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Authors

Kirch, Patrick V.
Green, Roger C.

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History, Phylogeny, and Evolution in Polynesia¹

by Patrick V. Kirch
and Roger C. Green

In the study of evolution, biological or cultural, a critical element is a methodology for distinguishing homologous from analogous characters. The historical knowledge this requires must be sought in the phylogeny of the modern groups under study and their common ancestors. Applying the methods for determining the phylogenetic relationships among cultural groups defined some years ago by Romney and Vogt to the results of recent archaeological and historical linguistic work, we examine Polynesia as a well-defined case of cultural "radiation" and divergence for which the specific shared ancestral traits can be distinguished from convergent developments in response to common selection pressures. We go on to present a series of hypotheses regarding some major factors underlying divergence and convergence in Polynesia in the hope that others will be stimulated to test them and thereby advance our understanding of this region.

PATRICK V. KIRCH is Director of the Thomas Burke Memorial Washington State Museum and Professor in the Department of Anthropology of the University of Washington (Seattle, Wash. 98195, U.S.A.). Born in 1950, he received his B.A. from the University of Pennsylvania in 1971 and his Ph.D. from Yale University in 1975. Before assuming his present position he was anthropologist with and, from 1982 through 1984, head of the Division of Archaeology of the Bernice P. Bishop Museum. His

1. This paper reflects initially independent responses on the part of the two authors upon reading certain theoretical parts of the Flannery and Marcus (1983) volume *The Cloud People*. One was recognition of the degree to which developments in Polynesian prehistory in the past three decades had formally fulfilled all the main conditions required for employing the methodology of the "genetic model." The other was the realization that in the Polynesian case it was now possible to specify more precisely than previously the main explanatory mechanisms or processes required to account for many of the similarities and differences encountered in the various Polynesian societies at the time of European contact. In short, a specific case of divergent, parallel, and convergent evolution within a distinct phylogenetic unit could be established. Green wishes to express his appreciation of being a Killam Visiting Scholar at the University of Calgary, where his ideas were presented in a seminar class in Problems in Oceanic Culture History, while Kirch wishes to acknowledge the stimulus of presenting an initial paper in the 1985 Society for American Archaeology symposium on evolutionary approaches in archaeology organized by T. Hunt and S. Studeman. The paper benefited from comments by Debra C. Kirch and two anonymous reviewers. Both of us also thank Susan Keech McIntosh for inviting us to submit the paper to CURRENT ANTHROPOLOGY and for her subsequent editorial assistance.

research interests are the archaeology and prehistory of Oceania, prehistoric agricultural systems, paleoenvironmental reconstruction, and human impact on island environments. He has published (with D. Yen) *Tikopia: The Prehistory and Ecology of a Polynesian Outlier* (Honolulu: Bishop Museum Press, 1982); *The Evolution of the Polynesian Chiefdoms* (Cambridge: Cambridge University Press, 1984); *Feathered Gods and Fishhooks: An Introduction to Hawaiian Archaeology and Prehistory* (Honolulu: University Press of Hawaii, 1985); the collection *Island Societies: Archaeological Approaches to Evolution and Transformation* (Cambridge: Cambridge University Press, 1986).

ROGER C. GREEN is Professor (Personal Chair) in Prehistory in the Department of Anthropology of the University of Auckland. He was born in 1932 and educated at the University of New Mexico (B.A., 1954; B.Sc., 1955) and at Harvard University (Ph.D., 1964). He has worked at the Bishop Museum (1967–70) and, as visiting professor, at the University of Hawaii (1981–82) and the University of Calgary (1985) and has held his present post at Auckland since 1973. His research interests lie in Oceanic culture history. He has published *Makaha Before 1880 A.D.* (Pacific Anthropological Records 31); with coeditor Marion Kelly, *Studies in Oceanic Culture History* (Pacific Anthropological Records 11–13); with coeditor J. M. Davidson, *Archaeology in Western Samoa* (Auckland Institute and Museum Bulletin 6 and 7); with coeditor M. M. Cresswell, *Southeast Solomon Islands Cultural History: A Preliminary Survey* (Royal Society of New Zealand Bulletin 11); and numerous articles in Oceanic prehistory and historical linguistics.

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Despite its roller-coaster-like history of popularity amongst anthropologists, the idea that evolution is fundamental to the understanding of cultural diversity predates even the Darwinian Revolution. Unfortunately, a resurgence of one kind of cultural evolutionism (often called neo-evolution) which still bore the shackles of Spencerianism ended up mired in a semantic morass of static evolutionary types—"bands, chiefdoms, states"—linked to concepts of orthogenesis and linear progress (Sahlins and Service 1960, Service 1962, Fried 1967, Carneiro 1970). Yet the anthropological goal of "understanding diversity" calls for an evolutionary theory of culture change, just as the explanation of organic diversity has depended upon neo-Darwinian theoretical advances (Dunnell 1980).

In his inimitable style, Flannery (1983b:362) recently declared that "if evolution is what you are interested in, then anthropology includes archaeology or it is nothing." To borrow further from his discussion, the promise of archaeology for the development of a cultural evolutionary theory is exactly analogous to that of paleontology in the study of biological evolution (cf. Dunnell 1982:21). We will take this analogy between archaeology and paleontology slightly further. Paleontology has made some of its most enduring contributions to evolutionary theory when it has concentrated upon the study of *divergence or radiation in groups of phylogenetically related lineages* (Gould 1980, Mayr 1982). George Gaylord Simpson's monographs on equid evolution (1951) and the insights which these brought to general patterns of adaptive radiation and speciation are a classic example. It is not just the ability to trace change over lengthy time periods that renders both paleontology and archaeology

powerful disciplines for evolutionary study; equally important is their ability to reveal the material conditions of variation and the selective forces that resulted in change and divergence. But more, as Gould (1986) has so eloquently argued, the success of the Darwinian concept of evolution rests upon the "triumph of homology"—the recognition that *history matters*. The significant contribution of paleontology (along with systematics in general) within the biological sciences is a rigorous methodology as well as the empirical evidence for establishing homologies, thus clearing the path for the analysis of evolutionary *process*. "Once we map homologies properly, we can finally begin to ask interesting biological questions about function and development—that is, we can use morphology for its intrinsic sources of enlightenment, and not as an inherently flawed measure of genealogical relationships" (Gould 1986:68). For the study of cultural evolution, archaeology (in conjunction with its sister historical disciplines, including comparative linguistics) offers the same promise of disentangling "homology and analogy, history and immanence," thus providing the foundation for addressing real issues of evolutionary process.

Unfortunately, in recent years archaeologists have tended to focus exclusively on a "systemic" approach which disregards history. Dunnell (1986:38–39) observed that a major element in the shift in American archaeology from "culture history" to the "new archaeology" was a focus on analogous, rather than homologous, similarities. "Explanations of analogous similarities have to be sought in laws that account for the appearance of particular forms under specifiable conditions rather than in the history of the data. . . . Predictably, the new archaeology took a dim view of diffusion and other processes that explain homologous similarities" (p. 39). What the new archaeologists failed to grasp is that the investigation of analogous similarity or convergence requires a prior methodology for establishing whether observed phenomena are indeed analogous. If not, presumed functional convergences may prove to be "just history." There cannot be a science of evolution, whether biological or cultural, that does not account for the historical issues (cf. Mayr 1982, Gould 1986).

Our view is that archaeologists who would understand cultural evolution as process not only must eschew Spencerian notions of "general" evolution in favor of a materialist (or nonessentialist) concept of "specific" evolution but will profit most by concentrating upon historically specifiable cases of divergence, where two or more phylogenetically related cultures (or societies, if one prefers) have arisen from a common ancestor. In such cases, where homology can be disentangled from convergent response to shared conditions, archaeologists can begin to apply evolutionary principles in a properly "timelike" explanatory framework, in which cause is ultimate and explanation is historical (Dunnell 1982:9). In this approach, it is explicitly recognized that the science of evolution—in which history matters, and in which *postdiction* rather than *prediction* is the key method of verification—is fundamentally different from the science of physics. It is, however, no less a science.

This approach is not wholly untried in archaeology. Flannery (1983a:2–3) cites a number of studies organized on the phylogenetic theme as the theoretical bases for the interpretation of the Zapotec and Mixtec civilizations (Flannery and Marcus 1983). Significantly, among the earliest studies cited by him are those of Goodenough (1955) and Sahlins (1958) for Polynesia. During the period from the late 1950s through the '70s, while Polynesian archaeology was developing a modern form and a coherent data set (Kirch 1982), interest in evolution on the part of prehistorians elsewhere in the world eschewed particularistic homology and centered on what Sahlins and Service (1960) termed "general evolution," the evolution out of a lower stage of successively higher levels of sociopolitical integration (e.g., Sanders and Price 1968 on Mesoamerica). These were frequently formed with the conceptual schemes of Steward (1955), Service (1962), Fried (1967), and Carneiro (1970), in which orthogenetic notions of "progress" (or "higher levels" of sociocultural integration) continued to dominate. In the Polynesian area, however, where the archaeological remains all seemed to fit comfortably within one such step or stage (i.e., as a series of Neolithic chiefly societies), such concerns had minimal impact, as they offered little promise of improving interpretation of the data being recovered. Rather, the Polynesian efforts of those decades fit more comfortably within that which Sahlins and Service (1960) termed "specific evolution," the tracing of particular historical trajectories of individual culture complexes over significant periods of time (sometimes also termed "multilineal evolution" [cf. Steward 1955]). We shall argue that the result of this has been to set the stage for a proper evolutionary approach in Polynesian prehistory. It is to the promise of Polynesian archaeology for unraveling both historical pathways and general processes of cultural evolution that we address the remainder of this essay.

The Phylogenetic Model

If we are to apply a phylogenetic model of evolution in archaeology and prehistory, we must have a clear idea of what is meant by a phylogenetic unit in cultural terms and how such a unit can be delineated. It simply will not do to declare any assemblage of seemingly related cultures an evolutionary "lineage" or "clade." Indeed, we must have a precise and rigorous methodology for the establishment of cultural homologies, just as morphological systematics, cladistics, and molecular phylogeny have provided a methodology for inferring biological history from its contemporary results. In fact, such a cultural evolutionary unit and the requisites for its empirical delineation were outlined some years ago by Romney (1957; see also Eggan 1954). Romney termed his approach the "genetic model," which "takes as its segment of cultural history a group of tribes which are set off from all other groups by sharing a common physical type, possessing common systemic patterns, and speaking genetically related languages" (p. 36). This "segment of cultural history," which Romney called the "genetic

unit," includes "the ancestral group and all intermediate groups, as well as the tribes in the ethnographic present" (pp. 36–37). In stressing that the "tribes" or groups under consideration should have demonstrable relationships linguistically, physically, and culturally (in "systemic pattern"), Romney outlined the criteria by which such a "genetic unit" could be rigorously defined (p. 36):

Physical type and language, we would say, have no causal relationship; there is no functional reason why a given physical type should occur with a given language family. Therefore, when these two variables do show significant concordance in their distribution this may well represent an important historical fact, namely that the explanation for their concordance can be traced to a common point somewhere in the past. A demonstration that these two factors are also uniquely accompanied by a systemic culture pattern . . . would strengthen the belief in a common origin.

Romney's propositions were substantially expanded and refined by Vogt (1964) in the introductory essay to a symposium on Maya cultural development (Vogt and Ruz Lhuillier 1964; see also review by Sanders 1966). Formalizing the "genetic model," Vogt stressed "the three factors that are taken to indicate a common historical tradition": (1) common physical type, in which there is convergence as one goes back in time; (2) common systemic patterns, "varieties of distinguishable systemic patterns characterizing earlier time levels within the genetic unit"; and (3) genetically related languages, "variations of what was the same language of the common ancestral group" (1964:10–11). Vogt expanded on the implications of the genetic model as follows (pp. 11–12):

In brief, the genetic model assumes that genetically related tribes, as determined by related languages, physical types, and systemic patterns, are derived from a small proto-group with a proto-culture at some time in the past. The model resembles that of the zoologist who views a certain species of animal as evolving and making an adaptive adjustment to a given ecological niche and then radiating from this point as the population expands into neighboring ecological niches. As the population moves into different ecological settings, further adaptive variations occur in the species. But these variations are traceable to the ancestral animal, or, in other words, back to the proto-type.

In the genetic model, as applied to human populations, we assume that a small proto-group succeeds in adapting itself efficiently to a certain ecological niche and in developing certain basic systemic patterns which constitute the basic aspects of the proto-culture. If the adaptation proves to be efficient, the population expands, and the group begins to radiate from this point of dispersal. As members split off from the proto-group and move into neighboring ecological niches, they make appropriate adaptations to these new situations and begin to differentiate—

that is, there are adaptive variations from the proto-type over time as the members of the genetic unit spread from the dispersal area.

Despite the obvious emphasis in Vogt's discussion on ecological adaptation as the driving force behind divergence within the genetic unit, he also mentions "culture contact" with other groups and "internal biological, linguistic, and cultural 'drifts'" as general factors leading to change. Vogt stops short, however, of developing the full evolutionary implications of the genetic model, ignoring such critical concepts as variation and selection.

