

while he asserts that their conclusions are wrong, he ignores the evidence they produce, such as the invasion of one area by boulders from another, and the gradual decrease in the number of the boulders as they recede from their parent source.

I had not sufficient time to search for numerous instances of striæ; but so far as the disposal of the stones are concerned, I can corroborate Messrs. Peach and Horne's observations in the greater part of Northmavine; and see no escape from the conclusion that, to the north-west of Hillswick, the ice-sheet flowed from the S.E. to N.W., while further south, and on the east side of the island, the direction was from north-east to south-west.

THOMAS STEWART.

WATER WORKS, GLASGOW, 14th Dec. 1881.

MR. HOWORTH'S REPLY TO MR. REID.

SIR,—In his former note Mr. Reid spoke of the theory of violent changes as extinct. To this I replied that in calling it extinct he must have overlooked the weighty opinion of Continental geologists. Mr. Reid now confesses that he did so, and that he was merely speaking of geologists on this side of the Channel, and he proceeds to justify his limitation by a sweeping depreciation of recent Continental writers on Post-Glacial geology. It certainly seems extraordinary that such an experienced and deservedly widely-known observer as Mr. Reid should permit himself to write thus in your widely-circulated pages. I hope I have as great a respect for the magnificent work done by Mr. Prestwich and those who have succeeded him as any one; but I must confess, and others will assuredly echo my words, that, judged by the abundance of its facts, the careful sifting of its evidence, and the brilliant character of its induction, no work produced on this side of the Channel, in recent years, dealing with Post-Glacial geology, can compare with M. Belgrand's *magnum opus* on the Seine Valley; while it is literally incredible how any one who has read any considerable number of the memoirs which have seen the light in recent years in France, Belgium and Italy, dealing with this very difficult period, can speak of them as in any way inferior to the contemporary writings of English geologists. They far exceed in number and in minuteness of treatment similar memoirs written here, for the very good reason that the people whose interest in these deposits has been excited by the discovery of remains of Palæolithic man in them is very much in excess there of what it is here. Leaving, however, this debateable land, where rival national reputations are necessarily weighed in a very uncertain balance, what is to be said of what follows? Mr. Reid tells us that "In most parts of the Continent the Pleistocene deposits appear to be represented by one tolerably uniform mass, like the Loess of the Rhine, or the Tundras of Siberia." Is this so? I was under the impression that the number and variety of the so-called Pleistocene beds in France and Southern Russia should be described by any adjective rather than *uniform*. Having postulated this, Mr. Reid contrasts the deposits on the Continent with the wonderful variety of the beds in Britain, their fossiliferous character,

and the splendid series of cliff sections and caves here. What can be the meaning of this? Where have we such continuous sections through these same deposits as can be shown in the banks of the great rivers of the Continent? In what respect are these deposits richer in fossils here than there? How are the British caves better situated for deciding the question than the caves of Belgium? I cannot understand what Mr. Reid means. We may indeed institute a comparison, but it is very different to the one made by him. When we cross the Channel, we find the Post-Glacial beds arranged in continuous series over an area of 200 degrees of longitude, in many places *in situ*, and undisturbed in sections showing the whole of the beds; while in Britain we are on the extreme edge of these formations, where they are dislocated and broken and fragmentary. Assuredly it follows that on the Continent we have every element for studying the problem correctly, while here we are in perpetual danger at every turn of mistaking a local and sophisticated section for one that is normal.

Mr. Reid contrasts my humble position as an antiquary given to respecting authority with his own exalted one of a Member of the Geological Survey, "who has learnt to believe nothing that he is told and only one-half of what he sees." I am not sure that this is a desirable byeway into which to drag the controversy. Such contrasts might if pressed lead to some unexpected comments. Surely it is better to put aside both geologist and antiquary for the nonce, and to address each of your readers as if he were a keen Philistine apt at judging evidence. Not merely *evidence of the senses*—for the facts are not in issue; we are agreed about the facts; but the more important evidence of *inference* and *induction*. In this view may I point your readers to another sentence of Mr. Reid's? He says: "The extermination of the Mammoth in Britain and Germany may be referable to human agency, while in Siberia it was gradually killed by the cold and want of food."

