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## ARCHAEOLOGY IN NEW ZEALAND



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## REVIEWS

**Patrick Vinton Kirch and Roger C. Green.** *Hawaiki, Ancestral Polynesia: An Essay in Historical Anthropology.* Cambridge: Cambridge University Press, 2001. 375 pp + xvii. Price UK £47.50 (cloth), £9.95 (paperback).

Academia is full of odd situations. Here is a book by two of the most eminent Pacific prehistorians, who bring together the evidence of archaeology, historical linguistics and comparative ethnography and put forward by far the most comprehensive and coherent reconstruction of early Polynesian culture yet to appear. One might reasonably expect *Hawaiki* to be hailed as a landmark. Yet I suspect that in the short run few Pacific specialists will read the whole book carefully or manage to grasp the extent of its achievement. My suspicions are strengthened by the two published reviews of the book I have seen. More later on the possible reasons for this response. First, the book.

Kirch and Green seek to integrate “a study in method with a substantive data-rich case: the reconstruction of the world of the Ancestral Polynesian homeland” (p.8). The meat of the book, the case study, is Part II. Each of its six chapters deals with a domain of what they call ‘Ancestral Polynesian culture and society’: principally, the physical environment, subsistence, food preparation and cuisine, other aspects of technology, social and political organisation, and cosmology, calendars and ritual practices.

### **Part I**

Part I, *The phylogenetic model: theory and method* begins by surveying the chequered history of historical anthropology and the ‘phylogenetic model’ of culture history. This model speaks of genetically related cultures in much the same way as we speak of related languages and species, i.e. as distinctive, bounded systems or breeding populations that have kept their integrity through the generations, while undergoing change, but which are subject to fission when

some members of the community or breeding population become isolated from the rest.

K & G argue, in chapters 2 and 3, that the phylogenetic model fits the circumstances of Polynesia very well. They ask "How is it possible to move beyond the strictly material evidence of potsherds, adzes, and shell fish hooks, postmolds and earth ovens?" Their answer is by 'triangulation', using as reference points evidence independently derived from each of the key disciplines: archaeology, ethnography, linguistics and, to a lesser extent, biology. Although K & G say (p.42) that they "seek to develop a triangulation method", this is hardly necessary. The principles of the method – to compare the testimonies of different disciplines – are well established in historical anthropology. Indeed, the authors note that many previous studies in historical anthropology have treated the diverse cultures of the Polynesian Triangle as cognate systems stemming from a common base, with local adaptations and mutations. What is distinctive about K & G's enterprise is that it is a much larger scale application of the triangulation method than any of its predecessors and it draws on richer evidence.

For reconstructing early Polynesian culture, the two critical disciplines are archaeology and historical linguistics. Biology (whether of humans, rats or plants) plays only a minor role, chiefly as a source of evidence regarding interaction between/isolation of regions. And although comparative ethnography is a vital source of historically attested data it is weak in methods for recovering the past. Whereas archaeology can locate its cultural assemblages precisely in space and (more or less precisely) in time, and historical linguistics has rigorous methods for determining the relative chronology of innovations and for distinguishing borrowings from shared inheritance, comparative ethnography can do none of these things reliably.

K & G regard the precontact cultures of Fiji and Polynesia as sharing a common source, namely, the Far Eastern variant of the Lapita complex. Far Eastern Lapita appeared in Fiji and Tonga around 3000 years ago and by about 2800-2700 BP had spread to Samoa, Futuna and Niuatoputapu. By about 500 BC (western Fiji) to 200 BC (eastern Fiji) the material culture of Fiji started to diverge from that of the Tonga-Samoa region. After 200 BC, according to K & G, there was a distinctive culture common to the Tonga-Samoa region, including the Niuas, Futuna and 'Uvea. This was Ancestral Polynesian culture.

K & G suggest that between 2200 and 1900 BP Polynesian speakers settled islands and island groups outside of core Western Polynesia, first the Tokelaus

and Tuvalu and Central Eastern Polynesia, and a bit later the Polynesian Outliers. They allow that the settlement of C.E. Polynesia may have been as late as 1600 BP. I note a swing of opinion among Pacific archaeologists towards the later date, and indeed some contemplate an even later date. A first settlement of Eastern Polynesia at 1700-1500 BP should not bother linguists. It is entirely consistent with the huge number of innovations shared by all subgroups of Polynesian exclusively of all other members of the Austronesian family, which points to a period of roughly 1000 years of more or less unified development between the breakup of Central Pacific and the breakup of Proto Polynesian (PPn).

Linguists agree the first branching in Polynesian was between Tongic (ancestral to Tongan and Niuean) and Nuclear Polynesian (ancestral to all the 30 or so remaining Polynesian languages. The primary divisions within Nuclear Polynesian are less clear but it is generally agreed that Samoan, East Futunan (Hoorn Is.) and East 'Uvean (Wallis is.) belong to separate primary branches. It is universally agreed that there is an Eastern Polynesian subgroup. There is some evidence (Marck 2000) that its immediate affiliations are with Samoan, Tuvalu and Tokelauan, rather than with E. Futunan or E. 'Uvean. Some of the Outliers, however, subgroup with Futunan, others with Samoan.

One problem with the concepts of Ancestral Polynesian culture and Proto Polynesian is gauging the extent of regional variation within core West Polynesia at the time Polynesian speakers began colonising other regions of Oceania. K & G say that "[T]here never was a single, unified Ancestral Polynesian society, rather a network of related communities dispersed over several islands and archipelagos" (p.129). Similarly, the first few centuries of the divergence between Tongic and Nuclear Polynesian was a dialectal one, between a northern and a southern dialect of PPn. What finally severed the dialect chain was the settlement of Central East Polynesia and other areas beyond core West Polynesia, such as the Tokelaus, Tuvalu and the Outliers.

Logically, there is no reason why the case for an Ancestral Polynesian culture, and for subsequent cultural continuity could not be based purely on archaeological evidence. Indeed, K & G try to define Ancestral Polynesian culture in discrete archaeological terms but acknowledge serious limitations in the evidence. More than 80 percent of pre-contact Polynesian material culture types were perishable. Between 100 and 300 types of objects (depending on the environment) can be expected in any comprehensive ethnographic account of traditional material culture on a Polynesian island but only about 20 to 40 such types are durable enough to be routinely recoverable by archaeology. And when

it comes to the intangible parts of a prehistoric people's way of life – their systems of kinship, status and rank, cosmology etc. – archaeology usually has even less to say.

By comparison, very extensive terminologies for both material and non-material culture are reconstructable for Proto Polynesian (PPn). For example, Ross Clark's unpublished English to PPn thesaurus lists PPn names for over 100 kinds of material objects, of which only 20 are represented in the archaeological record for Polynesia. K & G cite around 1000 PPn lexical reconstructions naming cultural entities. It follows that, for the most part, Ancestral Polynesian culture must be defined as the culture of the PPn speech community. While this dominance of the linguistic evidence may make archaeologists uneasy, it does allow degree of cultural continuity to be measured precisely in terms of the continuity of terminologies that name significant cultural concepts.

