CORRESPONDENCE

PATTERN OF ISLANDS

SIR,—The paper on " the Pattern of Some Pacific Island Chains" (Chubb, 1957) brought the writer letters of appreciation and approval, some of them enthusiastic, from geologists in many countries. It is evident, however, that Mr. Harold T. Stearns (1961) does not approve.

When the writer first sailed among the Pacific Islands in 1924, Daly's theory of glacial control was comparatively new. According to this theory all barrier-reefs and atolls would be based upon level platforms, mainly of volcanic rock, eroded by the sea during the Pleistocene glaciation, when its level stood some 300 feet lower than it does to-day. It involved all the reefs having the same age, and having grown during the same rise in sea-level. All the islands would have had a similar history, the only difference being that some volcanoes had been completely truncated and others not ; the former would support atolls and the latter barrier-reefs.

It was, therefore, expected that there would be a certain sameness about the islands, especially in the development of their reefs and the drowning of their coasts, but there proved to be none. No two islands were alike, even within the same group, and it was evident that each had its own individual history. As stated in the 1957 paper (p. 226) a post-glacial rise in sea-level " would have been everywhere equal, the sea cannot have risen by different amounts around different islands, yet the degree of drowning of the coasts and the stage in the development of reefs varies from island to island within any one chain. The only explanation of this fact is that each island subsided independently . . ."

Mr. Stearns (1961, p. 170) cites this passage and adds that he " has never found any fact to substantiate Mr. Chubb's conclusion." His meaning is not clear. The conclusion that each island subsided independently is based on the facts quoted in the previous sentence. Does he dispute these facts ? Does he believe that a post-glacial rise in sea-level would not be everywhere equal ? Does he deny that the drowning of river-valleys and the growth of reefs varies from island to island ? Does he claim, for example, that the development of reefs and coast-lines is the same in the atoll, Fenua Ura, the dissected and embayed volcanic island, Bora Bora, with its broad lagoon and barrier-reef, and the recently extinct, undissected, unembayed and slightly uplifted volcano, Mehetia, with its narrow fringing-reef, all of them islands in the Society group ? Anyone can verify these facts for himself by a brief study of a map.

Part of our disagreement is due to a different use of words. A statement in the 1957 paper to the effect that the original form of a volcanic island was lost meant merely that the island was so deeply dissected that none of its original surface was preserved, and it no longer showed any trace of its original conical form. It did not imply that an experienced geologist could not locate the crater and reconstruct the whole volcano in imagination with reasonable accuracy.

Readers of that paper may have noticed that there was a weak link in the chain of reasoning. The main purpose was to show that volcanic activity began at one end of an island chain, and moved progressively along it, erecting a series of volcanoes, each of which passed through a succession of stages, involving dissection, subsidence, and the growth of a barrier-reef, ending as an atoll. Thus each island was in a different evolutionary stage, according to its age and position in the chain. This theory required atolls at one end of an island chain, active or recently extinct volcanoes, undissected and unembayed, at the other end, and dissected volcanic islands in between, whose embayed river-valleys indicated subsidence. Unfortunately for the argument it appeared that in the Hawaiian and Samoan chains the valleys in the intermediate islands were not embayed.

Stearns, however, tells us that Pearl Harbour in Oahu, Hawaiian Islands, is due to the drowning of river-valleys, also that " all the islands, where not

veneered with recent lava, have deeply drowned river-valleys . . . Logs of wells at the mouths of these valleys indicate that the subsidence amounts to more than 1,200 feet." This is splendid news indeed ; the weak link in the chain is removed and a strong link put in its place. And it may be noted that the islands " veneered with recent lava ", i.e. the active or recently active volcanoes at the eastern end, do not have " drowned river valleys." Stearns also tells us that the harbour of Pago Pago in the Samoan group is a drowned river valley. So the Hawaiian and Samoan chains agree much more closely with the ideal pattern than had appeared.

But Stearns seems to believe that, by showing that the Hawaiian and Samoan islands, other than the recent volcanoes, have subsided, he is somehow disproving the writer's theory. The 1957 paper was, from beginning to end, a closely reasoned argument, but he has apparently failed to follow it. He does not produce a single piece of evidence antagonistic to the views expressed. In that paper the writer brought forward, not only the evidence in favour of his theory, but also that against it, and it is the latter, not the former, that Stearns shows to be mistaken. Thus he does not weaken, but strengthens the main argument.

On page 171 Stearns refers to the writer's " discussion of the Cook group, in which reefs 20 to 200 feet above sea-level are cited as evidence of uplift." He adds, " Although *some* of these reefs may be due to uplift, *many* of the lower ones are undoubtedly due to eustatic shifts of sea-level in Pleistocene times " (my italics). Exactly five islands in the Cook group have raised reefs. " Some " of these can hardly be less than two, and he asks us to believe that " many " (not all) of the remaining three are due to shifts of sea level.

