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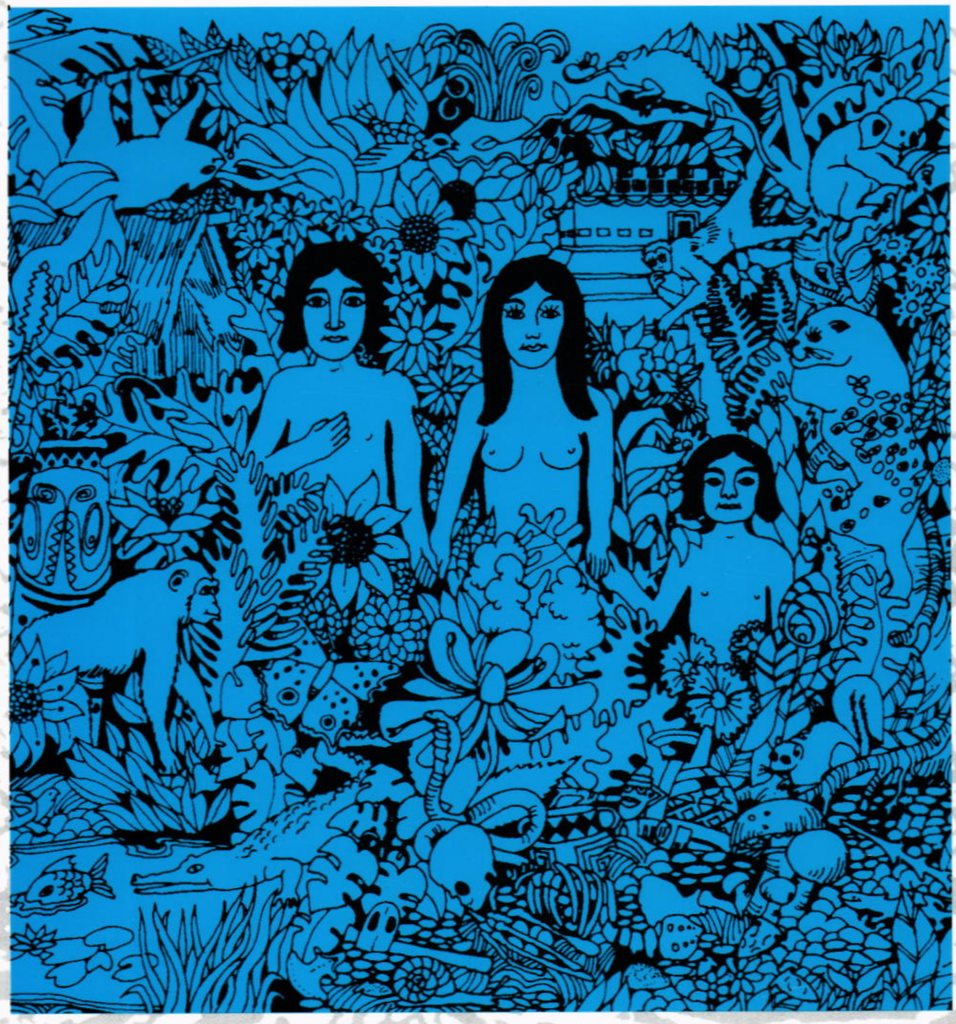
Stuart Bedford, Christophe Sand and David Burley (eds), ***Fifty Years in the Field: Essays in Honour and Celebration of Richard Shutler Jr's Archaeological Career***



FIFTY YEARS IN THE FIELD. ESSAYS IN HONOUR AND CELEBRATION OF RICHARD SHUTLER JR'S ARCHAEOLOGICAL CAREER

Edited by Stuart Bedford, Christophe Sand and David Burley

25
NEW ZEALAND ARCHAEOLOGICAL ASSOCIATION MONOGRAPH

ON SHUTLER AND MARCK 1975[1]

Jeff Marck

LOVE AT FIRST SITE

I first saw Richard Shutler Jr. in the summer of 1973 from the bottom of a ravine in the Loess Hills of southwest Iowa. Dick was about 200 or 300 feet overhead in a huge transport helicopter. He and other newer and older friends were waving vigorously. I waved back. They continued waving. I continued waving. This went on for some time until the helicopter manoeuvred a few hundred feet beyond the crest of the ravine and landed.

I thought it was a bit thrilling... the thunder of the big bird's engines... the roar of the wind down upon me. But I stayed meekly out of the way for the rest of Dick's visit when I found out minutes later that they were not waving in greeting. They had been trying to wave me away so they could throw down gigantic nets they had carried from 250 miles away. Nets with which they would haul out the samples the geologist had been accumulating over the summer in 50 or 100 pound burlap bags down at the bottom of the ravine. The State Archaeologist had persuaded the National Guard to help out our project as a training exercise and Dick had been invited along for the show.

