

Intrusion, integration and innovation on small and not-so-small islands with particular reference to Samoa

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Abstract

Investigations on small Polynesian outliers have illustrated how difficult it can be to identify archaeological evidence of intrusion, or to interpret the effect of any intrusion on the resident populations. In Samoa, the still meagre amount of artefactual and faunal remains from archaeological excavations adds to these problems. A review of the known Samoan archaeological sequence finds little or no evidence of intrusion, apart from a probable post-settlement introduction of pigs and dogs. This need not mean that Samoa was ever isolated from contacts with other islands.

In a recent paper, Addison and Matisoo-Smith (2010) proposed a “Triple-I Model” of intrusion, integration and innovation for the Samoan sequence. They suggested a possible arrival about 1500 BP of new people, who introduced new lineages of rats, dogs and chickens, new plants, new material culture, and new ideas, and tentatively proposed a route from the west through the low islands of Micronesia. Their paper stimulated me to think about the difficulties archaeologists face in identifying and interpreting evidence of intrusion and replacement, a subject that has long concerned me (Davidson 1970, 1974a). The present paper briefly considers problems in interpreting archaeological evidence of intrusion (or lack thereof) in several Polynesian outliers, and then reviews current evidence about aspects of the archaeological sequence in Samoa.

Polynesian outliers

Archaeological research on Polynesian outliers has been driven, not surprisingly, by the fact that these small islands in the geographical regions of Micronesia and Melanesia are today inhabited by people who speak Polynesian languages. Identifying the arrival of Polynesian speakers has been a major objective, which has, however, proved very difficult to achieve, as the following examples show.

Nukuoro is the northernmost of the known Polynesian outliers. It is a small atoll between New Britain and Pohnpei, with only a few hundred inhabitants. In the 1870s, the German ethnographer, Kubary, recorded traditionally remembered canoe arrivals from some 17 different islands stretching from Yap to Rotuma. Some of the arrivals stayed

and intermarried, some were killed, some were banished, some introduced new dances; those banished had introduced a new kind of murder (Kubary 1910: 6–8).

Despite these historical accounts, the known archaeological sequence on the atoll, beginning about 1200 years ago (Davidson 1971, 1992) shows no evidence of intrusion or new arrivals apart from the late appearance of the serrated-edged pearl shell coconut grater. This archaeological sequence falls within the timeframe within which linguistic models would expect the present Polynesian language and its immediate ancestor to have been spoken on the atoll. Although Kirch (2000: 179–180) considered that “there is no reason why the Nukuoro sequence should not be regarded as ‘Polynesian’ from bottom to top”, my own conclusion was that if Nukuoro had been uninhabited at the time of European contact it would never have been recognised as a Polynesian outlier.

Leach and Ward (1981) faced a similar situation on the nearby Polynesian outlier of Kapingamarangi. Although Emory (1965: 1–2) considered it exclusively Polynesian, earlier German ethnographers had found what they considered Melanesian and Micronesian as well as Polynesian influences (Eilers 1934: 155). Leach and Ward (1981: 93–97) had difficulty in suggesting a likely origin for the people. They found no evidence of intrusion in the archaeological sequence, although they pointed to ethnographic evidence of a type of food preservation and a method of roof thatching that suggested contacts, perhaps drift voyages, from Kiribati or the Marshalls.

The Polynesian outliers in the Santa Cruz group have much longer archaeological sequences than Nukuoro and Kapingamarangi, all beginning with ceramic occupations early in the first millennium BC. According to orthodox linguistic reconstructions, the present Polynesian languages cannot be descended from those of the original inhabitants, and must be more recent intrusions. The complexities of identifying Polynesian influences in Tikopia were well summarised by Kirch and Yen (1982: 341–342), who were able to identify some specific examples of Polynesian influence during their Tuakamali Phase (AD 1200 to 1800), without excluding possible earlier influences. In Anuta, however, it was still necessary to fall back on linguistics and oral tradition rather than archaeological evidence for Polynesian arrival (Kirch 1982: 251).

In Taumako there is archaeological evidence of contact and intrusion from both east and west at various times –

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stone adzes from Samoa, obsidian from Vanuatu, the relatively late appearance of terebrid and mitrid adzes, which spread through Micronesia and parts of Melanesia around 1000 years ago (Intoh 1999: 413–415), and other new items whose source is problematic (Leach and Davidson 2008: 320). It is not possible to identify a single point of introduction of Polynesian characteristics. Taumako is well placed to receive drift voyages from West Polynesia and there may have been many such arrivals. Again, if Taumako had been uninhabited at European contact, it would simply be seen as part of the Santa Cruz cultural area and the Samoan stone adzes interpreted as at best a result of trade, or more probably drift voyages. Detailed studies of skeletal remains from a large cemetery dating to the last millennium have struggled to identify the biological affinities of the population (Houghton 2008: 351–352).

