

distinct as they are if these steep mountain-sides had been exposed to annual rainfall of 100 inches for the last 200,000 years?

Above Grasmere there is a small lake, known as Easdale Tarn, which seems to have been partly formed by a terminal moraine, while earlier ice-markings down the valley seem to indicate that the glacier formerly pursued the same course which the stream from the tarn now follows. Though the drought of the season had been remarkable this stream was not a very small one, yet the work of erosion done by it since the Glacial period was not of a very startling description. It was not what I should expect a rapid mountain-stream (though checked by a small tarn) to effect in 200,000 years.

Such phenomena are by no means peculiar to the Lake District. The great fall of Niagara seems to be an example. There is, I believe, no trace of an earlier post-glacial river-channel. On the contrary, the ice-markings almost down to the water's-edge above the fall seem to show that the glacier followed the same track as the river. The river must, therefore, have commenced cutting out of the gorge as soon as the ice-cap cleared away. But Sir Charles Lyell estimated the time necessary to cut out this gorge at 35,000 years, while others have placed it as low as 12,000. Professor Winchell has estimated the time required to cut out the gorge below the falls of St. Anthony at 8000 to 10,000 years. I am not aware that there is any reason to think that this excavation did not commence until long after the close of the Ice Age.

Man probably existed on the earth in the Glacial, if not the Pre-Glacial, era. But is there any reason to suppose that he existed for at least 200,000 years without making any solid progress in civilization, and then suddenly made the great advances (emanating apparently from more than one centre) which has taken place in the last 10,000 (or perhaps 6000) years? It is not a case of an anthropoid ape slowly developing into a man during a period of 200,000 or 800,000 years; for the earlier skeletons appear to be those of fully-formed men.

For these reasons I think Dr. Croll refers the Ice Age to too remote a period. Further researches on the amount of post-glacial erosion and the erosive power of the streams or rivers engaged in it ought, I think, to enable us to decide the question one way or the other with a tolerable degree of certainty.

DUBLIN, *Sept. 5th*, 1887.

W. H. S. MONCK.

DR. HINDE ON THE ORIGIN OF CARBONIFEROUS CHERT.

SIR,—Permit me to reply to the article by Dr. Hinde, F.G.S., which appears in the *GEOLOGICAL MAGAZINE* for the present month (No. 280, p. 435). As I had an opportunity when attending the meeting of the British Association in Manchester of hearing the paper read in Section C, and as I had previously had the pleasure of a visit from its author in this city, I was not unprepared for the onslaught which afterwards took place. I have no wish to maintain a position which subsequent investigation has shown to be untenable, or which requires readjustment; and I am, therefore, quite ready

now to admit, what I stated in Manchester, that in examining the slides of chert under the microscope, I had, at least in many instances, mistaken forms of sponge-spicules for those of Crinoid stems. Nor was this, I submit, at all surprising when we recollect that these organisms are often very vague, and that the bands of chert occur intercalated with beds of limestone abounding in stems, ossicles, and plates of Crinoids. It was not unnatural, therefore, that I should have supposed the little discs seen in the chert-sections under the microscope to be minute cross-sections of these stems or ossicles.¹ Dr. Hinde's great experience in the examination of the sponge-structures of the Cretaceous beds has given him an advantage in this line of investigation which certainly has not fallen to my lot; consequently, when on examining my slides in this office, he stated that the forms were those of sponge-structures, I accepted his statement without question.

