

Another depression to 40 or 50 feet filled up these ravines. Then came final re-elevation, and it is possible that a downward movement is now in progress.

CORRESPONDENCE.

NAMES FOR BRITISH ICE-SHEETS.

SIR,—Although Professor Bonney does not, I believe, at present allow himself to be included among “glacialists who hold the ‘land-ice theory.’” to whom my letter on the above subject (*GEOL. MAG.*, March, 1901, p. 142) was addressed, his comments (*GEOL. MAG.*, April, 1901, p. 187) are particularly welcome as he shows, by practical application of two of the terms, that the proposed nomenclature may have its advantages even to the opponents of the ‘land-ice theory.’ Granting that the former existence of ice-sheets in this country is a disputed inference, we may nevertheless find the suggested terminology convenient in the discussion, even when it is denied that the terms represent anything more than an ill-founded conviction. From Professor Bonney and those who think with him I ask no more than that the nomenclature of the British Ice-sheets be accepted on this basis.

By the way, I will seek Professor Bonney’s permission to amend his simile; surely, in this case it is not that the glacialist is counting his birds before they are hatched, but after they are flown, by the indications in the roost.

In his playful suggestion of ‘Dogger-fjeld’ as a name for the ‘East British Ice,’ and in his accompanying argument as to the direction of ice-flow, Professor Bonney seems to have taken for granted that the Dogger Bank was a *pre-glacial* feature. But there is much reason to believe that this Bank is of *glacial* origin, while of the *pre-glacial* contours of the floor of the North Sea we know nothing. In areas of low relief the radial point of ice-flow must depend principally upon the incidence of maximum snowfall, and under changing conditions of climate may not remain fixed in the same place. I have elsewhere set forth facts indicating that the East British Ice underwent great changes in this respect during the progress of the Glacial Period.

The issue raised by Professor Bonney as to the transport of the Scandinavian boulders to our eastern coast has been frequently discussed in my writings on the Yorkshire drifts; and it seems almost superfluous to reiterate my opinion that the presence of these boulders does not imply their direct transport across the North Sea basin by land-ice. I was convinced by my prolonged examination of the Basement Clay of East Yorkshire that the invading ice-sheet had ploughed up a sea-bottom already strewn with boulders from the shores,—“wherefrom it follows that we must not place much confidence in the evidence gleaned from its erratics as to the actual direction and distance which the ice-sheet has traversed.”

By another friendly critic a well-grounded objection has been raised to the proposed term 'Cambrian Ice-sheet,' on account of the risk of confusion with the common stratigraphical use of 'Cambrian.' It would, perhaps, be safer to fall back upon the phrase 'Welsh Ice-sheet' (with subdivision into 'North Welsh' and 'South Welsh' if found desirable).

As previously stated, my more immediate object is especially to urge the adoption of names for the (hypothetical?) ice-sheets of our sea-basins, for which I have recently felt the pressing necessity. On the terms proposed for the land-areas I do not at present lay much stress, though it would be convenient if these could be fixed at the same time.

G. W. LAMPLUGH.

TONBRIDGE.

April 6, 1901.

THE SODIUM OF THE SEA.

SIR,—I am extremely obliged to Mr. Fisher for his kindly notice of my communication concerning the "Sodium of the Sea," but feel at a loss how to reply, owing to uncertainty as to whether Mr. Fisher has considered and rejected De la Beche's articles on Granite and Elvan, Divisional Planes, and Mineral Veins and Faults; or, has possibly overlooked such an ancient authority.

In addition to all that De la Beche and Dr. Sorby have written, and since the last edition of the "Physics of the Earth's Crust," we have the additional fact that all the types of fluid inclusions found in granites may be matched in different quartz-veins, so that all the arguments based on the fluid inclusions in igneous magmas must be prepared to meet the cases of the veins. My object in writing was not so much to defend the sea-water hypothesis, as to remind geologists that it existed. Throughout my own early training I was never allowed to forget that the weakest link in a chain is the measure of its strength, and I knew full well that the slightest slip in fact or argument involved public castigation in the Transactions of the Devonshire Association. If any of the younger geologists in Devonshire erred in discipline our captain, William Pengelly, rarely failed to pipe all hands on deck to witness punishment. Mr. Fisher, I expect, will agree with me that in the present day it is considered of far more consequence that a theory should present a solid appearance than that each link should be tested, and if defective, rejected, not only by the purchaser but by the chainmaker himself.

A. R. HUNT.

FOXWORTHY, MORETONHAMPSTEAD.

May 7, 1901.

INTERNATIONAL GEOLOGICAL CONGRESS.

SIR,—I regret that I omitted to express my thanks in my paper, "Geological Notes on Central France," published in the GEOLOGICAL MAGAZINE (February, 1901, p. 59), to the Directors, MM. Boule, Fabre, and Martel, for their kindness and consideration during the