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JOURNAL OF  
THE TRANSACTIONS  
OF  
The Victoria Institute,  
OR,  
Philosophical Society of Great Britain.

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EDITED BY THE SECRETARY.

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VOL. XXXVIII.



LONDON :

(Published by the Institute, 8, Adelphi Terrace, Charing Cross, W.C.)

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1906.

## ORDINARY GENERAL MEETING,

WAS HELD IN THE ROOMS OF THE INSTITUTE, ON  
MONDAY, APRIL 23RD, 1906.

MARTIN L. ROUSE, ESQ., B.L., IN THE CHAIR.

The Minutes of the previous Meeting were read and confirmed and the Secretary stated that he had received a telegram from Mr. Hudleston, Vice-President, regretting his inability to be present and to preside.

The following paper was then read by the author :—

*ICE OR WATER.* By Sir HENRY H. HOWORTH, D.C.L.,  
F.R.S. Review by Professor EDWARD HULL, LL.D., F.R.S.  
(Secretary).

**T**HE author of this work has been so good as to present me with Vols. I and II, and as he warmly invites criticism, it seems to me that the best return I can make for the gift of my valued friend is to examine some of his facts and arguments, and to try and induce him to accept ideas more consistent, as I conceive, with physical facts and sound reasoning thereon.

On receiving the volumes some months ago in the height of the busy season, it occurred to me that some of my colleagues of the Victoria Institute might be induced to undertake the preparation of a review which might be read before the Institute during the coming session, and discussed in presence of the author himself. But, failing in my effort, I resolved to keep the volumes, and as a "vacation task" undertake a review of their contents for the Session of 1906. The result is the present paper.

I need scarcely say I am no fit antagonist for such a master of physical dialectics as Sir Henry Howorth, nor can I lay

claim to the wide extent of reading from authors, not only British but European and American, evinced by the volumes themselves, especially on subjects which have occupied his pen and attention for sixty years, as he himself states. The present volumes are the third part of a trilogy directed against the prevalent errors of geologists according to the views of the author, of which the first is *The Mammoth and the Flood*, and the second is the *Glacial Nightmare*. The third volume of the present work is still in abeyance.

The author objects, and rightly, to have his views criticised by novices, some of whom "have never seen a glacier," and this being so it is necessary for me to show my credentials for the office of critic and controversialist.

Like the author, I have for many years been engaged in studying glacial phenomena both at home and abroad. My first lessons on the effects of glaciation in the region of vanished glaciers were received under an able master of this subject, the late Professor (afterwards Sir Andrew) Ramsay, amongst the hills and valleys of North Wales. Ramsay afterwards published a treatise, not mentioned by our author, *The Old Glaciers of North Wales*, and afterwards his celebrated paper on "The Glacial Origin of Lakes,"\* which, notwithstanding all that has been written on the subject by opponents of his theory, has not, I venture to state, been seriously undermined.

When carrying out the Geological Survey of Lancashire and Cheshire some years later, I carefully studied the drift deposits, which are there developed on a great scale, and are well known to Sir Henry Howorth. The late Mr. Edward Binney had previously been engaged on this work and had classed the divisions of the Drift into Boulder Clay (Till) below and sands and gravels above; to this series I added the "Upper Boulder Clay," a very important division which our author has (as it seems to me) overlooked or confounded with the Lower Boulder Clay or Till—a source of many errors amongst geologists.

My next work was amongst the mountains of the Lake District. Up to this time (1864) glacial phenomena had not been recognised as such in the Lake District, the boulders, *roches moutonnées*, and ice-striations having been accounted for on the hypothesis of Buckland's General Deluge in his *Reliquiæ Diluvianæ*. However, after the knowledge I had gained with Ramsay in North Wales, it was not long before I was able to announce to him that I had observed similar glacial phenomena

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\* *Quart. Journ. Geol. Soc.*, vol. xviii, p. 185.

in the valleys of Westmoreland as those which I had seen in the former district; and, map in hand, I set about a detailed survey of the glacial striæ throughout the whole of the Southern Watershed of the Lake District.

