

ground for a discussion which I cannot consider it profitable to continue.

WILLIAM B. CARPENTER

The Glacial Geology of Orkney and Shetland

OWING to an accident I did not see your number of September 13 containing my letter on the glacial geology of Orkney and Shetland and Prof. Geikie's article (vol. xvi. p. 414), until my return from Scotland a few days ago. Otherwise I should have troubled you sooner with a few observations thereon.

In the first place I wish to thank Prof. Geikie for the very courteous manner in which he has referred to the remarks of an outsider who has ventured to intrude on what the Professor has made, to such an extent, his own peculiar province.

In the next place I am glad to find that upon what was the most important fact in my statement, viz., the absence of raised beaches or other signs of recent elevation of the land in Orkney, Prof. Geikie agrees with me.

I call this the most important because it bears directly on the theory of wide-spread changes in the relative level of sea and land owing to secular causes, such as a change in the axis of the earth's rotation, or in the position of its centre of gravity. If it can be proved that the difference of level, which caused the raised beaches of the south of Scotland, and extended north along the coast of Ross and Sutherland, dies out as we proceed further north, and disappears altogether in Orkney and Shetland, it is truly a crucial experiment which shows that these raised beaches are due to local elevations of the land, and not to a general sinking of the sea.

This is the conclusion to which Prof. Geikie points, though he naturally finds it difficult to understand why the upheaval, so marked in Sutherland, did not affect Caithness and Orkney.

I believe I can add a few facts which may assist in removing these doubts.

At one of the places in Caithness mentioned by Prof. Geikie, where the existence of a raised beach might be possible, viz., in the sheltered Bay, between Freswick and Wick, I believe there is one, though less strongly marked and at a lower elevation than those in similar situations in Sutherland. I allude to a terrace which bounds the links of Keiss Bay, about half a mile inland from the present coast-line. I cannot speak positively, not having seen it for some years; but my recollection is that it is a perfect miniature reproduction of the terraces round Brora and other bays in Sutherland. If so, it is a positive proof that the elevation of the land died out towards the north, and we might reasonably suppose that somewhere about the line of the Pentland Firth was the neutral axis, on one side of which the land rose, while on the other it fell.

Be this as it may, the fact is, I think, incontrovertible that Orkney did not share in the southern movement of elevation. This rests not only on the absence of raised beaches, forming terraces, which might possibly have disappeared, but still more on the absence of all traces of marine action, such as pebbles, sand, or shells, on the low plains which must have been submerged.

I would ask Prof. Geikie to consider whether the single instance of the Loch of Stennis is not conclusive. If the sea had ever stood twenty or thirty feet higher relatively to the land than it now does, the whole plain up to the hills must have been a sheltered, shallow, inland fiord.

As the land rose to its present level this must have left not only a terraced beach at the foot of the hills, which might possibly have disappeared (though it is hard to see why it should have done so in such a sheltered situation), but the whole plain must have been a raised sea-bottom, strewed over with pebbles, sand, and shells. These could not have disappeared, and as they are nowhere visible and the plain consists everywhere of the ordinary rock, with a thin mantle of soil resulting from its disintegration by ordinary atmospheric causes, I am, I think, justified in assuming it to be proved that Orkney did not share in the recent movement of elevation which affected the rest of Scotland.

Now one word as to glaciation. I can assure Prof. Geikie that I do not think for a moment of setting my authority against his, and that if he is right in the instances of glaciation he tells us he has observed in Orkney, so far from being disappointed, I shall be pleased, for it will clear up what has long seemed to me a perplexing anomaly.

Of course Orkney must have experienced the full rigour of the glacial period, and it is only natural to expect that it should show the same abundant signs of glaciation as the adjoining counties of Scotland. Prof. Geikie will therefore excuse me if

I still retain a little of that healthy scepticism which is so conducive to the establishment of truth, and venture to plead that judgment may be suspended until there is further evidence. I do so mainly because the Professor's own statement is that during his visits to Orkney his attention was devoted mainly to the old red sandstone, and his remarks on glaciation were only incidental. Now there are some proofs of glaciation which are so obvious that there can be no mistake about them, others which may easily be mistaken, and which require close examination by a practised eye directed specially to them, to arrive at a just conclusion.

Boulders of foreign rock, perched blocks, rocks unmistakably rounded and polished by the ice plane, are among the former. But striæ require great practice and careful examination to be sure of them in a district of finely laminated sandstones which weather constantly into parallel lines or grooves. A tony clay again, from disintegrated rock, is often so like boulder clay that it requires close observation to distinguish one from the other. And finally where steep hills have crumbled away and filled up many places in the narrow valleys between them with their debris, as at Hoy, the appearances are very like those of glacial moraines.

