

Chapter 8

What may be learnt about the archaeology of islands from archaeologically derived models of the exploration of Polynesia, 1966–2001?

Doug Sutton, University of Waikato

Introduction

Polynesian archaeology is one regional specialization in the world-wide practice of archaeological investigations of islands, oceans and seas. It is timely to consider how Polynesian archaeology fits within that newly-articulated framework of theoretical and methodological advances concerned with islands. To do this, I examine the history of archaeologically-derived models of the exploration of Polynesia developed since the invention of radiocarbon dating.

The focus on exploration models has been chosen for four reasons. First, they are more ubiquitous than any other topic pursued by Pacific archaeologists in the recent past. Therefore they provide the best window on change over time in the ways in which Pacific prehistory is made. Second, models are chosen because they are at least as difficult to build as anything else Pacific archaeologists construct: the stratigraphical evidence for the designation of *earliest* is vaguest, the traces of anthropogenic environmental impacts are most ambiguous, and speculation is necessary to fill the yawning gaps between data points, which can have untoward influences. For those reasons the development of models of exploration test collective wit and perspicacity to an exceptional extent. Third, they occupy an esteemed place in the panoply of models produced by archaeologists. They are at once most publicized, most exalted and fiercely contested. Fourth and finally, those models are chosen as the focus of this work because they are of a kind necessary to almost all archaeological sequences. This makes them part of the currency and topicality which Pacific archaeology exchanges with other parts of the world of scholarship. They comprise one of the principal bases upon which the ability of Pacific archaeologists to speak to

issues of global importance is considered by archaeologists elsewhere, many of whom are also building models of exploration and population movements.

A succession of Polynesian exploration models has built up over the last 40 years. Those considered here were published by Sinoto (1966), Kirch (1984), Irwin (1992), Spriggs and Anderson (1993), and Kirch and Green (2001). Detailed consideration of these models leads to a summary of their differences and similarities and the theoretical content of each model as expressed in the vocabularies used by its author or authors, and recognition of substantial variation in the use of evidential support.

Doing these things makes it possible to consider how points of difference between successive schemes could be parsimoniously resolved, rather than remaining in the guise in which they are have been presented—i.e., as elements in alternative narrative accounts which exist in opposition to one another because of fundamental differences. Implementation of the five suggested resolutions presented below would strengthen the contribution which Polynesian research is making to ‘archaeology of islands, oceans and seas’, and lead to further development of the investigative capability of islands archaeology insofar as those resolutions influenced islands archaeology elsewhere (e.g., see Terrell 2004).

Similarities

The models are similar to one another in the degree to which they share three features. The first is the use of arrows to represent a succession of point-to-point ‘voyages’ in Sinoto’s terms or ‘explorations’ in Irwin’s language which connected widely separated places and resulted in first set-

tlement of formerly pristine island groups. In doing so they continue a long tradition in Pacific scholarship, going back to the arch-diffusionists. There are a couple of reasons for thinking that the pointed arrow may have outlived its usefulness.

The first is that since Sinoto's Voyage 1 was discredited and Biggs (1972) critically reviewed archaeological use of historical linguistics, exploration is seen as most likely to have occurred wherever possible on a nearest-neighbour basis rather than via a small number of 'heroic' long distance journeys. Second, the directional arrow was standard fare in illustrations of the prehistory of continents but is no longer widely used in either North America or Europe, although it has currency still in Australia (e.g., O'Connor and Chapell 2003: figure 1). There are alternative approaches by which exploration can be rigorously modelled and accurately represented. For instance, Hazelwood and Steele (2004) use a very different approach in considering whether intrinsic limitations are brought to the modelling of population dispersal when these are based on either variation in radiocarbon dates of early sites or variable distributions of discarded artefacts. They suggest that models must be built to test the veracity of each of those approaches, applying a statistical model based on population-level description of the demographic characteristics of the expanding populations involved in the European Neolithic transition and the late glacial colonization of the Americas population of the New World. This leads them to the view that narrow constraints limit the usefulness of radiocarbon and artifactual approaches to the mapping and timing of population movement.

The second shared feature is that the models express a framework of inferred facts dealing with four topics: source of first settlers, order of first settlements, date of each first settlement, and the intervals between them. This is more an assumed form rather than a necessary one. It mirrors the dendritic form in which linguistic histories and biological distances are often expressed but may not provide the clearest possible reflection of the realities of knowledge about exploration.

