JOURNAL OF

THE TRANSACTIONS

OF

The Victoria Institute,

OR

Philosophical Society of Great Britain.

EDITED BY THE HONORARY SECRETARY,
CAPT. FRANCIS W. H. PETRIE, F.G.S., &c.

VOL. XIX.

LONDON:
(Published by the Institute)
INDIA: W. THACKER & Co. UNITED STATES: G. T. PUTNAM'S SONS, *N.Y.*
AUSTRALIA AND NEW ZEALAND: G. ROBERTSON & Co., LIM.
CANADA: DAWSON BROS., *Montreal.*
S. AFRICA: JUTA & Co., *Cape Town.*
PARIS: GALIGNANI.
1885.

ORDINARY MEETING, DECEMBER 1, 1884.

H. CADMAN JONES, ESQ., M.A., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following paper was read by Mr. W. N. WEST (hon. treasurer), in the unavoidable absence of the author :—

ON THE RECENCY OF THE CLOSE OF THE GLACIAL PERIOD IN ENGLAND AND WALES, as shown by the limited depth of Post-glacial Stream-channels; the small extent of Denudation of Limestone Rocks; and the fresh aspect of Moraines. By D. MACKINTOSH, Esq., F.G.S.

1. *Brief Statement of the Order of Glacial Events.*—Those British and American geologists who have made the most extensive observations are now very nearly agreed in believing that there was *first* a period of land-ice which filled up the valleys, and covered many of the mountains; *second*, a submergence of the land which commenced *before* the close of the land-ice period, continued during a comparatively mild period, and did not terminate until *after* the commencement of a second period of land-ice.

2. *Condensed Statement of Professor James Geikie's Discoveries and Opinions.*—Striated rock-surfaces are found on certain parts of the sea-coast of Scotland, where they lie lower than the latest *post-glacial* "raised beaches" which, more or less, cover the glaciated rocks. Below the level of the present

raised beaches the sea (when the beaches were deposited) covered and protected the glaciated rocks. The sea was then twenty to thirty feet higher than now. Around the sea-lochs of the western Highlands and Sutherland, and likewise on the east coast, glaciers came down to the sea-level (when it was twenty to thirty feet higher than now) in Neolithic times. The epoch of the latest glaciers in Scotland was separated from the last great glacial period by the oldest submarine forests, and the buried trees at the bottom of peat-bogs. Then the land went partially down, and the latest (now) raised beach was deposited. Glaciers again appeared in the mountain valleys and came down to the sea-level. Neolithic man was then living, since we find his canoes lying at the bottom of the Carse clays, associated with the trees of the submarine forests. To this statement of Professor James Geikie's discoveries, I may add that Mr. Kinahan has found traces of the continuance of local glaciers in Ireland as late as the time when the 300 feet and 100 feet raised beaches were formed.

3. *Limited Depth of Post-glacial Stream-channels.* — In many parts of Wales, Cumberland, and elsewhere, on the sides of valleys (such as Nant Francon), gorges, from a few feet to at least nine or ten feet in depth, have been ploughed out, by what is locally called the "bursting of a thunder-cloud," in less than an hour, while pre-existing channels of streams have, to a great extent, been enlarged. But in many places, where post-glacial channels have been excavated by the *ordinary* action of streams, they have not reached a depth of more than a few feet, and that even in loose drift, or in soft and incoherent rocks. I could mention numerous instances in many parts of North Wales and Cumberland, but a few may suffice, namely, stream-channels around Llyn Ogwen; in Llanberis Pass; on some of the slopes of Snowdon; above the Penrhyn slate quarries; some of the brooks around Llangollen; on the east side of Minera mountain (west of Rhosllanerchrugog), where several rapidly-flowing streams have excavated channels in glacio-marine drift only a few feet in depth; in Cwm Llafar (under Carnedd Dafydd), where a foaming brook has made wonderfully little impression on the bottom of a narrow valley which, according to Ramsay, was scooped out by one of the later glaciers; in many of the Cumberland valleys, &c. The very fact that many of the post-glacial stream-channels of Wales and Cumberland are sufficiently shallow to admit of being crossed by cart and carriage roads, without bridges, is a striking proof of the trifling extent to which post-glacial streams have deepened their

channels. But it is necessary to guard the observer against mistaking pre-glacial excavations for the post-glacial channels of streams, because the upper boulder-clay of England and Wales has been generally deposited as a *wrapper* which, in a thin bed, rises to the summits of the knolls and goes down to the bottoms of the valleys which must have been excavated in glacial or pre-glacial times. According to Mr. De Rance, in a great part of Lancashire, glacial deposits occupy old pre-glacial valleys, producing the phenomenon of valley within valley. Mr. Searles V. Wood tells me that in Holderness, north of Hull, there are many examples of streams making channels in a mere wrapper of glacial drift which follows the undulations of the surface of the ground.

