COLONIZATION OF THE PACIFIC ISLANDS

John Edward Terrell, The Field Museum, Chicago

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One of the oldest debates in science is about whether events and processes that we can see going on around us are sufficient to explain the character of large-scale phenomena, such as the extinction of the dinosaurs, the evolution of new species and higher taxa, the geographical patterning of the world's flora and fauna, and the modern global distribution of *Homo sapiens*. In evolutionary biology, this debate is often glossed as one about *microevolution* vs. *macroevolution* (Eldredge 1985; Eldredge & Cracraft 1980; Mayr 1970:351). Most of the zoologists, paleontologists, and botanists I know do not think that macroevolution differs qualitatively from microevolution. As George Gaylord Simpson (1944, 1953) wrote years ago about tempo and mode in evolution, the assumption that things differing in scope need different causal explanations is rarely, if ever, needed. The mechanisms that produce or permit discontinuities to develop among individuals within a population are sufficient to explain the evolution of higher taxa.

Comparable debates in anthropology and archaeology about whether unusual explanations are needed to explain large-scale patterns of human diversity are less one-sided. In the Pacific, for example, there have always been-and still are-those who insist that the human colonization of a region as large as Oceania definitely requires abnormal explanations, i.e., events or circumstances that are qualitatively different from those used by historians and other social scientists to account for patterns of diversity among contemporary or historic people. Perhaps the most visible debate of this kind in the Pacific is about the character and role of migrations in prehistory.

If we were to use the word *migrationism* to replace "macroevolution" as the label for one side of this debate, it would be difficult to avoid the standard anthropological counterpoint *diffusionism* as the appropriate match for "microevolution." This would be misleading. The debate is not just about migrationism vs. diffusionism, although some have said it is. Diffusion is only one of the processes of interest to scholars working in the Pacific who favor ordinary over unusual ways of explaining cultural, linguistic, and biological diversity. We need other labels for this debate. Therefore, let us stick to the labels microevolution and macroevolution. Please give me the benefit of the doubt. Using these labels taken from evolutionary biology does not mean I want to convert you to social Darwinism or sociobiology.

Human Microevolution vs. Macroevolution

In a recent issue of *ANTIQUITY*, my Pacific colleague Peter Bellwood (1996b:882-83) tells us that the patterning of languages, cultures, and human gene pools observable on continental and millennial scales in the linguistic and archaeological records is so large-scale that it cannot be explained-and may even be overlooked-by those who like to build their explanations step-by-step out of ordinary, everyday processes such as borrowing, trade, competition, recruitment, adoption, marriage, moving around, and inventing new ways of meeting life's challenges. Nobody I know would strongly object when Bellwood writes that "the large-scale patterns in the records of archaeology and comparative linguistics give perspectives, completely independent of those derived from ethnography, on the changing patterns of human affairs through many millennia across whole continents and archipelagoes" (Bellwood 1996b:883)-although the words "completely independent" might give some trouble. But I doubt that it would be easy to draw up a short-list of fascinating large-scale patterns, and I bet there would be heated words if we had to agree on why big patterns exist (granting that they do).

Most of the examples of large-scale patterns that Bellwood offers are taken from linguistics, e.g., Indo-European, Austronesian, Sino-Tibetan, and Uto-Aztecan. Recognition of such higher-level language categories, however, is *ex post facto*, as is the ordering of the subfamilies, languages, dialects, etc. seen as their components (see Simpson 1953:324, 376). The observation that some language families are geographically widespread and are deeply subdivided, or differentiated, is not evidence that the evolution of such big patterns of linguistic diversity needs uncommon explanations.

Perhaps you agree with me that arguing whether colonization is a special type of historical *phenomenon* different from most things that have happened to people down through history normally generates more heat than light (although it is fun to argue about who got where when). It is probably more appropriate to use the word *colonization* as a category, not as a phenomenon, i.e., as the label for a number of *historical processes* that move people around from place to place more or less permanently (see Anthony 1990 for similar remarks).

