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1881.

ORDINARY MEETING, March 15, 1880.

J. E. Howard, Esq., F.R.S., in the Chair.

The minutes of the last meeting were read and confirmed, and the following elections took place :—

Member :—J. F. Bateman, Esq., F.R.S., London.

Associates.—General Sir G. Malcolm, K.C.B., London ; Rev. G. Blencowe, Sheffield; Rev. R. Phair, Canada; E. Skinner, Esq., M.R.C.S., Sheffield.

The following paper was then read by the Author :—

ON THE EVIDENCE OF THE LATER MOVEMENTS OF ELEVATION AND DEPRESSION IN THE BRITISH ISLES. By Professor T. McK. Hughes, M.A., Woodwardian Professor of Geology, Cambridge.

IN the course of some remarks made by the Duke of Argyll upon a paper which I had the honour of reading before this Society upon a former occasion,* his Grace said, "I wish the attention of geologists were more directed to the question connected with the admitted fact of sea-gravels at a high elevation on our Welsh and Scottish mountains." In consequence of which remark I was asked to put together such observations upon this subject as I had made myself or could collect from others, and communicate them to the Society.

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* The title of the paper (read March, 1879) was, "The Present State of the Evidence bearing upon the Question of the Antiquity of Man" ("Transactions," vol. xiii. page 316). The following is the text of his Grace's letter (Ed.) :—

"I concur entirely in the general argument of Professor Hughes on the antiquity of man.

I would observe, however, that it assumes, as most geologists do generally

I have endeavoured to comply with this request, and propose to take the questions thus:—

1. What are the facts referred to?

2. What are the inferences to be drawn from them as to the extent and relative age of the earth-movements, and their bearing upon the origin and age of the river-deposits from which the remains of man have been procured.

We have, first, the evidence of elevation in the raised beaches recorded at various localities all round our own coasts; and, secondly, the evidence of depression in the submerged forests.

Connected with the raised beaches we have to inquire into the direction of the drift of the stones of which the shingle is composed, as illustrating the set of the currents, the position of the straits, and open shores. We have to compare the shells and other organisms preserved in the old shore-deposits with those that exist on the nearest coast at the present day, and with those that characterize the same or adjoining areas when glacial conditions prevailed in that same area.

We have to consider the characteristics of a true, raised beach, and also whether all forest-beds over which the sea flows may be taken as evidence of submergence, or, if not, how we may distinguish the different kinds.

What it seems to me I am asked to do, is, therefore, to inquire into the earth-movements which have taken place in

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assume, that the gravels which have been found to hold human implements are exclusively *river-gravels*.

I entertain great doubt on this point. The distribution of our superficial gravels seems to me to indicate that some of them do not belong to any river system, but that they have been spread over hill and valley by marine action. If human implements have been found in gravels of marine origin, an entirely new element is introduced into the question.

My own belief is, that a submergence under the sea to the extent of upwards of 2,000 feet has been one of the very latest of geological changes. During part of this submergence, glacial condition prevailed over a large part of what is now Europe.

My further impression is, that man appeared on the scene when the land was emerging, and that the elevation was comparatively rapid. During this period it is most probable that heavy rains prevailed, and if so, the double action of elevation and of continual floods would greatly shorten the time required for the cutting out of the beds of streams or the deepening of valleys.

The Palæolithic weapons indicate a people somewhat in the condition of the Eskimo, and they may have been the outliers of races in a very different condition, who lived in non-Glacial climates to the south.

I wish the attention of geologists were more directed to the question connected with the admitted fact of sea-gravels at a high elevation on our Welsh and Scottish mountains."

Glacial and post-Glacial times, with special reference to the deposits in which the remains of man have been found.

Most men, who have been looking into these questions, have their own view of the sequence of events which have affected the physical geography of the country from Glacial times to the present day; but they should try not to let a theory colour their view of everything they examine. I will give you an opportunity of detecting and eliminating the personal error from my communication this evening by giving you, at the outset, a short sketch of what are the conclusions I have arrived at with regard to the later changes in or immediately affecting our country.

Out of the great mass of material collected, I have to select only a small number of cases; first of all choosing those which bear most clearly on the questions which interest this Society, and, secondly, out of them, selecting those in which the evidence is most clear. In such cases as that before us we must not expect to explain all the phenomena which Nature has left half-exposed to our gaze. We may be well content if we get clear evidence from a few, and in the others do not see anything contradictory to the obvious conclusions arrived at from them.

First, then, I will premise that I think it far more likely that secularly recurring cosmical combinations may determine the time and manner of earth-movements, than that, by any direct effect upon the amount of heat and light received from the sun, they have produced those great vicissitudes of climate of which we find evidence.

To be more particular, we find that, as now, in every latitude, glacial conditions always accompany great elevations, and that this confirms the conclusion we should arrive at from the distribution of the drift, &c.,—that during the Glacial Period the mountains of Wales and Scotland were very much higher, and that when, at the close of the Glacial Period, there are proofs of submergence, there we have evidence also of an amelioration of climate.

