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Foregone Conclusions?

In Search of "Papuans" and "Austronesians"[1]

by John Edward Terrell, Kevin M. Kelly, and Paul Rainbird

During much of the 20th century, human diversity and prehistory in the Pacific Islands were often framed in simple terms. Many agreed that there were two kinds of native peoples on the Pacific Islands, sometimes labeled as "Polynesians" (including Micronesians) and "Melanesians" and now more often as "Austronesians" and "Papuans." Furthermore, it was said that these two peoples had arrived in the Pacific during two separate periods of settlement. Some questioned the simplicity of this story, but it continued to dominate how many thought about the islanders and their past. The simplicity of this chronicle masks its deficiencies as a framework for understanding human diversity and prehistory in the Pacific. It is not just simple, it is too simple. It is also based on unrealistic assumptions about the character and differentiation of human populations.

JOHN EDWARD TERRELL is Curator of Anthropology and Director of the New Guinea Research Program at the Field Museum (Chicago, Ill. 60605-2496, U.S.A. [terrell@fmnh.org]). Born in 1942, he was educated at Harvard College (A.B., 1964) and Harvard University (A.M., 1968; Ph.D., 1976). Among his publications are *Prehistory in the Pacific Islands* (Cambridge: Cambridge University Press, 1986) and (with Robert L. Welsch) "Lapita and the Temporal Geography of Prehistory" (*Antiquity* 71:548–72).

KEVIN M. KELLY is Associate Research Scientist at the University of Iowa College of Public Health and Adjunct Associate Professor in that university's Department of Anthropology. He was born in 1956 and received his B.A. from the University of Iowa in 1977 and his Ph.D. from the University of Illinois, Urbana, in 1988. He has published "Gm Polymorphisms, Linguistic Affinities, and Natural Selection in Melanesia" (CURRENT ANTHROPOLOGY 31:201–19) and (with J. T. Clark) "Human Genetics, Paleoenvironments, and Malaria" (*American Anthropologist* 95: 613–31).

PAUL RAINBIRD is Lecturer in Archaeology at the University of Wales, Lampeter. Born in 1966, he was educated at the University of Sheffield (B.A., 1991) and the University of Sydney (Ph.D., 1996). His publications include "Entangled Biographies: Western Pacific Ceramics and the Tombs of Pohnpei" (*World Archaeology* 31:214–24) and "'Round, Black, and Lustrous': A View to Encounters with Difference in Chuuk Lagoon, Federated States of Micronesia," in *The Archaeology of Difference*, edited by R. Torrence and A. Clark (London: Routledge, 2000).

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Throughout much of the 20th century—especially after World War II—human diversity and prehistory in the Pacific Islands were often framed in simple terms. The words used to talk about people in the Pacific and their past varied, but not the basic intellectual framework supporting them. Many, perhaps most, agreed that there were two kinds of native peoples on the islands, sometimes labeled as "Polynesians" (including "Micronesians") and "Melanesians" and nowadays more often as "Austronesians" and "Papuans." Furthermore, and equally decisive, it was said that these two peoples or races had arrived in the Pacific during two separate waves or periods of settlement. Two peoples and two periods: an elegantly simple paradigm or model of the past.

Simplicity, however, is not always a virtue, and many of us nowadays are fascinated by the prospect that complexity—rather than simplicity—is a fundamental property of the universe. While a noted scientist once said that "it is the ability to formulate clear-cut questions which invite yes-and-no answers, where a technique exists, or can be invented, to obtain these answers, which separates the successful scientist from the merely competent professional" (Waddington 1977:122), even he insisted that scientific paradigms are tools for thought, not truths. Furthermore, as every carpenter knows, it usually takes more than one kind of tool to build a well-constructed house.

Now that we have entered the 21st century, we think it is time to review how well the paradigm of two peoples and two periods captures the origins and character of human diversity in the Pacific. We are skeptical about this paradigm not only because it is simple but also because we are concerned that this way of parsing human variation and history may now seem so self-evident that the fit of new discoveries in the Pacific to this way of framing the past is a foregone conclusion.

Here we come to two conclusions of our own. First, the simplicity of this chronicle of two peoples and two periods masks its deficiences as a way of understanding human diversity in the Pacific. This framework leaves most of the history of the Pacific and its people unseen and unexamined. Second, this framework is not just simple, it is too simple. It is based on unrealistic assumptions about the character and differentiation of human populations. As the geneticist Alan Templeton (1998: 647) commented recently, "human evolution and population structure have been and are characterized by many locally differentiated populations coexisting at any given time, but with sufficient genetic contact to make all of humanity a single lineage sharing a common, long-term evolutionary fate."

We think that much of what we say here about research on human diversity and prehistory in the Pacific during the 20th century could probably also be said about

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similar investigations elsewhere on earth, but we leave this for others to judge.

Two Peoples, Two Periods

Drawing comparisons between people in different parts of the Pacific has been standard practice for foreigners and Pacific Islanders for as long as anyone knows. It has been said that coastal people are taller and more intelligent than inland "bush" people, New Zealanders more savage than Tahitians, Samoans more savage than Tongans, and so on. One typification of Pacific Islanders—that they can be separated into "Polynesians" and "Melanesians"—was conventional for many years (Thomas 1997:133–34):

> Although nineteenth-century representations privileged color (the Melanesians being black and the Polynesians lighter skinned), the crucial point in modern anthropological comparison was until recently that Melanesian societies were egalitarian and Polynesian societies hierarchical; the division thus articulated with social evolutionary oppositions between tribes and chiefdoms, the more centralized and hierarchical Polynesian polities providing grist for the "origins of the state" mill. There would, perhaps, be nothing to discuss if the categories mapped real differences, on the basis of biological variation, language, social forms, ancestral groups, or whatever, but they do not. . . .

Nicholas Thomas immediately goes on to say that archaeologists, linguists, historians, social anthropologists, and others agree that the Polynesia/Melanesia dichotomy is basically misleading,[2] but then he adds:

> "Melanesia" lumps together populations with quite different backgrounds (Papuans and Austronesians), while "Polynesia" is better understood as an offshoot or subgroup within Austronesian "Melanesia" rather than a comparable entity. [But] I do not wish to belabor the point that the ethnological categories are invented, which is in any case made irrelevant by the fact that they have acquired substance through their use by scholars and by many Pacific islanders. . . . The point, then, is not that the categories are false, but that their persistence is sustained through reiteration and redefinition, rather than on the basis of self-evident human differences.

In other words, while here he is disputing "Polynesia" and "Melanesia" as proper categories, he retains the underlying supposition that there are two peoples in the Pacific—two "populations with quite different backgrounds." Instead of "Polynesians" and "Melanesians," he says, they should be called "Austronesians" and "Papuans"—and the so-called Polynesians should be included in the former (see also Thomas 1989*b*:212–13; 1997: 16–17).

However labeled, where did the idea that there are two peoples in the Pacific come from?

TWO KINDS OF PEOPLE

In 1813 James Cowles Prichard observed that the inhabitants of the South Sea islands could be divided into "two principal classes" (1973[1813]:250):

> The tribes which belong to the first of these are, strictly speaking, savages. They are universally in that rude unimproved state, which precedes all division of professions and employments. Consequently their political condition is that of perfect equality without any difference of ranks. Their physical character is of the rudest kind. Their form and complexion approximate to those of the Negro. The nations of the second division have greatly the advantage of the former in the condition of society and manners. Among these we find an elevated rank of people who are distinguished in many respects from the lower orders, and particularly in the physical description of their persons. Their form and complexion approach considerably towards those of Europeans, while the aspect of the inferior class borders closely on the rude and uncultivated constitution of the races arranged in the first division.

Prichard called the former class of people "the race of the Papuas" or woolly-headed Negroes. He did not have a distinctive name for the other people, "the tribes to be found in the more distant regions of the Pacific Ocean" (p. 281), and he saw the two as grading into one another. But he was plainly talking about what would later be called Papuans and Austronesians.

By 1843 Prichard's uncertainty about what to call the tribes in the more distant reaches of the Pacific had been resolved; he had decided that they might be termed "a race or family of nations, since a real kindred, or community of origin, has been proved, by affinity of language, to exist among them" (1843:326). Some spoke of these nations as "Malayan," "Polynesian," or "Oceanic." Since "the identity, or the near affinity, of the Malays

2. Those familiar with the conventional culture-area subdivisions of the Pacific will note that Thomas here makes no direct reference to Micronesia or Micronesians. In an earlier paper on this topic (Thomas 1989*a*) and in the volume from which this quotation has been taken, he does note that Micronesia is the third element of the familiar tripartite division of the Pacific islanders into Polynesians, Micronesians, and Melanesians. He acknowledges that a comprehensive review would have to give equal consideration to "Micronesia." In these publications, however, he focuses only on the juxtaposition of "Polynesia" and "Melanesia." While linguists consider most of the languages of Micronesia to be a well-marked grouping ("Nuclear Micronesian" [see Pawley and Ross 1993:239]) within the Oceanic subgroup of Austronesian, the Austronesian languages spoken in western Micronesia (Palau, Yap, and the Marianas) do not belong to this subgroup (Pawley and Ross 1993, 1995). The "Nuclear Micronesian" languages and that of Yap (Ross 1996) are regarded by linguists as most closely related to the Austronesian languages spoken in insular Melanesia (Rehg 1995).

and Polynesians" had by then been "fully established through the researches of Baron William von Humboldt," Prichard opted to call them "the Malayo-Polynesian, or, in short, the Malayan race" (p. 327).

Dividing the islanders into two separate classes, nations, or races of people, however labeled, has been conventional since the early days of European exploration in the Pacific. Until well into the 20th century it was also conventional to interpret this divide in predictably disparaging ways. In 1904, for example, *The World's History*, edited by H. F. Helmolt and published in London by William Heinemann, observed (pp. 308–9):

> Melanesia is [in its present stage of civilization] like a hollow between an elevation in the west, the Malay civilization, and a second somewhat lower elevation in the east, the Polynesian civilization. This by no means implies that the culture possessed by its inhabitants was in itself inferior or lacked originality. . . . It is only in political respects that the Melanesian is behindhand. The cause of this is to be found primarily in the character of the negroid race, and secondly in the absence of any stimulus from outside.

Similarly, the famous 11th edition of the *Encyclopaedia Britannica* not only gives "Melanesia" just a minor paragraph while devoting three and a half densely worded pages to "Polynesia" but, as if to add injury to insult, has this to say about Melanesians:

> The name . . . is derived from the black colour of the prevailing native race, the Papuan and its allied tribes. Many of these differ widely from the parent race, but all the Melanesian peoples have certain common characteristics which distinguish them sharply from the inhabitants of Polynesia and Micronesia. Their civilization is lower. The Melanesians are mostly "negroid," nearly black, with crisp, curly hair elaborately dressed; their women hold a much lower position than among the Polynesians; their institutions, social, political and religious, are simpler, their manners ruder; they have few or no traditions; cannibalism, in different degrees, is almost universal; but their artistic skill and taste, as with some of the lower African negroes, are remarkable, and they are amenable to discipline and fair treatment. Their languages, which exhibit considerable differences among themselves, have features which mark them off clearly from the Polynesian, notwithstanding certain fundamental relations with the latter.

As the 20th century progressed, commentators generally became more guarded about how vividly they portrayed Melanesians and Polynesians as different, but sometimes only marginally so. In 1901 Alfred C. Haddon (pp. 18–19) had written: "Suffice it to say, the features [of the natives of the Torres Straits, who are Melanesian in appearance] are somewhat coarse, but by no means bestial . . . and considering the isolation and favourable conditions of existence with the consequent lack of example and stimulus to exertion, we must admit that they have proved themselves to be very creditable specimens of savage humanity." As late as 1950, one authority remarked: "In New Guinea again there is also a strong dose of the Australian type, thoroughly mixed with Negrito and giving a stumpy, heavy-nosed, broad-mouthed, beetle-browed form of incredible ugliness" (Howells 1950:286).

It was conventional as well during the 20th century to say that the Melanesians had arrived in the Pacific as a people earlier than the Polynesian race. As A. H. Keane (1899:131) put it, "Everywhere the priority of the Melanesians is manifest; their origins are lost in the depths of an unfathomable past." It was further assumed that the Polynesians had arrived sometime early in the Christian era (e.g., Buck 1938, Marett 1912, Keesing 1941). Nonetheless, before World War II, there was also general agreement that the racial history of the Pacific was like a many-layered cake—a story about origins and human migrations that was far more complex than just the notion of two peoples and two periods (e.g., Buck 1938; Dixon 1923; Keesing 1941:44–45; and see Howells 1997).

SCIENCE IN THE ISLANDS

When the four fields of modern anthropology moved into the Pacific after World War II, they all faced the same obstacle: little of interest was known about the islands, the islanders, or their past. For linguists, the task facing them must have seemed particularly daunting, for there were known or rumored to be literally hundreds of as yet unrecorded languages. Perhaps the only thing that could be said with confidence about them all was what Sidney H. Ray had already said in 1907: that they could be "arranged in two divisions which have no common feature in grammatical structure and no likeness in vocabulary" (p. 287)—the Malayo-Polynesian or Austronesian (AN) languages, on one hand, and the Papuan or Non-Austronesian languages (NAN), on the other. As James Cowles Prichard reported in 1843, the former could all be shown to share a "community of origin." In contrast, as the linguist Arthur Capell commented, the latter are a mixed bag of languages that are not Austronesian and "do not appear to form one linguistic family, as the AN languages do" (1962:371).

Even these minimal claims were not as secure as they appeared to be. While the contrasts between the Papuan languages of New Guinea and the Polynesian languages spoken in the central and eastern Pacific seemed as certain as the obvious biological differences in appearance between fair-skinned Polynesians and dark-skinned New Guineans, it was hard to explain the disparity between race and language both in some coastal areas of New Guinea and on almost all of the islands, large and small, falling geographically between New Guinea and Polynesia—the many islands of Melanesia where AN languages are spoken (Bellwood 1979:19). The Polynesians

might look somewhat fair of face, but not so these Melanesians. "The speakers of Austronesian languages other than Melanesian are generally much more light-skinned than those of Melanesian languages, are racially distinct from them, and in Polynesia . . . they are racially homogeneous" (Wurm 1967:25–26).

