CORRESPONDENCE.

CONTINENT FORMATION.

SIR,—Having just returned from my usual summer vacation trip to the mountains, I have only now seen the June Number of your MAGAZINE containing Mr. Crosby's article on "Continent Formation," in which he criticizes the views of Prof. Dana and myself on this subject. No one can be more aware than myself of the extreme uncertainty of any views yet proposed on this most difficult subject. So far from objecting to such criticisms, therefore, I hail with pleasure anything whether in the way of advancement of new, or the criticism of old, views. My object in this communication is merely to correct what I conceive to be some misunderstandings.

1. Mr. Crosby (p. 242) says, "According to Prof. Dana's theory, the continents during the *course of geological times* have become higher and broader, and the oceans deeper and narrower. But *just the reverse is an unavoidable deduction from Prof. Le Conte's theory;* for as the refrigeration of the earth continues, the contraction along the longer or continental radii must, sooner or later, begin to gain on that of the shorter or oceanic radii, and from that moment the continents begin to subside beneath the surface of a universal ocean." The italics are my own.

I have two objections to make to this. (*a*) It is by no means "an unavoidable deduction," *unless*, while the conductivity on different sides were different, *the coefficient of contraction were the same.* But this is extremely improbable. (*b*) But even supposing the coefficient of contraction were the same, and that therefore "sooner or later" the inequalities would begin to grow less, it is quite evident that *that time has not yet arrived*, and probably will not while the earth is habitable. It is evident that the inequalities would increase to a limit which is yet far from being reached; for with the exception of a very thin crust, the mass of the earth is still incandescently hot.

2. Again, Mr. Crosby says (p. 243), "These theories rest at outset on an assumption which is not supported by a vestige of evidence, viz. that the earth was originally, and is now, of unlike composition along different radii or on different sides." "Where are the facts supporting it? Where are the analyses showing essential difference between continents and ocean-bottoms?"

In answer to this objection, I would simply say that Mr. Crosby overlooks the enormous size of the contracting body, and the comparative minuteness of the deformation by contraction. The *average* difference between continental and oceanic radii is only three miles,[1] or $\frac{1}{1300}$ of the whole. In a globe of 2 ft. in diameter this would be less than $\frac{1}{100}$ of an inch—a difference too small to be perceived. Now I am quite sure that a ball of clay 2 ft. in diameter, turned to a true sphere *while in a wet condition* and allowed to dry, would deform by contraction more than this. I think that even a ball of

[1] Extreme elevations are due to mountain-making, not continent-making causes.

https://doi.org/10.1017/S0016756800167135 Published online by Cambridge University Press

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