Now I observe that nearly all the conclusive proofs of glacial action are wanting in Prof. Geikie's enumeration. He has not seen, or heard of anyone who has seen, a single boulder or perched block, or even a single piece of foreign stone in Orkney.

As regards boulder-clay I would join issue on his instances, taking especially that of Kirkwall Bay, because it is typical of the other cases and so easily accessible that the facts can readily be verified.

I believe it to be disintegrated and not boulder clay, for the following reasons:—

1. The clay is not compact like that of genuine boulder-clay, but of looser structure, and often clearly made up of minute splinters of the disintegrated rock.

2. The stones in the clay are never foreign stones, and are not scattered irregularly, as if shot out into a huge rubbish heap, as in true boulder-clay, but arranged for the most part so that the original lines of stratification can be followed.

3. If the section which resembles boulder-clay be followed up, it will be found to merge insensibly in what is unmistakably the common disintegrated surface soil of the district.

There only remains the question of *roches moutonnées*, and here I speak with the greatest diffidence, for certainly Prof. Geikie ought to know a great deal better than I whether a hummock of rock is or is not "admirably ice-worn and striated" like those behind Stromness.

I can only say that I have looked at them often, and they appear to me to be very different from the *roches moutonnées* of which I have seen so many in Scotland, Wales, and Switzerland. They are not rounded, smooth, and polished, as if planned into shape by some gigantic tool, but simply irregular hummocks of rock, sometimes smooth and sometime rough, according to accidents in the bedding and weathering of the strata. So at least they seem to me, and even in the valleys of Hoy, where, if anywhere, there were local glaciers, the sections shown by the small streams and low coast-line, always, I believe, exhibit the same appearance of sandstone strata, coming at an angle to the surface, and with their edges not planed off, but passing gradually into surface soil by disintegration.

Of course I make these statements subject to correction. It may be that I have failed to see things because my eye is not sufficiently educated. But when we couple what is, I believe, absolutely certain, viz., the absence of the more prominent and obvious proofs of glaciation in the form of boulders and foreign rocks, with the equally certain fact that Orkney was an exception to the general rule of recent elevation, I think Prof. Geikie will admit that the interests of science will be promoted by any remarks which may lead to reasonable doubts, and therefore to conclusive investigation, as to the fact whether Orkney does or does not give proof of having been covered by a great polar ice-sheet during the glacial period.

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Explosions

I HAVE been waiting to see if Mr. Galloway's paper on "Explosions in Mines" published in NATURE, vol. xvii. p. 21, would lead to any correspondence. Your readers may be interested in an incident reported to me by the late Dr. Böttinger, of Messrs. Allsopp's brewery, Burton-on-Trent,

In their new brewery, near the railway station, the crushed malt is lifted from one floor to another by a series of cups revolving on a leathern band. The casing, which incloses the band, is full of floating malt dust while the revolution is going on, and on opening one of the doors of the casing a puff of malt-dust is sent out into the room. Soon after the brewery was opened, a workman went with an undefended light to make some examination of the working of the leathern band, and on opening the door of the casing an explosion followed; not of a very serious character, but enough, I think, to throw the band out of gear. The cause of the explosion is evident; the rapid combustion of the fine malt dust with which the air puffed out into the room was charged.

Dr Böttinger died a few years since, but it would probably not be difficult to get accurate details of the accident from Messrs. Allsopp and Sons.

A. MACKENNAH

Bowdon, December 11

Means of Dispersal

IN his great work, "Insecta Maderensia," Mr. Wollaston remarks upon the great affinity in the coleopterous fauna of Madeira with that of Sicily, and in his "Coleoptera Hispanidum," on the northern character of that of the Cape Verde Isles. Mr. Andrew Murray also found that out of 275 Cape Verde species 91 were common to the Canaries and 81 to the Madeiran group. The last author would seem to rely on the efficacy of now submerged continents as a means of transmission between the two areas.

Towards the end of the fifteenth and commencement of the sixteenth century, the Portuguese carried the sugar-cane from Sicily to Madeira and the Canaries. The means of introduction would probably be the same then as now; the young shoots of cane would be conveyed in boxes or baskets of earth from one locality to the other, as the writer once carried young cane plants from Car Nicobar *via* Rangoon to Penang, and has seen the same arrive in the last locality from the West Indies. There can be little doubt that many of these plants must have been carried from Sicily to the Atlantic Isles before a successful or sufficient introduction was made, and with the earth in which the plants were conveyed, many geodephagous and other coleoptera would find an enforced means of migration. The sugar-cane is also reported as having been introduced into Cyprus from Asia, and transplanted from there to Madeira, thus adding another link to the localities in which these coleopteral affinities have been detected.

It is not proposed that this was the sole, but only a probable means of the transmission of common forms in the coleopteral faunas of these widely-separated districts. The number of causes which have been factors to the same in the past may be in an inverse ratio to our knowledge of them.

