Art. XVIII.—*Dana's Confirmation of Darwin's Theory of Coral Reefs;* by W. M. Davis.

James Dwight Dana, born four years to a day after Darwin on February 12, 1813, naturalist of the United States Exploring Expedition under Wilkes from 1838 to 1842, and afterwards until his death in 1895 professor of geology at Yale University, was for over half a century a leading figure among American men of science. On the hundredth anniversary of his birth, it is fitting to call attention to a contribution that he made many years ago to Darwin's theory of coral reefs, a contribution which has long been overlooked although it supplies the most important confirmatory evidence of the truth of the theory that has ever been brought forward, and at the same time points the way to demonstrate the insufficiency of certain other theories. Dana's contribution consists in his having considered more fully than Darwin did, the changes that must take place in the shore line of the subsiding central island while barrier reefs are developing around it.

Darwin most ingeniously invented the theory of subsidence while he was in South America (in 1835?), before he had seen a true coral reef; he had afterwards only to test the theory by comparing its already deduced consequences with the facts that he observed during the voyage of the "Beagle" across the Pacific and Indian oceans, and with the records of other observant voyagers which he studied carefully after his return to England. The theory bore this test admirably; it explained the things that it was made to explain, and brought into reasonable relationship a large variety of facts that had previously seemed to be arbitrarily distributed. It was therefore regarded as "true" by its inventor; but apart from certain correlations of coral reefs with areas of recent uplift and with active and extinct volcanoes—correlations which appear to be less assured now than seventy years ago—the subsidence theory did not gain that increased probability of correctness which comes from the capacity to explain facts that were unknown or unnoticed at the time of its invention. Nevertheless, for forty years the scientific world accepted it as demonstrated. Darwin's diagrams of a subsiding island and an upgrowing reef have been reproduced over and over again on countless blackboards, as representing one of the great discoveries of geological science.

The introduction of other theories of coral reefs during the last thirty years is familiar matter. Mention will here be made only of a few leading contributions. During the voyage of the "Challenger," Murray saw the reefs of the Fiji islands; he could not explain them by Darwin's theory of subsidence, and he therefore afterwards* replaced that theory by what may be called the theory of outward growth, with its provisos of the organic upbuilding of submarine banks to serve as foundations for atolls, and of the production of lagoons by the removal of the inner part of barrier reefs by solution, no movement of subsidence being included in this theory. Still later, A. Agassiz, in the reports of his world-wide investigation of coral reefs,† emphasized the occurrence of uplifted coralliferous limestones, which might be worn down and dissolved away while new fringing reefs grew around them, thus producing barrier reefs and atolls in association with elevation instead of with subsidence; he suggested further that even volcanic islands might be worn down and obliterated, so that their barrier reefs would survive as atolls. At the same time, he reintroduced the idea of wide sea-cut platforms on the margin of volcanic islands, and the growth of a comparatively thin veneer of coral on the outer edge of the platform, thus producing a barrier reef without subsidence, elevation, or solution. Wharton went farther‡ in suggesting that a volcanic island might be worn down to a depth of 20 or 25 fathoms by marine agencies, thereby producing a submarine bank on which upgrowing corals could form an atoll, and thus, like Agassiz, accounted for atolls without postulating either subsidence, elevation, or solution.

Each of these theories has been more or less favorably received, for each one has the same kind of recommendation as that which sufficed to lead the scientific world to adopt Darwin's theory; that is, each theory explains the group of facts that it was made to explain, provided its postulates are accepted. Such a measure of success is of course commendatory, but it is not sufficient for the establishment of that high degree of probability which is recognized in geological science as demonstration. Even when only a single theory is under consideration, it must evidently do something more than explain the things that it was made to explain, before it deserves unquestioned acceptance. When several theories are advanced to explain a single group of facts and each theory succeeds in explaining what it was made to explain, the need of some independent means of verification is still more manifest. Independent verification of a theory is found usually in one of two ways,

*Proc. Roy. Soc. Edinb., ix, 1880, 505–518.

†Bull. Mus. Comp. Zool., xxxiii, 1899; Mem. Mus. Comp. Zool., xxviii, 1903.