It was obvious, too, that the 30 or so Austronesian languages spoken in Polynesia are so closely related to one another that some have called them dialects. In sharp contrast, the many hundreds of Austronesian languages in Melanesia are not only mutually unintelligible but in more than a few cases radically different in grammatical structure and vocabulary (Grace 1968; Pawley and Ross 1995:61). How could two peoples so different in appearance as Polynesians and Melanesians nevertheless speak the same—that is, historically related—languages? Why are the Austronesian languages of Melanesia so much more numerous and diverse than the Polynesian (and Micronesian) languages?

One way to solve this conundrum would be to deny the historical comparability of the Polynesian and Melanesian AN languages, and some linguists both before and after World War II did opt for this solution. Capell (1962) argued, for instance, that the Melanesian AN languages are "mixed," hybrid, or pidginized languages that cannot be studied effectively using only the traditional methods of historical linguistics, for they show, he said, differing combinations of Malayo-Polynesian and Papuan language traits reflecting differing histories of language contact between aboriginal dark-skinned Papuans and fairly recent Indonesian (Austronesian-speaking) immigrants.

It would be an understatement to say that this way of explaining the lack of concordance between biology and language in Melanesia was disputed by other linguists—notably George Grace at the University of Hawai'i and Bruce Biggs at the University of Auckland—who had been trained in what were explicitly heralded (e.g., Pawley and Green 1984:126) as the rigorously scientific techniques of linguistic research (the comparative method) forged in the 19th century to study historical interrelationships within the Indo-European family of languages. These linguists championed, in particular, the phonological work done on the Austronesian languages in the 1920s and 1930s by the German scholar Otto Dempwolff, who classified the Melanesian and Polynesian AN languages together as a single "Oceanic" subgrouping of languages within the Austronesian family.

In the process of overturning the "mixed origins" or "pidginization" theory explaining the Melanesian AN languages, classically trained linguists in the Pacific from the 1950s through the 1970s so strongly advocated Max Müller's dictum "Es gibt keine Mischsprache" ("There are no mixed languages") that virtually all research on language borrowing and contact-induced change in the Pacific was tarnished (e.g., Dutton and Tryon 1994; Lynch 1981; Ross 1997:213). William Thurston (1994:573) reports that there may still be a bias in the practice of historical linguistics in the Pacific in favor of viewing contact-induced change as somewhat abnormal or unusual. Nevertheless, there now appears to be greater willingness to think that the family-tree techniques of comparative linguistics and Dempwolff's Oceanic hypothesis may not be wholly able to account for the Austronesian languages of Melanesia (Grace 1986, 1997; Pawley and Ross 1993:432, 435; Ross 1997).

Whatever the case, it is obvious that Dempwolff's Oceanic hypothesis does not explain the lack of fit between biology and language in island Melanesia (Wurm 1967: 27, 32). Far from making this apparent contradiction disappear, the Oceanic hypothesis only serves to underscore it. Stated perhaps too crudely, the puzzlement is not just that while some Melanesians look like Papuans they speak more like Polynesians but also that some Melanesians, at least according to the supporters of Otto Dempwolff, are speakers of truly Austronesian languages, not mixed ones.

A CONUNDRUM

Perhaps linguists may reasonably claim to be color-blind to the apparent biological differences between Polynesians and Melanesians (Grace 1961:367) since there are no known biological determinants of what language, or languages, a person may speak. Biological anthropologists and human geneticists cannot so easily take the same route around the Melanesian conundrum. Many, perhaps most, biological experts since World War II have handled this dilemma not by being color-blind to human biological diversity but by turning a deaf ear to what linguists were saying about the place of the Melanesian languages in the Austronesian family. What could be ignored or put aside as irrelevant for biological research on the Pacific Islanders was the evident purity of the Austronesian languages of Melanesia. That so many people in the Pacific who speak Austronesian languages are neither Malays nor Polynesians surely must mean—or so it may have seemed—that biologically Melanesians are an amalgam, an "admixture," of pure Papuans with equally pure Polynesians, a commingling of historically different peoples, races, or population stocks.

If some linguists after the war took as one of their larger projects the job of confirming the purity of the Melanesian AN languages, the objective of many biological research projects in the Pacific was to show that Papuans and Polynesians are as different as people have always said they are and to prove that AN-speaking Melanesians are biologically—not just geographically—intermediate in their heritage between them. In this endeavor, ironically, historical linguistics continued to provide the underpinning intellectual framework. As the linguistics Andrew Pawley and Malcolm Ross remarked, "much of the main story line" came from their science because "no other discipline has had quite such coherent tales to tell" (Pawley and Ross 1993:426).

The irony here, however, is greater than it may seem. The struggles between proponents of rival linguistic methodologies in the 1950s, 1960s, and 1970s (Grace 1968, Wurm 1967) over the purity of the AN languages

of Melanesia were more about method and theory than about human history. Simply stated, almost no one took seriously Isidore Dyen's (1965) suggestions that the ancient homeland of the Austronesian family had been in Melanesia, not in Southeast Asia, and that the most remote historical relationships within the family were evidenced among the Melanesian AN languages. Instead, nearly everyone in the four disciplines of anthropology agreed, as people generally had in the previous century, that the Austronesian-speaking peoples of the Pacific are Asian, not Melanesian, in origin (Capell 1962:393; Grace 1964:366; Wurm 1967:31–34). When it came to writing about the islanders rather than about their languages, practically everyone used the same story line. First came the Papuans; then came the Austronesians. As Grace (1964:364) wrote, "By about 1500 B.C., Austronesian languages were present on Taiwan, in all major parts of Melanesia, and probably in various parts of Indonesia and the Philippines."

In 1965, in the midst of the battle that linguists were then waging over how to do linguistics in the Pacific, the two peoples and two periods way of looking at human diversity and prehistory took an unexpected turn. The biological anthropologist Gene Giles and two colleagues found biological evidence in New Guinea that Austronesian-(i.e., "MN" for "Melanesian") and Non-Austronesian-(NAN, i.e., Papuan) speakers living there come from "two populations with origins separate in time or space. One of these populations, the MN speakers, appears closely related to modern Southeast Asians and not an autochthonous Melanesian differentiation" (Giles, Ogan, and Steinberg 1965:1160).

If Giles and his colleagues had been linguists, this claim might not have been so unexpected, for this hypothesis is plainly color-blind. It is one thing to say that there is evidence for "a very significant biological difference" between Austronesian- and Papuan-speakers on New Guinea and therefore conclude that "gene frequencies of the MN-speaking New Guineans are closer to those found in Southeast Asia than are those of the NAN speakers," but it is another thing to say that Austronesian- and Papuan-speakers belong to two populations having separate origins in time or space. In the jargon of the biological sciences, this statement can be taken to mean that people speaking Austronesian languages in Melanesia are *not* related to their neighbors who speak Papuan languages and, in fact, constitute a monophyletic lineage or clade. Yet Austronesian-speaking people in Melanesia are basically indistinguishable from Papuan-speakers, and most experts have usually assumed that as a group or "population" they are polyphyletic in origin—a biological admixture of old-time Papuans and newer Austronesian migrants.

Giles's hypothesis was vigorously pursued by others, who similarly concluded that the gene frequencies he had studied were good "markers" of migrants into New Guinea by two "different population stocks," one Malayo-Polynesian, the other "pre-Austronesian" (Curtain et al. 1971:267, 269). In 1975, however, John Terrell and Joel Fagan analyzed the genetic data that had been gathered by then and discovered that the purported distinction between AN- and NAN-speakers in Melanesia did not hold up statistically. Human genetic diversity in the Pacific is too complex to subdivide the Pacific Islanders simply into two populations with separate origins in time or space (cf. Serjeantson and Gao 1995, 1996; Kelly 1996). The evident genetic difference between AN- and NAN-speakers in Melanesia reported by Giles and his colleagues can perhaps best be explained by natural selection (Kelly 1990, 1999).

COMPOUNDING THE DATA

There is no dispute that in the last 30 years of the century just ended, both the quantity and the quality of information about the Pacific Islanders at the command of the four disciplines of anthropology increased greatly. Because of archaeological discoveries since the war, however, we now know that people have been in the Pacific for at least 45,000 years (maybe even > 60,000 years [see Stringer 1999]). For some, however, the fact that the bottom has fallen out of previous notions of time in the Pacific has apparently made little difference. The same basic story of two peoples and two periods is still being widely told. This seems odd, since comparable archaeological and paleontological discoveries in Europe in the middle of the 19th century, which firmly established the antiquity of our species, toppled older entrenched ways of talking about human origins and migrations (Stocking 1987:74–77).

Two centuries ago, for instance, nobody knew how old the Austronesian languages were as a linguistic family. By the 1970s, however, many had concluded that they were at least 6,000 years old (Pawley and Green 1973: 54). The archaeologist Roger C. Green (1975:20) even argued for a time depth of more than 6,000 years just for the arrival of Austronesian languages in the Pacific—an event (if it was an event) that must have happened after the beginnings of the language family itself. Somewhat more conservatively, the linguist Stephen Wurm (1978: 216) argued a few years later that Austronesian-speakers had reached Melanesia about 3000 B.C. or so. However calibrated, the 5,000–6,000 years represented by these estimates strike us as adding up to a block of time during which a great deal worth recognizing and writing about must have happened.

In 1973 William Howells revisited the old problem of Polynesian origins in light of all the new data from archaeology, language, and biology that had been amassed since the war and came to a decisive conclusion—that Polynesian bodies and bones were simply too different from anything in Melanesia for their ancestors to be derived from there, regardless of what linguistics were by then saying. It was obvious, he said, that Polynesians were a homogeneous population that clearly stood out from the observable diversity of Melanesia (1973:228, 232). "As physical beings, the Polynesians simply could not have emerged from any eastern Melanesian population; they are just too different genetically" (p. 234). He was so convinced of this that he declared that their

"Proto-Polynesian" ancestors must have entered the Pacific via a route that kept them "rather well isolated from Melanesian admixture during that sojourn." Following in the footsteps of Peter Buck (1938), he opted for Micronesia as the trail that the Proto-Polynesians must have taken to get to Polynesia.

Six years later Howells reiterated his conviction that the biological picture was "too positive to leave any reasonable suspicion of an ancestral connection with Melanesians." In fact, he argued, it demanded that "the pre-Polynesians [i.e., their ancestors] had no important gene exchange with Melanesians before or en route to their colonization of Polynesia proper" (1979:283). However, his assessment relied on an understanding of human variation gained largely from studies of human skulls, bones, and blood groups. Modern research has painted a less obvious and, we would argue, more realistic picture of Polynesian antecedents. Studies of highly variable polymorphisms such as mtDNA and HLA, in particular, show that the Polynesian genetic repertoire includes a number of "Melanesian genes" (Kelly 1996).

AUSTRONESIA AND THE AUSTRONESIANS

Seen in hindsight, comments by G. B. Milner (1964:394) on a paper on the Austronesian languages written by Grace in the mid-1960s stand out:

> Until recently "Malayo-Polynesian" has been used to refer to a certain type of linguistic structure. Legitimate as it is to speak of peoples or ethnic groups who speak (or spoke) Malayo-Polynesian languages, it may be asked whether it is proper to change an adjective into a substantive, to speak, in the generalized context of material culture and ethnology, of *Malayo-Polynesians*. There is some risk that the unguarded use of this term by non-specialists may set in motion a process of hypostatization which would have regrettable consequences.

He observed that the term "Malayo-Polynesians" would have to subsume "Oceanic Negroes" (i.e., the Melanesians), Malays, Negritoes, and Polynesians, which would be comparable to lumping Scandinavians with West Indians on the basis of language and calling them all "Indo-Europeans." He cautioned that it is not desirable to speak of "Malayo-Polynesians" or "Austronesians," for such a practice "is likely to lead to the hypostatization of ethnic groups underpinning linguistic abstractions."

Mistaking abstractions for substantives is certainly an easy error to make. In 1981, for instance, the bioanthropologists C. Loring Brace and Robert Hinton (1981) published a paper on tooth-size variation in the Pacific in which they argued not only that the Pacific had been settled by just two separate waves of genetically different people but also that the migrants in the earlier wave had bigger teeth than those in the second. Commenting on this paper, Green remarked that "much as one might wish things in Pacific prehistory were that simple, I fear they are probably not" (1981:558). Specifically, he said, they had oversimplified the second wave of settlement—they had conflated "a *hypothetical* initial dispersal of Austronesian languages from Island Southeast Asia into Oceania around 4000 B.C. with a suggested Lapita expansion around 1600 B.C., allowing them to see these two events as essentially one migration, the second of their two-population scheme." He was alarmed that they had cited him to support such a notion.

By the 1990s, however, what Milner had warned about in 1964—turning the adjective "Malayo-Polynesian" or "Austronesian" into the substantive "Malayo-Polynesians" or "Austronesians" (even "Austronesia")—had become a widely accepted practice (e.g., Bellwood 1996; Lum and Cann 1998; Lum et al. 1994, 1998; Pawley and Ross 1993; Spriggs 1996). Signaling that certain languages are historically related by calling them Austronesian languages makes sense. Calling everyone who speaks an Austronesian language an "Austronesian" would, too, if labeling them this way told us something interesting about them beyond the fact that they happen to speak one or more Austronesian languages. Unfortunately, this does not appear to be the case. "Generally . . . there is little which can be characterized as *exclusively* or *uniquely* Austronesian held widely today in common across all Austronesian-speaking regions, and neither should we expect such a circumstance" (Bellwood, Fox, and Tryon 1995:3). If so, then why talk about "the Austronesians"?

STORIES TO TELL

Nowadays it is no longer conventional to regard biological differences among Pacific Islanders as signs of their differing moral leanings, mental abilities, and levels of civilized attainment. Yet it is still fairly commonplace to think of biology as history, to take it for granted that each of us "belongs to" or is "part of" an identifiable tribe, society, or population and that the history of these enduring human corporate entities has left its mark on the biology of those who are "inside them"—that is, to consider history somehow more or less permanently "encoded" in our genes and in our allegedly corporate gene pools. Here we do not contest that genes have stories to tell us about the past. We emphasize instead that these stories are hard to read and that the way we read them critically depends on the kind of history we are looking for and the kinds of "populations" we think we have.

