Review Article

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The Pacific Islanders. William Howells. New York: Charles Scribner's Sons, 1973. xvi, 299 pp. 16 plates, 14 maps, 6 figures, references, index. $12.50 (cloth), $4.95 (paper).

OVER the past twenty years or so, the disciplines of physical anthropology, archaeology, and linguistics have produced a wealth of information about the Pacific Islanders which would have been undreamt of in the earlier years of this century. A substantial piece of dry land has emerged from the storm-whipped seas of hypothesis which spanned the long era from Captain Cook to Thor Heyerdahl, and W. W. Howells is certainly one of its most literate residents. He has written this book, the first of its kind, on the physical anthropology and prehistory of Oceania and Southeast Asia. He sets out his ideas very clearly and avoids obscure jargon. However, while his attempts at objectivity in a field which has always been excessively subjective are highly commendable, they are not (and could not be) entirely successful. This is not the fault of the author; I doubt if anyone could write a totally objective book on such a spottily documented field of information. Howells in fact makes many sensible comments, and I might add here that I agree with his major conclusions, apart from some of those relating to Polynesia to which I will return later.

Since Howells is a physical anthropologist, it is not surprising that about half the book is given over to the Pacific Islanders themselves. The other half is concerned with archaeology and linguistics, and the whole triad together produce the historical picture which is the main thread of the book. Howells gives short shrift to the once-important discipline of historical ethnology, and doubts whether living cultural forms are safe materials in historical reconstruction.

The author, then, is following a most laudable course in trying to combine evidence from three relatively independent disciplines. What is more, he is doing this in an age of scholastic super-specialization, at a time when there is a certain disillusionment with culture-historical problems in general. I (the reviewer) share

Howells' sentiments on this matter—"Breathes there an anthropologist with a soul so dead as never to have asked: 'Where did the Polynesians come from?' " (p. 8).

The first quarter of the book is devoted mainly to physical anthropology—to the phenotypes and genotypes of the Pacific Islanders. Howells rejects the "Long Migrations" and the "Pure Races," and wheels in the computers. His view is that only two basic populations have entered the Pacific, "Melanesians" and "Proto-Mongoloids." The Melanesians include the Australian Aborigines and Philippine and Malayan Negritos, as well as the Melanesians proper, and as we now know they spread into New Guinea and Australia before 30,000 years ago, after a prior evolution which presumably took place in Malaysia and Indonesia. The Melanesians as so defined are not related to the African Negroes, either phenotypically or genotypically, although the Andamanese may be, and these remain as something of a problem. Since the drowning of the Torres Straits, and probably before, the New Guineans and Australians have drifted apart physically, and the former have of course mixed to some extent with the much more recent Proto-Mongoloid populations, whom we now know best as the Indonesians, the Micronesians, and the Polynesians. The Melanesians proper are now for the most part a very complex clinal population.

Gene frequencies add a little to the foregoing picture, but these are obviously bedevilled by the presently unmeasurable effects of natural selection and genetic drift. Serum gamma globulins and ear wax wetness or dryness seem to be the best candidates for differentiating Proto-Mongoloids from Melanesians, and Howells also gives us useful computer results from an unpublished thesis on genetics by Schanfield, which makes a similar separation. Australian gene frequencies suggest long isolation, while those of Polynesia may have been impoverished by the founder effect. So far so good, and all would probably agree. But one cannot help noticing that all these analyses in physical anthropology have to skip lightly over Indonesia owing to lack of information. As Indonesia is undoubtedly the main formative area for all Pacific populations and cultures in the broad sense, and as its pre-European population was probably at least ten times that of all the Pacific Islands and Australia put together (at a rough guess), then the Pacific anthropological community as a whole clearly has a problem on its hands. Nevertheless, I feel that Howells' views on physical anthropology make sound sense for the present, although his use of the general term "Melanesian," as opposed to Coon's use of the term "Australoid" for the same populations, may cause some confusion in the future. In a recent article of my own (Bellwood 1975) I have used Coon's terminology, and I feel a personal inclination to stick by it in the future.

