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REVIEW ESSAY

CONTEXTUALIZING OUR PAST: P-M LITE COMES TO MICRONESIA

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The Archaeology of Micronesia (Rainbird 2004, hereinafter *TAM*) is culture historian Paul Rainbird's second try to educate a wider audience about the (mostly prehistoric) archaeology of the northwestern tropical Pacific islands (for his first, see Rainbird 1994). Since Micronesia's archaeological record is among the less well known of the traditional geographic subdivisions ending in "nesia", this ambitious book, covering an oceanic region extending some three thousand miles from west to east, is a welcome addition to the literature. Compared with the article, *TAM* provides more anthropological and historical discussions, and, to be expected since a decade has passed, quite a lot more archaeology. For the table of contents, index, and front matter and to read an excerpt from Chapter 1 that gives the flavor of the whole, web-surf to <http://www.Cambridge.org/uk/catalogue/catalogue.asp?isbn=0521651883>.

TAM's first three chapters introduce readers to the what Rainbird calls the "non-region" of Micronesia (arguing the vast area is too diverse to fall under such a conventional label; pity that he did not prevail with Cambridge's title-maker) The last chapter briefly sums up, makes predictions for "the future of the past", and re-states what Rainbird hopes to have ac-

complished by re-orienting archaeological research in the region through a process he calls "contextualization" (a scholarly chore that he feels has been inadequately performed by previous Micronesians). Between the introductory and concluding chapters are the more overtly archaeological ones although there is much anthropogizing here, in line with the author's belief that "[a]rchaeology is about people, it is about constructing an understanding concerning people in the past..." (p. 13).

The archaeology chapters are organized by topic and by geographic area or island type. Chapter 4 covers the who, when, how and why of prehistoric settlement of the region, and Chapters 5-8 present findings from, respectively, the Marianas; Palau, Yap and the Carolinian atolls; the eastern Carolines; and other atoll groups and outliers (Marshalls, Gilberts, Banaba, Nauru, Kapingamarangi, Nukuoro). In these four chapters the reader may become a little disoriented or even discouraged from continuing because the data and interpretations are disparate and lack overall coherence. This reflects the different research interests, problem orientations, and amounts of fieldwork that have been conducted and the author's attempt to be fair to all. However, it also reflects the weakness of a confused position that

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overtly rejects science in favor of a people-based culture history but still relies on inherently ambiguous archaeological facts that need the rigors of scientific analysis to interpret reliably.

According to the inside cover, *TAM* has two warrants from Cambridge: to provide an up-to-date account of research and to integrate recent findings with new concerns of interpretation. While the first seems straightforward and has been done in workmanlike fashion, the success of the second is harder to affirm from this reviewer's perspective. Rainbird takes an approach to Micronesia's archaeological record that privileges language and ideas from the humanities over those of the sciences. His approach champions a long-held goal of archaeology, reconstructing past life ways, but not the explanatory focus shared by many practicing archaeologists trained in American universities over the last several decades. Rejecting the concept of ecosystem, or just about any kind of system or systematic relationship in cultural phenomena, Rainbird adopts metaphors (he calls them "motifs") to help convey his meanings and understandings. Thus we have the concepts of "fusion" "fluidity" and "flux" that invoke not only the physical ocean but also the kinds of cultural-historical processes that inevitably take place there.

To me, the "archaeological anthropology" (Rainbird inverts the usual sequence here) of the three f's harks back to Anglo-American trait-listing, with a strong dose of anti-evolutionism. It is a rather Boasian view of culture in which different traits combine and recombine like atoms; the Rainbirdian twist is that this occurs according to the will of individual agents ("people"). For example, in Chapter 5, selective borrowing of traits by the late prehistoric Marianas people occurred because cultural boundaries within the ocean are "fluid"; this allowed newly borrowed traits (from the Philippines) to "fuse" into "the existing corpus" (i.e., the culture of the Marianas at the time); the new elements introduced a state of "flux"; and this led to a rapid material culture change, exemplified in the adoption of, for example, *latte* stones (p. 132). Is this not what used to be called culture change by diffusion,

that old non-explanatory device favored by culture historians to describe material culture change? Rainbird's "new concerns of interpretation" appear to be very old ones indeed.