The third shared feature is a short vocabulary of process descriptors used in all the models. The meanings attributed to its key terms vary from model-to-model. I consider that further in the next section.

Differences

The models differ from one another in: the chronologies they offer; their uses of process descriptors; what is taken into account as evidence, and; how different categories are treated in relation to one another. I consider these issues in the following four subsections.

Chronologies

Inferred first settlement dates in the models under review are such that the values for:

1. Samoa and Tonga have been to 3500 BP and returned to Sinoto's value
2. Tuvalu and Tokelau are added by virtue of recent research
3. the Marquesas have been to 2100–2200 BP and returned to Sinoto's value
4. Easter Island appears to be the most volatile. However, Kirch and Green's estimate is the only one outside the range of the other three
5. Hawaii appears to be volatile too, but again all values except the Kirch and Green estimate overlap
6. the Society Islands are receding in time, though primarily on the basis of inference as to what might be found there
7. Mangareva was important in the most recent model
8. Kermadecs is uncertain due to limited field-work
9. New Zealand is becoming more recent
10. the Chathams is the most recent.

Turning to the intervals between first settlement events it is evident that only two of Sinoto's pairs of consecutive first settlement dates were large enough to be separated by the radiocarbon assays available at the time. In effect he was arguing for the archaeologically instantaneous first settlement of Society Islands, Marquesas, Easter Island and Hawaii.¹

Kirch's (1984) model includes no fewer than four pauses, some are archaeologically attested while others are supported either primarily or

¹Davidson (1984: figure 21ff) made this point on the basis of her review of sources relating to Polynesian voyaging which were published between 1957 and 1979 (see also Davidson 1993).

entirely by reconstructions of the order in which language subgroupings are thought to have separated. At critical points, linguistic analysis is used to infer the order of separations and archaeological data are then used to establish the absolute date in time at which or by which a separation occurred. Linguistic inferences are used to repair weakness in archaeological data in the service of historical anthropology. Irwin's model represents Polynesian first settlement as a continuous process which occurred at an accelerating rate until faced with the discovery of New Zealand at which point it paused. Kirch and Green (2001) smooth most of the pauses out of Kirch's (1984) staccato model and bring the ages of first settlements of western Polynesia, the Marquesas and Hawaii to within archaeological range of Sinoto's values. They abbreviate the Easter Island sequence and lengthen the past of the Society Islands significantly. Sinoto's suggestion that the Marquesas acted as a primary dispersal centre is long gone (Allen 2004). Attribution of a similar role to central eastern Polynesia is questionable because recognition of the continuous nature of the exploratory process undermines the need for 'centers' from which dispersal emanated. In this respect the Polynesian sequence parallels several continental models in which punctuated movements of anatomically modern humans (AMHs) between nodes have been disproved as research has intensified, more fully revealing continuous movement.

Pauses are very important in the inferred chronology of Polynesian exploration. They have reduced in number and migrated eastwards since 1970. As Irwin (1981: 482) noted in 1981,

10 years ago a pause model was set in Western Polynesia itself... It is now known that the whole area was more 'equally' early than first thought. The pause model did not work... But now the same old kind of model has simply shifted to the border between Western and Eastern Polynesia.

In 2000 Irwin suggested that the West Polynesian pause may have been confused with a pause at the Andesite Line, seen in West Polynesia and in between, in Micronesia (Irwin 200). Two pauses remained in Irwin's (1992) *Prehistoric Exploration and Colonisation of the Pacific*. Continuing the eastward shift of such pauses, one was between tropical East Polynesia and New Zealand, which took 500 years. The other is between New Zealand and the Chathams to which Irwin attributes a further five centuries.

These pauses too are under question. The first is no longer accepted by a narrow majority of members of the New Zealand Archaeological Association who attended the recent conference in Russell (Prickett 2002). In addition, as Davidson (1984: 24) and Sutton (1980) earlier noted, the radiocarbon dates upon which Irwin (1992: 107–108) and McFadgen and Yaldwyn (1984) depend for the pause prior to the first settlement of the Chathams do not relate to evidence of first settlement there. This suggests that further changes will be forthcoming, and that they may continue a trend towards less pause, more pace, greater continuity.