From his main witness of physical anthropology, Howells moves on to a summary of the linguistic evidence. He adopts the now widely accepted view that the Austronesian languages of Oceania (i.e., Melanesia, Polynesia, and Micronesia, excepting Chamorro and Palauan) form a major subgroup of the Austronesian family. He inserts a few details from Dyen's lexicostatistical analysis, but rejects a Melanesian homeland for Austronesian. However, the diversity of the Melanesian languages does suggest long settlement, and Howells quite rightly separates out the Eastern Oceanic subgroup, recently established in detail by Pawley, for special consideration. The members of Eastern Oceanic are Polynesian, Fijian, and some languages of the New Hebrides and Solomon Islands. Eastern Micronesian languages may have some remote affiliation. Howells sees these languages as a result

of Proto-Mongoloid penetration into eastern Melanesia and Polynesia from an ultimate Island Southeast Asian source. I heartily agree with all this, but would take issue on two points. Firstly, Howells clearly wants to bring in his Eastern Oceanic speakers and Proto-Mongoloids through Micronesia—this matter I will take up in more detail below. Secondly, Howells reiterates his original view (Howells 1933) that Fiji was settled first by Polynesians, and then later "Melanesianized" from the west. I have argued elsewhere that Fiji is more likely to have been settled by both Polynesian and Melanesian groups, or at least by a clinal population, from the beginning of the archaeological record before 1000 B.C. (Bellwood 1975). Gabel (1958) has pointed out that the Melanesian phenotype is strongest in the interior of Viti Levu, and he felt that "this condition, occurring as it does in the mountainous interior, which may be regarded as a refuge area, supports the theory that the Melanesian is the earlier component in Fiji." Whether one supports Howells or Gabel on this may be debatable, but Howells then goes on to use linguistic evidence derived from Pawley to support his case. The Pawley paper in question was presented in cyclostyled form to a Wenner-Gren Symposium held at Sigatoka in Fiji in 1969 (Pawley 1969), and has since been published by Pawley in totally revised form (Pawley 1972). The latter item was evidently not available to Howells, so his accuracy with his quoted sources is not in question. But a major point must be made. In 1969, Pawley suggested that Fiji was settled at an early date by people with a language ancestral to Proto-Polynesian, and then Melanesian speakers arrived later in the group, after the initial settlers of Polynesia had departed for (presumably) Tonga. This view of course supported Howells ideally. However, in his 1972 publication, Pawley tends more strongly toward a single origin for Fijian from a Proto-Fijian forebear, which later split into two dialect chains (see also Pawley and Sayaba 1971). While Pawley does add that there may have been two distinct speech traditions which contributed to the word stock of Proto-Fijian (1972: 128), he is clearly unwilling to press the matter very far, and as I would read the linguistic evidence, there is no longer a definite case for later Melanesian influence on the Fijian languages. There can be no certainty on this matter, but I would prefer a single major period of settlement in the group. If Melanesians did settle Fiji after Polynesians, then any time gap between them must have been very small indeed.

Following the reviews of physical anthropology and linguistics, Howells goes on to present three chapters of synthesis: on Australia and Tasmania; Melanesia and Indonesia; and Polynesia and Micronesia. This is where he brings in the archaeology to round off the picture.

For Australia and Tasmania, Howells rules out African or Ainu affiliations, and classes Tasmanians with Melanesians rather than as a Negrito population. He is unhappy with the Birdsell trihybrid theory and suggests two Pleistocene populations for Australia—Tasmano-Melanesian (represented by the Nitchie, Keilor, and Mungo remains), and archaic (represented by the Kow Swamp, Cohuna, and Talgai remains). The former affiliates closely with the Niah and Wadjak fossils, while the latter may be a result of mixture between Tasmano-Melanesians and the Solo population, presumably in Indonesia. Both groups migrated to Australia separately in the Pleistocene (there being no Negrito "wave"), and both have intermarried and contributed to the present pattern of Australian diversity. Howells is unwilling to

commit himself as to whether new populations entered the continent with dingoes and the small-tool tradition during the Holocene era. However, only the Tasmano-Melanesian population managed to reach Tasmania. The Howells view is probably the most sensible one for the present state of knowledge of Australia, and the major problem still seems to hinge on the date range for the Solo population (an unknown) and on the date of appearance of the archaic population in Australia.