Its shortcomings notwithstanding, Vogt's essay defines a series of analytical steps by which a genetic unit can be precisely and unambiguously delineated. As Vogt notes, these steps involve "the combined use of a number of linguistic, archaeological, physical anthropological, ethnological, and historical methods bringing to bear the full range of anthropological data as these become available from field and archival research" (1964:12). Stressing the analytical rigor of comparative and historical linguistics, Vogt proposes that analysis begin with "the definition of genetic units in terms of genetically related languages." The steps of analysis are then (1) plot the distribution of related languages; (2) calculate time depth, using lexicostatistics and glotto-chronology; (3) locate the dispersal area and spread of the protogroup; (4) reconstruct the protolanguage and protoculture utilizing the linguistic methods of lexical reconstruction; (5) use archaeological data to test the hypotheses generated by steps 3 and 4; (6) check the sequence of divergences derived from linguistic and archaeological data with the evidence of physical anthropology; (7) use ethnohistorical materials to "provide readings on the various branches of the genetic unit" between the time of European contact and the present; and (8) add ethnographic data on contemporary communities to "map variations in systemic patterns that have survived from earlier time levels and to detect cultural 'drifts' or trends that are still occurring in these living systems" (p. 13).

These analytical steps constitute a methodology for the delineation of an evolutionarily meaningful unit whose subunits can be demonstrated to have diverged from a common ancestor according to a historical sequence which can be precisely defined in both time and space. We believe that the choice of the term "genetic unit" is unfortunate, however, as it may easily lead to confusion between cultural and strictly genetic or biological evolutionary processes (a problem obviously foreseen by Romney in his caveats [1957:37]). We prefer the term "phylogenetic unit," which places the emphasis on the essential aspect of the model—the delineation of phylogenies or historical sequences of divergence from a common ancestor—and use this term below.

Polynesia as a Phylogenetic Unit

Islands are inviting theaters for the study of evolutionary divergence, as the Galapagos finches or Hawaiian

achatinellid tree snails have demonstrated in zoology. Addressing the problems of biological and cultural evolution among human populations, Goodenough (1957:146) observed that on continents "it is very difficult to explore cultural change within the framework of what biological evolutionists have called 'radiation'; that is, to examine critically the processes by which phylogenetically related cultures become progressively different from each other." Goodenough then pointed up the value of island societies which provide instances of "cultural radiation unaffected by external contacts" (p. 154). Sahlins also recognized the potential value of Polynesia for understanding cultural evolution and invoked a direct biological analogy in describing Polynesian societies as "members of a single cultural genus that has filled in and adapted to a variety of local habitats" (1958:ix). Significantly, he used the term "genus" with its full phylogenetic implications. In his now classic work on Polynesian social stratification, Sahlins attempted an explanation of Polynesian structural variation in terms of such an "adaptive radiation" model. Yet his study, carried out just when Oceanic archaeology was beginning to enjoy a renaissance, was restricted to the use of static ethnohistoric data and thus lacked not only temporal control but any rigorous methodology for distinguishing between homologous and analogic features of particular Polynesian societies. Consequently, it was more functional than evolutionary, attempting to correlate "types" of social structure (descent-line and ramage systems, principally) with corresponding sets of environmental conditions.

Nearly three decades later, Polynesian scholars find themselves in a more enviable position. Still commanding all the advantages that islands offer—discrete, geographically isolated units with contrastive environmental conditions—they can now employ a substantial body of temporally controlled data on prehistoric change and on historical relationships between individual Polynesian societies. Not all of these data are strictly archaeological in origin. Historical linguistics has had a very significant role to play, primarily because it provides a relatively precise, *independent model of the phylogenetic relationships* between the approximately 40 Polynesian societies (Pawley 1966, 1967; Green 1966, 1985; Clark 1979). The subgrouping model of Polynesian languages, with their divergence from a Proto-Polynesian language sometime after 1000 B.C., accords closely with archaeological evidence and with properly genetic models of population distance based on both metrical and nonmetrical skeletal traits (Pietruszewsky 1970, 1971).

As reviewed above, Romney (1957) and Vogt (1964; see also Sanders 1966) established the main elements of a methodological framework for phylogenetic analysis of cultural groups, that is, a method for precisely inferring cultural history from its results, establishing phylogeny, and permitting the disentanglement of homology from analogy. What has been accomplished in Polynesian archaeology over the past three decades is precisely to fulfill most of the conditions specified by Vogt as neces-

sary to the employment of this framework. The first element is that the tribes, societies, or ethnic groups of "Triangle Polynesia" (see fig. 1) share a physical type (Howells 1979, Houghton 1980), systemic cultural patterns (Burrows 1939a, Sahlins 1958, Goldman 1970), and historically related languages (Biggs 1971, Clark 1979) which allow them to be grouped together as a *unit of historical analysis* or, as Romney (1957:36) calls it, a "substantive segment of cultural history," i.e., a phylogenetic evolutionary unit.

Other societies which are also called "Polynesian" but sometimes fail to exhibit these characteristics in all three respects are grouped together under the term Outlier Polynesia (fig. 1). Though they certainly have a common linguistic ancestry with the other Polynesian societies (Pawley 1967), they share a variety of cultural, archaeological, and physical anthropological features with their Micronesian and Melanesian neighbors (Bayard 1976; Kirch 1984b, 1985b; Blake et al. 1983; Howells 1973). This situation is the result of significant historical contact with these unrelated or distantly related Micronesian/Melanesian societies (Kirch 1986b). The Outlier populations are thought to have originated in the West Polynesian area, after the development of a distinctive Polynesian race, language, and culture there, and to have been "blown back" on drift voyages into Micronesia/Melanesia, where they established themselves on some of the smaller and often more remote islands of these regions (Kirch 1984b; Ward, Webb, and Levison 1973).

The second element of the phylogenetic framework requires that there be convergence as the history of each group is traced back in time and that this be exhibited in physical type, language, and systemic cultural pattern. This is certainly the situation for the societies of East Polynesia, which converge into a recognizable East Polynesian language subgroup (Green 1966, 1985; Clark 1979), an archaic East Polynesian culture (Bellwood 1979, Kirch 1986a), and a common physical type (Pietruszewsky 1970, 1971). These in turn converge with the West Polynesian groups to form a Proto-Polynesian language (Pawley 1966; Biggs 1971, 1979), an ancestral Polynesian culture or society (Kirch 1984a:41–69), and a parental Polynesian population (Howells 1979; Houghton 1980; Pietruszewsky 1970, 1971). (In order to avoid confusion between reconstructions of language, culture, and biological populations—each of which derives from separate kinds of data—we use the terms *protolanguage*, *ancestral culture*, and *parental population*.) Moreover, the various societies termed Polynesian are all descended from a relatively small group that resided in a fairly restricted area. On present archaeological evidence this includes the West Polynesian island groups of Tonga, Samoa, 'Uvea, and Futuna. Thus, a Polynesian homeland developed in this region, linguistically and culturally, out of a more widespread and earlier Lapita cultural complex (Green 1981) and a dialect chain of Central Pacific languages (Geraghty 1983). A similar situation genetically is not so satisfactorily attested but can be argued theoretically (Terrell n.d.) and is recently

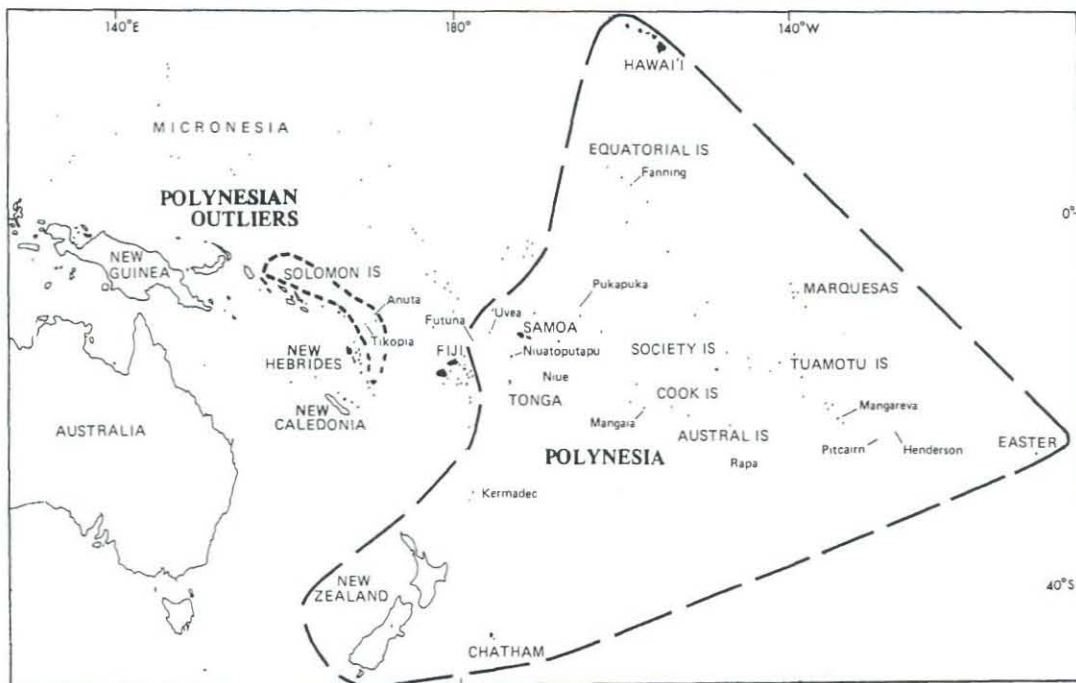


FIG. 1. *The distribution of Polynesian societies in Oceania.*

demonstrated by Western Lapita burials from Watom Island (P. Houghton, personal communication, 1986). Through migration, descendants of this original Lapita group spread over an extensive territory, while Polynesia (including Outlier Polynesia) and the original unit became internally differentiated biologically, linguistically, and culturally.

In Vogt's view, a cultural phylogenetic unit must be defined linguistically because of the relative exactness of linguistic methods as compared with those of ethnography and archaeology. This is certainly true for the Polynesian case, where all of the daughter languages and dialects belong to a linguistically well-marked subgroup in the Central Pacific subgroup of Oceanic Austronesian languages. Thus every case of a society considered for inclusion has a Polynesian language as the primary basis for defining it as suitable for consideration, although selected cultural attributes or physical characteristics which distinguish a Polynesian genotype and phenotype could also be set out. In many other areas of the world where isolation as a factor is not so great as in Polynesia and contacts with unrelated or distantly related societies and cultures are more frequent, this methodological principle may be more difficult to apply.

We will now review the analytical steps for the definition of a phylogenetic unit in terms of what has been accomplished for Polynesia within the past three decades.

1. *Plot the distribution of the related languages.* From the geographical distribution of Polynesian languages shown in figure 1, it can be seen that the languages of the "Polynesian Triangle" form a cohesive geographic unit in which *only* Polynesian languages are present. In con-

trast, the languages of the Polynesian Outliers are distributed along the northern and eastern fringes of Melanesia, in close proximity to a number of other Oceanic languages. This heightens the probability of external contact and linguistic borrowing at various times in their respective histories. Figure 2 shows the subgrouping of Polynesian languages in terms of their branching historical relationships, a key element in establishing phylogenetic relationships between various Polynesian groups.

2. *Calculate the approximate time depth of the linguistic differentiation.* Clark (1979:62-63), on the basis of lexicostatistics and glottochronological considerations, puts the separation of Proto-Polynesian from Proto-Fijian (based on Bauan Fijian) at 1500 B.C. and the breakup of Proto-Polynesian at around A.D. 1. Green (1981), in a more closely argued case based on both archaeology and linguistic evidence, agrees that Proto-Central Pacific and its breakup date to ca. 1500 B.C. but suggests various dates in the middle of the 1st millennium B.C. as more probable for Proto-Polynesian before it split into Tongic and Nuclear Polynesian branches. This is because the breakup of Nuclear Polynesian into Samoic Outlier and East Polynesian can be reliably argued to have begun by about the 1st century A.D. if not before. Similarly, Kirch (1984a) argues that an archaeologically recognizable ancestral Polynesian society existed by ca. 500 B.C. In short, becoming Polynesian linguistically (the pre-Polynesian stage) took place between 1500 and 500 B.C., and languages and dialects we would today identify as Polynesian existed only after ca. 500 B.C. and have retained markers of that status ever since.