These Polynesian outliers are all small landmasses with populations numbering in the hundreds, not thousands, and should therefore be susceptible to take-over by relatively small groups of new arrivals. Samoa is a much larger island group, which by 1500 BP should have had a population numbering many thousands. In the historic period, Samoans have been enthusiastic adopters of new plants and animals, new technology, new fashions, new songs and dances. It is not unreasonable to suppose that new introductions of perceived interest or benefit would have been just as enthusiastically adopted whenever they appeared. But is there any evidence for a package of new introductions at a particular point in time?

The Samoan sequence

The success of any intrusion about 1500 BP, as proposed by Addison and Matisoo-Smith, would depend at least partly on their suggestion that Samoa may have been settled somewhat later than Tonga and Fiji and may have experienced slow population growth and even abandonment for a time after initial discovery. I therefore first consider the question of early settlement and population growth before looking for evidence of continuity or change in subsistence, material culture, and ideas. Until human remains from dated archaeological contexts are available for study, little can be said about people themselves.

Early settlement

Apparent disjunctions between the archaeological sequences of Samoa, Tonga and Fiji have been a recurring topic of discussion for many years. During the first major archaeological research programme in Samoa in the 1960s, no evidence of Lapita settlement was found, in contrast to Tongatapu (Poulsen 1967, 1987) and Viti Levu (Birks 1973). This failure was to some extent explained by the chance discovery of the site beneath the lagoon at Mulifanua (Green 1974a; Jennings 1974). Green (2002) argued persuasively that there are almost certainly more Lapita sites, submerged or deeply buried, in Samoa. It is also highly likely that, as Clark (1996: 450) has argued, Lapita

decoration was abandoned in Samoa sooner than anywhere else. More recently, Rieth and Hunt (2008) also argued for a rapid abandonment of Lapita decoration.

Yet there is still a perceived “weak Lapita signature” in Samoa (Addison and Matisoo-Smith 2010: 8). The chronometric hygiene approach to Samoan radiocarbon dates (Rieth 2007; Rieth *et al.* 2008) has provided the basis for a view that Lapita settlement of Samoa was minimal, and permanent settlement may not have occurred until c. 2500–2400 BP. However, Clark and Anderson (2009: 414) have presented thoughtful arguments against what they characterise as “Lapita avoidance of Samoa” in the wider context of West Polynesia and Fiji. They point out that there is evidence of continuous occupation of the relatively close smaller islands of Uvea, Futuna and Niuatoputapu from the Lapita era and that Kirch (1988) demonstrated close relationships in design elements and motifs between Mulifanua (Samoa), Sigatoka (Fiji), Uvea and Niuatoputapu. They argue that Samoan plainwares cannot be derived convincingly from any known prehistoric assemblages in the region (or from Vanuatu or New Caledonia) but “are likely to represent a local development, signalling that older ceramic sites are likely to be present in Samoa” (2009: 415).

In the 1960s, Green and his colleagues did not have a clear understanding of Samoa’s complex geomorphology. There was no thought of looking for submerged sites in the lagoon on the north coast of Upolu. It was mistakenly assumed that the low coastal flat at Lotofaga on the south coast of Upolu, chosen for a beach midden excavation, would have been there since people had been in Samoa, whereas in fact it was younger and yielded a sequence of only about the last 800 years (Davidson 1969). By 2011, the site and the flat it had occupied had eroded almost entirely away, illustrating yet another aspect of the problem of finding early sites in Upolu.

It was also assumed, probably wrongly, that the Falefa Valley in eastern Upolu was an optimal location for settlement. The central flat grazed by cattle was an optimal location for site surveying, but it is prone to flooding and probably never had the density of occupation of, for example, the northwestern part of Upolu. Yet plain potsherds were found in four of the seven sites excavated in the valley (Green and Davidson 1974: 69, 85, 96, 117–131). One of these, the plain pottery site at Sasoa‘a, was initially excavated because it had been a nineteenth century village, listed in a missionary journal. The Falefa Valley investigations suggest a significant population in eastern Upolu by 2000 BP. The scattered but fairly common occurrence of plain pottery sherds in other ‘inland’ locations, including the vicinity of Pulemelei on Savai‘i (Martinsson-Wallin *et al.* 2007: 51–56), Mt Olo in western Upolu and Moamoa inland from Apia (Green 2002: 137), also suggests a significant population by about 2000 years ago, even if *in situ* deposits continue to be elusive.

