°, but for all that the effect of the Gulf Stream on Western Europe must be very considerable indeed, for the great body of heated water which leaves the Gulf of Mexico by the Straits of Florida, and then flows as a distinct marine river into a considerably higher latitude, must modify the temperature of the surface or drift currents which succeed the Gulf Stream proper in still more northerly seas, and of the winds that blow over those seas and carry their temperature to more northern regions.

The Meeting was then adjourned.

COMMUNICATION

From Mr. WARREN UPHAM; Assistant, United States Geological Survey.

The very important Paper by Professor Geikie I have read with the greatest interest, since his conclusions as to the probable causes of the accumulation of the ice-sheets of the Glacial period differ so widely from the views which from much observation and study I have come to hold with a good degree of confidence. He has devoted this Paper to the exposition of the difficulties and objections which beset my explanation of ice-accumulation as due to climatic conditions, chiefly the prevalence of snowfall during nearly all the year, attendant upon great elevation of the regions that became glaciated.

Most of these difficulties I cheerfully acknowledge, and yet think that the evidences of such Pleistocene elevation of North America and North-Western Europe are decisive. The researches of N. H. Winchell, McGee, Chamberlin, Salisbury, Leverett, and myself, in the United States indicate the divisibility of the Glacial period into at least two epochs of glaciation, divided by a long interglacial epoch, when the North American ice-sheet may have been entirely melted away. We thus agree with Professor Geikie, the late Dr. Croll, Wahnschaffe, Penck, De Geer, and other European glacialists, who find similar proofs of two or more glacial epochs, separated by intervals of mild climate. This repetition of the conditions producing ice-accumulation is justly insisted on by Professor Geikie as the strongest objection that can be urged against its explanation by high uplifts of the land. The relationship, however, which I suppose to have existed between the earth's contraction and the processes of mountain-building, whereby the earth-movements producing high altitude and glaciation were induced, may well have caused ice-sheets to be accumulated successively upon various parts of the earth's surface, not necessarily nor indeed probably existing at the same time upon all drift-bearing countries; and after an interglacial epoch, the same conditions might, as I have shown, be renewed upon any given area, as in North America and North-Western Europe. The supposed difficulties on account of widely distributed areas of glaciation and repetitions of ice-accumulation are duly considered in my *Probable Causes of Glaciation*, published

as an appendix in Professor G. Frederick Wright's *Ice Age in North America*. I may also mention, as treating this subject, in addition to the paper in the *American Geologist* cited by Professor Geikie, my articles in the *American Journal of Science*, III, vol. xli, pp. 33–52, Jan., 1891, and *Popular Science Monthly*, vol. xxxix, pp. 665–678, Sept., 1891.

But if the supposed interglacial beds are more properly to be referred to oscillations of the ice-front during a single glacial epoch, as is held by Wright, Lamplugh, Falsan, and others, there would be no such repetition of uplifting of the glaciated regions.

The vertical extent of the uplift needed to reinstate the Glacial period in Europe and North America, would be probably 3,000 to 5,000 feet, as Prof. T. G. Bonney has shown that an average lowering of the temperature of Europe by 18° Fahr. and of the northern part of North America by 13° would suffice. Though Professor Geikie is inclined to relegate the time of land elevation shown by the fjords to some epoch long antecedent to the Ice age, I feel sure that they can be proved to be of Pleistocene age. In North America submerged river valleys both on the Atlantic and Pacific Coasts extend to the depth of 3,000 feet beneath the present sea level; and the Sogne fjord, the longest in Norway, has, according to Mr. T. F. Jamieson (*Geol. Mag.*, III, vol. viii, p. 390, Sept., 1891), a depth of 4,080 feet. These glaciated countries stood lately at least 3,000 to 4,000 feet above their present height. This very remarkable condition and the equally extraordinary accumulation of ice-sheets belong to the same Pleistocene period, and I believe that they were causally related, the high altitude being the cause of the ice-sheets.

That the earth-movements which thus uplifted North America and North-Western Europe, permitting streams to erode the fjords and now submerged valleys, occupied the closing part of the Pliocene period and culminated in the early part of the Pleistocene or Glacial period, has been discussed and apparently demonstrated by Prof. J. W. Spencer, Prof. Joseph Le Conte, and the present writer. (*Bulletin of the Geological Society of America*, vol. i, 1890, pp. 65-70, 563–7; vol. ii, 1891, pp. 323–330, 465–476. Le Conte's *Elements of Geology*, new edition, 1891, pp. 589–594. *Geol. Magazine*, III., vol. vii, 1890, pp, 208–213, 492–7; vol. viii, pp. 92, 262–272, 330.)