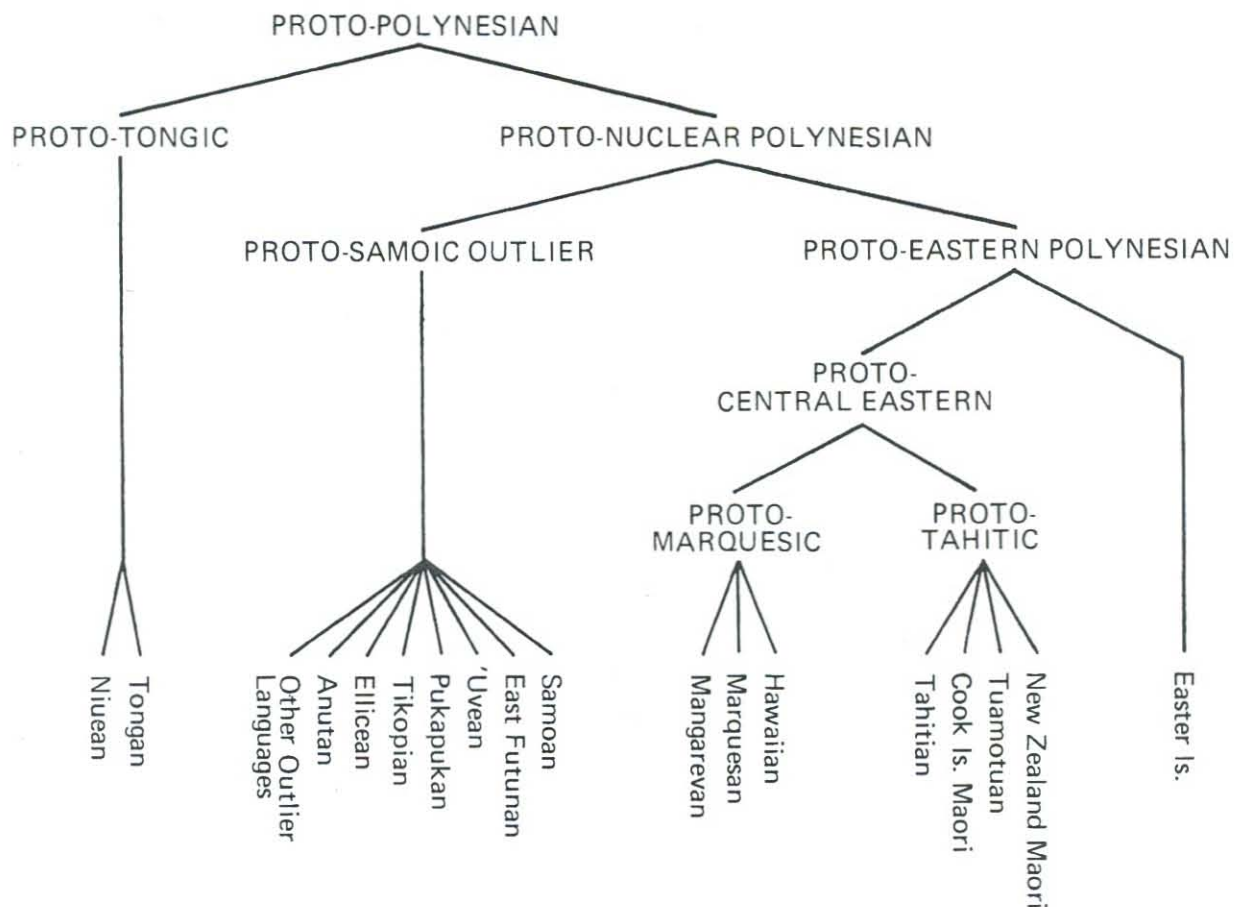


FIG. 2. The subgrouping of the Polynesian languages (after Biggs 1971).

3. Define the origin and the history of migrations. The differentiation of the Polynesian language and its ancestral culture from the Proto-Central Pacific antecedents and the Eastern Lapita cultural complex in the Fiji–West Polynesia region can be placed firmly in the Tongan, Samoan, Futunan, and 'Uvean subregion of West Polynesia. This placement requires the application of a “network-breaking” model (Pawley and Green 1984) to a chain dialect situation, first at the Tokalau Fijian–Polynesian level, when part of the Fijian Lau Islands and related areas were part of a more homogeneous region that included Polynesia and set it off from the rest of Fiji to the west (Geraghty 1983, Green 1981, Best 1984). A further break with Tokalau Fijian from the Polynesian dialects set the former off from those of the then occupied islands of West Polynesia at what is now the ethnographically and culturally established boundary between Melanesia (i.e., Fiji) and Polynesia. In the archaeological record this boundary becomes well marked in material culture such as adzes and ceramics at ca. 200 B.C. (Green 1981: 149–50; Best 1984). From this homeland, which under the density-dependent networking model employed is a region in which divergence develops rather than a single island group, migrations move west to the Outliers and east into East Polynesia (fig. 3).

Although we cannot here review all of the archaeological evidence upon which this scheme is based, we stress that it derives from a substantial corpus of excavated, radiometrically dated materials (Bellwood 1979, Jennings 1979, Kirch 1984a).

4. Reconstruct the protolanguage and the protoculture from the linguistic data. Biggs (1979) has reconstructed about 3,000 Proto-Polynesian lexical items and their common meanings, and from it Pawley and K. Green (1971) have drawn a carefully selected data set bearing on the main outlines of the protoculture. To this, cultural items have been progressively added (Kirch 1984: 41–69; Green 1986).

5. Test the linguistic reconstructions with archaeological data. Kirch (1984a) has coordinated these lexical reconstructions of Proto-Polynesian with the data of archaeology to define an ancestral Polynesian culture, and Green (1986) has set out more formally the strategies by which this may be done using linguistic, ethnological, and ethnohistoric as well as archaeological data. Furthermore, the sequence of dispersal and colonization leading to the modern distribution of Polynesian groups has been thoroughly investigated archaeologically and essentially confirms the linguistic subgrouping model (Jennings 1979, Kirch 1984a).

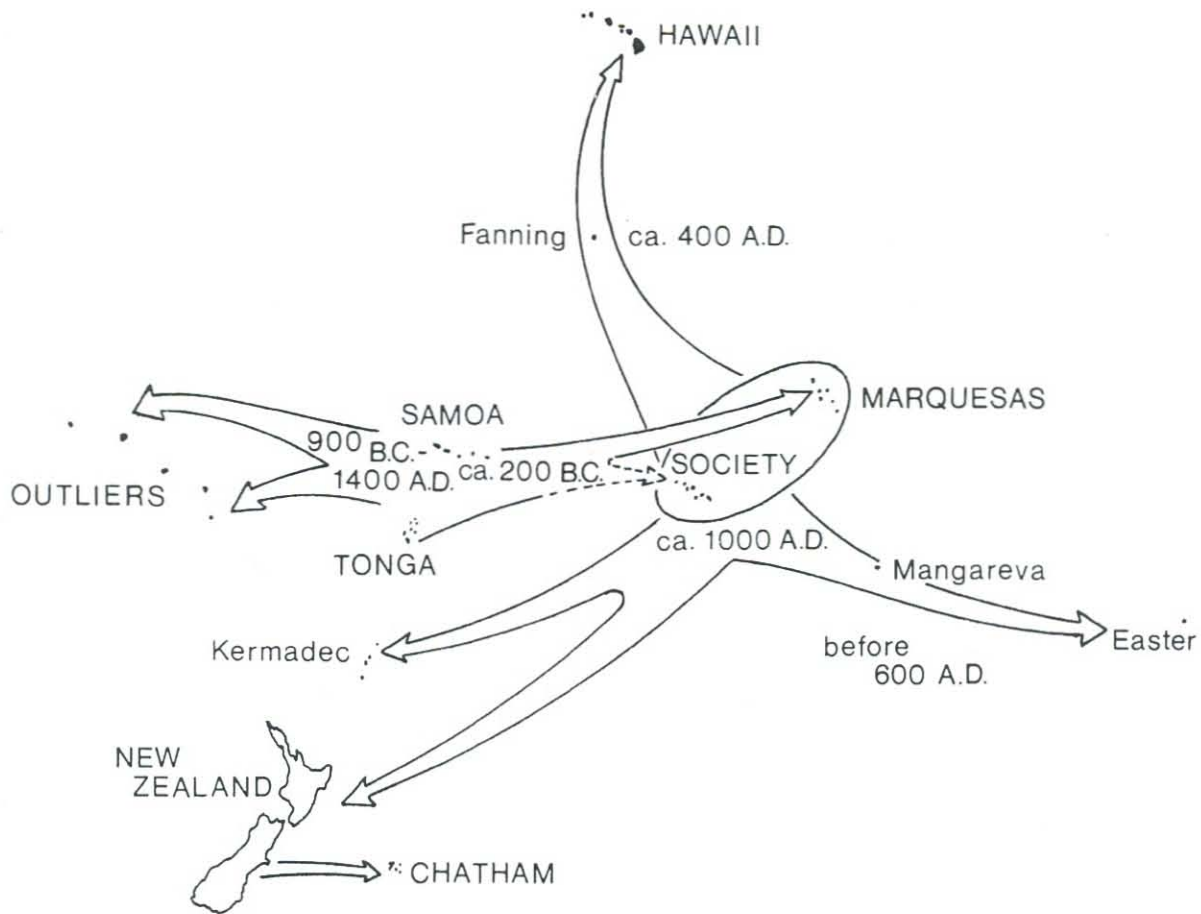


FIG. 3. Schematic summary of the dispersal of Polynesian peoples based on current archaeological evidence (after Kirch 1984a, with modifications).

6. *Test the reconstruction with physical anthropological data.* Burials with physical remains of this age have yet to be recovered by archaeologists in early sites in West Polynesia, but similarity and distance analyses of various late prehistoric and historic Polynesian populations produce genetic subgrouping models that closely match the linguistic and archaeological pictures (Pitrusewsky 1970, 1971; Howells 1979; Houghton 1980).

7 and 8. *Utilize ethnohistoric and ethnographic data to analyze systemic cultural patterns.* In the works of Burrows (1939a), Sahlins (1958), and Goldman (1970) we have appropriate examples of what can be done in the way of analyzing divergences among systemic patterns in existence today among the various Polynesian societies. What is lacking in these studies, however, is a full appreciation of the changes occurring between European contact and the ethnographic recording of the various Polynesian societies known in the 19th and 20th centuries. Correction of this bias through careful evaluation of the ethnohistoric sources is now taking place with the work of such scholars as Oliver (1974) in Tahiti, Salmund (1975) in New Zealand, Sahlins (1981, 1985) in Hawaii, and Dening (1980) and Thomas (1986) in the Marquesas. A further recent example, which goes be-

yond Polynesia to incorporate a wider sample of Oceanic societies, is the study by Marshall (1984) of structural patterns of sibling classification. Marshall's paper points to the great potential of controlled comparisons of systemic patterns within phylogenetic units.

In sum, specific evolutionary divergence within the Polynesian phylogenetic unit can now be studied simultaneously from several perspectives. Polynesian scholars are in a good position to address *processual* questions in cultural evolution, since the empirical basis for determining which Polynesian cultural similarities are simple homologies of a contingent history and which are convergences (independently evolved analogies) in response to common or repeated functional problems is now in place. With the branching relationships between Polynesian societies firmly established, it is possible to use the older ethnographic and ethnohistoric data in a more rigorous, controlled fashion, as, for example, in the search for convergent structures or for persistent or homologous traits reflecting a common ancestor. Historical linguistics further aids in the lexical reconstruction of proto-forms, including their semantic values, thus aiding in the delineation of variability in the ancestral group. But it is archaeology that contributes temporally

ordered sequences of change and their environmental contexts, that is, the direct evidence of evolutionary change.

The Polynesian Evolutionary Record

Demonstrating that Polynesia constitutes a phylogenetic unit is only the first stage in assessing its potential for evolutionary analysis. Of equal significance is the nature of the evolutionary record itself; have we available what paleontologists would call a "dense and continuous" record of change, not only in the Polynesian archaeological record but in the paleoecological record of environmental contexts for human action? We will briefly review the Polynesian evolutionary record, beginning with the common ancestral group, which we term "ancestral Polynesian society."

ANCESTRAL POLYNESIAN SOCIETY

To understand and ultimately explain evolutionary change in Polynesia requires that the common ancestral culture or society be precisely delineated and extensively reconstructed. The origins of ancestral Polynesian society (Kirch 1984a) by ca. 500 B.C. out of an earlier Lapita cultural complex are now fairly well understood and have been briefly summarized above. The importance of accurately reconstructing this ancestral society is underscored by the observation that "each culture is a product not merely of its current adaptation but of its past history" (Flannery 1983a:3; see also Sahlins 1976:23; Friedman 1979:32; Kirch 1984a:7). If we are to understand evolution, we must first know what is original and was selectively retained.

The reconstruction of ancestral Polynesian society has been advanced using not only the direct evidence of archaeology but also lexical reconstruction and controlled ethnographic comparison (see Kirch 1984a, Green 1986). The material culture and technology are now well documented, both archaeologically and lexically. Archaeologically, detailed temporal sequences of change in ceramics, adzes, and unretouched lithics have been developed which document the emergence of ancestral Polynesian society out of a more generalized Lapita ancestor.

In the area of subsistence, an earlier debate about whether ancestral Polynesians were agriculturalists (Groube 1971) has been resolved, and the importance of both the taro-yam complex and certain tree crops such as breadfruit can be argued from the lexical and ethnobotanical evidence (Yen 1973). The triad of domestic animals—pig, dog, and fowl—has been shown to be a component of ancestral Polynesian production. Advances have also been made in our knowledge of the range of marine exploitation strategies practiced by ancestral Polynesian populations. Certain specialized production technologies, such as anaerobic pit fermentation and ensilage of starch pastes, have been documented both archaeologically and lexically (Kirch 1984a). Ancestral

Polynesian settlement patterns are less well understood, although the lexical evidence suggests a variety of functionally differentiated structures (Green 1986). More direct archaeological work, however, needs to be devoted to the structure and variability of ancestral Polynesian household units.

It is also possible to make a number of statements regarding ancestral Polynesian social relations, largely on the evidence of lexical reconstruction and controlled ethnographic comparison. Understanding ancestral social structure is especially critical to an analysis of Polynesian cultural evolution, since much of the subsequent divergence in Polynesian groups was in such areas as descent-group structure, rank, stratification, and territorial control (Sahlins 1958, Goldman 1970). Hereditary chieftainship was certainly an aspect of ancestral Polynesian society, a structure which is probably traceable to the even older Lapita ancestral group (Hayden 1983; Pawley 1982, n.d.). Aside from the hereditary chief, other social statuses are lexically reconstructable (e.g., expert or specialist, PPN **tufunga*; warrior, **toa*; and navigator or expert seaman, **tautai*). A conical clan structure (Kirchhoff 1955) and two specific kinds of social group are indicated lexically: a landholding descent group (PPN **kainanga*) headed by a hereditary chief and a smaller, minimal residential group (PPN **kainga*).