Since he hasn't given up entirely on the empirical world, just a materialist philosophy of knowledge, Rainbird's "interpretive stance" could be called post-modern lite. Compromises with regular post-modernism (hereinafter p-m) have been necessary because in writing the culture history of non-literate peoples such as the ethnically varied Micronesian island groups, the primary "texts" (so essential in p-m cultural studies) are actually the stones-and-bones data generated by archaeologists who operate within a materialist, cultural evolutionary paradigm. This situation seems to have mandated the following inclusionary gesture to justify use of this material: "...it should be clear to the reader that alternative scenarios are being proposed [in *TAM*] to those that are normally derived from the dominant paradigms generally encountered in the region, and elsewhere in the archaeology of Oceania, to date. This does not mean that the 'baby is thrown out with the bathwater', as the methodologies of fieldwork practice are generally comparable amongst archaeologists and there is certainly very interesting work from all paradigms to be considered" (p. 69, brackets added). I am not sure what "methodologies of fieldwork practice" means but I think this would include survey and excavation methods that are concerned with strict record keeping, curation of materials for laboratory analysis, and making such information available to others. The rub is that in problem-oriented research, data are collected with certain analytical procedures already in mind and therefore it is a mistake to think that all the data collected in the field will be "comparable"; those of us who have actually tried to use others' data have learned this lesson.

Partially to blame for this confusion over which observations can and should be incorporated into one's interpretations is the p-m caveat that individuals are the causes of cultural variation -- what used to be called cultural differences and similarities and whose explanation has been a major aim of Anglo-American archaeology since at least the 1960s. As Rainbird

avers on p. 65, the difference between individual societies “is created through the agency of individuals,...” so that centering one’s explanations upon the “structural level of society...ignores the effects of human actors who are involved in a dialectic relationship with that structure, in a recursive manner both shaping and being shaped”. The problem here is circular logic. Yes, of course, individuals are creators of society (no people, no society, obviously), and, yes, of course, they have ideas, make artifacts and decide what to do with them. Also, they modify their physical surroundings, a point easily granted if one has ever been to a currently or previously inhabited island. However, these actions, past and present, are undertaken within what is usefully called an adaptive systemic context; the relationships involved are far more complex than simply “dialectic”. Non-circular logic says that individuals and their thoughts and actions are a *given* in the study of cultural difference. They cannot be the causes, since there is no way to *independently verify* inferences about what individuals did or thought except by invoking their material remains, which, of course, are the subject of archaeological explanations. To cite the “Indians behind the artifacts” as the *causes* of cultural system differences is a tautology. In his call for giving prominence to individual agency in our explanations, Rainbird is asking us to go backwards, to a pre-scientific age when the collector’s (here the cultural historian’s) erudition, personal integrity, honesty, etc., (all desirable traits in a scholar, certainly) are all that is needed to instill confidence in his/her interpretations (see H. Sidky 2003).

In his zeal to give the presumed actions of ancient individuals their due, Rainbird makes another logical error that leads to circularity. In Chapter 4 he assumes, like many other Pacific archaeologists these days, that paleontological data from sediment cores are just the same as archaeological evidence in their ability to indicate human activities. This fallacy, which flies in the face of the parsimony principle in science, leads to blithe acceptance of claims that charcoal particles observed in ancient wetland sediments are “proxy evidence” that people were present in the Marianas and Palau at least

a thousand years before the archaeological records so indicate, and not only that, that they fired the “pristine forests” and in so doing caused erosion and sedimentation in coastal areas. Such an argument entails many more assumptions than are necessary; a simpler explanation would invoke natural processes sure to have been operative in the past, as they are today.