Turning to Melanesia and Indonesia, Howells conjures the term "Old Melanesia" for the huge late Pleistocene land area ranging from Malaya and Indonesia through the Philippines and Wallacea, and on into the linked continent of Australia and New Guinea. He of course uses lowered Pleistocene sea levels for this reconstruction, and views the whole area as inhabited by Melanesian populations (including his Tasmano-Melanesian group and possibly some Solo remnants) from 50,000 B.P. onward. Adjacent to this province, on what is now the mainland of Southeast Asia, Howells places his second province of "Hoabinhia," the home of the Proto-Mongoloids. The small-statured "pygmy" populations of Malaya, the Philippines, and New Guinea are seen as local results of selection from a Melanesian gene pool, and these peoples have survived in pockets amidst the overwhelming movements of Proto-Mongoloids into Island Southeast Asia during the Holocene.

The incoming Proto-Mongoloids seem (if I read Howells correctly) to have been basically responsible for the introductions of horticulture and Austronesian languages into the Pacific, although Howells actually states that the first Austronesian speakers in Melanesia were physically Melanesian, and this may be so. The spread of pigs, horticulture, and people throughout eastern Melanesia within the past 5000 years then led to what Howells has called the "New Melanesia." He adopts the view (still widely held) that the expansion of Austronesian languages and horticulture through Indonesian and into Melanesia had something to do with the Lungshanoid culture group of China, although I would personally not agree here, and would prefer to look for a local Island Southeast Asian inspiration (Bellwood 1975). Furthermore, New Guinea itself may have had some innovative role in horticulture and the development of polished stone tools, although Howells seems unwilling to accept this view.

The views of Howells on Polynesia and Micronesia will probably turn out to be the most controversial in the whole book. For Polynesia itself he gives a perfectly acceptable summary and traces the ancestral culture to a Lapita ancestor in Fiji. There are few today who would disagree with this. He then points out that the Polynesian phenotype cannot be derived directly from a Melanesian forebear, and this again would be acceptable to most. But he then goes on to claim that ancestral Polynesians could not have migrated through Melanesia, and turns the spotlight on Micronesia, where there are populations who are obviously much closer to Polynesians in appearance. It is at this point that controversy comes in. Howells thinks that the ancestral Polynesians originated somewhere in the area around Taiwan, the Philippines, and western Micronesia, and migrated eastward from there through eastern Micronesia about 2500 B.C. Here they settled the volcanic islands first, and then adapted themselves to atoll environments before moving down into eastern Melanesia about 1500 B.C., where they established the Eastern Oceanic languages and the Lapita culture. He accepts Groube's view (1971) that the Lapita potters were basically without horticulture, and this supports his thesis of atoll adaptation

in Micronesia. He does, however, allow them taro and breadfruit, and derives the domestic animals through Melanesia. From a location possibly in the New Hebrides, these Eastern Oceanic speakers then settled Fiji and Polynesia, moved westward to spread the Lapita culture in western Melanesia (Howells sees the Lapita culture as appearing first in eastern Melanesia), and also moved back northward into eastern Micronesia where they masked earlier linguistic diversity.

Now all this seems to me to be very complicated, particularly with the complex back-movements which Howells has to postulate. I cannot see how Howells can derive the Lapita culture through Micronesia (it is unlikely to have sprung up unaided in the New Hebrides), and the archaeological evidence now is against his view that the culture is earliest in eastern Melanesia. Lapita sites near New Ireland and in the Solomons go back to 1000 B.C., and all the evidence points to direct transmission from eastern Indonesia across to the north of New Guinea at this time. The decorated pottery from the Marianas Islands may be cousinly to Lapita, but it is not Lapita per se. It is possible that Micronesian islands such as Ponape or Truk may produce pottery akin to Lapita in the future, but for the moment we have only negative evidence from eastern Micronesia. Green (1973) has also thrown some doubt on Groube's "strandlooper" hypothesis, and all the evidence seems now to point to the Lapita potters as horticulturalists moving rapidly eastward through Melanesia.

The linguistic evidence does not really support Howells either, unless one can see the languages of Yap and Nauru as witnesses for the earlier diversity which he proposes. And turning to the physical anthropology, why should not the Lapita potters have moved quickly and in small numbers through already-settled western Melanesia, until they were able to form more substantial settlements in Fiji and Polynesia? The Lapita settlers who remained in western Melanesia may therefore have intermarried with surrounding Melanesians, and have virtually disappeared phenotypically. Since Howells himself suggests that the main Pre-Polynesian movements were from the New Hebrides (p. 239), then he must himself be willing to accept absorption of this kind, as the New Hebrides today have few Polynesian-like populations.