‡Nature, iv, 1897, 390–393.

all the better if it is found in both of these ways. New kinds of facts may be discovered after a theory has been invented; if such facts, as well as the original facts, can be reasonably accounted for by the theory, the probability of its correctness is greatly increased. Or new consequences may be deduced from the theory, thus indicating the occurrence of specified kinds of facts not before noticed; if such facts are then found in their designated positions, the probability of correctness of the theory that thus gives its inventor the power of prediction is immensely increased.

The possibility of the development of new atolls by the wearing down of older uplifted atolls, or of new barrier reefs by the wearing down of uplifted older reefs, is one of the interesting complications of the simple theory of subsidence that Darwin touched upon briefly,* but that Agassiz' abundant observations have emphasized. This possibility, however, in no wise affects the origin of the earlier atoll or reef, whatever that may have been. True, Agassiz doubted or denied that the uplifted masses of "Tertiary coralliferous limestone" which he repeatedly found in the Paumotus were atolls of an earlier date, but no other adequate explanation of the origin of these great truncated conical masses, rising several thousand feet over a submarine plateau, has been suggested. Agassiz likewise denied the barrier-reef origin of certain uplifted coralliferous limestones of the Fiji islands, and thus differed from Gardiner,† who on the grounds of his own observations accepted this interpretation of their formation. If the uplifted limestones of the Paumotus and the Fijis are eventually proved to be atolls and barrier reefs of an earlier period than the present, a general theory that explains them will have all the more merit, in that it will then apply to more remote as well as to recent geological time.

Murray's theory of the formation of atolls by upgrowth from submarine banks of proper depth is eminently possible, if the banks can be provided in sufficient number; but possibility is not proof. The depth of the ocean is not supposed to be changed by subsidence in this theory; hence if subsidence is shown to have taken place in the production of barrier reefs, the theory that accounts for neighboring atolls without subsidence will be rendered less probable.

The development of a foundation for atolls by the truncation of a volcanic island in the manner indicated by Wharton is eminently possible, provided no change of level takes place during the progress of the work; but it is inconceivable that floating coral larvæ should abstain from establishing themselves

* Coral Reefs, 1842, 55, 146.
† Proc. Phil. Soc. Cambridge, ix, 1898, 417–503.

on so good a foundation until the truncation is complete. The ordinary relation of fringing and barrier reefs to their central island suffices to show that the work of marine truncation would be arrested by the growth of reefs, just as soon as the abraded platform became broad enough to afford a foundation for coral growth a moderate distance away from the outwash of fresh water and its detritus; and when such a reef is once established, the further truncation of the island by wave work is practically stopped; the waves of most lagoons are too weak to be effective agents in cutting away the land. Moreover the Alexa bank described by Wharton is as fairly explained by regarding it as a slightly submerged atoll as by taking it for a truncated volcanic island. Hence the theory of truncation, though easily conceivable as to its inorganic elements and received with approval by several writers, involves organic elements which are not admissible. It must be rejected as a means of explaining atolls.

The formation of veneering barrier reefs on the outer margin of submarine platforms cut by the sea around still-standing islands, an old idea* recently given prominence by Agassiz, seems entirely possible, provided that the coral larvæ can be prevented from establishing themselves on the platform soon after its cutting is begun; but the explanation of barrier reefs outside of broad lagoons in this way would involve the same difficulty that is fatal to Wharton's theory. However, if barrier reefs have sometimes been formed in this manner, certain special features should be found in close and constant association with them on the still-standing central island. For after a platform a mile or so in width has been worn by the waves, the central island should rise from the cut-back shore line in a wall of steep cliffs, as Darwin clearly stated,† and the general outline of the cliff-wall should be simplified as compared with the original outline; for marine erosion, after a brief introductory period of increasing irregularity of shore line, tends to decrease the sinuosities of the initial form. If any initial bays existed, due to irregular island-building by volcanic eruption, they should fill with delta plains while the intervening headlands are cut back. The wider the platform becomes, the simpler should be the island outline and the more continuous the cliff wall; but the cliffs should be interrupted here and there by valleys, as long as the area of the island is large enough to maintain streams. After the formation of a barrier reef, deltas should be rapidly built forward from each of these valleys in the quiet and shallow waters of the enclosed lagoon.

Now it may be confidently asserted that the central islands

* See footnote in Darwin's "Coral Reefs," 1842, 49.

