

*Thirdly*, What seems to us the most serious objection to the theory, remains to be stated. On the outside of coral reefs very highly inclined, no bottom is sometimes found with a line of 2000 or 3000 feet, and this is by no means a rare case. It follows that the reef ought to have this thickness; and Mr Darwin's diagrams, pages 48 and 98, shew that he understood it so. Now, if such masses of coral exist under the sea, they ought somewhere to be found on *terra firma*; for there is evidence that all the lands yet visited by geologists have been at one time submerged. But neither in the great volcanic chain, extending from Sumatra to Japan, nor in the West Indies, nor in any other region yet explored, has a bed or formation of coral, even 500 feet thick, been discovered, so far as we know. We state this objection, not as conclusive against the theory, but as one deserving the able and ingenious author's consideration.

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*Remarks on the preceding paper, in a Letter from CHARLES DARWIN, Esq., to Mr MACLAREN.*

*Down near Broomley, Kent.*

Dear Sir,—I have been so much pleased with the very clear, and, at the same time, in many points quite original manner in which you have stated and explained my views, that I cannot refrain from troubling you with my thanks. Your third objection appears to me much the most, indeed the only, formidable one, which has hitherto occurred to me. I fear I shall be tempted to reply to it at great length, but perhaps sometime you will find leisure to read my attempted vindication. With respect to the first objection, I can hardly admit that we know enough of the laws of elevation and subsidence to argue against the theory, because the areas of different movements are not more distinct. Some have been startled at my view on directly the reverse grounds to your objection, viz. that, according to their notions of probability, the areas of the same movements were too large and uniform. With respect to your second objection, all those who believe that exceedingly slow and gradual elevations are the order of nature, must admit a great amount of contemporaneous denudation, which would tend to annihilate the characteristic form of the fringing-reefs during their upheaval, and leave merely a coating on the upraised land of coral-rock either thicker or thinner, according to the original thickness, rate of growth of the reef at each successive level, and the rate of elevation; indeed I am surprised that there exists even one case, viz. at Mauritius, where the peculiar moat-like structure of a mere fringing-reef has been partially preserved on dry land.

Your third criticism strikes me as a very weighty and perplexing one.

It had passed through my head, but I had not considered it with nearly the attention it deserved, otherwise I assuredly would have noticed it in my volume. I had always intended to examine the limestone formations of England for comparison, but was prevented by bad health ; I was, however, led away from the subject, and baffled when I consulted published accounts, for the limestones all appeared to be uniformly spread out, and most, if not all of them, to be associated with layers of earthy matter, whereas a formation of the nature of a group of atolls, would consist of separate large patches of calcareous rock, which would be quite pure.—I was thus led from the subject, and did not reflect on their want of thickness. The want of thickness, however, in any limestone formation, until it be first shewn to be analogous in structure, form, and composition, to a barrier-reef, an atoll or group of atolls, evidently cannot be brought forward as any argument against the theory of the long-continued subsidence of reefs of these classes. During the elevation of all reefs in open seas, I think there can be no doubt (as is dwelt on at p. 117, 3d. vol.) that a considerable thickness of the exterior would be denuded, and the only parts preserved would be those which had accumulated in lagoons or lagoon-channels ; these would be chiefly sedimentary, and in some cases might contain (p. 117) scarcely any coral ; within barrier-reefs such beds would often be associated with much earthy sediment. Mr Lyell, in a note just received, in which he alludes to your criticisms, speaks of the limestones of the Alps and Pyrenees, as being of enormous thickness, namely, about 4000 feet. I do not know what their composition is, but I have no doubt that the strata now accumulating *within* the barrier-reef of Australia and New Caledonia, are chiefly formed of horizontal layers of calcareous sediment and not of coral.

I suspect that denudation has acted on a far grander scale than in merely peeling the outsides of upraised reefs. My theory leads me to infer that the areas, where groups of atolls and barrier-reefs stand, have subsided to a great amount and over a wide space. Now it appears to me probable, that a subterranean change, producing a directly opposite movement, namely, a great and widely extended elevation, would be extremely slow, and would be interrupted by long periods of rest, and perhaps of oscillation of level. When I think of the denudation along the fault, which goes across the northern carboniferous counties of England, where 1000 feet of strata have been smoothed away ; when I think how commonly volcanic islands, formed of very hard rock, are eaten back in cliffs from 100 or 200 to 800 or 1000 feet in height, I hardly see where we can stop, with respect to the probable limits of erosion on the comparatively soft, generally cavernous, tabular, though wide, masses of coral rock, standing exposed in great oceans during very slow changes of level. Most of the atolls which have been raised a few hundred feet are mere wrecks, and at the Friendly Archipelago where there are upraised atolls, there are large irregular reefs, also, which I have always thought were probably the basal vestiges of worn down atolls. Many submerged reefs, which may have had this same origin, occur outside the line of elevation of the Salomon and New Hebrides archipelagoes. The great steepness of the shores of upraised reefs (p. 65. Ehrenberg quoted, and p. 51.) would probably be unfavourable to the growth of new