It is not our aim to review in detail the nature of ancestral Polynesian society, other than to demonstrate that prehistorians can now reconstruct the key elements of the common ancestral society from which all later Polynesian groups diverged. We want to stress, however, that not enough is yet known of the *range of variation* in that society. It is important that in reconstructing ancestral Polynesian society we avoid a normative, essentialist characterization which masks significant variation. We already know that variation existed, for example, in ceramics (Kirch 1981), and a knowledge of such variation may provide essential keys to the first stages in evolutionary divergence.

THE PERSISTENCE OF ANCESTRAL PATTERNS

Because the phylogenetic relationships between Polynesian cultures are now well understood and because we have been able to reconstruct much of the ancestral culture, linguistically as well as archaeologically, Polynesian prehistorians are able to make reasonable assertions about the persistence of specific ancestral patterns in descendent groups, that is, about homologous structures and patterns. The numerous linguistic reconstructions possible from the modern daughter languages of Polynesia are ample attestation of just how pervasive such items are. In the area of art, Green (1979) has shown how the style of decoration on Lapita pottery survives into ethnographic examples in Polynesian bark cloth and tattooing and may be explained by common inheritance. Likewise, many of the basic structural elements of each of the chiefly Polynesian societies reflect just such homologies. As Kirch (1984a:281–82) demonstrates, the office of chief, the lineage concepts associ-

ated with it, the fundamental notions of *mana* and *tapu*, and the category of warrior and the attendant focus on status rivalry were not invented anew by each Polynesian society. Rather, these aspects of ancestral Polynesian society were all inherited by descendent populations, even as they were transformed into new variations.

Some traits may offer no selective advantage, that is, may be adaptively neutral (Dunnell 1980, Kirch 1980a). We suspect, however, that most of the strongly persistent aspects of Polynesian social structure mentioned above, as well as others in the areas of technology and subsistence, were successful in the long run precisely because of their selective value. That is to say, the existence of these traits conferred greater fitness on the population, ultimately measured as reproductive success. There are, within Polynesia, several cases of populations which either became extinct or abandoned their island environments (Henderson, Pitcairn, Nihoa, Necker, Raoul, and others [see Kirch 1984a:89–92]). These situations should provide ideal cases for examining the evolutionary or adaptive plasticity of Polynesian culture in the face of relatively extreme or harsh selection pressures.

DIVERGENCE: THE ARCHAEOLOGICAL RECORD

An evolutionary analysis requires a “dense and continuous record” of variation; we now briefly review the current status of this record for Polynesia in terms of four major geographic regions which themselves represent major phylogenetic segments.

West Polynesia. The archipelagoes of Tonga and Samoa, along with several smaller islands such as Futuna and ‘Uvea, constitute the “homeland region” in which ancestral Polynesian society developed and from which other subgroups diverged. Consequently, they display the longest archaeological sequences in Polynesia. At present, the most finely divided and detailed archaeological record of change is that for Samoa (Green and Davidson 1969–74, Jennings et al. 1976, Jennings and Holmer 1980), rivalled only by that for the smaller island of Niuatoputapu (Kirch 1978). The early portions of the Tongan sequence are relatively well known, but the archaeological evidence for the last 2,000 years of Tongan prehistory is sketchy (Kirch 1984a:219–20). The smaller islands of Futuna and ‘Uvea have also been archaeologically surveyed, and their sequences are known in part (Kirch 1981, Frimigacci, Siorat, and Vienne 1984). Active fieldwork continues in all areas, however, and the archaeological record promises to become increasingly fine-grained.

The Outliers. Until recently, the Polynesian Outliers were a virtual lacuna of anthropological, let alone archaeological information. In the past few years, however, the archaeological sequences of no fewer than five outliers (Nukuoro, Kapingamarangi, Anuta, Tikopia, and Taumako) have been revealed in substantial detail (Kirch 1984b). The archaeological record of Tikopia may be singled out as especially relevant for the study of evolutionary

change, and indeed, the Tikopian sequence has already been analyzed in explicitly evolutionary terms (Kirch and Yen 1982).

Central East Polynesia. The archipelagoes of the Cook, Society, Tuamotu, Marquesas, Austral, and Mangareva islands constitute a “core” region within East Polynesia which includes the area of initial divergence of ancestral East Polynesian culture. Of these groups, the archaeological record is most complete for the Marquesas (Suggs 1961, Sinoto 1979) and the Societies (Emory 1979), more spotty for the remaining groups (see Kirch 1986a). With the exception of the initial colonization phase, the Marquesan archaeological record is indeed “dense and continuous,” with a wealth of data on changing technology and material culture, settlement patterns, subsistence systems, social grouping, and ritual practice.

Marginal East Polynesia. The marginal islands and archipelagoes of East Polynesia, which were settled from the central area just discussed (see fig. 3), include Hawaii, New Zealand, and Easter Island, as well as the smaller Chatham group, Pitcairn, Henderson, and various others. Because they have long been loci of scholarly institutions, both New Zealand and Hawaii have enjoyed more archaeological investigation than any other Polynesian group (Davidson 1984, Kirch 1985a). Their archaeological records are exceedingly “dense and continuous,” well suited to a variety of evolutionary studies. The later prehistoric record for Easter Island is ample and rapidly increasing (Ayres 1975, McCoy 1979, Cristino and Vargas 1980), but little is known of the earliest phases of settlement. For the remaining islands, the records are spotty or incomplete.

In sum, three decades of intensive archaeological work have provided a mass of detailed temporal data on evolutionary change in Polynesia. While there are still gaps to be filled, each major geographic segment of Polynesia has at least one and usually more than one island or archipelago for which the record can already be characterized as continuous or finely divided, as well as rich in the diversity of available data (fig. 4). While it will be necessary to fill certain empirical gaps in the course of testing particular hypotheses, we conclude that Polynesian scholars are well positioned to ask meaningful questions about evolutionary process.

Evolutionary Process: Some Initial Propositions

Having argued the importance of history and phylogeny in cultural evolution and demonstrated that Polynesian phylogenetic relationships are now well understood (thus allowing the disentanglement of specific homologies from convergences), we now turn to the matter of evolutionary *process* in Polynesia. Specifically, we address what we believe to be key factors leading to both *divergence* and *convergence* between Polynesian societies. The propositions which follow are intended not as conclusions but as a set of *hypotheses of evolutionary*

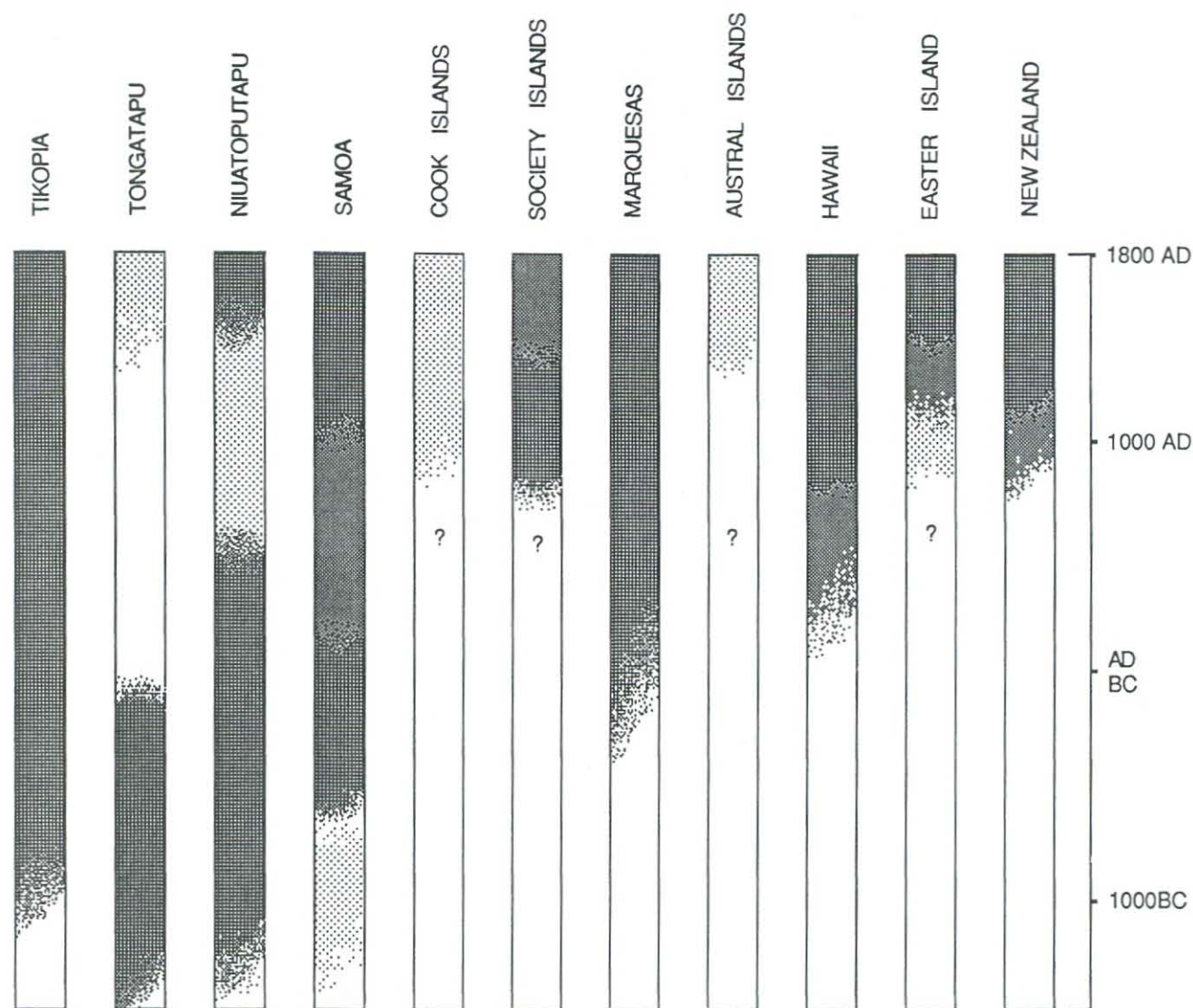


FIG. 4. The quality of archaeological data for key Polynesian island groups. Darkest shading indicates densest archaeological information.

process which we hope others will be stimulated to test on the rich archaeological data of Polynesian prehistory.

MECHANISMS OF DIVERGENCE

Isolation. Among the mechanisms of divergence which we can recognize within Polynesia, the most fundamental is isolation, the key factor which renders oceanic islands ideal theaters for phylogenetic studies. Colonization of a new island meant the immediate separation of the founding propagule from its "mother" population and the initiation of "allopatric" differentiation (cf. Mayr 1942). Isolation, however, is a quantitative phenomenon which varies tremendously throughout Polynesia. At one end of the spectrum are extremes such as Easter Island and the Chathams, apparently completely isolated following initial settlement. The Hawaiian Is-

lands and New Zealand were also relatively isolated but may have had some secondary contact with other Polynesian groups (Finney 1977). At the other extreme, some central East and West Polynesian archipelagoes had significant and continuous contacts with neighboring groups throughout prehistory; the significance of such low-level isolation will be further discussed below.

The founder effect. The importance of random-sampling effects or "drift" in the divergence of two isolated populations has long been recognized (Mayr 1942) and, as Cavalli-Sforza and Feldman (1981) argue, is doubtless a significant mechanism for cultural evolution as well. In islands, drift resulting from the colonization of a new island by a propagule which represents a subsample of the mother population has been termed "the founder effect" (Dobzhansky 1963:71-72). Vayda and Rappaport (1963:134-35) described the mechanism suc-

cinctly: "if the migration to an isolated place, whether a small island or a large continent, is by a relatively small group of people who are unable to reproduce in full the culture of the population from which they derived, then the culture in the new place will be immediately different from the culture in the homeland." In Polynesia, the founder effect played a role in initial divergence between colonizing groups, both culturally and biologically (cf. Houghton 1980; Terrell n.d.). The founder effect is archaeologically recognizable in technological differences between founding groups and their homeland equivalents (most striking would be the absence of domestic animals, such as pigs and dogs from Easter Island). A systematic analysis of the founder effect in Polynesia should now be possible on the basis of the detailed comparison of archaeological colonization assemblages from a variety of islands.

Colonization processes. The significance of drift or random assortment may have been overshadowed by other contingencies of colonization, notably the selective pressures imposed by new environments. The Polynesian region incorporates significant environmental variation, ranging from atolls to high islands to subcontinents, from tropical to temperate to subantarctic climates, from extensive tropical reef lagoon systems to impoverished rocky coasts. The colonization of a new island often meant a radical shift in environment, in turn requiring an immediate reassortment or segregation of technology and adaptive strategy (Yen 1973; Kirch 1984a:87-95; Davidson 1984). Many of the Polynesian archaeological assemblages which are assignable to colonization phases exhibit remarkable variation in material culture (e.g., the Marquesan Colonization Phase assemblages or those of the New Zealand "archaic"), interpreted as a reflection of experimentation and innovation in the face of new environmental constraints (Kirch 1980a:115-17).