In Europe, there is no better advocate of great earth-movements

during Pleistocene or Quaternary time than Professor Geikie himself, who, with Ramsay, has proved that the earth's crust at the Strait of Gibraltar, since the end of the Tertiary era, has been repeatedly uplifted much above its present height, allowing African animals to cross on dry land into Europe (*Quarterly Journal of the Geological Society, London*, vol. xxxiv, 1878, pp. 505–541), and who also believes that a land connection existed during the Glacial period from Britain to the Færöe Islands, Iceland, and Greenland (*Prehistoric Europe*, 1881, pp. 518–522, and 568, with Plate E). In Professor Geikie's admirable memoir on the geology of the Færöe Islands (*Trans. Roy. Soc. Edinb.*, vol. xxx, 1882, pp. 217–269), he shows that a vast amount of erosion has been effected there, and in like manner upon other lands bordering the North Atlantic, since the Miocene period. In comparison with the late Tertiary erosion so impressively exhibited, it is easy to accept the view that the deep but narrow Scandinavian fjords belong to a geologically short stage of great uplift during the late Pliocene and early Pleistocene epochs. The rivers continued to flow along the bottoms of these fjords until the increasing elevation of the land, as I think, brought on the ice-sheets, beneath which the land sank somewhat below its present height.

It is true that the duplication of glacial epochs accords beautifully with Croll's astronomic theory, which for several years met with general acceptance in America as well as in Europe. But the recency of the latest glaciation on both continents, which has been well stated by Wright, N. H. Winchell, Andrews, Gilbert, and Russell in American publications, and by Mackintosh, Southall, and others in the *Journal of Transactions of the Victoria Institute* (vol. xiii, and especially vol. xix, pp. 73–92), showing that the length of the postglacial epoch has been no more than 6,000 to 10,000 years, is inconsistent with the reference of that glaciation to astronomic conditions which ended 80,000 years ago.

Before receiving this Paper by Professor Geikie, I had it in mind to send, for some meeting of the Victoria Institute next year, a review of the principal theories which have been held to account for the climate of the Ice age; and in that Paper I hope to present more fully the grounds for my view as here briefly noted, and the difficulties which seem to me to forbid the acceptance of the other two theories which Evans and Croll proposed nearly thirty years ago.

THE AUTHOR'S REPLY.

I am sorry that my old friend Professor Hull is disappointed because, in trying to knock the "Earth-movement hypothesis" on the head, I have not presented him with some other explanation of the origin or cause of the glacial conditions of Pleistocene times. But I would remind him that the critic who essays to condemn a work of fiction is, fortunately for himself, not expected to produce another in its place. From the remarks made by Professor Hull, Professor Lobley, and Mr. Upham, it might be inferred that I do not believe in movements of elevation and depression. This is certainly not the case: all that I deny is that we have any evidence to show that the former excessive glacial conditions of Europe and North America were caused by great elevation of the land. Formerly I used to believe with most geologists that the Moel Tryfaen deposits were evidence of a depression of the land to the extent of 1,200 feet or thereabout, but after visiting that region some years ago, I felt convinced that the accumulations in question had been dragged into their present position by the old ice-sheet—the materials having of course been rearranged by the action of sub-glacial water.

Mr. Upham merely reiterates his belief in the Pleistocene age of the fiord-valleys of North-west Europe, remarking that it has apparently been demonstrated by himself and other American writers that the excavation of those valleys "occupied the closing part of the Pliocene period and culminated in the early part of the Pleistocene or Glacial period." This will be news to European geologists who have long thought that our fiord-valleys (in Norway and Scotland) are amongst the oldest valleys of erosion in Europe. Yet if Mr. Upham's contention were admitted, we should also have to admit that the fiord-valleys of North-west Europe are of more recent origin than the great lake-valleys of the Alps! Mr. Upham strangely does not see that if the fiord-valleys are simply partly-submerged land-valleys which owe their excavation to fluviatile action, their age and origin can have no bearing on the

question I have been discussing. The valleys were hollowed out by running water when the land stood 3,000 to 4,000 feet higher than now. Their excavation must necessarily have occupied a prodigious time, yet throughout that protracted period, rivers and not glaciers were their occupants. Clearly, then, if the fiord-valleys were excavated in late Pliocene and early Pleistocene times the land had then all the elevation required by Mr. Upham for the production of great ice-sheets, and yet no general glaciation took place until the hollowing out of the valleys had been practically completed. All that the glaciers have done has been to grind out hollows in the bottoms of the valleys, and to modify the general contour of the ground.