The most important single data source for K & G's project is the POLLEX project initiated by Bruce Biggs in the mid-60s. This is a comparative dictionary consisting of cognate sets from the Polynesian languages (and sometimes from other Austronesian languages) with associated reconstructions, i.e. proto-forms with glosses. The 1998 version contains over 2300 lexical reconstructions for PPn, and another 1300 or so attributed to lower order stages such as Proto Nuclear Polynesian and Proto East Central Polynesian. In addition, K & G can draw on a large body of reconstructions for higher order Austronesian stages. Over the last 10 or 20 years there has been a good deal of work on the culture of language community who spoke Proto Oceanic, an interstage ancestral to almost all the Austronesian languages of the Pacific Islands and strongly associated with Far Western Lapita. The largest database of all is Robert Blust's massive comparative dictionary (electronically available) which gives cognate sets for Proto Austronesian, and for lower order stages such as Proto Malayo-Polynesian, Proto Oceanic, etc.

The POLLEX database has long cried out for extended interpretive treatment, one that pays more attention to the meanings of etyma and to sets of terms than POLLEX does. There have been many small scale studies drawing on POLLEX, but none on this scale.

One thing lacking in Part I is a clear account of the reconstructive method of historical linguistics. This omission is unfortunate because persuading readers of the credibility of the findings in Part II surely rests largely on persuading them that the linguistic evidence is solid. Although K & G provide an extensive glossary of technical terms and a good discussion of methods of semantic

reconstructions these materials presuppose knowledge of more fundamental assumptions and inferential procedures in the reconstructive method. Readers may reasonably ask, how do linguists arrive at inferences about the precise forms and meanings of words attributed to prehistoric proto-languages? How do they distinguish layers of vocabulary that are directly inherited vocabulary from those that are borrowings? How do they determine relative chronologies for linguistic innovations?

The reconstructive power of historical linguistics stems from certain peculiar characteristics of language that makes it possible to trace continuities (for a limited time span) in particular components, much as geneticists do with genetic systems. At the same time these characteristics yield diagnostics for identifying layers of lexicon added to the language at different points in time. The most important characteristics are:

1. In 99 percent of cases form-meaning pairings in word roots are *arbitrary* (not functionally motivated). There is nothing about dogs that require they be named *dog*, rather than *chien*, *hund*, *kurii*, or any other combination of sounds. Except for sound-symbolic words, words are composed of phonemes (minimal sound units, such as *p*, *t*, *k*) that have no meaning in themselves. This means that lexical resemblances in ordinary words must be due to one of three factors: chance (amounting to fewer than four percent of cases), borrowing or shared inheritance.
2. Sound change tends strongly to be regular. If the sound *t* changes to *s* before *-i* in one word it will generally change in all words that meet that condition, and if *s* changes to *h* before *-a* in one word it will generally change in all such cases, and so on. Thus, related words (cognates) will show regular sound correspondences. Tagalog *hipag* 'brother in law', Malay *ipar* 'related by marriage', Sa'a *ihe* 'brother in law', W. Fijian *iva* 'son in law', can be shown to derive by regular sound change from PMP *\*hipaR*. Similarly, Tagalog *layag*, Malay *layar*, Motu *lara*, Fijian *laca*, Rarotongan *raa*, all meaning 'sail', derive regularly from PMP *\*layaR* 'sail'. Words that are accidentally similar, or borrowed, will in most cases violate the sound correspondences, and thus be identifiable.
3. From time to time "sporadic" irregular sound changes occur, affecting just one or two word forms, and motivated by well-known processes of assimilation or paradigm levelling. These mutations are valuable clues in subgrouping. Principles 4 and 5 make it possible to reconstruct the sequence, and some of the phonetic details of sound changes.
4. Certain directions of sound change are natural (attested), others are unnatural (not attested).

5. Once languages have separated any sound changes they undergo are irreversible.
6. Complex morphological paradigms (such as pronoun systems together with the specific forms) are never borrowed in their entirety.

Given that there was continuing contact across the Tonga-Samoa area after first settlement, how can one distinguish cultural inheritance from borrowing? Early linguistic exchanges between Tonga and Samoa are readily detectable if (1) they occurred after the diagnostic sound changes separating Tongic and Nuclear Polynesian: principally Tongic merger of \*s and \*h and loss of \*r, Nuclear Polynesian merger of \*l and \*r, and loss of \*h in most contexts, and certain sporadic sound changes, and (2) if the words concerned show diagnostic sounds. This leaves a sizeable residue of potentially undetectable borrowing. Marck (2000) provides the most systematic (but still partial) review of the evidence for borrowing in the core West Polynesia area. He identifies 99 words in POLLEX shared by Tongan and Samoan, East Uvean or East Futunan exclusively of Eastern Polynesian and the Outliers. That is a fairly small number and he gives statistical arguments to show that a good proportion of the 99 must be retentions from PPn.

POLLEX adopts other safeguards to deal with diffusion. If a cognate set is represented in two primary subgroups a reconstruction would normally be attributable to the immediately ancestral stage. So a cognate set found in both Tongan and Samoan should, in principle, require a PPn reconstruction. However, in POLLEX, a PPn reconstruction is made only if a cognate set is represented (a) in Tongic and (b) in a Nuclear Polynesian language outside of Western Polynesia, and/or (c) in a non-Polynesian language. This safeguarding practice means that, effectively, the breakup of PPn is equated with the divergence of Eastern Polynesian from the Tongic and Nuclear Polynesian languages of West Polynesia. The point is worth making that retentions from Proto Oceanic are the most solid of all PPn lexical reconstructions, and play a crucial role in the culture historical inferences.

## Part II

Ch. 4 deals with the physical environment and contains no real surprises. The fact that the location of the Polynesian dispersal centre is known makes it a good test of the reliability of PPn lexical reconstructions as diagnostics. There are PPn reconstructions that show that PPn speakers were familiar with high islands and a range of plants and animals including megapodes, snakes, palolo worms, and owls, all consistent with the accepted view that the language was spoken in the Fiji-Tonga-Samoa area. K & G cite 65 PPn reconstructions for the inanimate

environment. They refer to extensive published terminologies for fish (115 taxa) and birds and cite terminologies for molluscs (12 terms) and crustaceans (12) and other marine life.

The megapode is a nice case of triangulation at work. At first Western contact this bird was extinct everywhere in Polynesia and Fiji except Niufo'ou. However, Niufo'ouan (a Nuclear Pn language) and Tongan retained the Proto Oceanic generic for megapodes, \**malau*, indicating that when Oceanic speakers arrived in the Fiji-West Polynesia area, megapodes were probably more widely present. And indeed, archaeology has produced megapode bones from Fiji, Manu'a, Ha'apai and 'Eua.