But we do not gather from the calculations of physicists that the difference of temperature due to astronomical combinations could, under any circumstances, be so great as that which we see along the same parallel of latitude at the present time, and which must obviously be due to geographical causes. While geology does not point to any regular decrease of temperature, such as might be suggested by the knowledge of the secular cooling down of the globe, but rather shows recurring higher and lower temperature in the same area, we must bear in mind that the parts that were raised the

highest have to be depressed the lowest to resume their original relative position, and that there is plenty of evidence of successive movements of elevation along the same axes,—along the Pyrenees, for instance, and along the area we have now to do with, viz., Wales and North-Western England; so we need not at all assume a uniform elevation or depression over the whole of Britain at the same time.

We start then with this. There was a time when the mountains which run down through Scandinavia, Scotland, the north of England, and through Wales, were so high that icefields and glaciers prevailed along the whole range. Now we must refer to a few figures. If an elevation of a little over 2,000 feet extended over an area so wide as to take in some of the borders of the Atlantic to 300 or 400 miles beyond the coast of Ireland, where the sea bottom runs down from 2,130 to 10,700 feet in about 15 sea-miles, and it were all uplifted, we should have land all over from the Continent 400 miles out into the Atlantic beyond the coast of Ireland. The mountain ranges would be so much further from the sea, and the climate be less *insular* and less equable. The deep valley running down where now we have the Baltic Sea would bring glacier ice from Scandinavian fields; and from the Norway coast, probably, other ice would creep south. But we need not assume a uniform elevation. Probably, the amount of elevation was highest in the mountains, and at first, when Scandinavian ice predominated, it overrode the Scotch mountains. Scotch glacialists tell us they have evidence of this. Precipitation being equal, the more northern colder regions generated more ice, even if the mountains were not higher. So, from the mountains of Scotland, ice pressed the Lake District, from which, again, the ice held back the glaciers from the North Welsh heights, and turned them to the East.

But when submergence followed, the Scandinavian ice stopped short of Scotland; and so in turn each more southern group of hills was freed to distribute its own ice all round as the ground fell easiest for it.

This is the commencement of the period with which for our inquiry we have most to do. What kind of country was here before the Glacial Period we have not much to tell. A few marine deposits on our eastern coast tell what the creatures were that swam the seas some time before, but of the plants and animals upon our hills and plains we know but little.

After the period of extreme glacial conditions the land went down, and warm currents from the south and west approached the hills. We cannot suppose that all the forms of northern life were driven back at once, but by degrees they all retreated

to the north, and southern forms advanced. So, when we try to correlate glacial beds in England with those elsewhere, we must not too hastily assume that the regions characterized by certain forms were defined by the same boundaries as now hold good.

The downward movement carried down the land to far below the level at which it stands at present, and since that it has been coming up again with oscillations. Now, we must inquire in greater detail into the evidence.

What was the greatest height to which it rose in Glacial times we have not evidence to prove. But we can show a depth to which at least it must have sunk after the Glacial times.

On the western spurs of Snowdon, in a trough between Moel Tryfaen and the hills, there is a bed of sand and loam in which are whole and broken shells, most of them species found upon the coast to-day. With them are stones such as are found in the drift about, not much water-worn; some even retaining the glacial scratches; and, perhaps, most interesting of all, there are not uncommonly flints such as occur in gravel-beds in east and southern England,—sub-angular, ferruginous, some more, and some less rolled.

We will make these beds a central point around which we will collect our evidence. They have been noticed by Trimmer,* Darbishire,† Lyell,‡ Ramsay,§ and many others. They are well-known, and all the leading facts are well-established. First, as to its height above the sea. The bed in which the shells are found, according to Ramsay, runs up to 1,170 feet,‖ but Ramsay holds that beds precisely similar, and to be bracketed with them, but which have not yet yielded shells, run up to heights of 1,800 feet or more on the same mountain group.

At Macclesfield,¶ near Manchester, another bed like it, and containing mostly the same shells, was found by Prestwich at 1,250 feet above sea level, and near Congleton** Ramsay records another at 600 feet. From the time when the mountains of that district were so high that glacier ice crept down them, to the time when all the land went down from 1,200 to 1,400 feet below its present level, must surely have been an

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* *Proc. Geol. Soc.*, vol. i., p. 331.

† *Proc. Manchester Lit. Phil. Soc.*, 1863-4, p. 177.

‡ *Ant. Man*, p. 315.

§ *Phys. Geol., &c.*, p. 413.

‖ Darbishire estimated the highest point from which he obtained shells at 1,370 feet.

¶ Darbishire, *Geol. Mag.*, vol. ii., p. 192.

** Ramsay, *Phys. Geol., &c.*, p. 413.

interval sufficiently long, and the geographical and climatal changes involved must have been sufficiently great to have allowed the arctic forms to migrate north, and southern life to take their place; but what the shore in earlier Glacial times contained we cannot tell. It has not risen again, and lies still deep below the sea.

