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Archaeological Inference and Ethnographic Analogies: Rethinking the "Lapita Cultural Complex"

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When I was an undergraduate at Harvard in the early 1960s, Phil Phillips's famous phrase "New World archaeology is anthropology or it is nothing" (Phillips 1955:246–247) was new. Phillips and Gordon Willey, whose name is similarly tied to this famous maxim, were both familiar figures around the Peabody Museum where I spent much of my time. I thought I knew what Willey and Phillips meant by these words. They wanted archaeology to be about more than digging, typology, seriation, and radiocarbon dating with a smidgen of imaginative flesh-and-blood "reconstruction" added at the end of every monograph or journal article to make it look like we were real anthropologists, not just ruggedly handsome movers of dirt. But was I correct? What did it actually mean 40 years ago to yearn for an archaeology that was anthropology or nothing?

Until Death Do Us Part?

I think that there were several earnest intentions behind Phillips's maxim. It was advanced in the heyday of post-war America's love of science, at the highwater mark of the so-called "quantitative revolution" in the social sciences. That was the time of "compare and contrast" cross-cultural studies in anthropology, an era when it was routinely said that the legitimacy of any given statement about human beings made under the banner of the "Science of Mankind" depended on how many societies you had in your comparative sample. Needless to say, we wanted to join the quantitative revolution, and just think how many *more* human societies you could master scientifically with an archaeologist or two on your team!

Back then, too, it was widely acknowledged that something elusively called "theory" was terribly important if you were going to be a scientist, although we recognized that you had to be awfully careful when and how you used this word. There were mutterings at Harvard, for instance, about "what was happening to anthropology at Michigan." Too much theory, we sensed, was more deadly than too little. Nevertheless, many of us agreed that theory was nifty. As Phillips (1955:246) had explained in 1955, "acceptable fieldwork can perhaps be done in a theoretical vacuum, but integration and interpretation of archaeological data without theory is [*sic*] inconceivable."

So with the hindsight of 40 years and more, I think we felt back then that archaeology and anthropology needed one another for two chief reasons. There was the hope and promise that archaeology could extend anthropology's scientific grasp beyond the here and now—or the there and then—of the "ethnographic present." On the other side of the ledger, it seemed then that anthropology could return the favor by pumping invigorating injections of a secret ingredient called theory into archaeology's otherwise flaccid findings. In short, it seemed self-evident in those ancient days that archaeology and anthropology were made for each other.

Subservient, or Separate but Equal?

Reading again, however, Phillips's 1955 paper in *Southwestern Journal of Anthropology* on American archaeology and anthropological theory, I recently detected a degree of ambiguity in his message about the relationships between archaeology and anthropology that I do not recall seeing in the 1960s. On one hand, Phillips states that archaeology "lacks a systematic body of concepts and premises constituting *archaeological* theory," and therefore, since archaeologists must borrow their theoretical underpinnings from the field of study their work happens to serve, "American archaeology stands in a

particularly close, and, so far as theory is concerned, dependent relationship to general anthropology" (Phillips 1955:246). This is why he says New World archaeology is anthropology or it is nothing. Yet Phillips also says that "anthropology is more science than history," that the world of anthropology is a mixture of both recurrent and historically unique events, and that "paradoxically, archaeology in the service of anthropology, concerning itself necessarily with the position of unique events in space and time, has for its ultimate purpose the discovery of regularities that are in a sense spaceless and timeless" (Phillips 1955:247).

The paradox here is clear. Whereas archaeology is about what happened in the past, archaeology in the service of anthropology must grab for truths that rise above the peculiarities of any one place and time. Therefore, "since it appears that a comparative method alone will disclose such regularities, it follows that archaeology is faced with the problem of finding, in the seemingly endless flow of cultural and social events, forms and systems of forms that are not only comparable to each other, but also comparable to, or at least compatible with, the forms and systems of forms of cultural and social anthropology" (Phillips 1955:247).

As I recall, during the post-war quantitative revolution debating "what is history?" and whether history could be science—that is, a generalizing discipline—was much in vogue. So in this intellectual context, Phillips's concern about how archaeology (history) could contribute to anthropology (science) is perfectly understandable. What I now find surprising is that his stated resolution of this dilemma seems to undercut the premise that in this respect, archaeology is subordinate to anthropology. Specifically, Phillips says that to accomplish the job of doing science as well as writing history, "the operations of archaeology and cultural anthropology can be conceived as converging toward a [common or shared] synthesis" as archaeologists and anthropology "ascend" from *observation* (fieldwork) to *description* (what he called "integration") and finally to *explanation* (Phillips 1955:248).