The story about Austronesians and Papuans being told in the 1960s was founded on the assumption that if populations are corporate entities with "separate origins in time or space," then it should be possible to find specific biological traits—certain "genetic markers"—matching this fundamental historical difference. In short, that genetics can help us decide "who's who" in the Pacific. This assumption, however, rests on three key ideas, that human populations are historically enduring collective entities, that different human populations *do* have separate origins (a major assumption [see Templeton 1998]), and that some biological traits are stable enough over

time that they can tell us about the beginnings of identifiable corporate human groups rather than about their later history.

While he did not entirely reject Giles's use of biology to write history in the Pacific in the 1960s, Kevin Kelly's later use of the same genetic traits that Giles had studied led him to propose a different view of biology as history. Instead of assuming that certain genes are unchanging markers of a group's collective or corporate origins in time or space, Kelly asked whether differences in the frequencies of biological traits in different parts of the Pacific might be the consequence of natural selection favoring some kinds of traits (and hence the individuals possessing them) over others. Where Giles had used genes to do what might be called "corporate paleontology," Kelly used them instead to study human evolution. Said differently, Kelly was trying to discover not who's who in the Pacific but what happened to people after they got there.

Recently, however, there has been renewed interest in using genetics once again to pin down the *who* rather than the *what* of prehistory. One promising lead has been a genetic mutation in human mitochondrial DNA (mtDNA)—a nine-base-pair deletion—that is believed to have arisen by accident somewhere in East Asia. This deletion is found in unusually high frequencies throughout Polynesia (its overall frequency there is 93%) and Fiji (82%). In marked contrast, frequencies of this deletion observed among coastal and island Melanesians (0–23%) are similar to frequencies in Southeast Asia (0–20% [Hagelberg et al. 1999]). Thus far, this genetic trait has not been found among Papuan-speakers living in the Highlands of New Guinea (Richards, Oppenheimer, and Sykes 1998). A second promising lead is a highly specific set of genetic traits called the "Polynesian motif" (e.g., Hagelberg et al. 1999, Merriwether et al. 1999), which is found along with the nine base-pair deletion in eastern Indonesia, Madagascar, and farther out in the Pacific (Richards, Oppenheimer, and Sykes 1998). Overlooking its occurrence in Madagascar, it might be better labeled the "Oceanic motif."

It seems likely that the nine-base-pair deletion is so common among Polynesians and Fijians today because their ancestors must have gone through "severe population bottlenecks and expansions" (Richards, Oppenheimer, and Sykes 1998:1235; see also Hertzberg et al. 1989:508; Hagelberg and Clegg 1993:168; Merriwether et al. 1999:259). And since Polynesians and Fijians with the deletion also always have the Oceanic motif, Richards and his colleagues suspect that they know who their ancestors were. With a few rare exceptions, this distinctive genetic motif does not show up anywhere west of the Wallace Line (not even on Taiwan, where some say the Austronesian languages began). Combining these two lines of evidence, it looks as if the ancestors of the Polynesians (and Fijians), whoever they were, must have come from somewhere in the ancient voyaging corridor (Irwin 1992, Swadling 1996) between the Moluccas (the Spice Islands) and places farther out in the Pacific (Richards, Oppenheimer, and Sykes 1998:1235).

While this claim runs counter to the idea that all Austronesian-speakers must have come more or less directly from Asia, the idea that the forebears of the Polynesians (and Fijians) went through a bottleneck is not a new proposal (Harding and Clegg 1996, Hill et al. 1987, Howells 1979, Serjeantson 1989). However, explaining the distinctive homogeneity of modern Polynesians as the result of a genetic bottleneck in the past may not be the right way to describe what actually happened historically to their ancestors. A genetic bottleneck is a selection event leading to a severe reduction in effective population size during which genetic variation is lost (Ayala et al. 1995:188–89). Such genetic events may be sudden or protracted (for instance, a lengthy drought or some other kind of natural catastrophe). The outcome in any case is that only some individuals—those blessed with an advantageous genetic trait or traits—are lucky enough to survive. Metaphorically, in other words, they are the only ones who could pass through the neck of the bottle.

There is no evidence that the Oceanic motif combined with the nine-base-pair deletion gives anyone a biological advantage. It is, therefore, more likely that what happened to the ancestors of the Polynesians (and Fijians) was a genetic founder event (Barton 1998:108–13; Berry 1998:40–44; Grant 1998:84–90; Mayr 1970:124)—that Polynesians and Fijians today are so genetically similar to one another because they are all (more or less) the direct descendants of only a small number of forebears or "founders," sometimes spoken of as "the canoe-load" of people who made it to Fiji and Polynesia. Therefore, instead of being able to trace their ancestors back to everyone in the home region, Polynesians and Fijians are descended from only one or two families there (Houghton 1989, Simmons et al. 1966, Terrell 1986*a*, Vayda 1959).

Explaining the biological sameness of many Polynesians and Fijians this way carries with it an added implication. By definition, founder events lead to a loss of genetic variation. There may be no sure way to know what was lost during such stochastic events. It is anyone's guess, therefore, how biologically homogeneous or diverse other people were in the Pacific at the time when the ancestors of the Polynesians set sail. Some people living in the older settled parts of the Pacific must have had traits now seen also among modern Polynesians; some may have exhibited traits that were more like those currently seen among Southeast Asians; some may have looked more like today's Austronesian-speaking Melanesians. Plainly, it would be unwise to make much out of the *absence* of certain traits among today's Polynesians, although some have done exactly that (e.g., Hagelberg 1997; Hagelberg et al. 1999; Kirch 1997:105). Far from telling us where their ancestors came from, the biological sameness of the modern Polynesians and Fijians may conceal more than it reveals about their origins.

The Fragility of Simplicity

We have documented how saying that the Pacific Islanders can be classified as two kinds of people—sometimes labeled "Polynesians" and "Melanesians," sometimes "Austronesians" and "Papuans"—has become a truism in Pacific scholarship, something so often repeated that the shortcomings of such a framework for understanding human diversity and prehistory in the Pacific may seem inconsequential (Kirch 1997: 116–17). Yet we have documented, too, that when examined more closely, nothing is so simple. Consider these six examples:

First, as the archaeologist Les Groube (1971:313) wrote years ago, we now know that the Polynesians did not "come from" anywhere: they *became* Polynesians *after* their ancestors settled Fiji and western Polynesia some 3,000 years ago (Green 1995). Furthermore, although light-skinned Polynesians may look biologically closer to Southeast Asians than dark-skinned Melanesians, their linguistic relationships show them to be the close cousins of today's Austronesian-speaking Melanesians.

Second, we have already observed that what is now known biologically about the Polynesians makes it certain that their ancestors could not have come directly from Asia. Archaeology, linguistics, and molecular genetics all seem to be telling us the same thing: that the immediate forebears of the Polynesians must have come from somewhere in Melanesia. Even investigators who see them as Taiwanese in origin (e.g., Hagelberg et al. 1999) have to argue that the forebears of today's Polynesians went through one or more population bottlenecks (i.e., founder events) once they got into the Pacific to explain why Polynesians do not look genetically more like Asians than they do.

Third, what archaeologists make of the early potsherds found in the Pacific that are decorated in the ornate Lapita style may influence how much confidence they have in the model of two peoples and two periods. However, "What is Lapita?" is a much contested issue. Some see this pottery as heralding the arrival in Melanesia of foreign-born Austronesian-speaking agriculturalists from somewhere in Taiwan or Southeast Asia; others are less confident that we should see Lapita archaeological assemblages as the sign of both a swift human migration and a crucial turning point in Pacific prehistory (for a summary of current ideas about this pottery and what has been called the "Lapita cultural complex," see Terrell and Welsch 1997). But however Asian or Melanesian its antecedents may be, this pottery cannot be traced directly back to Taiwan. While some may still be assuming that one day Lapita sites will be found in Southeast Asia, so far they have not been. We suspect that most archaeologists now accept that this pottery style was created somewhere in the Bismarck Archipelago northeast of New Guinea, where the oldest known Lapita pottery (dating back to around 3,300–3,200 years ago [Specht and Gosden 1997]) has been found.

Fourth, while Micronesia is often ignored when prehistory and human diversity in the Pacific are discussed, archaeological evidence shows that people using pottery colonized the Marianas archipelago in western Micronesia at the same time as or shortly before Lapita pottery first appeared in Melanesia (Craib 1999). Data currently coming from Palau, also in western Micronesia, may indicate a different but equally early settlement. Although this early Micronesian pottery has some technological affinities with Lapita pottery, stylistically it is not Lapita pottery—which would seem to suggest that 3,000 or so years ago people were voyaging eastward into the Pacific from several places west of there. Yet even this suggestion must be qualified. Malcolm Ross (1996) has argued that Yapese (the language of islands located between Palau and the Marianas) can be traced to the Admiralty Islands in Melanesia. We should mention, too, that today there are two Polynesian-speaking populations in Micronesia (Nukuoro and Kapingamarangi), and the distinctive use of *kava* in Pohnpei (where it is called *sakau*) and Kosrae (*seka*) also links parts of Micronesia linguistically with Polynesia. Furthermore, while the languages spoken on Yap (western Micronesia) and in the Caroline Islands (eastern Micronesia) are mutually unintelligible, there is now ethnographic, historical, and archaeological evidence for a long-term tribute system (the *sawei* network) linking these widely dispersed parts of Micronesia. These are but a few of the complexities to be found within the boundaries of the region traditionally called "Micronesia" (Intoh 1999, Rainbird n.d.).

Fifth, although many have looked for clear-cut genetic markers to distinguish between "Polynesians" and "Melanesians"—or between "Austronesians" and "Papuans"—it is now certain that the genetic history of people in the Pacific is far more entangled (Terrell 1988) than this. Discrepancies between the simple notion that there are two different kinds of people in the Pacific and the growing body of genetic information showing gene flow between people in different places are now so commonplace that some view them as a sign of the "influence" of "Papuans" and "Austronesians" on one another (e.g., Merriwether et al. 1999). Moreover, when gene frequencies are being reported nowadays, they are often being selectively given their own historical explanations, some of which are anything but simple (e.g., Hagelberg et al. 1999).

Sixth, some say that the management of certain plants and animals (chiefly ones thought to be of Asian origin) fueled Lapita's eastward expansion into Polynesia some 3,000 years ago. Lapita, it is said, was a by-product of the Asian "Neolithic Revolution" based on early rice cultivation (Bellwood 1996; Spriggs 1997:84–87). As far as anyone knows, however, rice cultivation in the Pacific in prehistoric times only got to the Mariana Islands in western Micronesia; even there, rice may have arrived only 500 years or so before the Europeans reached Micronesia in the early 1500s (Rainbird 1994:335–36). What is more, some archaeologists are beginning to think that a Neolithic revolution was not even required to get people moving around the Pacific after the end of the Pleistocene. By 6,000 years ago the world's sea levels had

finally stabilized to within a few feet of their modern position. Experts are only starting to document the impact that this new equilibrium may have had on coastal ecosystems and human settlement. For example, along the northern shores of New Guinea—a region viewed by some as strategic for understanding prehistoric Southeast Asian–Melanesian connections (Kirch 1997:55; Spriggs 1997:98)—it is probable (Terrell and Welsch 1997) that newly stabilized coastal lagoons reached levels of natural resource productivity great enough to support significant local human population growth fueled mostly but perhaps not entirely by the harvesting of wild foods (notably, fish, shellfish, nuts, and edible starch from the pith of the sago palm).

Discussion

Some may protest at this point that every scholar knows that the scenario of two peoples and two periods is too simple. Yet it is still commonplace, in fact, to read statements about human diversity and prehistory in the Pacific such as the following (Redd et al. 1995:605; see also Chen et al. 1992, Clark and Kelly 1993, Hagelberg and Clegg 1993, Lum and Cann 1998, Merriwether et al. 1999, Stoneking et al. 1990):

> There are two opposing models for the origins of modern Polynesians. According to the "express train to Polynesia" model, Polynesians are primarily descendants of Neolithic voyagers from island southeast Asia, an expansion estimated to have begun 6,000–8,000 yr ago. According to an alternative model (the "Melanesian" model), Polynesian origins involved a founder event from a genetically diverse source population(s) from Melanesia, where human settlement dates to at least 30,000 yr ago.

The telling phrase here is "There are two opposing models for the origins of modern Polynesians." Two peoples, two periods, and here two hypotheses—there would seem to be something almost magical about the number two.

We suspect that there are basically three ways to react to our observation that the enduring idea of two peoples and two periods is a generalization about the Pacific that has outlived its worth. As in trying to decide what to do with an old car, one can opt for denial, repair, or replacement.

Whether the first option makes sense may depend on how one labels people in the Pacific. As Thomas has observed, many archaeologists, linguists, historians, social anthropologists, and others today agree that the Polynesia/Melanesia dichotomy is fundamentally misleading. Yet we have seen that even Thomas evidently accepts the idea that there are two peoples in the Pacific "with quite different backgrounds" provided the labels are changed to "Austronesians" and "Papuans." Both of these substitute labels are ones that linguists use to talk about language in the Pacific. Remembering Milner's warning that we must be cautious about turning adjectives into nouns, may we safely rescue the idea of two peoples and two periods by switching labels from "Polynesian" to "Austronesian" and from "Melanesian" to "Papuan"? No, this legerdemain does not work. What should we call the Austronesian-speaking Melanesians?

This well-recognized but often ignored conundrum runs deeper than many may realize. Some Austronesian languages in Melanesia, for instance, are more closely related to the Polynesian languages than to the other Austronesian languages (Pawley and Ross 1995:57). In addition, while linguists may not be entirely sure where the Austronesian languages of the central Pacific (Fijian, Rotuman, and all the Polynesian languages) first branched off from the Austronesian family tree (Pawley and Ross 1993:440), they generally concur that the Polynesian languages are descended from a dialect of Old Fijian and that Fijian has its closest ties with the Austronesian languages spoken in some parts of Vanuatu and the southeastern Solomons in eastern Melanesia and not with their geographically more remote Melanesian and Indonesian cousins.

