what then have they in addition been derived? The only other form of rocks we know of are the igneous rocks, granites—when not metamorphic—syenites, basalts, etc.; but no one can affirm that even these are parts of the original non-sedimentary globe; they are no doubt largely re-melted sediments. But for my purpose I assume that they are, and I find that the area of igneous rocks exposed to denuding agencies is about $\frac{1}{12}$ the area of the whole land, and there is reason to suppose that this proportion has endured since the earliest rocks we know of were formed.[1] It follows that if all the land areas, igneous and sedimentary, were denuded at the same rate —as no doubt they have been in the aggregate—the process of accumulation of the existing thickness of sedimentary rocks has taken 12 times as long as if they had been derived directly from a bare original crust. Now it is only on the latter supposition, which we know to be contrary to fact, that Mr. Wallace's calculation could be true in principle or result.

But whether I was right or wrong in the figures given, it is a fact admitted by all geologists since the time of Hutton, that the sedimentary materials of the globe have been used up over and over again, and any calculation of the age of the Earth based upon the rate of accumulation of sediments and their aggregate bulk which ignores this, as Mr. Wallace's does, is either incomplete or fundamentally wrong.

T. Mellard Reade.

Oct. 5th, 1883.

REPLY TO MR. SKERTCHLY.

Sir,—Since the above reply to Mr. Wallace was written Mr. Skertchly has published a letter in the Geological Magazine, on the same subject, in which he says, "First, I fail to see the slightest connexion between the area of exposed igneous rocks and the number of times sedimentary beds have been 'worked over' again. Surely at the beginning of geological time *all* the land was igneous, and practically that area has been diminishing ever since. This can therefore afford no clue to the question." To which I reply, *Geological Time* is the time of which we have *geological* knowledge, and Mr. Wallace's calculation as well as my own is limited to that time. The earliest recognized system of sedimentary rocks are Laurentian, and there is absolutely no data to prove that the igneous areas even in this period were greater in proportion to the sedimentary than they are now—if there is, I shall be glad to hear it. The hypothetical period between the Laurentian and the time when *all* the land was igneous is anterior to the date at which any calculation of the "Age of the Earth" based on sedimentation can commence, for there are no data on which to work. An inspired seer might perhaps tell us something of this period; but as I have no pretence to fill that rôle, it is useless for me to attempt it.

Mr. Skertchly also says, "Thirdly, Mr. Reade supposes the denudation of sedimentary rocks would reduce the mean thickness." As

[1] This question is discussed in my Chemical Denudation in Relation to Geological Time.

I am not aware that I ever held such an extraordinary view, I ask in what part of my letter this supposition is supposed to be contained, My reasoning is altogether based upon the *mean* thickness and superficial extent of the sedimentary strata of the earth, or otherwise their actual bulk, so the question as to whether Mr. Wallace has under or over-estimated the *maximum* thickness is quite immaterial.

Your correspondent says in conclusion that he has "never seen a *single fact* that tells against the view of the permanency of oceanic areas." I feel that this statement of what he "cannot see" is conclusive, and that further argument is useless.

Nov. 3rd, 1883. T. Mellard Reade.

MIDDLE HEADON AND MEADEND BEDS OF HORDWELL CLIFF.

Sir,—What could have induced Mr. Keeping to charge my father with error, and with saying that a bed which he had described as underlying the *Upper* Freshwater *only*, underlay the *Lower* Freshwater?

One of the objects of my father's paper was to correct antecedent errors, as to the extension of the Lower Freshwater into Barton Cliff (where Lyell had asserted that it occurred), and to show that the marine bed theretofore known only at Headon Hill, where it occurred between the Upper and Lower Freshwater, and was then known as the "Upper Marine" (now called the Middle Headon), but which had not been observed in Hordwell Cliff (and indeed had been expressly stated by Lyell not to occur there), did occur there, viz. at the ravine near Milford, 10 to 12 feet above high-water mark; and my father proceeded to describe it as occupying exactly the same position, relatively to the Upper and Lower Freshwater, that it did at Headon Hill, *i.e.* between the two, which is the position Mr. Keeping claims for it.

As the only bed which answered to Mr. Keeping's version of my father's description, viz. close to the beach and underlying all the Lower Freshwater, was the one which my father had described at Meadend, I naturally took him to mean this; for he named no locality.

"Paddy's Gap" is this ravine where my father described the then called "Upper Marine" as occurring, and *over*lying the Lower Freshwater, "the remaining portion of the Cliff to the eastward being, he considered, more from position than from its organic contents, the *Upper* Freshwater;" and from Mr. Elwes' letter, it appears that he, Mr. Keeping, Mr. Dawkins, Mr. Willett, and Mr. Shore have found the bed exactly in the position my father assigned, both geologically and actually; for even to the comparatively unimportant particular of its position, 10 to 12 feet above high-water mark, Mr. Elwes's statement that it is *in situ* "13 feet above the shore" shows that my father was right, and that what he described was no slipped mass as Mr. Keeping asserted, and as Mr. Elwes, strangely enough, repeats. Instead therefore of these gentlemen having, as they think, proved my father's error, they have demonstrated his accuracy in all respects.