In these Moel Tryfaen beds some fifty-four species have been found; of these, thirty-seven are still living in the neighbouring sea.* Now, we do not get gravel and sand continuous from this height down to the level of the shore. But on the northern shores of Cardigan Bay, in promontory of Lleyn and Anglesea, Ramsay records them. Near Macclesfield nearly the same group of shells occurs.† A happy combination of conditions preserved these two records of the great submergence. Between these two points there is no evidence of any similar beds so high; but at a lower level, gravel, sand, and loam occur at various places along the North Wales coast, and with numerous shells up to between 200 and 300 feet. These seem, from the cumulative evidence to be given hereafter, to belong to a later period of the same great submergence, and as the land had not risen high enough to reintroduce glaciers upon the heights, so there was no recurrence of colder forms along the shore; but the migration of southern forms towards the north had still gone on, and so we find but two or three arctic types among the shells.

For instance, in the Vale of Clwyd, at various points we find a mass of sand and gravel associated with red clays with boulders, some round, some broken, some showing glacial striæ, and some not. The shells occur both in the sand and in the clay—more commonly in the sand. The sand and gravel are oftener in the middle, with clay above and below; but sometimes there is hardly any clay, sometimes no sand.

The shells are almost always fragmentary, and all but two are common on the coast but six miles to the north.

They are :—‡

Dentalium abyssorum or tarentinum.
Littorina littorea.

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* More recently, Mr. Gwyn Jeffreys has given a list of the Moel Tryfaen fossils, corrected up to the latest date (*Quart. Journ. Geol. Soc.*, June 9, 1880). He says that there are sixty species, besides three distinct varieties, of which eleven are arctic, or northern, and the rest still live in Carnarvon Bay.

† Darbishire, *Geol. Mag.*, vol. ii., 1865, p. 293.

‡ Most of them were determined for me by Searles Wood, Jun.

Trophon clathratum (=T. truncatus=Fusus Bamffius).
Pleurotoma (Mangelia) rufa.
P. turricula.
Turritella terebra.
Artemis exoleta.
Astarte borealis.
Cardium edule.
C. echinatum.
Mytilus edulis.
Ostrea edulis, young.
Tellina balthica.

If we follow these beds round the eastern flanks of the Welsh hills to the south, we can trace them now in continuous sheets of sand and gravel, now in detached terraces and patches down to the basin of the Severn. They are probably the beds mentioned by Murchison* and by Trimmer.†

If we examine them, following the excellent work of Mr. Shone,‡ of the Chester Society of Natural Science, as they stretch at lower levels across the estuary of the Dee, and here and there along the western coast of Lancashire, towards the Lake District, we find just the same evidence,—re-sorted drift, with shells, some showing affinity with northern types, but most of them like recent temperate forms. Before we leave the north and western shore of this primæval sea, let us consider what the obvious simple explanation of the whole may be.

When the lofty ice-clad land began to be submerged, the sea lifted and floated off the ice that came down to its level in glaciers, so all the lower lands beyond the mountains were still loaded with iceberg *débris*, which could not have come if one ice-sheet prevailed over all the mountains.

This *débris*, too, was different, according as the ice from Scandinavia, Scotland, or North Wales prevailed. As still the land went down, the climate changed, and the ice receded, no longer reaching the sea level, and so no longer bergs floated from the ice-foot with their load to distant spots. But on the flanks of hills the accumulated *débris* of the earlier times was eaten into by the waves, as on that sinking land successive parts were brought within their action. Then, as now, the shingle travelled round the coast from bay to bay, and any

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* *Sil. Syst.* † *Proc. Geol. Soc.*, vol. ii., p. 200.
‡ *Quart. Journ. Geol. Soc.*, vol. xxxiv., p. 383.

mass of older gravel which fell in left traces all along. So the flints from ancient gravel-beds, much further east, got scattered all round this ancient coast-line.

Now to return to the deposits on the flanks of the North Wales mountains, we shall see that the above explains their character.

First, the shells found on Mount Tryfaen are of earlier date, and therefore of more arctic type than those that lived along the shore when the emergence, after the great depression which lifted off the glacier ice, had gone on much longer and raised the land 1,000 feet more.

Then as to the flints which I have found all round the coast—in the plateau gravel of St. David's, in the shell-bearing beds of Moel Tryfaen, in the clays and sands of Anglesea, Colwyn, in the Vale of Clwyd, and in the gravels of the Cheshire plains. A submergence of 1,000 feet would leave Wales a small group of islands, and the gravel travelled from the east when, with a depression of a few hundred feet, the sea rolled through the straits between the Lancashire and Cheshire hills, swept along the Malvern ridge, and from the Severn to the Dee. I think it more probable that the flints came with the eastern gravel at this period than that they were carried from the North of Ireland, because they occur only in this later drift.