Let us also say that historical processes, unlike equilibrium processes, are kinds of circumstances and events leading to change over time. Not all historical processes are equally informative. Specifically, if we want to study what happened in the past over the time span **T** and our observational techniques do not allow us to detect changes during intervals of time shorter than **t**, we can subdivide prehistoric change into three characteristic frequencies: (1) low frequency changes, much less than 1/ **T**; (2) middle-range changes; and (3) high frequency changes, greater than 1/**t** (Simon 1973:10). Changes at low frequencies will be so slow that even archaeologists may not be able to observe their effects. They can be treated as constants. Changes occurring at high frequencies will be so rapid that we cannot see what is happening. The world at that level will also look unchanging. But as Herbert Simon notes: "The middle band of frequencies, which remains after we have eliminated the very high and very low frequencies, will determine the observable dynamics of the system under study . . . . Hence, we can build a theory of the system at the level of dynamics that is observable, in ignorance of the detailed structure or dynamics at the next level down, and ignore the very slow interactions at the next level up" (1973:10-11).

Structuring how we think about change over time in this way helps, I think, to see why there should be no *a priori* reason to say that human colonization-however large-scale-has been determined either by very high or very low frequency changes, developments, or events, i.e., by changes we could not have noted had we been there to observe them. Hence, there is no particular reason to think we need to build unusual theories to explain large-scale patterns of human diversity.

It is also wise to keep in mind that the mid-range changes leading to prehistoric human colonization were unlikely to have been only ones altering people's surroundings. It is sometimes convenient to reduce the dynamics of change to summary notions such as "population pressure" or "carrying capacity," but these are shorthand explanations, not sufficient ones. When such explanations are unpacked and looked at closely, we should expect to find, among other things, that human cognitive processes of planning, decision-making, collective action, and the like must have been part of what happened, e.g., when people were "responding to population pressure." Put simply, prehistoric human colonization was social as well as biological, active as well as passive.

RIMs and TRIMs

Bellwood (1996b:887) and others say that language families such as Austronesian, Indo-European, and Sino-Tibetan have achieved their large-scale distributions mostly through population expansion and the inheritance of ancestral characteristics by the direct biological, cultural, and linguistic descendants of the people who first started speaking in these ways (but see Bellwood 1996a:293-94). If the exclusiveness of this pattern of inheritance seems doubtful to you, William H. Durham explains that "genuine cultural hybrids" among human populations are rare. It is normal, he says, for new cultures to evolve in isolation because "there exist a number of effective barriers to hybridization-ecological, psychological, linguistic, and cultural-that act as transmission isolating mechanisms (TRIMs), by analogy to the reproductive isolating mechanisms (RIMs) of speciation theory in biology" (1992:333).

According to Bellwood and others, the development of early agriculture was the "engine of change" driving the world's major linguistic colonizations. "Over time, populations have tended consistently to move out from . . . zones of primary agriculture through demographic growth, rather than in; their languages have moved outwards with them" (1996b:887). It is possible to trace these expansions after thousands of years not only because ethnic populations are normally kept by TRIMs from interacting closely with one another. Bellwood adds that the adoption of agriculture by prehistoric hunter-gatherers was rare except in agriculturally marginal zones where foraging aborigines were able to "maintain sufficient demographic balance against incoming agriculturalists to allow for successful interaction and diffusion of ideas and techniques" (1996b:886). Thus the prehistory of human colonization for Bellwood and others is more about who "came out on top"-linguistically, culturally, and biologically-than about "who got where first and when" in this world.