† Coral Reefs, 1842, 49.

of barrier reefs, as represented on large-scale charts, are not as a rule characterized by these systematically associated features. The markedly irregular shore line of Kandavu in the Fiji group, with its many headlands and bays, cannot have been produced by marine action either before or after the barrier reef was formed; and the absence of extensive deltas in the bays discredits the idea of a long still-stand of the island during the cutting of the assumed platform around the headlands in the way postulated in this theory. The platform would be most effectively cut during a period of slow subsidence, provided no coral reefs grew up in the way of the waves; but the essence of this theory lies in the exclusion of subsidence. In the typical barrier reef of Bora Bora in the Society group, the shore line of the central dissected volcanic island is of very irregular pattern, such as marine erosion cannot produce; the sprawling ridges descend gently to their extremities in the lagoon, and are not cut off in cliffs; hence this barrier reef cannot be regarded as a veneer on a wave-cut platform. Moreover, the embayments between the sprawling ridges are little filled with delta plains; hence the island cannot be supposed to have stood still during the long and slow development of the barrier reef by outward growth. The value of wave-cut platforms therefore seems to be limited to narrow examples on which fringing reefs can first establish themselves.

Furthermore, if a veneering barrier reef were uplifted and exposed to normal erosion, it would take the form of a more or less dissected terrace; new-cut ravines would disclose its volcanic structure, the upper surface, strewn with coral sand and silt, would transect the rock structure, and its outer edges might retain patches of the veneering reef. Whether terraces of this kind occur or not may be left to observant explorers to determine.

Murray's theory of outward growth and solution, whereby fringing reefs are converted into barrier reefs during a prolonged still-stand of a volcanic island, is also a manifest possibility; but it involves several consequences, easily deducible from the theory but not usually stated with it. For example, if the central island be several miles in diameter and a thousand or more feet in height, its streams will wash down abundant detritus upon the fringing reef. By the time the reef has grown outwards far enough to be called a barrier, the stream-borne detritus will have formed deltas fronting the mouth of each valley; and with farther outgrowth of the reef the deltas will become laterally confluent, so as to form a low alluvial plain around the original shore line of the island; and this original shore line should not exhibit sinuosities of the kind that are produced by subsidence.

Moreover, the structure of the reef should show cross-bedding on a gigantic scale, for the reef advances on a foundation of its own talus; and in the inclined layers of the talus disordered structures might be locally expected, as the result of submarine "landslides," if they may be so called. During the outgrowth of the reef its superficial parts will be more or less dissolved away in the excavation of the lagoon; hence if such a reef is uplifted, so as to form a terrace around its mountain center, its stratification will be inclined at a significantly steeper angle than the slope of the foundation on which it is built; while in the case of a reef formed during subsidence a large part of the reef may consist of horizontal strata.

The first of these deduced consequences of the theory of outward growth is rarely if ever confirmed by the facts. Existing barrier reefs are not as a rule associated with alluvial plains around the inner border of their lagoons. Delta-plains of moderate extent often occupy the bay heads, but the very form of the bays upon which they encroach shows that subsidence has taken place; and the small area of many delta-plains indicates that the subsidence has occurred during the formation of the barrier reef. Hence the theory of outward growth during a still-stand of a central island is not acceptable as a means of accounting for reefs of this class. The theory cannot be accepted as explaining actual barrier reefs, unless by way of exception, however reasonable it is and however easily it may be conceived.

The second deduced consequence of the theory of outward growth appears to be seldom supported by the facts, for the strata of uplifted reefs, as far as I have read, are usually horizontal. This consequence may perhaps be supported in a few cases, where uplifted reefs are described as possessing inclined strata; but the descriptions of these cases do not suffice to distinguish reefs of outward growth on a still-standing foundation, where the whole reef should consist of inclined strata, from reefs of upward growth during subsidence, where only the peripheral strata should be inclined and the rest horizontal. In several articles in which uplifted reefs are described, the attitude of their stratification is not mentioned; perhaps because it could not be recognized; perhaps because districts of recent elevation have not often in the past offered favorable conditions for the development of thick reefs in which, after elevation, the lines of structure could be easily detected. But the real difficulty here seems to be rather that the observers of uplifted reefs did not have the alternative theories of reef formation and their contrasted consequences clearly in mind; and the records made by such observers are often wanting in critical value. For example, on Eua island of the Tonga group, Lister has described a limestone terrace which he regarded as an uplifted barrier reef; his text is silent as to the attitude of its strata, but his sections show them horizontal. Gardiner describes* the interbedding of washed volcanic detritus with horizontal limestones in an uplifted reef on Viti Levu of the Fiji group, but the context does not make clear the relation in which these alternating strata stand to the foundation on or against which they were deposited. Andrews describes† with considerable detail some of the uplifted limestones on certain members of the Fiji group, but he often fails to mention the attitude of the limestone and "soapstone" layers in masses which he describes as stratified, and his cross-sections cannot be safely utilized because it is impossible for the reader to separate observed from inferred structures.‡ This brief review would seem to indicate that, for the present, no safe decision can be made for or against the theory of outward growth on the evidence of uplifted reefs, because their structure is too imperfectly known.