Here we are coming to another point of great importance in our inquiry. What evidence can we find that any beds upon the lower slopes showed evidence of ancient drift *remanié* by the sea. In the Vale of Clwyd there are beds of clay, gravel, sand with shells, which we have seen are all but two the same which now are found upon the shore some five miles off. In the clay are glaciated stones and rocks which must have come from other areas across the watershed to the west. Now, it is clear that when there was land-ice to carry them so far, conditions must have been unsuitable for such a temperate group in that same place. Again, we find sticking in the clay fragments of rock that have been striated by glacial action, then broken up, and the fragments scattered. The specimens show that the later fractured sides have never suffered glacial action. There are also in the sand and shingle balls of clay with pebbles stuck all over the outside, just such as now occur where cliffs of glacial drift are washed by the sea at Colwyn and elsewhere, and pieces of the clay fall on the shore, are rolled, and have stuck on them the pebbles of the gravel on which they roll. Similar clay-balls may be seen along the coast of Sheppey, covered in like manner with small stones and shells.

So we may have in such an area marine beds partaking of the characters of the oldest drifts, but, when looked into carefully, obviously of later date, and only made up of the *débris* of the older drift.

It is quite possible that the beds described by Mr. Shone, from lower ground near Chester, may be older than those in the Vale of Clwyd, and distributed along the straits before the land rose high enough to form the estuary of the Clwyd, or it may be that northern currents kept a more arctic fauna here and there, or perhaps they were in part derived from older Glacial beds. The reason of the more arctic character of the Chester beds is not quite clear.

Now, if we follow these beds to the east we shall find similar sands and gravels, but perhaps more largely derived in some places from the Trias sandstone, half-across England. On the north the bounding shore is not so clear as round the hills of Wales. The beds of Macclesfield, we have seen, correspond with those of Moel Tryfaen, and the shells are much the same.

Still more to the east and a little further north we find the marine sands of Kelsea Hill containing a not very arctic type of shells, while in the old boulder clay of Dimlington Hill on the coast north of Holderness, in company with Mr. Leonard Lyell, I found a small lenticular mass of sand full of shells such as occur in what used to be called the Bridlington Crag. There were among them Nucula Cobboldiæ and Astarte compressa, with the two valves united, and other decidedly arctic forms. These shells are now in Mr Lyell's collection who gives the following list:—

Saxicava rugosa.
Astarte borealis.
A. compressa (young, both valves united).
Nucula Cobboldiæ.
Turritella (from clay).
Cyprina Islandica.
Tellina balthica.
Mya.
Fusus striatus (from clay).

Here I take it we have, far away from the high mountains, evidence in the one deposit of the time when the sea was chilled and arctic life prevailed, in the other deposit record of the time when changes of level had let in warmer currents, and the temperate forms of life.

These movements extended north through Scotland, where traces of shell-bearing sands are recorded up to 500 feet, and

evidence of much more recent elevation has been seen by some, but this is not quite clear.

To cross to Sweden. The beds of Uddevalla* show an upheaval to 200 feet since most of the shells now living on the coast of Norway had arrived.

And in the recent voyage of Nordenskiold he records in many places, in still more northern latitudes, clear marks of elevation in geologically recent times up to at least 500 feet.†

This is the period that is of most importance in our present inquiry; and we learn, putting the evidence all together, that after Glacial times the land went down along the Cambro-Scandinavian range of mountains, and the encroaching sea used up old Glacial beds; that it took a long time before the arctic forms of life that haunted the shores of the old Glacial land were driven north, and temperate life came in. The mountain range went down far lower than it stands at present, and then a reversed movement commenced, and all the land has been coming up, with interruptions, ever since, and from an unknown depth has been again raised up in places to at least some 1,800 feet. It is probable that since Glacial times the land was much more extensive than it is now, and that this was due to elevation, not of the mountain ranges, but of the seaboard lands. We see this, not so much in the fact of the sunk forests, as in the size and character of the trees in them; for they are of far more luxuriant growth than we now find so near the sea.

Now we are closing round the country of which we most desire to learn the history, namely, that drained by the large rivers along whose banks we have evidence of man's sojourn in palæolithic times. There are a few cases where, it is said, implements of that date have been found along the borders of the Lake District, and where the forms of mammals usually associated in those early times with man have been preserved. But, as a rule, they are absent from the gravels there; and to explain this fact some hold‡ that they were all pre-Glacial, or earlier than the latest glaciers, and that they have all been swept away by ice. I cannot accept this view in its present form. Man may have followed hard upon the receding glaciers, when in the latest period of their existence the climate was so far ameliorated, owing to depression of the mountain ranges,

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* Lyell, *Phil. Trans.*, cxxv., and *Ant. Man*, p. 63.

† The Arctic voyages of Adolf Eric Nordenskiold. Lond. 1879. P. 324.

‡ Tiddeman, *Brit. Assoc. Report.*

that ice no longer came so low, nor covered the whole coast. This would explain the solitary appearances of his remains recorded here and there; but I fail to see the force of the reasoning upon which it is assumed that man and his associates once were there, and their remains have been all swept away by ice.