Ch. 5 deals with food production and gathering. Linguistics tells rather little about animal husbandry. It gives lexical reconstructions for pig, chicken and dog but tells nothing about their economic importance. Archaeology indicates that these animals were probably minor elements of the diet in PPn times. However, linguistics "greatly extends our picture of Ancestral Polynesian horticulture" (p.128), attesting two prevalent subsystems: shifting cultivation and arboriculture. The strandlooper hypothesis is put to rest. There are 27 PPn terms for cultivated/domesticated plants which have no wild relatives in Polynesia and whose meanings are constant in virtually all daughter languages. Many have Polynesian names that continue Proto Oceanic and indeed Proto Malayo-Polynesian names. These, then, were crops brought by Lapita peoples into NW Melanesia and then into the central Pacific at or very close to the time of first settlement.

Sixteen terms associated with horticultural technology are cited, some more strongly attested than others. The pattern of terms is consistent with swidden cultivation, using dibble sticks. The authors conjecture that on the larger volcanic islands with ample fertile land the emphasis in PPn times, as in historical times, was on shifting agriculture. On coral atolls and small islands aboriculture was more important. No PPn terms for irrigation are reconstructable. There seem to be two systems of terms, one western Polynesian-Fijian the other Eastern Polynesian, and they view these as post-PPn developments.

Ch. 6 deals with cooking and cuisine. Linguistic evidence is abundant: it includes 19 PPn terms for food preparation, 19 for cooking technology, about 15 for kinds of containers, and 8 terms for the pudding complex.

The long chapter on material culture (ch. 7) contains many instances of archaeology, linguistics and comparative ethnography providing complementary or corroborative evidence. Ethnographic studies show that large high island Polynesian cultures had many more manufactured objects than atoll societies, with small high islands scoring in the middle. But the proportion of perishable items remains about the same in each.

Archaeology offers rich data when it comes to industrial tools but the typology of stone and bone tools has always presented problems. Ethnographers and linguists arrived too late to record all the functions and named taxa of Polynesian industrial tools. K & G posit a fairly varied Ancestral Polynesian adze kit, and discuss regional variations in types, associated with crossing the Andesite line. Several PPn terms for cutting and grinding tools are reconstructable but for adzes and axes only *\*toki*, which they think was generic for adze- or axe-heads and hafted tools and *\*mata(q)u*, glossed 'axe' in POLLEX but which they argue more likely referred to a tattooing chisel or comb.

There is a lengthy section on ceramic, wooden and woven containers. The complex typology of ceramics in Western Polynesia in the late 1st millennium BC is reviewed, including variation between Samoa and other islands. The range of archaeologically-attested ceramic vessel types is not reflected in the linguistic record, where only the generic *\*kulo* 'earthenware vessel' is a moderately secure PPn reconstruction. K & G also posit PPn *\*saka* 'water jar with handle' and *\*paa* 'bowl or plate, ceramic or wooden'. The evidence for *\*saka* is very shaky while *\*paa* '(wooden) plate or bowl' is well attested but the 'ceramic' extension is an educated guess.

No fewer than 33 terms are reconstructable for the bark cloth complex, and a fair number of terms for clothing and bodily decoration, weapons, games and sports, musical instruments, canoes, and cordage. K & G cite 28 PPn terms for household items and related items, 10 terms to do with warfare, nine for games and four for musical instruments. They list 27 PPn terms for to do with outrigger canoes and cordage. On the question of the chronology of the double-hulled canoe, there is a firm reconstruction for PPn but not for earlier stages.

Ethnographic accounts show that very similar methods of capturing sea creatures are used in most Pn societies. Were any of the major methods post-PPn diffusions? That seems unlikely on various grounds. Twenty six PPn terms naming various strategies and gear for obtaining seafood are presented. Many continue Proto Oceanic etyma, e.g. the terms for reef foraging, netting, poisoning, trolling, angling, and torch fishing. The conclusion must be that these

are shared retentions of old strategies. (I can add one more reconstruction: *\*tautahi*, which besides its sense of 'mariner, fisherman' was used as a verb 'to go fishing (in a boat, esp. trolling)', and as a noun 'fishing trip': Thus Samoan *tautai a'e* 'return from a fishing trip', Tongan *toutai* 'fish, go fishing', *toutai-i* 'steer or pilot a fishing boat', n. 'fishing', Rarotongan *tautai* 'to fish', n. 'fishing'.)

The terms do not indicate which fishing methods were most important. Netting and trolling are only weakly indicated in the archaeological assemblages. Molluscs, inshore and reef fish and turtle predominate, mostly types taken by netting and spearing or poisoning, ethnographically. Pelagic fish were a minor component in the diet.

Archaeological evidence for angling and trolling gear is sparse. There are angling hooks only from Samoa and Niuaotupou, and only one possible lure shank found in sites in West Polynesia. All that linguistics has contributed here are the terms *\*paa* 'trolling hook, lure' and *\*mataqu* 'fishhook'. (Here K & G cite *\*paa* as continuing Proto Oceanic *\*paa*, but the latter should be *\*bayan* (Ross et al. 1998).) Was the PPn *\*paa* a one-piece or two-piece lure? The two-piece lure, with shank of pearlshell and separate point of other (non-pearl) shell or bone material was found in early Eastern Polynesian assemblages and was ethnographically known throughout Polynesia. However, evidence from older Lapita sites outside of Polynesia supports the conclusion that one-piece lures made from large Trochus shells are an old type that was continued in PPn but subsequently replaced.

In chapters 8 and 9 the authors tackle Ancestral Polynesian social structure, beliefs and ritual practices at some length (77 pages). They warn that these chapters are "not for the faint-hearted" among archaeologists, as most of the time the only lines of evidence here are linguistic and ethnographic. Some of their syntheses here are very ambitious and should provoke debate. But their arguments are at least stimulating and generally supported by a wider range of evidence than those put forward in the extensive earlier literature on these topics.

K & G's approach to social organisation focuses on the status of dwelling houses and associated buildings and spaces as well as on 20 PPn terms for social statuses and 24 kinship terms. They discuss linguistically-indicated changes in architectural forms from Proto Oceanic to PPn society. There is a detailed discussion of the roles of chiefs and priests. Building on comparative work begun by others, K & G propose that PPn had named descent groups, called

*\*kainanga*, controlling rights to land, with an *\*qariki* ‘priest chief’ as the head. They also posit named residential groups, *\*kaainga*, associated with a specific house site and estate, and (less securely) *\*saqa + X*, as a compound term specifying social group under the influence or control of X, a person of rank. They note that *\*saqa* ‘social group’ occurs only in Tonga-Samoa and Outliers, and so could be a diffusion postdating PPn. They note the uncertain place of Ppn *\*sau* ‘chief, ruler’ in the system of leadership terms and entertain the possibility that *\*sau* denoted the ranking or most senior of a set of related *\*qariki*.

