I.

Further Observations on the Permanence of Oceans and Continents.

I.

THE literature relating to the much-debated permanence of ocean basins increases fast, and two fresh contributions have lately been made to it. In the *American Journal of Science* for September (vol. xliv., p. 183), Mr. McGee writes on "The Gulf of Mexico as a Measure of Isostasy"; while in the *Quarterly Journal of the Geological Society* for November (vol. xlviii., p. 519) Mr. R. J. Lechmere Guppy treats of "The Tertiary Microzoic Formations of Trinidad, West Indies." The authors, however, approach the subject from totally different points of view, and it may be of interest to compare their results. The subject is an interesting one, but the unbiased geologist will be much perplexed with the conflicting opinions expressed by the various authorities.

Mr. McGee starts on the assumption that the earth's surface is in a state of nearly stable equilibrium—hydrostatic equilibrium he calls it, but we do not like the term to be applied to so imperfect a fluid as the earth's crust. This equilibrium is indicated by the fact that the crust is densest under the sea and lightest under mountain ranges; therefore, as long as there is no transfer of matter from place to place the dense sea-bottom will balance the lighter mountains. Rivers, however, gradually transfer the land to the bottom of the sea; consequently the loaded sea-bottom will continually subside, perhaps carrying with it the adjacent flat shore, while the unloaded continental areas will slowly rise.

As an abstract proposition no fault can be found with this argument; but no quantitative measurements of the earth's density sufficient to prove any near approach to a hydrostatic equilibrium have yet been made. When, also, Mr. McGee points to estuary after estuary as showing recent subsidence, we cannot help observing that he does not always clearly distinguish between loss of land from marine erosion and true subsidence. He also makes no allowance for the slow compression that estuarine deposits are always undergoing, as the organic matter decays and the particles pack closer. The historical evidence for the post-Roman subsidence in Holland is

scarcely satisfactory, and sand-dunes do not necessarily indicate subsidence. So much of Mr. McGee's paper is taken up with the attempt to strengthen his case by the appeal to evidence of doubtful value, that we cannot ignore it; the Gulf of Mexico, however, is the region with which he is primarily concerned. To that area we will now turn.

The Gulf of Mexico is a nearly land-locked sea, which receives the detritus brought by the Mississippi and other rivers and derived from the erosion of about half the North American continent. This detritus, however, is nearly all deposited in the northern half of the Gulf, so the area of degradation is about 6½ times the area of deposition. If the theory be correct, that the rise or fall of an area depends on the weight removed or piled upon it, then the continent of North America must be a rising area, and the northern half of the Gulf of Mexico a subsiding one.

Such is probably the case, but there is some danger of confusion between cause and effect. It is quite as logical to argue that the land is denuded because it rises, as that it rises because it is denuded. We may also observe that if from any cause the land sinks beneath the sea-level, the part that subsides most rapidly necessarily becomes first a bay and then probably an estuary, for river-valleys will tend also to follow lines of depression. Suppose, on the other hand, that the floor of the Gulf of Mexico were to rise; the Mississippi would discharge further to the east, beyond the West Indies, and we should be told that the depression beyond was caused by the accumulation of sediment. Unless the theory of stable equilibrium be of merely local application, it is difficult to understand how land once beneath the sea-level can ever rise again; yet we know that extensive marine deposits often occur high above the present sea-level.

Mr. Guppy, writing of a district similar in many respects to that dealt with by Mr. McGee, treats the subject from a point of view altogether different. He is concerned mainly with the geological and biological evidence, which go to indicate that what is now dry land was once a deep sea, and the facts relied on are these:—Trinidad possesses in its southern half certain Tertiary formations, which from the character of their fauna seem to indicate a depth of at least 1,000 fathoms. These deep-water deposits are absent from surrounding areas; and from their partial occurrence, combined with the evidence obtained from the geology of the surrounding islands and coast of South America, Mr. Guppy arrives at these conclusions. During the Cretaceous and Eocene periods there was a sea having a depth of 1,000 fathoms or more in the area now occupied by the microzoic rocks of Trinidad. During the same period the northern mountains formed probably an unbroken range with the littoral Cordillera of Venezuela, and constituted the southern boundary of a Caribbean Continent. These mountains may be regarded as one of those "stable areas" which have never been submerged since Palæozoic

times. The Orinoco was then in existence, but brought no appreciable sediment so far from what was then the mouth of the river.