The earlier publications cited in the 1957 paper (Marshall, 1930) ; Chubb, 1934), make it clear that the movement postulated is not a simple uplift, but a tilting of a portion of the crust about an axis (Marshall's neutral line ; Chubb's fulcrum line) with subsidence on one side and uplift on the other. The evidence for this is that on the north-western side of the line there is only one tiny volcanic island, Aitutaki, with broad lagoon and barrier-reef, and two atolls, Manuae and Palmerston ; and on the south-eastern side the five islands with raised reefs, whose amount of uplift is a function of their distance from the axis. Thus in Rarotonga the uplift is 20 feet., the distance 13 miles ; in Atiu 70 feet, 45 miles ; in Mitiaro 92 feet, 60 miles ; in Mauke 100 feet, 85 miles ; and in Mangaia 200 feet, 130 miles. It is hardly possible to imagine better evidence of tilting.

The Cook islands are an old group with a long history. Evidence is lacking as to whether the islands arose one by one or more or less simultaneously, but it is clear that each island was first built up above sea-level and later, probably after subsidence, reduced to a shoal by wave action, until all, or nearly all, of the group was submerged. This was followed by uplift, dissection, and growth of fringing-reefs, then by subsidence with development of barrier-reefs. The tilting, which involved renewed uplift of the south-eastern islands and further subsidence of the north-western ones, was the latest phase in the group's evolution. The story is told by Marshall (1927 ; 1930) ; the writer has done little more than tabulate his findings (Chubb, 1934).

Stearns writes (p. 171) " ample evidence exists that the Hawaiian archipelago, which is a good example of island chains, was built over two great rift zones with a migration of the volcanic activity lengthwise along both rifts ending with Hawaii at the south-east as the youngest island." So far we are in agreement, but he adds " the presence of atolls at the north-east (sic, north-west) end of the chain with stacks and fringing-reefs to the south-east can be explained better by the downward tilting and/or greater age of the chain to the north-west ; and hence more erosion for the islands on the north-west end."

So he allows the possibility of tilting of the Hawaiian islands, though not of the Cook group, where the evidence is much stronger. But according to his account the history of the Hawaiian chain is comparatively simple. The islands were built up one by one, from north-west to south-east, a process

occupying several tens of millions of years, for " the older volcanoes became extinct in early or middle Tertiary." And during this long period there was no subsidence ; the crust somehow managed to support this ever-increasing load and all the islands remained stationary. Finally, however, when the chain had more or less assumed its present form, it was tilted down towards the north-west " as a unit mass."

The Cook islands are a small and compact group, less than 450 miles from north-west to south-east or, if the outlying Palmerston atoll be excluded, only some 250 miles. When all its islands had become established the group may well have acted as a unit mass. This is far less likely in the case of the Hawaiian chain, strung out along a line 1,600 miles long, its north-western islands probably as old as, or older than the Cook group, while its south-eastern islands are still growing to-day. The prolonged stationary period, implicit in Stearns' theory, followed eventually by downward tilting, involves geological processes far less probable than the simple proposition that the movement of material from below to above the surface will normally lead to subsidence. There can be little doubt that every volcanic island in the central Pacific started subsiding even while it was being built up.

Stearns' final statement, that " there is evidence of tremendous changes in the shape of the floor of the Pacific in Tertiary and Quaternary time which have left shorelines probably eustatic up to 1,200 feet above sea-level," is truly fascinating. Obviously any islands standing on the parts of the Pacific floor affected will have shared in the tremendous movements, with the result that the benches cut in them by the 1,200-foot sea may to-day be at any height above or below sea-level. This perhaps explains why no trace of benches at this elevation was seen by the writer in any of the islands he visited, though he found plateaux or benches at 1,300–1,500 feet on Hivaoa, at 2,500–2,600 feet on Nukahiva, two of the Marquesas islands, and at 800 feet on Rurutu in the Australs.

However, the continents are bound to show the effects of a 1,200-foot eustatic rise in sea-level, which would necessarily have affected the Atlantic, Arctic, and Indian Oceans, and the Mediterranean and Caribbean Seas, as well as the Pacific. It would have cut a 1,200-foot bench around all mountains and all hills of suitable height, throughout the earth. It would have submerged most of the eastern half of the United States and two-thirds of Canada, all the British Isles except for certain mountainous regions, vast areas of northern Europe, nearly all the Soviet Union, three-quarters of Australia, half of South America, and extensive areas elsewhere. Almost exactly 50 per cent of the land surface of the globe lies below the 1,200-foot contour ; all this area would have been mantled by superficial marine deposits, and its terrestrial fauna and flora destroyed. There is no evidence that this has occurred.

The writer's views are based upon an interpretation of the observed facts in the light of two fundamental principles (*a*) that the transfer of material from below to above a flexible crust will cause subsidence, and (*b*) that water seeks its own level. Both principles would seem to be axiomatic.

L. J. CHUBB.

GEOLOGICAL SURVEY,
KINGSTON 6,
JAMAICA.
25th November, 1961.

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SIR,—The writer's policy is to avoid published " letter " controversies by eliminating resolvable differences by preliminary exchanges of opinion prior to publication. His criticism[1] of Mr. Chubb's 1957 article, therefore, was sent to Mr. Chubb before being offered for publication with the following statement: . . . " Possibly after reading my paper and checking the references you will prefer preparing a new paper to having mine submitted for publication, since it is so negative." Mr. Chubb replied, on 3rd January, 1961,— . . . " I am now concerned with other lines of work and do not wish to become involved in any controversy about the Pacific Islands . . . I have no present intention of publishing any more on the subject."