reefs, and therefore to the protection afforded by them. I can conceive it very possible, that should, at some period, as far in futurity as the secondary rocks are in the past, the bed of the Pacific, with its atolls and barrier reefs, be raised in reefs, by an elevation of some thousand feet, and be converted into a continent, that scarcely any, or none of the existing reefs would be preserved; but only widely spread beds of calcareous matter derived from their wear and tear. As a corollary from this, I suspect that the reefs of the secondary periods (if any, as is probable, existed), have been ground into sand, and no longer exist. This notion will certainly at first appear preposterous; its only justification lies in the probability of upward movements after long periods of subsidence, being exceedingly slow and often interrupted by pauses of rest, and perhaps of oscillations of land, during all which the soft coral rock would be exposed to the action of waves never at rest.

This notion, preposterous as it will probably appear, would not have occurred to me, had I not several times, from independent reasons, been driven to the conclusion, that a formation to be preserved to a very distant æra (or which probably is the same thing, to be elevated to a great height from its original level *over a wide area*) must be of great extent, and must be covered by a great thickness of superincumbent matter in order to escape the chances of denudation. I have come to this conclusion chiefly from considering the character of the deposits of the long series of formations piled one upon another, in Europe, with evidence of land near many of them. I can explain my meaning more clearly by looking to the future; it scarcely seems probable, judging from what I see of the ancient parts of the crust of the earth, that any of the numerous sub-littoral formations (*i. e.* deposits formed along and near shores, and not of great width or breadth), now accumulating on most parts of the shores of Europe (and indeed of the whole world), although, no doubt, many of them must be of considerable thickness, will be preserved to a period as far in the future, as the lias or chalk are in the past, but that only those deposits of the present day will be preserved which are accumulating *over a wide area, and which shall hereafter chance to be protected by successive thick deposits*. I should think that most of the sublittoral deposits of the present day will suffer, what I conclude the sublittoral formations of the secondary æras have generally suffered, namely, denudation. Now, barrier and atoll coral reefs, though, according to my theory, of great thickness, are, in the above sense, not widely extended; and hence I conclude they will suffer, as I suspect ancient coral reefs have suffered—the same fate with sublittoral deposits.

With respect to the vertical amount of subsidence, requisite by my theory to have produced the spaces coloured blue on the map, more facts regarding the average heights of islands and tracts of land are wanted than all those, even if perfectly known, which this one world of ours would afford; for the question of the probable amount, or, which is the same thing, the probable thickness of the coral-reef, resolves itself into this,—What is the ordinary height of tracts of land, or groups of islands

of the size of the existing groups of atolls (excepting as many of the highest islands or mountains in such groups, as there usually occur of "encircled islands" in groups of atolls)? and likewise what is the ordinary height of the single scattered islands between such groups of islands?—subsidence sufficient to bury all these islands (with the above exception) my theory absolutely requires, but no more. In my volume, I rather vaguely concluded that the atolls, which are studded in so marvellous a manner over wide spaces of ocean, marked the spots where the mountains of a great continent lay buried, instead of merely separate tracts of land or mountainous islands; and I was thus led to speak somewhat more strongly than warranted, of the probable vertical amount of subsidence in the areas in question.

Mr Lyell in the note alluded to, thinks we are much too ignorant of intra-tropical geology (and ignorant enough we certainly are) to affirm that calcareous rocks of the supposed thickness of coral reefs, do not occur. I am inclined to lay considerable stress on this. I do not expect the foregoing view will appear at all satisfactory to any one besides myself,—I believe, however, there is more in it than mere special pleading. The case, undoubtedly, is very perplexing; but I have the confidence to think, that the theory explains so well many facts, that I shall hold fast by it, in the face of two or three puzzles, even as good ones as your third objection. Believe me, my Dear Sir, yours very truly,

CHARLES DARWIN.

*Description of an improved Tilting Apparatus for emptying Waggon at the termini of Railways, Shipping-Places, &c., as used at the Magheramorne Lime-Works, Ireland. With a Plate. By JAMES THOMSON, Esq., F.R.S.E., M.R.I.A., F.R.S.S.A., Civil Engineer, Glasgow. Communicated by the Royal Scottish Society of Arts.\**

The apparatus may be generally described as consisting of three parts, viz:—

- 1st, The cast-iron brackets or quadrants for supporting the machine, Plate I. *a a a*.
- 2d, The tilting-frame upon which the waggon is placed, *b b*,—and
- 3d, The malleable iron-swings for suspending the frame to the brackets, *c c*.

The supporting brackets *a a a*, are bolted to the wooden frame *d d*, of a moveable shipping platform, by means of which

\* Read before the Royal Scottish Society of Arts, and working model exhibited, 10th January 1842, and the Society's Honorary Silver Medal awarded, 14th November 1842.