

and there in the valley bottoms. The chief centre of activity probably lay west of the centre of the island.

Petrographical details of the andesites and anamesites, descriptions of groundmass and included minerals of each, and chemical analyses are given. As regards the age of the constituents, the Author arranges them in the following order, commencing with the oldest:—magnetite, olivine, augite, mica, felspar, nepheline.

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### CORRESPONDENCE.

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#### THE ST. BEES SANDSTONE.

SIR,—In the short notice in the last number of the *GEOLOGICAL MAGAZINE*, of Mr. Goodchild's paper on the above, read before the British Association at Edinburgh, I read that he considers the St. Bees Sandstone equivalent to the Bunter. I entirely disagree with him in this view. It is well known to those who are conversant with the Bunter Sandstone formation that it consists of the Upper Soft Red and Mottled Sandstone. The Pebble and Conglomerate beds and the Lower Soft Red and Mottled Sandstone, only the two lower divisions occurring in the North of England. These are well marked divisions, with beds totally unlike the St. Bees Sandstone.

I believe it to be much more probable that the St. Bees Sandstone is a large development of the Red Marls, Sandstones, and Gypsum beds that lie between the Upper and Lower Magnesian Limestone in Nottinghamshire and Yorkshire, and known as the "Permian Middle Marls and Sandstones." There is a break between these Sandstones and Marls and the Lower Magnesian Limestone quite as large as between the St. Bees Sandstone and the Magnesian Limestone and Penrith Sandstone. But breaks between two formations are often only local, caused by thinning away of beds, and there is really no great unconformity between the Bunter Sandstone and the Magnesian Limestone series (now called Permian) of Yorkshire, and I should not be surprised that in some locality it was found that the one passed up into the other. I will not quarrel with Mr. Goodchild for calling these lower formations "Lower New Red Sandstone" (the old term) though I do not like it, for in Yorkshire these beds are chiefly limestone, but I must protest against the St. Bees Sandstone being called Bunter, a formation, I consider, on a higher horizon.

W. TALBOT AVELINE.

OATLANDS, WRINGTON, SOMERSET,  
*December 5th, 1892.*

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#### ON THE SUPPOSED CONFLICT BETWEEN GEOLOGY AND PHYSICS.

SIR,—The late Dr. James Croll, while contending that there was ample proof from geology that conditions suitable for life on the Earth must have existed "far more than twice 20 millions of years ago"<sup>1</sup> (the narrow time limit of 20 million years only being supported by some physicists):—nevertheless Dr. Croll could not solve the following difficulty.

<sup>1</sup> Dr. Croll's paper in the *Quarterly Journal of Science*, July, 1877, p. 317.

If the earth (completely crusted over) have existed "far more than twice 20 million years," *a fortiori* then must the sun have been present longer than this period; for it is obvious that a habitable globe could not survive without the sun. But it is certain that if the sun have endured anything like this time, the contained store of heat must have been such—as Dr. Croll points out—that the solar nebula at that primitive age "would have extended beyond our Earth's present orbit, and of course our Earth could not at that time have existed as a separate planet" (Dr. Croll's paper in the *Philosophical Magazine*, May, 1868, p. 372). To infer the degree of expansion of the sun at that remote epoch, we have merely to deduce (an easy process) what the sun's temperature then was at his present rate of cooling.

Dr. Croll does not attempt to solve the difficulty he lays stress on; but is it not obvious, in view especially of the recent ideas as to space being<sup>1</sup> comparable somewhat to a "meteoric *plenium*," not to mention the continued other friction opposed to the flying globe—that the Earth's present orbit could not have been its original orbit? But the Earth must have come in an enormous distance towards the sun in over twice 20 million years, which is the lowest limit for the period which geology demands for the Earth's past existence, according to the evidence afforded by the known rate of deposition of sedimentary strata, etc. This kind of evidence is not vague, but mathematically convincing. Dr. Croll remarks, viz. "We have not sufficient data to determine how many years have elapsed since life began on the globe, for we do not know the total amount of rock removed by denudation; but we have data perfectly sufficient to show that it began far more than twice 20 million years ago" (*Quarterly Journal of Science*, July, 1877, p. 317).