Use of process descriptors

The shared vocabulary of process descriptors is problematic, not in its content but in its muddled usage. For instance, Kirch discriminated between dispersal and colonization in his book on the *Evolution of the Polynesian Chiefdoms* (1984: 71–86), but used discovery, colonization and settlement as more or less equivalent terms, in his influential essay on *Rethinking East Polynesian Prehistory* (1986). In 1992 Geoff Irwin clarified the meanings of key terms very significantly. 'Exploration' was seen as being based on systematic methods and strategies and not referred to as dispersal, avoiding any sense of 'scatter'. 'First settlement' could be proven by the earliest well-attested evidence of human presence, whether archaeological or palaeoenvironmental, and was sometimes inferred in the absence of empirical evidence. 'Second' or 'later settlement' also occurred, but was not referred to as 'secondary', which freed such events of any connotation of lesser importance. 'Colonization' occurred when occupation was established. 'Post-colonization changes' included abandonment of islands, decline in voyaging, cultural and linguistic differentiation, and other sociocultural changes which have been seen as the outcomes of cultural adaptation by Kirch and Green (2001). Spriggs and Anderson (1993) refer to colonization as an 'event' which has occurred when early initial, or first settlement has occurred.² Kirch and Green (2001: 79–81) use the terms 'discovery' and 'colonization' to refer to distinct events, though their use of 'first settlement' combines both in the sense that first settlers are seen as discoverers who established settlements, thus colonising a place. They use the term 'settlement' as a gloss for both 'discovery' and 'col-

²See Spriggs and Anderson (1993) use of 'colonized' as a synonym of 'first settled' in their paraphrase of Sutton (1987)

onization'. Elsewhere they use 'discovery' and 'colonization' together, with the clear implication that each refers to a different phenomenon. In one passage they state that New Zealand was "discovered, explored, and eventually colonized around AD 1000–1200" (Kirch and Green 2001). Reading their other usages, summarized here, leaves one uncertain as to whether one, two or three discrete events were involved, and whether these are thought of as simultaneous, concurrent or consecutive.

What counts as evidence

Sources of evidence are variously used; all by some, sometimes, but neither always nor consistently, while far fewer sources of evidence are used by others. Explicit reasons are sometimes offered to explain inclusions or exclusions.

For instance Sinoto (1966) used typological studies of artefacts which had been excavated and more or less accurately radiocarbon dated by the standards of the time at which he wrote, though very little of that dating would pass as accurate enough now. Kirch (1984, 1986) used archaeological evidence, linguistic reconstructions, and palaeoenvironment data indicating human induced ecological change in his critique of Sinoto's work. Irwin (1992) argued against the use of linguistic phylogenetics, preferring instead the use of archaeological data which informed and was anticipated on the basis of his reconstruction of the logic underpinning Pacific navigation. Spriggs and Anderson (1993: 200) state that they use "the results of radiocarbon dating applied to pollen cores, sediment columns and archaeological samples from Hawaii and the rest of East Polynesia". Actually, they do less than that as most of the archaeological radiocarbon dates are rejected and most of the relevant palynological and sedimentological dates are unmentioned in their paper. Kirch and Green (2001) attempted a correlation of linguistic and archaeological evidence in a major project intended to advance historical anthropology, and based on "a triangulation method in which historical linguistics, archaeology, comparative ethnology and biological anthropology independently contribute their data and assessments to the common objective of historical reconstruction" (Kirch and Green 2001: 42). They also use palaeoenvironmental information, sometimes but not consistently.

All of this illustrates that there are fundamental issues at play. One is the degree to which palaeoenvironmental evidence is actually being used in building exploration scenarios. The other

question is of a general form and vastly more important than the first: how can categories of evidence which are very different in terms of epistemology be used together to form an aggregate whole which is more reliable or more robust as historical reconstruction than any of its constituent elements would be if used as the sole basis of reconstruction? Reviews of Kirch and Green (2001) raised this question in critiques of the use of historical linguistics in triangulation method but that is only part of the quandary, because archaeologists typically use multiple, divergent sources of evidence so the combinatorial issue applies to much more than the triangulatory use of the protoloxeme and semantic reconstructions. I comment further on this point later in the paper.

