COMMENTS ON "GEOLOGY OF LAU, FIJI."

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ABSTRACT Nine authorities have coöperated in making the recently published report on the geology of eastern Fiji. Among the particularly noteworthy results of their study are: (1) the mid-Miocene age of the emerged Futuna limestone, which is visible in most of the Lau archipelago; (2) the richness of the limestone in dolomite; (3) field evidence forbidding application of the Darwin-Dana subsidence theory in explanation of the bedded, somewhat coralliferous limestone; and (4) evidence that in each island the limestone is a veneer on a volcanic pile, which in each of a number of cases had suffered marine abrasion just before the deposition of the carbonate rock. The present paper briefly discusses: (1) the cause of the dolomitization; (2) the meaning of the fact that the emergence of the many islands in post-Futuna time was only slightly differential—a new argument against the subsidence theory of the living coral reefs; and (3) the thesis that the glacial-control theory of *these* reefs is itself an "antecedent-platform theory," and, in the writer's opinion, the best version of it.

IN 1929 J. E. Hoffmeister, H. S. Ladd, and H. L. Alling reported on a visit made to the famous Falcon Island of the Tonga group; they were impressed by the speed with which this recently emerged, pyroclastic mass was truncated by the ocean breakers, forming a platform suitable for the plantation and upward growth of an atoll reef. Three years later Hoffmeister published his field study of Eua Island, also in the Tonga group. In 1934 Ladd gave the results of his field study of Vitilevu Island in western Fiji. The recently issued "Geology of Lau, Fiji," by Ladd and Hoffmeister is the product of still another field study, the chief aims of which were to "tie the geological history of Tonga to that of western Fiji," and "to obtain data bearing on the coral reef problem." (See references 1 to 4.)

The "Geology of Lau, Fiji" is an excellent example of the value of coöperative research by specialists and also illustrates the principle that experience gained in the field study of one Pacific archipelago gives special power to the same workers who go to wrest the geological secrets from other island groups. The Lau report, while corroborating many conclusions of earlier visitors to these islands, records a wealth of new facts that give food for thought. The details are legion and a full summary is not here attempted; the intention of the present commentator is rather to emphasize a few of the notable products of the field and laboratory study.

One outstanding discovery is that 15 out of the 25 more important islands of the Lau group, spread over an area measuring about 100 miles by 150 miles, show outcrops of bedded (only locally coralliferous) limestone, called the Futuna formation and referred to the mid-Miocene (stage *f* of the East Indian stratigraphic column). Of this limestone 32 chemical analyses were made, the specimens coming from Fulanga, Ongea, Kambara, Katafanga, and Namuka. Every specimen was found to be rich in magnesia and in most cases the rocks can be fairly classed as dolomitic. Since some are unaltered or only slightly altered, "it is difficult to see how the magnesia could be introduced with so little recrystallization. The possibility of original deposition of magnesia with calcium carbonate does not seem to be entirely out of reason" (Sanders and Crickmay, p. 257 of the Lau report).

Is not the last statement an under-statement, somewhat too cautious? It does not mean that rhombs of dolomite are formed directly from sea water, either inside or outside animal or plant bodies; more probable is speedy reaction of initially exsolved aragonite and calcite with the magnesia-rich water, to form the double carbonate. In this sense no student of an old open-ocean dolomite or magnesian limestone should object to the idea of large-scale "original" deposition of magnesium carbonate on the ocean floor.

In the report there seems to be no mention of the reason why the magnesia-rich limestone was precipitated instead of a more purely calcareous sediment. For the "original" deposition of abundant magnesium carbonate on the floor of the open ocean several variables in the physico-chemical conditions can be ruled out—namely, pressure, salinity, and mere duration of exposure of first-stage carbonate to sea water. The one variable to be suspected as a major control is temperature. In a survey of the dolomite problem, Blackwelder found evidence for this conclusion. It is supported by the experiments of Leitmeier, Walker and Gerrie, and others, and by (unpublished) experiments of the present writer with alkalized sea water kept for 10 weeks at blood temperature. In consonance is Clarke's discovery that the content of magnesium carbonate in skeletons and shells of many marine organisms increases with increase of temperature for the water in which they live (6 to 8).

