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TAHITI-HAWAII A.D. 1100-1300: FURTHER COMMENTS

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It is sometimes the case in commentary that scholars fail to indicate differences between the state of the evidence at a time a conclusion was drawn and the situation as it exists now. I believe this applies to some comments on the hypothesis for contact between Tahiti and Hawaii during the interval A.D. 1100-1300.

While Cordy (1974a:67) initially summarizes correctly the situation with respect to changes in the Hawaiian fishhook head type HT⁴ as the evidence was interpreted in 1966, by 1971, with additional dates, Emory and Sinoto had revised their opinion. In a subsequent analysis (Green 1971), from which Cordy quotes, I attempted to show why they had erred in using seriation to place the lower levels of H-8 with head type HT⁴ after rather than earlier than those of H-1 where HT⁴ is lacking from the lowest level. Therefore, I am incorrectly viewed by Cordy (1974a:71) as holding to an earlier position (Green 1966:30). Rather, I should be seen as agreeing that the fishhook evidence from Hawaii no longer supports the hypothesis for contact with Tahiti in the interval A.D. 1100-1300. This, however, does not negate the possibility of earlier contact from one to the other of the two island groups as a source for that particular innovation.

The precision of Cordy's discussion of the linguistic arguments could also be improved to his advantage. For example, he (1973:71) is wrong in asserting that glottochronological dating, as that term is normally understood, every yielded A.D. 200-500 year dates for contact between Tahiti and Hawaii. His figures are from Emory's 1963 analysis, and as I tried to show (Green 1966:11), did not yield what most on linguistic evidence alone, would understand as "glottochronological dates".

First, glottochronological methods which yield "dates" are not usually done with "total" vocabulary, the basis for the A.D. 250 result Cordy has cited. Nor does glottochronology normally use radiocarbon dates of archaeological events to infer linguistic splits from which to derive retention rates. Thus Emory's methods of determining linguistic cognates led not only to inappropriate cognate percentages, but also used false archaeological and linguistic assumptions to calculate ages. In this light citation of any results drawn from Emory's work, such as the A.D. 504 and 517 results employed by Cordy, are far older than those which would be obtained by traditional linguistic methods, and can not be cited either to negate or support the A.D. 1100-1300 contact hypothesis.

The only linguistically based glottochronological estimates known to me are not for contact, but for the separation of Tahitian from Hawaiian, in an analysis in which both languages are placed in the same subgroup. For this, Elbert (1953:Table 3) suggested an age shortly after 930 A.D. Yet even this result did not employ the usual method of single cognates, and the one that does, on Elbert's analysis, has yielded a 51 percent retention of cognates, a result Elbert rejects as too low because of word tabooing in Tahitian. In sum, glottochronology does not provide wrong, but no useful estimates of age for contact between Tahitian and Hawaiian. Nor do I think it ever can, as this is not its purpose.

Further, in assessing the linguistic evidence for contact (i.e. borrowing) between Hawaiian and Tahitian, Cordy (1974a:69) largely expresses a feeling "that Eastern Polynesian language relationships are still far from certain". While this is acceptable (little is ever certain in historical linguistics) the comment does not affect the state of the evidence supporting this particular hypothesis. Rather, linguistic evidence of contact has depended first, on evidence for the assignment of Hawaiian to subgroup different from Tahitian; and second, on the citation, none the less, of uniquely shared innovations between Tahitian and Hawaiian, which, because they are shown not to be shared retentions, may be cited as evidence of later contact.

Some evidence for this was developed in 1966, when it was shown that Hawaiian more likely subgroups with the two Marquesan dialects and Mangarevan, than with the Tahitic subgroup. Additional comparative evidence in support of the Hawaiian-Marquesan-Mangarevan subgrouping hypotheses, and for the initial differentiation of Proto-Eastern Polynesian into Eastern and Proto-Central Eastern, is now available. Thus, the empirical content of the two subgrouping hypotheses has increased since they were proposed, whereas it has not increased for competing lexicostatistical subgrouping proposals by either Elbert or Emory which would rule out contact. However, while the quality of the evidence for the revised subgrouping has improved, the quality of the comparative evidence for contact between Tahiti and Hawaii remains poor. What is required to alter the situation is countervailing evidence in support of competing subgrouping hypotheses, or new and more convincing evidence of contact taking into account a wider range of comparative linguistic data.