It is difficult to believe that Samoa, once discovered, would not have experienced steady population growth, and inconceivable that it might have been, for a time, abandoned. But even if this were the case and permanent

settlement began only about 2500 BP, by 1500 BP there should have a sizeable population.

Subsistence

In his review of settlement patterns in Samoa, Green argued strongly for continuity in Samoan subsistence, as in other aspects of its culture. As he noted, Samoa generally lacks the usual indicators of intensification and “early to mid twentieth century AD Samoan agricultural practices, for the most part, seem to have prevailed throughout the prehistoric sequence” (Green 2002: 147). In the mid twentieth century there were minor improvements towards labour saving, but the actual farming of village land, as described in Fox and Cumberland (1962), continued much as it may have done since the first canoes arrived.

This is of course negative evidence – no intensification – rather than actual positive evidence for any agriculture at all.

Burley (1998: 355) somewhat dismissively described Lapita agricultural activity in Tonga as “possibly limited to a low-energy swidden-type cultivation system” secondary to foraging in relative importance for settlement location, although he has subsequently questioned this assumption (2007: 194). Addison and Matisoo-Smith (2010: 6) cite various sources (including Green 2002) as evidence that “sites dating to after the abandonment of pottery in Samoa show intensive landuse patterns”. They also cite a personal communication to them from Burley for “large scale field clearance and agricultural intensification in Ha’apai by 1200 BP (date uncalibrated)”. It behoves us to be very clear about what is meant by intensification in Polynesia (Leach 1999). More clearance reflects more people but need not necessarily involve new people, new plants, or new horticultural techniques.

Samoa horticultural practices as described in Fox and Cumberland (1962) and observed by archaeologists working in Upolu and Savai’i in the 1960s did not appear to reflect intensification, but they provided Samoans with ample vegetable foods for subsistence and ceremonial occasions as well as supporting, for a time, a significant banana export industry. Carson (2006) emphasises the lack of intensification in recent Samoan horticultural practice.

Microfossil evidence of aroids, yams and bananas has been reported in Lapita sites in Vanuatu (Horrocks and Bedford 2005, 2010; Horrocks *et al.* 2009), and taro and lesser yam in the Lapita era in Fiji (Horrocks and Nunn 2007). Fall (2010) has presented evidence for the appearance or increase in pollen of a variety of plants associated with cultivation in Vava’u and Ha’apai during the first millennium BC, including taro. It is therefore reasonable to suppose that plants such as taro, yam and bananas would also have arrived in Samoa with the initial colonists. It is no longer necessary to infer the presence of domestic plants from artefacts assumed to be vegetable peelers, although these were present in the plain pottery deposit at Falemoa on Manono (Janetski 1980: 125–127). Of course there may have been new additions, particularly of new varieties, but new introductions sufficient to cause major change in practice seem unlikely. A low island route

through Micronesia is particularly unlikely, as atolls do not generally support the cultivation of plants most important in Samoa apart from breadfruit, which is normally grown in settlements rather than in gardens. Kava and paper mulberry are not normally found on atolls, and are also not grown to any extent in gardens. I think it unlikely that introduction of new plants led to intensification of gardening.

Archaeological evidence about fishing, fowling and the presence of commensal animals is still limited, as Green (2002: 146–147) noted. This is partly because of the poor survival of bone and shell except in sandy coastal sites. Even coastal sites, however, have generally not been very productive of faunal remains. Evidence from To’aga on Ofu in Manu’a does not suggest change (Nagaoka 1993: 207), apart from loss of some wild bird species (Steadman 1993). But as Steadman notes, this was not comparable to the initial impact of humans in other island groups. This in turn suggests that To’aga was not a site of first footfall in Manu’a.

Without studies of plant microfossils and in the absence of field evidence of horticultural systems, pigs were pressed into service as proxies for horticulture in Lapita sites (e.g. Green 1979: 37). But it is perfectly possible to have horticulture without pigs or, indeed, pigs and chickens without horticulture. At present it looks as if chickens may have been the only domesticates present in the earliest part of the Samoan sequence, with pigs and dogs arriving later. The introduction of pigs, in, particular, would have been highly significant, but need not have been part of a package. The known distribution of pigs in Micronesia (Wickler 2004: 32) suggests that they are very unlikely to have arrived in Samoa by a Micronesian route.

Material culture

At the end of Green’s programme in Samoa, he was able to describe Samoan plain pottery in some detail (Green 1974b: 245–253) and put forward a clear account of the development of the Polynesian stone adze kit that was then taken to Eastern Polynesia and further elaborated there (Green 1974b: 253–265). He showed that shell adzes and a few stone adze types were present in Tongan Lapita sites but not in Samoa. Some stone adze types found in the Tongan sites were also present in Samoan plain ware sites, but the Samoan plain ware sites also had a range of new types that were not present in Tonga.