Travelling now south the estuary of the Wash gives little information on this question. The whole surrounding country is so low that 20 ft. depression would leave but a few gravel-mounds here and there above the water, and the sea is kept back by silting up of channels and artificial banks. However, at March, a town in the Cambridge Fens, some twenty miles from the Wash, a gravel-bed* occurs full of sea-shells of recent temperate facies. At Manea, immediately beyond, at the same level in sand and loam, the Corbicula fluminalis is abundant. This little shell, now found no nearer than the Nile, is characteristic of the older river-gravels, which were deposited we know after Glacial times, because the older gravels have more, the newer less, of their material derived from glacial drift.† I see no reason why in the March gravels there should not be remains of man washed out to sea from those that lived along the river banks or were lost along the coast. The wonder is we have not found marine deposits of this age with the remains of man. The beds, so far as I know them in Norfolk and in Suffolk, from which palæolithic implements have been procured rest on Middle Glacial, a much older series; but if marine beds derived from these and representing the deposits of the estuary or mouth of the large rivers along which man lived were exposed we might expect to find some traces of him, and it would be difficult to distinguish the newer from the older beds. But we have not in all this area evidence of extensive earth-movements in the existence of marine deposits far above the present sea level.

The only marine quaternary beds in the Hertfordshire‡ district are, I believe, of much earlier date, and do not bear upon the question now before us.

Coming now to the Thames basin, we have again but scanty evidence of upheaval. There is, however, some. In the extensive excavations for brickmaking near Sittingbourne, it was clear that there were at least two divisions of

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* Seeley, *Quart. Journ. Geol. Soc.*, vol. 1866, p. 473.

† Seeley, *Quart. Journ. Geol. Soc.*, vol. xxii., p. 479. Fisher, *Camb. Phil. Soc.*, 1879.

‡ *Quart. Journ. Geol. Soc.*, vol. xxiv., p. 283.

the gravel underlying the brick-earth, in the lower of which remains of oysters occurred. Even this was not a very clear case, but as far as it went it pointed to a slight rise along the lower reaches of the Thames, and to a more rapid denudation of the valley in consequence.

The investigations of Mr. Spurrell in the Crayford brick-pits show that since man lived on the shore of the Thames estuary, beds of sand and gravel, containing remains of the mammoth and tichorhine rhinoceros, of the Corbicula fluminalis and Unio littoralis, have accumulated to a depth of at least 37 feet over the remains of man. If we may assume that these were estuarine deposits, they must have been upheaved some of them more than 50 feet above the level of the river. Having recently examined the ground carefully with Mr. Spurrell, and dug out numbers of the worked flints with my own hands, I am convinced that the evidence is quite satisfactory.

In the Somme valley, the other valley of greatest importance in our inquiry, there is a shell-bearing bed at Menchecourt,* but this is also at quite a low level, and merely points to estuarine conditions running further up the valley, but to no extensive elevations in palæolithic times; and along the valley there appears to be no trace of any higher beds, with marine or estuarine remains, as yet discovered, although the flanks of the valleys have been so extensively cut into for brick-earth and gravel.

We will now see what we can obtain from an examination of the coast lines of the Channel.

Here, of course, we have to deal with raised beaches and submerged forests, and, to begin, I will offer a few words of caution, as it is not every deposit containing shells lying above the highest tide that can fairly be considered a raised beach; nor is it every old forest over which the sea flows at every tide that can be truly called a submerged forest.

It is well known that, in a bay, especially where the sea rolls in over a long-shelving shore, the waves run up far on to the beach, carrying shells and stones above the line to which the water could raise them against a wall or cliff. So when, from the destruction of a headland, or other local change, the sea cuts down such a shelving shore, and leaves a cliff, the base of which it scours, it would appear, at first glance, that we had there, in the highest portion of the old sand margin, a raised beach, and it would be received as evidence of an elevation of the coast.

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* Prestwich, *Trans. Soc. Ant., Lond.*

A few solitary shells in sand and stones are not enough. It is wonderful how the wind can carry, especially with the broken water of a storm-driven wave, shells and stones far on to the land. We must be careful not to be taken in by blown sand, even when there are here and there small layers of shells in it. I have myself seen a Nassa travelling up the slope of a sand-dune under the action of the wind alone. When such shells drop over into the trough beyond, and get buried up, they might well be taken by some to indicate that the whole shore has been so far raised.

We should examine carefully whether the deposit is undoubtedly marine, the shells, stones, sand, &c., showing clear evidence of having been sorted by water; and, secondly, whether it is quite clear that, under no conditions of long shelving shore, they can have been carried thither with the relative height of sea and land in other respects unchanged.

Yet there are raised beaches, and, as they are conspicuous features in a coast section, there are plenty of descriptions of them. Sedgwick and Murchison described a raised beach in Barnstaple or Bideford Bay (*Trans. Geol. Soc.*, series 2, vol. v., p. 279). De la Beche, in the *Geological Observer*, describes and gives sketches of some raised beaches (p. 452 *et seq.*), and gives some useful cautions at p. 261. In the Report on the Geology of Devon and Cornwall he mentions more. Godwin Austen, Pengelly, Carne, have, too, described various others, and a useful summary is given by Mr. Ussher in the *Geol. Mag.*, 1879.

