

Having been answerable for the theory which attributes the origin of these pebbles (I mean the quartzite pebbles chiefly) to the Old Red Sandstone of Scotland—to which they bear a remarkable resemblance—I wish now to state that I have abandoned the view of their North British origin altogether in favour of that proposed by Mr. J. W. Harrison, who considers them to have been derived from the concealed ridge of older Palæozoic rocks, which we now believe to underlie the Mesozoic strata of the Centre and East of England. At the time I suggested the Scottish source, I was freshly and vividly impressed with the resemblance of the reddish or “liver-coloured” quartzite pebbles to those of the Old Red Conglomerate of the Lesmahago and other districts. But I all along felt the difficulty (on which Mr. Harrison lays just stress) that the number and size of the Bunter pebbles decreases from the Central District of England towards the North-west, which ought not to be the case if they had had the origin I attributed to them. Further reflection leads me to think that the objection is fatal to the view either of myself or of Professor Bonney, notwithstanding the microscopic resemblance which he points out to the quartzites of the Highland rocks. Indeed, it is difficult to picture to one’s self how the pebbles could have “got round” the promontory and barrier formed by the Silurian rocks of the S. of Scotland without having been very thickly strewn over the submerged tract of the N.E. of Ireland and of N. Lancashire; but such pebbles are almost entirely absent from the Bunter Sandstone of Antrim and Downshire.¹

A reference to Plate IX. of my Palæo-physiographical maps will assist in making this tolerably clear. The ridge of old rocks which occupies the Eastern Counties and ranges as far North as a line drawn from the Wash to the mouth of the Avon, shown on the map, very probably contains beds of quartzite, porphyry, and hornstone, etc., such as are found in the Bunter of Staffordshire; and the supposition of such a source seems to be attended with less difficulty than that of any other yet proposed.

5, RAGLAN ROAD, DUBLIN,
5th May, 1883.

EDWARD HULL.

ON THE SO-CALLED PLANT-FOSSILS FROM CENTRAL WALES.

SIR,—In his recent communication on this matter (GEOLOGICAL MAGAZINE, April, 1883, p. 192) Mr. W. Keeping expresses the opinion that *Nematolites Edwardsii* “is a Coralline Alga.” That object has formerly been described as “solid bodies of pale-chocolate colour and earthy constituency.” Now, it seems difficult to understand why a Coralline Alga should be more easily converted into earthy matter of pale-chocolate colour than branched burrows and tunnels of annelids should be filled up by such a sediment, and I consequently fail to see any evidence whatever why the opinion which has been expressed in my former communication should be altered. As to *Nematolites dendroideum*, I have not hitherto said anything on it, and

¹ It may be objected, truly, that the stage of the “Pebble Beds” is but sparingly represented in N.E. Ireland.

Mr. Keeping might be quite right that it cannot be a worm trail. But, to conclude from the poor specimen hitherto described, its mode of branching is so very unlike that of a true plant, that it seems very difficult to believe that it should be of vegetable origin—at least until better specimens should have proved it. Concerning *Buthotrephis major*, its different modes of occurrence (usually “as a delicate impression, or as half-compressed solid bodies,” sometimes upright in the sediment, “when the circular sections and tip end of the branches come to resemble rain pittings”) harmonizes so perfectly with the branched trails and burrows of some annelids—which now creep on the surface of the mud, now make burrows in it—that there is no reason why that object should be regarded as something else. And Mr. Keeping has failed to give any satisfactory statement to prove its true plant-nature. Lastly, as to *Myrianites Lapworthii*, it might perhaps in some cases be convenient that such bodies should have their names, but it ought not to be forgotten that the specific value of such a name is *nil*.

STOCKHOLM.

A. G. NATHORST.

CHALK MASSES IN THE CROMER DRIFT.

SIR.—To properly deal with all the questions raised by Mr. Searles Wood's letter in the May Number of the GEOLOGICAL MAGAZINE, would be more than is possible within the limits of a letter. I may, however, be permitted to observe that if the Chalk Masses, in which term I include the whole, whether of solid or reconstructed chalk, from the western side of the Wolds of Central Lincolnshire, as supposed by Mr. Wood, it ought to be possible to trace them up their origin through a train of such blocks. Has Mr. Wood done this?

There is no difficulty whatever as regards levels in the derivation of the great mass of Chalk Boulders in the Cromer Drift from the Norfolk Chalk. To use a harmless expression, it seems like “taking coals to Newcastle” to bring chalk boulders from Lincolnshire into Norfolk. But it is far from me to deny the possibility of such an origin, if sufficient evidence were adduced in its favour, which I venture to think has not yet been done. Mr. Wood, to say the least, is peculiar in his view that all the large masses are not genuine, but “reconstructed” chalk. In this opinion I differ from him along with some pretty good authorities, both old and new. Does he affirm that the Old Hythe Pinnacle of Chalk, from 70 to 80 ft. high, figured by Sir Chas. Lyell, was of “reconstructed” material, or—what can be tested at the present moment—that the boulder figured in my paper (page 231) is not of solid chalk, or that those shown in Clement Reid's careful survey section are not genuine? In conclusion, I may add that whatever may be the exact locality or localities of the Chalk Cliffs to which the boulders may eventually be traced, it cannot invalidate my reasoning as to the mode in which they have been quarried, detached, rafted off, and stranded.

May 5, 1883.

T. MELLARD READE.