It is much easier in my view to see the Lapita potters as Proto-Mongoloid migrants (to use Howells' term) moving eastward through Melanesia, settling and intermarrying with resident Melanesians. This explains why Lapita pottery is not now always found in areas with Eastern Oceanic languages, and it explains the pockets of Polynesian-like phenotype (excluding the Polynesian Outliers) in Melanesia. The settlement of eastern Micronesia may then have taken place from eastern Melanesia by a Proto-Mongoloid population, and not vice versa. Howells clearly has problems (p. 239) in some features of his movements of Pre-Polynesians westward into Melanesia, and the view presented here would overcome some of these. Furthermore, the Motu language of Papua, which Howells sees as evidence of this westward movement by Eastern Oceanic speakers, is now removed from the Eastern Oceanic subgroup by Pawley (1972: 10).

What all this really boils down to is that Howells can derive support from physical anthropology for his view, but gets very little from archaeology and linguistics. A Melanesian migration route for Polynesians is not necessarily a certain conclusion, but it seems to be the most likely one.

Such criticisms aside, I regard Howells' synthesis as a most valuable addition to the literature of Pacific prehistory. There are of course additional trivial topics in the book with which I might take issue, but I would prefer to spare the reader. The field covered in the book is one in which few hypotheses remain unmodified for long, although as I pointed out at the beginning, there is now a much more solid basis of fact to work from than there was in the "prescientific" era of twenty years ago. Long may Professor Howells provide us with revised versions of this book as new data come to hand.

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Review Article

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Prehistory and Ecology in a Windward Hawaiian Valley : Halawa Valley, Molokai. Patrick Kirch and Marion Kelly, editors. Pacific Anthropological Records no. 24. Honolulu: Department of Anthropology, Bernice P. Bishop Museum, 1975. 207 pp., 45 tables, 79 figs. $7.50.

CONSIDERABLE recent research has been devoted to describing the temporal and spatial variability in the prehistoric subsistence economy of the Hawaiian islands. This ecological orientation is based on the belief that cultural development in Hawaii was determined by feedback between environmental and cultural factors. *Prehistory and Ecology in a Windward Hawaiian Valley: Halawa Valley, Molokai* reports the results of a major archaeological research project which exemplifies the recent ecological approach in Hawaiian studies. The Halawa Valley Project was designed, under the guidance of Roger Green, to collect data on settlement and subsistence patterns and to permit comparisons with similar data collected by two concurrent projects at Lapakahi, Hawaii and Makaha, Oahu. These three projects (Halawa, Lapakahi, and Makaha) shared a common theoretical orientation and similar research methods (see pp. v–vi), and the information gathered by these projects now forms a comprehensive data source for synthetic analyses of Hawaiian prehistory.

For each project, a traditional land section *(ahupua'a)* was selected for survey, detailed mapping of surface features, and selective excavations. The reasons for selecting an *ahupua'a* as the analytical unit are clear. During the late prehistoric period, an *ahupua'a* was a largely self-sufficient community with access to necessary subsistence resources including fishing areas, agricultural land, and a minimal use hunting-gathering area. As a generalized, almost modular, social and economic unit, the *ahupua'a* was a partial microcosm of Hawaiian society. A comprehensive archaeological investigation of an *ahupua'a* thus should show (1) the settlement system for a community, including the community organization and the integration of diverse extractive activities, and (2) the local sequence of development for this

settlement system. Then, by comparing the sequences from several *ahupua'a* with contrasting environments, it is possible to isolate both the pattern of development general to the Hawaiian islands and the variants to that pattern determined by specific environmental conditions. (Environmentally, the three *ahupua'a* [Halawa, Makaha, and Lapakahi] are quite different from each other. Lapakahi is an upland *ahupua'a*, an arbitrary territorial strip cutting across the ecological zones. It has no permanent stream and limited rainfall. In contrast, Halawa and Makaha are valley *ahupua'a*, each of which encompasses an entire catchment for a permanent stream. Halawa, as a "windward" valley, usually receives more rainfall and a larger stream flow than a "leeward" valley like Makaha. This windward-leeward contrast results from a rain shadow created by the central mountains; however, the particular conditions of a valley are determined by many additional factors. For example, leeward valleys like Waimea, Kauai may receive ample stream flow because the catchment of the central stream extends deep into the rainy central mountains.)