A question is at hand: was the tropical ocean warmer in Futuna (mid-Miocene) time than it has been normally since, say, Jurassic time? No one can answer that query with assurance, but it is worth noting that the Miocene limestone of Christmas Island in the Indian Ocean and even Miocene limestone as far north as Japan are rich in magnesia, if not definitely dolomitic. Speculation can go further and pose two other questions: (1) Can this particular stage in Miocene oceanography be correlated with the celebrated Miocene stage of extra-warm climate in Greenland; (2) is the dolomitic limestone beginning at the depth of 640 feet in the outer part of the Funafuti atoll of Futuna age? Is the assignment of such a date for the deposition of this material really disproved by the fossil content of the Funafuti column?

In their summary of the geological history of Lau, Ladd and Hoffmeister make clear that some long-standing views concerning the group need correction. They write: "Almost without exception, the small subcircular islands in Lau have well-developed basin shapes, and many of the islands that are partly formed of volcanic rock have such basin shapes. Early investigators were impressed by these striking basins and interpreted them as elevated atolls. We have shown that in Fulanga, Namuka, and, possibly, Kambara and Ongea these interpretations appear to be correct. Detailed examinations of the limestones of these islands offer some support to the atoll interpretation. The basin shapes of these islands are probably due primarily to organic growth, subsequently modified by solution and other erosive processes." On the other hand, other islands do not "show clear evidence that they were once atolls, hence their basin shapes cannot be directly attributed to organic growth" (p. 168). The basins in islands of this second category are explained chiefly by subaerial solution, after emergence, an idea shared by Gardiner in the case of Fulanga.

Ladd and Hoffmeister "believe that Lau's reefs, both Recent and elevated, can be most satisfactorily explained without any form of Darwinian subsidence" (p. 173). They add: "No thick elevated reefs such as are demanded by the subsidence theory could be located, though we carefully examined islands from which such reefs had been reported by earlier workers. Since the Lau limestones were formed their history has been mainly one of elevation and erosion; wave cut terraces, leeward erosion remnants, and veneering reefs support this interpretation" (p. 174). Their argument against an application of the Darwin-Dana theory to the reefs living in eastern Fiji is strengthened by a deduction that emerges from the field facts recorded in their report. The mid-Miocene Futuna limestone rests on piles of volcanic rock. At its base the limestone carries pebbles and boulders torn out of the volcanics. The contact of the two formations is visible in most of the islands; a good number of examples are listed in the following table, which names islands surrounded by living barrier reefs, gives the widths of the corresponding lagoons, and marks with the letter X those islands where the contact of volcanics and limestone is visible.

Island	*Width of lagoon (miles)*
Exploring Isles	
Vanua Mbalavu (X)	21
Malatta (X)	21
Susui (X)	21
Thikombia (X)	21
Avea (X)	21
Mango (X)	½
Tuvutha (X)	½
Katafanga (X)	3
Oneata (X)	5
Lakemba (X)	5
Ongea	2
Yangasa	8
Namuka	1
Mothe and Karoni (X)	1

In every case the surface of contact is close to the existing sealevel. Evidently, then, any subsidence of these well separated and individualized masses of rock, if the subsidence were real, could not have been markedly differential. But the subsidence theory demands widely different degrees of sinking in the Lau archipelago. For, according to this theory, the width of atoll or barrier lagoon, located on a volcanic pile, should in general vary directly with the amount of sinking of each of these more or less conical piles. The second column of the table shows the width of the lagoons to range from ½ mile to more than 20 miles. This failure of the subsidence theory to meet the facts in an area 150 miles long and scores of miles wide is particularly serious, when the theory is applied to the young, living reefs.

Of course that evidence does not mean that some atolls and barrier reefs were not formed by local or regional subsidence in pre-Pleistocene time. If, as Ladd and Hoffmeister think, the Miocene limestones of Fulanga and Namuka are atoll formations, the subsidence theory might offer a reasonable explanation of these local thickenings of the Futuna limestone, though even in these instances Suess's idea of a positive eustatic shift of sealevel during the Miocene is still worthy of consideration.

In 1944 Hoffmeister and Ladd advocated in detailed argument "the antecedent-platform theory" of the world's coral reefs, which they have preferred throughout the fifteen years since they visited Falcon Island. This paper may be regarded as a supplement to the Lau report. On page 389 of the paper (9) we read: "The Antecedent-Platform theory holds that any bench or bank—even one not 'smooth'—that is located at a proper depth within the circumequatorial coral-reef zone can be considered a potential coral-reef foundation and that, if ecological conditions permit, a reef could grow up to the surface without any change of ocean-level. This is a general principle that applies to coral reefs of all ages—preglacial, glacial, and postglacial . . . Many of those who postulate an antecedent platform seem to feel that a second requirement—a rising sea-level—is also necessary. The rise in sea-level may be due to an actual rise in ocean-level or to a sinking of the land. It is our belief that such a sea-level change may stimulate reef growth, but we hold that it is not *essential* for the formation of a flourishing barrier or atoll reef."