Rainbird’s version of Marianas and Palauan environmental history, in contradistinction to the usual allegation of innate or inept destructiveness of humankind (see, for example, Bahn and Flenley 1992), is that the paleontological data show how effective and beneficial human actions were in the past -- early settlers decided to change their new environment and did so (e.g., cutting down forests so that erosion would occur and create lowland agricultural soils, just like they had back in their original islands). How do we know this? The paleontological record contains the proxy evidence of human intent, namely, charcoal particles; and the tautology is complete. Another example of the conundrum introduced by accepting ancient fossil pollen in a paleosediment core as evidence for human presence comes from Palau. The pollen is identified as giant swamp taro, a common cultigen in Micronesia, yet the sediments in which it is recorded have been dated to c. 4500 BP. Instead of looking for a paleontological explanation involving botanical biogeography for this observation (was it actually a wild form, a precursor to cultivars in use throughout Micronesia now?), the swamp taro pollen is cited as evidence for human presence (they must have planted it). Again, the undue complexity and circularity of this argument is plain, and we have no answer as to why there is no archaeological record that old in Palau.

Like other authors who use such a logical sequence to interpret paleontological data and to write culture history, Rainbird ignores the real world conditions that cause variability in the paleontological record. For example, he does not heed the implications of sea level decline that is known to be associated with terrestrial erosion, nor does he take seriously the possibility of non-human caused fires in the tropics, for which ample independent evidence

exists that relates pre-human fires to El Niño drought frequencies. Thus while Rainbird allows that “very interesting work from all paradigms [should be] considered”, this does not extend to the implications of some of this work, such as geological and climatological phenomena. *TAM* is outstanding in its ignorance of physical environmental factors as shapers of cultural system responses. It is clear to me that this negligence has everything to do with an anti-science “interpretive stance” that is far from inclusive.

Actually, and ironically, parts of Rainbird’s interpretations (which I would call explanation-sketches), resemble deductive scientific arguments in their appeal to external reality for coherence and verification. For example, in an account of the megalithic sites like Nan Madol in Pohnpei and Lelu in Kosrae which lack counterparts elsewhere in the Eastern Carolines (although all the high islands in this area were occupied at about the same time, 2000 BP, and seemingly were settled from Melanesia by like-cultured folk), Rainbird invokes social-organizational differences that developed over time due to (gasp!) demography and geography. Thus he states on p. 251, “...one thing that Pohnpei and Kosrae had was large and probably stable populations compared to neighbouring atoll populations, owing to resource availability. They also were not fragmented high islands like those in Chuuk Lagoon”. I especially appreciate Rainbird’s debunking of the alleged (by Cordy, Athens, Bath, Ueki, and others) centralized character of the societies whose members erected Nan Madol and Lelu, using a logic worthy of a materialist and the cogent theorizing of cultural anthropologist Glenn Petersen.

Notwithstanding his fairly frequent forays into the empirical world for verification and his less-than-perfect inclusionism, Rainbird takes pains to distance his interpretive approach from that of American-trained anthropologists and archaeologists, who continue to dominate the field with their allegiance to science. Work inspired by island biogeography is singled out as especially benighted for implying that islands and their inhabitants’ cultures, can be studied as “natural laboratories” consisting of eco-

systems with fairly clear boundaries (i.e., shorelines and reefs). According to p-m think, this violates the idea that islanders were not isolated but rather connected by the ocean that surrounds them (the oft-quoted words of Epeli Hau’ofa (1993) are quoted again here. Interestingly and, in contradiction to Rainbird’s assessment, island biogeographers and others have long recognized the social permeability of island ecosystems. This includes cultural anthropologist William Alkire, whose fundamental discoveries regarding land shortage and drought as major conditioners of small island cultural system dynamics, are damned on p. 45 with what is faint praise but actually alludes to a great achievement in science: “In its generality and within the concerns of ecological anthropology, Alkire’s argument has a certain validity for certain places at certain times”. It is not the truth of Alkire’s findings that is called into question here; it is its conformance to a scientific approach which led Alkire to ecosystemic analysis to explain cultural behavioral variability.