I am not sure how obvious the parallels may be, but this understanding of the world's expanding ethnolinguistic colonizations is basically the "Eve Out of Africa" model of human origins favored by some molecular biologists-but without *Homo erectus*. The Eve model, of course, is chiefly Mayr's model of geographic speciation (Mayr 1970:278-95) applied to *Homo sapiens*. As applied to linguistic colonization, Bellwood and others are saying, in effect, that (a) a major new adaptation (the invention of agriculture) led to human population growth in certain restricted parts of the world; (b) this demographic success tipped the competitive balance in favor of the subspecific populations possessing this new adaptation (e.g., early "Austronesian-speakers," "Indo-European-speakers," etc.) who were able to hand the adaptation down to their offspring; (c) except under abnormal circumstances, these expanding ethnolinguistic populations replaced rival populations of hunter-gatherers who could neither beat nor join them. That this view of linguistic colonization is essentially a biological model may explain why this understanding of human colonization is favored by some biologists and geneticists (e.g., Cavalli-Sforza et al. 1994; Diamond 1996).

I am not opposed to biological models in the social sciences. Yet drawing analogies between speciation and colonial linguistic success is questionable. Was early agriculture so dramatically adaptive? Did agriculturalists normally replace hunter-gatherers in a way comparable to the replacement of *Homo erectus* by *Homo sapiens* in the Eve model? Were TRIMs effective at keeping agriculturalists and hunter-gatherers apart?

What does the human colonization of the Pacific suggest answers to these questions should be?

Colonization of the Pacific Islands

We know people first got to Australia, New Guinea, and neighboring islands at least 50-60,000 years ago. It is anyone's guess what subsistence practices were like back then, but that was long enough ago most would say we can call the first Pacific Islanders foragers by default. While there is some archaeological evidence of gardening in the highlands of New Guinea as early as 9,000 years ago (Bayliss-Smith and Golson 1992), the general opinion today is that managed plants and animals (notably pigs, dogs, and chickens, which are thought to be of Asian origin) did not become important until the 2nd millennium B.C. (Spriggs 1996). Thus, the first observation we can make is that Bellwood's phylogenetic model of colonization in the Pacific has nothing instrumental to say about 90-95% of Pacific prehistory.

Bellwood (1996a, 1996b), Matthew Spriggs (1996), and others correlate the introduction of managed species of plants and animals into the Pacific with the first appearance-around 3,200-3,500 years ago-of a kind of early pottery called Lapita. This pottery, they say, marks the arrival of Austronesian-speaking colonists from island southeast Asia. They acknowledge, however, that by then New Guinea had developed "its own independent Neolithic trajectory" (Spriggs 1989:608), so much so, that the migrating Austronesians "initially avoided a perhaps heavily-populated and already-Neolithic New Guinea mainland" (Spriggs 1989:609; see also Pawley and Ross 1993: 449).

The archaeological record for the Pacific between the end of the Pleistocene and the first appearance of Lapita pottery is far from what we need to have to be able to say much with confidence about almost anything you care to suggest (Gosden 1992). Nobody currently doubts, however, that managed plants and animals became important to people in the Pacific after ca. 6,000 years ago. This consensus, however, is a far cry from saying that these newly managed resources fueled population growth that was unprecedented enough to provoke the extensive colonization of new islands by anyone, Austronesian-speaking or otherwise. To confuse matters more, the great diversity of subsistence practices seen in the Pacific today also makes the characterization of what is, or is not, a managed subsistence economy in Oceania problematic (Bourke 1990). In short, it is anyone's guess how much of a role managed plants and animals played in the lives of Pacific Islanders when Lapita pottery became so fashionable.

Bellwood and others agree that the evident rapidity with which the art of making Lapita pottery traveled around the Pacific-from the Bismarck Archipelago in western Melanesia to Tonga and Samoa in western Polynesia-would be hard to explain as just a response to population pressure based on domestic arts of plant cultivation and animal husbandry. There seems to be growing consensus in Pacific archaeology that the scattered islands east of the Solomons had not been colonized by anyone-Neolithic, Mesolithic, or Paleolithic-until their colonization around 3,000 years ago by people who made Lapita pottery. But people had been sailing around the Solomons and the islands of the Pacific to the west of that archipelago for a very long time before then. Lapita pottery evidently appears in the archaeological record of New Caledonia, Vanuatu, Fiji, Tonga, and Samoa quickly after its first appearance in western Melanesia; some say it took less than 100-200 years for Lapita to spread from the Bismarcks to Fiji, Tonga, and Samoa. It is anyone's guess what finally got people with Lapita pottery to risk the open seas east of the Solomons. Dire need or hunger hardly seem likely causes. Wanderlust, a sense of adventure, a pioneering spirit, and the like instead have all been suggested.