Ladd and Hoffmeister have in fact made it abundantly clear that the living reefs of eastern Fiji do rest on platforms cut-and-built by the ocean waves, operating on volcanic piles. The cutting and building took place in pre-Futuna time, millions of years before the Pleistocene Glacial period. This region thus illustrates one of the chief postulates of the glacial-control theory, which, however, assumes that the platforms now surmounted by living reefs were cut or built or cut-and-built during exposure to wave-action for long periods included between the Pre-Cambrian and the Pleistocene. Until the reef-building, wave-resisting corals were evolved, the ancient platforms were developed with surfaces well below sealevel. On these platforms local, pre-Pleistocene reefs may have grown up to sealevel, but, as long as the new masses of reef material lay between sealevel and the hundred-fathom isobath, they remained weak structures and, with the death of defending corals on their faces, were liable to complete destruction by the low-level abrasion of the Pleistocene glacial stages. In other words, the low-level abrasion merely smoothed platforms which had been prepared long before the Pleistocene or cut smooth platforms in thoroughly weak rocks—unconsolidated sediments and pyroclastics. This brief re-statement of one aspect of the glacial-control theory is made because of the mistaken ideas of Ladd and Hoffmeister, W. M. Davis, and others concerning the history of the platforms whose recognition is so vital a part of the glacial-control theory.

The Lau report is not quite clear as to the origin of the Fijian platforms. There are repeated references to truncation of the volcanic piles by pre-Futuna wave-action, a process that is practically ineffective at 50 fathoms, a depth which, according to Ladd and Hoffmeister, is not too great for the beginning of growth of barrier or atoll reef. On the other hand, they report cases where pelagic shells and deep-water algal growths built up the platforms to the "zone of coral reef growth," thus adopting one postulate in Murray's explanation of atolls.

Presumably the first of the two methods of final preparation of platforms would apply to many, if not most, of the greater atolls and barrier-reef structures of the world. Such cases represent a fundamental difficulty for the antecedent-platform theory as phrased by Ladd and Hoffmeister. For, long before abrasion could bring the platform surface down to the 50-fathom level or even to the 30-fathom level, coral larvae could settle on that deepening surface of erosion, form defending reefs, and so prevent further lowering of that surface. To have produced a cut-and-fill platform at the depths corresponding to those of the flat-floored lagoons of the great atolls and barriers (including the Exploring Isles example in Lau), there must have been an interruption in the lusty growth of reef corals over most of the tropical zone. Such interruption is provided for by the glacial-control theory, but is not provided for in the statements of the platform idea by Ladd and Hoffmeister, Agassiz, Andrews, Chamberlin, Guppy, or Wharton. To the present writer the glacial-control theory is the only workable, published form of the antecedent-platform theory, and among its merits is its success in accounting for another vital fact—the comparative narrowness, and therefore youth, of nearly all the coral reefs now living in the world.

If some of the Lau volcanic piles were crowned with atolls of mid-Miocene age and at a time when their platform surfaces were near enough to sealevel to permit the rounding of pebbles and boulders—the conclusion of Ladd and Hoffmeister—there must have been failure of vigorous reef-growth just before the beginning of Futuna time. Here, then, we would have to assume a time hiatus in reef growth, a hiatus which had nothing to do with the Pleistocene chilling and muddying of the tropical water as its level slowly fell because of continental glaciation. Perhaps such an interruption of coral growth in the warmer parts of the mid-Miocene ocean might be attributed to the relatively high temperature which has been suggested as a cause for the enrichment of the Futuna limestone in magnesia. Many experiments by Mayor and by Edmondson have proved that, if the water temperature rises to 90° F., many species of reef-building corals are killed (10, 11). That temperature is only about 10° higher than the maximum temperature of surface water in the Lau region. Is it, then, not possible to find in excessive warmth a second reason for a temporary removal of defending reefs in the *warmer* regions of the tropical belt?

A final remark: While the author of the glacial-control theory believes it to be the best general explanation of the living reefs of the world, he would be the last to deny that some local reefs, barrier and atoll, have in past geological time been developed according to the terms of the subsidence theory; or to deny that possibly some platforms for coral reefs have been prepared under conditions different from those visualized by the glacial-control theory.

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