The results were published in a paper, illustrated by drawings, in the *Edinburgh New Philosophical Journal*, then edited by Professor Edward Forbes.\*

On being appointed to the charge of the Geological Survey of Ireland, I, together with my colleagues, especially Mr. Kinahan, made a special study of the Drift phenomena. Wherever the glacial striations were observed, their directions were carefully inserted on the six-inch maps, and have resulted in showing a beautiful system of ice-movement directed from an axis of maximum precipitation crossing Ireland from Antrim to Mayo, with occasional centres of dispersion—as, for example, in the Wicklow and Killarney mountains.†

Some time previously the late Rev. Maxwell Close, a most able and learned observer, had produced a glacial map showing the direction of the ice-movement over a large part of Ireland, on which my own later map was partially founded. Close was also the discoverer of the marine shells of living species in County Wicklow, in gravel at an altitude of 1,200 feet above the sea-level, in keeping with those of Moel Tryfaen in North Wales.

As for the rest, it may suffice to state that I have visited glaciers in Switzerland and Norway, and paid special attention to the moraines, both lateral and terminal, of several existing glaciers.

Having thus stated my personal observations and experiences, I should hope sufficiently, to allow of the right to be heard regarding Sir H. H. Howorth's views, I proceed to offer some remarks on a few selected subjects in these volumes, premising that they deal with only a small portion of the wide field over which the author has thrown his net, or over which he has run tilt against many able and distinguished antagonists. I shall confine my remarks to three subjects. First, the cause of the "Ice Age," or Glacial Epoch. Second, the erosive effect of glacial ice; and third, the power of glacial ice to transport

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\* One of my drawings was afterwards reproduced by Lyell in his *Antiquity of Man*, with due acknowledgment; it is a *roche moutonnée* in Ambleside churchyard. More recent observers have been less careful to recognise my priority in this field.

† A map showing the general Glaciation of Ireland accompanies my little volume on the *Physical Geology of Ireland*, 2nd edit.

matter and surmount obstacles. It need scarcely be observed that to deal with these subjects otherwise than briefly, would be impossible in an essay such as the present. The author passes in review the various theories that have been propounded to account for the Glacial Period. He examines the astronomical theories of Croll and Sir R. Ball, and rejects them on what seems to me sufficient grounds, notwithstanding the high authority of the authors of the theories themselves.

The author then goes on to deal with the views of Lyell, Professor J. Geikie, Professor Prestwich, Chamberlin, and others, and finally concludes with rejecting the generally accepted evidences of a Glacial Epoch of Post-Tertiary age. I hope I am not misrepresenting my friend, but the following are his words:—

“Are we obliged, or in fact are we justified, in invoking a great Ice Age with its portentous ice-sheets . . . in order first to account for the striæ on the polished rocks and on the boulders, and secondly, for the manufacture of angular drift? To my mind the questions only need to be asked to answer themselves.” Again: “I have shown that the striæ can be, and ought to be, assigned to an entirely different agent than ice if we are to follow inductive methods.” (Preface, p. xliii.) He then goes on to dispute the glacial origin of moraines, referring them to the movement of stones and boulders over the rock-surfaces by the action of concurrent and divergent streams of stones, in many cases covered by drift.

To this I will reply, that no one who has studied the symmetrical arrangement of lines of grooving and striation over the glaciated surfaces of solid rocks in glacial districts could for one moment suppose they had been produced by the rubbing of stones and boulders promiscuously passing over the surface.

Such statements as those quoted induce the doubt whether so gifted an observer as the author has not allowed his better judgment to be warped by a mistaken conception of the nature of glacier ice.

When the author comes to deal with the latest theory, namely, that called by Mr. G. K. Gilbert “The Epeirogenic Theory,” he evidently feels that he is treading on more dangerous ground than when dealing with the views of the before-named glacialists. In the first place this theory has the support of a number of very distinguished adherents in America—and to a less extent in this country—at the head of which stands the venerated name of Professor J. Dana, followed by those of Chamberlin, Warren Upham, Professor

J. W. Spencer. To these may be added Dr. Nansen and the author of this essay.\* The gist of this theory is that it attributes the cold climate of the glacial period to the elevation of the land far above the present levels, as shown by the submerged (or "drowned") valleys continuous with those of the present day and passing under the ocean down into the abyssal floor as shown by the soundings. Sir Henry Howorth admits that this theory has a good deal to be said for it. He says "The one theory which still has a respectable following, not in this country, but in America,† is the so-called Epeirogenic theory of an ice age. It is based on a very plausible and true idea, namely, that the low temperature of high latitudes is very largely caused by, and dependent on, the high level of the land there, and if we could secure a sufficiently elevated mass of land in high latitudes in so-called glacial times we should have done a good deal to explain the glacial theory." (Vol. ii, p. 2.)