Actually I didn't have to stay out of his way at all. The other students and myself were immediately his comrades-in-arms, Dick kind of trooping around the place, asking us constant questions, engaging us, finding out about our backgrounds, our interests and the progress of the immediate project. When the call came a few weeks later to help with an emergency Paleo-Indian dig led by Dick, we all volunteered without a second thought.

The Iowans weren't necessarily much aware of or interested in Dick's Pacific Island or Hominids in East Asia work. He was mainly known to the other archaeologists and students as the person who conducted a trenchant inter-disciplinary investigation of Tule Springs, a site which had been claimed, by people Dick referred to as "the Southern California lunatic fringe", to have extremely ancient remains of human activity. Dick's group, which included geologists, subsequently found no evidence of human activity in the horizons concerned.

I was finishing up a bachelors degree in anthropology at the University of Iowa Department of Anthropology in 1973 and Dick was our new department head. New the year before, I think. But I never thought to go in and introduce myself to such a lofty individual. My BA had mainly been concerned with African economic anthropology and economic development. I had shifted to Pacific prehistory that spring semester.

I forget how it came to pass that I would have shifted to Pacific prehistory and determined to study under him without having actually introduced myself to Dick. But somehow that was the case and the image of him in his eternal fishing hat smiling and waving out of the helicopter is certainly the first time I ever saw his face.

If the reason I had not introduced myself to Dick had anything to do with timidity towards elevated individuals, that waned during the next two years as I worked closely with him. All the graduate and undergraduate students had unusual access to him, as I look back on it.

Dick's standards for making time available for students set some of us up for a bit of disappointment with other professors over time[2]. I can't recall him ever being too busy or tied up with anything to speak to any of us for as long as it took to work through whatever idea or question we had on our mind. And then there were the endless social events at his and Mac Marshall's homes.

1. I am grateful that the editors invited me to write on this topic. I apologise for the paucity of references but I have been in the field through the writing of this paper and few sources but memory have been available to me. But I reckon I wasn't being asked to write a standard academic paper and am especially glad to see Dick reach his 80th birthday in such good shape and to participate in this volume in his honour.

2. Such experiences apparently being the norm. See Wm G. Bowen and Neil L. Rudenstine. 1992. *In Pursuit of the PhD*. Princeton University Press.

Mac was similarly generous with his time. And they were the two most prolific publishers in the department or close to it. I'd also like to mention Ed Koslowski from those years. My patient anthropological-linguist professor, who, more than Dick and Mac, had to deal with me when my enthusiasm was off somewhere, way ahead of my organisation or training.

It would be nice to pepper this section with anecdotes reflecting on Dick's "personality" because, of course, some archaeologists have rather a lot of it... more than we would like, sometimes. But I have few such memories from those years or those that have transpired since. Rather there is the memory of a steadfast, faithful mentor. A great motivator who drew us into a swirling world of unfolding discoveries and helped us lay our individual courses according to our talents and interests. There is however one very special anecdote for which I have mixed memories. In 1977 when my father lay dying at the University of Iowa Hospitals, and my mother was too horrified to visit as much as she might, and my parents' first grandchild was 10,000 miles away with us on Saipan and as yet unseen by them... it was Dick and Jamie who were taking him magazines and chocolates every day and pumping him up and keeping him going and painting a picture of what we were doing on Saipan that he otherwise never would have had. The gravity of Dad's situation was concealed from us by all concerned, especially Dad because he wanted us to finish up on Saipan in an orderly manner. He died about twenty days after we got back. Few people close to the family understood how he lasted so long. They didn't know Dick and Jamie.

Dick has wide interests in history, the arts and sciences and was constantly introducing us to wonderful people from other departments and disciplines. He remains intellectually gregarious and a great promoter of our disciplines and the universities at large in the sense of his ability to bring pleasant, thoughtful, motivated people together from across normally unrelated sections of the university and work on a vision of research and education which is stimulating to the people doing the work and relevant to the students and societies they serve. As many of his friends from all over the world have mentioned to me for many years, he is surely the most faithful correspondent any of us know.

Something I've always remembered from the Iowa years was Dick's after-lunch nap. He had a large office and I don't recall if it was a couch or camp-cot in there but Dick normally had a nap on it after lunch. He was about the age I am now, fifty, fifty-one or something. We students wrote his naps off to his advanced age but as more becomes known about the science of sleep it turns out we'd all be better off if we did so as well. But on we go, slugging through that drowsy part of the early afternoon when whole continents have the good sense to snooze.

THE STORY OF THE 1975 PAPER

During the 1973-1974 academic year, I took linguistics courses because I had to but soon realised that I would be doing language and prehistory for the rest of my life. Towards the middle of 1974 Dick mentioned that he would be going to the Australia and New Zealand Association for the Advancement of Science meetings in January of 1975. He asked if I would care to join him in writing a paper in the archaeology-and-language idiom of Pawley and Green from New Zealand. We did so, Dick presented the paper and it was published some months later (Shutler and Marck 1975) in *Archaeology and Physical Anthropology in Oceania*[3].