There are a number of minor errors in this chapter. For example, Anuta *manuu* is not the Anutan reflex of *\*mana* (p. 244). A false connection is made between *tupuna*, *tupunga* ‘ancestor’ and *tupu* ‘grow’ (p.244). A number of Rotuman “cognates” are actually loans from Polynesian sources, e.g. Rotuman *kainaga*, cited as a cognate of PPn *\*kainanga* (pp.211-2) is plainly a Polynesian loan because it shows *k* for expected glottal stop. However, the errors make no difference to the case for the PPn reconstructions.

Chapter 9 discusses cosmology, religion, ritual and calendars. They go beyond the famous trio, *\*mana*, *\*tapu* and *\*noa* to look at other components of the sacred and supernatural world. They review Marck’s recent study of the first order anthropomorphic gods of Polynesia. They discuss rituals of life, death and growth, and give a reasonably complete set of reconstructions for the PPn lunar calendar and other seasonal markers and their associations with ritual and subsistence practices.

### Conclusion

As you would expect in an ambitious work of this scale, there are plenty of things in *Hawaiki* that one might take issue with. But the book is an intellectual achievement that is both formidable and useful. There are some domains where the reconstructions break substantial new ground. However, its achievement is not so much in proposing a novel view of early Polynesian culture and society – the general outlines have been clear for a long time – as in giving a remarkably full set of hypotheses, which are laid out in detail. Their more solid reconstructions greatly extend the baselines for the study of adaptive and regional change in Polynesian cultures. Their more speculative hypotheses offer a challenge to those who would refute or refine them.

I turn now to the question of whether specialist readers will appreciate the book’s achievements and if not, why. Some negative indications come from a pair of reviews (each quite short, a couple of pages) recently published in *JPS*

111(2). Because one of these reviews has the potential to cause a deal of mischief I will discuss it at some length.

The review by Paul Geraghty, a well-known Oceanic linguist (and on occasion, a co-author of mine) more or less dismisses the linguistic component of the book. Geraghty cites a number of errors and instances of sloppiness in the handling of linguistic data. He charges K & G with uncritical use of POLLEX, arguing that they greatly understate the amount of borrowing in PPn, borrowing that can lead to spurious lexical reconstructions. He bluntly asserts that “many supposedly Ancestral Polynesian items mentioned in this volume are clearly later arrivals” (p.173). He ends by saying (p.174) that “I am far from convinced of the reality of the authors’ reconstruction of the way of life of the Ancestral Polynesians; and, at least from the linguistic point of view, it has to be said that the authors have...produced a load of cobblers.” The implication is that the linguistic case put forward by K & G is mostly rubbish.

This conclusion of Geraghty’s is ludicrous. The review as a whole is deficient on several counts. For a start, the author does not understand statistical significance. Progress in science does not depend on perfect accuracy. POLLEX certainly contains some problematic comparisons but for the most part the reconstructions it puts forward are supported by massively detailed and reliable data. At a conservative estimate, 90 percent of the 2300 PPn lexical reconstructions are uncontroversial. In addition (and there is there is no mention of this in the review) K & G make extensive use of additions and refinements to POLLEX provided in papers and monographs by various linguists who have worked on particular semantic fields (Clark on birds, Hooper on fish, Marck and Clark on kin terms, Pawley and Marck on terms for people and social groups, and so on). The authors themselves have improved on some of the glosses given in POLLEX, using ethnographic sources. The sum total is a vast body of solid data, one that linguists working in other languages families and scholars in other historical disciplines would die for.

How many spurious reconstructions is “many”? Geraghty manages to find eight or nine PPn reconstructions that he thinks represent post-PPn borrowing by Polynesian languages from Fiji or elsewhere, along with four PPn reconstructions that he says are recent Samoan and Tongan coinages. The 13 cases put forward by Geraghty are asserted baldly, without supporting argument or references. But even if we grant all 13, that is 13 out of 2300. Half of one percent is not a significant proportion. The obvious question is never asked: Do these particular possibly spurious reconstructions make a difference to the

overall pattern of findings? The answer is they make about half a percent of difference.

Geraghty adds the confident but rash speculation: “No doubt many more spurious PPN reconstructions will be discovered, including perhaps a few more plant names from America” (173). On the contrary, there is considerable doubt, unless “many” is equated with less than one percent. Even 100 spurious reconstructions would not necessarily carry much weight, unless they were also crucial to the argument.

As it happens, Geraghty is probably wrong about at least four of the 13 cases, namely *\*fasu* ‘uterine nephew’, *\*tautahi* ‘master fisherman’, *\*matau* ‘axe’ and *\*kumete* ‘wooden bowl’. Each of the first three has cognates in Oceanic languages outside of the Central Pacific group (Fijian, Rotuman, Polynesian) and has been reconstructed at least as far back as Proto Eastern Oceanic in publications that Geraghty fails to cite (Marck 2000, Ross et al. 1998). *\*kumete* is widely attested in Eastern Polynesian and Outlier languages as well in Western Polynesia and Fiji. And to say that the plant names *\*pulaka* ‘swamp taro’ and *\*wii* ‘*Spondias dulcis*’ are not PPN but are “clearly later arrivals” is an exaggeration; there are arguments both ways. So here we find a probable error rate of 30 percent in the reviewer’s claims and some slack scholarship to boot. Let him who hath not sinned cast the first stone.

The reviewer suggests that the compilers of POLLEX do not claim the reconstructions labelled Proto Polynesian, Proto Tongic, etc. were part of particular proto-languages, but are merely labels for sets of comparisons. That is not my understanding. They are attributions to proto-languages. Where the compilers of POLLEX have doubts about the status of particular reconstructions, e.g. because of borrowing, they generally add footnotes saying so. The reviewer says that K and G “fail to understand that semantic reconstruction is based on agreement among highest-order subgroups not on a show of hands equating semantic reconstructions with numbers of reflexes.” This is a reference to the counting of number of daughter languages that reflect particular PPN words. But this comment is quite unfair. In general K & G are careful to note the distribution of cognates and meanings across subgroups.

One claim in the review is interesting: that the number of PPN reconstructions could be doubled by a thorough search of dictionaries of non-Polynesian languages. I hope Geraghty is right – that is a matter for future research. But why does he say “the authors fail to realise” this? They say nothing on this matter, apart from citing in figures of Marck (2000) figures concerning which

PPn reconstructions in the 1994 version of POLLEX have known cognates outside Polynesian (937) and which do not (1390).