Such behavioral and technological innovation is subject to selection (both natural and cultural [see Cavalli-Sforza and Feldman 1981]) in that innovations perceived as successful (cultural selection) or those which contribute to population fitness (natural selection) are differentially transmitted. An archaeological example of selection and differential transmission over time can be found in East Polynesian fishing technologies. The sudden proliferation of fishhook types in the Marquesan colonization period and the longitudinal transmission and persistence of only a limited subset of this initial variation offers an unambiguous instance of selection contingent upon colonization (Kirch 1980a, b; 1984a:88-89). Further examination of fishhook variation throughout other Polynesian archipelagos in this light seems promising, as does work with other aspects of technology, subsistence adaptation, and settlement pattern.

Area effects, especially the relationship between size, carrying capacity, and time to extinction, are well recognized by island biogeographers as a significant aspect of oceanic evolution (MacArthur and Wilson 1967:68-93; Williamson 1981:57-67; Keegan and Diamond n.d.). We

are reminded again of the cases of prehistoric extinction on several Polynesian islands. No fewer than 12 islands are known from archaeological evidence to have been colonized by Polynesians only to be abandoned later, prior to their rediscovery by Europeans (see Kirch 1984a: table 9). These include the well-known Pitcairn Island of *Bounty* fame, Nihoa and Necker in the leeward Hawaiian chain, a number of equatorial atolls, and Raoul in the Kermadecs. With the exception of Christmas Atoll, none has a land area exceeding 40 km²; Nihoa and Necker, with substantial archaeological evidence of Polynesian settlement, have areas of only 0.6 and 0.2 km². Compounding these areal limitations are a range of other ecological constraints, such as low supplies of fresh water, the absence of reef or lagoon resources, lack of timber, and, in the case of Raoul, volcanic eruptions. When the ecological limitations of these islands are considered, what is most surprising is not that their Polynesian populations became extinct or abandoned them but that the effort at colonization was made in the first place.

The full archaeological potential of these small islands for elucidating sequences of attempted colonization followed by extinction or adaptive failure has yet to be taken advantage of. In the case of Henderson Island, however, excavations by Sinoto (1983; see also Kirch 1984a:90-93, fig. 23) revealed a local occupation sequence in which a remarkable technological adaptation to the environmental constraints of this upraised limestone (*makatea*-type) island is documented in fishing gear, adzes, and other tools. That the small Polynesian population was intensively exploiting the limited faunal resources on Henderson in an effort to survive is further suggested by the faunal evidence for the local extinction of a storm petrel and two pigeon species (Steadman and Olson 1985). Stratigraphic excavations on other abandoned Polynesian islands, such as Nihoa and Necker, have the potential to reveal much about the processes of island colonization under the most marginal conditions.

Area effects and ecological constraints were significant in cases of successful colonization as well. It is surely no coincidence that the most highly stratified Polynesian societies arose in large, resource-rich archipelagos or that atoll societies with the most limited resources generally remained at the lowest levels of sociopolitical complexity and integration (Sahlins 1958). It is essential, however, not to take these environmental correlations to the extreme of environmental determinism; in Polynesia, as elsewhere, local ecosystems posed constraints and offered possibilities, but it was culturally directed human actors who were the active agents of sociopolitical change.

Long-term environmental selection. While colonization often entailed radical shifts in environmental parameters, the selective effects of environment were not limited to the early stages in island settlement. Island biologists and anthropologists alike have recently become aware of just how dynamic insular ecosystems have been in the Holocene (see Kirch 1984a:123-51 for a review of recent evidence). Some environmental

changes, such as tectonically induced shoreline modifications, were directional, while others, such as rainfall perturbations and cyclonic hazards, were stochastic. Probably more significant was the permanent modification of island habitats by colonizing human populations themselves. Deforestation, faunal depletion, and erosion are recurrent themes in Polynesian environmental prehistory with major evolutionary implications (Spriggs 1986, McGlone 1983, Flenley and King 1984, Olson and James 1984). The initial elaboration of the unique statuary complex of Easter Island—what Sahlins (1956) termed an instance of “esoteric efflorescence”—could perhaps be ascribed in strictly evolutionary terms to drift combined with cultural selection (in the sense of Cavalli-Sforza and Feldman 1981). But the subsequent collapse of the entire statuary cult and its sociopolitical context in late prehistory cannot be understood without a knowledge of the extreme environmental selection pressures which themselves were an artifact of human-induced deforestation and erosion combined with dramatic population increase (McCoy 1976; Flenley and King 1984; Kirch 1984a:264–78).

External contact. Isolation is relative in Oceania, and external contact played varying roles in the evolutionary trajectories of specific societies. There has been a tendency to extoll the “laboratory-like” virtues of oceanic islands that accrue from isolation, but this notion of closed systems can be carried too far. To some degree, the significance of external contact can be predicted by a knowledge of geographic isolation (in which both distance and configurational effects must be taken into account). It is, however, the archaeological record that provides the best gauge of external contact as a mechanism of divergence for any particular group. A case in point is Tikopia (Kirch 1986b), the evolutionary trajectory of which was drastically affected at several times by a variety of external forces, including later immigration, long-distance exchange, and drift voyaging. Similarly, Best (1984) has extensively documented, with archaeological evidence, the complex history of external contact which has helped to shape the prehistoric sequence of Lakeba in the Lau Islands. As our archaeological knowledge of external contact has increased, it has become evident that relatively few Oceanic islands were ever fully isolated and, to the contrary, many of those in the southwestern Pacific have been part of extensive long-distance exchange systems for hundreds or even thousands of years. The teasing out of these prehistoric exchange networks from archaeological data is a major task facing Oceanic prehistory today.

PARALLEL EVOLUTIONARY PROCESSES

Other major factors of evolutionary change in Polynesia resulted not in divergence, but in significant homoplasy or parallel evolutionary processes. We discuss three of these common trends below.

Demographic factors. Archaeological evidence suggests that Polynesian colonizing propagules were small, probably fewer than 100 persons in most cases. A reproductive strategy emphasizing fecundity and a high in-

trinsic growth rate would have offered the highest selective value for such small propagules (indeed, anything less than a high- r strategy would have courted rapid extinction [see McArthur, Saunders, and Tweedie 1976]). Thus, by the time of European intrusion, all of these island societies had reached relatively high density levels with density-dependent cultural controls on population growth (including abortion, infanticide, celibacy, and various forms of overt competition). On virtually every island there was thus a transition from an initial stage in which selection favored a high-growth demographic strategy to a late prehistoric stage favoring successful competition over growth. This kind of demographic transition has received much attention in the theory of island biogeography, where it is generally referred to as the r/K -selection continuum (McArthur and Wilson 1967; Pianka 1970; Diamond 1977; Kirch 1980b:42; Williamson 1981; Keegan and Diamond n.d.).

While theoretically the r/K -selection continuum involves a logistic growth scenario, there is no a priori reason to regard all Polynesian societies as having followed such a mathematically elegant progression. Indeed, substantial evidence now suggests that extinction, overshoot, oscillating, and step demographic curves are all exemplified in particular island cases. Even within a single archipelago such as Hawaii, it appears that demographic change varied considerably on the local level (Kirch 1985a:289). Understanding demographic process will be critical in any attempt to explain evolutionary change in Polynesia. Each case must, however, be approached on its own terms, with due consideration for historical factors including the effects of both cultural and natural selection as revealed in the archaeological record. The ideal r/K -selection continuum is a heuristically useful model; it does not a priori constitute an acceptable explanation for the evolution of any particular Polynesian society.

Intensification of production. A second widespread trend in Polynesian evolutionary trajectories is the intensification of production, not only in agricultural systems but also in animal husbandry, marine exploitation, and various other forms of resource exploitation (e.g., adz production). The specific forms of intensification are frequently unique to particular islands, reflecting local environmental conditions as well as the historical results of the founder effect and initial colonization processes (see above). Thus, for example, agricultural intensification in Tonga had its agronomic expression in dry-field rotation of yams and aroids with a significant arboricultural element, while in Hawaii both dry-field cropping of sweet potato and the irrigation of taro were carried to intensive levels. Despite the particular form of expression, however, the underlying trend—increased labor input per unit area and investment in permanent facilities—is reflected in virtually all Polynesian societies. We believe that this trend reflects, in part, common responses to identical or highly similar sets of selective pressures, notably population growth and environmental circumscription. Equally significant, however, was the shared inheritance of a hierarchical sociopolitical system with an emphasis on interlineage

competition and pre-emption, as well as inherent organizational abilities to direct and maintain intensified production systems.

Competition. A third parallel trend widely evident in Polynesia is increasing competition between sociopolitical groups over time. The lexically marked social status of "warrior" is reconstructable to Proto-Polynesian, and the systemic social pattern of status rivalry (Goldman 1970) is arguably an aspect of the ancestral society as well. While these social aspects of competition were therefore homologies inherited by all daughter groups, in virtually all Polynesian societies selection appears to have favored new and more overt varieties of competition, leading to changes in settlement pattern (the construction of fortifications, as with the New Zealand *pa* or Rapan hill-forts), war strategies and tactics, and most significantly, sociopolitical structure and patterns of land tenure. These latter changes were noted some years ago by Burrows (1939b), who remarked on the parallel shift from landholding groups with a genealogical, lineage basis to an outright territorial pattern based on overt conquest and redistribution. From an evolutionary perspective, we suggest that these parallel developments are again highly significant, reflecting both the persistence of ancestral cultural patterns (status rivalry) and the selective pressures of circumscription, resource limitation and degradation, ecological perturbations, and population growth.

CONVERGENCE

We must comment briefly on one further aspect of Polynesian evolution, the analogic emergence of similar traits or structures in two or more societies. In dealing with putative cases of convergence it is essential to eliminate the alternative explanations that such structures reflect the persistence of ancestral forms (homology) or result from contact and borrowing. For example, the elaborate *kava* ritual of West Polynesia is almost certainly the result of extensive late prehistoric contacts between Samoa, Tonga, and Fiji and cannot be ascribed to convergence. On the other hand, the development of true class stratification in both Tonga and Hawaii, as well as similar structures of dual paramountship in these two societies, cannot be explained by either inheritance or contact. A more material example is the invention in Hawaii, Easter Island, and New Zealand of the two-piece fishhook, evidently a convergent response to identical functional problems: the absence of suitable pearl shell and the need to overcome shear stress (in bone or stone) in large hooks. The careful search for and analysis of such instances of convergence offers possibilities for further isolating and understanding the selective pressures responsible for it.

Summary

We have argued above that in the study of evolution—whether biological or cultural—a critical element is a methodology for distinguishing homologous from analog

ous traits or characters. That is, evolution cannot be considered solely as process independent of specific history. The historical knowledge required to disentangle homology from analogy must be sought in the phylogeny or branching relationships of the modern groups under study and of their common ancestors. Methods for determining the phylogenetic relationships among cultural groups, including linguistic and biological as well as archaeological data, were defined some years ago by Romney and Vogt but have seldom been systematically applied in cultural evolutionary studies.

Because of the advantages which islands offer as controlled situations for evolutionary studies, Polynesia has long been recognized as an ideal region for the investigation of cultural evolution. Earlier attempts to deal with the Polynesian case in evolutionary terms were largely unsatisfactory because of a lack of temporally controlled data on prehistoric change. Over the past few decades, however, a resurgence of archaeological and historical linguistic work in Polynesia has now made possible the precise delineation of phylogenetic relationships among Polynesian societies. Furthermore, the archaeological records of prehistoric change for many Polynesian groups are now richly detailed, and the common ancestral group—ancestral Polynesian society—can be reconstructed in some detail. As a result of these developments, the potential for evolutionary studies in Polynesia is greater than ever before.

Recognizing that we now have in Polynesia a well-defined case of cultural "radiation" and divergence for which the specific historical legacy of shared, ancestral traits can be distinguished from analogic or convergent developments in response to common selection pressures, we have also presented a series of initial propositions or hypotheses regarding some major factors underlying both divergence and convergence in Polynesia. Given a significant current interest in Oceanic prehistory, we hope that others will be stimulated to test these propositions and thereby advance our understanding of this vast region.

Comments

PETER S. BELLWOOD

Prehistory, Australian National University, Canberra, Australia. II III 87

Kirch and Green deserve praise for expressing so lucidly an approach to Polynesian prehistory which, to my mind, is the essence of sound common sense. The scattered islands of Polynesia have provided a unique setting for human biological and cultural evolution and lend themselves well to considerations of history and phylogeny. It is refreshing to see that archaeology is at last moving away from the former fashion of denigrating all forms of culture history and that two leading Polynesian archaeologists feel that the time is ripe to underline the significance of an approach which focusses on such con-

cepts as common ancestry and homology, as opposed to history-free analogy. I have two additional comments to make from a broader, extra-Oceanic perspective.

Kirch and Green are quite correct to stress the importance of small and widely separated islands for a study of evolutionary divergence, but I think that many prehistorians, and possibly even these two authors, overstress the difficulties of making similar studies in continental or large-island arenas. For instance, I believe that a similar approach, utilising comparative data from linguistics, archaeology, and biological anthropology, can be applied quite successfully to all the Austronesian-speaking populations, from Madagascar through the islands of Southeast Asia and eastward to Polynesia. It is a common assumption amongst prehistorians that islands will tend to have simple culture histories while those of continents will tend to be hopelessly intertwined. In many cases this assumption is undoubtedly justified, but each case deserves consideration on its own merits.