Two other points deserve mention in this connection. First, the theory of outward growth is not an alternative that excludes the theory of upward growth; the two processes can go on together or alternately. Second, Darwin fully recognized this possibility: he accepted outward growth without subsidence for a reef three miles wide, in which the ridges earliest formed at the back of the reef stand at the same height as those last formed in the front;§ in another case he keenly decides in favor of a stationary period after subsidence because of "the broad belt of low land at the foot of the mountains" (128), while in a third he concludes in favor of upward growth during subsidence, because of "the small quantity of low alluvial land at the foot of the mountains" (128). He concluded that "subsidence supervening after long intervals of rest . . . probably is the ordinary coarse of events" (130); also, that a reef "could not increase outwards, without a nearly equal addition to every part of the slope . . . and this would require a large amount of sediment" (74).

It would thus appear that the outgrowth theory of Murray and the truncation theory of Wharton and Agassiz, when tested by certain consequences that have not been explicitly stated by their inventors, fail to satisfy the requirements of observation. They must, therefore, be either modified or discarded. It remains to be seen whether Darwin's theory of subsidence must suffer the same fate when tested in the same manner.

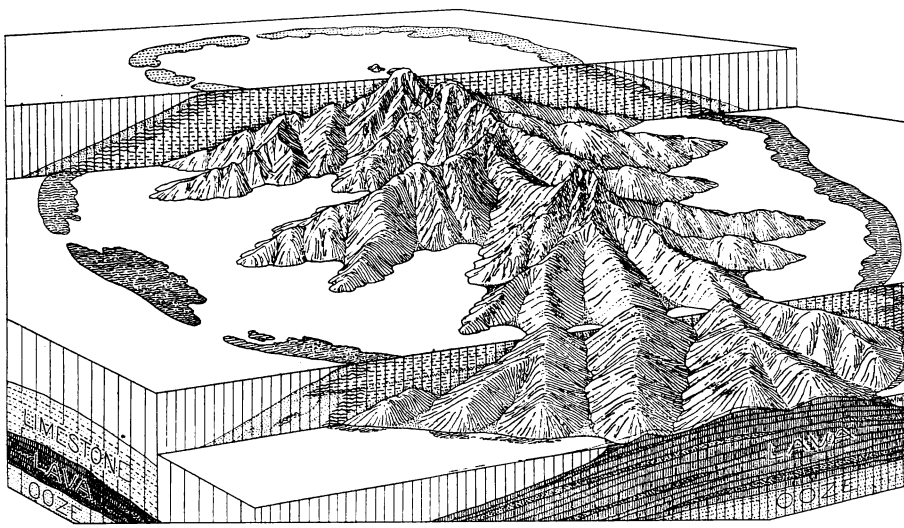
* Quart. Journ. Geol. Soc., xlvii, 1891, 500–607; see p. 600.
† Proc. Camb. Phil. Soc., ix, 1898, 417, 503; see p. 454.
‡ Bull. Mus. Comp. Zool., xxxviii, 1900, 1–50.
§ Coral Reefs, 1842, 74, 75.

The diagrams by which Darwin illustrated the consequences of his theory are simple transverse sections, and as such they do not sufficiently represent the deductive side of the problem. If they be transformed into block diagrams, in which surface and section are both exhibited, their value in this respect is increased; for, as Passarge has pointed out, block diagrams show what an author thinks rather than what he sees; indeed, it may be claimed for them that they help their author to think, even compel him to think. Therein lies half their value; while the other half lies in the ease with which the reader can, with their aid, understand the thought of the author.