What I find ambiguous about this message is that if, as Phillips initially says, New World archaeology is intellectually subservient to anthropology (sadly, New World archaeologists have no theory to call their own), then why does he later explain that *both* disciplines converge toward one another when they pursue their common goal of discovering "regularities in the relationships given by the methods of descriptive integration" (Phillips 1955:249)? Talking about archaeology and anthropology as *convergent* rather than *complementary* disciplines makes it sound more as if they are "separate but equal" sciences than that they are "servant and master." Knowing what Phillips meant by the expression "forms and systems of forms" must be key to resolving this seeming contradiction.

Forms and Systems of Forms

The 1960s seem long ago. Archaeology and anthropology have been through a lot since then, separately and collectively. Asserting nowadays that archaeology is anthropology or it is nothing rings rather hollow. It is no longer as obvious as it (perhaps) was back when Alfred Kroeber's *Anthropology* (1948) still defined the field what anthropology is all about as an intellectual pursuit, academic discipline, or science. Today, too, many archaeologists show clear signs of great discomfort when anyone insists that archaeology—New World, Old World, or Third World—has no theory of its own.

I should confess at this point that I never actually thought in the 1960s that what was viewed as anthropological theory—for instance, what George Peter Murdock, A. R. Radcliffe-Brown, E. E. Evans-Pritchard, or Claude Lévi-Strauss had to say about human beings—was so heady and exciting that archaeology was doomed to being only anthropology's gofer, minion, or subordinate. I thought then, as I do now, that archaeology and anthropology are convergent, not complementary, disciplines; they are two sides of the same coin, not two separate disciplines with divergent mission statements. Neither side is "heads," neither "tails." Or, to switch metaphors, it may be true that Anthropology when spelled with a capital *A* has two heads (and maybe more, such as linguistics and bioanthropology), but they both share the same lungs, bowels, and heart. And I thought then, and still do, that what is needed to fill the innards of this hydra-headed branch of learning is not an elixir of highfalutin theory imported from exotic foreign places. No, I was convinced then and remain convinced that what is required to warm the belly is not something full of empty calories called theory but instead solid home-cooked meals in the form of reasonable, clearly defined, and properly balanced scientific models (Winterhalder 2002). In other words, I believed then and still do that archaeology and anthropology need one another not because archaeology, American or otherwise, is theory-poor while anthropology is theory-rich, but instead because both desperately need something perhaps akin to what Phillips called "forms and systems of forms."

Anyone who has read Willey and Phillips's 1958 classic text *Method and Theory in American Archaeol-*

ogy knows what Phillips probably meant by these words. He was suggesting that archaeologists need ways of organizing what they recover from the ground—"intelligible units of comparative study"—so that they can map their findings onto, or in terms of, the kinds of "social equivalents" or "social units" that anthropologists after the war were taking about: *communities, societies, peoples,* and the like (Willey and Phillips 1958:48–56). With this aim in mind, Willey and Phillips (1958:21–24, 51) suggested that what they called an *archaeological phase* "offers the best hope of incorporating archaeology into general anthropological science."

These days most archaeologists recognize that anthropologists have been busy lately "deconstructing" once revered sociocultural units such as "societies" and "cultures" in favor of notions of agency that are more tangible, more individual, more down to earth (see "Lapita as People" section below). So archaeologists today would be decidedly out of step with current anthropological theory if they were to continue using "forms and systems of forms" such as these as the rationale for saying that archaeology and anthropology must stay wedded to one another. Therefore a much better rationale is needed for keeping archaeology and anthropology together than what Phillips wrote about as forms and systems of forms. Consider, as an illustration, the case of Lapita pottery.

The Riddle of Lapita

"Lapita" is the name that Pacific archaeologists have given a (sometimes) ornately decorated style of pottery found on a number of islands in the Pacific geographically scattered between the Bismarck Archipelago just northeast of New Guinea and the smaller archipelagoes called Fiji, Tonga, and Samoa located far out in the central Pacific (Terrell and Welsch 1997). This pottery may be the oldest in Oceania. It dates to around 1,000–1,500 B.C.