Now this, I maintain, has actually been done; and it is no fault in our author that he has not seen his way to accept this theory, because much of the evidence on which the fact of the high elevation not only of the Arctic regions, but of those lying to the south of the Arctic circle, has been founded

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\* In my paper on "Another Possible Cause of the Glacial Period," *Trans. Vict. Inst.*, 1898.

† The statement that the Epeirogenic theory has not many adherents in this country is only partly true. Like every new idea, it takes time to spread; but that it is gaining adherents there can be no doubt. The existence of the submerged valleys is scarcely denied by any who have taken the trouble to examine the matter for themselves. There is only one outspoken opponent, and Professor Spencer has sufficiently answered him; but my charts with the isobathic contours, showing the sub-oceanic terraces and valleys, have been laid before Lord Kelvin, Lord Avebury, Mr. Teall, Lord Ducie, Professor Spencer and others, besides scientific assemblies in Dublin, Bristol, Manchester and Glasgow, two of these being British Association Meetings; and lastly, the Royal Geographical Society and the Victoria Institute. I here insert a copy of a letter recently received from Professor T. Rupert Jones, F.R.S., formerly Secretary to the Geological Society, which will serve to show the views of a very leading and experienced geologist on the subject of the submerged valleys. The letter is dated 18th June, 1905, and is as follows:—"Dear Dr. Hull,—I am delighted to find that your conclusions with regard to the 'Submarine Platform and Valleys' have been so clearly and exhaustively reviewed with pleasing concurrence and strong support by Dr. Spencer in his paper published in the *American Geologist* of March last, and of which he has kindly sent me a copy." Till I received this gratifying letter, I was unaware that Professor Rupert Jones was a supporter of my views. That he is so is a source of much satisfaction.—E.H.

has been obtained within very recent years—perhaps after some of the pages from which I quote were written.

The Epeirogenic theory in general characters resembles that of Lyell—which has been rather slightly touched upon by our author—but it differs therefrom in this respect, that Lyell's theory is based on the interchange of land and sea, rather than on the vertical uplift of the land. Lyell showed in his great work (*Principles of Geology*) that if the great mass of continental land was disposed round the pole—and its present position occupied by the ocean—glacial conditions would be the result. Of this there can be no doubt; but there is no evidence that such a distribution of land and water took place in Post-Pliocene times. It was an hypothesis and nothing more.

The Epeirogenic theory, on the other hand, is based on actual observation by means of soundings along both sides of the Atlantic and more recently by Nansen in the Polar seas. These observations unquestionably prove that the existing river-valleys entering the ocean are prolonged outwards under the surface, and traverse the continental platform in the form of cañons, with well-defined sides, to depths of several thousand feet. As such valleys could only have been eroded under the atmosphere, the inference is simple and inevitable, that these areas were in the condition of land when the valleys were in course of formation.

The credit of working out the form and direction of these "drowned valleys" on the American side is chiefly due to Professor J. W. Spencer, whose name scarcely occurs, I regret to say, in the volume now under review; but undoubtedly it would have added much to the value of this work if there had been a full treatment of the subject regarding the formation of the sub-oceanic physical features.

As members of the Institute are aware, the writer has contributed several papers descriptive of these submerged valleys on this side of the Atlantic\* to the *Transactions*, and the determination by Dr. Nansen of similar features bordering the Arctic lands (including the continental platform and the valleys by which it is traversed) ought to assist in dissipating the unreasonable prejudice which has retarded the general acceptance of the results at which we have arrived.

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\* Vols. xxx, p. 305, xxxi, p. 259, xxxii, p. 147; and Professor Logan Lobley, vol. xxxiii, p. 419.



According to our author, Professor Dana was the originator of the Epeirogenic theory (vol. i, p. 135), and his views are thus concisely given:

“Dana argued that the fiords which exist so much in northern latitudes were valleys eroded by streams during a formerly greater elevation of the land in high latitudes. The culmination of this uplift, he argued, gave rise to a high plateau climate, with abundant snow-fall, forming an ice-sheet. This movement of elevation was followed by one of depression, during which the ice-sheet was melted away; and this again was followed by another elevation, bringing the land to its present height.”

These views are supported by Mr. Warren Upham, and I may add are very closely in harmony with those I have advocated for many years, and with those of Professor Spencer.

The key to the problem lies in the occurrence of an “Interglacial” epoch, an epoch of depression succeeding that of high elevation, and followed by a partially recurring cold period of re-elevation. It seems to me that had our author recognised these stages he would not have experienced the difficulties on which he dwells: for example (p. 136), where he speaks of Greenland, Scandinavia and North America being “at a much lower level in the so-called glacial times than they are at present.” The evidence for this statement is derived (I presume) from the occurrence of the raised beaches, with marine shells at various places in these countries—but these terraces are in fact post-glacial; more recent than the later glacial period, and certainly than the interglacial.