However, the use of the *ahupua'a* as the primary analytical unit is restrictive in certain cases. From isolated *ahupua'a* studies, it is difficult to investigate the regional patterns which were associated with district-wide or island-wide organizations. Although issues of community-level organization and adaptation have been thoroughly investigated, the organization of the complex Hawaiian chiefdom which overarched the community organization has been neglected in resent research. This limitation is evident in the Halawa monograph where issues of external organization and regional context are treated only peripherally (see p. 179, for example). (Douglas Yen [personal communication] has suggested another restrictive factor to *ahupua'a*-related research. Especially in areas like Lapakahi where *ahupua'a* divisions are topographically arbitrary, using protohistoric/historic organization units as the basis for analysis may hamper investigations of earlier organizational patterns.)

The Halawa Valley Project monograph is subdivided into six reports which summarize the results of the project's several coordinated investigations. The following sections will briefly introduce these reports to the reader.

Report 1 (Kirch: "Excavations at site A1–3 and A1–4: early settlement and ecology in Halawa valley") describes the excavation, stratigraphy, and results of midden and artifact analysis from a coastal site, A1–3. This site contained stratified dwelling and hearth/oven features which spanned an occupation period from A.D. 650 to 1700. Because there was only one manifest cultural level (Layer IV, about 30 cm) for the whole period, temporal affiliation of artifacts was determined by arbitrary 10 cm levels and feature association. Results included evidence for major change in material culture, a shift in diet from fish toward animal domesticates (dogs and pigs), and the presence of early round-ended houses.

Kirch also reports on the excavation of a strata cut into taluvial deposits at A1–4. The exposed layers were primarily geological but were interpreted by Kirch as evidence for early shifting cultivation (see later discussion).

Report 2 (Rosendahl: "Surface structural remains in Kapana") summarizes the intensive surface mapping of the traditional land division of Kapana. Rosendahl's map (Fig. 31) provides a vivid representation of an inland archaeological zone. It shows the spatial association between varied surface features including irrigated and nonirrigated terraces and residential and ceremonial complexes. Mapping and

extensive excavation (Reports 3, 4, and 5) of Kapana provide an in-depth investigation of the interior component of the valley's settlement system.

Report 3 (Riley: "Survey and excavation of the aboriginal agricultural system") analyzes the diverse data on irrigation farming in Halawa. Relying on a 1915 map of pondfield complexes and several maps of existing archaeological features, Riley formulates a four-part typology of irrigation. He also reports on the excavations of agricultural terraces and ditches, the stratigraphy of which is used to document techniques of construction and sequences of land use.

Report 4 (Hendren: "Excavation of eight inland prehistoric habitation sites") describes excavations of terraced residential areas located on the taluvial slopes of the interior valley. The goals of these excavations were (1) to determine the criteria for distinguishing habitation terraces from the associated agricultural terraces and (2) to describe the spatial and temporal distribution of these habitation terraces. Among the features associated with these terraces were earth ovens, hearths, paved floors, and subfloor flexed burials, the latter described for the first time in Hawaii. Although midden and artifacts were relatively scarce on these sites, most sites were assumed to represent a permanent inland occupation. The primary time span for these interior sites was A.D. 1350–1650.

Report 5 (Rosendahl: "Test excavation at site A1–30 [Kapana heiau]") describes the exploratory excavations at a complicated interior valley site. Rosendahl concludes that the main features, three paved terraces, were part of a prehistoric *heiau* (religious shrine) because of their formal similarity to *heiau* features in Lapakahi and Makaha. On the basis of associated midden, domesticate artifacts, and an earth oven, Kirch (p. 178) suggests that this site may have been a *mua* (men's house) which served both ceremonial and dining uses.

Report 6 (Kirch: "Radiocarbon and hydration-rind dating of prehistoric sites in Halawa") summarizes the absolute dates and their associations for the Halawa Valley Project. The correspondence between radiocarbon and hydration-rind dating is an important methodological contribution to Hawaiian archaeology because it affirms the reliability of an inexpensive dating method, hydration. Such a method is essential for Hawaiian archaeology for which temporally diagnostic artifacts are extremely rare.

These six reports which are the core of the Halawa monograph are marked by an unusually high quality of data presentation. The combination of ample illustration, summary tables, and descriptions makes the data accessible for reanalysis and comparison. It is my feeling that the volume's primary contribution will be its data content.