In their discussion of evolutionary processes, Kirch and Green stress the significance of demographic factors. Apart from the growth trajectories that they discuss, however, there is a much broader general significance to the available data. The Polynesian islands can tell us, relatively unambiguously, just how rapidly small founding populations (one or two canoe-loads) developed into populations numbering up to 200,000 (as in the Hawaiian case). Small founding populations thus reached uninhabited islands, introduced a range of highly productive domesticated plants, and within about a millennium increased their original population sizes by a conceivable 10,000 times. Statistics of this kind must surely make all of us think very hard about the significance of the earliest developments of food production all over the world. Hypotheses that invoke population pressure as a prime mover behind cultural development may be out of vogue, but hypotheses that deny roles to population growth and expansion in situations where agriculturalists had access to suitable environments, either previously uninhabited or simply hunted/collected, may be quite unrealistic. From this point of view, Polynesian prehistory has a very important tale to tell.

R. C. DUNNELL

Department of Anthropology, University of Washington, Seattle, Wash. 98195, U.S.A. 2 IV 87

Recognition that "cultural evolution" in the social sciences is unrelated to evolutionary theory as employed in the natural sciences (e.g., Blute 1979, Dunnell 1980) has renewed interest in using evolutionary theory in archaeology. Kirch and Green's synthesis of Polynesian prehistory is such an effort. As a preliminary account, their overview has virtues. Most important are the kinds of questions asked of the Polynesian data and the new integration of those data implied by the questions. Variation, the importance of distinguishing homologous and analogous similarities, the crucial role of homology in evolutionary explanation, and the value of documented

local extinctions are observations pertinent to a Darwinian approach.

Beyond terminology and programmatic assertion, however, one struggles in vain to find any vestige of Darwinian evolution here. To the contrary, the approach suggested is Lamarckian and embedded in an essentialist (Mayr 1959, Sober 1980) metaphysic inimical to the materialism underlying Darwinian evolution. In fact, Lamarckian vitalism and essentialism are two prominent characteristics of "cultural evolution" that set it apart from scientific evolution. In the last analysis, Kirch and Green offer, at least in methodological terms, a "cultural evolution" model reworked in the terminology of evolutionary theory. Although these features of their methodological program are pervasive, they are subtle, as witnessed by the long-standing confusion between "cultural evolution" and scientific evolution.

A fundamental characteristic of Darwinian evolution is its two-step structure. The generation of variation is *undirected* and *independent* of selection, the process responsible for patterning evolutionary change. In spite of their Darwinian intent, Kirch and Green frequently depict evolution as a one-step process in which variation is directed by the same forces that determine selection. For example, they tell us, "The colonization of a new island often meant a radical shift in environment, in turn *requiring* (italics mine) an immediate reassortment or segregation of technology and adaptive strategy." Even more clearly, statements such as "in Polynesia . . . local ecosystems posed constraints and offered possibilities, but it was culturally directed human actors who were the active agents of sociopolitical change" expose their Lamarckian vitalism. Confusion between reason-giving and scientific cause (Dunnell 1980) allows the incompatibility of these statements with Darwinian evolution to pass unnoticed, just as the same confusion is sometimes taken as a justification for a separate "cultural evolution." In science, cause is embedded in the theoretical system; it is not attributed to the phenomena being studied.

Kirch and Green's commitment to essentialism is less obvious given their criticism of stages, the standard essentialist constructions of "cultural evolution," and their emphasis on variation. But they simply replace stages with smaller-scale essentialist "societies." In the materialist ontology that underlies Darwinism, "things"—be they biological species or sociocultural societies—are transitory configurations of continuously changing phenomena. "Things" do not exist; they are always in the process of becoming. That Kirch and Green treat societies as empirical is apparent when they talk about "an ancestral Polynesian culture." Societies are not simply analytic tools for the description of phenomena. Thus they seem to construe development as the *transformation* of one society into another. Change, in this framework, is synonymous with difference.

As Mayr (1982) and others (e.g., Hull 1965, Lewontin 1974) point out, essentialism is the single greatest impediment to an understanding of evolutionary theory. Because essentialism characterizes not only the predic-

tive, ahistorical sciences but also the structure of common sense, it creeps into archaeological writing almost invisibly (Dunnell 1982). Kirch and Green compound this by their efforts to establish a scholarly precedent for their view in the works of Flannery (1983a), Romney (1957), and Vogt (1964), all structured by an essentialist metaphysic thinly disguised by the metaphorical use of "genetic."

As damaging as this entanglement with Lamarckism and essentialism is to Kirch and Green's efforts, their methodology implies an even more basic problem. They argue that evolutionary understanding of Polynesian prehistory is a matter of reinterpreting "facts" that have accumulated over an extended period of investigation. In this they assume that phenomena can be known in an objective, theory-free manner. Yet a principal function of evolutionary theory is to dictate how phenomena must be described in order to be explained by the theory. Failing to realize that adopting a new theory *requires* the creation of new data is not only the major flaw in Kirch and Green's essay but also the greatest impediment to the implementation of evolutionary theory in archaeology generally. In spite of their intention to use scientific evolution, Kirch and Green revert to "cultural evolution." Their theory has come to fit their facts, acquired in a different framework.

Essays such as Kirch and Green's are useful first steps in the implementation of scientific evolution in archaeology. Practitioners have to be convinced that new insights will be forthcoming, and in this, Kirch and Green have made a contribution. But such essays are damaging when they treat evolutionary theory as an interpretative algorithm commensurable with previous paradigms. It is all too easy to forget Mayr's (1959) observation that evolutionary theory is not just a different theory, it is a different *kind* of theory. Until archaeologists take this admonition to heart in all of its ramifications, the use of evolutionary theory in archaeology will remain a linguistic veneer masking traditional archaeology.

TOM DYE

Department of Anthropology, Yale University, Box
2114 Yale Station, New Haven, Conn. 06520, U.S.A.
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A central issue in evolutionary study is the unit upon which selection acts. Population biology has the allele, paleontology the species. Kirch and Green are unsure of the selection unit within the Polynesian "phylogenetic unit." Their use of linguistic models suggests *language groups*, their discussion of failed colonies on small islands *populations*, and they refer indiscriminately to *cultures* and *societies*, at one point leaving the choice of terms up to the reader. This matter deserves further thought, as interesting propositions are impossible to evaluate with the selection unit unspecified.

It may be best to preserve the distinction between culture and society as notional and relational concepts, re-

spectively (see Firth 1951:27), especially since the ability to identify sources of lithic materials holds out the promise of identifying "interaction spheres" in areas of relatively homogeneous culture such as New Zealand (Reeves and Ward 1976, Davidson 1984) and Hawai'i (Cleghorn et al. 1985) as well as in culturally diverse regions such as Fiji, Tonga, and Samoa (Dickinson and Shutler 1971, Best 1984).

Kirch and Green introduce Romney's genetic model and Vogt's eight-stage Mesoamerican culture-historical research strategy to Polynesianists. These look like the agenda for Green's career in Oceania; Mesoamericanists should note the strategy's success here. To support the claim that Polynesia offers a relatively "dense and continuous" prehistoric sequence, they review the record of Polynesian archaeology and prehistory, in which both have good reason to take pride; without their primary research, claims of density and continuity could not be sustained. They acknowledge some gaps, and will surely help to fill them, but here they discount the effect these gaps have on the methodology proposed and the interpretations offered.

For example, in Vogt's first stage the authors add, as a "key element," a somewhat controversial (cf. Clark 1979) subgrouping of the Polynesian languages and the historic relationships that might be inferred from it. This tactic is necessary because lexicostatistics prove unreliable with languages related as closely as those of central East Polynesia (Vogt's Stage 2) and because the early portions of the archaeological sequences for the Society Islands, Easter Island, the Tuamotus, and Hawai'i are poorly known (Vogt's Stage 5). Again, Groube's (1971) proposal that West Polynesia's earliest settlers relied on naturally abundant pristine resources in their rapid colonization of the region is "refuted" by evidence from "ancestral Polynesian society" which, according to the authors, originated some seven centuries later. Best (1984), who has excavated the only finely stratified, undisturbed deposits dating to initial occupation of the region, finds much in his data to support elements of Groube's proposition.

A similar note of caution should be voiced over the weight placed on semantic values assigned to reconstructed lexical forms in characterizing "ancestral Polynesian society." The *Wörter und Sachen* method works best where the denotata of reconstructed lexical forms are material objects and becomes intractable when the "thing" referred to is a social relation structured by a complex of interdependent rights and duties. A case in point is the claim, based on linguistic data that have since been questioned (Lichtenberk 1986), that the institution of hereditary chief was part of "ancestral Polynesian society." The term **'ariki* is securely reconstructed for Proto-Polynesian, but the semantic value assigned it is modeled on the rights, duties, and modes of succession associated with chiefs of contact-era societies in full land situations. Since sociopolitical power in contact-era Polynesia was rooted in some degree of control over access to land, could the rights and duties of an **'ariki* in a "propagule" of fewer than 100 persons on an

island covered with virgin forest have been comparable to those of a contact-era chief? This is an important issue in Polynesian prehistory, and the authors should be commended for attacking it with synchronic linguistic data. Diachronic archaeological data will contribute to an understanding of social and political change in prehistoric Polynesia when investigations focus on the material preconditions for establishing specific sets of social relations (Hommon 1976).

A list of "hypotheses of evolutionary processes" is presented for "testing" against the Polynesian prehistoric record, but, according to Dunnell (1980), there is as yet no decisive test for competing mechanisms of evolutionary change. The statement that archaeology is able "to reveal . . . the selective forces that resulted in change and divergence" suggests one, but it is not described. One is needed to differentiate evolutionary study from descriptive phylogeny.

Confusion is introduced by the suggestion that both isolation and external contact lead to divergence, which seems to represent a shift in scales of comparison. "Isolation" here apparently refers to subunits within the Polynesian "phylogenetic unit" and "external contact" to contact between Polynesians and non-Polynesians. The dangling question of contact within the Polynesian "phylogenetic unit" and its evolutionary effect once loomed too large in the minds of prehistorians but still deserves theoretical treatment.

These points aside, the central thesis that Polynesian prehistory provides a promising field for rigorous historical study is well taken. Scholars unfamiliar with Kirch's and Green's substantial contributions will find them rewarding.

CHRIS GOSDEN

*Department of Archaeology, La Trobe University,
Bundoora, Victoria 3083, Australia, 8 IV 87*

The idea of evolution is one of the intellectual commonplaces of our time. A commentator on Darwin's influence has summed up the impact of his thought as follows: "During the past hundred years or so evolutionary theory has functioned in our culture like a myth in a period of belief, moving effortlessly to and fro between metaphor and paradigm, feeding an extraordinary range of disciplines beyond its original biological field" (Beer 1985:17). Because the idea of evolution is such a central part of our scientific and popular culture, it is difficult to resist the suspicion that problems are often cast in a familiar evolutionary framework to make them seem better understood than in fact they are. This is my main impression of Kirch and Green's article.

The authors seem to imply that the hard work of thinking about Pacific prehistory is mainly done. We have a framework of interpretation (the phylogenetic model), and the main necessity now is more survey and excavation to provide missing data points. However, much necessary justification is missing from their argument. The first and most basic question remains unanswered—why use an evolutionary framework at all? As

far as I can ascertain, their chief reason is that an evolutionary framework enables us to study history. I fully concur with their interest and feel that the recent resurgence of work in the specific historical contexts of social change and human action in both anthropology and archaeology is extremely healthy. What I am not at all sure about is whether one can profitably use a biological analogy to understand human history. Biologists study the history of species, which generally span millions of years; it is uncertain how far their concepts allow us to interpret the last 3,000 years of Pacific prehistory. At the least the use of an evolutionary framework requires some justification. The limits to the analogy between change in biological species and social change also need discussion. Much of the most interesting recent work in palaeontology has concerned the rate of continuity of change in species, centring upon the debate over punctuated equilibrium. Kirch and Green make no mention of the uncertainties in the discipline from which they take their framework, nor do they discuss their position on the nature of social change or its rates. It is important to know how far they think an evolutionary analogy can be pushed.

Uncertainty also surrounds their idea of history. In order to understand history within an evolutionary framework, it is necessary to distinguish between homologous traits, considered by them the very mark of history, and analogous ones. By this reading, history is reduced to inheritance, a usage familiar to biologists perhaps but not to social scientists, who would include continuity, coincidence, and disjunction, that is, homology, analogy, and much more. Homology is distinguished from analogy here by reference to a reconstructed ancestral Polynesian society. Historical linguistics plays a major part in this reconstruction, supplying a whole series of terms for status and kin differences from protolanguages. Status terms are compared with ethnographic accounts of Polynesian societies to flesh out possible relations between people 2,500 years ago. There is, however, no certainty that semantic shifts have not occurred in the intervening period. Nor are the ethnographic categories of Polynesian society as stable as they appeared 20 years ago. Sahlins's (1981, 1985) recent work has stressed that the initial contact between Polynesians and Europeans was the meeting of two different structures of thought. Ours is an understanding of another, Polynesian, understanding, and anthropological categories of society may have to undergo redefinition in the light of this fact. Also, European contact brought about drastic changes in Polynesian society. Recent surveys are consequently of a state of society greatly changed from pre-contact days, and attempts to view prehistoric communities in a recent light might be suspect. Suspicion is increased when archaeologists bring anthropological ideas into contact with their own very different data base. How we can recognise particular forms of prehistoric Polynesian society from archaeological evidence alone is a basic question to which Kirch and Green pay scant attention. Instead of being part of a pre-formed answer, the starting point from which all diversity is measured, ancestral Polynesian society should be part of

a question—was it really as the linguists reconstruct it, and can we judge this from the archaeological record?