I am not sure if he means this, but I take the following exhortation on p. 43 to imply that archaeologists also have failed to appreciate the social permeability of island ecosystems: “If boundaries have existed in the pre-European past of the region they certainly are unlikely to have been static for the whole time, and archaeologists cannot afford to ignore the temporal significance, the fluidity, of boundaries, including the modern one under discussion here”. Just in case I am not wrong, here are some examples that show this has not been our failing. In a review of island biogeographical approaches in archaeology and as an introduction to *The Archaeology of the P’eng-hu Islands*, Tsang (1992) notes that back in 1973, archaeologist J. D. Evans had “already pointed out that although the sea is a barrier to the island communities, it can also be, in certain respects, a most effective medium of communication between them once adequate water transport becomes available. Therefore (quoting Evans, ‘it is a reasonable postulate that island communities are on the whole more open to some kinds of cultural contact than to others’ “.

Further to the point that islands have never been thought by archaeologists as truly isolated during their human occupations, Tsang points out that Evans in the same work noted another obvious characteristic of islands that gives rise to social connections: "...from the archaeological standpoint, what is important is that the restricted range of locally available resources often means that it is relatively easy to pick out objects made of substances which must have been brought from elsewhere, either as raw material or as finished products...In this way we can build a picture of the range of contacts available to a particular community at a given time...Conversely, many islands have special products which are in demand over a wide region, and the study of the distribution of these may give valuable evidence about the extent and nature of the trade or other contacts in a particular area at a given time."

John Terrell (e.g., 1977) is another biogeographically oriented archaeologist who explicitly recognized the connectedness of island societies, using the concept of "regional geographic system" as a study tool. In the Pacific island literature of the 1980s and 1990s one encounters numerous discussions of the social aspects of the geographic expansion out of Melanesia into the remote Pacific of the so-called "Lapita peoples", putative ancestors of the Polynesians (see Kirch 1997). Kirch and others have argued that the settlement of the remote islands of Polynesia was accomplished by maintaining social connections between "parent" and "daughter" communities. I would add that without connections among small islands and larger ones, there could be no "long-term history" in the former. Such is predicted by biogeographical theory, for example, the "sources and sinks" models of H. R. Pulliam (1988).

In denouncing biogeography as inappropriate for anthropology, and by extension, for archaeology, Rainbird has conflated analytical distinctions and historical conventions. Thus he rejects the label, Micronesia, as inaccurate and an historical accident that implies a false separation among island peoples, just as he rejects the ecosystem concept for being too rigid to accommodate the idea of inter-island com-

munication. I, for one, must ask, how about that connectedness of people on islands? Did not frequencies of inter-island connections *vary* in the past, and is that not interesting? If so, then we have the beginning of an open-ended research program in which many different questions related to the conditions under which interactions increase, remain steady, or decrease (and there are other possibilities) can be explored and relevant data sought to answer them. Rainbird's approach precludes such a generative research program, being essentially restricted to seeking the physical signs of inter-island connectedness and bringing them to the fore, for example, his discovery of an "enveloped cross" engraving at a sacred site in Pohnpei that resembles enveloped crosses recorded in Melanesia (p. 196). All such a narrowly focused effort achieves is substantiating that interactions occurred. But we knew that already.

Here is not the place to review all facets of *TAM's* archaeological coverage in the data chapters, which is certainly better than was possible a decade ago. However, from familiarity with Marianas archaeology, I will comment further on Chapter 5, "Identifying Difference, the Mariana Islands". First, Rainbird has compiled a lot of material but not all that should be here. Much of the new work of which I am aware remains unpublished, unmentioned, and unevaluated in Chapter 5. Absent are discussions of the implications of all the large data sets generated during surveys and excavations on private and military lands in the 1990s and early 2000s (just some of them are mentioned). Even the results of a project that encompassed 1% of Guam's land area, in an interior upland setting that included three different physiographic zones, and in which Rainbird was a student-participant, are not considered nor is the four-volume report cited. Important findings of that project include the wide diversity and high frequency of subsurface features at inland sites not associated with contemporary *latte* stones, and of human interments being primarily females of childbearing age and also not associated with contemporary *latte* stones; settlement preferences for limestone substrate over volcanic

substrates; increasing site density from c. 1200-1400 CE followed by near abandonment. These results and others from recent projects require re-assessment of generalizations about late prehistoric Marianas cultural organization that have been based upon investigations at large coastal sites to the exclusion of other settings.