In addition to data presentation, an explicit goal of the Halawa volume is to propose a cultural sequence for Halawa (Table 45, p. 181). In the summary chapter ("Halawa valley in Hawaiian prehistory: discussion and conclusions"), Kirch sums up the project's results as diachronic trends in various aspects of local development. Especially well documented are the trends in material culture, settlement pattern, and subsistence economy.

With certain notable exceptions (e.g., Emory, Bonk, and Sinoto 1968), there has been only limited research on the prehistoric chronology of Hawaiian material culture. The artifact collections from A1–3 are, therefore, significant because they are temporally intermediate between the Bellows site on Oahu (A.D 400–800) and

the protohistoric period. Distinctive artifact forms, namely untanged or incipient tanged adzes and unnotched two-piece fishhooks, are seen as transitory between an "archaic" Eastern Polynesian complex which would have characterized Hawaii's first immigrants and the fully evolved material culture described in ethnohistoric sources.

There are, however, certain difficulties with using the collections from A1–3 for chronological analysis. Because artifact counts, especially of diagnostic artifacts, are small, statements on both the formal characteristics of artifacts and their presence/absence must remain preliminary. In addition, virtually all cultural material from A1–3, which represents a thousand-year time span, comes from a 30 cm largely undifferentiated level. Although dates appear to stratify well with depth, artifact associations are somewhat tenuous. The conclusions are thus quite general because the collections lack both the size and the close associations necessary for establishing a more detailed chronology.

For the valley's settlement, a shift in residential locations is recognized. During the earliest period (A.D. 650–1350), habitations were clustered near the coast (A1–3) where they had easy access to all major microenvironments in the valley. Then, during the succeeding periods (A.D. 1350–1800), habitations became dispersed inland, accompanying an expanded valley population. At this time, habitations were distributed generally through the valley but especially on the taluvial slopes where they were directly associated with irrigated and nonirrigated agricultural fields (see Report 2). This diachronic trend may, therefore, indicate an increased emphasis on the agricultural sector of the economy, related to increasing population.

Perhaps the most ambitious goal of the Halawa Valley Project was to describe the development of the prehistoric agricultural system. As I will discuss, the report is most successful in defining variation in the valley's agricultural complex; however, the evidence for the evolution of the agricultural system is problematical.

In Report 3, the variation in nine irrigation complexes is used to propose a four-part typology designed to represent a range from the most simple to the most complex irrigation complexes in the valley. Type I complexes consist of small "spreader terraces" which are constructed across intermittent stream courses. During periodic flow, such rudimentary irrigation features slowed water and spread it over agricultural surfaces. (The reader is referred to similar floodwater features in Makaha [Hommon 1969, 1970*a*]). Types II, III, and IV are forms of "true" irrigation systems which tap water from permanent streams and distribute it to terraced taro pondfields. These types vary according to the following parameters: water source (II, tributary stream; III and IV, main stream), ditch length (increasing II–IV), complexity of water distribution (increasing II–IV), total area and number of pondfields (increasing II–IV), size of average pondfields (increasing II–IV), and type of terrace (II and III, stone-faced; IV, often unreinforced mud bund).

This variability in size and complexity of irrigation corresponds well with the irrigation systems described in other areas of the Hawaiian islands (Hommon 1970*b*; Hommon and Barrera 1971; Yen et al. 1972; Earle 1977). For the late prehistoric/historic period, a pattern emerges in which small interior pondfield sites contrast with the much larger pondfield sites located on the alluvial floodplains near the coast. (A similar contrast in irrigation systems exists elsewhere in the Pacific; see,

for example, the Mangaian pondfield complexes [Allen 1971]. It seems likely that the lack of more complex irrigation in Polynesian islands outside of Hawaii may be a reflection of the severely limited extent of coastal alluvium.) Although the interior sites were numerous and are frequently described archaeologically, it was the few coastal sites which, in the late prehistoric/historic period at least, predominated in total area in irrigated taro production (e.g., 86.7 percent in Halawa).

Riley correctly points out that his typology "represents essentially a continuum proceeding from types that make the greatest use of natural environment to those that most drastically alter natural topographic and hydrographic features" (p. 111). Logically, this typology could be seen as a developmental sequence. The progression of types from I through IV represents an increase in capital investment. Natural conditions of slope and water availability are replaced by artificial facilities like terrace and ditch complexes designed to increase agricultural productivity. Such apparent intensification can then be interpreted as a response to increasing production requirements based on a growing population.