Geraghty's review fails in its main responsibility, which is to discuss the most important hypotheses and arguments of the book. It would have been helpful if the reviewer had told us which specific parts of the picture of Ancestral Polynesian culture he finds unconvincing. For example, it would be useful to have commentary on the strengths and weaknesses of the 26 PPn terms for fishing strategies and gear, the 12 terms for kinds of containers, the 33 terms for the bark cloth complex, the 20 terms for status and role, the 24 kinship terms, the 29 terms for crops, the 65 terms for the inanimate physical environment, and so on. A more balanced view would be to acknowledge that the overwhelming majority of the PPn reconstructions cited in K & G are generally accepted and that many of their proposals about early Polynesian culture are supported by strong evidence.

The review by an archaeologist, Simon Best, is fairer. It congratulates the authors on opening up new horizons and providing a synthesis of data that will stimulate research and debate. But Best says almost nothing about the proposed reconstructions of Ancestral Polynesian culture and nothing at all about the linguistic evidence, perhaps feeling that it is beyond his competence to evaluate the former when it is so heavily dependent on the latter. He finds fault with K & G's dating of two key events in the archaeological record. Whereas K & G place the differentiation between Fiji and West Polynesia in the period 500-200 BC, Best holds that Fiji-Tonga-Samoa region remained archaeologically homogeneous until about the 1st century AD. Best argues that "Polynesia became Polynesia because Fiji became Fiji" (171), with a radical rearrangement of "the Fijian system" about 1000 years after the settlement of Fiji and the Tonga-Samoa area. I don't think K & G would disagree that more radical changes took place in Fiji than in West Polynesia. Best's contention that the whole of Fiji and West Polynesia remained homogeneous until 2000 BP is, I believe, a minority viewpoint. However, his view that 2200-1900 BP is several centuries too early for Polynesian settlements beyond core West Polynesia has more support (see earlier discussion).

I fear that in the short term Pacific prehistorians will not give this book its due. Why not? I think there are several reasons. First, the bulk of the crucial evidence treated comes from historical linguistics, a field that most archaeologists find arcane and daunting, and so are inclined to ignore. Second, in this age of specialisation, some scholars are wary of, even hostile to interdisciplinary syntheses. And there are specialists who are inclined to see

generalists as encroaching on their territory and are quick to look for faults that might allow them to dismiss their work. Third, there is the paradox of quantity. The more evidence is given, the harder it is for people to comprehend it. Most specialists like to pick away at small bits. *Hawaiki* is broad in its scope and very dense: it seeks to document almost every major cultural subsystem. Eyes used to reading about particular subsystems of a language, or relatively meagre fragments of material culture recovered from archaeological sites, or particular subsystems of a language, are likely to glaze over when confronted with highly concentrated syntheses of ethnographic, linguistic and (where available) archaeological evidence of everything from kinship systems to nights of the moon and from fishhooks to anthropomorphic gods. And because the book seeks to provide a comparative treatment of many domains of culture in fewer than 300 pages, the treatment of the evidence for each cultural subsystem is necessarily fairly brief and sometimes superficial, and leaves one craving a fuller account. The book could easily have been expanded to three or four volumes.

In the longer term, however, I think that *Hawaiki* will be seen as a landmark synthesis.

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**Anderson, Athol and Murray, Tim (Eds).** *Australian Archaeologist: Collected papers in honour of Jim Allen*. Canberra. Coombs Academic Publishing, The Australian National University, for Centre for Archaeological Research and Department of Archaeology and Natural History, The Australian National University with the Department of Archaeology, La Trobe University. 2000. 454 pp., Plates, Figures, Tables. HB (Home) \$A95, (Overseas) \$A87; PB (Home) \$A72, (Overseas) \$A66.

**Anderson, Athol, Lilley, Ian & O'Connor, Sue (Eds).** *Histories of Old Ages: Essays in honour of Rhys Jones*. Canberra. Research School of Pacific and Asian Studies, The Australian National University. 2001. 444pp., Figures, Tables. PB (Home) \$A70, (Overseas) \$A64.

These are remarkable volumes. Consider bulk. *Australian Archaeologist* contains 32 papers by 40 contributors; *Histories of Old Ages* also 32 papers (two numbered 28) plus seven interluding items and 45 contributors. But how to do justice to a total of 898 pages? Simply to list titles and authors would constitute a shopping list (eminently unreadable, an insult to all concerned). So after considering the achievements of Jim Allen and Rhys Jones and surveying the books contents, I shall refer to only a few of the contributions in any detail, partly as a taster of the whole, partly because they appeal to my own interests. To most of those 85 authors, especially longstanding colleagues, who receive no further, or only passing, mention, my apologies. Here are many well-written and wise words. Dear reader, here are riches with little dross.

Sadly, Rhys Jones died of cancer on 19th September last year, having retired as Professor Emeritus from the Australian National University the previous June. *Histories of Old Ages* (the title comes from an englyn [stanza] for the Welsh antiquarian, Edward Lhuyd [1660 - 1709]), becomes therefore a memorial as well as a tribute to a scholar who took from his native Wales to his adopted country, in the words of the editors' foreword, 'fine qualities of intellectuality, collegiality and friendship' (p.ix). Truly, he spanned two worlds, the first person as far as I know, to publish an article on the Tasmanians in Welsh (in *Y Gwyddonydd* in 1965).

For Jim Allen, there is comparable honour. *Australian Archaeologist* is just that; collected papers presented to him in 2000, by then Emeritus Professor at La Trobe University and Visiting Fellow at the Australian National University. Not surprisingly, given their common seedbed, these volumes have many similarities. Each includes interesting, often funny, though neither condescending nor trivial, information about them as people, not just scholars. The papers are well written, not unduly long, thoroughly referenced and appropriately illustrated. For these high standards one must thank Atholl Anderson, contributor and an editor of both books. Surely it was he who skillfully guided them to publication.

In broad terms, each set of essays reflects the particular interests, not just archaeological, of the recipients: (It is not a trivialeity, remembering his cultural urbanity, that during the making of Tom Haydon's famous - and to some critics infamous - film *The Last Tasmanian*, involving both Jones and Allen, the latter completed reading both *Bleak House* and *Dombey and Son*). And I must admit that to appreciate both these interests, and the present scope of Australian archaeology compared to how I first encountered it on a visit to New South Wales from Otago in 1962, initially I went back to those excellent general surveys of the subject by Josephine Flood and John Mulvaney, so changed has the discipline become.

In 1963 Rhys Jones arrived at Sydney University from Cambridge on a one-year tutorial appointment in archaeology. The following 1963-4 summer, Jim Allen, student in the Department of Classical Archaeology at that University, was a member of the fieldwork group Rhys led to investigate some Tasmanian sites. From such modest beginnings were these essays engendered.

Most of the contributors are based at Australian universities, writing on Australian topics, predominantly fieldwork-oriented. For Allen five contributors appear more than once, for Jones, eight, while nine contributors appear in both volumes. For both recipients there are, of course, impressive, detailed lists of their publications.

