metal such as iron or copper turned to a true spherical form while red-hot and allowed to cool, would deform more than that amount. Is not the burden of proof, then, on the other side? Ought not the objector to show cause why he assumes so preternatural a homogeneity?

3. But says Mr. Crosby, p. 244, "If we admit that the earth is of different composition on different sides, it would certainly be contrary to all analogy to suppose that the areas of different composition are sharply marked off from each other. Yet the steep slopes of oceanic depressions require according to these theories an abrupt change in radial contraction."

I would remind Mr. Crosby that according to my view (and also to Prof. Dana's) this *steep slope of oceanic basins is due to mountain-making not continent-making causes.*

4. In making some estimates of the amount of contraction, p. 244, Mr. Crosby takes account only of the contraction by solidification. But manifestly this is only a part, and perhaps but a small part of the whole contraction by cooling; and in addition to this there may be other causes of contraction besides cooling.

There are several other points which I might notice, but I fear it would make this letter too long.

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THE PERMANENCE OF OCEANIC AND CONTINENTAL AREAS.

SIR,—As a believer in and advocate of the "hypothesis of the permanence of oceanic and continental areas" now "becoming fashionable," and in the course of many years' daily work among rocks never having seen or heard of an actual case of a true "deep-sea" deposit, I should like to make a few remarks on Mr. Mellard Reade's paper on the "Age of the Earth."

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.

Secondly, as to the maximum thickness of rocks, which is what Mr. Wallace deals with, the tendency is rather to overestimate than underrate it. For example, it is usual to estimate the thickness of the Cretaceous rocks by adding together the maximum thicknesses in different localities, but this gives quite an erroneous result, and if applied to West Norfolk would make the result too great by about 2700 feet. In other words, 2800 feet of rock in various other localities were formed while only 100 feet were deposited in East Anglia. I am not taking account of beds removed by denudation; for there is no proof that the Maestricht beds, Upper Greensand, Gault, or Wealden ever existed there, and the Neocomian is under 100 feet. But to add together all these beds and take the sum as indicating the time of deposition, is as incorrect as it would be to take the beds now forming in the Black, Caspian, and Mediterranean Seas, and calculate their age from the sum of their thicknesses. This I believe has been a frequent source of error in estimating geological time, and it would be easy to give many illustrations of this from other beds.

Thirdly, Mr. Reade supposes the denudation of sedimentary rocks would reduce the mean thickness. This could only be the case if the area of deposition were continually changing its site or increasing its area. It is true that any given sediment *may* be spread over a wider area than the material originally occupied (though this is probably only the case in fluviatile beds), but as a broad fact the area of the land—or denuded surface—is greater than the area of deposition, as we know that all sediment is thrown down near the shore. We must treat this question as a whole, and not take isolated facts. Moreover, we believe the actual area of deposition not only is not increasing, but, viewed on as large a scale geologically as we have just done geographically, remains practically the same. Hence every ounce of freshly denuded igneous rock swells the actual thickness, and no amount of redistribution can reduce it, as Mr. Reade seems to think.

Supposing, lastly, that Mr. Wallace's calculations were all wrong, and Mr. Reade's curious figures (such as $\frac{12}{9326} = 777$) all right, it does not touch the main point at issue, namely, the question of the permanency of oceanic areas. I have not yet seen a single fact that tells against this view.

SYDNEY B. J. SKERTCHLY.

THE OLIGOCENE STRATA OF THE HAMPSHIRE BASIN.

SIR,—Your correspondent, Mr. Henry Keeping, is quite in error in supposing that in any remarks made at the Geological Society I had any desire to question the general excellence of his memory. The principle on which I did insist—and it is one which I am sure will command the assent of all geologists—is this, that when we have the observations of competent investigators carefully recorded on the spot, these ought not to be lightly set aside in favour of other observations, quoted from memory only, after an interval of thirty years. Under similar conditions, I should be quite as ready to distrust my own memory as I am that of your correspondent.

The case in question stands as follows:—Webster and Lyell, in their accounts of Hordwell Cliff, did not notice the so-called "Upper Marine Band." It appears to have been first discovered by the late Mr. F. Edwards, about the year 1840. In 1846 the late Mr. Searles Wood, who worked in conjunction with its discoverer, gave a full description of the bed and described it as being clearly underlaid and overlaid by freshwater strata. Dr. Wright, who described the section in 1851, and the late Marchioness of Hastings, who published her final account in 1853, independently studied the section, and both of them assert that the marine bed was covered with freshwater strata, the thickness and succession of which they minutely describe.

Now both the last-mentioned authors state that they employed your correspondent to assist them in exposing and measuring the