Dick and I were avid readers of the literature emerging from Andrew Pawley, Roger Green and their colleagues. Roger and Andrew were writing synthetic archaeology-and-language works in what sometimes seemed to me defensive tones, laboriously laying out why it was possible to do synthetic work in *some* instances. They may not have thought of it that way but the context was, and often remains, a broad distrust on the part of general anthropologists at least, of such synthesis due to the elusiveness of convincing correlations in Indo-European and some other language families or parts of language families.

Neither apologising for anything nor belabouring a presumptive connection between the dispersal of Austronesian languages and yet-to-be linked Insular Southeast Asian archaeological traditions, Dick and I wrote the ANZAAS paper, Dick keeping the idiom "true" to the framework of archaeology and myself doing so for linguistics.

The paper concerned the general dispersal of Austronesian languages and the particular relationship of Oceanic languages to non-Oceanic Austronesian. We culled statements by linguists for information relevant to the original geographical source of Austronesian speech and the geographical source of what became Oceanic Austronesian speech in later millennia. It included general speculation as to the nature of the emergence of Austronesian speaking peoples out of Taiwan into Insular Southeast Asia and on to the Pacific. The question of time depth was reviewed. Speculation was offered as to what the relevant archaeological horizons and linkages might be. This was framed both in terms of what was known at

3. Which later became *Archaeology in Oceania*.

the time and what might be expected and searched out given the linguistic relations.

I told Dick flatly that Austronesian speech originated in Taiwan and that Benedict's suggestion of external relations having to do with Tai-Kadai languages seemed, at least, popular amongst Tai-Kadai specialists.

Dick was concerned about the level (antiquity) of Austronesian diversity in western Melanesia due to Dyen's lexicostatistical study (Dyen 1965) and Dyen's continuing comments in print and correspondence about the possibility of the earliest Austronesian centre having been in Melanesia. I explained that linguists had been aware, since Milke's (e.g. 1961) work and before (esp. Dempwolf, cf. Dempwolf 1929), that Proto Oceanic was ancestral to all Austronesian in Oceania[4] and that it post-dated Proto Austronesian by some substantial but unknown period of time.

And it was clear, linguistically, that the corridor of entry into Oceania by early Austronesian speakers was across the islands north of New Guinea and thence onward into the further reaches of what are now called central and southern Near Oceania and on to Remote Oceania (Micronesia, East Polynesia and, in some senses, Western Polynesia and Fiji). Howells (1973) was the only recent work that continued to examine the Micronesian corridor at about that time. Still, Dyen's lexicostatistical study left some non-specialists wondering if there remained linguistic reasons for considering scenarios other than the Melanesian corridor or even for doubting that Proto Oceanic was younger than Proto Austronesian.

Our study in vocabulary statistics was a bit of a veiled invitation to Dyen to quit speaking of "diversity in Melanesia" as if it involved greater antiquity than anywhere else. More generally, it was an attempt to deconstruct the lexicostatistical approach for non-specialists by providing an alternate method of statistical comparisons of vocabulary agreements. We chose one that would be more likely, given the time depth involved, to discover vocabulary agreements consistent with the true genetic relations of the languages involved. One more likely to be consistent with the diagnostic[5] comparative linguistic evidence of genetic relations that was developing at the time and those which the coming decades of work by the linguists might reveal. And especially one that would make use of a known proto language whose immediate external relations were the subject of much interest and were being discovered in an initial sort of way by other methods.

The statistical method we employed said, more or less, that if you are going to do lexicostatistics or lexical statistics for languages related so anciently, wouldn't it be more interesting to eliminate chance losses and retentions for the main target of study, Oceanic languages, by taking Proto Oceanic itself as the point of reference and compare its agreement amongst various non-Oceanic Austronesian?

Our method turned out to be no more accurate than lexicostatistics and the results are at variance with the external relationships now understood for Oceanic. We proceeded with the sections comparing Proto Oceanic vocabulary to groups of Minahasan, West Indonesian, Philippine, Formosan and Moluccan languages with the knowledge that it was only slightly more likely to identify real subgroups than lexicostatistics itself[6]. The purpose was to call attention to the differing kinds of results obtained when the general assumptions of lexicostatistics are transposed to a different method which, theoretically, would have a lesser chance of being wrong.

The result of the method was:

- to find the highest agreement of Proto Oceanic with some Minahasa languages which are now known to have no special relationship with Oceanic and
- to fail to suggest any special relationship with certain Moluccan languages which are now classified as Central-Eastern Austronesian as is Oceanic.

This was a different result than that of standard lexicostatistics and I *would* say that it worked to discourage interest in vocabulary agreement studies. I *would* say this in the sense that citations of and personal communication about Dyen (1965 and elsewhere) by archaeologists became less common and less troubled in the coming years. But then no one subsequently referred to our paper at all, at least not any time soon. So if the loss of interest in lexicostatistics on the part of Southeast Asian and Oceanic archaeologists was, in part, due to our paper, that is not obvious in the literature.