For the sake of discussion, let us take it for granted that "what Lapita is" (but see Terrell 1989) as an *archaeological phenomenon* is now reasonably known. Some will disagree, but "what Lapita was" as a *historical phenomenon* cannot be said to be similarly well known and understood. What Phillips described as the "integration and interpretation of archaeological data" concerning Lapita pottery—particularly in comparison with the current archaeological inference and model building elsewhere in the world—are decidedly underdeveloped at present, in spite of allusions now and then in the Pacific archaeological literature to French historians, essential *conjonctures,* and the *longue durée.*

Some scholars say Lapita pottery marks the arrival of Austronesian-speaking peoples in Oceania from Taiwan or southern China (Spriggs 1997); others disagree (Oppenheimer and Richards 2001). Some say this unusual pottery must be the hallmark of an equally unusual "ethnic group" or "people" (Green 1992; Kirch 1997); others disagree (Terrell et al. 2001). Some say this ethnic group was the ancestral stock from which today's Polynesians arose; others disagree (although on this point, many actually agree). What are we to make of such claims?

Lapita as People

Consider the currently popular interpretation that this pottery is the imprint of a distinctive "people" or "peoples," commonly glossed as "the Austronesians" (Bellwood et al. 1995; Kirch 1997; Spriggs 1997:87). Setting aside the usual concerns that most archaeologists voice about "unit definition" and about whether artifact assemblages are, or are not, equivalent to what ethnographers have (sometimes) called "peoples" or "societies" and the like, I believe that Pacific archaeologists cannot afford to ignore the fact that at present there is little agreement in anthropology and the social sciences generally on what terms and phrases like "ethnic," "ethnic group," or "people" mean (Banks 1996:4–6).

As used in general parlance in North America, the basic idea behind these words and phrases seems to be the commonsense notion that every one of us "belongs to" or "is part of" an identifiable group of people who all share distinguishing traits of language, culture, and biology that are not of their own choosing. Many people would probably add that such traits, like the color of your skin or your sexual orientation, cannot be cast off. Such definitive characteristics are a part of who you are in spite of all that you might do to pass for some other kind or class of human being. Some would add that who you are in this enduring sense is *primordial*—your "heritage" is so much a given part of your identity as a person even at birth that your roots predetermine not only what you are like, but what everyone else "in your ethnic group" is like as well.

This way of thinking about ourselves and other people, however, is not encountered worldwide, and most anthropologists today would agree, I believe, that "ethnicity" in this sense is not a universal category of culture (Linnekin and Poyer 1990). Personhood in Pacific societies, to offer an example, often seems to be a combination of belonging defined by what many of us would call "blood kinship" and belonging defined in-

stead by residence and human nurturance (see Pomponio 1990). The best analogy I can think of is that ethnicity (if we must use this word) in the Pacific often comes across as something comparable to what we would call "citizenship," although to be honest even this parsing is too sharply drawn. It might be closer to the mark to say simply that many people in the Pacific do not base primal "belongingness" merely on what we see as "biology." Generally speaking, much more than biology comes into play.

There is a lesson here that archaeologists should attend to. They need to be cautious about using terms and concepts drawn either from general parlance or from the social sciences as if we all knew and agreed on what such notions are meant to convey. To return to our example, interpreting Lapita pottery as "people," "culture," or "society" is looking at the past through a glass darkly. Or, to switch metaphors, while you may think you are saying something wonderfully profound about this pottery, you may just be pulling the wool over your own eyes.

My main point, however, is not that archaeologists fastidiously need to avoid using certain words and technical phrases because anthropologists have now come to dislike them. What is important is the reasoning behind why anthropologists are now uneasy about such terms (or "forms," to use Phillips's term) and the interpretative concepts they usually stand for.

Our ideas about "ethnic groups," "peoples," "societies," "cultures," and the like are chiefly grounded on the conventional notions that (1) each person belongs to an identifiable and enduring group, tribe, society, or population; (2) these collective entities are historically enduring phenomena; and (3) these kinds of human associations are stable enough over time that the traits they exhibit can tell us about the origins of such persistent human corporations. Many anthropologists today see that each of these notions can be undermined by consulting the ethnographic record; so should archaeologists (Terrell 2000, 2001; Terrell et al. 2001). I will return to the point.