To be blunt, I cannot understand why it is necessary to cast the new interest in history abroad in Pacific studies in a stale evolutionary framework. Using such a framework, Kirch and Green have reduced the question of history to the following of inherited traits and have cast cause in mainly physical or environmental terms (isolation, population, colonisation, environmental selection, etc.). Archaeology is the only means of understanding the full time depth of social change in the Pacific. Archaeologists will have to develop ideas and methods relevant to their particular data sets and learn to spot social change in the archaeological record. It is not enough to borrow frameworks of analysis from other disciplines with their own histories, modes of thought, and peculiarities of data.

CHANDLER W. ROWE

Bernice P. Bishop Museum, Honolulu, Hawaii, U.S.A.
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As with most culture areas characterized as a discontinuous landmass, there are many problems in determining the culture history of Polynesia. Answers to questions involving migrations and settlement patterns require utilization of all available data from the various subdivisions of anthropology and geographical and environmental data as well. The authors ably apply an evolutionary concept to develop what they term the Polynesian evolutionary record. I accept their delineation of an ancestral Polynesian society. We know that in the ethnological present certain elements of this common cultural base have been intensified in different subareas and island groups and minimally developed in others. For example, carving is common to all groups but was intensified among the Maori; the same is true for chieftainship among the Samoans and featherwork in Hawaii. These variations are due to multiple factors, among them environment (there are high and low island groups in Polynesia) and the degree of contact with other regions.

The linchpin of the authors' study is the data provided by archaeology. Archaeological investigation is relatively recent in the region, having started in a systematic way only early in the 20th century and truly developed after the hiatus created by the events of World War II. The increasing quantity of anthropological data currently available for various island groups together with the development of sophisticated methods of dating archaeological materials have made possible such conclusions as are put forth by Kirch and Green. This important and masterful work should stimulate additional thought and research.

JOHN TERRELL

Field Museum of Natural History, Roosevelt Rd. at Lake Shore Dr., Chicago, Ill. 60637, U.S.A. 8 IV 87

Kirch and Green exaggerate the success with which their goals of determining "phylogenetic relationships" and

reconstructing "ancestral Polynesian society" have already been accomplished (for instance, see Davidson 1984:13–28; Irwin 1981; Lichtenberk 1986). My remarks, however, will concentrate on their aims and methods. What they say is disturbing. If we must do what they say we must, then archaeologists and others working outside Polynesia may see their advice as counsel for despair.

Methodologically, this article revives proposals entrenched in 19th-century inductivism and the atheoretical (even antitheoretical) skepticism of the likes of Franz Boas (e.g., 1896). It can be argued that practical methodologies are not built on such prescriptions (Terrell 1982; 1986b:6–12, 241–43). Why, then, do Kirch and Green bring back these requirements? They appear to be looking at the world from a historical linguist's point of view. Their research agenda and the methodology they prescribe for it have been set, it seems, by the agenda and procedures of the comparative method, and they are implying that what works for historical linguistics will work (and already has) for Polynesian archaeology. While Green and his colleague Andrew Pawley have vigorously developed linguistic scenarios for Pacific prehistory (Pawley and Green 1973, 1984), such an analytical framework is not without its difficulties (Terrell 1986b:243–54).

If we accept the elementary (and not very realistic) view that when a parent or "proto-" language splits, its descendants go on to change in separate ways in isolation (i.e., historically related languages change, or radiate, as independent isolates), then:

1. To reconstruct the parent language of a family or subgroup of related languages, we may compare its descendants to see what they have in common because *if the languages studied are independent witnesses*, then what they have in common is probably what they have inherited from their common parent. This approach, in simple terms, might be called "the sift-through-the-differences-to-find-the-similarities-that-must-also-be-homologies-or-identities" research program. Similarly, Kirch and Green infer, if we want to reconstruct ancestral Polynesian society, it may be equally appropriate to look for common threads shared by different Polynesian societies, modern or archaeological. Yet doing so is not without its dangers, because, as they acknowledge, isolation and living on islands do not automatically go together even in Polynesia (Kirch and Yen 1982; Terrell 1986b:122–51). Nor is it easy to assume that sociocultural similarities that resemble ancestral traits are necessarily homologies, i.e., intergenerational learned transmissions rather than reinventions.

2. Following the prescriptions of the comparative method, a properly reconstructed subgroup or family of languages is in a way *by default* also a phylogenetic tree of the languages included. The kind of history captured by a language family tree matters to comparative linguistics because *historical change gets in the way of proving family ties* among languages if the branchings on their genealogical tree are unknown. Proof of family membership in essence amounts to getting history out of the way. This is apparently what Kirch and Green

mean by "history matters." Disentangle descent, they say repeatedly, and then we will be able to study modification (i.e., "processual questions") using comparative methods. But if history (defined narrowly as homology) is a problem because it can get in the way of analyzing process, then concentrating on places like Polynesia where two or more societies have arisen from a common ancestor is to be avoided. Have Kirch and Green no doubts about what is known or knowable about Polynesia? Would it not be better to focus on historically *unrelated* societies where homologies are not lying in wait for us on the path to process?

Kirch's important book (1984a) on the evolution of Polynesian chiefdoms can be seen in this light as a cautionary tale against studying cultural divergence within phylogenetic units. Did some Polynesian societies converge on a similar sociopolitical pattern because of an inherited predisposition (an ancestral Polynesian proclivity for chiefdoms) or because of external selection pressures? As Kirch's book illustrates, who can tell?

Kirch and Green take the phrase "history matters" from an essay by Gould (1986) on Darwin's contributions to historical methodology. But tracking down homologies, as Gould observes, was only one of Darwin's techniques. How, then, does history matter if our goal is not to prove family ties or reconstruct ancestral societies—if, instead, what we want to do is study causal processes such as isolation, the founder effect, colonization, selection, and so on?

We need history to understand human diversity just as biologists of the living world need paleontology to understand organic diversity (Terrell and Fagan 1975). But we should not beg the question "How much history do we need to know?" by insisting that we must first reconstruct phylogenetic units and trace our way back to ancestral prototypes. Like Darwin, let us also *deduce* and model causal processes so that we can judge how much of a slice of time we need to study (Clark and Terrell 1978, Terrell 1986a).

Kirch and Green apparently agree with Gould that "kind, extent, and amount of similarity provide the primary data of historical science" (Gould 1986:66). It is true that the primacy of similarity is a deeply rooted bias in Western thought. However, a strong case can be made for the primacy of diversity—both similarities and differences—in Nature and in the realm of human affairs as the fundamental paradox at the heart of all scientific work (Terrell 1977:237–39; 1986b:267–68).

EVON Z. VOGT

*Department of Anthropology, Harvard University,
Cambridge, Mass. 02138, U.S.A. 10 IV 87*

This article is an exciting application to Polynesian cultural history of what has previously been called "the genetic model." Kirch and Green's first accomplishment is to argue cogently that the model is better labeled "the phylogenetic model," thereby placing emphasis upon its

essential aspect—"the delineation of phylogenies or historical sequences of divergence from a common ancestor." I am convinced by their argument and have already adopted their suggestion in a forthcoming article (Vogt n.d.) that brings the Maya case up to date.

In spite of the fact that the model was first developed and applied in Mesoamerica (Romney 1957, Vogt 1964), Mesoamericanists have been slow to embrace and utilize it. There have been only three notable exceptions: Flannery and Marcus (1983) have published a brilliant application of the model to Zapotec and Mixtec cultural history in Oaxaca, and Lee has successfully applied it to the extinct Coxoh Maya of the upper Grijalva River basin (Lee 1979, Lee and Markham 1976) and to the prehistory of the Tzotzil-Tzeltal of highland Chiapas (Lee 1985).

In their application of the model to Polynesia, Kirch and Green introduce a useful refinement to keep the three strands clear: a nice distinction among "protolanguage," "ancestral culture," and "parental population." Along all of these strands, it is clear that the Polynesianists are now well ahead of the Mayanists in their reconstruction of the protolanguage and their search for the Polynesian homeland, as well as in their reconstruction of ancestral Polynesian society and of the variations therefrom. Both the Maya area and Polynesia are badly in need of additional research on biological similarities and variations, especially genetic studies, in their indigenous populations. An impressive beginning has been recently made in the study of genetic characteristics in Polynesian populations (see Kirch 1985b), but for further progress we all need to study closely the methods and conclusions of the path-breaking work of Greenberg and his collaborators (1986; see also Greenberg 1987) on the comparison of linguistic, dental, and genetic evidence bearing upon the aboriginal settlement of the Americas.

I am delighted to see the phylogenetic model so productively utilized in Polynesia, and it is my hope that this paper will inspire other scholars to apply it in still other parts of the world.

R. L. WELSCH

*Field Museum of Natural History, Roosevelt Rd. at
Lake Shore Dr., Chicago, Ill. 60637, U.S.A. 8 IV 87*

Kirch and Green present a formal (and, they assume, rigorous) methodology for demonstrating a phylogenetic unit, which they say in principle should represent a set of populations or communities with a common genetic, linguistic, and cultural ancestral population. This they feel is a necessary first step for investigating cultural evolution. It is, however, questionable whether such an elaborate procedure is widely applicable to areas without the kind of isolation presumed for Polynesia. Certainly it cannot be usefully applied if comparative linguistic data are not available. Nor is it clear that their arduous (but perhaps less rigorous than assumed) methodology for demonstrating phylogenetic units has any obvious payoff.

In most ordinary settings, human groups are constantly interacting with groups and communities that do not originate in the same ancestral units (either linguistically, culturally, or genetically), and if evolutionary processes have any meaning at all, they will certainly be actively shaping these communities. I should think that it is just this sort of complex sex of conditions that makes evolutionary questions interesting for anthropologists and archaeologists. As a special case, Polynesia may offer some useful insights into evolutionary processes, but to understand cultural evolution requires methods and models of evolutionary processes that can be understood and applied in more typical situations. Kirch and Green's notion of cultural evolution seems an overly simplistic analogy drawn from biology, a conception that was actively being debated when Steward, Romney, and Vogt were writing. They offer very little that is analytically new, and several points are insufficiently thought through.

It is not clear from this article (or from the literature, for that matter) how binding "inherited" cultural patterns are on changing human communities. Each new generation may not create every aspect of its society anew, but it is not bound to "inherited" patterns in the way that organisms are typically bound to their genes. Isolating a phylogenetic unit may well insure that the study communities have something that is culturally "inherited" along some historical pathway, but as far as I can understand, such historical pathways do not imply a causal force in any general sense. What can the authors mean, for example, by saying that certain bark-cloth designs and tattooing "may be explained by common inheritance" from Lapita decorations? Given some observed similarity between bark cloth, tattoos, and Lapita sherds, what kind of explanation (in any evolutionary sense) do the authors intend as a result of a common historical pathway?

Despite Kirch and Green's insistence to the contrary, the data from Polynesia's archaeological record are so thin that they appear obliged to draw historical, phylogenetic, and evolutionary conclusions from linguistic markers. Clearly, hypothetical protolanguage reconstructions can tell us very little about the daily social processes that may have inspired the terms, the context in which they were used, or the social forms to which they were applied. This is not evolutionary analysis but a return to late 19th- and early 20th-century speculation using linguistic clues in the absence of any more substantial data from other independent sources.

The most convincing examples of evolutionary arguments alluded to are, not surprisingly, very narrowly focused studies that link environmental variables with cultural variations. Such examples are reminiscent of the work of Steward and, while plausible, do little to advance our understanding of cultural evolution much farther. Unfortunately, the state of the art in Polynesia seems to consist in either these small focused studies or the employment of all of the traditional gimmicks that have been exploited by archaeologists and anthropologists in the Pacific for a century.

RALPH GARDNER WHITE

PK 13,500 Punaauia, Tahiti, French Polynesia. 29 III 87

Kirch and Green seem to be discussing important matters on a highly abstract plane from a very restricted point of view. I cannot evaluate archaeological methodology. Furthermore, I am more interested in typological comparison than in historical matters, although I do not exclude historical data ipso facto. Perhaps I can provide a linguistic parallel:

For decades now I have been filling requests for data and commentary from ethnotaxonomists: kinship sets, color systems, ethnobotanical systems, ethnozoological systems, and others. Generally speaking, my correspondents inferred evolutionary development from the simplest to the most complex systems. Historically speaking, I found this counterintuitive and tried to find out if they were really thinking phylogenetically rather than ontogenetically or what-not. Their data were usually from genetically diverse sources, and I could seldom see any justification for historical inference or conclusion. As far as I could see, the conclusions were the result of typological comparison. Unfortunately, none of them responded. I feel that the matter is of basic intellectual importance. It amounts to seeking a valid answer to the question, "How does one evaluate inferred facts?"

No reasons (let alone proof) have been given for assuming that various domains of culture are subject to the same language encoding processes; besides, no evidence has been advanced to show that like domains exist universally. In fact, almost all of the studies I have seen look remarkably like ethnocentric projections onto xenographic areas. However, for the sake of clarity, let us restrict ourselves to matters of colour. Simplified, Kay and Berlin set up a series of stages as follows: Stage 1, white, black; Stage 2, white, black, red; . . . Stage 7, black, white, red, green, yellow, blue, brown, purple, pink, orange, grey (eight to eleven terms). (For fuller listing, see Branstetter 1977, Kim 1985, Kay and McDaniel 1978.) Branstetter (1977) does try to provide historical reconstructions of individual lexical items and set forth prehistoric colour systems, and Kim (1985) has done a magnificent job of depicting the Korean colour system from within. However, both seem more or less to accept the validity of the presumed evolutionary sequence. Many of the assumptions involved strike me as gratuitous.