LETTERS RECEIVED.

Major-General A. W. Drayson, F.R.A.S., writes:—

The geological portion of Professor Geikie's Paper in regard to the Glacial Period shows such vast research and attention to detail, that I cannot presume to offer any remarks thereon. When, however, I find that he has devoted some three pages to demolishing what he terms the "Earth-movement hypothesis" and does not even refer to any other cause, I venture to offer some remarks: more especially am I disposed to offer these remarks, because a writer on the Ice Age in the *Edinburgh Review* for April, 1892, after pointing out that the assumption of the Earth being pulled away from the Sun, and thus causing the Ice Age, lacks the essential element of scientific truth, despondingly remarks that "there is nothing else to fall back upon."

Instead of there being nothing else to fall back upon, other than "assumptions" and mere hypothesis, there is a cause for the Ice Age, which has merely to be examined by competent geometricians, and the proof will be manifest that it is unanswerable. I make this statement, not on my own conclusions only, but because a considerable number of able geometricians have carefully tested every detail and have told me that the case is proved.

As briefly as possible I will explain what this cause is.

More than 300 years ago the three principal movements of the earth were said to be, a daily rotation, an annual revolution round the sun, and a conical movement of the axis of daily rotation round the Pole of the Ecliptic *as a centre.*

The reason why the earth's axis was supposed to trace a circle round the Pole of the Ecliptic *as a centre* was, because the observations of 300 years ago were not sufficiently accurate to reveal the fact that the Pole of the heavens (which is that point in the heavens to which the axis points) was continually decreasing its distance from the Pole of the Ecliptic, the imagined centre of the circle.

About 150 years ago it became generally admitted that the Pole of the heavens in its circular course, slowly decreased its distance from the Pole of the Ecliptic, and had so decreased its distance during 2,000 years at least.

Although this decrease in distance of the two Poles was a recognized fact, writers on astronomy continued to state that the one pole traced a circle round the other pole *as a centre.*

More than 30 years ago the above contradiction was brought to my notice, and I devoted ten years to the investigation of the problem, with the following results.

First, that the movement hitherto defined as a conical motion of

the earth's axis was in reality a slow second rotation of the earth, which caused the two half axes of the earth to describe cones.

Second, that the centre of the circle which the earth's axis traced was 6 degrees from the Pole of the Ecliptic, and the radius of this circle was 29° 25′ 47″, and that the decrease in distance of the two poles was due to this position of the centre of the circle.

From a knowledge of these facts I was able to arrive by calculation at results hitherto imagined to be impossible in astronomy, and the proof that the radius and position of the centre of the circle were as above stated was undeniable.

Third. From the fact that the centre of the circle traced by the earth's axis was 6 degrees from the Pole of the Ecliptic, it followed that during the tracing of this circle there would be a variation of 12 degrees in the distance of the two Poles, and a corresponding variation of 12 degrees in the extent of the Arctic circles and tropics.

From a knowledge of this curve, I was able to state more than 20 years ago that at about 3000 B.C. the Arctic circles and tropics extended about 2 degrees more than at present. That at about 5600 B.C. they extended about $6\frac{1}{2}$ degrees more than at present. That at about 13500 B.C. they extended nearly 12 degrees more than at present, at which date the Last Glacial Period was at its height. At about 21500 B.C. the Arctic circles extended about $6\frac{1}{2}$ degrees more than at present, and at about 24000 B.C. about 2 degrees more.

Hence the Last Glacial Period terminated not longer than about 6,000 years ago, and lasted not longer than about 18,000 years.

These dates were 20 years ago so utterly at variance with geological theories, that my proofs would not even be looked at. Within the last year or two, however, geologists from geological evidence have come to exactly the same dates that geometrical astronomy proved 20 years ago.