Although logically attractive, this proposed "sequence" cannot adequately be evaluated with the available information because no absolute dates were obtained from the agricultural complexes (this point is fully recognized by the authors, pp. 111, 168). Nonetheless, there is some circumstantial support for the sequence. At one location, there is apparently stratigraphic evidence for the superposition of a type III complex over a type II complex (pp. 105–111). There also may be a spatial association between type II complexes and residential sites dating to A.D. 1250–1750, while type IV complexes may be associated with late prehistoric/historic residential sites. I feel that a strong case cannot be made based on this associational information because the report does not make clear how sites were selected to lessen probable bias in the small sample (8 house sites).

Alternatively, it is possible to interpret the typological variation in irrigation complexes as a direct reflection of different topographic situations. The steep, broken topography of inland areas requires the small, irregularly shaped pondfields clustered in available pockets of alluvium. In contrast, the gently sloping alluvial soils near the sea permit much larger, more regular pondfields spread over the extensive alluvial deposits. Here, longer and more intricate irrigation ditches are required because of the more extensive areas and gentler slope. From this perspective, there is no reason to suggest a local evolution of irrigation technology beyond the necessary adjustments of a general technology to specific situations. The question, therefore, is reformulated to ask why certain locations are selected at different points in time. For example, the "simplest" pondfield complexes (type II) may be the *latest* in the sequence, constructed only when marginal lands (steep, rocky, and spatially removed from marine resources) were finally put into production.

Another key inference drawn by the Halawa report is that there was a long term trend in agriculture from extensive to intensive practices as shifting cultivation became largely replaced by pondfield cultivation of taro (p. 115). The hypothetical shift in agriculture seems to be based implicitly on Boserup's (1965) theory that agricultural intensification (particularly the trend toward permanent agriculture) is related to increasing population density. A major difficulty in applying this thesis to Hawaii is that in areas like Kauai, where both shifting cultivation and irrigation

were possible, there was, in the historic period at least, a strong preference for irrigated methods (Earle 1977).

The stongest evidence for early shifting cultivation is the stratigraphy from A1–3, where there were various alluvial/taluvial layers containing terrestrial gastropods and/or charcoal. It is argued that the gastropods indicate the presence of a native forest in lower Halawa, the charcoal indicates burning of that forest associated with shifting cultivation, and the alluvial/taluvial deposits indicate periods of agriculturally induced erosion. A single carbon-14 sample from the lowest level yielded the date of A.D. 1200–1220 $\pm$ 100 (p. 163). It should be noted, however, that since both gastropods and charcoal occur from the lowest to the highest layers in the strata cut, there is no evidence for either long-term environmental degradation or agricultural change.

Although the stratigraphic evidence from A1–4 can be interpreted as indicating agricultural cycles of burning, erosion, and fallow, this inference is weakened by a failure to investigate alternative explanations for the periodic burning. For example, because coastal Halawa has limited rainfall (averaging less than 750 mm) and periodic droughts, it would seem that natural fires would be a distinct possibility. Or, nonagricultural uses of fire in the Pacific include burning for hunting and for encouraging nondomestic plant species.

The evidence for a chronological replacement of shifting cultivation by taro pondfield cultivation depends mainly on the interpretation of the pondfield excavations. In the pondfield profiles, a distinction is made between the upper horizon, indicating soils modified by irrigation, and the lower horizon, with charcoal and terrestrial gastropods indicating an earlier stage of shifting forest cultivation. However, an examination of the profile description provided in Appendix D suggests that they are compatible with descriptions of Hauula paddy soils (Cline 1955: 570–571). In other words, the full effects of *in situ* soil development have not been considered. Only the upper horizon is seen as indicating modifications from pondfield irrigation; however, during ponding the lower horizon would also be modified, producing a profile comparable with those described in the report. The charcoal and gastropods could be evidence as easily for the initial clearing of a forest for pondfield construction as for shifting cultivation.

To conclude this review, my reactions to the Halawa report were very positive but with some reservations in regard to specific conclusions. The volume shows a coming of age of ecological and community settlement pattern studies in Hawaiian archaeology. It includes a comprehensive presentation of the project's data which should be immediately usable for comparative studies. Equally important is the report's emphasis on developing local chronologies, necessary for both time-space systematics and processual studies. Although I question specific inferences drawn by the authors because alternative possibilities were not examined, I believe strongly that researchers should present their personal interpretations which can always be reevaluated with the accompanying data.*

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