4. *Time Indicated by the Vertical Extent of the Pedestals of Boulders.*—Boulders may be said to be without pedestals when the rock-surfaces on which they rest extend continuously under them with little or no change of level; in which case the boulders, owing to their particular forms or positions, are not capable, to an appreciable extent, of concentrating or intensifying the action of rain water, which, in a *diffused* state, would appear to exert scarcely any denuding influence around the boulders. They may be credited with acquired pedestals where the underlying supports have been caused by the pluvio-torrential action resulting from wind-blown rain, and by the form and extent of the water-collecting surface furnished by the boulders. They may be said to rest on appropriated or usurped pedestals, where the latter depend on the *previous* removal by denudation of the surrounding rock, in which case they are merely "perched blocks," and can therefore furnish no evidence of the vertical extent of circumjacent denudation which has taken place *since* the boulders came into their present positions.

5. *Boulders on Limestone Rocks North of Llangollen—Distribution and Description.*—About two and a half miles north of Llangollen, a ravine above Brook House leads up to a high limestone plateau on the left called Craig-yr-ogof. On then going north towards a ravine called Nant-hen-Gastell, many boulders of Arenig felsite may be seen on grass-, fern-, or heath-covered surfaces, with a hollow on one or two sides caused by the down-splashing of concentrated rain-water from the boulders. After crossing this ravine, and turning to the left as far as the brink of the innermost well-defined cliff-line, a monstrous-looking boulder of nearly black felsite, about eight by seven by five feet in diameter, may be seen in a perched position close to the brink. It has a rather irregular

fragmentary pedestal (Fig. 1) of limestone from eight to nine inches in average height above the surrounding ground; but it is possible it may have found this pedestal ready made, so that it can admit of no reliable calculations concerning the time required for the circumjacent denudation of the limestone

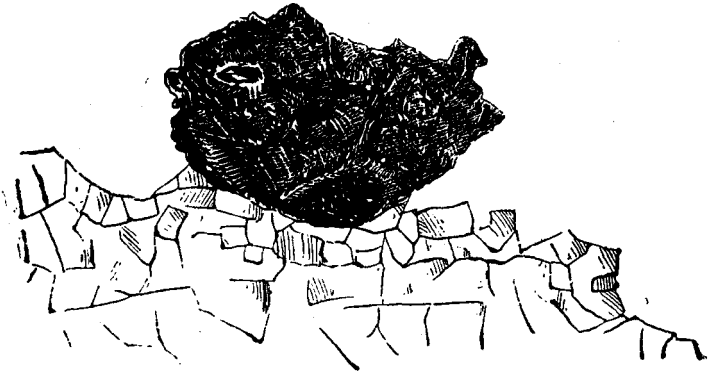

Fig. 1.

rock. Farther north a small boulder may be seen resting partly on limestone, with a surface around the boulder which has been lowered only about two inches by rain since the boulder came into its present position. On the neighbouring limestone outcrops other boulders may be seen, around which the action of rain has lowered the general surface to the extent of only a few inches. From a comparison of facts, and after rejecting boulders which may have found ready-made pedestals, I have been led to the conclusion, that if we include hollows (chiefly on the leeward, but likewise on the windward side of boulders, and to a small extent all round the boulders) the average vertical extent of circumjacent denudation since the boulders were left by melting ice has not been *more* than about six inches, and that this extent does not necessarily indicate a period of more than a few thousand years (see sequel). Before leaving this plateau, it may be well to state that many of the boulders show signs of having fallen from a great height (probably from floating ice), for the boulders have not only been fractured, but the limestone rocks on which they fell have been rent and shattered to a very striking extent. It ought not to be forgotten that *many* of the boulders would appear to have fallen on bare limestone rock, as there are no traces of drift or boulder-clay around or under them, so that

the circumjacent denudation of the limestone may have commenced immediately after the fall of the boulders. The absence of boulder-clay may be readily explained by the inability of submarine currents to transport it in an easterly direction across deep valleys and steep ridges, and finally up the steep and (in many places) overhanging face of the Eglwyseg cliffs.