The Orinoco seems, therefore, to have discharged its sediment into a land-locked sea greatly resembling the existing Gulf of Mexico. If Mr. Guppy's account of the geology be correct, the estuary of the Orinoco and the Gulf of Paria ought, on the theory adopted by Mr. McGee, to have formed a subsiding area ever since early Tertiary times, for it is over that area that the sediment has been deposited. Such may have been the case, though we have as yet no direct evidence of the fact. When, however, we read that the southern half of Trinidad is occupied by deposits formed at the great depth of 1,000 fathoms, while the northern half has never been submerged since Palæozoic times, it is not easy to reconcile this rise of the sea-bottom with Mr. McGee's theory.

The views of Messrs. McGee and Guppy, as we remarked, have only quite lately been propounded. If we refer to literature a little earlier, however, there are numerous papers by other authors with a direct bearing on the same subject; and we may allude especially to four papers by Professor James D. Dana,[1] in which the permanence of the continental area of North America since the beginning of Palæozoic times, at least, seems to be satisfactorily proved. "The facts illustrate strikingly the great truth that the earth's features, even to many minor details, were defined in Archæan time, and, consequently, that Archæan conditions exercised a special and even detailed control over future continental growth. The extension of North America to the most eastern point of Newfoundland, and beyond it, was determined in this beginning time; and likewise, that of the European Continent to the Hebrides, in front of the Scandinavian Archæan area." The main result is briefly summed up by Professor Dana in the following letter, while Dr. Blanford and Mr. Jukes-Browne add some further comments from their point of view.

II.

WITH regard to the discussion on the Permanence of Oceans and Continents now proceeding in NATURAL SCIENCE, I may say that the argument from North America is brief: that the successive formations, from the Archæan onward, follow one another quite regularly over the surface that lies outside of the northern Archæan area, and bear

[1] "Areas of Continental Progress in North America, and the Influence of the Conditions of these Areas on the work carried forward within them," *Bull. Geol. Soc. America*, vol. i., 1889, pp. 36–48.

"Archæan Axes of Eastern North America," *Amer. Journ. Sci.*, vol. xxxix., pp. 378–383, 1890.

"Rocky Mountain Protaxis and the Post-Cretaceous Mountain-making along its Course," *ibid.*, vol. xl., pp. 181–196, 1890.

"On Subdivisions in Archæan History," *ibid.*, vol. xliii., pp. 455–462, 1892.

evidence throughout of shallow water origin; and that in this way the continent was extended from the Archæan southward, south-eastward, and south-westward.

The evidence as far as the eastern half of the continent is concerned was clearly manfest before 1845 (my first paper on the origin of continents was published in 1846 in the *American Journ. Sci.*, 2nd ser., vol. ii.), by which time the States of Connecticut, Massachusetts and New York had been geologically surveyed, and their final reports published, including a general coloured geological chart of the country by James Hall; and, besides, incomplete surveys had also been made of some other States. Further, the Professors W. B. and H. D. Rogers had so far completed the surveys of Pennsylvania and Virginia that they brought out in 1842 (*Trans. Assoc. Amer. Geol. and Naturalists*) their admirable paper on the Appalachian Mountain system, their flexures, &c. They showed that the flexing, faulting and uplifting for the 1,000 miles of their length was a sequel to the long series of Palæozoic deposition, a folding of the strata from the bottom (the Cambrian, as now called) to the top, confirming all other evidence that the strata were continental formations, and the work, regular continental growth.

Afterwards the earlier Mesozoic (that is, the Jura-Trias), added only brackish water or fresh-water beds, along some portions of the Atlantic border; and later the Cretaceous and Tertiary followed, finishing the continent to its eastern limit.

Similar facts complete the argument for continental permanence as regards the western half. By permanence, as I have always said, is meant, not the absence of deep subsidences, or of deep continental waters at times—say 2,000 or 3,000 feet in the interior, or on the borders, but the non-occurrence of oceanic depths and alternations with oceanic conditions.

JAMES D. DANA.

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