4. Other than the Western Micronesian languages of the Marianas (Chamorro) and Belau.

5. "Diagnostic" is not a term all comparative linguists would claim as their own. Not as one with much history of use, in any event. Here I use it in the sense that detailed, broad comparisons of well described languages can sometimes produce findings of such specific and multiple sharings of *innovations* in phonology, morphology and other primitive or basic aspects of the languages to allow absolute certainty about the status of some, many, most or all of the languages in a genetic tree as such trees are commonly understood by non-specialists.

6. But see Gray and Jordan (2000) for an example of mass comparisons (based on Blust [n.d.] that discerned, for instance, the link between Oceanic and other Central-Eastern Polynesian languages (but lumped Niuean with Rennellese and Samoan with Tongan when the actual groupings are Niuean with Tongan and Rennellese with Samoan).

Our most important linguistic observations occurred on page 85 where we referred to unpublished work by Paul Geraghty, Andrew Pawley and Robert Blust reported to us in personal communication the year before. That work, reported to us by Pawley, involved the discovery by those researchers of little wisps and threads of things that *do* identify uniquely shared histories in a diagnostic manner where lexicostatistics or other statistics of lexemes, or most anything else linguistic, do not. As we related, beginning on that page, it was becoming clear that Proto Oceanic was most immediately related to certain languages of Eastern Indonesia and that the place where Proto Austronesian was spoken seemed to be Taiwan.

At that time, the general work of archaeologists and linguists, especially Pawley, Green and their growing cadre of colleagues writing in the language-and-archaeology or archaeology-and-language idiom, was seeing the northern Melanesia corridor become the central focus of research on the origins of Oceanic Austronesian (Lapita) society.

There was rather less enthusiasm for the Taiwan homeland hypothesis amongst archaeologists. It was not until Bellwood's (1978) work that a second reference to the probability of a Taiwan origin for Austronesian speech emerged in the archaeological literature, the growing confidence of linguists in that theory and common publication of relevant findings notwithstanding. These remained rather timid and tentative citations for about ten years and people coming into the work today often get only as far back as Bellwood (1985) (e.g. Gibbons 2001) when looking for an early, vigorous statement about Taiwan origins for Austronesian speech by an archaeologist. They rarely seem to have any familiarity with the literature of the linguists from those decades at all, Bellwood's (1978) position or ours.

This amuses, bewilders or offends the linguists working on the problem as we have always been talking about the origins of Austronesian *speech* and have focused for three decades or more on the possibility that it was Taiwan.

REFLECTIONS ON THE PAPER

Here I review the general intellectual climate of 1975 as reflected in our paper: our general understanding or misunderstanding of who the early Austronesian speakers were, what they were doing and ultimately came to accomplish and how long it took for them to do it.

There are three aspects of the work I recall quite vividly at this point in time.

The first was probably not general but specific to Dick and myself. It involved a unilineal bias where we did not imagine that people who once had rice would abandon it. Thus the title of the paper: "On the dispersal of the Austronesian *horticulturalists*". We could only imagine that rice was adopted in the areas, roughly, where it was found in historical times, *after* the initial dispersal of Austronesian speech *beyond* those areas. I am not familiar with current thought on why Austronesian speakers abandoned rice cultivation as they breached the Wallace Line area. Whether the abandonment of rice culture, as we now understand it, had more to do with the particular kinds of landscapes in which they sited their settlements, abandonment due to the general procurement strategy of peoples at the forefront of encroachment into Oceania or other factors is not a matter with which I am familiar.

The second memory is a little more flattering to Dick and myself where we simply forged ahead without apologising for doing language-and-archaeology and *did* language-and-archaeology. The work proceeded with the individual constraints of the two disciplines and was not "wrong", in the main, even after most of three decades of further archaeological, linguistic, and, ultimately, genetic work on "the" problem.

Gibbons (2001) is one bioscientist who, along with some colleagues that she quotes, seems to think the linguists and, apparently, archaeologists have often been "wrong". But linguists and, especially, archaeologists generally have at least minimal exposure to concepts of cultural process and, as I will relate below, the vehicles of linguistic dispersal, material culture dispersal, social systems dispersal and the dispersal of human genes are human *cultures*. When the geneticists say the linguists or archaeologists are "wrong" they seem, in Gibbons' paper and her examples from other geneticists, to be talking about one thing (human genetics) when linguists are talking about another (languages) and archaeologists yet another (archaeological material cultures and the human societies which were their vehicles).

My third memory is our general concept of the time frame of the Austronesian dispersal... the total absence of data or precedent for imagining that the Austronesian dispersal in general or certain parts of the Oceanic dispersal in particular could have happened as quickly as subsequent research has demonstrated.