6. *Boulders on Limestone Rock-surfaces near Clapham, Yorkshire.*—After walking about a mile and a half along Thwaite-lane, east of Clapham, and crossing one or more walls in a northerly direction, one arrives at the base of a steep limestone escarpment. On rounding the east end of this escarpment, and walking up a stone-covered slope on the left, the lower part of the great limestone plateau marked Norber on the Ordnance maps suddenly comes into sight. It cannot fail to be noticed that the surfaces of the limestone rocks are often flat, and terminate in miniature cliffs with steep brinks, as if whole blocks or fragments of rock had been removed by land ice, floating ice, or, according to Professor Phillips, by powerful currents. Partly resting on fragmentary rocks, and partly on flat and extensive rock surfaces of a light grey colour, there is what may appropriately be called a grim array of many hundreds of huge and black Silurian grit and slate boulders, which are apt to suggest the idea that they are about to spring into life! I think it must be obvious to any one who has made the pedestals of boulders a special study that many, if not most, of these boulders have found resting-places on pre-existing fragmentary projections of limestone rock, after the manner of the perched blocks which may be seen in all countries which have undergone extensive glaciation, and I have little doubt that, had Mr. Tiddeman's attention* been particularly directed to the subject, he would have agreed with me that the formation of the pedestals must have partly, if not chiefly, taken place before the boulders were left on them by the melting of the ice which transported them. But Mr. Tiddeman's object was the more important one of discovering glacial striæ on the pedestals under the boulders where they had been protected by the boulders from the action of rain.

7. *Evidences of the Pre-existence of many of the Pedestals.*—That many of the pedestals must have existed before the arrival of the boulders would appear from the following facts:—1. Many of the boulders have no pedestals, and many

* See *Quart. Journ. Geol. Soc.*, vol. xxviii.

have pedestals which do not fit the boulders; in other words, a pedestal may look narrower than a boulder when viewed endwise, but wider than a boulder when viewed sidewise; many boulders resting on flat surfaces may be seen projecting over miniature cliffs, and many have two or more pedestals with vacant spaces between them which could not have been excavated after the arrival of the boulders, because the latter would have protected the underlying rock-surfaces from the action of rain. In the case of the Norber boulders, which rest on divided pedestals with one or more vacant spaces under which preserved glacial striæ may be seen on looking in from without, it is clear that the vacant spaces could not have been excavated by lateral pluvial action after the arrival of the boulders, because the pluvial or any other kind of aqueous action would have obliterated the striæ.

The following figures will give an idea of the more typical supports of boulders on Norber plateau:—

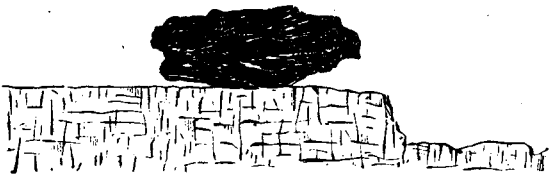

Fig. 2.

Fig. 2 represents a boulder resting on a flat limestone rock. The depression on the right probably existed before the boulder came into its present position, otherwise there ought to have been likewise more or less of a depression on the left.

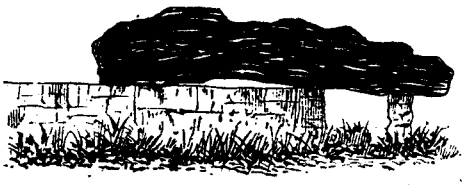

Fig. 3.

Fig. 3 shows a boulder mainly resting on flat rock, with a small subsidiary pedestal. It is clear that both must have existed before the arrival of the boulder.

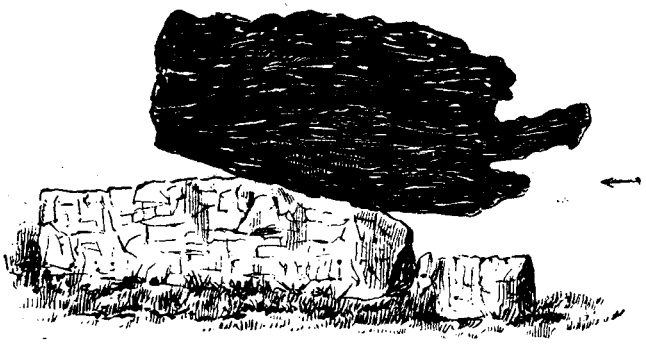

Fig. 4.

Fig. 4, when viewed in the direction of the arrow, appears to have a pedestal much narrower than itself; but, when seen side-wise, the pedestal (not fitting the boulder) would seem to have existed before it was usurped by the boulder.

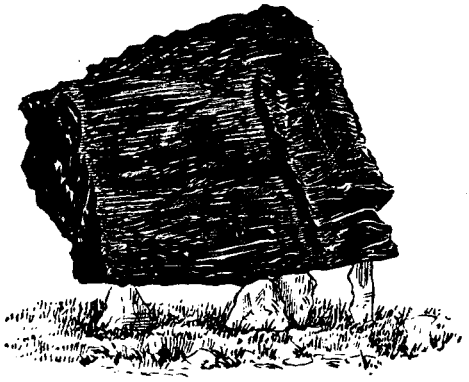

Fig. 5.

Fig. 5 is a view of a *typical* perched block which must have been tranquilly laid down by melting ice on pre-existing fragments of rock.