As the movement herein briefly described is proved by geometry, has been tested and found accurate by numerous competent examiners, and as it proves that 15,000 years ago the Arctic circle reached to 54 degrees latitude, and hence explains the main facts of the Ice Age, and also gives its date, it appears remarkable that eminent geologists should despondingly state that besides those vague speculations which they have demolished, there is nothing else in astronomy to fall back upon.

As remarked by Professor Geikie the assumed elevation and depression of the earth's surface is not only a mere speculation but fails to explain the facts. When geologists examine the movement of the earth herein described they will find an ample explanation of that which they require.

Mr. H. P. MALET writes:—

Whilst thanking Professor Geikie for his interesting Paper may I offer a few brief remarks on the Glacial theory.

We are told that "it is no longer disputed that in Pleistocene times vast sheets of ice. . . covered broad areas in Europe and America." The Pleistocene time is at the head of the Tertiary system, but no date is as yet fixed for the group. James Croll tells us in *Climate and Time* that the Glacial epoch began about 240,000 and ended about 80,000 years ago. Mr. Smith in his *Great Ice Age of North America*, gives about 15,000 or 20,000 years ago for the end of the frozen time. Professor James Geikie kindly sent me his papers on the *Evolution of Climate*, including some very suggestive maps of the varied condition of this Earth:—No. 1 gives the Palæozoic epoch, when the sea ran up through Central America, Europe, and Asia; No. 2 gives the Mesozoic condition, when the same highways were open. It is an accepted fact that the light warm water of the tropics runs up to replace the cold sinking water of the Arctic region. We know that this warm current gives warmth directly and indirectly to the neighbouring regions, therefore no Glacial Period existed in Europe or America at that time.

No. 3 gives the same regions in the Tertiary system; the American Channel is closed, but the Atlantic and the European channels are open. As the Pleistocene group is in this system, and as warm water still found its way through Europe, it is difficult to suppose that glacial times existed in the Temperate Zone of Europe. The maps seem to represent a very true geographical condition at each period—without date. We have no charts giving altitude in those old days, but there can be no doubt how the Highlands have at all times contributed to the filling up the lowlands by their denuded particles.

The Address points out several "salient facts" to reckon with before the glacial climate can be securely accepted. In addition to these I found in India the same actions going on by water forces as are attributed to ice by the glacial theorists. I found old moraines in the midst of plains in the Taptee and Beenea Valleys. These were left by river water-falls: rocks fall on the water and on the ice, they are carried as far as the forces can carry them, and are left to mark the spots where the moving power left them.

The subject has been much complicated by clever theories, but when we return to nature and trace the changes of climate as I did in *The Times* of February, 1891, I see no reason to give a Glacial Period to Europe in the Pleistocene group, when the geographical conditions were approaching their present state. Professor James Geikie told us in his *Evolution of Climate*, that "Geological climate has been determined chiefly by geographical conditions—therefore if Europe and America were covered by ice sheets in the Tertiary system, why were they removed?"

Mr. JOSEPH JOHN MURPHY writes:—

I have read Professor Geikie's Paper on the Glacial Period with

interest, and yet with disappointment at seeing so little new light thrown on the difficult and interesting subject of secular changes of climate.

Before speaking of the general question, there is a special point, on which, though not myself a geologist, I must venture to differ from Professor Geikie. He says:— "No one acquainted with the physical features and geological structure of Scotland and Norway can doubt that the valleys which terminate in fiords are of great geological antiquity. Their excavation by fluviatile action certainly dates back to a period long anterior to the Ice Age." On general grounds I think this statement is partly misleading. Not very many sea-coasts are cut up into fiords; and it cannot be a mere coincidence that fiords have been formed chiefly on those coasts where glaciation is most favoured by the geographical conditions, namely, on mountainous coasts, in high latitudes, and where exposure to prevailing west winds from the ocean promotes an abundant snowfall. Norway, Scotland, and the west of Ireland, presents such coasts; but the most conspicuous instances will be seen by a glance at a map of the world, to be at the northern and southern ends of the western coast of the American continent. From Vancouver's Island northward, and from Chiloe southward, the coasts of the continent are cut up into fiords and islands by sounds which are submerged valleys; while in the lower latitudes, both northern and southern, the coast, from Vancouver's Island to Chiloe, is remarkably unbroken.