The most recent trends in folk taxonomy seem to favour universal categories as against cultural relativism. I have no argument with Kay and McDaniel's (1978) contention that the human organism is so built that it conforms to particular categorizations of the colour spectrum: human beings see the way they do. But to go from there to saying that (the) Tahitian (language) encodes colours in keeping with Stage (*n*) is a very long step. Such statements are basically inaccurate and misleading. Languages are cultural phenomena, produced and transmitted by human beings, who do any encoding that there is to be done. Furthermore, the time element has to be tied down more carefully; an ethnographic

present tense will not do. Finally, such a statement is contrary to fact as far as the Tahitian language is concerned. Tahiti has been in contact with the outside world, particularly with Western civilization, for some 200 years now and has not yet acquired a proper word for "colour." It is still very difficult to ask a Tahitian in Tahitian for a list of basic colour terms. It is not that he has any difficulty in handling abstractions but simply that he abstracts differently. An abstraction that would include colour terms would include words such as *mottled*, *striped*, *spotted*. A taxonomy is a way of organizing items, and itemization of the observed world is only part of the task.

Historicity? There has been a set of three mutually contrasting terms covering the visible spectrum, extending back to Proto-Polynesian: *tea* < **tea* "light (colour)," *uri* < **uli* "dark (colour)," *'ura* < **kula* "ruddy (colour)." Contemporaneous with but outside this set there has been the colour term *re'are'a* < **re'are'are* "yellow (colour)." This is an adjectival formed by reduplication from the noun *re'a* "ginger, yolk, pollen" < **re'ea* "ginger, yolk, (?)," giving us a modicum of historical grammar. The ternary set is not fully extinct even today; it was better known in 1940 and alive though a bit old-fashioned in 1800. I hypothesize that the term for "yellow" became an incremental core around which a new cluster of colour and other descriptive terms has gradually been developing into a new system ever since. In 1900, for the folk taxonomist, it was approximately encoded: black, white, red, yellow, grue, (grey), violet, pink. "Grey" (*rehu* "ash, grey" < *refu* "ash, [?] grey") is included in parentheses because it has the basic meaning of "ash," which Kay and company exclude. The idea of "orange" is expressed as "orange (the fruit) yellow." There is no good native word for "brown," though some shades are expressed by "grey" and others by "red." The French word *chocolat* has been used quite a bit. Since 1900, the term "grue" has been more or less artificially and arbitrarily replaced by "grue" = "blue" and "grass" = "green." But there are still people (including myself) who say "grass grue" for "green" and "(ocean) grue" for "blue." In any case, that does give us a glimpse into cultural linguistic history. The original ternary set did not evolve; it was overwhelmed.

Reply

PATRICK V. KIRCH AND ROGER C. GREEN
Seattle, Wash. 98195, U.S.A. 7 v 87

As usual, a claim that some form of evolutionary method or model involving culture may prove productive in the analysis of a particular area's prehistory draws the expected types of response: "stale evolutionary framework," "19th-century inductivism and atheoretical (even antitheoretical) skepticism," or "a return to late 19th- and early 20th-century speculation using lin-

guistic clues." Such comment is neither conducive to fruitful discussion nor helpful except to indicate its author's hostility toward any renewed exploration of this topic. These people dismiss out of hand any argument that uses the term "evolution"; they have no need of the concept or procedures based on it and prefer to do their prehistory by other means. This is the position of Gosden with his emphasis on "social change," Terrell with his insistence that "diversity" is the primary concept in scientific work, and Welsch with his view that in ordinary situations "constant interaction" between non-related groups is the main problem requiring analysis.

In contrast are the supportive comments of Bellwood, Rowe, and Vogt, to which there is little to add. However, Bellwood raises one point worth emphasis, namely, that the approach should be applicable in large island and continental areas as well. Certainly the Mesoamerican cases cited by ourselves and Vogt demonstrate this, as does Bellwood's (1985) own work in Island Southeast Asia. One could further cite the potential of these procedures in parts of Africa (as in the Nile Valley or the Bantu case) or in Japan, large portions of China, or substantial areas of northern North America. These are just some of the more obvious places where the methodology has a chance of success because biologically, linguistically, and culturally related groups have recently, and for some time in the past, occupied a given zone. Polynesia, while it has certain advantages, is not the atypical or special case Welsch claims, nor is the ordinary world of recent prehistory totally dominated by constant interaction between randomly distributed and nonrelated groups.

Serious discussion of the hallmarks of a modern evolutionary approach in archaeology owes much to Dunnell, and not surprisingly his comments are both critical and insightful. In his terms our conceptual framework would not qualify as "scientific evolution," only a transformed kind of "cultural evolution" at the specific societal level. He feels it still entangles us with positions that are both Lamarckian and essentialist.

With regard to Lamarckism, the problem one faces is the necessity of dealing with *cultural* as well as solely genetic modes of transmission and therefore of employing a mechanism for the inheritance of "acquired characteristics." Indeed, in our present understanding, cultural variation does not simply arise randomly; people frequently invent identical ideas almost simultaneously and are often purposively innovative in response to natural and cultural selective pressures. These new cultural patterns, which confer greater "fitness" on the individuals that adopt them, are then transmitted not only generationally but laterally and even between genetically unrelated groups. Thus our answer to Welsch would be that within a phylogenetic unit long-lasting cultural similarities that constitute valid homologies between groups (because they can be demonstrated to have arisen through transmission from an ancestral entity along historical pathways common to those groups) are phenomena whose existence (not origin) is to be explained by the cultural as opposed to genetic mechanism

of transgenerational inheritance and not by the operation of some kind of contact between the groups or by continuing reinvention or parallel innovation. Gosden is quite wrong, however, in the claim that our whole idea of history reduces simply to inheritance and does not include continuity, coincidence, disjunction, and much more, as is revealed by our discussion of convergence, founder effect (random loss), failed colonies on small islands (extinctions), innovation, and short- and long-term adaptation. A concern with Lamarckian issues of inheritance is thus not some fatal flaw, only one of the necessary considerations in cultural matters.

Dunnell's charge of essentialism is founded on the view that to talk of entities is to commit the fallacy of viewing "things" as empirically existing rather than seeing them in the Darwinian sense of continuously changing phenomena in the process of becoming. Our problem is that while we agree that this is so with all conceptions of entities, where it is the variation rather than the "type" that is real, operationally we still have to describe as shorthand reference points empirical "somethings" in time and space when describing actual evolutionary sequences. Hence the paleontologist's use of extinct "species" in phylogenetic models, both with and without branching, to order the entities recovered in terms of time and degree of posited relationship. Hence also our labelling of something as ancestral Polynesian society, Proto-Polynesian language, and parental Polynesian population. All were continuously changing entities with quite varied features, and as with a protolanguage one merely picks as a reference point some particular time, place, and content within the reconstructed continuum on which to focus the description.

Here we might remark, because it draws comment by a number of people, that in our view the current level of understanding of ancestral Polynesian *society* is still very rudimentary and constitutes only a starting place for further work. We certainly agree with Gosden, Dye, and Terrell (a) that we do not expect to rely solely on comparative linguistics for this kind of information, (b) that semantic-history hypotheses for proto-meanings in the social and political domain are more difficult to develop than for some materially based items subject to easier direct documentation by archaeology, and (c) that archaeologists need to develop their own independent means for recovering data that bear on such social issues as ranking, segmentation in social and political units, and household differentiation (cf. Green 1986).

Dye points to an important issue with regard to the kinds of entities to be used in analysis, namely, the units upon which selection acts. Linguists have their languages, with dialects, dialect chains, and communalects, geneticists their interbreeding or potentially interbreeding populations, but archaeologists (and to a degree ethnologists) have to make do with complexes, periods, phases, tribes, cultures, and societies. Kirch (1980) deals with this problem at some length, and it would appear that our units in archaeology must in this approach be on a level commensurate with those used by linguists and biological anthropologists: Polynesian culture and

society will not do. Rather, what we are talking about are Samoan, Tonga, and other ancestral societies, and not ancestral Polynesian society, Lapita, or any other societal or cultural entity in the general sense.

Dunnell's view that we are somehow fitting theory to facts none of which were collected in the framework described may be contrasted with Dye's remark that the strategy proposed looks like "the agenda for Green's career in Oceania." The point is that the facts gathered have not been randomly accumulated but collected, as all "facts" must be, within some conceptual framework, in this case a framework that had the reconstruction of phylogeny as one, though not its only, objective. Thus a definite strategy has been followed over two or three decades, and this is why it is now possible (a) to make the attempt to disentangle some of the thorny issues of analogy and homology within an explicit methodology for regional culture change and (b) to outline some hypotheses of how that change might have occurred or the similarities arisen so that continued archaeological work can discover, describe, and explain the phenomena in a relevant and useful format. The evolutionary framework employed is not simply some fashionable "window dressing" unrelated to what has gone before, nor does it imply, as Gosden claims, that the hard work of thinking about Pacific prehistory is mainly done. Our view is that the hard work of thinking about Polynesian prehistory can now seriously begin, since we can hardly claim that we are in anywhere near the same position with regard to Melanesia and Micronesia, where few "facts" have been consistently collected within any particular conceptual scheme, where dense and continuous sequences are seldom available, and where potential phylogenies for linguistic, biological, and archaeological materials are still largely unclear.

Dye and Welsch both raise the issue of "external contacts" as being of some importance everywhere to archaeological explanations of both similarity and difference. Our view is that isolation and external contact are two ways of looking at the same thing, and by "external contact" we mean not just contact from outside the Polynesian region as a whole but contact beyond the individual island or archipelago (see discussion of units above). In Polynesia the extent of divergence in isolation, coupled with the false notion of a cessation of voyaging in the past, has to some extent been overemphasized. Continuing contact between island groups and within them has been the norm throughout the occupation sequences of many islands and island groups.

In respect of ancestral Polynesian society, Dye (correctly in our view) argues that on the available evidence the early colonists of Fiji-West Polynesia initially heavily exploited the natural resources of the region. Groube's proposition was, however, that the first colonists of this area were not horticulturists at all but "strandloopers" and that horticulture, along with the pig, entered the area hundreds of years later. We do not see evidence in support of Groube's position, only that the eventual dominance of the horticultural component in the system, present in ancestral Polynesian society

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Calendar

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September 28–October 3. Second International Congress of Human Paleontology, Turin, Italy. Symposia: Pre-Hominids (Martin Pickford, coordinator), Australopithecinae (Donald C. Johanson, coordinator), *Homo habilis* (Phillip Tobias, coordinator), *Homo erectus* (Marie-Antoinette de Lumley, coordinator), *Homo sapiens neanderthalensis* (Giacomo Giacobini, coordinator), *Homo sapiens sapiens* (Jan Jelínek, coordinator). Write: Giacomo Giacobini, Laboratory of Human Paleontology, Department of Human Anatomy and Physiology, Corso M. D'Azeglio 52, 10126 Turin, Italy.

October 22–25. Nineteenth Algonquian Conference, Washington, D.C. Write: Ives Goddard, NHB Rm. 85, Smithsonian Institution, Washington, D.C. 20560, U.S.A.

1988

February 17–21. Association for Social Anthropology in Oceania, annual meeting, Savannah, Ga., U.S.A. To receive newsletter with information on the program, send U.S. \$20 membership fee to Juliana Flinn, Department of Anthropology, University of Arkansas, Little Rock, Ark. 72204, U.S.A.

April 7–10. Center for Environmental Design Research, International Symposium: Traditional Dwellings and Settlements in a Comparative Perspective, Berkeley,

Calif., U.S.A. Major sessions: The Traditional Dwelling Unit, The Form of Traditional Settlements, Interpretations and Representation of Traditional Dwelling Environments. Write: Jean-Paul Bourdier or Nezar Al-Sayyad, Center for Environmental Design Research, University of California, Berkeley, Calif. 94720, U.S.A.

July 24–31. 12th International Congress of Anthropological and Ethnological Sciences, Zagreb, Yugoslavia. Write: Anita Sujoldzic, general secretary, Organizational Committee, c/o Laboratory of Anthropology, Institute for Medical Research and Occupational Health, Mose Pijade 158, P.O. Box 291, 41.000 Zagreb, Yugoslavia, or Linda A. Bennett (American coordinator), George Washington Medical Center, 613 Ross Hall, 2300 Eye St. N.W., Washington, D.C. 20037, U.S.A.

August 29–September 2. Australian Rock Art Research Association, 1st Congress, Darwin, N.T., Australia. Symposia on rock art studies in the Old World, the Americas, and Australia and Oceania, the rock art of northern Australia, recording and dating methods, interpretation, site management, conservation, standardisation, promotion and publication. Write: AURA, P.O. Box 216, Caulfield South, Vic. 3162, Australia, or, for registrations from Africa, O. Odak, Kenya Archaeological and Ethnographic Research Agency, P.O. Box 10614, Nairobi, Kenya.

October 17–20. 6th Inuit Studies Conference, Copenhagen, Denmark. Write: Jens Dahl, Institute of Eskimology, Fiolstraede 10, 1171 Copenhagen K, Denmark.