After his early work in Tasmania (and writing on Childe), and having taken his ANU PhD, Allen went to the University of Papua New Guinea, where he greatly extended local fieldwork, including research on coastal trade - one genesis, surely, of the massively significant Lapita Homeland Project, which, at one stage, had 15 research projects under way at one time. Later, as its first Professor, came the building of the Archaeology Department at La Trobe University, and of the Southern Forests Archaeological Project.

For Jones, his achievements might seem somewhat more dramatic, reflecting what Mulvaney calls his 'congenial Welsh fervour' (p.19), though less far flung than Allen's. After that initial work in Tasmania (as if the island had been awaiting a Welsh palaeolithic specialist to focus his attentions to its cold confines), there were those eye-catching though smaller subjects, such as fire-stick farming (at first a popular museum article), while alongside grew the work on the fate of Australia's megafauna. One outcome was that extraordinary year of research in Arnhem Land with Betty Meehan, linking, one might say, with

studies of the significance of pleistocene/holocene transformations, among other things demonstrating on the ground the non-inevitability of agriculture.

Allen's volume starts with papers of personal reminiscence. The next section includes Rathje on Lapita, which, given Allen's role in the Lapita Homeland Project, also figures extensively in papers in the other sections, as well as items on Tasmania, that persistent touchstone of Australian archaeology.

I was drawn to Jones and Meehan writing on the fundamental importance for Tasmanian studies of the 1965 ANZAAS meeting at Hobart. In a Congress atmosphere where major turning-point theories of continental drift were driving home the importance of Tasmania within the roaring forties, and Bill Jackson was arguing about the likely significance of the human factor in high fire frequency, Jones produced the results of his Rocky Cape excavations. With a preliminary sequence from the four excavated sites, he showed 'that the analysis and what direct dating that existed, did not confirm [Tindale's] original scheme' (p.51), which the latter courteously acknowledged.

At that same conference Les Groube had presented novel ideas about New Zealand archaeology. In his paper in this volume, unique in its theme, as a preliminary to retracing the route of malignant tertian malaria to Melanesia, he discusses the background 'within West Africa when the 'lateral transfer' of an avian malaria to a new host *Homo sapiens sapiens* created one of the most dangerous parasites known to man. It ends ca.3000 years ago with the expansion of the Bantu out of West Africa when FM became not just a regional but a pan-tropical menace' (p.131). This, as Groube puts it, is archaeology of the invisible.

Ian Lilley, who demonstrates in both volumes his interest in archaeologists' motivations as much as their actions, writes with feeling of Allen's role in coastal PNG studies. He, says Lilley, 'is the only archaeologist to have constructed a predictive general model of prehistoric developments in coastal PNG, or to have explicitly linked those developments with global theoretical questions concerning the dynamics of sociocultural evolution and their archaeological correlates' (p.249). When Lilley insists that Allen's persistence in looking beyond immediate academic horizons is met with outside parochialism, evidence of the general 'Balkanisation of the discipline' (p.260), I can only agree with his apt metaphor.

Green writes with his usual assurance on the Lapita culture, considering 'the cultural model for intrusion, integration and innovation' (p.372). He quotes (p.388) the sceptical Allen against himself: 'Our main argument with the out-of-Asia model is the notion of movement from a higher centre of culture bringing advances in subsistence, technology, and society to less progressive regions. Local equivalents of much of the material culture and subsistence activities which characterise the Lapita culture complex have now been shown to be present in Western Melanesia before Lapita'. Green ponders the extent of the word 'much' in the above quotation.

*Histories of Old Ages* begins with moving personal papers by Betty Meehan, Mulvaney and Schrire, the latter writing passionately 'of the power of Rhys Jones' work on the Tasmanian holocaust of the [19th] Century' (p.25). Tasmania also figures in essays by Gamble, Sim and West, and Cosgrove and Allen. In addition, there are contributions strongly oriented towards field research on human ecology, dating, the environment, diet and the arts, not forgetting others on the Fijian megafauna, on Russian interests in Aboriginal culture, and on Blandowski's natural history expedition of 1856-7 to the Darling /Murray Rivers' junction.

Bowdler writes persuasively of Jones' use of typology in his PhD thesis, 'his magnum opus', before considering 'the implications of its apparent lack of issue' (p.35). She quotes (p.42) the view of the splendid Isobel McBride in 1986 that archaeologists 'have swept aside the classificatory challenges of the corpus of stone artefacts', which, Bowdler asserts, 'goes hand in hand with a diminution of interest in culture history' (p.42). The two factors certainly can have this sort of relationship.

As I said above, Lilley is interested in academic motivation. Here he analyses the differences between Jones and Lourandos concerning the extent ('intensification') of cultural change in mid - to late Holocene times. These differences he links with the broader issue of the inevitability or not of the transition of society from foraging to sedentary agriculture. Here is not the place to consider 'the flavour of Marx's and Engel's original unilinear-evolutionary tendencies'(p.85) in relation to this still relevant matter. However, given the political context of so much archaeological research today, the nuances Lilley claims to have been infused in the 'scholarly relationship between a (neo)Marxist and a mildly eccentric Welshman'(p.79) gives this essay topical as well as philosophical significance.

Hiatt, so often an intellectually free-running social anthropologist, writes of *Homo mobilis*. Who, it could be asked? Taking data 'from modern non-human primate species' he suggests 'that sexual competition within early human communities is likely to have constituted a potential for fission and colonisation of runaway proportions in the resource-rich and predator-free environment of Australia' (p.112). Given that problems always existed for potential elopers wishing to escape 'regimes of infant bestowal and gerontocratic polygyny' (p.117), there would surely have been many opportunities when this environment favoured enlarging the human frontier. But *Homo mobilis* could not always be on the move. 'For a long time, between the golden age of travel sometimes known as the Dreamtime and the appearance of the emissaries of Christ, youth had little option but to respect the reproductive interests of the elders' (ibid.). Lovely.

Finally I must mention Spriggs' paper. Always carefully fluent on Pacific chronologies, Spriggs insists that one cannot ask the question 'why?' of seaborne colonisation without first determining 'when?'. Thus, of East Polynesia, he and Anderson had argued that it 'had indeed been settled after a long pause, of over 1000 years, in Western Polynesia' (p.243), producing a very different cultural history to that proposed by those favouring no such gap. Spriggs adds somewhat dryly: 'Subsequent re-dating of claimed early sites in East Polynesia has supported the later chronology' (ibid.).

In a country where an academic subject has few professional workers, they tend to receive more individual scrutiny than when their numbers are larger (though to a degree one can hide in a crowd, no matter how red one's shirt). Academic pioneers have great opportunities, but equally great responsibilities, especially to those they teach, who are often both subsequent colleagues and successors. Neither Allen nor Jones would have seen themselves as absolute pioneers in their discipline, but, after blooding, both men emerged as fine scholars who indeed pioneered many areas of research - where, to literalise the metaphor, especially if one includes the island chains to the north, it can be said there is still much to be explored.

