From the junction of the two rivers eastward, slate is seen below Table-mountain Sandstone; and on the latter is a long stretch of the Dwyka Conglomerate to the coast, greatly disturbed for the most part, and pierced by two dolerite-dykes, between which a patch of Ecca Shales is preserved.

The author concludes that the marble was deposited on the granite, and probably on the Malmesbury Slates near by, before they were disturbed; that it does not extend far under the neighbouring hills; and that some of its local detritus indicates that the rivers ran at higher levels within relatively recent times.

CORRESPONDENCE.

MR. DEELEY AND MR. HARKER: "TWO BIRDS WITH ONE STONE."

Sir,—In my view there is no more profitable way of advancing knowledge than by good-humoured controversy. I only wish my opponents' banter was a little more playful. Mr. Deeley is mistaken in supposing that I, of all people, can object to his attacking old problems. What I called impertinent, was attacking *very old problems* without first learning what other men had had to say to them, coupled with the assumption that the long life's-work of such patient masters of their craft as Studer, and Forbes, and others in the Alps, was going to be all set right by Mr. Deeley's summer jaunt to Mont Blanc.

What they proved and what recent experiments in the laboratory have confirmed is the plastic nature of glacier ice. Mr. James Geikie, who formerly advocated Croll's transcendental theory of ice-motion, has completely abandoned it in his new volume on the Ice Age, in favour of Forbes' view. Mr. Deeley, some time ago, had a private transcendental theory of his own on the subject, which I cannot find that anybody understood, much less adopted. I do not know whether he still holds to it, or is now satisfied by the experiments of McConnel, Kidd, and others, that Forbes was completely right. I take it from some phrases he uses that, like Mr. Geikie, he, too, has surrendered. If he has not, we are beating the air, for I am bound to say I neither understand the physical nor the mechanical basis of his ice theory.

If he now holds, as all the world holds, that ice is a viscous body, then he must also hold that it acts like one, and that when it has a sloping back it will not move at all on account of the shearing resistance of the ice, unless the slope of its back is equal to that of a glacier-bed when motion first ensues in a glacier. Forbes showed that this meant a considerable slope. The question for Mr. Deeley (entirely apart from all geological difficulties) is how to secure and maintain such an ice slope as would carry boulders to Britain from the Christiania Fjord, and then move on till it terminated in a scarped cliff of ice at the 100 fathom line, and this when the upper part of the Dovrefelds was entirely free from ice as it now is from markings. This is one only of a dozen difficulties surrounding an ice hypothesis which has been evolved apparently without any thought of the

critical problems which have to be met at every turn. To those whose geological and mechanical reasoning follows the same groove as my own, it seems impossible, as it seemed impossible on the same grounds to Pettersen, to Bonney, to Matthieu Williams, and to Milne-Holme, whose experience of glacial phenomena, combined with a knowledge of Western Norway and Eastern Britain, entitle them, I think, notwithstanding Mr. Harker's sneer, to the very first rank as authorities on the geological side of this particular issue.

I have argued the case out in detail in my book on the "Glacial Nightmare," at which I sincerely wish Mr. Deeley would look before advancing arguments which have all been answered by anticipation.

Mr. Deeley's reference to the Antarctic ice, which also occupies a considerable space in my book, seems to me entirely beside the question. The Antarctic ice, in so far as we have evidence, is planted on a high plateau of land. When it reaches the sea it does not march on as the Norwegian ice-sheet is supposed to have marched on, athwart the deep Norwegian channel, and then across the North Sea to the 100 fathom line, and there expose a great cliff, but it breaks off into icebergs *in shallow water*, and these icebergs float away. All this is perfectly rational. How it in any way supports the North Sea monster I know not. In the one case the ice behaves like other ice; in the other case it would behave, it seems to me, as no ice ever behaved before or since, except in a geological nightmare.

I will now turn to Mr. Harker. Although he affects to despise the virtue of modesty, he says "he has not written a word on the Scandinavian ice-sheet, and has kept his views on that subject modestly to himself." Is this so? In the Transactions of the Yorkshire Geological Society, where he discussed the question of these boulders at length, he distinctly refers them to Scandinavia, and actually says, "*the movements of the ice*, and the consequent directions of transport, render this conclusion probable." I think, after this, he ought to have called the boulders not "damaging" but "damaged."