8. *Pedestals Formed since the Arrival of the Boulders.*—These pedestals vary from almost nothing to about seven inches in height. Those of them which, from their shape, would appear to have been left by the splashing down of rain-water from the boulders, may average about six inches in height, or nearly the same as the pedestals on the Eglwyseg

plateau near Llangollen. I do not remember having seen boulder-clay on the surface of the flat limestone rocks of the Norber plateau, though the spaces between many of the blocks in situ are often partly filled with a kind of grass-covered earth which may have resulted from the accumulation of the insoluble part of the limestone. On the supposition that the boulders were transported by a great sheet of land-ice (as Mr. Tiddeman believes), little or no boulder-clay would be allowed to gather, according to Professor Geikie (*Prehistoric Europe*, p. 289). Had the sea, with boulder-laden floating ice, swept over the high rocky plateau, there would have been still less likelihood of boulder-clay being deposited.

9. *Rainfall of the Above Plateau.*—It may be desirable to compare the rainfall of the two plateaux described in this paper, especially as in, or near to, the two localities it was very nearly the same in 1881. At Austwick, near Norber plateau, it was 41·70 inches, while at Llangollen, it was 42·81 inches. According to Professor Phillips, the mean annual depth of rain, from 1837 to 1850, was 43·3 at Settle (which is not very far from the Norber plateau).

10. *Pedestals of Boulders in Ireland.*—My attention has lately been directed to a work entitled *Fissures, Fractures, and Faults*, by Mr. Kinahan (of the Irish Geological Survey), in which he states that on the Arran Islands, where the rainfall is great, the limestone has weathered away from four to six inches since the glacial period, as proved by the unweathered pedestals of limestone under the erratic blocks; while inland similar pedestals are seldom three inches in height.

11. *Bearing of the Above Facts on the Time which has elapsed since the Close of the Glacial Period.*—After making a series of calculations based on the results of observations made in the two districts described in this paper, I was led to the conclusion (as already hinted) that the *average* depth of the hollows which have been excavated around boulders by the pluvio-torrential or mechanical action of rain-water (assisted by its chemical action under favourable conditions) is *not more* than about *six inches*. With regard to the rate of denudation, the extension of many flat rock-surfaces under boulders (especially on Norber plateau) shows that it must be exceedingly slow. But if we allow a thousand years for the excavation of only an inch in depth of the hollows around the boulders, this would give us not more than 6,000 years as the time which has elapsed since the boulders were left in their present positions through the melting of the ice by which they were transported. These calculations are only vaguely

approximate, but I think they are sufficient to show that many geologists have shown a tendency to exaggerate the time which has elapsed since the close of the glacial period.

12. *Fresh Appearance of Ice-marks and Moraines.*—Whatever difference of opinion there may be concerning the time indicated by the height of the limestone pedestals of boulders, all must admit that the recency of the close of the glacial period is forcibly suggested by the extent to which ice-marks on rocks have been preserved in positions where they could never have been protected by drift from atmospheric action, and where their freshness is consequently owing to the comparatively short time they have been in existence. In many of the upper valleys of Cumberland, glacial moraines present so fresh an appearance, that an unsophisticated person newly introduced to the district might readily suppose that they had not been more than two or three years in existence. On some of these moraines stones must have been so delicately equipoised on each other by the retreating glacier, that a touch of one's finger would now be sufficient to make the fabric topple down!

13. *Perennial Snow or Ice on High Plateaux during Neolithic Times.*—Professor Geikie, as already stated, is of opinion that the second great glacial period was divided into two by a mild interval. But though this may have been the case in Scotland, or farther north, it may not have been so (at least to the same extent) in England and Wales; and I think we are therefore at liberty to believe that on the *high plateaux* in the north of Wales and England, which form the main subject of this paper, ice, or ice alternating with snow, may have been perennial, though it may have been different in the lowlands and farther south. If so, ice or snow on the Eglwyseg and Norber plateaux, which rise to between 1,000 and 1,300 feet above the sea-level, may have lingered until about 6,000 years ago, so as to protect the supports of boulders from the action of rain.

14. *Close of the Glacial Period in North America.*—It is well known that many American geologists (including several who have been honoured by the Council of the Geological Society of London) are convinced that the glacial period in the Niagara and Michigan district terminated so late as about 6,000 years ago. From recent reports it would appear that the Niagara Falls have lately been receding at about the rate of ten feet in twenty-four years, or about two and a half feet in a year; and this accords with the results of observations made by the late Mr. Belt and Dr. James Hall, who found that the Falls had receded in solid rock about three miles since the Niagara

channel had been partly filled up with glacial drift. Dr. Andrews has made a series of very precise observations on the raised beaches of Lake Michigan, which show that the surrounding country rose out of the glacial sea between 5,500 and 7,500 years ago. Many rivers besides Niagara have made new channels in the glacial drift by which their old channels were choked up.