These papers have considerable significance, I think, as sources for a general history of Australian archaeology of which the Australian people could well be proud. An academic discipline always invokes an outline map of its own uncertain future. The best writing of history involves unlocking views of the past

once thought unknowable and still much unknown, though (as Childe used to say regarding knowledge) now perceived as knowable.

My thanks are due to Jack Golson for comments on an earlier version of this review.

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**O'Connor, Sue.** *30,000 Years of Aboriginal Occupation: Kimberley, North West Australia.* Terra Australia 14. ANH Publications and The Centre for Archaeological Research, The Australian National University, Canberra, Australia. 1999. xviii, 155 p. : ill., maps ; 30 cm. \$A42.00.

This monograph describes work undertaken in 1984 and 1985 on the west Kimberley coast and off shore islands. It reports on excavations at four rock-shelter sites: Widgingarri 1 and 2, Koolan shelter 2 and High Clifty Shelter. Widgingarri 1 and Koolan 2 have sequences dated from ca. 28,000 BP and although the Widgingarri 2 shelter is undated, O'Connor argues on stratigraphic grounds that it is of similar antiquity. The High Clifty Shelter has a late Holocene sequence. Both the Widgingarri shelters and Koolan 2 have radiocarbon chronologies interpreted to reflect substantial gaps in occupation around the time of the Late Glacial Maximum (18,000 radiocarbon years BP). Thus, the sites help to inform on the debate in Australian prehistory concerning the human response to high glacial aridity.

The monograph is broken into seven chapters. Chapter 2 provides details of the study area and ethnographic information on the people who inhabit it today or in the recent past. Population is clearly concentrated on the coast, particularly the coastal river mouths and islands, and economic pursuits emphasise shoreline resources.

In Chapter 3, the current and past environment is described. Much attention is given to estimates of past sea levels, particularly during the end of the Late Pleistocene when sea levels are thought to be 130-120 m below present levels. These levels would place the west Kimberley rock shelters over 200 km from coast, and it is the switch from coastal to an inland location that is used to explain the hiatus in occupation.

Chapter 4 through 6 provide details of the rockshelter excavations beginning with Koolan Shelter 2. The oldest date from the site,  $26,500 \pm 1050$  BP, is from marine shell and thought to represent only a minimum age. From 24,000 BP until 10,500 BP the site is abandoned, the return of occupation closely correlated with sea level rises that once again gave the site a coastal location. Koolan only became insular around 8,000 BP. Faunal preservation at Koolan 2 is unusually good by Australian standards, with shellfish, crab, fish and turtle remains all argued to be the result of human predation. Macropod remains are also present but are thought to have been brought from the mainland. Despite the abundant species and good preservation, the faunal remains seem to reflect only exploitation of the immediate environment.

Chapter 5 provides details of the excavation at Widingarri Shelters 1 and 2. These sites are located further inland than the Koolan Shelter and are argued to have been abandoned later, at approximately 19,000 BP. Like the Koolan Shelter, faunal material is well preserved but not so charcoal, leading to some difficulties in establishing a chronology. A full dated sequence cannot be constructed at each of the shelters independently and instead stratigraphic markers, particularly the appearance of quantities of shell, are used to relate the Shelter 1 and 2 sequences together.

The chronological sequence at Widingarri Shelter 2 is the most complete hence the artefactual material from this shelter was fully analysed. In the Holocene sequence, flaked points make their appearance, associated with a radiocarbon determination of  $4970 \pm 60$ , one of the earliest dates for points in Northern Australia. Like the Koolan Shelter, faunal preservation is good, particularly in the last 5,000 years of the sequence. O'Connor argues that the range of species would sit well with an interpretation of the site as a base camp to which men, women and children returned small amounts of the daily catch.

Chapter 6 deals with the last of the rockshelters to be excavated, that on High Cliffy Island. Unlike the previous sites, High Cliffy Shelter preserves a sequence dated only to the Late Holocene, with a basal date of around 3,200 BP with occupation continuing for 700 years. A variety of explanations for the relatively short sequence are canvassed including the suggestion that the rockshelter surface was regularly cleaned thereby removing evidence of later occupation. In addition to the rockshelter site, survey revealed a rich archaeological landscape on the island, with several open sites and a number of stone arrangements.

The monograph concludes with a final chapter summarising the regional sequence divided into the Pleistocene, early Holocene and mid-Holocene to European contact. Clearly, relatively limited work has been undertaken in the west Kimberley and the sequence will be augmented by future studies. Still, the sites investigated are rich enough to allow the author to comment on a range of issues current in the Australian literature from the nature of the human response to Late Pleistocene aridity through to late Holocene intensification.

As with other monographs in the Terra Australis series, this work provides a great deal of primary information on a series of important excavations. Compared to some other Australian Pleistocene sites, those in the west Kimberley have preserved good faunal assemblages. The variety of information obtained from these sites will no doubt ensure that this monograph forms an important resource for those interested in the record from this period.

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**Orton, Clive. *Sampling in Archaeology. Cambridge Manuals in Archaeology.* University of Cambridge Press, Cambridge. 2000. 261 pp. NZ \$59.95. paperback.**

A book on sampling in archaeology might not be considered a good read on a cold evening unless you are suffering from insomnia but I think most archaeologists would find something to keep them awake in this book. It should be on everyone's shelf or readily available in your local library.

Archaeology is all about sampling and sampling issues, and those along with chronology, form the core methodological problems for our discipline. Orton writes in a relaxed avuncular fashion imparting wisdom won through years of consideration of these problems in a very practical manner. He keeps most of the algebra to the appendix but points the reader to lots of recent references that should give you all the mathematical detail or examples you need to apply the approaches he discusses.

The book is composed of 9 chapters. In the first chapters he gives a basic introduction to statistical sampling theory and some of the history of its application in archaeology. This serves as a sound basis for the following

chapters, which deal with archaeological applications at different scales from the region to the artefact. Before the examples though, we get a journey into archaeological theory. Back in the early days of archaeological sampling fever in the 1970's, sampling was often applied in a somewhat simplistic mechanical fashion that often seemed to defy archaeological knowledge, resulting in survey of strips of water or mountaintops. At the same time some people wondered what we should consider our samples to be representing, as we knew that the sites preserved or even artefacts recovered need have little or at least some complicated relationship to what was originally deposited. Orton discusses these issues in depth. He stresses that sampling methodology has in fact become much more developed and archaeologists are only now applying approaches suitable to their particular problems. For example the method of 'adaptive sampling' is much closer to archaeological practise. In adaptive sampling knowledge gained during the survey process is allowed to systematically modify the survey. For example archaeologists typically start to look harder once a site is found, thinking that where there is one there are likely to be more. Adaptive sampling would allow you to incorporate this behaviour into a systematic approach which would allow you to still treat the recovered data in a statistical manner.