The need for resolutions

The purposes of Polynesian archaeology can be glossed as follows: to map movement of peoples into and throughout Remote Oceania, to do with sufficient accuracy to enable the processes of change in human biology, culture, society and ecology to be better understood, and to form by these means the basis of scholarly enquiry into causes of change, diversity and relative homogeneity seen in the Pacific and elsewhere in the world's oceans and seas.³

Detailed examination of the exploration models show that the inferred facts which are the focus of this paper have varied a lot but changed little over the last forty years, leaving an overall impression of unstable churn rather than directional change in historical reconstruction based on increased precision. This is exceptional in contemporary science.

Means by which those resolutions can be achieved

I suggest things could be improved through change in five areas:

1. adoption of an islands framework
2. development of higher-resolution, continuous chronological sequences on Polynesian islands
3. the more rigorous use of process descriptors and the development of inferential time-place systematics as necessary parts of the investigative process

³This statement of purpose is influenced by papers by Terrell (2004), Anderson (2004) and Renfrew (2004), and proceeds from the idea that islands, can when considered carefully, make things visible.

4. use of high-resolution techniques in stratigraphical research, including cultural palynology
5. integration of excavation and environmental strategies so both are optimized.

First, the case for adoption of an islands framework is straightforward. Archaeology originated in Europe and the underpinnings of world archaeology were developed there and then in North America. Pacific archaeology inherited assumptions that came from continental contexts, the most general may date back to Thomsen and the origins of archaeology, others to Kroeber in the Americas and Kossina in Europe, some to the cultural evolutionists of the 1950s (see for example Hardin et al., 1960, and therein a remarkable foreword by Leslie A. White). But, as Jim Allen (2003) has noted recently, there can be basic differences between cultural and communication processes on continents and those which occur on islands in oceans and seas. He emphasizes the need to base assumptions about cultural process and interactions on the most directly relevant situations and to inform those assumptions with knowledge of the realities of island ecology, history and anthropology.

Second, there continues to be a pressing need for the development of fully accurate chronologies for the cultural sequences on each of as many islands and archipelagos as possible. It is important to note that current approaches to sampling for whole site dating, methods used for the combination of dates from within one site and between different, though interrelated stratigraphies are under question (Jones 2002; Jones and Nicholls 2002; Nicholls and Jones 2001). Taken together, these recent analyses indicate that for New Zealand the earliest date of occupation is currently misstated, while the duration and periodicity of occupation of key sites has been incorrectly calculated and is, as a result, quite wrong, and that full redress of these problems is a long-term undertaking (Higham and Jones 2004). This has obvious implications for the broader Pacific and suggests the need for new field and laboratory-statistical dating protocols, strengthening if not superseding those in use at present.

With those in place there could be increased emphasis on continuous cultural sequences of the long duration, and necessarily more consideration of synchronic cultural occurrences as they relate to neighbours and other contemporaries, antecedents and descendants.⁴ There

⁴This remark is, in part, a reaction to an historical tendency towards the treatment of Lapita as a discrete

could also be an emphasis on the use of well-dated sequences as the single most important bench-mark alongside which natural and cultural changes could realistically be sized and scaled. In particular, the use of such independent chronology to clarify the strengths of historical linguistics as itself, rather than as a form of enquiry which, as Kirch and Green have urged, should not substitute for archaeological investigations in the Pacific.

Third, the use of process descriptors should be revised as advocated by many over the last twenty years (Davidson 1984: 222–225; Kirch 1984: figure 27ff; Irwin 2001; Graves and Addison 1995: 381). It is recognized that the discovery, colonization and settlement of Polynesian islands, and others, can be consecutive events with significant ecological, demographic and cultural implications (see for discussions of these issues in disparate islands research Broodbank 2000; Jones 2002; Vesteinsson et al. 2002; Whitley 2001). The key point of debate is whether archaeological methods can discriminate between discovery, colonization and settlement. In the Aegean, North Atlantic, including the Faroes, in the Falklands and the Caribbean archipelagos, those discriminations are routinely attempted in current research (references above and papers in Fitzpatrick 2004; Burney et al. 1993). In the Pacific, opinions are mixed but moving positively. For example, Anderson's position has recently adopted the Graves and Addison (1995) scheme in considering the early phase of the sequence in Fiji. I am optimistic that evidence of discovery can be satisfactorily identified.