15. *Astronomical Proofs of the recent Close of the Glacial Period.*—According to Lieut.-Colonel Drayson, in the *Quart. Jour. Geol. Soc.* for 1871, it would appear that 13,000 years before Christ the Arctic circle came down to latitude 54° 35′ N., or to about the latitude of St. Bees and Whitby. The climate commencing about 21,000 years ago, would become more and more extreme up to about 15,000 years ago, and then gradually more and more equable to about 6,000 years ago.

16. *Historical Objection to the recent Close of the Glacial Period.*—Some notice ought to be taken of an objection to the recent termination of the glacial period made by Professor Bonney, on the ground that, if it came to a close so late as 6,000 years ago, the climate would have continued down to 3,000 years ago sufficiently cold to excite the notice of historians. But, according to Colonel Drayson's calculations, the climate would have lost its extreme character much earlier than 3,000 years ago. It may likewise be remarked that glacial conditions in Britain never extended farther south than the Bristol Channel and Thames estuary, so that in countries farther south the climate 3,000 years ago may not have been sufficiently extreme to invest it with historical interest.

THE CHAIRMAN then said, I am sure all will be glad to hear the remarks any one present might have to offer.

Mr. E. CHARLESWORTH, F.G.S. (a Visitor).—Geological science is one of those branches of human investigation which, charming and attractive as it is to all who love the attempt to increase the boundaries of human knowledge, is nevertheless, one which I have felt, ever since I first took it up as a study, to be replete with uncertainties; and of all the uncertainties that beset the geologist in his pursuits I think the one I should single out as *par excellence* the most perplexing, and the most unsatisfactory, is the attempt to measure geological time. Some of the greatest writers on the subject of geology have told us that there are evidences of life having existed on this planet countless years ago, and that, in point of fact, we can only gain an idea of geological time by comparing it with what we may term astronomical time. We know that we may penetrate millions of miles through space, and then be no nearer its confines than before. But when we are told by great authorities

that geological time is capable of sustaining such a comparison, and when we consequently begin to speak of millions of ages as a mere atom in the ocean of geological time we find ourselves confronted with another class of men, of equal authority with those to whom I have alluded, who say that such a way of putting the matter is open to question, and that a hundred thousand, or at most a couple of hundred thousand years afford ample time to account for all the phenomena we see around us, or which the geologist finds presented to him, when he digs, tunnels, or mines into what, as a conventional term, is called "the crust of the earth." But we are also brought face to face with another class of authorities who tell us that hundreds of thousands of years are altogether out of the question, and that the time necessary for the production of all these phenomena can be comprised in a period of some thirty, forty, or fifty thousand years. There are also a few men of unquestionably high standing and whose ability is deserving of the highest appreciation—among whom I may mention Dr. Dawson, whose book on geology no one can read without acknowledging that it is written in a thorough spirit of geological research, and is based on a great amount of good reasoning—who say the Scriptural record will account for everything, so far as we have yet known it. Well, then, amid all these conflicting opinions how is it possible for us to arrive at anything like a generalisation with regard to this matter, such as we can unhesitatingly accept? The only answer I can give is that the case is hopeless. But do not let me convey to the minds of those present the impression that, therefore, geology is one of those studies which can be taken up without a feeling that there are grand truths, and grand generalisations, to be deduced from it, as to which we are all agreed. I feel that Mr. Mackintosh has given us a most interesting paper. One of what I think he considers his strong points is this: Referring to the case of a boulder found in a certain position—it might be supposed that that boulder had been taken up by one of those huge masses of floating ice, of which he speaks, and that, when the temperature was heightened and the seas melted, the boulder fell to the bed of the ocean. There, of course, the boulder remained, and then came a period of elevation, during which the bed of the sea was thrown up, and the boulder, being on the surface, became exposed to the action of rain—Mr. Mackintosh says the rain beats down on the boulder, and drops of water are driven off in a shower of spray, which cuts a channel round the boulder; and he is inclined to think that the time these channels take to form is represented by one inch of depth to a period of one thousand years. This is a very ingenious idea; but, to my mind, it simply amounts to this—that the next observer may say he thinks that an inch of excavation represents ten thousand years, while another may say that an inch represents twenty thousand years, and another may assign a longer period. I think Mr. Mackintosh has overlooked one thing, namely: Supposing the channel which is found surrounding the boulder has been cut out by the spray that has fallen from the boulder, when the first gutter begins to be formed the water falling from the boulder