In “identifying the difference” posed by the Marianas, Rainbird concludes on p. 133 that these islands are probably best treated as “Micronesian outliers” because they are so different from the (more Micronesian?) Carolines, Marshalls, and Gilberts -- this from a scholar who spends hundreds of words on why Micronesia has no cultural unity. If this doesn’t exemplify the p-m culture historian’s inability to cope with variability through analysis along relevant dimensions I don’t know what would be. It is dismissal-by-classification-without-explanation (other than that the Marianas people interacted with ancient Filipinos who provided the new traits that define the Latte Phase). To be more positive, Rainbird has called attention to an intriguing pattern in the Marianas data, namely, the simultaneous late prehistoric advent of the commensal murid *Rattus exulans*, megalithic architecture, and rice cultivation and its associated processing implements. To Rainbird this is a short but satisfactory trait list that strongly indicates that the Marianas people were connected with people in the Philippines, not isolated and evolving on their own, so to speak, and leads to the speculation that the “flux” occasioned by cross-cultural interactions caused the Marianas culture to change. One could more perceptively ask, why did it take so long? Why was there not enough “interaction” during the preceding two and a half millennia? Asking such questions leads in some new research directions, ones that just might involve regional geographic systems models!

That the findings of all major surveys and excavations, not to mention smaller projects, were not incorporated into Chapter 5 may be better understood and contextualized, once the following facts are considered. Over the last two decades, the vast majority of archaeological surveys and excavations in US-affiliated

Micronesia (all the islands except the Gilberts, Banaba, and Nauru) have been (and continue to be) carried out under non-academic auspices (although limited university- and museum-based research has been conducted out of Japan, the U.S., New Zealand and Australia and is increasing apace). Designed to comply with local and federal historic preservation laws, these projects, which number in the hundreds, are reported in a “gray literature” of uneven quality and completeness. In part such deficiencies stem from the limited effectiveness of local historic preservation offices which, lacking professional staff, cannot adequately regulate the conduct of archaeology in their islands and in part from an absence of scholarly review of compliance reports.

This is the 10,000-pound elephant in the room problem, *TAM*’s unacknowledged burden of missing data, most heavy in the Marianas and Palauan cases. The elephant is not unmanageable, to continue the metaphor, but it is not easily done. To provide a “coherent and comprehensive account of the archaeology of the region” (the intention expressed in Rainbird’s Preface and Acknowledgements, p. xi and notwithstanding his coy self-evaluation on the final page in the book: “The material I have used -- and I am confident that I have not missed too much -- has not resisted the interpretations I have constructed...”), one must attend to this massive literature by direct and intensive scholarly interaction with it (this would include cleaning out the metaphorical elephant’s stall too). The author has not undertaken this really difficult but necessary task, instead reserving his scholarly attention for a relatively small number of publications, technical reports that were accessible to him and relying upon secondary compilations and personal communications with certain researchers, biases included but not criticized. Perhaps there was too little time and money to do a thorough vetting of all the major contract reports, much less the minor ones with their sometimes surprising results. Rainbird says he spent “an extended period of time” as a Visiting Fellow at the Australian National University to complete this work, but it is clear he did not visit the technical libraries of the region’s

local historic preservation offices nor did he visit the contract firms where one finds the compliance reports and the data bases generated on contract.

Its lack of comprehensiveness notwithstanding, should you buy this book? Yes, if US\$45 (for the paperback; US\$90 clothbound) is not a problem given the low production values; if you want an update from the 1994 *JWP* article; and if you must have your very own printed bibliography of a goodly number of works pertaining to Micronesian history and prehistoric archaeology. The illustrations, all black and white and some overly large for their information content, are very poorly reproduced (except for the photographs, which are fine). Also marring the reading experience is the evident lack of careful copy editing. Whatever happened to those sticklers contracted by Cambridge to work with authors to make sure standards were kept up, who caught every dangling and misplaced modifier, incomplete sentence, and misspelling in one's text? Perhaps this kind of editorial oversight has become too expensive in these days of long title lists of nearly always poorly selling academic books. Especially toward the end, the writing gives the impression of haste and exhaustion (author and reader are both glad it's over). *TAM* is not recommended for classroom use without proper contextualization, some of which I have tried to provide here.

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