

“On the other hand, there may be grounds for endeavouring to establish contemporaneity between the Upper Devonian series of North Devon and the Carboniferous Slates of the South of Ireland, upon the principle of geographical rather than chronological distinction.”

Guided by the opinion of Professor Phillips, we should certainly be warranted in keeping distinct the application of the terms Old Red Sandstone and Devonian, which, when used synonymously, are productive of much confusion. In this case of course the Lynton Sandstone (Foreland Group) of North Devon should be classed as Old Red Sandstone, and excluded from the Devonian system.

MUSEHOLD, NORWICH, 10th August, 1878. HORACE B. WOODWARD.

“COAST ICE ON A RISING AREA.” REPLY TO DR. G. LINNARSSON.

SIR,—Two days ago, on returning from a long geological excursion which I had been making in the interior of Japan, I received several numbers of the GEOLOGICAL MAGAZINE. In the Number for February I read the letter of Dr. G. Linnarsson of Stockholm, criticizing my views of glacial phenomena which appeared in a series of articles in your MAGAZINE, under the title “Across Europe and Asia.” If Dr. Linnarsson had awaited the completion of my article, or if he had only carefully interpreted those portions of it that were in his possession at the time he wrote, I think he would have seen that he was campaigning against an imaginary foe.

He has failed to observe that my travelling notes are only a series of fragmentary jottings collected and subsequently written out under considerable difficulties. Under such circumstances, feeling my fallibility, I held myself open to correction, and it is therefore now with pleasure that I thank Dr. Linnarsson for having incidentally pointed out my *oversight* in ascribing the presence of erratics at higher levels than their parent rock to the action of coast-ice on a *rising* area.

I say *oversight*, advisedly, first because this is a phenomenon which is so universally referred to by all writers on these subjects, and secondly because previously I myself, in the GEOLOGICAL MAGAZINE, Dec. II. Vol. III. Nos. 7, 8, and 9, when writing more generally upon coast-ice, have referred to these appearances as being due (as many before me have suggested) to the action of coast-ice on oscillating or sinking areas. For example, in one place I make the following note: “Other blocks again are shown to have travelled from low plains to the summit of hills, which is explained on the supposition that the land at the time of their deposit was slowly subsiding, and the ice-fields of successive years were raising the blocks higher and higher.”

With regard to the remainder of Dr. Linnarsson’s criticisms, which form the substance of his correspondence, I hardly feel that I can acquiesce in the manner in which he has treated my communication. One of my chief objects, when speaking of the appearances which I saw along the coast of Finland, was to show that it was by no means

necessary for us to imagine that every surface which had been scratched and rounded must have been produced by a continental sheet of ice, and it was only to explain some of these phenomena that I suggested an agency which I believe has been hitherto overlooked, namely, that of coast-ice upon a rising area.

Dr. Linnarsson seems to say that from observations made from railway waggons and steamers I think myself "enabled to refute the views since many years universally held by Scandinavian geologists."

If he had only read my paper attentively, he would have seen that my views respecting the glaciated appearance of low countries like that I saw in Finland are by no means only founded on what I saw from the railway waggons of Scandinavia and the steamers of Finland, but also from observations made upon the shores of some "4000 miles of coast in Labrador and Newfoundland."

To convince Dr. Linnarsson that I am not acting so impulsively as he would give the readers of the GEOLOGICAL MAGAZINE to understand, I may refer him to several other papers which I had previously written upon the same subject (see *GEOL. MAG.*, Decade II. Vol. III. 1876, pp. 303, 345, 408; see also *Quart. Journ. Geol. Soc.* 1877, vol. xxxiii. p. 929). If, even without reading these papers, he had only waited until the completion of my travelling notes, he would have saved himself the necessity of proving the former existence of a glacial climate.

In several portions of my paper I very distinctly incline towards what Dr. Linnarsson would make me appear to be so antagonistic to. Thus, for example, in the GEOLOGICAL MAGAZINE, February, 1878, near the end of my paper, I most clearly state that my arguments antagonistic to the existence of polar ice caps have "only been in regard to the production of certain phenomena." Amongst these apparent glacial phenomena are appearances which may be seen in many portions of the world along coast-lines, islands and low-lying countries, and as illustrations of these I have taken portions of Finland, Labrador, and Newfoundland. Dr. Linnarsson admits the abrading power of coast-ice, and I also think that, in common with other geologists, he will admit that there is such a phenomenon as the slow elevation of the land. If he does this, I think he must then admit the existence of those effects which must result from the combined influence of these two agents. That *all* the so-called glacial phenomena to be seen in Sweden and other countries are by any means to be attributed to this action, I by no means wish to advocate, but at the same time I must confess that I should find it difficult to prove that the scratches and furrows which I have often seen upon a coast-line, and which the inhabitants tell me were produced by the shore ice of last winter, were the consequence of some continental glacier which may have perhaps existed some 10,000 years ago. It would be equally antagonistic to my sense of reason to endeavour to prove a similar origin for the furrows and scratches which in a rising area graduate upwards and backwards from those produced last year at the water's edge.