It should also be noted that few would any longer back lexicostatistical results to make any determination of subgrouping at the close level of cognate agreement scores exhibited by these languages (cf. Groube 1973:235). The values are not significantly different. Such linguistic evidence as exists for subgrouping is likely to be limited in extent and comparative in origin, but not lexicostatistical.

Despite the above I would fully support Cordy's general conclusion. There is now a sufficient body of new and old linguistic and archaeological data to question the adequacy of the evidence formerly cited in support of the hypothesis of Tahitian-Hawaiian contact between A.D. 1100 and 1300 (or at any other time) especially if it is viewed as the main basis for culture change in Hawaii. I would only add that there is also insufficient early archaeological data from Tahiti and other East Polynesian islands to evaluate a closely related archaeological hypothesis that the earliest Hawaiian cultural complex derives from the Marquesas rather than Tahiti, or some other island group (Green 1971). While this does not affect the status of a linguistically related hypothesis for which a growing body of data is available, it does mean that these two hypotheses can not be cited as mutually self-supporting until the other archaeological possibilities are examined.

In short, care is needed in referring to earlier works for materials supporting various conclusions in the area of Hawaiian-Tahitian contact, as the context in which they occur has changed.

Cordy's final comments raise issues of scientific procedure. One hopes, in an enthusiasm for general systems theory, the new generation of Pacific archaeologists does not overlook the fundamentals of how change occurs in science (cf. Morwood 1974) by insisting on an overly strict deductivist framework, or a naive falsificationist position. Most scholars who reject an inductivist-deductivist distinction, work on the interface of increasing the empirical content of one of several competing hypotheses, while decreasing, through falsification, the content of rival explanations. Every so often, when a new more inclusive hypothesis is formulated, older ones are either abandoned or subsumed because the new one has an obvious excess of empirical content over its predecessors. Inductivism alone leads nowhere, while a strict deductivist account fails to allow for the ever changing status of scientific hypotheses.

Therefore, when Cordy (1974a:72) argues for Hawaii "That all possible explanatory hypotheses of cultural change should be analysed" I support him, but I disagree when he claims that "before hypotheses are accepted, valid testing with valid data must take place". The lack of valid testing with valid data has not been a flaw in the Hawaiian-Tahitian hypothesis; rather, assessments of its empirical base have always occurred. What has happened is that empirical content, which was the basis of that theory versus all rival explanations of cultural change in the period A.D. 1100 to 1300, is now capable of sufficient falsification, given the most recent interpretations of the relevant evidence, for Cordy to render it uncompetitive vis-a-vis other possibilities whose empirical content Cordy (1974b) has developed elsewhere.

In archaeology one normally accepts the superiority of a particular hypothesis at a specified point in time because a close review (test) of all known evidence relevant to it at the time (though perhaps not in the future) convinces those concerned that it subsumes most, if not all, of the possible data supporting both the central hypothesis and its logically entailed subsidiaries, especially when these are compared to rival or preceding theories.

There is no valid test with valid data which thereafter makes an hypothesis forever acceptable. This is the deductivist's delusion, for it assumes (1) that firm confirmation or refutation is, in fact, the outcome of a properly conducted scientific experiment, (2) that invalid tests and invalid data, as well as the valid kind, actually exist, and (3) that such tests and data are able to be distinguished from the valid kind. I fear all we really have as archaeologists, whether cultural historians or general systems theorists, are ever increasing amounts of potentially usable data organized by better theories. The theories are accepted as better because they accommodate the new data, where their predecessors did not. On that basis, Cordy, myself, and probably most others will accept his proposed "fate" for the special case of Tahitian-Hawaiian contact 1100-1300 A.D. as a faulty explanation of cultural change in Hawaii. Many would agree to his incorporation of the general case of "contact" within a broader range of other related hypotheses now developed for those events. But it would help all, when theories are being challenged, to keep historical context clearly in view, assessing our conclusions in that light.

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