W. L. DISTANT

Supplementary Eyebrows

I MET a gentleman a few days ago who has on either side of the forehead a supplementary eyebrow branching off from the superciliary ridge near the supra-orbital notch, and passing obliquely upwards and outwards for about $\frac{1}{2}$ inch across the forehead. Beneath these brows, which contain large and coarse hairs, are lines of soft down-like hair, one on either side occupying the usual position of the eyebrows. Since my attention was drawn to this subject I have noticed that many persons have a short secondary spur of hairs at the points indicated. Artists, I believe, have noticed this deviation from the normal eyebrow-line, as we occasionally observe it in portraits of Puck and other mischievous sprites.

There is a spot about midway between the orbits in animals that I have examined (namely, horses, dogs, and cats), whence the lines of hair-insertion into the skin radiate in various directions. If we consider the secondary eyebrows of man as a reversion to an ancestral type, we must conclude that our hairy progenitors also possessed such a radiating point of hair-insertion upon their foreheads, and that the secondary eyebrows are only remnants of a hairy covering which originally enveloped the whole face.

W. AINSLIE HOLLIS

Brighton

Diffusion or Cohesion Figures in Liquids

WITH reference to the above, allow me to relate some experiments made several years ago, and easily repeated.

1. Take a tall precipitate glass, fill it with water, drop into it a piece of lump or refined sugar and four or five grains of common

salt. Let the vessel remain quiet, so that when the sugar is dissolved there may be different densities in the fluid from top to bottom. Then lightly touch the surface with a piece of lunar caustic (silver nitrate), and observe the figure which results.

2. The experiment may be repeated with sugar, diluted sulphuric acid, and barium chloride, the figures varying with the proportions of the ingredients used.

3. Take a common tumbler glass filled with water, dissolve in it half a tea-spoonful of common salt. Touch the surface of the solution with the point of a pen filled with ordinary black ink, and the characteristic figures are produced.

F. R. S.

Brighton, December 12

Meteor

AT 8h. 13m. ($\pm 2m.$) P.M. on December 9, a brilliant meteor passed from 32 Camelopardalis ($\pm 1^\circ$) through μ Lyræ ($\pm 1^\circ$), and disappeared about 6° beyond; time of passage, 1'6 ($\pm .3$) sec.; mag., 8 (± 2) χ Lyræ; colour, emerald green; track, yellow, visible 1 second; seen from $51^\circ 24' 43''$ N., $2' 13''$ E. This may enable a northern observer to fix the position.

Bromley, Kent

W. M. F. P.

ON THE CAUSATION OF SLEEP

THE last number of *Pflüger's Archiv* (vol. xv., p. 573) contains the following interesting note by Dr. Strümpell:—

"In the autumn of last year there was received into the medical clinic of Leipzig a youth, aged 16, in whom various phenomena of anæsthesia gradually developed themselves to an extent which has very rarely been observed. The skin of the whole surface of the body was completely insensible, and that in respect to every kind of sensation. The most powerful electric current—a burning taper held to the skin—was not able to produce any pain or even a sensation of touch. Almost all the accessible parts of the mucous membrane of the body exhibited the same insensibility to pain. Also all those sensations which are classed together under the name of 'muscular sense,' were entirely absent. The patient, when his eyes were closed, could be carried about round the room, his limbs could be placed in the most inconvenient positions without his being in any way conscious of it. Even the feeling of muscular exhaustion was lost. In addition there came on also a complete loss of taste and smell, amaurosis of the left eye, and deafness of the right ear.

"In short, here was an individual whose only connection with the outer world was limited to two doors of sense—to his one (right) eye, and his one (left) ear. Moreover, both these remaining doors could at any time be easily closed, and in this way it was possible to investigate the consequences of completely isolating the brain from all external stimulation through the senses. I have frequently made the following experiment, and often showed it to others:—If the patient's seeing eye was bandaged and his hearing ear was stopped, after a few (usually from two to three) minutes the expression of surprise and the uneasy movements which at first showed themselves ceased, the respiration became quiet and regular; in fact the patient was *sound asleep*. Here, therefore, the possibility of artificially inducing sleep at any time in a person simply by withholding from the brain all stimulation by means of the senses was realised.

"The awakening of the patient was as interesting as the sending him to sleep. He could be awakened by an auditory stimulation, as, for example, by calling into his hearing ear or by visual stimulation, by allowing the stimulus of light to fall upon his seeing eye; but he could not be woke by any pushing or shaking. If he was left to himself he did eventually wake up of his own accord in course of the day, after the sleep had lasted many hours, the awakening being due, it might be, to intrinsic stimuli started in the brain, or it might be to slight external unavoidable stimuli acting through his still functional sense organs, and making themselves felt in consequence of the sensitiveness of the brain being increased during the repose of the sleep."