The accompanying block diagram exhibits three stages in the subsidence of a dissected volcanic island: for convenience of drawing, the island is represented as standing still while the ocean rises. In the first stage, a discontinuous fringing reef has been formed; its relation to the island and to sea level is shown in section on the front face of the first block. The second block shows a later stage; the size of the island has now been much diminished by subsidence, and the volume of the reef has been correspondingly increased in its transformation from fringing into barrier position; the inferred horizontal and slanting structure of the reef, with abundant volcanic detritus in the landward part of its horizontal strata, is shown in the front face of the second block: an assumed but unessential pause in subsidence is there indicated by a temporary turn in the reef from upward to outward growth; at the level thus defined, a thin wedge of far-advancing delta deposits is intercalated in all the embayments between the earlier and later calcareous strata of the lagoon, and below this level the thickness of the outer slanting layers is increased. The proportion of inner or lagoon strata, lying horizontal, to the outer talus deposits, lying at a strong angle, may be varied within reasonable limits. For example, in the front face of the third block, where the central island has nearly disappeared, the up-growing reef is represented as of decreasing perimeter, with correspondingly small external talus, thus following Darwin's original diagram. But the most significant feature is yet to be mentioned, although the diagram has probably already suggested it. Darwin recognized the diminishing size and final disappearance of the subsiding island as an essential consequence of his theory: but another equally essential consequence, of which no mention appears in his writings, is the transformation of the relatively simple shore line of the initial island into an indented shore line, as the sea enters the valleys between the ridges of the dissected and subsiding island-mass. As long as subsidence continues, no large deltas can be built forward in the bays by the decreasing streams, no strong cliffs can be cut in the ridge-ends by the relatively quiet lagoon waters. Here are several consequences of the theory of subsidence which were entirely unsuspected by its inventor. If they are matched by the facts, the theory from which consequences so significant and so specific can be deduced will gain a greatly increased probability of correctness.

It requires only a brief examination of large-scale charts of the Pacific island-groups to discover that the central islands within barrier reefs are repeatedly characterized by irregularly

Fig. 1.



embayed shore lines, that the bays seldom contain extensive deltas, and that the ridge-ends are not strongly cut off in cliffs. Kandavu and Mbengha in the Fiji group (Admiralty chart 167), the western members of the Society group (chart 1060) and especially Bora Bora (chart 1428), Gambier island, a western member of the Paumotu group (chart 1112), and Rossel island (chart 1473) in the Louisiade archipelago of British New Guinea, may be instanced among many others as affording good illustration of at least some of these significant features, particularly of embayed shore lines. Furthermore, various uplifted reefs already mentioned are described as having horizontal structure; and Gardiner even mentions, as above stated, beds of volcanic detritus interbedded with the coralliferous limestones in one of the Fiji elevated reefs.

Now, in view of the remarkable accordance that is thus found between several peculiar consequences of the theory of subsidence and several features of the central islands in barrier reefs, and in memory of the failure of certain consequences of the theories of outward growth and of sea-cut platforms to match observed facts, an open-minded inquirer cannot hesitate in making choice among the several explanations that have been suggested for barrier reefs, and with them of atolls. We cannot now, as Guppy did,* "pass over the theory of subsidence because the more recent facts concerning the ocean depths and the regions of living and upraised reefs compel us to regard it as no longer necessary." In view of the peculiar features of the central islands of barrier reefs, the theory of subsidence is to-day as valid as it ever was. Indeed, after one has fully appreciated the value that attaches to a successful confrontation of unexpectedly deduced consequences with previously unobserved facts in the verification of a theory from which such consequences have been derived, it is difficult to refrain from giving full acceptance to Darwin's theory, notwithstanding the doubts that have been raised against its sufficiency by more recent investigators. But nothing is gained by over-hasty confidence in a good case. Before one adopts a final conclusion as to the origin of certain reefs, the evidently desirable thing would be to have the whole problem examined again in the Pacific by an investigator who should impartially bear in mind all the theories and all their consequences.

In the meantime, however, I desire to emphasize the point that it is to Dana that we owe the discriminating test above mentioned, which may be applied to the several theories that have been proposed to account for coral reefs, particularly for barrier reefs; for it was Dana who first directed careful attention to the features that ought to be expected on a subsiding central island as a means of testing the theory of subsidence, and hence o other theories also.