would lie in it, so that the spray falling upon it after it is filled would not have one hundredth part of the mechanical power the first drops would have. If the gutter be three or four inches wide, and the same depth, the spray falling into it would, perhaps, only have the power of deepening the channel one inch in a very long period. This is a point Mr. Mackintosh seems to have overlooked. Among the natural phenomena he mentions are the Falls of Niagara. I remember that when I first read Sir Charles Lyell's book I was delighted with his measurements of geological time, as illustrated by those Falls. He said he saw the river cutting its way through a series of rocks, and he found that the Falls retreated a few inches every year. By measuring the number of inches so cut away, he arrived at the conclusion that, at the very lowest computation, it must have taken the river over thirty thousand years to cut the length of channel it had excavated. Now, when I visited the Falls of Niagara and went to the bottom of the ravine, and passed along the ledge of rock that runs between the water and the rock, I found that there was a constant spray of water from the waterfall washing the face of the rock, and that the Fall thus acted on the face of the precipice of rock more or less according to the direction and force of the wind; and I felt how much the amount of wear of the rock must depend upon influences which are not always taken into consideration.

General G. Skene Hallowes (Acting Honorary Secretary).—I observed the same back action of the spray from the Fall when I was at Niagara, but, the weather being calm, it was not so excessive as some have described it to be.

Rev. W. B. Galloway, M.A.—Perhaps I may be permitted to remark that the glacial theory, as held by Mr., now Professor, James Geikie, amounts to something very astonishing. It supposes that in Scotland and in England —at least in the northern part of this country—there was a glacier—a continuous one—of from 2,500 to 3,000 feet in thickness; that in the whole of Switzerland, or at least the valley between the Alps and the Jura, the ice was piled up in an unbroken mass to, at least, the same thickness; that in Sweden and Scandinavia, generally, the ice was 7,000 feet thick, and that in Connecticut, according to Professor Dana, the thickness of the ice was from 6,000 to 8,000 feet. Now, the great ice barrier at the antarctic pole was calculated by Sir James Ross to be 1,000 feet in thickness, so that in the estimates I have mentioned we have placed before us for belief something really prodigious. At one time, when it was the general opinion that a universal Deluge had covered the mountains, as the Scriptures relate, there was an objection made that there was not so much water as would produce that result. But this difficulty does not really exist to the same extent since Lyell affirmed that the depth of the water is fifteen times greater than the height of the land, and also, that two-thirds of the globe are covered by water. But what strikes me as an extraordinary change in the position taken upon this question is, that while the objection formerly made to the Scriptural account of the Deluge was that there was not enough water to cover the land, we are now asked to substitute for the water an

enormous quantity of ice—a deluge of ice for a deluge of water. Then Sir William Thompson, of Glasgow, comes in with the opinion that it has taken from twenty millions to two hundred and forty millions of years for the cooling down of the earth to its present temperature; all this enormous thickness of ice having been in the intermediate period, while the slow cooling of the earth has been going on according to Sir William. There are, certainly, evidences of tropical and semi-tropical temperature even in the London basin. Is it not an extraordinary contradiction to assume that, while there are these tropical or semi-tropical indications, there was this ice-sheet? And furthermore, as to the termination of this period, how are we able to say that it has yet terminated? It is not so many years since an elephant, embedded in ice at the mouth of the river Lena, was disclosed by the breaking up of a great mass of ice, and there are similar things continually occurring now. The glaciers of the Alps are even now diminishing in volume, and this diminution may account for the circumstance referred to by the last speaker. Now, it seems to me that the paper read to-night has put forward one very remarkable circumstance namely, that more than one of our great boulders had evidently fallen from a great height, and had broken the rock underneath it. This, of course, indicates *floating* ice, which may have been carried on the waters of the Deluge. At the time I first made acquaintance with geology—when Cuvier had obtained his celebrity, and Buckland had published his *Reliquiæ Diluvianæ*—Cuvier remarked on the subject of the ice which had disclosed the elephant at the mouth of the Lena, that the cause must have been sudden. It must have been as sudden a cause which brought the animal into its position among the ice as the freezing up of the elephant itself; for it was so fresh when embedded in the glacial ice, that, when some thousands of years afterwards it was brought to view, the wild dogs fed upon its carcase. The skeleton is now preserved in the Museum at St. Petersburg. Cuvier remarks further, in regard to the causes to which the glacial period has been attributed, one of them being the very slow alteration of the earth's orbit, that no gradually operating cause, such as that which is imperceptible even in thousands of years, could possibly account for a sudden change of climate, which must evidently, at once, have frozen up that large animal in thick ice, and preserved it from the effects of a temperate atmosphere for thousands of years. If the cause were a universal Deluge, as Cuvier believed—and I do not know that we have had a greater geologist since—then I think it is time for us to re-consider the changes which the theories of geologists have been undergoing. They are evidently now working their way back from the 240,000 years ago, which is the date of the glacial epoch, as assumed by Professor Geikie, and which was further assumed by Mr. J. Scott Moore, in his *Pre-glacial Man*, and by Lyell, who supposes it may have occurred from that period to a million years ago. Surely this gradual diminution from a million of years, or hundreds of thousands, may lead us to hope that science is at length coming back to its older, and, I think,

