

terraces look very much like old beach lines, but as they have not been cut into I cannot say for certain.

A reference to the Ordnance Map, No. 82, south-west, will explain the relative position of the localities above referred to.

I am, yours truly,

J. M. MELLO.

ST. THOMAS'S PARSONAGE, BRAMPTON, CHESTERFIELD,
July 22nd, 1867.

DR. T. STERRY HUNT'S THEORY OF THE EARTH.

To the Editor of the GEOLOGICAL MAGAZINE.

SIR,—I have read with considerable interest the very ingenious theory of the "Chemistry of the Primeval Earth," by Dr. Hunt, which is contained in your issue for August, and beg your permission very briefly to ask the Doctor how his theory is compatible with the following facts respecting the mean densities of the sun and larger planets, or whether the theory of their extensive hollowness does not more satisfactorily account for their low mean densities than does that of the sun, the earth, and, by inference, all the planets increasing in density to their centres.

The following are approximately the mean densities of the sun and the larger planets:—

Sun... ..	1.42	Uranus	1.0
Jupiter	1.37	Neptune... ..	0.5
Saturn	0.5		

and those of the smaller planets are—

Mercury... ..	6.6	Earth	5.5
Venus	5.6	Mars	5.6

The densities of the asteroids are unknown, but should they be ascertained, I venture to predict that they will probably be found of higher mean density than are any of the planets just enumerated. All the large planets have very low mean densities; all the smaller planets have high and nearly uniform mean densities.

How are these facts to be accounted for on Dr. Hunt's theory of condensation and increase of density to the centres?

I am, yours obediently,

T. P. BARKAS.

NEWCASTLE-ON-TYNE,
August 6th, 1867.

ON THE SEQUENCE OF THE DRIFTS IN THE EASTERN COUNTIES.

To the Editor of the GEOLOGICAL MAGAZINE.

DEAR SIR,—With reference to Mr. Wood's suggestion, that I should give complete sections from his "upper drift" to the beds exposed on the coast, I wish to say that I have not materials by me to work out the details he asks for, and it appears to me that the point at issue would not be explained by exact particulars of surface contour, and the position of the crags in relation to the overlying drifts. There is no difference of opinion as to this, and all are agreed that the gravels underlying the Boulder-clay of High Suffolk correspond in height with much of the gravel superimposed on the

clay in the coast cliffs. This I admitted in my first paper, and am quite aware it presents a *prima facie* case in favour of Mr. Wood's views; and furthermore on the view I suggested I should expect that the variety in the component materials of Mr. Wood's "middle drift" would prevent any certain distinction being observable between them and the gravel seen on the coast, even if a section happened to expose their junction.

The difficulty Mr. Harmer raises seems to me to be equally applicable under any view; if, for example, the coast beds at Trimmingham are much above the level of Norwich why are they not, on Mr. Harmer's view, intercalated between the crag at Thorpe and the beds Mr. Harmer has identified with Mr. Wood's "middle drift." Surely some cases ought to occur among the numerous exposures of Norwich Crag in Norfolk, in which the Boulder-clay of the Cromer cliffs can be seen to intervene between the Norwich Crag and Mr. Wood's "upper-and-middle-drifts." The absence of Boulder-clay as the highest member of the cliffs of the Norfolk coast (the equivalent of that in High Suffolk) I have already admitted in my first paper *might*, on Mr. Wood's views, be the result of denudation; but its absence throughout the district, wherever Boulder-clay is known to form the base of the cliffs, is rather remarkable. Mr. Gunn and Mr. Wood I am aware believe that it does exist in the low cliffs at Pakefield and Corton, but if *all three divisions of the drifts* are developed at these points, within a height of thirty or forty feet, it involves the difficulty of a great attenuation of Mr. Wood's two upper divisions after they leave the high land and descend more than 200 feet to the sea-level. At Corton, the assumed equivalent of Mr. Wood's "upper Boulder-clay" is but from three to nine feet thick; at Hasboro' ten feet; whilst in High Suffolk the Boulder-clay attains a thickness of at least sixty feet.

The occurrence of derivative fossils would seem to be rather an uncertain guide in the classification of the drift series. Mr. Taylor (at page 238) observes that the coast clay has been formed principally by the wreck and denudation of the Lias (and the editor adds, of the Kimmeridge clay); but this is really no distinctive feature, as the Bedfordshire Boulder-clay, which is evidently an extension of the High Suffolk clay, is literally loaded with these fossils, and the late Mr. Trimmer (in the quotation given by Mr. Gunn) described his "upper Boulder-clay" (the "upper drift" of Mr. Wood) "as characterized by an abundance of oolitic detritus." Unless it is assumed that the materials of the Boulder-clays have been derived from a distance, and in each from different directions, it seems probable that successive deposits in the same localities should contain similar derivative fossils.

In the observations I have made I have wished rather to leave the succession of the drift deposits an open question than to lay down unequivocally any order of sequence. It is a subject that may well be held in suspense, and the evidence in relation to it seems scarcely of a nature to base exact conclusions upon, or to afford materials for mapping out the various subordinate divisions of

the drift series. The existence, however, of Mr. Harmer's "Third Boulder-clay," as a distinct formation, seems to depend on the certainty of the coast beds being inferior to those of High Suffolk. The small patches of Boulder-clay in the Yare Valley are clearly more recent than the drifts that have been cut through to form the valley; but does it not seem less improbable that the coast beds may be identical with them, than that these isolated patches of a marine deposit should have been the solitary result of the submergence during which they were formed?

GEORGE MAW.

BENTHALL HALL, BROSELEY,
August 6th, 1867.

"THE LOB-WORM EPOCH."

To the Editor of the GEOLOGICAL MAGAZINE.

SIR,—Colonel Greenwood's remarks in the August number of the GEOLOGICAL MAGAZINE on the "Lob-Worm Epoch" tempt me to lay before your readers a few facts concerning the rocks of that period, as shown in this neighbourhood, and the results obtained by their examination during the last few years.

Mr. Salter and myself have for some time felt convinced that most, if not the whole, of the Cambrian rocks belonged to a fossiliferous period, and accordingly in our own report to the British Association in 1865, on the "Lower Lingula-flags" (Menevian group) and its fossils, it was stated that, "though the purple band series have not yet yielded any definite traces of these higher forms of fossils, we are scarcely warranted in looking upon that as a proof of their absence; neither is it likely that so rich, though limited, a fauna should come so suddenly into existence." Since then I have been fortunate enough to find fossils in these identical purple beds, which prove the facts at that time only conjectured.

In a paper by Mr. Salter and myself, read before the Geological Society on June 19th, an account is given of the finding of a *Lingulella* in the red rocks of the Lower Cambrian-rocks, hitherto deemed quite destitute of higher organisms than worms, and belonging to the very series mentioned by Mr. Baily. I have found also, subsequently to the reading of the paper referred to, a whole colony of species (trilobites, etc.) still lower down, showing, beyond a doubt, that much, if not the whole, of the so-called "worm epoch" represents a time when animals of much higher forms than worms were in existence, and flourished in the seas of the period. I therefore feel satisfied that if active explorations be carried on in North and South Wales, it will be proved that the series throughout is truly fossiliferous, but I am also sensible that some time will be required to decide the fact, since the working of the strata is, in many ways, difficult, and the deposit from its very nature, as a rule, unfavourable to the exhibition of organic remains.

Moreover we are sure to find, especially in so extensive a series, much that is but very slightly fossiliferous, or, indeed, almost barren, intervening between colonies of rich faunas. Such is really the case