Dana first learned of Darwin's theory when the Wilkes Expedition reached Sydney near the end of 1839. It was several months earlier, during "the ascent of Mt. Aorai on Tahiti, in September of 1839," that he first conceived the production of an embayed shore line as a necessary result of the subsidence of a dissected land-mass. "Sunk to any level above that of five hundred feet, the erosion-made valleys of Tahiti would become deep bays, and above that of one thousand feet, fiord-like bays, with the ridges spreading in the water like spider's legs." It is important to notice that this principle, simple as

* Victoria Inst., 1888, 1–16; see p. 6.

it is, had no place in geology or geography at the time of Dana's voyage. He appears to have been the first fully to recognize, and he was surely the first to state with emphasis, the simple origin of the alternating bays and headlands in an irregular shore line by the partial submergence of a dissected land-mass. He may have been preceded a few years in the general recognition of the idea by De la Beche, who in his "Researches in Theoretical Geology" (London, 1834) states that, on the island of Corsica, "the west coast plunges suddenly into the sea, and the valleys are continued beneath it, presenting numerous inlets where the level of the sea meets the inclined bottom of the valley" (p. 193): De la Beche's attention was, however, given chiefly to the submarine part of the valleys, and only incidentally to the associated outline of the coast. No one, I believe, has hitherto quoted the above sentence in this connection: in Penck's thorough review of coastal forms in his "Morphologie der Erdoberfläche" (Stuttgart, 1894), reference is made to De la Beche's discussion of submerged valleys, but Dana is named as the first to recognize the origin of bays by submergence (582).

Dana, on the other hand, explicitly and repeatedly emphasizes the effect of subsidence in producing bays, and as repeatedly adduces indented shore lines in confirmation of Darwin's theory of coral reefs. In his first report he says,* when explaining the consequences of subsidence: "The very features of the land, the deep indentations, are sufficient evidence of subsidence to one who has studied the character of the Pacific islands," and in a footnote he refers to a later chapter where the following explicit statement is made, under the general heading, "Evidence of subsidence," and the special heading, "Deep bay-indentations in coasts as the terminations of valleys": "In the remarks upon the valleys of the Pacific islands, it has been shown that they were in general formed by the waters of the land, unaided by the sea; that the sea tends only to level off the coast, or give it an even outline. When, therefore, we find the several valleys continued on beneath the sea, and their enclosing ridges standing out in long narrow points, there is reason to expect that the island has subsided after the formation of the valleys. For such an island as Tahiti could not subside even a few scores of feet without changing the even outline into one of deep coves or bays, the ridges projecting out to sea on every side. . . . The absence of such coves, on the contrary, is evidence that any subsidence which has taken place has been comparatively small in amount" (393). A similar statement is made in Dana's first

*Geology, U. S. Expl. Exped., 1849, 131.

book on this subject, "On Coral Reefs and Islands,"* and in the several editions of his larger book, "Corals and Coral Islands."†

It is singular how seldom the value of Dana's significant evidence has until recent years been appreciated by other students of coral reefs. It is inconspicuously referred to in the appendix to the third edition of Darwin's "Coral Reefs" (London, 1889), prepared by Bonney, who says in this connection that fiord-like indentations in the rocky coasts of islands are "generally admitted to be one of the strongest evidences of subsidence all the world over" (310–311), but the editor does not draw special attention to this evidence as affording an independent and therefore highly valuable confirmation of Darwin's theory. Dana's statement is noted by Krämer,‡ who gives it local application in explaining certain bays of the Samoan islands, but he fails to recognize its general value in relation to the subsidence theory of coral reefs. It is quoted by Gardiner,§ but without recognition of its importance, for he says: "Such evidence when applied to volcanic islands is, I submit, of very doubtful value." Murray does not mention Dana's evidence; Agassiz rejects it, for he writes: "There are . . . no indications that either the Marquesas or Mehetia has been subjected to the effects of subsidence, as Dana assumes."‖ Dana's statement in this connection was: "The Marquesas are remarkable for their abrupt shores, often inaccessible cliffs, and deep bays. The absence of gentle slopes along the shores, their angular features, abrupt soundings close alongside of the island, and deep indentations, all bear evidence of subsidence to some extent; for their features are very similar to those which Kaui or Tahiti would present, if buried half its height in the sea, leaving only the sharper ridges and peaks out of water."¶ Evidently, then, the importance of Dana's contribution to Darwin's theory has been slow in gaining general recognition. This is probably because an understanding of the reasonable evolution of coastal forms has not yet gained general possession of the scientific mind; or perhaps because some students of the coral reef problem still adhere to the obsolete explanation of bays by marine erosion, an explanation which Dana explicitly excluded; can the delay in application of Dana's principle to the coral reef problem be because there is as yet no sufficiently general understanding of the principle