Lapita as a Cultural Complex

Pacific scholars today frequently also say that Lapita was a "cultural complex" (Green 1992; Kirch 1997; Spriggs 1997). This tag is not simply a refined way of talking about the archaeological assemblages excavated at Lapita pottery sites. This label is more interpretative than that, akin perhaps to what Phillips called "functional interpretation" (which he likened to what anthropologists conventionally used to call "ethnology"; see Phillips 1955:248).

Some critics of Lapita archaeology (I count myself among them) insist that before anyone can call Lapita a cultural complex, archaeologists need to spend much more time looking at Lapita archaeological assemblages at the raw descriptive level that Phillips dubbed "historical integration," by which he meant "almost everything the archaeologist does with his field data: typology, taxonomy, formulation of cultural 'units,' and investigation of their internal and external relationships in space and time" (Phillips 1955:249; for an example showing that such attention is needed, see Smith 1999). However, again I merely want to note here that the concept of a "cultural complex" evidently comes from anthropology—including the pen of the great linguist Edward Sapir (1916). If archaeologists want to use this technical phrase, then I think that they should listen carefully to Sapir's lucid cautions about the use and abuse of culture concepts (Sapir 1916:28–29).

For Sapir and others of his generation, calling something a cultural complex was *not* just an odd way of referring to what the great European archaeologist V. G. Childe, for instance, spoke of as a "culture" (Childe 1956). Specifically, for Sapir and others, a cultural complex was *not* "the whole of a culture" (Sapir 1916: 15). Instead, this phrase meant a "definite nucleus of associated traits" (Sapir 1916:30), such as the rituals, words, and cultural paraphernalia of the Native American Sun Dance or the Ghost Dance. The point to note is that in their opinion such associated traits are able to travel, to "diffuse" together from society to society, *without requiring the people adopting them to abandon all their old ways* (their former "culture") *in favor of these new ones*.

Historically speaking, therefore, this anthropological concept has had two distinguishing features that few archaeologists writing about the so-called "Lapita Cultural Complex" acknowledge. First, if it is granted that Edward Sapir knew what he was talking about, then this concept is decidedly *not* equivalent to what V. G. Childe and most archaeologists today would probably call an "archaeological culture." Thus the question arises: Do archaeologists working in the Pacific want us to see Lapita as an archaeological *cultural complex* or as an archaeological *culture*? Second, this seasoned concept is more than just a way of labeling a certain form, or kind, of cultural evidence; this tag stands for a particular kind of diffusion process, a particular manner in which cultural traits can move, or diffuse, from place to place (from "culture" to "culture") coupled with one another as meaningful trait associations (perhaps somewhat like what geneticists today call "haplotypes").

Judging by what has been written about this pottery and Lapita archaeological sites since the 1960s, most Pacific archaeologists see Lapita as an archaeological culture or cultures, not as an archaeological cultural complex (Terrell 1990, 1996). But was Lapita a culture? Most of the pottery found at Lapita sites is quite plain and was probably intended only for household use (e.g., cooking, storage). In contrast, the pottery jars, bowls, and the like decorated in the impressive Lapita style are so unusual that I suspect anyone acquainted with the art and material culture of Melanesia today would have no trouble agreeing that this distinctive cultural expression must have articulated something more extraordinary than ordinary, more esoteric than utilitarian (put simply, these elaborately decorated pots at least in some communities were not merely seen as great for cooking and handy for putting things in).

Here is my personal wager. While I do not imagine this is the hypothesis that was intended when Lapita was first described by Pacific archaeologists in the 1970s as a cultural complex, I think they should now seriously weigh the idea that this is precisely what Lapita was, at least in some areas of its ancient sphere of distribution in the Pacific: a cultural complex, not a culture. I suspect that Lapita shows us that at first the craft of pottery making in the Pacific was not just seen as a giant leap forward in domestic technology. I suspect pottery making in its early days in Oceania may have also served as a medium for "esoteric" expression (if I were writing a museum label, I might be tempted to say that the ornately decorated Lapita pots probably had a "ceremonial function"). In short, elaborately decorated Lapita pots were not just prestige ware, but part of a meaningful cultural complex.