sounder frame of mind, when Cuvier and Buckland were at the height of their celebrity; and I would earnestly press upon those who may feel an interest in the subject of geology to examine whether there is not more reason in believing that there was a universal Deluge than there is in a belief in the existence of ice 8,000 feet thick in Connecticut, 7,000 feet thick in Scandinavia, and 3,000 feet thick in Scotland, while beds of similar thickness filled the whole of the valley between the Alps and the Jura, in Switzerland, so that blocks of granite slid down from the Alps, carried by that huge glacier, and planted themselves on the Jura. I do think that, under the circumstances, I may be pardoned for retaining the impressions created by my earlier studies, and for holding that no more doubtful scientific conjecture has ever been put before the public than is contained in the glacial theory, treated on in the writings of Lyell, and of Dr. James Geikie, in his published work, entitled *The Great Ice Age*. I trust I have not been inaccurate in any of the statements I have made; but I did not notice until yesterday that this subject was to be brought forward, and I have not had time to refresh my memory in regard to it. There is one point on which I would supplement what has been said with regard to the cutting of the river channel by the Falls of the Niagara. Is it not assumed that the rock through which the river is cutting that channel has always been of the same hardness? We know that there was a human skeleton found in a rock in the West Indies, and that it is now in the British Museum. When that man's body first became embedded in the limestone it could not have been as hard as it since became. Is it not, then, very conceivable that at the period the Falls of Niagara began to cut their present channel, the rock may have been very much softer than at the present day, and much in the same condition as that limestone?

The CHAIRMAN.—It struck me, as I read the paper, that the weak point in it was, what appeared to be, the arbitrary assumption as to the cutting of one inch of channel round the boulders, in a thousand years.

Mr. CHARLESWORTH.—Quite so.

Mr. HASSELL suggested that if there were any earth round the boulders there would be a very great alteration in the course of time.

Mr. CHARLESWORTH.—I should suppose that if any of those boulders were on the soft earth the rain would soon sweep away a good deal of it.

Mr. HASSELL.—I agree with Mr. Charlesworth as to the uncertainty of calculations which are based on the assumption that what has happened in the past has gone on at the same rate as what is occurring now. It was well known that a severe frost, in a particular year, will break off many inches, or even yards, of rock; and in the case of Niagara one sharp winter might have the effect of rending away several feet of the rock. I do not think any one would hold that denudation goes on at the same rate during all periods of time. The inference is in favour of the rate differing with varying circumstances. Then again, as to the supposed thickness of the ice in Scandinavia and elsewhere during the

glacial period, it appears to me to be little more than an assumption. If there has ever been a mass of ice—a sea of ice—13,000 feet thick formed from water, it would have required many thousands of feet of water for its production, and as all water forms ice at the surface first, and thickens gradually downwards, the degree of cold sufficient to have formed such an immense thickness must have been very intense. But much of the ice of the glacier period was not confined to the ocean, it is said to have swept the whole surface of Europe. Now, where did these mighty masses come from? If from the sea, how did they reach the upper mountain valleys, such as those of the Alps and the Jura? If formed as the glaciers in those places are now, namely, by the pressure of the snow in the upper parts of the mountain valleys, where did all the snow come from? Snow is very light, and much more expanded than ice, so a much greater thickness of snow must have fallen than the thickness of the glacier formed out of it. What a prodigious fall of snow that must have been which resulted in the formation of a glacier 8,000 feet thick. Here, then, it appears to me that much more information is needed before one can accept the conclusions of some geologists as to the extent and duration of the so-called great ice age. On the whole, I think we cannot fairly base any conclusion as to the antiquity of man on the data that have yet been furnished.

The meeting was then adjourned.

REMARKS ON MR. MACKINTOSH'S PAPER

BY PROFESSOR T. RUPERT JONES, F.R.S.

I have carefully read Mr. Mackintosh's paper, and, taking his data as established, I do not find any adverse criticism to offer; quite otherwise, his statements and arguments are very clearly put.

I may remark that the results of the Rev. Osmond Fisher's calculations as to the time when the "Recent Period" (equivalent, I presume, to the end of the last glacial period) began, coincide generally with Mr. Mackintosh's views. That is, Mr. Mackintosh looks back 6,000 years, and the Rev. O. Fisher to about 8,000 years for the same, or nearly the same, period.