So then we have the apparent fitness that then the sun was hotter than at present, the planets were further off. As the sun cools down, the planets approach him, this fact equalizing the conditions for life on the Earth over a far longer time-epoch than would otherwise be possible. This consideration affords apparently plenty of margin for past geological time, without coming into any conflict with physics.

The idea of a 20 million years' margin for the age of the sun's heat, enunciated by Lord Kelvin, depends on the assumption (in aspect arbitrary?) that the sun was formed by the gravitational approach of widely diffused matter in a primitive state of rest, for which "state of rest" we have surely no evidence. On the contrary, all analogy (all we observe in the skies now) goes to indicate that the matter whose collision formed the sun, was originally in motion. If this primordial motion naturally contributed to the heat developed at the concussion of such moving matter, whose encounter generated the solar heat: then any store of heat that geological time may require, could have been irrefragably produced at the sun's formation.

<sup>1</sup> In a letter in "Nature," March 28, 1878, p. 243, on "The Age of the Sun's Heat in relation to Geological Evidence," an analogous solution to the above difficulty was offered by the present writer.

The necessity for the narrow 20 million years' margin, which clashes with geology, is seen then apparently not to have the slightest foundation. It rests on the gratuitous hypothesis that the sun's heat was derived *solely* from gravitation, entailing an approach of matter in a primitive state of repose. The quantity of heat generated under these premises was calculated originally by Helmholtz to suffice for 20 million years of solar radiation.

Moreover, the sun is not yet cooled down: so that a notable part of the 20 million years period, which is the inexorable limit of the above hypothesis, must be spread over *future* time. How much is left for *past* duration of the solar system and for geological history of our globe then? Neptune and Jupiter were certainly shed from the revolving-contracting solar nebula some millions of years before the Earth, *i.e.* before the Earth had a separate existence. Some millions of years must be then inevitably lopped off the other end of our already contracted time-margin. What is left over for the Earth's past existence then: so that on the (exclusive) gravitational hypothesis of the source of the sun's heat, no geological epoch worthy of that name would remain.

S. TOLVER PRESTON.

HAMBURG, Dec. 14, 1892.

#### THE MAMMOTH AND THE GLACIAL DRIFT.

SIR,—I wish Mr. Jukes-Browne had devoted a little more of his last letter to Geology and a little less to offensive personalities. To these latter I do not propose to reply. What is alone interesting to your readers in this correspondence is to fix the exact age of the Mammoth, a matter of importance not only to the geologist but more especially to those devoted to the early history of man. To the settlement of this problem Mr. Jukes-Browne's last letter adds nothing. He reverts to two cases he had already quoted, one of them the well-known case at Hoxne, where, as I showed, there is not only no positive evidence forthcoming but which was riddled through and through by Mr. Flower. There can be no doubt whatever that judging by the published evidence the case of Hoxne breaks down. There is some evidence that at that place the drift beds overlie the Mammoth bed. There is none that will bear criticism that they underlie it.

The second case from Burgh, where it was not the Mammoth but the *Elephas antiquus* that was found, I have already criticized.

I must correct a curious delusion of Mr. Jukes-Browne, that on this question I have set myself against the best authorities. The best English authorities on the age of the Mammoth known to me are Professor Dawkins, Professor Geikie and Dr. Hicks,<sup>1</sup> all of whom virtually agree with me, or rather, I with them. The French geologists are almost without exception on the same side, while among the geological surveyors, to whom perhaps Mr. Jukes-Browne limits "authority," Mr. Lamplugh and Mr. Skertchly have been liberally quoted by myself, but as a matter of fact authority has and ought to have very little place in geology any more than in any

<sup>1</sup> See Dr. Hicks's letter.—EDIT. GEOL. MAG.