In no part of the British Isles are the three divisions of the drift deposits better shown than in the County of Lancashire, with which Sir H. H. Howorth was so honourably connected some years since\*; and out of the numerous sections of these deposits I would point his attention to the fine section in the valley of the Ribble, a few miles above Preston, which I figured and described many years since. Here at a point where the river makes a fine curve in its course, the banks rise to about 120 feet in height—the whole in drift deposits representing the three stages above referred to. They are as follows, downwards:

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\* As M.P. for Salford.

*Section in the Banks of the Ribble near Balderstone Hall.*

	Approximate thickness.
3. <i>Upper Boulder Clay</i> ; Red, partially stratified clay with some stones round or sub-angular	60 feet.
2. <i>Interglacial Beds</i> ; Stratified beds of water-worn gravel and sand ... ..	50 "
1. <i>Lower Boulder Clay</i> (Till). Dark, stiff clay with angular blocks and pebbles ... ..	10 "
Total	120 feet.

The sandstone supporting these deposits was not visible at the water edge, but was doubtless close underneath.\*

These deposits are spread over a large area of the north and centre of England, and are representative of the three divisions of the glacial period—the lowest of the land ice-sheet—and period of maximum cold and elevation; the middle, of the Interglacial submergence and the return of warmer conditions due to the greater influence of the Gulf Stream; and the Upper, of partial re-elevation and deposition under the waters of a glacial sea, charged with mud derived from the still existing glaciers which retained their hold on the higher levels of Wales, Cumberland and the Scottish highlands. The occurrence of this stratified upper boulder clay with shells explains one of the difficulties which have beset our author, as also Mr. Warren Upham. The shells do not (as far as I am aware) occur in the Till or Lower Boulder Clay, but only in the Upper Boulder Clay which was deposited in sea-water.†

Our author, when dealing with the fiords, denies that they are partially submerged river-valleys. When writing on the subject of the Norwegian fiords for this Institute, I assumed as beyond controversy that such was the case‡; but our author raises the objection that they are deeper some distance up from their mouths than at the outlets themselves. This remarkable fact, the knowledge of which is derived from the soundings on the Admiralty charts, I had ascertained for myself, but it did not lessen my belief in the fluvial origin of these remarkable

\* "Geology of the Burnley coal field, etc.," *Mem. Geol. Survey*, p. 129, Fig. 26 (1875).

† There may be conceivable cases where shells may be met with in the Till, but these are quite exceptional. The Till, when resting on solid rock, has its floor generally striated and polished.

‡ "On the Physical History of the Norwegian Fjords," *Trans.*, vol. xxxiv, p. 125.

physical features. The deepening of the central part of the fiords I attributed to the erosion of the glaciers which occupied the valleys during the Ice Age, and in this view I am supported by Professor Spencer and, I believe, Dr. Nansen. An additional cause of the shallowing towards the outlet is the accumulation of vast deposits of moraine matter, thrown down by the vanished glaciers of this period.

Now, Sir Henry Howorth is very unwilling to credit glacier ice with any erosive power over its floor; but he gives his case away when (dealing with this subject) he says:

“To be a little more concrete, I would urge that ice, being a viscous body, when armed with suitable tools in the shape of stones, can polish and in *some measure erode*, but cannot, except under very exceptional and peculiar conditions, and in very limited areas, excavate and dig!”

The author seems aware that in dealing with the erosive power of ice “armed with tools,” he is treading on very slippery ground, and if glacier ice thus equipped, and of enormous thickness (in the case of the Sogne Fjord probably 5,000 feet) can “in some measure erode,” why not during long ages can it not grind a hollow where it is most thick and presses on its floor with greatest weight, namely, in the centre of its course towards the sea?

It was for this reason that Ramsay suggested that to glacier ice was due the deepening of the great lakes (if not their actual and initial formation) on both sides of the Alps, and to this cause alone can the deepening of the Scandinavian fiords in the central portion of their course be referred.

Sir H. Howorth denies that glacier ice is capable of passing over hills or elevated ground lying in its path, or “to travel over the enormous stretches of more or less level country” (Preface, p. 37). This statement I can meet with several examples taken from each of the three countries constituting the United Kingdom, and they are derived from personal observation.