Needless to say, some may still take the first option in reaction to what we say here and possibly agree with Pawley and Ross (1995:60) that the story of Lapita pottery

> bears on the conundrum of how Polynesians and Micronesians, with their Southern Mongoloid physical type, could have come out of Melanesia, where the so-called "Melanesian" physical type, marked superficially by tightly frizzy hair, dark skin and relatively large teeth, is allegedly dominant. The answer appears to be that they did not "come out of" Melanesia: some Oceanic speakers *moved through* Melanesia into the central Pacific, and they moved through rapidly enough to retain their Southern Mongoloid phenotype. Today's Austronesian speakers in Melanesia have acquired Melanesian characteristics in varying degrees by intermarriage in the intervening millenia and by gene flow after the initial dispersal of Oceanic speakers.

We suspect, however, that which of the three options is selected may depend on how one assesses the traditional paradigm of two peoples and two periods. One yardstick sometimes used when making such judgments is how well a given approach accounts for things without leaving too many loose threads. Using this yardstick, we think that the failure of this paradigm to bring Papuan-speakers and the Austronesian-speakers of Melanesia neatly into the cultural, biological, and linguistic tapestry of the Pacific is a major loose end.

There is another possible measure of a model's usefulness—predictive accuracy (Forster n.d.). It was once customary, as we have noted, for Pacific experts to chide their colleagues if they tried to compress Austronesian prehistory into the so-called Lapita period (now conservatively estimated to have been the several hundred

years before and after ca. 1000 B.C., although previous estimates more generously defined the period as ca. 3,500–2,100 years ago [see Green 1989:208]). In the last years of the 20th century, however, many insisted that there had to be an intimate connection between Lapita pottery and the arrival of Austronesian-speakers in Oceania (Kirch 1997, Spriggs 1997). Largely because of this conflation of Lapita pottery and Oceania's Austronesian languages and the popular conviction that the Lapita people were pre-Polynesians (Kelly 1996), some anticipated—made the prediction, so to speak—that expert analyses of the bones of Lapita people would show them clustering statistically with modern Polynesians and Asians, not with today's Melanesians (e.g., Hagelberg and Clegg 1993, Pietrusewsky 1985).

Regrettably, there are still less than a score of human skeletons associated archaeologically with Lapita pottery (Pietrusewsky, Hunt, and Ikehara-Quebral 1997*a*: table 6). Depending on the particulars of each individual examined, cautious studies have established that these human remains (1) cannot be classified as much more than "Oceanic," that is, look neither "Polynesian" nor "Melanesian" (e.g., Pietrusewsky 1989); (2) have "affinities with Polynesians, Southeast Asians, including Taiwan Aboriginals, and East Asians" (Pietrusewsky, Hunt, and Ikehara-Quebral 1997*b*:289); (3) have dentitions that suggest "that the Lapita wares found on this small island were associated with Melanesians" (Turner 1989:296); (4) "are largely irrelevant to questions of Polynesian origins" (Spriggs 1997:123); (5) have a morphology that is "fully compatible with the known Polynesian skeletal phenotype" (Houghton 1989:327); and so on.

One case study seems especially instructive: the relatively complete and reasonably well-preserved skeleton dating to around 500 B.C. found at the "type site" of Lapita on the Foué Peninsula of New Caledonia. This individual, a female, is similar to the other previously described Lapita-associated individuals, but this is not all: "although similarities with Polynesians were noted, the great majority of the skeletal and dental features observed in [this] specimen, and in other Lapita skeletons, suggest affinities with skeletal series from Fiji and eastern Melanesia" (Pietrusewsky, Galipaud, and Leach 1996:49). The authors of this study then observe that "the extant Lapita-associated skeletal record, including this new specimen from New Caledonia, may say more about the contemporary indigenous inhabitants of eastern Melanesia than it does about the ancestors of the Polynesians" (p. 49).

It is true that in the context of the Pacific, the Polynesians (and the Micronesians) may look (physically) like the exception to the rule, but is it really inconceivable that these actually are the remains of some of the early ancestors of today's Polynesians and Austronesian-speaking Melanesians? We know that today's Melanesians are biologically diverse. Why should we assume that anyone in Melanesia 3,000 years ago looked like today's hypothetical "average" Polynesian or Melanesian? Furthermore, as Green (1989:209) and others (see Terrell, Hunt, and Gosden 1997) have asked, is there any reason to believe that the prehistoric Pacific islanders were cut off from the rest of the world? Would it not make more sense to think that people before, during, and after the Lapita period were often—perhaps always—coming and going, arriving and departing, and that by the time voyagers from somewhere in eastern Melanesia (possibly Vanuatu?) finally made the first landfalls in Fiji and western Polynesia around 2,800 years ago (Anderson and Clark 1999, Burley, Nelson, and Shutler 1999), Austronesian-speaking Melanesians were already a thoroughly "admixed" population? Finally, human genetics is not like mixing paint. Admixture does not inevitably lead to overall sameness (i.e., a new kind of homogeneity). In the real world of 3,000 years ago, there is no particular reason that everyone with Lapita pottery would have looked the same—or that they would have all looked like today's Polynesians and Melanesians.

From this perspective, the paradigm of two peoples and two periods is not so much wrong as neither here nor there. Why should we believe that after the initial settlement of the southwestern Pacific at least 45,000 years ago, people stayed away from Melanesia until the Lapita period? If they did not, then we must have a more complex model of the past.

Conclusion

We would guess that few experts today believe that people in the Pacific were ever truly isolated from the rest of the world. Furthermore, even some of today's advocates of a rapid Taiwanese migration of Austronesian-speakers through Melanesia 3,000–4,000 years ago acknowledge that this is an oversimplification (Kirch 1997: 44; for discussion, see Terrell 1997*a*). Nevertheless, some still say that the prehistory of this part of the world can be divided into two peoples and two periods (e.g., Diamond 2000; Gray and Jordan 2000; Kirch 1997:116–17) even though this requires assuming that (1) people in the Pacific were effectively on their own until the arrival of foreign-born Austronesian-speaking migrants around 3,500 years ago and (2) this purported migration was a turning point in history, an "event" so clear-cut and decisive that even today linguists, archaeologists, and others can identify it, tell us who was involved (Asian colonists), and say how these new settlers accomplished all that some say they did (Terrell 1990, 1996).

We accept that there can be payoffs for imagining some regions on earth as isolated places and modeling the history (and prehistory) of these regions as an episodic series of short, decisive turning points punctuating otherwise fairly monotonous periods of stasis or equilibrium. One can then treat a vast region like Melanesia or the entire Pacific as a historical "unit of study" (Spriggs 1997). But why would anyone want to do so?

We have been touching, directly or indirectly, on one likely answer to this question. Until recently, scholarship in the Pacific was largely in the hands of individuals trained in the academies of Europe and the United States

or in local enclaves of Western scholarship in Hawai'i, Australia, and New Zealand. Put simply, it may be that the isolation of the Pacific Islands from the rest of the world and the seemingly uneventful character of their history are simplifying assumptions that may "just seem right" to scholars educated in these scientific and cultural traditions (Kuklick 1996, Rainbird 1999). It is perhaps a well-kept professional secret, but much of everyday science runs on "our native good judgment" (a dictionary definition of common sense). That is, basic common sense is the "default setting" most of us use when we are trying to solve most problems, however humdrum or esoteric. Therefore, even experts are likely to deal with the world as if common sense were good enough until they encounter something that really makes them stop and think (Terrell 1999).

But even if the notion that islands are isolated, uneventful places is a traditional Western idea (van Dommelen 1999), we doubt that this is the only reason that the paradigm of two peoples and two periods may still feel right to some experts. This conventional wisdom readily intersects with other commonsense notions about the world and how it works, and when this happens the apparent remoteness of people in the Pacific and the evident monotony of their past may appear increasingly self-evident. One such reinforcing notion is the widely accepted belief that we have already alluded to that each of us "belongs to" or is "a member of" an identifiable ethnic group, people, or population. This belief treats identifiable (or self-identified) human groups *as if* they were islands, a conviction that the anthropologist Alexander Lesser (1961) called "the myth of the primitive isolate" (Terrell, Hunt, and Gosden 1997). These two mutually enhancing commonsense notions may intersect with a third notion that we have identified, namely, that ethnic groups, populations, etc., can be analyzed as if they were "corporate individuals" with ancestors, descendants, relatives, and "patterns of hierarchical descent" (Mace and Pagel 1994:551). Taken together and accepted as self-evident, these notions may thwart our chances of understanding human diversity and prehistory.

Anyone with a reasonably seaworthy boat or raft will encounter no overwhelming barriers to interisland voyaging all along the great chain of islands and archipelagoes that stretches far out into the Pacific between Southeast Asia and the Solomon Islands (Irwin 1992). There is still much to be learned about how social ties and voyaging interactions may have drawn people together in this "Asia to the Solomons" seaway from the earliest days of human settlement (Irwin 1992, 1999; Terrell and Welsch 1997; Terrell 1998). Western explorers, sailors, and scholars have been hearing linguistic evidence for such a likelihood, however, for hundreds of years. Logic suggests that languages spoken in this great voyaging corridor between Asia and the farther Pacific might have common patterns and a connected history. They do; we now call them the Oceanic branch of the Austronesian family. Conversely, logic suggests that languages spoken outside this maritime corridor might exhibit less evident commonality, less apparent historical organization. They do; we call them the Papuan or Non-Austronesian languages (Terrell 1986*b*:42–64, 247–54).

Seen this way, the islands of the Pacific fit a different paradigm from the one that we have been discussing (Terrell 2000*a*). We would argue that these islands form an enormous geographic array of local and island populations that, in all likelihood, kept more or less in touch with one another ever since the first arrival of people at least 45,000 years ago. Those initial settlers and their descendants, together with new colonists from outside the Pacific from time to time, pioneered distinctive but interconnected historical pathways (Templeton 1998; Terrell 1988, 1997*b*, 1998; Terrell, Hunt, and Gosden 1997). From this perspective, it is unbelievable that the story of the islanders is just a tale about two peoples and two periods of human settlement. It is far more likely that the prehistory of the Pacific was always marked by the kinds of give-and-take between people in different places that not only made us human beings in the first place but have helped to define our species and its history ever since.

Comments

PETER BELLWOOD
School of Archaeology and Anthropology, Australian National University, Canberra, A.C.T. 0200, Australia.
5 IX 00

This article raises a very important point for discussion, namely, the significance of major language families in human history, but it is marred somewhat by condescending language and by its unfortunate belief that there are lots of scholars out there who use the linguistic terms "Austronesian" and "Papuan" to claim that Pacific prehistory is no more than a record of two migrations, one around 45,000 B.P. and the other around 3,000 B.P., separated by a gulf of silence and archaeological emptiness. If there are those who hold such peculiar views, I have yet to meet them.

Put bluntly, I regard the basic contents of this paper as a *cri de coeur* for archaeologists and biologists who wish to denigrate (1) the concept of prehistoric migration in any form, (2) the historical reconstructions of comparative linguistics, and (3) the role in Pacific colonization of the ancestors of those Austronesian-speakers who today live *west* of the Bird's Head of New Guinea (approximately 99% of the living total of 300 million people). This is a paper of straw enemies and innuendo which avoids any direct engagement with the central linguistic questions which are its *raison d'être*, these being simply (1) how and why these language families originated and (2) how and why they have spread from the homeland regions which their genetic relationships demand must once have existed. In the case of Austronesian, the component ancestral languages spread more

than halfway round the world with a clear history of *genetic differentiation*, never mind how much language shift or contact-induced change might have occurred in regions, such as Island Melanesia, on the periphery of the total distribution.

Indeed, the problem with this paper is that it is far too heavily focused on Island Melanesia and Polynesia, hardly centers of gravity for the distribution of the Austronesian language family as a whole. It almost completely ignores those regions of Island Southeast Asia where the Austronesian languages originated. If the authors are right about their "great voyaging corridor," then how do we explain the existence of Austronesian languages in Taiwan and the Philippines, not to mention Madagascar and Vietnam, presumably all regions which are not in the corridor? Furthermore, since they are obviously happy with the view that Polynesians migrated across large distances, then why deny that ability to the other prehistoric speakers of Austronesian languages and have them wallowing instead in some ill-defined "Asia to the Solomons" seaway for the past 45,000 years?

Having said this, it is necessary to reiterate that the issue of the significance of the linguistic terms "Austronesian" (one genetic family of languages) and "Papuan" (several genetic families of languages) is an absolutely essential one for understanding Southeast Asian and Oceanic prehistory. Without discussion of such issues our disciplines would shrivel and die. I also agree with the authors that simple explanations are not always good explanations. But I would accuse them in turn of needless simplicity in conflating the two separate issues of population origins and subsequent history into one narrative model, namely, that of Terrell's "entangled bank." I have no difficulties with the idea of an entangled bank as representing *some* aspects of the prehistoric trajectory in the western Pacific or anywhere else in the world. But I am unable to regard it as forever dominating human affairs. From time to time in history, events have occurred which have absolutely nothing in common with an entangled-bank type of world, and one of these is long-distance population spread from a homeland region, demographically successful and highly motivated. After all, this is exactly how many of us today visualize the Eastern Polynesian migrations from an ultimate Western Polynesian homeland.