So for the submerged forests, there are sources of error often overlooked. If a low estuary or seaboard marsh gets silted up, as, for instance, the Wash, the mouth of the Somme, or the marsh behind the shingle bank at Westward Ho, and then along the seaward side sand-dunes are blown, as at the mouth of the Somme, or shingle drifts along the coast, and forms a bank, as at Westward Ho, then the high tides are checked, and peat accumulates, and trees grow in the shelter behind the bank. But in a changing, sea-washed coast, these banks of sand and shingle are sometimes swept away, and the sea rushes with every tide across the forest land; soon the trees perish at their roots, break off, and perhaps are floated away or buried up in mud and sand. The water, too, running with every ebb into and through the porous soil, carries off some of the underlying silt, and so, sometimes, the whole is lowered gently towards the sea.

Again, we must be careful that we have not got only the waterlogged wood and drifted vegetation sunk in the estuary

or in the sea—such as is found in masses at the mouth of the Mississippi and other large rivers. Indeed, it seems hardly safe to infer submergence on the simple evidence of the bed itself, unless you find the stools of the trees *in situ*, with their roots penetrating the underlying soil, and also find the bed passing beneath low-water mark.

De la Beche, Henwood, and others have described the submerged forests along the southern coasts. In the *Quart. Journ. Geol. Soc.** is a paper by Mr. Smith, of Jordan-hill, "On recent depressions in the land," in which he gives many legends and traditions probably founded on the obvious marks of changes of level along the coasts of the channel. He also records, on the authority of Capt. White, R.N., who, under the direction of the Admiralty, surveyed parts of the coast, that there were stumps of trees *in situ* not less than 60 feet below high-water in the Bay of Cancale, where the tide rises and falls about 50 feet.

There are many sources of error when we regard the evidence of submergence or of elevation, but when we have got rid of these there still remain plenty of well-authenticated cases of raised beaches and submerged forests to show that movements always have been going on, now up, now down, and therefore we must allow for the acceleration or retardation in the rate of waste in all the valleys within the area so affected. As we trace these movements north to the borders of the mountains, we find evidence of greater sinking and greater elevation, perhaps because we have there the mountains as our gauge on which are marked the various depths by nature in terraces and banks of shells, but more as I believe because along the mountain chains the movements were always greater. This point is clear, that after Glacial times the land went down in places, probably at least 2,000 feet below the sea, and then the ice was lifted and melted off. After that the land began to rise, and by this time the sea was warmer and the forms of life less arctic, as seen at Moel Tryfaen, Macclesfield, and later on the Vale of Clwyd and Kelsea Hill. And now destruction of all softer beds went on, whether by the sea eating along the coast, or streams tearing the mountain-sides and flanks of hills, or larger rivers undermining as they flowed along banks of old glacial drift. In all the earlier deposits resulting from these agencies we see the great preponderance of glacial drift used up by denudation, as compared with the proportion found in the later beds, when the covering of drift

* Vol. iii. (1847), p. 234.

had been in a great measure removed. Then man advanced as the land rose, now bared of ice, and lived along the rivers that drained that land, and with him the large mammals associated with him in palæolithic times. Earth-movements still went on, and probably still are going on, checking the rate of waste in one valley and hurrying it in another, adding another element of uncertainty in all our calculations of the date of man's appearance.

The CHAIRMAN (J. E. Howard, Esq., F.R.S.).—I may tender the thanks of all to Professor Hughes for his very able and interesting paper. Although no geologist, it has been necessary for me to acquaint myself, in a measure, with some geological facts, having been for nineteen years chairman of a slate-quarry in North Wales. I am, therefore, able to confirm what Professor Hughes has stated with regard to the features of the country, and I believe, as he has been telling us, that there has been an amazing variation in the level of that part of the country, with a depression perhaps of 1,500 feet, nd subsequent elevation, whilst the amount of disturbance was much less on this side of the island. The question of alteration of temperature is one that it is clear we have not yet got to the bottom of.] I think I am justified in saying we have proofs of a climate in Greenland which allowed a growth of vegetation similar to that of the Southern States of North America, such as magnolias, &c., that would involve perhaps an average of twenty degrees higher temperature than exists now in Greenland. I do not believe that any alteration which we can conceive in the elevation of the mountains in this part of the world, would cause such a difference of temperature. Moreover, I have had in my hands a section for the microscope, of a small tree from the extreme north, which seems to imply a totally different climate to that which now prevails. The circumstance of any possible change of the earth's axis, will not allow, as Professor Hughes has said, of any such alteration of climate as we are considering for Greenland. The question is, under what conditions have these changes arisen? Of course we have yet very much to learn. I now ask those more acquainted with the subject than I am, to give us the benefit of their knowledge. So far as mine extends, it is confirmatory of what Professor Hughes has been telling us, with this exception, that I do not think the facts bear out the inferences as regards the alteration in the climate.

Captain F. PETRIE.—Before the discussion commences I have to read the following letter from Professor W. Boyd Dawkins, F.R.S. :—

"The Owens College, Manchester, March 12th, 1880.