I suspect this may have been why it evidently took so long—a thousand years or so—for pottery making to reach mainland New Guinea from the nearby Bismarck Archipelago where the oldest Lapita pots have been excavated. Lapita's more ornate pots may have served as part of the paraphernalia of some kind of cultural complex, say a cult or dance complex (for a classic ethnographic example of such a cultural complex in New Guinea, see Bateson 1936), and consequently it was harder for the craft of pottery making to diffuse on its own apart from this complex "trait association." In sum, initially pottery making in Oceania was not just "a material culture trait" that traveled on its own; instead, pottery making at first may have been part of a "nucleus of associated traits" that restricted its easy diffusion from "society" to "society." Judging by how cultural complexes are known to move in Melanesia today, at first people had to beg, borrow, or buy the whole cultural complex, not just one of its elements.

What is the message here for archaeology? We know enough about how cultural complexes travel from place to place in the Pacific at the present time to insist that Pacific archaeologists cannot afford to be ethnographically naive. If they are going to call Lapita a cultural complex, then they need to take this notion seriously and weigh the hypothesis that at least in some instances Lapita was precisely what they are saying it was—part of a nucleus of associated traits that diffused from one community to another 3,000 years ago in ways perhaps comparable, say, to trade in dance complexes in Melanesia at the present time (Roscoe 1989; Sillitoe 1978). (I wonder, for example, if this may not be one reason archaeologists often describe the other elements of bone, stone, and the like found in Lapita archaeological assemblages as "polythetic.")

Lapita as History

The last point I want to make here will be brief. For reasons that are only partly obvious, many popular interpretations of Lapita archaeological assemblages are embedded in old-fashioned migrationism. This is not necessarily bad (Anthony 1990), but it is not necessarily good, either. I suspect there are three chief reasons many see this material as the sign of a sudden and swift migration three or four thousand years ago made by Asian (Austronesian-speaking) seafarers with a way of life that was radically different from the ways of people who had been living in the Pacific for the previous 40,000 to 60,000 years. First, many see today's Polynesians as the direct descendants of "the bearers of Lapita culture" (Spriggs 1997:87), and tracing the supposed migrations of the ancestors of the Polynesians has been a cottage industry among foreigners living in the Pacific for several hundred years. Second, historical linguistics continues to provide the main storyline for most broad-scale reconstructions of Pacific prehistory. As many archaeologists dealing with the riddle of the Indo-Europeans know all too well, migrationism and historical linguistics somehow seem to be wedded to one another. Third, despite the fact that the oldest Lapita pottery comes from the Bismarck Archipelago (which is in Melanesia) and, geographically speaking, Lapita has "a Melanesian distribution with a Polynesian extension" (Kennedy 1982:24), many people still look at Pacific prehistory from a Polynesian point of view. As someone once said, Polynesia is the tale that wags the Pacific dog. And I would argue (see, for example, Terrell 1996) that many

current archaeological models of the Lapita phenomenon reduce the prehistory of Melanesia down to little more than a story about how the assumed Lapita ancestors of the Polynesians swept out of Asia and traveled through Melanesia to get to Polynesia.

I think there are several reasons to be cautious about migrationist explanations in archaeology, but I would like to end this essay by noting only two further points. First, as the geneticist Alan Templeton (1998:647) recently commented, "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." Dividing our species into huge racial or ethnic blocks of people and then moving them out (via great "diasporas") from their native "homelands" is not the only way to look at our human diversity and prehistory. Second, seen from an anthropological perspective, it is almost impossible to imagine how "Lapita people" could have traveled as a "packaged shipment" of racial, cultural, and linguistic traits all the way from Asia to Polynesia. But that is an argument I will only raise and not try to explore here (see Terrell et al. 2001, 2002).

Discussion and Conclusions

In 1955 Phillips described the relationship between archaeology and anthropology as one between servant and master. This assessment was evidently not as debatable then as it is now. Many archaeologists today would argue, I think, that what Phillips called "social facts" are not the only facts they need to know when they want to explain archaeological phenomena. Perhaps because of the 1960s ecology movement, the Green movement, global warming, Darwinian evolutionism, and maybe even today's postmodernist disparagement, many now accept that history cannot be explained only by taking into account social facts and anthropological theory.