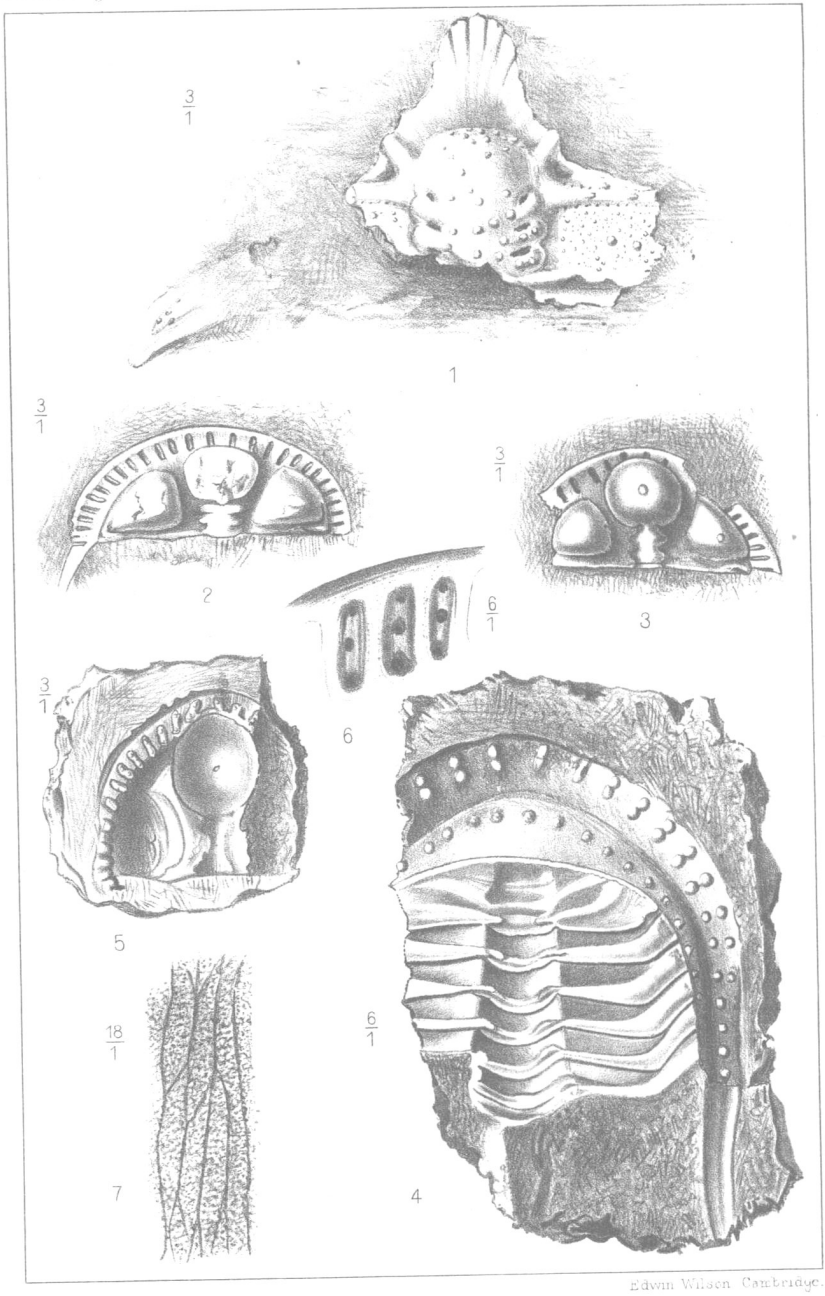
If Mr. Harker no longer believes that ice brought the boulders from Scandinavia, then *cadet quæstio*. To disprove that hypothesis was the purpose of my writing. To be coy in making a confession on such a point after what he has written is to borrow a weapon from another sex than ours. We are not striving for some rhetorical advantage, but for the truth, and the truth is not served by carefully putting under a bushel the light that we may possess, and taking refuge in struthious logic. The question between us is, how are we to account for the Laurvig boulders? I suggested as a possibility that they may have been partially ballast, and partially stones used as anchors, net-weights, etc.

Nothing that Mr. Harker has yet said seems, to me, to have reduced the probability of that suggestion, *and it is only a suggestion.* He asks me whether the Vikings "ballasted their ships with little pieces of rhombenporphyr, and used small pebbles of laurvikite for

anchors." Is this supposed to be argument? If Mr. Harker thinks that the big stones, which were on the beach a thousand years ago, when the Vikings were about, have not been ground into small ones by this time, he is probably singular in his views, but apart from this most ballast is simply gravel. He next alleges the fact that the sea is invading the land on this coast. How the fact that the coast is a retiring coast affects the relative position of the beach as between high-water and low-water I know not. Does Mr. Harker think that when the sea invades the land, to the extent of a hundred yards, say, it leaves its old beach behind. Again, he says, "there is no *port* in Holderness": what has that to do with it? It was probably because there was no port there that the whole fleet of pirate ships which attacked Northumbria in 793 was lost on this very coast, and that many others were similarly lost at other times. Again, he bids me remember that a couple of the boulders in question were found at Cambridge. This I learnt after I wrote my first paper, and I am bound to say that it immediately struck me as a fact not for me to digest, but for the champions of the North Sea ice-sheet to take to heart. Does Mr. Harker postulate a Norwegian ice-sheet in Cambridgeshire? else how does he account for these stones? That Cambridge and all the Fenland was not ravaged in every direction by the Vikings when the country round the Wash was virtually a lake *I do know*, and I need not draw the necessary inference. The sporadic character of the finds is surely a lesson in itself to be coupled with the admission made by Mr. Harker, that the stones about which we are discussing have not been found inland in Yorkshire, and only on the shore. If an ice-sheet or icebergs brought them, and others like them, to Cromer and Cambridge, how is this to be explained? Again, I would seriously ask if any human being who has seen ice at work, either in glaciers or icebergs, ever saw anything less like ice-moved stones than these rounded water-worn boulders? Ice carries a considerable part of its great stones intact and unweathered on its back from end to end without rolling or rubbing them, and then deposits them with the boulders made by its own streams in moraines or in detached blocks at its terminus and sides, and does not select one or two special points easily accessible to boats and ships, *and there only* leave a few choice specimens *of water-worn* stones mixed with a vastly greater proportion of stones which have confessedly come from the opposite direction!

I must repeat, in conclusion, that the question is too important to be settled by a few flippant sentences. My position is that on every ground, *a priori* and empirical, the evidence goes to show that it is impossible to attribute the transport of these stones to an ice-sheet or icebergs from Scandinavia, as Mr. Harker argued in his well-known Yorkshire Memoir, and as I presume from his ambiguous phrases he argues still. If he can suggest a more reasonable and simple explanation of the presence of these stones where they have been found than mine, I will gladly accept it. Hitherto he has failed to do so: hence these tears!

H. H. Howorth.



Edwin Wilson Cambridge.

NEW BALA TRILOBITES.

https://doi.org/10.1017/S001675680000577X Published online by Cambridge University Press