The chief point which I wished to advocate in my paper was that

the action of coast-ice on a rising area has produced many phenomena which are inexplicable by the action of continental glaciers. On the other hand, Dr. Linnarsson shows that there are phenomena which I did not even refer to in my paper that may have been produced by a continental sheet of ice, but not by coast-ice.

To all this I see no objection, and so long as Dr. Linnarsson only claims a reasonable proportion of the so-called glacial phenomena as the result of the action of his continental sheet, I shall be content to see the leavings sifted and a certain proportion of them set down to the credit of coast-ice acting on a rising area.

So much then for the general argument embraced in Dr. Linnarsson's communication. I will now turn to one or two of his details.

First, I cannot agree with Dr. Linnarsson that the scratches produced by coast-ice are "independent of the slope of the land." In making this statement, it appears to me that he has apparently omitted to notice the most effective of the methods in which coast-ice acts. Looking at a coast-line generally, the slope of the land will be at right angles to its direction, and the scratchings and furrowings due to the action of ice being produced by the driving in or "raftering" of the pack-ice upon the ice-foot or "balacada," the markings which are produced must be parallel to this movement, that is, at right angles to the shore-line. If, on the other hand, the "currents and winds" are very oblique or parallel to the coast-line, the balacada will usually remain unmoved, and will only be chafed as the pack-ice floats along its outer edge, or what the inhabitants of Labrador and Newfoundland call the "drain."

If we consider the way in which scratchings are produced by the dragging and sliding of coast-ice back towards the sea, and say the markings which will remain behind are independent of the slope of the land, we shall next, perhaps, be induced to say that the direction in which rain runs from the roof of a house is also independent of the direction of its greatest slope.

At the commencement of Dr. Linnarsson's letter, when speaking of my arguments, he says, "I think that most of your readers do not need to have the failings of such reasonings pointed out." The reasons here referred to are my arguments in reference to the action of coast-ice. What must the readers of the *GEOLOGICAL MAGAZINE* think of the authoritative statements and arguments of Dr. Linnarsson, when they read at the termination of his letter that "The rocks (on the coasts of Sweden and Finland) are there so hard and compact, and the force of the waves so small, that their action on the rock surface is hardly perceptible. A rock may be exposed there for hundreds of years to the waves without the finest scratches being abraded." Whilst remembering that a rock exposed to the waves must also be exposed to the atmosphere, six lines further on they then read that, "In the open air the scratches usually become obliterated in a few years," etc.?

All this is brought forward, I may mention, in opposition to an opinion I expressed that all glaciated rocks, in order that their scratched surfaces may retain their character, "must always have

remained above sea-level or else have been shielded by some protective covering during both subsidence and elevation," a view from which I do not yet see the slightest reason to waver. If the country is a cold one, such for example as we might imagine during the retreat of a continental covering of ice from the face of Sweden, and depression were taking place, coast-ice, it seems to me, would grind every furrow from the surface of the rocks, as they gradually sank down at any particular time, the abrading action taking place from a level above that of high water to one below low water, and every portion of the surface of the country being many thousands of years in sinking through the abrasion region. On elevation this erasing action would be repeated. If the country were not a cold one, atmospheric agencies, a continual exposure to an artillery of pebbles, the grinding of sand, and like causes, would, according even to the most modest calculations, have sufficient time for the production of a similar result.

JOHN MILNE.

YEDO, June 17, 1878.

THE QUARTZITES OF THE BUNTER CONGLOMERATE.

SIR,—As I happen to be very familiar with the quartzites of the Bunter Conglomerate of the Midland Counties, may I be allowed to question the correctness of one or two statements which have been lately made in the pages of the *GEOLOGICAL MAGAZINE*. 1. I cannot admit that the typical quartzite of this Conglomerate is lithologically identical with that from Budleigh Salterton. 2. I greatly doubt whether the fossiliferous pebbles from the Birmingham Drift, now in Jermyn Street Museum, have been derived from the Bunter Conglomerate. At any rate, the rock, though a quartzite, does not appear to me that of the Bunter pebbles: it more nearly resembles that from the Lickey. 3. I have many times searched for fossils in the pebble beds of Staffordshire, and have only twice found them: these were obscure annelid burrows. Hence I cannot admit that there is any palæontological identity with the Budleigh Salterton rock. From physical considerations it would require very strong evidence to induce us to believe that the Midland Counties pebbles came from S. Devon. I have no doubt Professor Hull is right in assigning to them a northern origin (Permian and Trias of Midland Counties, p. 60). I have myself identified them in more than one place in Scotland. For example, they abound in a conglomeratic red sandstone of Lower Carboniferous age in Arran, mixed, however, with fragments of schist, greywacke, etc. These softer rocks have almost invariably perished on the southward journey—so that it is a case of survival of the most durable.

T. G. BONNEY.

ST. JOHN'S COLLEGE, CAMBRIDGE, Aug. 12.