One of the expectations raised by speciation models in biology is that once a species with a favorable new adaptation (or newly valuable preadaptation) has expanded its range, it may split up into subpopulations that eventually either will go extinct in their new territories or will evolve into new subspecies or reproductively isolated sister species. Bellwood implies that language populations also "speciate" in this general way-and hence, his model of language expansion *via* population dispersal may be labeled a phylogenetic model. However, the claim that languages speciate cladistically has been strongly contested in the field of Indo-European studies since the middle of the 19th century. Linguists I know would say this assertion is a gross simplification. Many Pacific linguists, however, seem to concur that the colonization history and later diversification of the Austronesian family of languages hold to the expectations of biological models of speciation fairly well (Pawley and Ross 1993).

I am not so sure. Granting, however, that this may be true, what seems striking about human diversity in the Pacific is that so little that we know about culture and human genetics in southeast Asia and the Pacific correlates with large-scale linguistic patterns of relationship, as a recent volume about the so-called "Austronesians" inadvertently shows (Bellwood et al. 1995). Austronesian languages are spoken today by people as different as Polynesians, southeast Asians, Melanesians, and the people of the Malagasy Republic. Nearly half-maybe 400-of the total number of known Austronesian languages are spoken by Melanesians. Although light-skinned Polynesians are considered by many experts to be biologically closer to southeast Asians than dark-skinned Melanesians, the irony is that their linguistic relationships show them to be close to Melanesian Austronesian-speakers. In a word, nothing is simple, and clearly Austronesian linguistic relationships tell us a lot about the Austronesian languages and maybe little about anything else, although taking a comparative words & things approach to the Austronesian languages does raise interesting hypotheses about ancient Oceanic material culture and social life (Pawley & Ross 1993).

Therefore, I would say that the model of "ancient phylogenetic dispersals" in the Pacific favored by Bellwood and others gives an artificial as well as a narrow picture of human colonization in the Pacific. As an explanation for human diversity, it does not look robust. But what can be said more positively about the colonization of Oceania?

The Settlement of Oceania

Discovery of the great antiquity of human settlement in the southwest Pacific came as a surprise to Pacific experts. Scarcely a generation ago, nearly everyone thought the Pacific was our last major frontier before Russia and the United States started to explore outer space. Now it looks like the Pacific was one of *Homo sapiens* earliest frontiers. Moreover, people not only got to the Pacific much sooner than we had anticipated but, to exaggerate slightly, people got everywhere-at least as far as the northern Solomons-nearly all at once. Nobody knows why. It is hard to believe foraging pressures or population growth forced *Homo sapiens* so far from Africa so early. While some have used Pleistocene sea level changes as the driving force behind Pacific colonization, this argument is difficult to sustain (Clark 1991).

The speed with which people colonized Oceania as far as the Solomons is one reason my colleague Geoff Irwin (1992:36-37) at the University of Auckland says that anyone with a canoe in prehistoric times would have found no insurmountable barriers to travel back and forth among the great chain of islands and archipelagoes stretching between southeast Asia and the Solomon Islands. He argues that after people reached them, southeast Asia, New Guinea, and much of island Melanesia would have all been integral components of an ancient "voyaging corridor" in the Pacific, a canoe seaway running from Melanesia back to mainland Asia (Irwin 1992:5-6, 19).

It is conventional to say, as I have noted, that the management of certain plants and animals (chiefly ones thought to be of Asian origin) fueled the Lapita expansion beyond the Solomons after 3,500 years ago (Gosden 1992). By 6,000 years ago, however, Holocene sea levels at last had risen to within a meter or two of their current position. We are only beginning to understand the magnitude of the impact this stabilization evidently had on natural resources and patterns of human subsistence in Irwin's voyaging corridor.