All the same, Phillips was getting at something that I suspect we all sense has not changed much since 1955. Looked at from afar, what are collectively called The Sciences are like the Blind Men and the Elephant. Every science specializes in gathering its own peculiar information about only a limited number of dimensions, or variables, of the world around us or in the past. Therefore, to know and understand the world and the universe, scientists have to share information across their disciplinary boundaries. To reach beyond the social sciences for an example, molecular geneticists are experts at reading the genome, but other kinds of specialists are more skilled at gathering and analyzing information about people, their medical histories, their lifestyles, customs, behaviors, and all the rest that must also be understood to make sense of human biological diversity so that basic scientific information can be turned into medically helpful knowledge.

It may be a cliché, but the past is (or was) a foreign place. Archaeologists who do not keep this basic wisdom in mind risk forcing their own impressions and cultural notions on a world that was unlike their own and must be treated on its own terms. Archaeologists who fail to give the past its due have much to lose, for they thereby forfeit their best chance of learning how different the past was (Terrell 2000).

One way to give the past its due is to use historical and ethnographic information on what people now do (or are documented historically to have done) to shape ideas and scientific models about the past that are capable of taking us beyond the kinds of commonsense assertions, for instance, that archaeologists sometimes make (all too often, I believe) about the behavior and intentions of commoners and the upper crust in ancient Mesoamerica, Mississippian chiefdoms, or warrior kingdoms in times of old.

Most archaeologists know that neither history nor ethnography should be used for caulking chinks in our archaeological knowledge of the past (Terrell 2000). Although to some it may seem dangerous to read history and ethnography as instructive sources of good archaeological hunches and hypotheses, the hazards are trivial compared with the dangers of blindly following our own limited sense of what makes sense. And let us not forget: the test of any good idea in archaeology, whatever its source, is whether it helps archaeologists look for things in the archaeological record that they might otherwise overlook or underrate. Admonitions about the risks of using ethnographic analogies in archaeology may be wise, but using such warnings as an excuse for not reading history and ethnography is foolish.

What about the other side of the coin? Can sociocultural anthropology in North America get something out of its current marriage of convenience with archaeology? The answer used to be, as I noted at the beginning of this chapter, that by keeping in touch with their archaeological colleagues, anthropologists can get more grist for the mill—more "societies" to compare and contrast. It can be argued, however, that Phillips's expressed confidence that anthropology was "more science than history" was mostly wishful thinking even at the time he was writing.

Yes, it was fashionable in the 1950s to want to be a science, but there has always been a strong bent in the "Science of Mankind" toward antiquarianism and historical particularism. "Compare and contrast" studies in anthropology have more often than not led to the celebration of contrasts between societies rather than the discovery of universal or broadly general truths about human beings and the human condition. Furthermore, anthropology spelled with a capital *A* focuses most of its attention on the history and behavior of a single species. Combined as it is with a predisposition to favor the particularities and peculiarities of our chosen species, this intellectual fixation has long threatened Anthropology's coinage in the academic marketplace. Why? Because scholars who are basically interested only in what they themselves are studying may be little motivated to listen to what anyone else inside or outside Anthropology is saying if what is being said is not about their own favorite topic.

So asking whether anthropologists today still have anything to gain from archaeologists may be asking a good question but asking it too narrowly. Insofar as sociocultural anthropologists continue to be interested in learning about the human condition and its past, I think the answer to this question is most definitely yes. But to what extent is this mission still a shared goal or value within Anthropology? As far as I can tell, this is a question that right now has no sure answer.

References

Anthony, David W.
1990 Migration in Archeology: The Baby and the Bathwater. *American Anthropologist* 92:895–914.

Banks, Marcus
1996 *Ethnicity: Anthropological Constructions*. London: Routledge.

Bateson, Gregory
1936 *Naven*. 2nd ed. Cambridge: Cambridge University Press.

Bellwood, Peter, James J. Fox, and Darrell Tryon
1995 The Austronesians in History: Common Origins and Diverse Transformations. In *The Austronesians: Historical and Comparative Perspectives*. P. Bellwood, J. J. Fox, and D. Tryon, eds. Pp. 1–16. Department of Anthropology, Research School of Pacific and Asian Studies. Canberra: Australian National University.

Childe, V. Gordon
1956 *Piecing Together the Past: The Interpretation of Archaeological Data*. London: Routledge & Kegan Paul.