* New York, 1853, 118–119. † New York, 1872, 319–320, and 1890, 273–274.

‡ Bau der Korallenriffe, Leipzig, 1897, 24.

§ Proc. Camb. Phil. Soc., ix, 1898, 490.

‖ "Coral Reefs of the Tropical Pacific," Mem. Mus. Comp. Zool., 1903, xxviii, 5.

¶ "Coral Reefs and Islands," New York, 1853, 122; "Corals and Coral Islands," New York, 1872, 325.

that a theory, even if it is well recommended by explaining the things it was invented to explain, still needs confirmation by independent and unexpected evidence before it deserves to be accepted as "demonstrated"?

But the most curious feature in all this discussion is that Darwin himself did not announce in the second edition of his "Coral Reefs," Dana's confirmatory evidence of the theory of subsidence that had been published in the first edition of this famous book. Darwin was so open-minded, so fair-minded, so scrupulously careful in considering all sides of a problem, that his failure to bring forward Dana's evidence can only mean that he did not understand it. This is the more remarkable when one re-reads his "Coral Reefs" attentively, and notes the critical and impartial consideration that he gave, not only to his own theory of subsidence but to various other theories, and the thoroughness with which he deduced their consequences for confrontation with the facts. He examined the volcanic crater theory of atolls and dismissed it because it seemed "improbable that there should have existed as many craters of immense size crowded together beneath the sea, as there are now in some parts atolls. . . Nevertheless," he adds, "if the rim of a crater afforded a basis at the proper depth, I am far from denying that a reef like a perfectly characterized atoll might not be formed; some such, perhaps, now exist."* Agassiz reports reefs of this kind in the eastern archipelago of the Fiji group. Darwin carefully considered the submarine bank theory and recognized a certain value in it; he wrote, if "corals were to grow up from a bank with a level surface some fathoms submerged, having steep sides and being situated in a deep sea, a reef not to be distinguished from an atoll might be formed; I believe some such exist in the West Indies. But a difficulty of the same kind with that affecting the crater theory renders . . . this view inapplicable to the greater number of atolls" (89; also 55). Even the newer information adduced by Murray as to the shower of organic material that falls on the sea floor has not altogether relieved this difficulty. Darwin explicitly, though briefly, considered complicated cases of elevation succeeding subsidence, and of subsidence succeeding elevation (140, 145); he recognizes such complications as interruptions in subsidence: "At the Society archipelago . . . the shoalness of the lagoon channels . . . and the broad belt of low land at the foot of the mountains indicate that, although there must have been great subsidence to have produced the barrier reefs, there has since elapsed a long stationary period" (128). He makes repeated mention of uplifted reefs of various kinds, and considers in some detail

* "Coral Reefs," 1842, 89.