See my reference to the Rev. O. Fisher's calculations, and other correlative matters, in the Proceedings of the Geologists' Association, vol. viii., 1884, No. VI., p. 352.

BY THE REV. HENRY BRASS, M.A, F.G.S.

Mr. Mackintosh's idea derives strong corroboration from the comparatively recent formation of the deltas of the rivers of the Alps.

For example, the delta of the Rhone, in the Lake of Geneva, has, according to Sir C. Lyell (*Principles of Geology*, chap. xviii., 8th ed.), increased "more than a mile and a half," "in about eight centuries."

He states that the remainder of the delta is "about five or six miles in length," so that, at this rate, the delta is less than 4,000 years old; and, as it narrows and shallows towards its apex, even this estimate must be considerably reduced.

Now, what was the river doing with its sediment *before* it commenced to form the delta? Possibly, as Lyell suggests, it was filling up some lake basins in the upper part of its course, but as the contents of these basins (as I gather from frequent observation) do not much exceed (if they do at all) the contents of the delta, and as many of them may have been filling (partially, at least,) contemporaneously with it, we cannot but be forcibly impressed with the comparatively recent origin of the present physical condition of the country.

Or, did the river (as some have conjectured) formerly flow through the Valley of the Trient into the Arve, at Chamouni? This would not have been possible without very great alterations of level, inasmuch as the Valley of Chamouni is about 2,000 feet higher than that of the Rhone at Martigny, and the lowest point between them is about 3,500 feet higher!

The most probable conjecture seems to be that the Great Rhone Valley was filled with ice, and that the *Alpine glaciers extended down into the Lake of Geneva*, a very few thousand years ago; and that *on*, or *soon after*, the retreat of the glacier, the delta began to be formed.

I wish that competent observers would work out this problem in the case of other Alpine deltas.

With reference to the last paragraph of Mr. Mackintosh's paper, I cannot but think that there *is* good historical evidence, *that climate has been undergoing a change.*

E.g.—(1) Job apparently dwelt in the land of the Crocodile and Hippopotamus ("Leviathan" and "Behemoth"), yet he mentions "ice" twice, "snow" five times, and "frost" three times; and speaks of streams "blackish by reason of the ice," and "the face of the deep is frozen;" phenomena hardly to be found in those lands in the present day! (2) So also the way in which the old Roman historians and poets speak of the Winter seems to imply that the climate was much more severe in their day:—

e.g.—*Horace* (1 Odes, ix.) ". . . . geluque
Flumina constiterint acuto."

Hard frozen rivers are certainly not characteristic of the Roman Campagna *now!*

Livy speaks of armies going into winter-quarters as a general custom.

Even the great Hannibal went into winter-quarters at Capua. But, surely, the winter is the only pleasant and suitable time for physical exertion in the *Capua of to-day!*

Doubtless, many other illustrations of this point will occur to those present at the meeting on Monday. And, indeed, one can hardly conceive it possible for the hot and enervating climate of *Modern Italy* to have developed so hardy and vigorous a race as the ancient Romans!

The same remark would, more or less, apply to the ancient Chaldeans, Assyrians, Medes and Persians, Egyptians, and Greeks, who, in this respect, contrast most favourably with their modern representatives.

FROM THE REV. W. GUEST F.G.S.

The paper of Mr. Mackintosh has evidently been written after a close personal examination of the phenomena in question, and, considering the brevity to which the author has confined himself, is one of remarkable clearness. I regret that I cannot be present when the paper is read, but, as a member of the Institute for some years, I should be deeply obliged if the expression of my personal thanks could be conveyed to the writer.

To me it has been evident for several years that the question which would come to the front in geological research was that of the probable date of the close of the Glacial Period. The Victoria Institute has done the very highest service in accentuating the importance of this inquiry, and it is very much to be wished that, after the manner of the British Association, there were a fund at its disposal to encourage investigation. No research could yield more valuable results, or help better to throw light on very critical problems. It is too much to expect gentlemen to pursue on their own account such laborious studies, journeys, and field inquiries, as the subject demands; but Mr. Mackintosh has led the way, and placed all members of the Institute under the greatest obligation. It is of moment that the matter should not rest at this point.

MR. MACKINTOSH'S REPLY.

I thank the speakers for their kind comments. In reference to a remark by the Rev. W. B. Galloway, on the glaciers, I may say that Professor J. Geike believes that small glaciers came down the Scottish glens to near the sea level as late as the Neolithic times, if not later. In regard to another point, I would say that on the Norber plateau in Yorkshire, in many places rain falling from boulders has made no impression on the surrounding flat limestone rocks. I wish to add that there is much truth in Mr. Charlesworth's remark on the mere mechanical action of rainwater on limestone rocks; but when the chemical and mechanical action are combined, there must be lowering of the surface to a considerable extent.