We now understand the dispersal of Austronesian speech beyond Taiwan as a rapid complex of events, even in its initial stages. Dick and I had imagined a lumbering, incremental dispersal driven from the rear by population growth and the slowly accumulating need for more coastal land or land in general. At this point in time it is generally understood that it was, instead, the pull of optimal, Lapita-like, environments that was drawing Austronesian speaking peoples further and further from Taiwan. So far as I recall, other writers on the subject by about 1975 were similarly disinclined to assume that the dispersal could have been so rapid.

Working backwards, we posited that the arrival of Austronesian speakers to the northern Philippines probably didn't occur later than 5500 B.C.. Some of that was due to radiocarbon dates that were later corrected or dismissed but in the main neither Dick nor I nor anyone else understood, in print at least, that the dispersal from Taiwan to the Philippines and eventually to Fiji and Western Polynesia was so quick. Or that it was, or became along the way, a result of the pull of optimal micro-environments rather than the push of populations that had either to abandon the coasts and live inland or seek new islands. By this later formula and giving some thought to the total coastlines available along the dispersal route, we wondered if the initial movement of Austronesian speech out of Formosa might not have occurred as early as 7000 B.C..

For the Fiji-Tonga-Samoa area there was the inclination of the linguists to assume that Proto Central Pacific and Proto Polynesian developed in geographically compact areas... Proto Central Pacific in Fiji and Proto Polynesian in Tonga. The archaeology at the time seemed to support a staged progression into the area: first the settlement of Fiji (and the development of Proto Central Pacific), then the settlement of Tonga (and the development of Proto Polynesian), then the settlement of Samoa (and the divergence of Proto Tongic from Proto Nuclear Polynesian)[7].

How innocent we all were. We now know that the entire area was settled very rapidly from about 1,000 B.C. (Anderson and Clark 1999; Burley 1998; Burley *et al.* 1999; Clark and Anderson 2001). Proto Central Pacific (a possible immediate common ancestor of Fijian and Polynesian) was thought to have developed during a pause in Fiji before people moved on to Western Polynesia. Now the Central Pacific hypothesis is weaker as more details are analysed, and it seems certainly to have been spoken through Fiji and Western Polynesia as a whole, if innovations ever spread over such vast distances, rather than to have been due to any pause and containment on the Fijis. However, we now understand that Proto Polynesian developed through Western Polynesia as a whole (Pawley 1996) where in 1975 we had not imagined that a single language could be maintained over such vast distances.

Few historically known Island societies maintained a single language over such a large area but it was towards them (e.g. the Marshalls, Kiribati) that we might have been looking. It would only later be apparent how slowly the early populations of Western Polynesian grew to "fill" the environment and Dick and I were still working with an implicit model that had the islands reaching substantial population densities relatively soon after their settlement. Or at least we did not imagine that ratios of visiting/migrating people to sedentary people around Western Polynesia would be high enough to sustain a single language through the area for such a long period of time (Marck 2000:Chapter 9; Pawley 1996).

CHANGES IN PARADIGMS SINCE 1975

Our article ended with the statement that:

> Hopefully, as the nature of the Oceanic dispersal in the Pacific becomes clearer, the value of the Pacific for culture change studies will be enhanced. With the definition of genetic connections, intrusive influences, and time depths of differentiation, the cultures of the Pacific will take on a new dimension in their utility for the study of culture change (Shutler and Marck 1975:106-107).

That has now happened in many ways. By far the most significant developments, to me personally, have been the understanding of demographics, geography and language differentiation around Fiji and Western Polynesia in the first millennium B.C. The genetics are relatively undeveloped but of some use.

Both archaeologists and historical (comparative) linguists have an inclination towards what the geologists call "uniformitarianism". In geology, this is the idea that, in the main, the processes that occurred in prehistory are to be observed in geological processes which are ongoing or can be inferred from processes to be observed in recorded history. In archaeology and historical (comparative) linguistics it has been the idea that, in the main, sociolinguistic and other cultural processes impacting languages can normally be understood in terms of such processes as they have been/are observed in historic cultures. Christy (1983) has worked towards formalising linguistic notions of the term and Labov (e.g. 1994(1):21-25) has noted the need for such a term and some of the history of its use in historical (comparative) linguistics.

In geology there are exceptions to the notion that most ancient geological processes remain current in our own

7. Gibbons (2001) reflects this dated point of view and she was not apparently in touch with the archaeology and linguistics of the 1980s and 1990s when she wrote her (2001) paper.

era: catastrophic events such as the Earth being hit by meteors the size of small moons, breaching of great lakes or seas and the pouring of their contents into the Mississippi River or the Black Sea or whatever. But eventually the evidence becomes overwhelming and the events concerned are not so different than less dramatic historic examples.

But in the instance of how we now understand the first millennium of settlement in Fiji and Western Polynesia, the linguists and archaeologists have the wonderful excuse that no historic colonisation of continents or islands really gave us analogies for what, precisely, the general outline of early Central Pacific prehistory now seems to be.