I have long argued that the Austronesians represent a population node, albeit not a hermetically sealed one, with a linguistic homeland in Taiwan and a history of dispersal through Island Southeast Asia and the Pacific during the past 5,000 years. This history has involved both demographic-range expansion and, in Melanesia in particular, intensive contact-induced change, not only linguistic but also genetic (Bellwood 1997, 1998, n.d. *a*, *b*). The Austronesian languages spread for the most part but not entirely with native speakers. I am not able to argue fully for these conclusions here, and I know that the authors of this paper do not agree with them. But I do find it odd that they should wish to demote language families on the Austronesian scale to mere sidelines, irrelevant for what they regard as more significant issues of cultural and biological evolution. They need to consider the Austronesian language family more seriously in terms of its *total* extent, not just the Melanesian periphery. They also need to remember that "Austronesians" and "Papuans" (Non-Austronesians) have met *not only* in Island Melanesia. What of the pre-Austronesian populations of Taiwan, the Philippines, and Indonesia? I, for one, see their continuing existence in a biological sense in many parts of Island Southeast Asia as well as in Melanesia. And how, in their final two paragraphs (unless I misunderstand them, in which case my apologies), can the authors hope to convince linguists (e.g., Blust 1995) that the direct ancestors of the Oceanic languages have been spoken in the western Pacific for 45,000 years?

JOEL BRADSHAW
Department of Linguistics, University of Hawaii, 2840 Kolowalu St., Honolulu, Hawaii 96822, U.S.A. (*bradshaw@hawaii.rr.com*). 9 IX 00

I must confess to being a slow learner on the question of "Austronesians" and "Papuans" (meaning "Non-Austronesians"). When I arrived in Morobe Province, Papua New Guinea, in 1976 to document Numbami, the most peripheral and conservative-looking Austronesian language on the south coast of the Huon Gulf, I soon discovered that my hosts did not think of their village the way I did, as a frontier outpost of "Austronesia," like one of the Saxon Siebenburgen defending the frontiers of Christendom in Transylvania. Instead, they traditionally saw themselves as part of a relatively sophisticated coastal—as opposed to inland (or "bush")—cultural orbit, despite acknowledging a period in their recent history during which they lived well inland in a merged village with Papuan-speakers in order to escape revenge attacks from their Austronesian-speaking neighbors up the coast. Before that, they had been forced to abandon their earliest known villages on an offshore island and nearby promontory to escape periodic raids by canoe-loads of Papuan-speakers from down the coast.

For 200 km to the southeast of the present Numbami-speaking home village, all the coastal villagers now speak Papuan languages. (I would bet that more than a few Austronesian-speaking villages once dotted that long coastline, but how could I prove it?) Morobe Province is even more unusual in that most of its large Austronesian-speaking population lives well inland, often at high elevations. Speakers of Austronesian as well as Papuan languages can thus be found on both sides of the salient coastal-inland divide. It therefore seems to me extremely unlikely that the ancestry of everyone in Morobe Province who now happens to speak an Austronesian language can be traced to the "Lapita peoples," as Kirch (1997) would have us believe. At the same time, I find equally improbable the apparent suggestion herein by Terrell, Kelly, and Rainbird (or Terrell 1986*b*) that Papuan languages were confined to the scenic byways off the main Oceanic highways, so to speak. After all, they

are still spoken on the once major island entrepôts of Ternate and Tidore off Halmahera.

In Morobe Province, language-family heritage did not become a widely recognized marker of community identity until German Lutheran missionaries arrived in 1886 and adopted two local languages for two separate evangelical and educational circuits: Jabêm for Austronesian-speakers and Kâte for Papuan-speakers. The pioneering Austronesianist Otto Dempwolff himself not only advised the missionaries on linguistic matters but also devoted the last months of his life to writing up his *Grammatik der Jabêm-Sprache*. Of course, by the time the mission began to make inroads among the Austronesian-speaking mountaineers, its lingua franca had begun to switch to Tok Pisin. So the German-inspired "Pan-Austronesian" and "Pan-Papuan" church circuits in Morobe lasted less than a century.

Most of my research in historical linguistics has centered on reconstructing the pathways by which so many New Guinea Austronesian languages have shifted away from their inherited verb-medial (VO) word-order typology toward the verb-final (OV) typology that prevails among Papuan languages. The degree of "Papuanization" has been much more thorough in some places than in others, but most Austronesian languages on the New Guinea mainland show at least some evidence of innovative adaptations to Papuan models. (In some places, especially toward the western end of New Guinea, the typological adjustments have tended to go the other way: Papuan languages show innovative features typical of nearby Austronesian languages.) Implicit in all my research has been the assumption that the two types of languages have been in contact for a long time and that multilingual populations have periodically shifted their linguistic toolkits from Papuan to Austronesian or vice versa.

Nevertheless, it was not until the 1990s, after reading quite a few histories of imagined communities and invented traditions and then investigating more closely the actual groups on the ground around the Huon Gulf, that I began to question my earlier use of pseudoethnonyms derived solely from chance present-day linguistic affiliations. Even in an article emphasizing the perpetual ethnolinguistic turbulence in coastal New Guinea (Bradshaw 1997), I sometimes violated my own thesis by indulging old habits of talking about language-defined groupings as if they were long-enduring, isolable social entities.

So I agree with Terrell, Kelly, and Rainbird (and Milner 1964) that "Austronesian" and "Papuan" make better linguistic than ethnic archetypes and that we can no more carve up Oceania into Papuans and Austronesians than we can carve up Britannia into Celts and Anglo-Saxons, however appealing such dichotomies are to the popular imagination. We know better than to think the British Isles were settled in just two major waves of migration. Why should we think the settlement of Oceania was any simpler?

DAVID V. BURLEY
Department of Archaeology, Simon Fraser University, Burnaby, B.C., Canada V5A 1S6 (*burley@sfu.ca*).
16 IX 00

The paper by Terrell, Kelly, and Rainbird is most difficult to comment upon. On the one hand, I sympathize strongly with their underlying thesis—we must be careful of overly simple models leading to foregone conclusions in Oceanic prehistory. The region is an extremely complex one, historically populated by maritime-adapted cultures with a penchant for exploration and interisland voyaging. As a consequence we can expect considerable gene flow as well as cultural exchanges throughout prehistory. On the other hand, I suspect that I am one of the individuals at whom the authors take aim. In the face of their conclusions, I continue to believe that the spread of Austronesian-speaking peoples with Lapita pottery was a critical watershed in the settlement of the western Pacific. These are the colonizing populations for all of the islands of Remote Oceania eastward to Tonga and Samoa and ultimately the ancestors of the Polynesians as a whole. The movement of the Lapita peoples is a well-documented chapter in world prehistory based on a considerable platform of archaeological, linguistic, and, more recently, genetic data (see Kirch 1997, Spriggs 1997, among others). It is not, as the authors would have us believe, a simplistic assumption.

This paper must be given credit for providing a historical critique of the conceptual development and use of a Melanesian/Polynesian dichotomy in Oceanic anthropology. These concepts are rooted in a framework of 19th- and early-20th-century racial categorization that ignored linguistic and cultural affiliations. The conundrums, alternatives, and fudging that resulted do make interesting fodder for discussion. Yet to suggest that a large core of contemporary researchers have now replaced "Melanesian" with "Papuan" and "Polynesian" with "Austronesian" on the hypothesis of two peoples, two migrations is surely an overstatement. The Lapita migration through Near and into Remote Oceania does not seem to have been a single or a simple event. It occurred over a 500-year time span, and it undoubtedly represented many exploration and settlement episodes by groups having quite different motivations. With newly established radiocarbon chronologies for Vanuatu (Bedford 2000), New Caledonia (Sand 1997), Fiji (Anderson and Clark 1999), and Tonga (Burley, Nelson, and Shutler 1999), Jared Diamond's (1988) so-called express train has been slowed considerably, if not derailed. That these different groups of people shared the structural and decorative elements of a Lapita pottery tradition and that their descendants in Remote Oceania now speak Austronesian languages nevertheless belies a common ancestry in the longer term.

Much of the paper's concern is with the disparity between how people look and what languages they speak. It is asserted, for example, that "Dempwolff's Oceanic hypothesis does not explain the lack of fit between biology and language in island Melanesia." Dempwolff's

Oceanic hypothesis is about a phylogenetic subgroup of languages that diverged from a common parent (proto-Oceanic) at some time in the distant past. Since the biology of island Melanesia that is being referred to is in the present, why should we expect Dempwolff's hypothesis to explain such variation? The prehistory of Fiji and Tonga, the adjacent archipelagos in one of the so-called bottlenecks for Polynesian origins, illustrates the difficulties of such expectations.

The archaeological records for the Lapita period in Fiji and Tonga are virtually identical in terms of chronology, the material record, the processes of settlement, and subsistence economy (see, e.g., Best 1988, Burley 1998, Clarke 1999). The facts of geography imply that the west coast of Viti Levu in Fiji was probably the first landfall for Lapita settlement, but within a very short period of time people had moved to other areas, including the islands of Tonga. There is every reason to expect that these colonizing peoples looked alike and that the phenotype was of Polynesian form. Supported by comparative linguistics (and logic), it can also be argued that these people spoke a common language (proto-Central Pacific). The problem today is that the population of Fiji, exclusive of Lau, is Melanesian in appearance, placing Fiji, in a historical sense, among the island groups of Melanesia. How can this be if these peoples have a common ancestry that went through a "bottleneck" less than 3,000 years ago? Obviously in the post-Lapita Fijian past there have been considerable interaction and gene flow with island Melanesian peoples to the west, a situation that does not obtain farther east in Tonga. Given that the Fijian and Tongan archaeological records are very similar in the first 1,000 years of occupation, the phenotypic transition of an entire archipelago may well have taken place within the span of less than two millennia. Why, then, could this not have occurred elsewhere in island Melanesia? If so, why are language and biology now so incompatible in Terrell et al.'s view?

This paper represents one more volley in an ongoing and often acrimonious debate in Oceanic prehistory (see Kirch and Green 1987, Kirch 1997, Terrell, Hunt, and Gosden 1987, Terrell and Welsch 1997). The debate ultimately focuses upon the applicability of a phylogenetic model for culture history, the association of Lapita ceramics with Austronesian-speaking peoples, and the validity of historical linguistics for reconstruction of the past. I must conclude that there is nothing in the present set of arguments that would lead me to question any of the associations or methods stated above. To the contrary, it is precisely the merging of linguistic, biological, cultural, and archaeological data that makes the prehistory of Oceania so exciting.

ROSS CLARK
Institute of Linguistics, University of Auckland, Auckland, New Zealand. 15 IX 00

Terrell, Kelly, and Rainbird present a catalogue of discontents with research on the prehistory of Pacific peoples. Their difficulty, however, seems to be not so much with intractable problems or neglected areas of study as with errors in terminological usage and the supposed tyranny of certain abstract conceptual schemata. I will comment on just three points relating to linguistics.

Citing Milner, Terrell et al. describe talking about people as "Austronesians" as an "error" (though later they moderate this to advice to be "cautious" with such usages). But there is no error, either grammatical or ontological, in using "Austronesians" as a noun, provided one is clear what one means by the term—most commonly, "speakers of Austronesian languages." There may, as Terrell et al. maintain, be few or no common cultural or genetic traits that can be ascribed to this large and diverse group of people today. But the fact of their all speaking languages belonging to the same family has always seemed to be of some significance beyond linguistics alone. Milner and Terrell et al. attempt a reductio ad absurdum by an analogy with "lumping Scandinavians with West Indians on the basis of language." But even if the Englishman may be unwilling to consider the Jamaican an "Englishman," the fact that the Jamaican speaks English does reflect something important about his history.

The authors argue that "Austronesian" and "Papuan" are mere "substitute labels" for what were once referred to as "Polynesian" and "Melanesian." Scholars have apparently been in the grip of some dualistic archetype which impels them to divide Pacific peoples or languages into two groups; when Polynesian versus Melanesian is no longer credible, they begin to talk of Austronesian versus Papuan. But this equating of the two distinctions can be maintained only by emptying them of all content. Consider the following passage (Ray 1926:24):

> In 1892 I showed definitely that there were in New Guinea languages which were entirely different in vocabulary and grammar from the Melanesian and Polynesian of the islands and also from the undoubtedly Melanesian of New Guinea itself.
>
> For these non-Melanesian languages of New Guinea I used the name Papuan. This did not imply any community of character or origin in the languages so-called, but merely served as a convenient term to indicate their archaic features as the probably aboriginal languages of the great island of New Guinea.

Anyone approaching the above with the presumption that, as Terrell et al. repeatedly insist, "Polynesian/Melanesian" and "Austronesian/Papuan" are merely alternative terms for the same distinction will be baffled by Ray's reference to Papuan languages as being neither Polynesian nor Melanesian. Nor do his words support the claim that a "two peoples, two migrations" school of thought has dominated the field for a century or more, as he emphasizes that "Papuan" is simply a cover term for "non-Austronesian" in the New Guinea context and does not imply a unity of origin. This heterogeneity is

duly explained in all accounts of Papuan languages since Ray's time. (Likewise, those who still feel that "Melanesian" has some use as a linguistic term are clear that it is simply a useful shorthand for "non-Polynesian Oceanic.")

Binary distinctions are, of course, a basic component of our thinking. It is hard to move far in understanding languages (or anything else) without being able to say "These languages have property X, whereas these do not." The discovery of the linguistic unity of Austronesian and its subgroups was an important step in our knowledge of human history in the Pacific, and it is only right that it should be mentioned again and again. But to elevate this distinction to the status of a "paradigm" or "framework" from which it is necessary to seek escape is no better than caricature.

If there is a paradigm or framework in the discussion, it could be classical comparative linguistics. Terrell et al. credit Grace and Biggs with promoting this method in the post–World War II era (which seems to function as a kind of Dark Ages in their account), though noting that they were following the trail blazed by Dempwolff some decades earlier. Apparently research on language contact was "tarnished" under the Grace-Biggs regime—a surprising claim when one considers that one of Biggs's major contributions to the field included both a comparative reconstruction of Eastern Oceanic languages and a classic language-contact study (based on the results of the reconstruction) which illuminated multiple Polynesian strata in the language of Rotuma (Biggs 1965). Good language-contact studies require very good descriptive and comparative data, which probably accounts for their relatively late appearance, but the field could fairly be said to be flourishing at the moment (Dutton and Tryon 1994, Geraghty and Tent n.d.).