The blurring of process descriptors in Polynesian archaeology, described earlier in this essay, did not help the development of interest in discriminating between discovery, colonization and settlement. On the contrary, it has apparently had the effect of allowing the continuation of lower resolution research in Polynesia than occurs elsewhere in islands archaeology. It would thus be useful to move the use of process descriptors from passive and muddled to clear, consistent and inferential. This would involve the use of an analytical vocabulary of descriptors based on the key issues under investigation, and aligned with the lived realities of the history of Polynesia. The terms discovery, exploration, colonization and settlement appear have broad support. In future, they could be used widely and consistently. In the general case, attribution of a site to one of these descriptor categories would be justified only by the presence of evi-

culture, of brief duration, strangely disembodied from other busy, interactive Pacific cultures and societies.

dence directly related to the process content of the descriptor in question. For instance, a site attributed to discovery could be expected to show evidence of pristine environment, initial anthropogenic impact, earliest reliable dates, limited use of more inaccessible local resources and artefact forms and materials from a source region.

Consistent space-time systematics could then develop through the analysis of well-dated portable and non-portable artefact assemblages which were discovered in strata attributed to specific descriptors. This would help to repair the present situation in which there are no agreed time-place systematics in Polynesian archaeology. Adopting the approach suggested here would make the use of time-place systematics an active part of the Pacific-wide investigative process.

Fourth, there is a pressing need for the use of high-resolution techniques in stratigraphical research, including cultural palynology in Polynesian research. For instance, there is at present a considerable mix of methods in use in palynological studies relevant to identification of the earliest human presence. Narrowing the range of methods used, to produce consistently high-resolution research would be advantageous.

Fifth, in their work work in northern Europe and the North Atlantic, Edwards and Mithen have sought the integration of excavation and environmental strategies so both are optimized (Edwards and Mithen 1995). Following that lead, this paper calls for a reintegration of archaeological chronometrics and cultural palynology into the core of archaeological methods, following their successful application in island contexts elsewhere in the world. This can be achieved simply in two key areas: (i) the inclusion of well-established palaeoenvironmental change when it is specifically enough dated to actually contribute to the objectives of archaeological research.⁵ Obviously there will be concern at the widening gaps which exist between some palynological methods used in New Zealand archaeology and those now in place in the most acute investigations of the dispersal of Neolithic populations through Europe and across the North Atlantic. (ii) Further collaborative research with dating scientists to establish the general use of optimal sampling and statistical methods is needed, which should repair a schism between the two which was identified recently by Allen (2003: 38).

⁵This supports Anderson's (1995: 116) objection to the possibility that palynology might become 'archaeology by proxy'.

One invaluable source of insights into cultural processes is the library of specific understandings which has accumulated from anthropological observations of island life. So there may be very good reasons for making sure that interpretations of archaeological, linguistic and environmental data and whatever else is used archaeologically time to time are grounded in what are believed to be the most directly relevant understandings. Similarly, investigations should be scaled and dated on the basis of those insights. More often than not they will be from islands in oceans and seas, not the Pacific alone but the others also, and less often from continents.

Conclusion

Particular conclusions reached in the body of the paper are as follows:

- There is a high degree of churn in dates of discovery or, to make the same point differently, little by way of sustained directional change in the archaeological timing of Polynesian exploration.
- In the absence of an agreed time axis and the presence of diverse goals and interpretative methods, there are no generally agreed archaeological time-place systematics which apply across Pacific archaeology.
- Instead, several semi-discrete, variously founded and differently intended schema are in simultaneous use.
- Under those conditions there is plurality and dissonance in the current conceptual framework.
- However, these conditions may not prevail. This is because of the momentum in the archaeology of islands as a new, distinctive research practice.
- In particular, it supports the recognition of islands as distinctive, highly valuable research settings (Burney 1997; Terrell 2004) and the further development of research methods capable of enabling islands to more clearly 'make things visible'.
- A focus on islands would see a shift of emphasis from culture area approaches, and the content and boundary condition issues they evoke, to a more generalized search for understandings of cultures that inhabited oceans and seas.

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