First. A fine glacier formerly descended the Langdale Valley in Westmoreland, having its source in the snowfield which occupied the Central Mountain heights of the Lake District. At its lower end occurs a ridge, a few hundred feet in height, thrown athwart the valley itself, which might well have been supposed to form an effectual barrier to the movement of the glacier—not at all! The striations, which are perfectly distinct, and parallel to the centre of the valley, are seen to ascend and pass over the obstruction to the opposite

side, showing that the hill was no effectual impediment to the ice-movement.

The second example is taken from the Firth of Clyde. Those who know this part of Scotland will recollect that the Valley of Loch Long enters the Clyde opposite Greenock in a direction at right angles to that of the latter. A glacier descended from the Argyllshire highlands through Loch Long into the Firth of Clyde, which is very deep at this part of its course, and on the south side of the Firth the ground rises out of the water into considerable hills. These are formed of basaltic rock, mammillated and striated with glacial markings. But the remarkable fact is that the striæ point in the direction of the Loch Long Valley—not in that of the Clyde; in other words, approximately north, not westward, which is the direction of the banks of the Clyde at this place. It is clear, therefore, that the glacier, coming down from the north, passed right across the Clyde basin and ascended the high ground forming the southern bank. The evidence is perfectly clear in this case\* that the ice ascended the ridge opposed to its course.

My last instance will be taken from Ireland, of which a glacial map will be found in my little work *The Physical Geology of Ireland*.† This map does not support the view that “a glacier cannot travel over enormous stretches of country,” as it shows that the whole of the central plain of Ireland was covered by an ice sheet moving along lines in a southerly direction and originating in an axis running along the borders of Ulster. Now here we have (at least) one remarkable example of the power of glacier ice to ascend and pass over obstructions to its course and to travel over large stretches of country.

Again; standing on Bray Head, about 900 feet above the sea, and 200 feet above the plain, and formed of Cambrian grits and slates, we observe that the rocks are finely glaciated and striated by lines pointing in (approximately) a north-west direction, that is to say, over the plain, formed of carboniferous limestone which stretches away at a depth of several hundred feet beneath our feet. In other words, the ice, moving over the plain from the north-west (the position of the central axis of movement), has ascended the slopes of Bray Head and passed over the summit in the direction of the sea. When I first observed these phenomena I was, I confess, struck with amaze-

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\* The glacial striæ at this place were marked by myself on the 6-inch map when I was carrying out the Geological Survey in 1870.

† 2nd edition 1891.

ment, and I had to fall back on the theory of the *vis a tergo* arising from the enormous accumulation of snow over the area of the central axis of dispersion, supplemented by Tyndall's views of the molecular movement arising from the diurnal melting and re-gelation of the ice in the body of the glacier itself. I should add that the flanks and summit of Bray Head are strewn with boulders of limestone, granite and other rocks foreign to this neighbourhood, the sources of which can be determined in several localities over the Central Plain.

With these examples I conclude my essay and criticism. There are many points on which I agree with the author; as, for example, the estimate of about 10,000 years (or less) for the glacial period advocated by Gilbert, Upham and Prestwich; but I fail to find that he has grasped the full significance of the phenomena presented by the Post-Tertiary Ice Age, or that he has recognised the changes of level which the crust has undergone during that period, or the effects resulting from these changes.

On reading over this paper again after the interval since it was written, I am sensible that it is far from being a sufficiently comprehensive review of the work of Sir Henry Howorth. Even the points dealt with would, with advantage, have merited a more extended consideration. But I hope it will be admitted that I have endeavoured to meet the questions on which we differ in a fair and courteous manner. For my own part, no one dislikes controversy more than myself.

#### DISCUSSION.

The CHAIRMAN.—I am sure we are immensely interested in this review. Although we have not all had time to master the book, of which this is a review, still we have had the pleasure of hearing Professor Hull set forth his deductions of the evidences of the river valleys underneath the sea through the submarine plateaus, and he has most ably proved his points. I now call upon any who have remarks to make to speak to us on this subject. I hope we shall have a very interesting discussion. I regret the absence of Sir Henry Howorth.

Mr. PILKINGTON, M.Inst.C.S., having discussed the subject at some length.

Professor LOGAN LOBLEY, F.G.S., said:—I have not had the advantage of reading Sir Henry Howorth's work, and so I am scarcely prepared to discuss.

I am very glad to be present to hear the remarks of Professor Hull. I generally agree with what he has to say on geological subjects, and in his paper I am almost in full agreement—there may be one or two little points on which I differ—but generally speaking I am quite in agreement.