BRONWEN DOUGLAS
Division of Pacific and Asian History, RSPAS, Australian National University, Canberra, ACT 0200, Australia (*bronwen.douglas@anu.edu.au*). 19 IX 00

I welcome this paper as one of the unnamed "some" alluded to for having "questioned" the "two peoples, two periods" model of the Oceanic past (Douglas 1979, 1998, 1999). Terrell, Kelly, and Rainbird offer a valuable critique of the recent reconstitution by linguists, archaeologists, and biological anthropologists of the orthodox dichotomy of "Polynesians" (plus "Micronesians")/"Melanesians" as "Austronesians"/"Papuans." To a non-specialist in these disciplines the argument is informative and convincing. I leave its evaluation to experts and focus on the history of European taxonomies of Pacific islanders within which the authors locate their critique.

They ask: "Where did the idea that there are two kinds of people in the Pacific come from?" and answer: "Dividing the islanders into two separate classes, nations, or races of people has been conventional since the early days of European exploration." Their primary culprit is the 19th-century ethnologist Prichard. There are historical and logical problems here. First, the binary construction of Pacific humanity was never homogeneous or uncontested: inaugurated by Forster in 1778 as "two great varieties of people" (1996[1778]:153) and given its modern contours by Dumont d'Urville as "two distinct races" (1832), it has recurred, retreated, and mutated. Second, if the number two is "almost magical" for recent scientific writers, it was magic to which Europeans were oblivious before the taxonomic age and to which Forster's immediate successors were often immune. Third, while in the 18th century "class," "nation," and "race" were synonyms for "variety," their modern meanings differ significantly: notably, in the early 19th century "race" acquired its scientific sense of hereditary physical differences between circumscribed human "races"; in the late 20th century the assumed reality of "races" was disputed by geneticists and racism became problematic, but the concept of "race" retained its ontological status. In debunking "the simplicity of this chronicle of two peoples and two human migrations," Terrell et al. ignore semantic and discursive shifts and singularize several intellectual stories by assuming that different dualisms are the same and meanings do not change.

In 1610 the Spaniard Quirós differentiated people in both eastern and western Pacific islands along a continuum of skin color: "the people of these countries are many, their colors are white, dark brown, mulattoes, and Indians, and mixed of one and the other" (Sanz 1973:23). Later anthropologists assumed that this inventory mimicked the realism they accorded dual racial taxonomies, but Quirós gave such "differences" no geographic or racial coordinates. Neither did his successors before Forster, Roggeveen, and Bougainville, though Bougainville thought he saw "two very different races" *within* Tahiti (1771:214). Terrell et al. establish Prichard's division of Pacific islanders into "two principal classes" by quoting decontextualized passages from works published in 1813 and 1843 and asserting that the earlier passage "plainly" referred to "what would later be called Papuans and Austronesians." This is blatantly teleological in terms of the history of representations, equivalent to the "foregone conclusions" about islanders that the paper properly denounces. It is also wrong, since in 1813 Prichard's criteria were not linguistic but social and physical: all Pacific people were "branches of one stock," a "race . . . of which one tribe is savage, and the other civilized"; white skin was an evolutionary attribute of "Civilization" (1973[1813]:223–26, 233, 250). In his third edition Prichard revised the schema in view of copious new information about Pacific people and languages and recent racial classifications such as Dumont d'Urville's. His "one stock" became "*three* principal groups": the "Malayo-Polynesian tribes" were defined by "affinity of dialects" and paralleled Dumont d'Urville's "Polynesians," but the "Oceanic Negroes" and "Alfourous," which split Dumont d'Urville's "Melanesians," had in common only physical similarity to "the Negro races of Africa" (1836–47:4). There was also a discursive shift: a lifelong evangelical monogenist, Prichard in 1813 used

the term "race" rarely and always in its loose 18th-century sense, whereas by the third edition his prose had been infiltrated by the language of scientific racism.

Terrell et al. tiptoe around both racism and antiracism: leaving racism to speak for itself or playing it down as "predictably disparaging," cloaking antiracism in the euphemism "color-blind" and the seeming objectivity of biological and genetic terminologies. Yet the politics of racism and antiracism are integral to their case—to skirt them obfuscates the story. Historically, the four main terms in their recursive dual sets are incommensurate, particularly with respect to race. "Polynesia" (from Greek *polloi*, "many") is a descriptive geographical term given oceanwide scope by its inventor, Charles des Brosses (1756), and unevenly contracted to its current regional, linguistic, and ethnic limits. Dumont d'Urville's classification was explicitly racial because his term "Melanesia" (from *melas*, "black") referred to the skin color of the inhabitants, denigrated as "very inferior to . . . the coppery race" (1832:6). "Papuan" (from Malay *papuah*, "black, frizzled") is the oldest and most substantive term: heard by early-16th-century Spanish travellers in the Spice Islands and applied descriptively to the "black" inhabitants of what they called "New Guinea" (Gelpke 1993), a term of bitter racial disparagement in Europe by the 19th century, attributed indigenous political reality in Papua New Guinea and West Papua. Terrell et al. aptly condemn the hypostasis of linguistic adjectives into ethnic substantives, but "Papuan" had ethnographic substance long before it was a linguistic abstraction. Conversely, its hypothetical linguistic correlate "Austronesian" (from Latin *australis*, "southern")—the most recent and racially neutral of the terms—is now often used academically as an ethnic substantive but lacks the indigenous credibility which the older terms enjoy thanks to their postcolonial recuperation in nationalist, ethnic, and regional identity politics.

Quirós's human kaleidoscope, Forster's "two great varieties," Prichard's "two principal classes" of 1813 are of analogous orders of difference from Dumont d'Urville's "two distinct races" as is the "color-blind" modern linguistic and biological consensus of "Austronesians" and "Papuans." Apart from subject matter, dualism is all these representations share—those of Quirós and the later Prichard shared not even that. Otherwise the semantics and racial politics differ markedly. While applauding Terrell, Kelly and Rainbird's assault on the reduction to simple binaries of complex, entangled human pasts, I mistrust their parallel reductionism: the misleading "simplicity" that conflates incommensurate concepts and theories and elides complexity in the equally entangled pasts of the history of representations and ideas.

R. C. GREEN
Department of Anthropology, University of Auckland, P.O. Box 92019, Auckland, New Zealand. 14 IX 00

The theme of two in the settlement of the Pacific before European contact is still, after 20 years, a gross simplification of matters and continues to be a position I do not hold. Here Terrell et al. and I are in agreement. Their concern is that this way of looking at human variation and Pacific prehistory has become self-evident, a "foregone conclusion" for research. I don't know why they think this, especially when numbers of us are publishing accounts that are rather differently constructed. Moreover, in them prehistory in the Pacific is described as a rather long, complex, and eventful set of developments—not at all the "apparently monotonous past" averred to be generally portrayed in this literature.

Provocatively, Terrell et al. ask why anyone would want to treat the entire Pacific as a historical "unit of study" in its own right. The short answer is that it yields interesting insights into some of the enduring deep structures and events which underlie the ephemeral happenings and short-term occurrences making up the detailed regional and island group histories that we also write. Broad overviews allow taking up everything from the colonization of the Pacific, posed as an extended series of five punctuated events, to thematic treatments of successive developments in Lapita or interregional sequences.

For example, Ancient Near Oceania, as I call it, has seen multiple external contacts over some 40,000 years prior to the appearance on the scene of Oceanic-speaking Austronesian populations in Modern Near Oceania ca. 3,400 years ago. A number of different parental paleopopulations (in the New Guinea Highlands, Bougainville, and Australia) with an antiquity of 10,000+ years can be identified, along with at least a dozen fundamentally distinct language stocks. Some may have very deep but currently untraceable roots within the region, but it is likely that a reasonable number among them had origins outside it.

Moreover, at the point when Modern Near Oceania developed, during the mid-Holocene rise in sea levels, and came to be culturally distinct from Australia and Island Southeast Asia, there is, as I put it, sound evidence for "additional external inputs in each region 6,000–3,500 years ago." Allen and O'Connell (1995) and Swadling (1996) concur. I am unaware of any large body of scholars believing after the initial settlement of the Pacific 45,000 years ago "everyone stayed away from Melanesia until the Lapita period." That seems a straw issue no longer requiring extended comment.

In recent writings I find little use for the term "Papuan" as a reference to Near Oceanic languages not Austronesian and see no justification for it as either a biological or a cultural category. As disturbing is continuation of "Melanesian" as an analytical category. Certainly people have used it that way in the past, but there is no valid linguistic subgrouping called "Melanesian Austronesian languages," only Oceanic, which

covers a very much wider Pacific region. And there is no biologically well-marked population that can be termed "average" Melanesian, even if deemed to be of diverse and polyphyletic origin. In labelling people from the Pacific genetically, the question "What should we call the Austronesian-speaking Melanesians?" is meaningless because "Melanesian" defines an unacceptable biological category.

Terrell et al. cite Turner's claim that the teeth of Watom Lapita skeletons were of Melanesian affinity. In reality they are like teeth of local populations on New Britain speaking Oceanic languages. However, this comparison and that to two other local population samples from Thailand and Hawaii are not valid statistically (Konigsberg 1992). Along with Pietrusewsky (1996), Visser and others (Green 1995) have shown that Watom dental and skeletal features exhibit close affinities with ca. 1,700-year-old skeletal series from Fiji and other parts of western Remote Oceania, including a Fijian Lapita skeleton from Lakeba and another from Tonga. Pietrusewsky, Hunt, and Ikehara-Quebral (1997*a*) have also published a Wayan Fiji Lapita skeleton exhibiting Asian affinities, an observation Kirch and associates made about the Near Oceanic Mussau Lapita remains. This cluster of early skeletal samples from western Remote Oceania identifies what one paleo-population once looked like, usefully designated neither "Melanesian" nor "Polynesian." It indicates the kind of people associated with the Remote Oceanic Lapita cultural complex and perhaps even Lapita in the Bismarck Archipelago.

To summarize, I continue to inveigh against the use of "Melanesian" as a serviceable analytical device when writing the Pacific's culture history and demonstrate this in overviews which include discussions of linguistic, biological, and culture-historical matters without using Papuan or Melanesian analytically. Others should also move in that direction.

In contrast, when writing about historical linguistics, "Austronesian" remains a sound category, though a great difference obtains between that broad concept and one designated "Proto-Austronesian." Correlating various language subgroups in Austronesian with particular archaeological units has to be in terms of proto-languages and their subgroups, often conceived of as dialect chains. The result is a lessening degree of confidence in outcomes when one proceeds from low-level to high-level proto-languages like Proto-Malayo-Polynesian and Proto-Austronesian. Not all claims are of equal merit. One has to be more guarded when suggesting cultural or biological correlates for Proto-Austronesian.

The recent Oceanic Austronesian input into parts of Near and Remote Oceania is often viewed as a cultural horizon stretching from the Sabah/Maluku area of Island Southeast Asia to the Polynesian homeland region. Good grounds exist for characterizing this as a late event in Pacific colonization. First, a reasonable case may be made for its association with a core of cultural items recurring in sites from one island group to another between 3,400 and 2,700 years ago. Next, a very plausible though not definitive case may also be made for its association with Proto-Eastern-Malayo-Polynesian and Proto-Oceanic. Moreover, when a decorative art style from tattooing and barkcloth appears on the horizon's elaborate red-slip vessel forms in association with a range of other items, it constitutes a Lapita cultural complex. When people carry this complex to western Remote Oceania as first inhabitants, they form "a rapid and integrated burst of colonisation" approaching an ethnic category (Irwin 1992). The horizon therefore represents a punctuated temporal event in Pacific prehistory, where language, culture, and biology exhibit an expected cohesion. In this situation the proto-languages are reasonably tied to a distinct archaeological entity and a biological paleo-population, which become firmly intertwined in those people who first settled western Remote Oceania.

Because this horizon encompasses a narrow temporal band across the southwestern Pacific, it approximates a "fast train," unlike the other parts of the Austronesian language family and its proto-languages, with their presumed archaeological and biological correlates, which precede or follow it. In those instances any postulated train moved more slowly, with several pauses. As commentators have noted previously, Pacific prehistory is not completely entangled; it tracks aspects both reticulate and phylogenetic. Terrell et al., in condemning a too simple view of the Pacific's past, provide a rather opaque account in relation to those, undeterred by foregone conclusions about Austronesians and Papuans, that give phylogeny its due.

MICHIKO INTOH
Department of Social Research, National Museum of Ethnology, Suita, Osaka, 565-8511, Japan (*intoh@idc.minpaku.ac.jp*). 15 IX 00

Micronesia is often ignored when prehistory and human diversity in the Pacific are discussed, as Terrell et al. point out—and the limited space given to it in this paper is no exception. Also, the descriptions of Polynesian-speaking populations in Micronesia are rather misleading. In this discussion, I note some new data and review linguistic, genetic, and archaeological evidence in relation to the colonization of Micronesia and the complexity of Austronesian dispersal into the Pacific.

Terrell et al. report evidence of long habitation, more than 3,500 years, in the Marianas archipelago and Palau in Micronesia. Additional palaeoecological data are now available from Yap. Analyses of two sediment sections indicate a major period of forest destruction accompanied by fire at 3,300 b.p. (Dodson and Intoh 1999). If these data are accepted, it seems that western Micronesia was colonized about the same time or even before the Lapita-making Austronesian people reached the Admiralty Islands.

Too much stress may have been put on Austronesian dispersal through Melanesia in association with Lapita pottery. There was no fence, of course, separating Micronesia and Melanesia as people moved into the Pacific.

Looking at movements to the southeast (Melanesia) and to the northeast (Micronesia) as a whole should give us a more coherent view of dispersal from Asia into the Pacific.

Despite the long habitation history of western Micronesia, the rest of Micronesia was not settled until 2,000 years ago. Terrell et al. mention only the Polynesian influences to indicate the complexity of human diversity in Micronesia and use linguistic evidence concerning kava to demonstrate the link with Polynesia. However, the influence of Polynesian-speaking populations in the history of Micronesia is late and minimal compared with that of the Nuclear-Micronesian-speaking population coming from Melanesia. Also, it is not clear that the use of kava (*sakau*) was brought from Polynesia. It is more likely that it was brought from Melanesia, possibly from Vanuatu, where use of the plant apparently began and where Nuclear-Micronesian-speakers came from (Linch 1996, Lebot, Merlin, and Lindstrom 1992).