DEAR SIR,—I regret that I am unable to come to your meeting next week to support my friend Professor Hughes, on whose essay I have no criticism to offer, as I agree with the whole of it.—I am, dear Sir, yours truly, W. BOYD DAWKINS."

Rev. W. B. Galloway.—I have long been interested in the progress of geology, and have watched its many changes, and the theories which have been raised and rejected. Facts have been collected under erroneous theories, but even an erroneous theory has the advantage of exciting the interest of those who favour it, and in educing extra facts, all of which go eventually to the discovery of the truth. Those facts which Professor Hughes has so interestingly and clearly placed before us this evening, we must all accept. There is no difference of opinion as to the facts, but in regard to the cause which has influenced the deposits of these various substances, there may be very different conjectures. The Glacial theory was not the theory when I first became acquainted with geology, but the Diluvial theory, which was maintained by Buckland. It appears to me that the Glacial theory has been put instead of that Diluvial theory, and that it has been put in place of it with very great disadvantage. Taking the fact of an universal deluge, many of those things which have been so clearly described may be accounted for by diluvial deposits. There arose the question how so great a quantity of water as to submerge all the earth existed; but that has been answered by Lyell and others, who found that the average depth of the sea is about fifteen times the height of the land. Nothing, therefore, of a difficulty exists as to the quantity of water; but as to the manner of submersion some difficulty might arise. It is admitted that there is a sunken continent in the Southern Ocean, and the fact of its sinking may have influenced the change of the earth's axis; and may not the description given by the late Polar expedition of the Palæocrystic ocean suggest some thoughts to us of the effects of a sudden change of axis? Now, if a change of axis took place on this earth—and I think strong evidence can be brought forward of such a change from the shifting of the magnetic pole—an idea was once started that there is a nucleus of the earth revolving differently from the earth itself, over which the changing body of the earth may slip,—and if that change of axis were sudden, inevitably the waters of the sea must have been thrown over the land in a very violent manner. Near the North Pole the fields of ice must have been thrown in a southerly direction—towards the south-east and south-west—drifting over the continents, and carrying portions of rock—huge boulders—which might be deposited here and there. That those boulders have been deposited chiefly in the northern regions, that they do not extend to the tropical climates, favours the idea that they were carried by icebergs from the north. There is this evidence of the change of axis in the geological facts taught by Lyell, that the deposits of coral in the neighbourhood of Vienna, and in parts of the north of Italy, give evidence of a similar temperature having existed there and in Jamaica, the same kind of substances being found in both. There is now a great difference of latitude between these places—about thirty-one degrees; and there is evidence that at one time there was no such difference. There is also this fact, an astronomical fact. If we take into account the idea of the best astronomers—I believe of all of them—that the secondary planets were

originally part of the diffused substance of the planet itself, and revolved with it, it is probable that the plane of the revolution of the moon would correspond nearly with the plane of the earth's equator. But the plane of the moon's orbit is at an angle of about five degrees and nine minutes to the plane of the earth's orbit round the sun, while the plane of the earth's equator is at an angle of about twenty-three degrees and a half. It was formerly more—it will by-and-by be less. This shows a change of the inclination of the earth's equator, and consequently of what originally was the axis of the earth, to the extent of the difference between these two. That difference will be eighteen and a half degrees, and that difference is also about the distance between the magnetic pole and the geographical pole. Thus, if the magnetic pole at one time coincided with the geographical pole, their present amount of divergence points towards the same conclusion, that there has been a change of axis to that extent, and so corroborates the fact of a universal deluge, which would necessarily follow such a change, if sudden or very rapid. Then, in regard to the flint formation, I think I can produce specimens which seem to suggest a meteoric origin. I have specimens which contain vegetable roots, &c., which suggest that the flints are as much of terrestrial origin as a sea formation.

Mr. D. Howard.—There is one question I should like to ask, and that is with regard to the changes of level in the Atlantic, whether they have been specially studied, and whether they are comparatively local, or if the whole country moved together, because it would very much depend upon this as to how far we should expect to find that the whole of England sank together and rose together? It might be possible that Wales might be submerged, and France not. Otherwise it seems difficult to understand how we entirely miss the marine gravels on the French coast.

Mr. T. K. Callard, F.G.S.—We are greatly indebted to Professor Hughes for the admirable paper we have before us to-night, and for the large amount of information given to us by one who has a right to speak with authority upon these questions. We are also indebted to his Grace the Duke of Argyll for asking the questions which have given rise to this paper. One of the questions has been answered very satisfactorily. I think we have had clear proof of the depression and elevation of the land. We have also had proof that these depressions and elevations have taken place in recent geological times. But there is one other question put by his Grace which, if we had had time, I should have liked to have heard answered, viz., the origin of the gravels containing the implements of man. It is evident that the Duke of Argyll is not quite satisfied that these gravels are river deposits. Professor Hughes speaks of "the inferences to be drawn from the extent and relative age of the earth-movements, and their bearing upon the origin and age of the river-deposits from which the remains of man have been procured." It is possible that these valleys,—the valley of the Thames and the valley of the Somme,—may have been formed before the land went down. If so, when the land was re-elevated, instead of a great length of time being required for the erosion of the valleys, they would