The great question of the cause of the glacial period, as it is termed, has been worked out, I think, very well by those who have supported the Epeirogenic theory. I may say I am very much in accord with that theory. The fact of there having been a glacial period can admit of no doubt from those who observe nature in the regions which have been subjected to this inferential glacial action. Two or three weeks ago I was in North Wales and I saw there abundant evidence of glacial action. These evidences of course are well known to geologists. Mr. Pilkington has said that there could not have been a glacial period. I should recommend him to take a little tour to North Wales and see for himself the evidence that there has been such a glacial period. He says the earth is too hot now and oppressive; when it was cooler there could not have been any glacial period; but may I venture to say that there *is* a glacial period now which he cannot doubt. There is a glacial period in Greenland at the present time. Is the earth too hot for glacial conditions there? There is a glacial period all round the North Pole and all round the South Pole, where we have glaciers going off fully thirty miles in length. My friend William Bruce, of the Scottish Antarctic Expedition, passed an iceberg as large as the Isle of Man, floating past the land *area* which surrounds the South Pole; so we have a glacial period on the earth at the present time in certain areas, and the only question is whether these glacial conditions have been more extensive at that time than they are at present; not whether there is or has been a glacial period. *There is a glacial period.* The only question is whether the glacial conditions now existing have at a former period extended over more extensive regions than at present. We have, not only in North Wales, but in the North of England, the Lake District, in Wales and many parts of England and Ireland, sure evidences of previous glacial action.

I was very glad that Professor Hull brought out prominently his differentiation between the older glacial clay and the newer glacial clay, for it has always been a difficulty to account for the remains of fragile fossils in the boulder drift, seeing that the ice action would probably crush these to pieces. In Northampton some time ago I collected from the glacial clay which overlies the Oxford clay very complete fossils indeed, which I identified from the Lower Lias clay, from the Upper Lias clay, and some from the Kimmeridge clay—fossils not crushed or injured, but as good and sound as when they were in the original clays. That shows that the whole of the glacial deposits which we have in the Midlands have not been the result of land ice, but that these particular glacial deposits have been deposited there from ice-masses floating over the sea, and they have been dropped and have not been the result of the pushing on over the land by ice. We have centres of dispersion of ice, as in the north of Ireland and Scotland, and through the Lake District and Wales, centres of dispersion of large glaciers. On the other hand the glacial deposits above referred to have been the result of material brought by floating ice and deposited in water.

As regards the time that is given by Sir H. Howorth of 10,000 years, that seems to be inadequate to explain all the changes that we know have taken place during the glacial period, for we have the elevation of shells on Snowdon, 1,300 feet, which must have taken place during an epoch of depression of the land during that period, and 10,000 years seems too small an amount of time to allow for these great changes.

Mr. ROUSE.—(Referring to the shells.) Are they at all associated with any glacial phenomena? They are shells that are living in the Irish Sea now, quite recent shells.

Professor LOGAN LOBLEY.—If we allow sufficient time there is no difficulty at all in imagining the great uplift elevations that were necessary to produce a climate such as would cause severe glacial conditions, for we must remember that although 5,000 feet seems a tremendous change of level, that is only about one mile or one four-thousandth part of the diameter of the earth, and with expansion and contraction of the masses of the globe. A very slight amount of expansion or contraction would account for an alteration to the extent of one four-thousandth part, and we have

evidences in the Himalayas of land having been raised to 14,000 feet since the miocene period, so we have no difficulty in imagining this elevation.

All these phenomenal things abundantly confirm the conclusions geologists have come to, that there has been a post-tertiary glacial period, and that there were glacial conditions on a much more extensive scale than exist at the present time.

Mr. J. BRIDGES-LEE.—I have not had the advantage of reading the work of Sir Henry Howorth. I know something of Professor Hull's work and I have listened with considerable interest to the paper which he has read. There is apparently even in these days a fair amount of misconception abroad about the glacial question, which I take to be a matter of certainty, as much as anything is certain.