the subsequent degradation of an uplifted fringing reef; the margin of such a reef "would generally during a slow rise of the land be either partially or entirely worn down to that level at which corals could renew their growth on its upper edge. On some parts of the coast-land of Mauritius there are little hillocks of coral rock, which are either the last remnants of a continuous reef, or of low islets formed on it" (55). It is but a step from this case to that of the uplifted and worn-down atolls, which later exploration has brought to light, but the failure to recognize the action of solution made this step difficult: "the supposition . . . that the upraised parts [of atolls] have been worn down by the surf, and thus have escaped observation, is overruled by the considerable depth of the lagoons of all the larger atolls; for this could not have been the case if they had suffered repeated elevation and abrasion" (146). Darwin attentively examined the possibility of the outward growth of reefs from a still-standing foundation, and, although he overlooked the process of solution in forming a lagoon, accepted this process, as above indicated, for certain special cases (52, 75), but rejected it for most barrier reefs, presumably because they show only a "small quantity of low alluvial land at the foot of the mountains" (128; also, 43–45); possibly also because of the extravagantly large amount of coral waste that outward growth demands in the formation of a reef of increasing perimeter in water of great depth, in comparison with the economy of coral waste in a reef, the perimeter of which decreases as the depth of the sea is increased (74, 101). If he did not recognize the possible value of solution in the formation of lagoons, he did explicitly mention the movement of water "thrown over the outer edge" of a reef; "the current thus produced would tend to prevent the channel being filled up with sediment, and might even deepen it under certain circumstances" (54; also 45). He concisely stated the unanswerable objection to the theory of veneering barrier reefs on wave-cut platforms, which had been suggested for the Society Islands even in his time: "It will, perhaps, occur to some that the actual reefs formed of coral are not of great thickness, but that before their first growth the coasts of these encircled islands were deeply eaten into, and a broad but shallow submarine ledge thus left, on the edge of which the coral grew; but if this had been the case, the shore would have been invariably bounded by lofty cliffs, and not have sloped down to the lagoon channel, as it does in many instances" (48, 49).

And yet this most able and impartial investigator, who so logically deduced many consequences from all the theories of coral reefs that had been invented in his time, failed to go one step farther, and to deduce the effect of subsidence in producing an embayed shore line on a dissected island. He must surely have read Dana's paragraph on this point. Indeed, in describing the Marquesas Islands, he says: "Dana infers . . . from their steepness and deeply indented outline, that they have subsided"; * but the general principle here involved did not engage his attention. Even the explicit reference to Gambier Island, a western member of the Paumotu group, in Dana's report† as exhibiting forms that are indicative of subsidence, independent of the occurrence of its well-developed barrier reef, seems entirely to have escaped his notice. How regrettable it is that the great naturalist lost the pleasure of appreciating the value which this evidence possesses in giving independent confirmation to his theory!

In recent years several students of coral islands have, especially in Australasia, given to the evidence afforded by drowned valleys the importance that it deserves. The latest of these is Marshall, of Otago, New Zealand, who writes as follows in his recent essay entitled "Oceania," in the "Handbücher der Regionalen Geologie": "The deep inlets that intersect the coast line of . . . [several islands of the Society group] are clearly due to stream erosion. Prolonged marine action would have shallowed or filled them up or at least would have built up bars of coastal debris across the entrances. The author is therefore strongly of opinion that the absence of cliffs at the termination of the radiating spurs, the presence of deep water in the lagoon, and of far-reaching inlets prove that marine erosion has not had any influence on the form of these islands at the present sea level . . . Finally the deep inlets appear to be drowned stream valleys and their nature strongly supports the belief that the islands have been subjected to an important movement of subsidence."‡ His conclusion is: "The author's observations on the islands of the Cook and Society groups cause him to support Darwin's theory as perfectly full and sufficient to account for the reef phenomena in those islands."§ "Darwin's theory still holds good in the majority if not in all coral areas."‖ This direct testimony from an observer in the antipodes is most welcome. It reaches me through a friend since the writing of this article was begun, and is most apposite in providing precisely that impartial revision of the problem by an open-minded investigator which was pointed out above as desirable before any final conclusion should be adopted. In view of the result of this expert and first-hand investigation, further delay

* Coral Reefs, 2d ed., London, 1874, 201; also, 163.
† Geology, U. S. Explor. Exped., 1849, 131.
‡ Vol. vi, Sec. 2, No. 9, 1911, p. 13.
§ Ibid, p. 7. ‖ Ibid, p. 30.

in reaching a decision seems hardly necessary. The alternative theories of outward growth on still-standing islands, of veneering barrier reefs on sea-cut platforms, and of upward growth on up-built submarine banks, certainly deserve to be considered wherever they can find application. The down-wearing of uplifted reefs and atolls is evidently pertinent in certain cases of complicated history. Every case must, as Agassiz repeatedly insisted, be independently investigated. Nevertheless, it may now be fairly said that the theory of subsidence deserves for a number of well studied examples the acceptance that it long enjoyed, and that it for a time and in part lost. It affords not merely a possible explanation; it exposes a well-supported explanation of many barrier reefs, and hence probably also of many atolls. The postulate of subsidence, on which Darwin's theory is based, is justified and established by the independent evidence brought forward by Dana.