After publication of the criticism, however, Mr. Chubb prepared a reply for publication, a copy of which was sent to the present writer for the rejoinder which follows :—

Obviously, as pointed out by Mr. Chubb, a part of our disagreement is in semantics. The writer accepted Mr. Chubb's statement that " each island subsided independently " as a literal expression of his opinion but his subsequent statement that " every volcanic island in the central Pacific started subsiding even while it was being built up " is acceptable.

The problem in the Pacific is too large to justify verbal sniping at this level between scientists for publication. My recently published paper[2] points out that much data remains to be gathered before we can establish that the 600-foot shoreline is eustatic and that the evidence for eustatic origin of the 1,200-foot shore is far less complete.

Chubb assumes that the continents stood still during the greater eustatic movements and that 50 per cent of the earth would have been flooded with the sea at 1,200 foot. My hypothesis assumes a rise of the continents which caused the ocean basins to become smaller ; hence the water rose eustatically on islands. I stated, in connection with Mr. Chubb's hypothesis, that the shorelines in the Cook Islands must be re-examined and compared to those on other islands to determine whether the shorelines are high due, entirely to uplift or whether some of them are eustatic, as projected by me, or whether they may be eustatic shorelines superimposed on islands previously raised above sea level by folding.

Some of the emerged reefs in the Cook Islands are exactly at the levels of world-wide glacio-eustatic shifts of sea level. The writer believes it possible that eustatic levels higher than the present sea have beveled older reefs there and left them high and dry. Marshall[3] thought that the emerged atoll of Mangaia predates the Pleistocene. If so, it must have been eroded by all glacio-eustatic sea levels.

The problem of finding shorelines on tropical islands is usually difficult and the absence of a reliable glacio-eustatic sequence of shorelines proves nothing except the need for further search. Preservation of such shorelines may be accidental in some places and the discovery purely fortuitous. Geologists in the field err when they look only for textbook evidence such as emerged benches and reefs because only a few fossils lodged in a protected crack in bed rock at some level above the present sea is sufficient evidence to prove former submergence of great significance. Such a crack in basalt filled with ancient fossils was found on Lanai Island in the Hawaiian group at 1,069 feet above sea level.[4] Systematic search has not been made for this type of evidence on islands in the central Pacific, except for the Hawaiian Islands. Even in Hawaii, the search is never finished because new road cuts and newly drilled wells expose evidence of sea levels not previously recognized.

I am glad to have the data in my criticism of Chubb's paper " strengthen his hypothesis " if the reader is informed regarding the geologic problems and

seeming contradictions. Mr. Chubb's paper will doubtless stimulate more thinking and research in the geologic history of the Pacific Islands which is a field in which much work remains to be done before all the eustatic movements can be correlated and island histories deciphered.

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16th January, 1962.

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TIE-LINES OF CO-EXISTING PYROXENES

SIR,—During 1961 several papers and letters have been published in the *Geological Magazine*, dealing with co-existing pyroxenes in igneous and metamorphic rocks as well as in nodular inclusions in lavas. In order to lessen the apparent controversy, resulting from the different approaches to the problem by the various authors, it might be useful to compare the results of the thermodynamical calculations of Kretz with the conclusions of the purely chemical approach of Bartholomé.

The formula used for the calculation of the K_d value of Kretz is the following :

$$K_d = \frac{X_o}{1 - X_o} \cdot \frac{1 - X_c}{X_c} \quad \text{where } X_o = \frac{Mg}{Mg + Fe_2} \text{ in orthopyroxene}$$

$$\text{and} \quad X_c = \frac{Mg}{Mg + Fe_2} \text{ in clinopyroxene}$$

Consequently the formula for K_d can be written as follows :

$$K_d = \frac{\left[\frac{Mg}{Mg + Fe_2}\right]_o}{1 - \left[\frac{Mg}{Mg + Fe_2}\right]_o} \cdot \frac{1 - \left[\frac{Mg}{Mg + Fe_2}\right]_c}{\left[\frac{Mg}{Mg + Fe_2}\right]_c}$$

$$K_d = \frac{\left[\frac{Mg}{Mg + Fe_2}\right]_o}{\left[\frac{Fe}{Mg + Fe_2}\right]_o} \cdot \frac{\left[\frac{Fe_2}{Mg + Fe_2}\right]_c}{\left[\frac{Mg}{Mg + Fe_2}\right]_c}$$

$$K_d = \left[\frac{Mg}{Fe_2}\right]_o \cdot \left[\frac{Fe_2}{Mg}\right]_c$$

The K_t value of Bartholomé is defined as follows :

$$K_t = \left[\frac{Fe_2}{Mg}\right]_o \cdot \left[\frac{Mg}{Fe_2}\right]_c$$

If both formulae are compared it becomes clear that $K_t = 1/K_d$. This conclusion is confirmed by the results of the calculations of both authors.