In the first instance, the archaeological evidence is now clear: the area was settled lightly and as a whole soon after 1000 B.C. (Anderson and Clark 1999; Burley *et al.* 1999; Clark and Anderson 2001). Then a number of things happened that were quite remarkable and not subject to uniformitarianism analogy in either the archaeological or linguistic instances. As mentioned above, totally remarkable to the linguists is the notion that the massive innovations of Proto Polynesian occurred over Western Polynesia as a whole (Pawley 1996). Clearly, Proto Polynesian was a language that evolved around Western Polynesia as a whole and disintegrated first into northern (Samoan centred) and southern (Tongan centred) dialects and then into distinct languages (Pawley 1996) as the ratio of visiting/migrating people between north and south to total population north and south declined below some critical level (Marck 2000:234).

Notable in Western Polynesia was the very long time population took to grow (Kirch 1984) to a density approaching what was observed at the time of European contact. It was not until the end of the first millennium B.C. that local populations became so large that the ratio of migrants to residents finally declined sufficiently to fail to sustain a single language. The earliest settlement of Western Polynesia was clearly accomplished by a small number of people whose population grew naturally[8] rather than through continuing immigration. Either the early Polynesians purposely kept out anything but a trickle of new immigrants or immigration from the west was insubstantial due to the immediate neighbours (Fiji, Vanuatu and the others) being in a similar state of early population growth (and low population density) (Marck 2000:233).

I would never have imagined, in 1975, that our knowledge of the situation would be so detailed by now that I could conclude, about early Western Polynesia, that we should give some thought to the particular feedback processes of ethnicity (rather clan-based in 1000 B.C. but more island-based by B.C.-A.D.) and linguistic identity sustaining each other:

> This is of moment as the motives of consanguineous kin for accepting linguistic innovations are ontologically different than accepting those of affinal kin or other social groups. In the latter instances one accepts innovations to affirm the equivalence of groups. In the former one accepts innovations to affirm membership in one's own. I suggest that we must posit substantial circular or stream migration internal to Western Polynesia during the Pause which would have been organised around consanguineous affiliations. Differences in speech that arose over time and space would have been adopted *within* those groups *across* various islands and *between* those groups *upon* any particular island. We might also imagine affinal or more general social motives for internal migration as these are readily apparent from ethnographic analogy but the consanguineous motives are not (Marck 2000:233).

Work on kin terms in prehistory has taken me into Bantu studies in recent years and there I have found the most remarkable parallel development in theory and methods of archaeological and linguistic synthesis in prehistory. To become instantly familiar with the researchers and their framework, one can most conveniently start with the combined 1994-1995 volume of *Azania: Journal of the British Institute in Eastern Africa*. None of our work in the Pacific is quoted, so far as I have noticed, and, so far as I have noticed, those of us in Austronesian and Pacific studies have never quoted any or much of theirs. Yet they are so clearly operating with the same constraints as the archaeology-and-linguistics researchers in the Pacific. This I might phrase something like:

> Let the archaeologists do good archaeology and let the linguists do good linguistics. If certain parallels in paths of evidence and inference become too striking to ignore and are clearly the result of prehistoric archaeological cultures and speakers of prehistoric languages being, to greater or lesser degrees, one in the same, don't ignore the obvious because your Anthro 101 teacher said you're supposed to.

Kirch and Green (2001:Chapters 1 and 2) consider the history of such ideas in some details. In that work they name and discuss the "triangulation method". It should

8. A demographic term referring to growth (or loss) before factoring the effects of immigration or emigration.

make our inter-disciplinary methods clearer to some people and provide the catch-phrase that has always been lacking.

"THE PROBLEM" AS IT IS UNDERSTOOD AND MISUNDERSTOOD TODAY

Oppenheimer and Richards (2001) have recently published a trenchant overview of language-and-archaeology theories about the dispersal of Austronesian speaking peoples, reflecting upon the significance of recent developments in human genetics in light of those ideas. In a more cursory piece, Gibbons (2001) has reflected on the "origins" of the "Polynesians" from that same general perspective. However in certain instances, where Gibbons found the linguists and archaeologists to be "wrong", it would appear rather to be a case of a lack of understanding of what they/we were talking about in the first place. The more disciplined work of Oppenheimer and Richards also has a few related sorts of errors.

Oppenheimer and Richards is a mainly welcomed review of archaeological and linguistic models of Austronesian dispersal but finds a straw man amongst those models and, I must say, God save the Bantuists from the geneticists if Gibbons' (2001) discussion of "the" Austronesian puzzle and her quotes from other geneticists are any indication of what the geneticists, in general, read into our linguistic and archaeological work. Gibbons and some of her colleagues that she quotes don't seem to understand that linguists, when speaking as linguists, are speaking of language and not archaeological cultures or genetics. Or that archaeologists, when speaking as archaeologists, are speaking of archaeological cultures and not linguistics or genetics.