Dr. Hinde's recent investigations undoubtedly show that siliceous sponge-structures enter far more largely into the composition of Carboniferous chert than has hitherto been suspected, and even, that they exceed in numbers other organic forms; all this I now willingly concede. But I am not prepared to go to the full length of Dr. Hinde's demands, as I understand them, nor to abandon as untenable the proposition that much of the silica of Carboniferous chert has been derived by a transmutation process from the waters of the ancient seas. Not only are there to be found forms, such as those of Corals, Brachiopods and Polyzoa and ossicles of Crinoids, originally calcareous, now occurring silicified in the chert, but the amorphous cementing material of the organic structure which may be supposed to have originally been calcareous has now been transmuted, or may have been directly deposited from the waters under such favourable conditions as those supposed by Mr. Hardman and myself in our original memoir.² A similar conclusion has been arrived at by Prof. Renard by a process of investigation analogous to, though quite independent of, that pursued by ourselves. Whatever doubt I might have entertained in regard to my own conclusions, I cannot extend to those of so competent an observer as Prof. Renard. Let me ask Dr. Hinde, does he deny the possibility of siliceous bodies or masses having been formed by the transformation process? If so, he is confronted by the evidence of a large number of the ablest observers, both British and Foreign, amongst whom may be mentioned Bowerbank, Rupert Jones, Sullivan, Sterry Hunt, and Bischof.³ If so, it is *Hinde contra mundum*; and let me say that these observers appeal to Nature as well as does Dr. Hinde himself.

I feel obliged to Dr. Hinde for having called my attention to the fact that Professor Sollas had previously identified siliceous sponge-spicules in the chert-sections which I had forwarded to him for

¹ From what Dr. Hinde has stated in his original paper (Phil. Trans. 1885, note, p. 433), it would appear that a more competent observer than myself has fallen into a similar error; from which it will be inferred that it is a matter of much difficulty, and requiring great experience to distinguish the one from the other.

² Scientific Transactions, Roy. Dublin Society, vol. i. new ser. (1878).

³ Bischof: "Chemical and Physical Geology," Cavendish edit. vol. ii. p. 486, *et seq.*

examination. When I wrote my paper for the Royal Society, I had altogether forgotten this circumstance, which I much regret; a copy of the paper itself had been carefully laid aside by me for reference, and suffered the fate of most such papers.¹ Having, however, now referred to this document, I will put it in evidence on the question now discussed, and the reader will hear what Prof. Sollas has had to say. The author examined five of my slides of chert-sections under the microscope, and states as the result of his examination; "in the first place, to completely confirm his (Prof. Hull's) clear descriptions of the appearances presented by them (the chert-sections); and next, to establish the truth of my supposed detection of sponge-spicules," as shown in the plate accompanying my original paper. This is very valuable testimony.

As to the question of the geological position of the so-called "Yoredale Series," I have only to say that I have used the term as it is understood on the Geological Survey throughout the wide district where, in conjunction with some of my colleagues, I mapped these beds some years ago in South Lancashire, Cheshire, Derbyshire, and the adjoining parts of Yorkshire, an area of not less than 2000 square miles. What may be the exact relations of these beds to those of the Valley of the Yore as described by Phillips, I am not prepared to say. It is well known that the Lower and Middle Carboniferous strata undergo considerable alteration both in character and thickness as we proceed northwards from Derbyshire and Cheshire; and I can only now refer Dr. Hinde to my paper on the Classification of the Carboniferous Rocks published in the Quarterly Journal of the Geological Society, vol. xxxiii. for my views on this subject.

In conclusion, I have to add, that if I have misquoted Dr. Hinde, as he affirms, I can only express regret, as I took special care not to do so, as will be seen by referring to my paper in the Proc. Roy. Soc. vol. xlii. p. 305. My late valued colleague, Mr. Hardman, whose name occurs in this controversy, is not now with us to take his share therein, but I feel convinced he would have concurred in what I have written.

EDWARD HULL.

GEOLOGICAL SURVEY OFFICE, DUBLIN, 7th Oct. 1887.

OBITUARY.

JOHN EDWARD LEE, F.S.A., F.G.S., ETC.

BORN DECEMBER 21ST, 1808; DIED AUGUST 18TH, 1887.

DEVONSHIRE has lost another excellent geologist and antiquary in Mr. John Edward Lee, of Villa Syracuse, Torquay. Mr. Lee was born at Newland, Hull, Dec. 21st, 1808. His father having died when he was very young, he was brought up by two uncles, Avison and John Terry, and at sixteen he entered their shipping office in Hull. From the earliest period of his life he took an interest in science, beginning with Entomology, and while living

¹ Prof. Sollas's paper was published in the *Annals and Magazine of Natural History* for February, 1881.