The Nuclear-Micronesian colonization took place around 2,000 years ago, and linguistic evidence indicates an origin in the Solomon-Vanuatu region (Intoh 1997). This date is close to the date of dispersal from Samoa to eastern Polynesia. Is this a coincidence? The answer will depend on what we learn about human genetic mixing, as Terrell et al. indicate.

Genetic studies have certainly shown some evidence of mixing between eastern Micronesia and Melanesia (Lum and Cann 1998, Lum et al. 1998). The variation is, however, within the range of difference for physical features among "Asians" and is obviously different from the Australoid range (with which Melanesians are physically identified). This is why earlier researchers mistakenly suggested that Polynesians came through Micronesia. This also leads to some intriguing questions: If Austronesian-speaking Melanesians were already a thoroughly admixed population by 2,800 years ago, as Terrell et al. argue, why don't Micronesian populations show greater variation? Was there another bottleneck between Melanesia and eastern Micronesia? More specifically, did some Austronesian communities resist interaction with neighboring Melanesian communities and then later emigrate?

A further topic must be raised. Terrell et al. state that Polynesians could not have come directly from Asia and that they "do not look . . . like Asians." Which Asian populations do they have in mind? Modern Asians in Southeast Asia? We must remember that genetic change occurred not only in the Pacific but in Asia as well, and perhaps most rapidly in the period following the initial Austronesian voyages into the Pacific. One example can be found in Japan. The Jōmon population, living in Japan since before 12,000 years ago, is thought to be derived from southern China. In cranial features the Jōmon people do not look like modern Japanese but do resemble prehistoric Polynesians (cf. Hanihara 1993, Katayama 1996). After admixture with Yayoi immigrants around 2,300 years ago, their morphological features radically changed. Although the expansion of Yayoi-type populations is not well understood, similar changes in Southeast Asia are conceivable.

A recent study of human leucocyte antigens among the Taiwan aboriginal tribes demonstrated similarity with the New Zealand Maori (Lin et al. 1999). Further physical, biochemical, and genetic studies will eventually provide us with sufficient data to substantiate arguments about the nature of population movements in the Pacific.

FIONA M. JORDAN AND RUSSELL D. GRAY
Department of Psychology, University of Auckland, Auckland 92019, New Zealand (*f.jordan@auckland.ac.nz*). 14 IX 00

We read Terrell, Kelly, and Rainbird's paper as an attempt to clarify the difficulties and dangers of simplistic conceptual frameworks for Pacific prehistory. We agree that periodic reexamination of the assumptions and models that inform research is desirable. However, we are not convinced that the way forward lies in further handwringing about "foregone conclusions." Models are lies that lead us towards the truth. The "two peoples" model is a much better first approximation to the genetic, linguistic, and archaeological data than a maximally connected network (e.g., Bellwood 1991, Blust 1995, Lum and Cann 1998, Melton et al. 1995). Terrell et al. note that the situation is more complex in Island Melanesia and Micronesia, but this does not mean that everyone in the Pacific interacts with everyone else or, in Terrell et al.'s words, "that these islands form an enormous geographic array of local and island populations that, in all likelihood, have remained more or less in touch with one another ever since the first arrival of people at least 45,000 years ago." Rather than seeing the "two peoples" and "entangled bank" models as mutually exclusive, we have argued that they "are best characterized as two ends of a continuum of modes of human prehistory" (Gray and Jordan 2000:1053). Our task, therefore, is to determine where on this continuum specific localities and peoples lie. Patterns in the Sepik coast region may differ from those in Polynesia or the Philippines. We believe that resolving the details of human settlement, colonization, and diversity in the Pacific requires (*a*) more genetic, linguistic, and cultural data, (*b*) methods that enable these data to be analyzed precisely, and (*c*) a coherent framework for synthesizing the different types of data.

Elsewhere (Gray and Jordan 2000) we have argued that one potentially useful way of achieving *b* and *c* might be to use evolutionary biology's quantitative phylogenetic methods to analyze linguistic and cultural data. Reticulation can be quantified with these methods by measuring the fit of items on an optimal tree. Specific patterns of reticulation can then be investigated with methods that do not assume a tree model, such as spectral analysis and split decomposition (Bandelt and Dress 1992, Hendy and Penny 1992). Our preliminary application of phylogenetic methods to Austronesian linguistic relationships (Gray and Jordan 2000) found both a

strong colonization signal and evidence for reticulation between some languages (e.g., Chamorro and Palau appear to have borrowed from Oceanic languages).

Most Pacific scholars would no doubt agree that people were "coming and going, arriving and departing" throughout the period of human settlement in the Pacific. However, this statement is so vague that it is of little practical use. What we would like to see is more definite predictions. We challenge Terrell et al. to be more specific in their reply: Who was coming and going? From where? At what time? What current data support these inferences? What patterns may lie waiting in the archaeological, linguistic, and genetic record? If the Austronesian expansion was not clear-cut, exactly how diffuse was it? If Terrell et al. are unable to be more specific, then their theoretical concerns will become irrelevant to those doing research in the Pacific.

AKIRA KANEKO
Department of International Affairs and Tropical Medicine, Tokyo Women's Medical University, 8-1 Kawada-cho, Shinjuku-ku, Tokyo 162-8666, Japan (*akirak@research.twmu.ac.jp*). 15 IX 00

In the Pacific, there is generally no malaria east of 170°E longitude and south of 20°S latitude. The so-called Buxton line, which defines the eastern limit of anopheline breeding, separates malarious Vanuatu from malaria-free New Caledonia and Fiji. The only malarious island outside the Buxton line is Aneityum, the southernmost inhabited island of Vanuatu (Buxton and Hopkins 1927).

The distribution of malaria may affect human genetic factors in several ways. Haldane (cited in Livingstone 1971) suggested that the geographical distribution of thalassaemia was due to a selective effect by malaria. In a malarious area, the heterozygote—the carrier of the thalassaemia gene—enjoys some advantage which balances the disadvantage of the homozygotic state. Other red-cell polymorphisms for which there is now reasonable evidence to support a "malaria hypothesis" include sickle-cell anaemia, glucose-6-phosphate-dehydrogenase (G6PD) deficiency, Duffy-blood-group negativity, and ovalocytosis.

In Vanuatu malaria was mainly hypo-mesoendemic but with hyperendemic foci in certain years and on some islands. The transmission was generally more intense in the northern islands than in the south. The mean rate of G6PD deficiency among male subjects in Vanuatu was 7.4% (Ganczakowski et al. 1995, Kaneko et al. 1998), which was similar to those in other malarious areas in the southwestern Pacific—Papua New Guinea (6.7% [Yenchitsomanus et al. 1986]) and the Solomon Islands (10.4% [Ishii, Asahi, and Kawabata 1994])—as opposed to a very low rate in non-malarious Fiji (0.1% of indigenous Fijians of predominantly Melanesian origin and 1.2% of Indians [Buchanan, Wilson, and Nixon 1973]). In our study, a positive rank-order correlation was found between malaria incidence and G6PD deficiency rate on different islands in Vanuatu (Ganczakowski et al. 1995, Kaneko et al. 1998). The geographical correlation of G6PD-deficiency distribution with malaria endemicity confirms that this genetically determined disorder may confer relative protection against the human malaria parasite (Allison 1960, Motulsky 1960).

A reasonable hypothesis in Vanuatu is that malaria endemicity was introduced to the islands with the first human settlement from the northwest a long time ago. A geographical pattern of malaria endemicity similar to the present situation was then probably established, the north being more malarious than the south. Transmission of malaria then possibly selected for G6PD deficiency over many generations, and the G6PD rates may now be either at equilibrium or still increasing.

In the Pacific, malaria is mainly a disease of Austronesian-speaking Melanesians. Long ago Papuans may have protected themselves from malaria by settling in the highlands, where mosquitoes could not survive, although in recent years several malaria outbreaks have been reported there. In the Polynesian islands anophelism has for unknown reasons never become established.

How did malaria endemicity become established up to Vanuatu but not in Fiji, New Caledonia, and the Polynesian islands during the process of human settlement? Female anopheline mosquitoes have to have mammalian blood to survive. At present *Anopheles farauti*, the malaria vector in Vanuatu, gets blood only from humans and cattle. There are two possible scenarios for the establishment of anophelism, one before and one after human settlement. In the first, anopheline mosquitoes may have obtained blood meal from bats and seals before human settlement and adapted to human blood thereafter. Malaria parasites can remain in humans quite a long time—*Plasmodium falciparum* for several months and *P. vivax* for several years. Austronesian-speakers had to carry the parasites in their bodies during their expansion to the Pacific. In the second scenario, they may also have carried anopheline mosquitoes in their boats either as adults or in their aquatic stages (larvae or eggs). Then anophelines became established on Aneityum Island in Vanuatu but not in the Polynesian islands probably because of unknown environmental factors. The Polynesians' ancestors may also have protected themselves from malaria disease by moving farther to the east.

Our recent epidemiological and genetic studies in Vanuatu show that variations in humans and parasites at different frequencies in different geographical areas represent a major obstacle for the development of malaria control strategies, including malaria chemotherapy and vaccination (Kaneko 1999). Malaria in Island Southeast Asia and in New Guinea have several common features, and malaria in Vanuatu seems to be part of that in New Guinea with modifications by small-island effects. We malariologists can get ideas for improving control strategies in this region from Terrell et al.'s discussions (Kaneko et al. 2000) and also hope to contribute further to a better understanding of human origins in the Pacific from our point of view.

J. KOJI LUM
Department of International Affairs and Tropical Medicine, Tokyo Women's Medical University, Tokyo 162-8666, Japan (*jkl@research.twmu.ac.jp*). 12 IX 00

Terrell et al. present an interesting summary and critique of the conceptual frameworks used by many to examine patterns of Pacific human diversity. Their thesis is that dichotomies describing Pacific islanders are simplistic and not only of little utility but counterproductive for understanding the complexity of Pacific prehistory. They argue that rather than thinking of the Pacific in terms of simple dichotomies and isolated populations we should consider that they "in all likelihood, have remained more or less in touch with one another ever since the first arrival of people at least 45,000 years ago." It is well established that contact throughout the Pacific was technically possible. The real issues are how much contact was permitted once island populations became established and how this contact influenced the evolution of societies and languages (Lum 1998). The important questions are how much and with whom rather than whether contact occurred (Lum and Cann 2000). Unfortunately, the far-reaching criticisms of Terrell et al. are not balanced by an equally detailed alternative. The description of their preferred conceptual framework is brief and vague. By omitting specific statements of the intensity, duration, and geographic extent of contact among populations, they propose the only thing simpler than a dichotomy: a singularity of continuously interacting populations. Thus, their alternative is of no utility other than as a null hypothesis for differentiation among populations. In fact, dichotomies are observed when examining many aspects of Pacific island populations. Terrell et al. argue that these dichotomies are not useful because they do not coincide. Although I agree that focusing on any one dichotomy is simplistic, I would argue that a series of dichotomies results in meaningful complexity. Computer software exemplifies useful complexity resulting from binary contrasts.

Because of format constraints I am limiting my specific comments to the biological aspects of the article. Terrell et al. propose that the morphologically and genetically homogeneous populations of Polynesia and Micronesia are independently derived via founder events from a diverse Melanesian source. This view is improbable for a number of reasons. First, since there is no relationship between non-coding mtDNA and morphology, it is improbable that the stochastic loss of variation in both would result in Polynesian traits' resembling those of Island Southeast Asians. Second, the same improbable pattern of diversity loss is implied to have occurred in Micronesia. Third, the mtDNA nine-base-pair deletion clade diversity within Polynesia and Micronesia exceeds that found in Melanesia. Thus, the scenario proposed by Terrell et al. would require parallel, improbable losses of morphological and genetic diversity in Polynesia and Micronesia followed by a loss of diversity in the putative ancestral mtDNA lineages within Melanesia. The proposed founder events are also inconsistent with the framework of continuous contact vaguely proposed by Terrell et al. The mtDNA data are more consistent with an origin of Polynesians and Micronesians in Island Southeast Asia and a colonization route along the north coast of New Guinea (Lum et al. 1994; Redd et al. 1995; Sykes et al. 1995; Lum and Cann 1998; 2000; Lum et al. 1998; Richards, Oppenheimer, and Sykes 1998).

The claim by Terrell et al. that "Polynesians and Fijians with the deletion also always have the Oceanic motif" is incorrect. Redd et al. (1995) defined the "Polynesian motif" (Terrell et al.'s Oceanic motif) as having the nine-base-pair deletion and three single base substitutions at 16217, 16247, and 16261 (numbering from Anderson et al. 1981). The nine-base-pair deletion cluster in Polynesian populations includes sequences with the "Oceanic motif" and others (Lum et al. 1994, Redd et al. 1995, Sykes et al. 1995, Cann and Lum 1996, Richards, Oppenheimer, and Sykes 1998, Lum and Cann 2000), suggesting colonization by a diverse, maternally related population. Several nine-base-pair-deleted and unrelated sequences are found in Polynesia, throughout Asia, and in the Americas but not in the highlands of New Guinea (Lum et al. 1994, Sykes et al. 1995, Cann and Lum 1996). The distribution of these sequences suggests a proximate origin in Asia.

Terrell et al. assert that most geneticists have turned "a deaf ear to what linguists were saying," view island populations as static entities in space and time, and are interested in searching for "the *who* rather than the *what* of prehistory." Many of us not only explicitly test the correspondences between genetic, linguistic, and geographic patterns but also examine both neutral and functional genes within populations to evaluate differential disease susceptibility and treatment.

In summary, although Terrell et al. raise some interesting points, their reasoning is often inconsistent and their alternative is vague to the point of uselessness. I fear that articles that criticize without presenting specific alternatives do little more than polarize an already contentious field.