A number of years ago during the period called the glacial period there was an immensely larger amount of ice action than now in this island, and I take it that the glacial action that is talked about is glacial action in this part of the world. It is a fact that there was this glacial action; it is proved by such an enormous amount of evidence that I take it that geologists who have devoted their lives to the study are practically unanimous about this, as regards this country as well as throughout the rest of the world. The cause may be open to a certain amount of discussion, and a good deal of doubt of the total number of causes which have been at work. It is impossible for people to be altogether free from doubt. I notice Professor Hull has not alluded to a theory which I have always been in the habit of associating with the glacier period, that the motion of the earth's axis, the motion about its own centre, the motion of an hour-glass, described an angle, and this would tend to affect the motion, angle and incidence of the sun's rays upon this part of the earth and tend to affect the temperature. A great increased elevation of the mountains in Norway and parts of England, Ireland and Scotland would undoubtedly be the cause for the development of much larger quantities of ice and snow in temperate and high latitudes. There is abundant evidence now to show us that in many regions the land was very much higher at or about that time than it is at the present time.

One of the theories for the possible causes which might have affected the temperature in this country might be alterations in the



surface level in some very distant places. We must remember that at this present time the temperature of England is above the ordinary temperature of other countries in the same latitude. We are warmed by the Gulf Stream ; we are warmer than we should be if the Gulf Stream did not come our way.

There have been a great many causes but there is abundant evidence to show there was an enormously increased amount of ice at the period of which Professor Hull has been speaking.

Then about the striæ going over hills and across valleys, one gentleman who has addressed us said there is no movement in the ice. I cannot help saying we are dealing with something which has been so much investigated it is outside the region of controversy. Professor Tyndall made a very careful series of examinations, and the exact rate of motion of a glacier has been determined ; and not only has the rate been determined but the reasons why ice moves have been pretty well worked out.

Ice is one of those curious things which behaves in a curious way. When water freezes it expands. Most other articles contract. Water expands, and if you take out the ice at or about a freezing temperature and crush it by hydraulic pressure you could crush it into any shape. When the pressure is put on the ice yields ; it becomes liquid ; but it solidifies again immediately pressure is taken off ; and, speaking from memory, I believe that Professor Tyndall succeeded in squeezing ice into a lens, and other forms. At the bases of heavy glaciers where you have ice hundreds of feet thick in some places there would be enormous pressure at the bottom. The ice at the bottom will be in contact with the earth, and the upward convection of heat will tend rather to raise the temperature of the floor of a glacier towards the bottom. The pressure of the ice upon this will cause it to liquefy in the neighbourhood of the solid surfaces, and then solidify again immediately, so that the glacier moves on. The ice at the bottom gets crushed by the heavy pressure and the temperature at the bottom is at, or near, the melting point because it comes in close contact with the earth at the bottom. It has been proved, the ice where the pressure is greatest, will melt underneath, slightly melt, and solidify again immediately when it has got to a place where the pressure is less. That helps to account for the flow of the glacial ice. I do not know if that is the full explanation. There is

another explanation which might perhaps apply. No bodies in Nature are absolutely rigid; rigidity is a negative quantity, and ice is a body which has a certain small residuum of fluidity appertaining to itself, in the same sort of way as lava or treacle, so that it flows slowly. This is our hypothesis based upon certain broad facts, but we have the fact that the glacier does move forward, and that in moving forward it will succeed in moving over hillocks of moderate height at least, and will leave traces of its movement behind; and that traces are left of a perfectly unmistakable character, rocks getting polished, scarred and striated, which can only be accounted for by the assumption that there has been extensive glacial action.

Mr. ROUSE.—Would not the existence of caverns running underneath glaciers for some distance, would not that be in keeping with Tyndall's theory of the ice melting at the bottom in coming into contact with earth and then afterwards solidifying again?

Mr. BRIDGES-LEE.—The bottom of the glacier would tend always to be at most of the bottom in a melting condition. All glaciers flowing over uneven surfaces, and the sun's rays melting the surface, the water runs down through the crevasses to the bottom, and so works out along the basin of the glaciers. For every glacier is practically the same: from the end of the glacier you have a stream of water issuing, and that water is made up of a number of little rills which have melted during the day time, owing to the action of the sun on the surface.

Professor HULL.—I think the discussion has been one of very great interest. We congratulate Mr. Pilkington on surviving to the present day and being present here after those terrible periods of cold that he has passed through in Canada. I do not see how the observations that he had made then, and which he has now recounted, really affect the question with which I have endeavoured to deal in my essay. I think the questions stand quite aloof. I will only refer to one point, where he said there is nothing in the Bible which indicates the existence of a glacial period. Quite true; but can you suppose that in Palestine, in that warm climate, anything in the shape of glacial ice would have been present to attract the attention of the writers of the Old Testament history? But notwithstanding that, let me say that Mount Hermon in the Lebanon, which rises 12,000 feet above the sea, was undoubtedly

covered with perennial snow, and sent down a magnificent glacier to a level of 4,000 feet above the present surface of the Mediterranean. That glacier is represented by the great moraine on which the cedars of Lebanon are growing at the present time; and it was identified by Sir Joseph Hooker. The existence of a glacier at that period was long before the writers of the Old Testament were born.