Oppenheimer, Richards and Gibbons and some of Gibbons' colleagues seem to believe that Pacific/Austronesian linguists and archaeologists assume no mixing of Austronesian speakers' or the Lapita complex makers' genes with "aboriginal" Melanesians unless they/we say so specifically, preferably vociferously, and there Gibbons thinks she has found a champion in the archaeologist John Terrell.

I shall first quote from Oppenheimer and Richards' article's abstract:

> This model... proposes a common origin for all Austronesian-speaking populations... from south China/Taiwan around 6,000 years ago. However, it is becoming clear that there is, in fact, little supporting evidence in favour of this view (Oppenheimer and Richards 2001:157).

And now from Gibbons:

> Although this ["the Polynesians"... "out of Taiwan" model - JCM] model was often touted as an interdisciplinary synthesis, in fact it is no favorite of archaeologists, many of whom have for years preferred a more "integrated" model, with at least some mixing between Melanesians and Austronesian speakers from Southeast Asia... (Gibbons 2001:1735).

Oppenheimer, Richards and Gibbons all assume a mixed idiom on the part of linguists or even linguists and archaeologist where there is and has been none. The linguists have never claimed there was an Austronesian "people", other than in the sense of people speaking Proto Austronesian and descendant languages. And no archaeologist has ever claimed that the Lapita complex was made by any narrow "racial" group. The archaeologists have been convinced for some decades, as have the linguists, that Lapita sites are more or less perfectly associated with the entry and dispersal of Oceanic Austronesian speech. But the "race" question was dormant for decades, due to lack of relevant science, and not normally addressed.

Linguists have "failed" to note any mixing of Proto Oceanic with the older Melanesian languages (because we can find none). But I have been doing this kind of work for over 25 years and our "failure" to specify that we have always assumed a fluid, dynamic situation with respect to the population genetics situation is a result of:

1. Not setting out to study such things in the first place.
2. Not, until recently, being able to ask the geneticists what *they* were finding (as they were not capable of such analysis until recently).
3. Not wanting to mix idioms in the absence of relevant data.

It is not a result of the kinds of assumptions Oppenheimer, Richards and Gibbons believe we have made, their conclusions about our assumptions are unwarranted and they are fighting straw men, often with the help of their liberal references to John Terrell who has made a bit of a career fighting the same straw man. It's a false polemic created by Terrell for unknown purposes. But Oppenheimer, Richards and Gibbons have seized upon it as a real polemic in the Pacific prehistory disciplines. They would all be better off ignoring Terrell who, as one linguist said, "Doesn't understand and doesn't *want* to understand", and going to the (commonly unspecified) sources Terrell claims to be so gallantly fighting.

Generally, I would say that the archaeologists and linguists involved worked with the notion that the early Austronesian speakers in Oceania who participated in the Lapita material/procurement complex were similar to Pacific Islanders today with respect to their systems for and frequency of incorporating "outsiders" in matrimony or other liaisons involving offspring.

Those systems are fluid and dynamic today and would presumably have been so in the past. It is obvious to anyone who has read linguistic and archaeological works on Pacific and Insular Southeast Asian prehistory or conversed casually or specifically with the researchers involved that this fluidity has *always* been their model. Oppenheimer, Richards and Gibbons, for reasons that are never made clear (other than references to Terrell), assume that linguists and archaeologists have had certain opinions about the human genetic situation that we, in fact, have not. If they are aware of any of these purported offending opinions in print, they gave no examples. They simply, plainly, unfortunately spend a great deal of time wrestling Terrell's straw man.

We are not working with a single problem but a complex of interrelated problems. There is the complex of linguistic problems: subgrouping and its implications, reconstruction of cultural vocabularies and those implications, borrowings and their implications, and so on. There is the complex of archaeological problems: continuities and discontinuities in material cultures, dating and its refinements, the siting of the settlements in environments, the sourcing of traded materials and the continuously emerging area of "archaeological science", and so on.

In addition to solving subgrouping problems, the linguists have made enormous contributions in showing the general socio-economic complex of seafaring, social, material and procurement cultures apparent at the Proto Austronesian or, more commonly, the Proto Malayo-Polynesian level.

It is indisputable that speakers of daughters of Proto Austronesian spread to the far places where we find Austronesian languages today. The languages were spread by *people* and that much is indisputable.

But this does not, by itself, tell us whether there was also a great deal of procurement, social and material continuity through these speech communities through time. Zorc (1994) sets out our general knowledge of the type of vocabulary with which we are concerned. Through that and similar works (e.g. Blust 1995; Ross *et al.* 1998) it becomes apparent that the basics of Malayo-Polynesian life and economics as we have observed them amongst Malayo-Polynesian speakers in historic times had their beginnings and much more in the socio-economic systems of the Proto Malayo-Polynesian and Proto Oceanic speakers themselves.