Green, Roger C.
1992 Definitions of the Lapita Cultural Complex and Its Non-Ceramic Component. In *Poterie Lapita et Peuplement*. J.-C. Galipaud, ed. Pp. 7–20. Nouméa: ORSTOM.

Kennedy, Jean
1982 Archaeology in the Admiralties: Some Excursions. *Bulletin of the Indo-Pacific Prehistory Association* 3:22–35.

Kirch, Patrick
1997 *The Lapita Peoples: Ancestors of the Oceanic World*. Oxford: Blackwell.

Kroeber, Alfred L.
1948 *Anthropology: Race, Language, Culture, Psychology, Prehistory*. New York: Harcourt, Brace.

Linnekin, Jocelyn, and Lin Poyer, eds.
1990 *Cultural Identity and Ethnicity in the Pacific*. Honolulu: University of Hawai'i Press.

Oppenheimer, Stephen, and Martin Richards
2001 Fast Trains, Slow Boats, and the Ancestry of the Polynesian Islanders. *Science Progress* 84:157–181.

Phillips, Philip
1955 American Archaeology and General Anthropological Theory. *Southwestern Journal of Anthropology* 11:246–250.

Pomponio, Alice
1990 Seagulls Don't Fly into the Bush: Cultural Identity and the Negotiation of Development on Mandok Island, Papua New Guinea. In *Cultural Identity and Ethnicity in the Pacific*. J. Linnekin and L. Poyer, eds. Pp. 43–69. Honolulu: University of Hawai'i Press.

Roscoe, Paul B.
1989 The Pig and the Long Yam: The Expansion of a Sepik Cultural Complex. *Ethnology* 28:219–231.

Sapir, Edward
1916 *Time Perspective in Aboriginal American Culture, a Study in Method.* Memoir 90, Anthropological Series, no. 13, Geological Survey, Department of Mines. Ottawa: Government Printing Bureau.

Sillitoe, Paul
1978 Exchange in Melanesian Society. *Ethnos* 43:7–29.

Smith, Anita Jane
1999 An Assessment of the Archaeological Evidence for Cultural Change in Early West Polynesian Prehistory. Doctoral dissertation, School of Archaeological and Historical Sciences, La Trobe University, Bundoora, Victoria, Australia.

Spriggs, Matthew
1997 *The Island Melanesians.* Oxford: Blackwell.

Templeton, Alan
1998 Human Races: A Genetic and Evolutionary Perspective. *American Anthropologist* 100: 632–650.

Terrell, John Edward
1989 Commentary: What Lapita Is and What Lapita Isn't. *Antiquity* 63:623–626.
1990 Storytelling and Prehistory. *Archaeological Method and Theory* 2:1–29.
1996 Lapita as History and Culture Hero. In *Oceanic Culture History: Essays in Honour of Roger Green.* J. Davidson, G. Irwin, B. Foss Leach, A. Pawley, and D. Brown, eds. Pp. 51–66. Special Publication. Dunedin: New Zealand Journal of Archaeology.
2000 Archaeology, Material Culture, and the Complementary Forms of Social Life. In *Fleeting Identities: Perishable Material Culture in Archaeological Research.* P. Ballard Drooker, ed. Pp. 58–75. Carbondale: Center for Archaeological Investigations, Southern Illinois University.
2001 [Editor] *Archaeology, Language, and History: Essays on Culture and Ethnicity.* Westport, CT: Bergin & Garvey.

Terrell, John Edward, Terry L. Hunt, and Joel Bradshaw
2002 On the Location of the Proto-Oceanic Homeland. *Pacific Studies* 25(3):57–93.

Terrell, John Edward, Kevin M. Kelly, and Paul Rainbird
2001 Foregone Conclusions? An Analysis of the Concepts of "Austronesians" and "Papuans." *Current Anthropology* 42:97–124.

Terrell, John Edward, and Robert L. Welsch
1997 Lapita and the Temporal Geography of Prehistory. *Antiquity* 71:548–572.

Willey, Gordon R., and Philip Phillips
1958 *Method and Theory in American Archaeology.* Chicago: University of Chicago Press.

Winterhalder, Bruce
2002 Models. In *Darwin and Archaeology: A Handbook of Key Concepts.* J. P. Hart and J. E. Terrell, eds. Pp. 201–223. Westport, CT: Bergin & Garvey.