May I ask, nay, entreat, Mr. Reid to furnish one tittle of evidence in support of this emphatic statement? I have tried to supply your readers with a very considerable amount of evidence, all of which tells entirely against such a view. Mr. Reid cannot seriously suppose that all this evidence can be answered by an *obiter dictum*, a mere dogmatic assertion without any proof whatever, and that your readers will accept it as conclusive, even if all that most exalted brotherhood to whom this generation is under such deep obligations, viz. the Geological Surveyors, were to shout the aphorism in chorus. As to the statement that the Mammoth together with his companions were exterminated *by man* in Europe, I fear that the Philistine crowd, when it confronts such a statement with our present knowledge, will assuredly smile diplomatically, if it does not have recourse to some unseemly sarcasms.

Mr. Reid says a decrease of one degree in the temperature in a century would be very rapid from a geological point of view.¹ I reply, whether rapid or the contrary, the problem I have invited

¹ Would it not be safer to say from Mr. Reid's geological point of view?

Mr. Reid, and those who think with him, to solve, and which I cannot find any one willing to face, is how, with such a change only as one degree in a century, or anything like it, we are to account for the preservation of the flesh of the Mammoth in a series of carcasses found in the whole length of Siberia, and the condition of which will not admit of their having been thawed once since they were first frozen. There is the gauge. I challenge Mr. Reid to pick it up, and to give us something in the shape of a reasonable explanation.

Mr. Reid disapproves of my quoting Cuvier and Buckland and D'Archiac, inasmuch as they are obsolete authorities on this point. Why D'Archiac—whose career was parallel with Lyell's, whose opinions are quoted by Lyell with the greatest respect, and whose text-books are those in ordinary use in France—should be deemed in any way an obsolete authority, considering the immense amount of work he did in elucidating these very deposits, I know not. As to the other two names, I only quoted them as a protest against Mr. Reid speaking superciliously of a theory which had the imprimatur of such excellent observers. Perhaps before the subject is thrashed out, we shall be able to furnish him with a list of names which will surprise him, of geologists who have virtually indorsed Cuvier's and Buckland's views on the subject, and this too quite recently.

Mr. Reid says: "After several years of study of Pleistocene beds, I think that as a rule things did then progress faster, and that we are now in a position of exceptionally slow changes." This I most cordially accept, but I must say that this is not the Uniformitarian doctrine laid down by the leader of the Uniformitarian school in England, a most distinguished geologist very well known to Mr. Reid, on a famous occasion not three years ago. I also accept Mr. Reid's restriction of the invocation of cataclysm to cases which cannot be explained without it. It is because I have been for years trying to find an adequate explanation of this very difficulty, without invoking a cataclysm, and have failed, that I have written the papers which have appeared in the GEOLOGICAL MAGAZINE. I most cordially invite Mr. Reid or anybody else to try to solve the matter. Not however by *ex cathedra* statements. The problem is much too difficult and the evidence much too consistent on one side to admit of its being dismissed by one of those superficial waves of the hand which neither antiquaries nor Philistines have been trained to appreciate. The world cannot in these days be addressed, at least in the domain of science, in this pontifical fashion, and is apt to resent it. At all events that unmannerly part of the world which lives in Lancashire requires some definite reasons before it does the kowtow, and submits to oracular dicta. We are not discussing matters of fact which have come under the observation of such good eyes as those of Mr. Reid, and which we should at once accept, but *inferences* from facts of which we claim to be as good judges and as well trained as the great men whose Olympus is in Jermyn Street. I hope Mr. Reid will forgive my treatment of his reference to the antiquaries. If I did not value very highly his opinion, and his work, I should not be scribbling this letter.

DERBY HOUSE, ECCLES, Jan. 10th, 1882.

HENRY H. HOWORTH.