MICHAEL PIETRUSEWSKY
Department of Anthropology, University of Hawaii-Manoa, Honolulu, Hawaii 96822, U.S.A. 15 IX 00

The mantra that runs throughout this paper—that "two peoples, two migrations" is bad science and should be abandoned by researchers interested in Pacific prehistory and human biology—seems to me to have backfired. Terrell et al. provide no new (or other) empirical evidence to support a viable alternative explanation of the observed diversity and patterning evident in Pacific peoples and cultures. Beyond this, they seem to deny the conclusions of a plethora of studies in physical anthropology and genetics, not to mention the rest of anthropology, that point to a non-Melanesian source for the ancestors of Polynesians. Further, they are guilty of their own oversimplification of the issues and the selective use of re-

ferenced works. Given the limitations of space, the remaining comments will address some of the biological issues and data discussed (or not discussed) in this paper, issues that will reveal a very different view of Pacific human biology from that presented by Terrell et al.

The authors' explanation of what actually happened historically to the ancestors of Polynesians and the meaning of the genetic and biological similarities of Fijians and Polynesians will leave many readers puzzled and unsatisfied. In addition to miscommunicating the definition of "genetic bottleneck" (saying that selection has played a major role in determining who survives these drastic fluctuations in population size while referencing a paper on speciation!), Terrell et al. have no satisfactory explanation for the overwhelming biological similarities between Polynesians and Southeast/Eastern Asians and the overwhelming lack of such similarities between Polynesians and Melanesians. If the ancestors of Polynesians originated from people present in Melanesia, then why is there little if any evidence from biological anthropology to support this conclusion?

A few of the examples cited by Terrell et al. to corroborate their main claim that human diversity and prehistory in the Pacific are too complex and multifaceted to support a "two peoples, two migrations" scenario deserve comment. Their first example, that language and biology do not perfectly correlate with one another, should come as no surprise to anyone. Their second, that all that is known biologically about Polynesians suggests that their ancestors could not have come directly out of Asia but came from somewhere in Melanesia, will astound biological anthropologists who have worked or are now working in the Pacific. Numerous studies of skulls and teeth (e.g., Brace and Hunt 1990; Brace et al. 1990; Howells 1990; Pietrusewsky 1994, 1996, 1997; Turner 1990) have demonstrated relatively close biological similarities between Polynesians and Southeast Asians/East Asians. These same studies find no significant similarities between Polynesians and the people now living in Melanesia. Multivariate craniometric analyses (e.g., Pietrusewsky 1999, 2000) have consistently demonstrated that Melanesians group with Australians and Tasmanians, in dendrograms of distances and in canonical plots, while Polynesians occupy a separate branch which includes cranial series representing East Asia, North Asia, and Southeast Asia. While Terrell and his colleagues may regard studies of bones and teeth as nonmodern and therefore of little or no value, similar conclusions have been reached in analyses of mitochondrial DNA, HLA, and the human Y chromosome (e.g., Hagelberg 1998; Hagelberg et al. 1999; Lum et al. 1998; Melton et al. 1995; Redd et al. 1995; Richards, Oppenheimer, and Sykes 1998; Su et al. 2000). Some of the same works are referenced by Terrell et al. but with different conclusions. While pinpointing an ultimate homeland in mainland eastern Asia may not now or ever be possible, certainly there is overwhelming biological and genetic evidence suggesting a homeland somewhere at least in island Southeast Asia or possibly Taiwan but not in Melanesia.

The discussion of the so-called Lapita-associated skeletal record in this paper overlooks several important points. First, the dates for most if not all of these skeletons fall late in the Lapita temporal sequence and thus postdate the arrival of the first people in Polynesia. The first Polynesians were already living in Polynesia before the people buried at these sites had been born, and therefore these skeletons are questionable models of Polynesian ancestors. Furthermore, they are few (about a dozen), incomplete (e.g., none of the skulls are complete), and poorly preserved, which makes detailed comparisons difficult if not impossible. Whether they actually represent "Lapita people" is also arguable. Pietrusewsky, Galipaud, and Leach (1996) concluded that many of the Lapita-associated skeletons show resemblances to people now living in Melanesia, and most are in the right place (i.e., Melanesia) and have the appropriate antiquity.

Finally, Terrell et al.'s proposed paradigm shift that sees Pacific peoples as having remained more or less in contact since people first arrived there 45,000 years ago flies in the face of the vastness and isolation of the island world of remote Oceania and the human differentiation of near Oceania at the time of European contact.

In summary, the issue is not whether the paradigm we use is simple or complex but that independent lines of evidence, including skulls, teeth, and molecular data, are unanimous in their support of a relatively rapid expansion of the ancestors of Polynesians from a source in island Southeast Asia or eastern Asia, an expansion which ultimately resulted in the colonization of remote Oceania and Polynesia.

MARTIN RICHARDS
Department of Chemical and Biological Sciences, University of Huddersfield, Queensgate, Huddersfield, West Yorkshire HD1 3DH, U.K. (*martin.richards@hud.ac.uk*). 3 IX 00

As Terrell and his colleagues say, genes do have stories to tell us about the past, but we must be careful how we read those stories. It has been usual in recent years to regard the genetic evidence, and especially that of the mitochondrial DNA (mtDNA), as supporting the idea of an Austronesian/Melanesian dichotomy, but it does indeed look as if things are rather more complicated (Oppenheimer 1998).

From the point of view of mtDNA variation, Polynesians are very different from the people of the Indo-Malaysian archipelago (e.g., Redd et al. 1995, Sykes et al. 1995). Indonesian mtDNA is highly diverse, with a time depth of tens of thousands of years. Of course, this diversity could have been brought to the archipelago much more recently, since colonizing people may well have carried considerable diversity along with them from their ancestral source. However, there are good reasons for thinking that at least some of this ancient diversity was generated within the archipelago.

By contrast, remote Oceania is astonishingly lacking in diversity. Although there are a few widespread

mtDNAs that most likely originated in New Guinea, the vast majority belong to a single clade, haplogroup B (defined by the nine-base-pair deletion that Terrell et al. refer to). In fact, it is not quite true, as Terrell et al. suggest, that all of the haplogroup B lineages in Polynesians have the "Polynesian" (or "Oceanic") motif; its immediate ancestral type, with three out of the four signature mutations, is widespread as well. But the motif type is indeed the predominant type in the Pacific.

The immediate ancestor of the motif type, which occurs alongside the motif in the Pacific, is tens of thousands of years old and occurs throughout East Asia. It is especially common and diverse in Taiwan, which has been an important factor in the argument that mtDNA supports a model of recent expansion from Taiwan through Southeast Asia and into the Pacific (e.g., Melton et al. 1998). A west-east expansion is indeed suggested by this evidence, but in fact it could have begun at any time in the past 30,000 years or so. The age estimate for the motif itself in its most westerly location—eastern Indonesia—is of the order of 17,000 years (Richards, Oppenheimer, and Sykes 1998). In fact, this estimate is subject to enormous uncertainty (due to lack of data), but it does seem unlikely that it arose less than 5,000 years ago. Therefore, it looks very much as if the most important component of Polynesian mtDNA has its roots in pre-Neolithic Wallacea. Subsequently, there were founder events (incorporating some mtDNAs originating in New Guinea along the way) as the motif and its ancestor type were both carried east. Terrell et al. are right to point out that this is a better description of the process than our phrase "successive bottlenecks," although our intention was the same (although bottlenecks do not imply natural selection but are, like founder events, instances of random genetic drift).

Where does this leave the Austronesian/Melanesian dichotomy and the idea of an agriculture-fueled Austronesian dispersal? It suggests that there have been people in eastern Indonesia since long before the introduction of rice agriculture and that it was these people—perhaps in the famous "voyaging corridor" (Irwin 1992)—whose maternal lineages were carried into the Pacific 3,000 years ago. This is an intriguing outcome, since whilst the distant ancestry of haplogroup B lies in mainland Asia, the motif itself appears to have evolved close to the "Melanesian" orbit of New Guinea. Thus Polynesians would appear to be the result of a minor offshoot of an ancient Wallacean population in the same way that modern Eurasians are a minor offshoot of East Africans (Tishkoff et al. 1997, Watson et al. 1997, Quintana-Murci et al. 2000).

It also seems to imply that the Austronesian languages may have been indigenous to the archipelago. If Austronesian had been brought into the region by northwestern agriculturalists, as many believe (Bellwood 1997), this would mean that a widespread and diverse native population would have to have been converted wholesale to the new language as the agriculturalists spread. It seems very unlikely.

Terrell et al. also comment on the effects of genetic drift, and it may be worth adding to that. Wherever the ancestral groups were based, most of the mtDNA lineages that they were carrying must have been lost as they spread east. So whilst the maternal lineages of the Austronesians of the Pacific may be of eastern Indonesian ancestry, there may nevertheless have been an agriculturalist input into island Southeast Asia which has been lost in Oceania because of founder effects on the female side. To explore this possibility, we need much more detailed analyses of Southeast Asian variation, both on the mtDNA and on other genetic markers (e.g., Su et al. 2000).[1]

Reply

JOHN EDWARD TERRELL, KEVIN M. KELLY, AND PAUL RAINBIRD
Chicago, Ill., U.S.A. 5 X 00

We thank our respondents for their often frank (and for some, perhaps restrained) remarks. Since readers will find acknowledgment of our concerns as well as further examples illustrating them in these commentaries, we would like to focus on perhaps the most important question that anyone can ask: So what? Does it make any difference when scholars and others use finer-grained ways of studying human diversity and prehistory in the Pacific than what we have summarized in our paper as "two peoples and two periods"? Perhaps appropriately, we limit ourselves to two general observations and then offer a few remarks about some of the specific comments that have been made.

First, if we had to boil down our thesis to a catchphrase, we would probably opt for this one: Try to know *whose* history you are writing.

While nowadays some may do so, we know nobody who self-identifies as an "Austronesian" or a "Papuan (i.e., Non-Austronesian)." Even if some now do, our concern is not that people might elect to do so. Rather, we think that the issue at the heart of our paper can be looked at from two directions. Have some people in the Pacific done so *consistently enough* down through history that it makes sense to try to capture (or reconstruct) their collective past and write about it? Alternatively, have some people in the Pacific *acted together* consistently enough down through history that we can write about their history even though they did not realize that they were behaving as "Papuans" or "Austronesians"?

We accept that if you parse the world carefully, it is possible to find—or at any rate, define—instances in which certain traits of genetics, language, and culture all appear to be, metaphorically speaking, packaged together, that is, in which particular people seem to live collectively together in their own world, speak in their

1. I thank Vincent Macaulay and Stephen Oppenheimer for reading the manuscript.

own distinctive ways, and have their own exemplary traits and practices. In other words, we would agree with anyone who said that you can always find places where race, language, and culture seem to go together as a package at any given moment in time. But how easy is it to find places where particular traits of biology, language, and culture *stay together* over time?

This is not a new question, and it is not an easy one to answer. It may sometimes be useful to assume that race, language, and culture have marched together for thousands of years in the Pacific in two parallel columns that can be labeled "Papuan" and "Austronesian." But when is this assumption useful? When is it wise? When is it foolish?

We think that deciding whether labels such as these stand for parallel and enduring human phenomena and historical lineages needs to be a research objective, not a research assumption. If people characteristically do *not* come into this world prepackaged as part of a historically enduring corporate phenomenon marked by diagnostic traits of biology, language, and culture that are stable over time, then *whom* scholars are writing about when they claim to be writing about "Papuans" and "Austronesians" is a question that should not be dismissed lightly.

This does not, of course, mean that scholars and others are forbidden to talk about Papuans and Austronesians *as if* these ostensibly corporate phenomena (and supposedly historical lineages) were genuine actors on the stage of world history. In some contexts, it may make perfect sense to talk about Papuans and Austronesians. What are these contexts? When does talking this way make sense? What kinds of sense does this reductionism make?

These several questions take us to our second general remark. We think that Milner was right to caution scholars about the dangers of turning adjectives (such as "Malayo-Polynesian") into nouns ("Malayo-Polynesians"). One menace is the one that Alfred North Whitehead (1967[1925]:51) called "the fallacy of misplaced concreteness"—the intellectual trap of seeing abstract notions as concrete things. Another hazard might be called "Ptolemy's fallacy" (Terrell 2000*b*:28)—assuming that abstractions such as "Papuans" and "Austronesians" refer to concrete phenomena (in this instance, assuming that they point to historically enduring social aggregates) and then getting results from analyses based on this assumption that appear to make good sense but unfortunately do so for the wrong reasons. A classic example would be seeing the earth as the center of the universe, navigating successfully from Athens to Cairo on the basis of this commonsense assumption, and then thinking that your successful voyage proves that the sun moves round the earth. Another example, we think, would be the conclusions that Gray and Jordan (2000) have drawn from their success in using a computer to model the Austronesian language tree.

This last example takes us to a few specific remarks about some of the comments that have been offered about our paper. We are in full agreement with the many respondents who variously note that the scholarly history of Oceanic prehistory is more complex than presented in our austere text. We are not suggesting—as intimated by Burley—that scholars should abandon the merger of linguistic, biological, cultural, and archaeological data. We are as committed to integrative and interdisciplinary research in Oceanic prehistory as anyone else. Burley has evidently mistaken our questioning of language dichotomies for criticism of the collaborative research process.

We caution against Lum's assertion that "a series of dichotomies results in meaningful complexity." We are concerned that some will conclude from Lum's comments (and to a lesser extent from those by Jordan and Gray) that problems with overly simply dichotomies can somehow be overcome by turning to sophisticated statistical procedures. Invalid assumptions lead to invalid conclusions. As the saying goes, "garbage in, garbage out."

On a more technical point, we note the criticisms of our discussion of population bottlenecks and natural selection (Lum, Pietrusewsky, Richards). We recognize that saying that individuals survive bottlenecks because they possess advantageous genetic traits is putting a complex argument much too simply. However, the proper labeling of the genetic consequences of severe population reductions was not our core concern in this paper. What we were trying to emphasize was that the genetic variability lost during such events, however classified, is unknowable and therefore can only be the subject of speculation and not analysis. Those who would cite missing genetic data as evidence of a population's affinities (or lack of affinities) do so at their own risk.

In closing, we again acknowledge that there are alternative interpretations to consider. We are therefore content to have readers review our paper, read these comments, and then decide for themselves where they want to stand.

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