The other issue is essentially getting our sampling approaches up to speed with considerations of taphonomy. At one level it is rather depressing to think of all the taphonomic issues which might have to be dealt with to allow you to compare samples of different types of materials or sites or even the same material from different parts of a site. However Orton provides a good set of terms to be used in discussions of these kinds and sets the readers onto references to work, often in other disciplines, which deal with similar types of issues. Research into these methods is active world-wide and among colleagues here at Auckland.

The main set of chapters (4-9) provide discussion of sampling at different scales. Everyone planning a field survey should read Chapter 4 and contract archaeologists might find some useful approaches here. Chapter 5 concerns sampling within the site or find spot (off-site archaeology is also discussed). Of particular interest here is evaluation of methods for sampling which will be representative of structure. Sampling using knowledge based on geophysical survey is considered. Chapter 6 deals with artefact and ecofact level sampling with special reference to problems of quantifying floral and faunal remains. Chapter 7 deals with sampling from artefacts and ecofacts,

for example thin section analysis of ceramics. Finally Chapter 8 is unique in that it looks at issues that arise when sampling museum collections. All of these chapters are gold mines of information and useful references. One of the key points which Orton takes some pains to discuss is the fact that archaeologists are very often engaged in cluster sampling without recognizing the fact. For example a thin section where the grains are counted is a cluster sample, as the entities being studied all come together and are not independently selected. The same is usually true of any bulk sampling procedure. This needs to be considered when dealing with the data in a statistical fashion.

I found it heartening to read this book as you can see that we have made considerable real progress in this aspect of archaeology over the last 30 years and it bodes well for the future of the discipline as a whole.

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**Marshall I. Weisler. *On the Margins of Sustainability. Prehistoric settlement of Utrok Atoll, Northern Marshall Islands*. 2001. BAR International Series 967.**

This publication is further testimony to the remarkable capacity of human populations to successfully colonise the most marginal of landscapes. It is also testimony to Marshall Weisler's capacity for successfully carrying out fieldwork in such environments and more importantly to see that the results and analyses are carried through to publication. It is a well presented and easily readable monograph which greatly enhances our knowledge of Atoll archaeology. The artefact illustrations done by Les O'Neill are true works of art and highlight this crucial component in such publications. There are a few typos and signs that the Microsoft empire is making severe inroads on the English language (eg., excellerated [page 4]) but the quality of the photographs and the overall publication is high.

The fieldwork associated with this publication was carried out while Weisler was the Chief Archaeologist for the Historic Preservation Office, Republic of the Marshall Islands. It demonstrates that Cultural Resource Management and research archaeology can be very effectively merged. Indigenous

participation and cooperation is also shown to be an essential component of such ventures.

The monograph begins with an introductory chapter detailing the study area. Included is the geomorphological history, cultural and historical setting, previous atoll archaeology (of which there is not a great deal) and finally issues and a research design. This chapter is detailed and sets the research within a robust context. A little more information or discussion on the timing of the emergence of the atolls (about 3000 years ago [page 1]) particularly as this timing has significant implications for human settlement would have been useful. The research design is ambitious and includes origins, chronology, sea level change and islet development, material culture, marine subsistence and terrestrial production and landscape change.

Chapter Two moves onto the archaeological landscape starting with details of the modern settlement patterns and subsistence activities and outlining the results of a series of surveys. Chapters Three and Four which respectively present details of the excavation of settlement sites and horticultural sites are the heart of this monograph. The extensive excavations and their detailed description provide a wealth of evidence relating to the human settlement pattern and subsistence activities. Such detail is often missing from publications which can then require the reader to accept various assertions without the evidence. It is the opposite in this case where Weisler has presented all the excavations in a clear and detailed format. This provides a convincing body of data to back up his conclusions. The detailed excavation and efforts to date the abundant aroid pit systems on Utrok add much needed detail to these somewhat understudied horticultural systems. While they are very obvious features on the ground, establishing their chronology requires very specific excavation technique and analysis. In this Weisler builds further on his previous experience and publications (Weisler 1999, 2001).

Chronology is detailed in Chapter Five with a total of 20 dates (mostly AMS) each being discussed in fine detail even to the point of outlining the dates of when samples were collected and submitted. Again this sort of detail provides a solid set of data from which conclusions can be drawn. What was lacking in this chapter was a summary discussion of the dates, the fine detail almost clouding the general picture.

Material culture is outlined and discussed in Chapter Six. A total of 165 (175 on Table 6.1) pre-contact items were recovered, the vast majority of which was worked shell (53%), followed by shell adzes and ornaments. Sixty-five per cent of the pre-contact artefacts came from one site. All the artefacts, not surprisingly largely fashioned from an array of different shellfish, are discussed in detail and as noted above the illustrations are superb. Overall there was not a great of recovered artefactual material and as later noted by Weisler (p.124) very few of these artefacts came from dated contexts which very much limits any meaningful discussion of chronological change or regional comparison. Historic artefacts are also briefly discussed but the vast majority of them are related to mid and later twentieth century occupation.

Subsistence patterns are outlined in Chapter 7. Again these are outlined in fine and full detail. The results are gleaned from the complete recovered sieved sample (ie., no discard in the field) and provide a convincing picture of prehistoric subsistence patterns. The archaeological evidence is also compared to substantial ethnographic data collected by Weisler.

A synthesis of the data is presented in the concluding Chapter Eight. Here the stated research designs are revisited and discussed. All are succinctly discussed except for the question of origins which does not appear, although the reasons for this may already have been hinted at in the outline of the research design where Weisler states that "it is indeed, a complex issue and one that can only be adequately resolved with a diverse array of data" (page 6).

In the concluding comments Weisler rather lamely suggests that the lack of archaeological evidence for resource depression and extinctions might suggest that human populations in Utrok lived in some sort of sustainable manner (although the opposite is hinted at on p's 106 and 124). To demonstrate the initial impact of human colonisation on this atoll would require the excavation of sites that were associated with the first years of initial colonisation (which is emphasised by Weisler here). Any resource depletion or extinctions would have occurred very abruptly. This appears to be the case on even much larger Pacific landmasses (Bedford and Spriggs 2000; Holdaway and Jacomb 2000). If those sites are not located it is unlikely that such evidence will be found. From a number of the figures (Figure 2.24, 3.2, 3.44) it is obvious that substantial coastal erosion has occurred and is ongoing. This may well have carried away the evidence for

the very earliest settlement. On current evidence, that is consistent across the entire Pacific, the lack of evidence for it should assume that it has not been found or has not survived rather than the suggestion that some sort of utopic harmony having existed between human populations and native fauna.

BAR series publications have in the past been of variable quality but this publication authored by Weisler does not suffer from that. It is of high quality and worth more than a passing glance both for its significant contribution to the archaeology of atolls and a very real demonstration of the human capacity for adaptation to the most marginal of environments.

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