The connection between glaciation and the formation of fiords is obvious enough. Most valleys have been excavated; and these, except some which have been eroded by the sea, are due either to fluviatile or to glacial action. Mountain valleys excavated by running water are in general deep and narrow—the most conspicuous instances are the canyons of the Colorado, and the Via Mala in Switzerland, which is a canyon—and, though on a much smaller scale, the ravine-like valleys of the so-called Saxon-Switzerland are of this class. Mountain valleys excavated by glaciers are on the contrary deep and wide; and it appears to be generally agreed that most of the valleys of our European mountains are of this origin. When such a valley descends into the sea it becomes a fiord. It may be the fact that most of the greater valleys of Norway and Scotland existed as river valleys before the Glacial Period, but if so, during that period they became filled with glaciers, which, by their excavating action, gave the valleys their present form and contour.

I am fully convinced that no merely geographical changes can possibly account for the glacial climate; and I agree with Mr. Croll that its causes were astronomical. But I think he has failed to explain rightly how these causes operated.

I must here point out that the extent of glaciation depends in no degree on mean temperature, but exclusively on summer temperature. The "snow-line" is the line of summer snow, and theory and

observation agree in showing that the extent of glaciation depends chiefly on the height of the snow-line so defined. There is a region in Eastern Siberia where the ground, at the depth of a few feet, is frozen all the year round, showing that the mean temperature of the year is below frost; and yet over that frozen subsoil cattle graze, crops of rye are harvested, and pine forests flourish. It is obvious that if from any cause the extremes of that climate were to disappear, while its mean temperature were to remain unchanged, so that there was a temperature below freezing for every month of the year, all the precipitation would be of snow, which would remain unmelted, and the land would be covered with continual ice like Greenland.

There is an astronomical cause which must produce such changes. The major axis of the earth's orbit is unchangeable, but the minor axis is subject to slow fluctuating changes of length; and as the sun is always in one of the foci of the ellipse of the earth's orbit, it follows that the narrower the orbit, the greater is its eccentricity, and the greater the difference between the earth's perihelion and aphelion distances—in other words, its least and its greatest distance from the sun. Now, when the earth's aphelion occurs in the summer of either hemisphere, there must in that hemisphere be a cold summer; and a cold summer, as we have seen, produces glaciation, so that the hemisphere having an aphelion summer had a glacial climate. During the winter of the same hemisphere, the earth was at its perihelion, or minimum, distance from the sun, giving the glaciated hemisphere a mild winter, which had no effect whatever on its glaciation; and the opposite hemisphere had at the same time an intensely hot summer, which promoted evaporation, part of which evaporation must have fallen in snow on the glaciated hemisphere. It thus appears that at definite times in the past, the two conditions of maximum glaciation must have been fulfilled in each of the Earth's hemispheres, namely, a cold summer and a snowy winter.

If this view of the nature and cause of the glacial climate is correct, the northern and southern hemispheres were never glaciated at the same time. But the periods of great eccentricity of the earth's orbit last for a long time, and during their continuance the two hemispheres were glaciated alternately, at the geologically short interval of about 10,500 years; at the end of which period the perihelion and aphelion have arrived at opposite points in the circle of the year to those which they respectively occupied at its beginning. Either solstice coincides with the perihelion or aphelion once in about 21,000 years; so that if, as is nearly the case at present, the northern mid-winter falls in perihelio and the southern in aphelio, at the end of half this period, or 10,500 years, the positions of perihelion and aphelion, relatively to summer and winter, will be reversed.

This explains the fact of inter-glacial periods: while there was

a Glacial Period in one hemisphere there was an inter-glacial period in the other; during the continuance of great eccentricity in the earth's orbit, glacial and inter-glacial periods alternated with each other in the opposite hemispheres.

My theory on this subject has been suggested by Mr. Croll's, but it is not the same. Mr. Croll, for reasons which I fail to understand, though I have read them carefully, places the glacial climate in the hemisphere which has its summer when the earth is nearest the sun, and consequently, as it seems to me, when the heat of summer is greatest, and the snow of the previons winter is most completely melted away. It is certain that at the present time, the nearest approach to a glacial climate, as shown in the greatest extent and the lowest descent of glaciers, is not to be found in countries of intense winter cold like Siberia, but in regions of cold summer and abundant snowfall, like the shores of the Straits of Magellan. Practically these remarks summarise my views as given to the Geological Society (*on the Nature and Cause of the Glacial Climate*), and the Belfast Natural History Society.

NOTE.

Professor Geikie has seen the foregoing letters. He offers no further remarks.—ED.