have been cleared of the detritus that filled them in a comparatively limited period. Had such been the case, it would have destroyed the argument for the antiquity of man. I should be glad to hear from Professor Hughes whether, in his opinion, the land went down to a depth sufficient to allow of the water covering those spots where the implement-bearing gravels are now found. If it did not, then the falling and rising of the land, to my mind, gives no clue to the age of the river deposits. But are these gravels really river deposits? This is the point which the Duke of Argyll is inclined to dispute. There are two or three reasons why I doubt these gravels being river deposits, and one is the height at which they are found. They are not merely in patches on the slopes of the valley, but are spread on the highest ground of the Somme Valley,—a condition of things which, it appears to me, difficult to understand; for I cannot see how the river Somme could have deposited these gravels at such heights. Another question arises, how came the flints shattered? the slow movement of the water from the very small fall it had at that time, not amounting to so much as 2 feet in a mile, could not have done it. I should not have expected to find flints in this condition [producing them], simply from gradual river erosion.

Mr. S. R. Pattison, F.G.S.—I think the paper read by Professor Hughes has been most exhaustive of the whole subject. With the motives of true science he has abstained from drawing deductions which the facts did not allow of finding; and I suppose that although we should all have been very glad indeed to have had the correlation of the river-gravels with any of those gravels which have been mentioned to-night, yet he has resolved that question by reference to what he supposes to have happened during the retreat of the glaciers. Beyond this I think he has not led us, nor did his paper profess to do so. I suppose that enjoyment is to be deferred to some future time. We have been many years pulling down old theories and turning opinion to the Glacial period, and probably at some more modern time we may be pulling down the Glacial theory. But I should be very sorry to attempt to lay any crude speculations before the Society, especially in the face of a paper so full of facts as that which we have heard to-night.

Professor Hughes.—I think I must pass by some of the questions that have been raised, as they wander a little beyond the subject which we have been discussing. With regard to the age of the Thames deposits as compared with those of Moel Tryfaen, the point which I wish clearly to bring out is this: that after the close of the cold period the ice was lifted off by submergence of the land, and our story begins when the land came up again. In the earlier deposits, of course, the shells and the various other organisms, show that the arctic forms had not all gone away. In the later deposits we have more and more southern forms. It was in this post-Glacial period that the Thames Valley received the deposits which we find in it. The included remains show that these deposits are fluviatile or estuarine. With regard to the many remarks and ingenious theories which have been brought forward by the second speaker, it is perfectly true that a deluge is not impossible; but the question as to whether on examining the surface of the earth you have

reason to believe that it was universal or local, is another thing; and in the periods with which we are now dealing there is not a sweeping away of all the animal life which lived there at any one time. It is better to receive a simple explanation than to try and explain obscure phenomena by reference to violent shiftings of the axis of the earth. The persistence in the forms of life would offer a difficulty in accepting such a theory. The alterations in the magnetic pole is another thing. Those changes are too rapid to depend upon the great changes of the axis of the earth. With regard to the question of the solid nucleus that has been referred to, the friction between the solid nucleus and the crust would be too great to allow us to entertain any such opinion as that at all. It was brought forward some years ago, and it was shown by mathematicians at the time that it was an impossibility, and it would not help us in the present case, because it cannot be shown that the changes coincide with those climatal variations we have to explain. The simple explanation of wood being found in flint is, that there were plants on adjoining land, and fragments were washed into the cretaceous sea, and the part of the chalk in which they were embedded was replaced by flint. With regard to the palæocrystic ocean, the manner in which the ridges are formed is this: when you have a large ice sheet formed on the water, and that sheet contracts by the reduction of temperature, you have to allow a mètre in every thousand for contraction, which amounts to a considerable quantity when you are dealing with the great fields of ice in the Northern seas. After this chasm has been formed the water between the walls of ice freezes, and then comes another change of temperature; expansion thrusts the ice walls together, and squeezes the newly-formed ice out. This, happening year after year, causes those great hummocky ridges which have been spoken of.* There is no doubt about elevation often being local, because many elevations which we have been able to observe are local. In New Zealand there was a clean cut, which could be traced right across the country. In South America it has also occurred. With regard to the question raised by the Duke of Argyll as to whether those gravels are exclusively of river origin, I can only say that the gravels I referred to are known by their contents to be of river origin, If his Grace had mentioned any particular group of implement-bearing gravels which he thought were not of river origin, then I might have discussed the question. I fully allow the probability of the existence of contemporaneous marine beds containing human remains, but in this paper have dwelt chiefly on the bearing of the admitted fact of sea-gravels at high elevations on the question of the antiquity of man. With regard to the shattered flints, almost all flints of this kind (referring to specimen exhibited) are shattered by surface action,—the action of changes of temperature due to frost and sun.

The meeting then adjourned.

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* Nordenskiold, op. cit.