Then the archaeologists tell a similar story in the massive similarities of the general material culture and loci of siting in environments by early "Lapita" people through Melanesia and into Western Polynesia. Similarly, local variants of sites with red-slipped pottery and a commonly associated non-pottery material complex are now becoming better understood in Insular Southeast Asia, the Mariana Islands and Taiwan.

The main criticisms of our (the archaeology-and-language) model tended to be unpublished and come from botanists who kept pointing out to us that many of our "Proto Malayo-Polynesian[9]" plants and animals originated in New Guinea rather than the purported Proto Malayo-Polynesian homeland... which we take to be the Philippines. My private comments to the botanists through the 1970s and 1980s were to the effect that people were obviously getting around Wallacea prior to the arrival of Austronesian (speaking) seafarers, so perhaps the plants and animals concerned were already up to the Philippines due to the agencies of earlier but less effective seafaring systems. Now I would add that the Austronesians seem to have dispersed so rapidly through the Philippines and Eastern Indonesia that *they* may have brought some of the plants and animals up from the New Guinea area and into the Proto Malayo-Polynesian speech community heartland before the language disintegrated into highly distinct local and regional varieties.

Neither the archaeologists nor linguists have ever said much about "race" or human genetics. The abundance of phenotypic variety was observed with a shrug. The observation in conversation amongst mentors and colleagues was commonly that Islanders tend to inter-marry rather freely and that such a fluid genetic situation probably existed from the beginning of the Austronesian dispersal/expansion. Genetics wasn't our area of expertise and we simply awaited further developments in that science.

Now that the "race" question can be examined in some detail scientifically many of the kinds of things Oppenheimer, Richards and Gibbons relate are immensely welcome and informative.

With respect to those genetic characteristics inherited only from women, Gibbons relates "When geneticists first studied the maternally inherited DNA from the mitochondria... in Polynesian, Melanesians and Southeast Asians... Researchers found that about 90% to 95% of Polynesians have inherited a deletion seen in Southeast

9. The language ancestral to all Austronesian languages of today other than those of Taiwan.

Asians, including Taiwanese, but rarely in Melanesians" (2001:1736).

With respect to those genetic characteristics inherited only amongst men, the Y chromosome, Gibbons relates that *some* genetic markers in Polynesia *do* seem to originate in or near Taiwan. In what I take to be a continuation of a Y-chromosome discussion, certain common (male) Polynesian markers are said to be known most commonly from Near Oceania, to a lesser extent in Southeast Asia and hardly at all in China or Taiwan. Others which are found in over half the Polynesian men studied are found only in Melanesian and eastern Indonesian men. Various studies were involved in Gibbon's report and one also found a particular mutation known mainly from "the southern Chinese and other East Asians, including Taiwanese".

To a social anthropologist, the obvious suggestion given this information would be that Austronesian speaking, Lapita making societies were matrilineal as they moved into or beyond the Proto Oceanic heartland and on to Polynesia, as Hage (1999) has recently posited as a reasonable possibility given the Proto Oceanic kin term system. It is, after all, matrilineal societies that recruit outside men more than any of the vice-versas.

Overall the *information* in Oppenheimer, Richards and especially Gibbons' articles is wonderful as it, in general, finds: traces of mainland or Taiwan genes and others picked up along the way. What else would anyone familiar with Austronesian speaking societies expect? Far from "solving" a controversy or "the" problem, Oppenheimer, Richards and Gibbons have simply confirmed the vague (unpublished) expectations of most archaeologists and linguists. As a bioscientist Gibbons seems to assume that anyone in their right mind would have an opinion on such matters. Well. Curiosity, yes. Opinions, no.

And Gibbons in particular doesn't seem to have a very strong sense of human communities and how they descend through time. Consider the following passage:

> Although geneticists and archaeologists now agree on at least some degree of mixing, it remains a mystery where the seafarers initially set out from. Based on the Y data, it's not Taiwan. "We have trashed this idea for a Taiwan homeland completely," says Li Jin, a population geneticist at the University of Texas Health Science Center in Houston (Gibbons 2001:1737).

This profoundly misunderstands "the" problem. The Austronesian *languages* came from Taiwan. Their vehicles were human *cultures* and their seafaring and other technologies. Language and material culture transformed over time because that's what they always do. The genetics transformed over time because that is the nature of (most) human communities, especially those that migrate[10].

Shutler and Marck (1975) stood the test of time in a reasonable manner because it did not mix disciplinary idioms and remained true to the two (archaeology and linguistics) that it employed. Whatever that paper did or did not add to the science of the situation, the geneticists shed less light than they might imagine on "the" so-called "debate" with the level of (mis)understanding of archaeology-and-linguistics occasionally apparent in Oppenheimer and Richards (2001) and replete in Gibbons (2001).

So. God speed, Dick. Happy 80th birthday and many happy returns!!!

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