I feel gratified at the concurrence of Professor Lobley with what I have stated in my paper. I think it is too late to dwell upon them or to add anything to what I have said. I should like to say that in regard to the centres of dispersion of the ice period in Ireland that even at the extreme south-west of Ireland, where the temperature is much the same as that at Biarritz, in the west of France, there were large glaciers coming down from the mountains of Kerry which were covered with snow, and sent down glaciers through the valleys into the sea and on to the land. Their traces are very clearly shown, so that the extension of the ice must have been very prevalent over a large part of the British Isles.

Mr. Bridges-Lee has referred to a possible cause of the glacial period, namely, the movement of the axis. No one can deny that if there had been such a change in the equator, with regard to the ecliptic, it might have brought about such a change as would produce a glacial period, but I am so strongly impressed with the view that it was owing to the elevation of the whole land along Europe and West Africa that it is unnecessary to have recourse to such recondite reasons as that referred to. I am really unwilling to accept any other theory for that remarkable period in geological history. I am obliged to you for your kindness; I trust that nothing I have said, or any opinion expressed, could possibly give offence to the author of this work.

The meeting closed with the usual votes of thanks.

COMMUNICATION.

The following communication was received from Mr. F. W. HARMER, F.G.S.

Dear Professor Hull :—

I fear that our friend Sir H. H. Howorth is so confirmed in his own views that your well-meant effort to convert him has but little chance of success. On the other hand, his views seem to be making no progress ; in spite of the earnestness and forensic skill of his writing I do not think he has made a single proselyte among field geologists.

A vast amount of information has been collected during the forty years over which my interest in this subject extends, as to the pleistocene deposits of England, and the erratic boulders they contain. Dealing with the subject as a whole, it is found that these drifts arrange themselves in clearly defined groups, different alike in origin and distribution. Now it is hardly fair for Sir Henry to imply that those who think that the most satisfactory explanation of this distribution is that it is due to the action of ice, are like men half asleep, under the influence of some absurd and senseless "nightmare," unless he has himself something better to offer. To suggest that these deposits may be due to a great flood is a guess, pure and simple. Before such a view can be entertained, much less discussed, it is necessary to show, in detail, that it can be made reasonably to accord with the observed facts, and with all of them.

For some years I have been endeavouring to construct an erratic map of England and Wales, and hope shortly to publish it. I believe it will be found that the land-ice hypothesis gives, not only a possible, but the only satisfactory explanation of the distribution of the drift. I shall respectfully challenge Sir Henry to show that it can be as well explained on his hypothesis.

It would be easy to give instances as to the movement of erratic blocks having a similar bearing on the question as the striations mentioned in your paper. There is, for example, the well-known case of the Shap granite boulders, which occur along a trail starting from the mountain region of Westmoreland. Crossing the Valley of the Eden, the bottom of which is between 500 feet and 600 feet only

above the sea level, it climbs the Hainmoor pass (about 1,400-1,500 feet), descending thence along the Tees valley to Darlington (about 150-160 feet). From Darlington the Shap boulders are carried in two directions, first to the mouth of the Tees, and to the south-west along the Yorkshire coast, from Saltburn to Flamborough Head and Spurn Point; and secondly, along the Vale of York, through which they have been traced as far south as Barnsley and Doncaster.

Let me give another case from East Anglia, equally interesting. There are found in Lincolnshire on the west slope of the Wolds, as at Market-Rasen, and elsewhere, some peculiar erratics of Neocomian age, which are as easily identified as Shap granite. Boulders of the same kind are exceedingly common in West Norfolk, not only on the low ground bordering the Wash, but also on the higher land of the chalk escarpment. From this region I have traced them in a south-east direction, forming a broad but well-defined trail, which crosses the valley of the Little Ouse (50 feet), and then climbs the boulder-clay plateau of central Suffolk (over 200 feet), finally reaching lower ground to the north of Ipswich. It is difficult to understand how the distribution of these two groups of erratics, in regions open on all sides to the sea, to which flood water would naturally flow along the easiest route, could be explained in any reasonable manner by Sir Henry's hypothesis. If, however, all the similar cases which might be given had to be considered together, the difficulty would be, I think, insuperable.

Yours very truly,

F. W. HARMER.