In my own research area, the Sepik coast of New Guinea, we now think that by the 2nd millennium B.C., newly formed lagoons along New Guinea's northern coastline (and elsewhere in Irwin's voyaging corridor?) may have become naturally productive enough to support major human population growth based on wild foods (e.g., fish, shell fish, and sago). To draw an American analogy, perhaps it was not so much domestication (e.g., corn in central America) that fueled prehistoric culture change in the southwestern Pacific during the 2nd millennium B.C. as the naturally increasing abundance of certain wild resources (somewhat like salmon runs in the rivers of western Canada & the United States). It seems likely that the growth of human populations along the Sepik coast following the expansion of these lagoonal systems would have extended human social and economic horizons in all directions-including eastward toward the Solomons and westward toward island southeast Asia.

A recent survey of the archaeological evidence for managed food resources from pre-Lapita, Lapita, West Polynesian, and early East Polynesian sites shows that there is actually little direct evidence of domestication anywhere in Oceania until about 1,000 years ago. Chris Gosden has written that the evidence is somewhat better than this assessment but notes, in any case, that the colonization of the Pacific east of the Solomons may not have been all of a piece. People probably shaped their subsistence mix in differing ways in different newly colonized places (1992:61-63). I suspect the successes of early colonists was based far more often on a wide spectrum of food resources, both wild and carefully managed, than on a handful of domesticated species (Yen 1995).

My final point about prehistoric human colonization in the Pacific is by no means the least important. Irwin (1992), Gosden, and others have pointed out that there may have been exploratory moves out to the more distant parts of Oceania in advance of first permanent colonization. Settlement beyond the Solomons evidently did not begin, as I have noted, until sometime before 3,000 years ago. "We must recognize," Gosden says, "that colonization is a process of exploration and experiment that may have had a number of phases before full-scale settlement was established" (Gosden 1992:62). But when people first started sailing with settlement in mind, they evidently moved with remarkable speed as far as Fiji, Tonga, and Samoa. It is uncertain how soon afterwards they left Tonga and Samoa to explore the rest of what is now central and eastern Polynesia. Some say there was a long pause in western Polynesia; others say that was not the case. Whatever the truth, it seems certain that people needed to be well-motivated, trained, and equipped to sail beyond the Solomons and perhaps even more so to sail east from Tonga and Samoa. And Gosden might add, "it may be misguided to think that the earliest settlers of an island always arrived with a full package of agricultural resources" (1992:62).

It can be argued for several reasons (Hunt and Graves 1990:110-13; Terrell 1997; Terrell et al. 1997) that people as a rule like to minimize risk and like to keep in touch with one another. We often think of islanders as isolated folk. However, I think we must assume that prehistoric colonists, like people today, tried to keep their ties alive with other people near and far (through marriage, adoption, feasting, exchange, friendship, etc.) for social, psychological, and survival reasons. Ancient island colonists must have worked to avoid situations that might lead to their isolation.

In short, there were multiple human strategies behind the successful colonization of Oceania. I doubt the strategies involved were unusual or unique to the people who made Lapita pottery. Certainly nobody has yet argued that the so-called Lapita people or peoples comprised an integrated society, kingdom, confederation, colonial empire, or plantation system. I wager they were not all that different from other people in the voyaging corridor between Asia and the Pacific after 6,000 years ago.

Conclusion

Nobody needs to be against using biological models in the social sciences to wonder if seeing an analogy between speciation and human colonization is as useful as some say. Questioning the analogy, at least in the Pacific, highlights several basic issues about prehistory that are hard to answer. Was the early management of certain food species dramatically adaptive? Did "early agriculturalists" normally replace "hunter-gatherers" in some way comparable to the replacement of *Homo erectus* by *Homo sapiens* in the Eve model? Were TRIMs effective at keeping agriculturalists and hunter-gatherers apart? I suspect the answer in each case is that we need a lot more evidence and greater willingness to keep an open mind to alternative models.

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