Green could say very little about durable fishing gear or personal ornaments, the other main classes of artefact that have proved useful in Lapita archaeology elsewhere and in East Polynesian archaeology. Bone and shell artefacts do not survive in the volcanic soils in which most of the excavations by Green and his team took place. Since then, there have been several excavations in Samoan coastal sites with reasonable conditions for preservation of artefacts made from shell, bones and teeth, although artefacts have still been sparse. The picture that seems to have emerged, however, is one of loss of durable material culture, rather than intrusion of new items that are archaeologically visible.

Hiroa (1930: 418–523) devoted more than 100 pages to

fishing in his detailed work on Samoan material culture. There are 4 pages on the use of the octopus lure, 2 on walled weirs, and about 10 on trolling lures. The rest is about fishing practices, nets, traps, and other perishable items. Simple one-piece shell fishhooks were part (probably a minor part) of the initial Samoan fishing kit and lasted in small numbers for some time, with examples from To‘aga in Manu‘a (Kirch 1993: 160–162), Lotofaga on Upolu (Davidson 1969: 244, 247) and Potusa and Falemoa on Manono (Janetski 1980), but seem to have disappeared before Hiroa’s time. Archaeological evidence of the typical Samoan ethnographic trolling lures, well described by Green (1974b: 271–274) on the basis of nineteenth century collections in the Peabody Museum at Salem and the Smithsonian, is limited to what are quite possibly historic period examples at Potusa (Janetski 1980) and a doubtful fragment from a relatively recent context at Lotofaga (Davidson 1969: 244, 247). At this rate, the only durable item of fishing gear to have survived throughout the Samoan sequence may prove to be the octopus lure, represented archaeologically by the cowrie shell caps, whose identification is often tentative (Kirch 1993: 162; Davidson 1969: 244; Janetski 1976: 71–73, 1980: 124, 125), but not by the characteristic stone sinker of recent times.

Personal ornaments also present a picture of loss as much as gain. Some of the shell ornaments found in Lapita sites elsewhere have been found in plain pottery contexts, notably narrow shell rings, usually interpreted as arm rings, the occasional shell “bead” and two bone or ivory “beads” (Kirch 1993: 162–165; Janetski 1976: 72, 73, 1980). At some point, these all disappeared. For the ethnographic period, Hiroa (1930: 615–634) described an array of items of perishable material – wooden and coconut fibre combs, ornaments of human hair, leaves, flowers, fruit, seeds, and feathers – and only a tiny component of shells for necklaces. Although for the archaeologist this represents loss of potential evidence, Krämer was moved to begin his account of Samoan ornaments as follows: “There is no ornament more beautiful than one fashioned of flowers and leaves, such as probably best developed on the South Sea islands (1995: 329).

The main durable ornaments in the ethnographic record are the pale fuiono of nautilus shell units that was part of the tuiga headdress, and for which the shells, according to Krämer (1995: 335), came from Tonga, and the ‘ula lei – the whale tooth necklace, for which the teeth often came from Fiji (Krämer 1995: 335). Krämer thought that the pale fuiono was an innovation unique to Samoa. The ‘ula lei potentially has wider connections, to East Polynesia as well as elsewhere in the central Pacific. But no evidence of either seems to have been found yet in Samoan archaeological sites.

An important feature of much of the Samoan material culture sequence is that Samoans made very little use of shell for artefacts, apart from so-called scrapers and peelers. It is unclear whether the alleged broken tridacna shell adze from Potusa (Janetski 1980: 123–124) is actually an adze; the only other reported shell adze is a small example,

probably Cassis shell, from To‘aga (Kirch 1993: 158). Hiroa (1930: 353–354) reported only two examples in the Bishop Museum collections. There is no sign of the terebrid and mitrid adzes using the aperture end of the shell, which swept through much of Micronesia and parts of Melanesia about 1000 years ago (Intoh 1999: 413–415). Nor are there yet any examples of the terebrid tool with the bevel on the apex end, which flourished in early East Polynesian sites and has a sporadic distribution elsewhere, including in a Lapita site in Ha‘apai (Davidson *et al.* 2011). As noted above, the early shell ornaments seem to have petered out.

In the case of material culture, then, the stone adze sequence can be seen as very strong evidence of continuity, with no reason to suppose that innovations were due to anything other than Samoan craftsmen availing themselves of the abundant supplies of high quality raw material. In other aspects of material culture – ceramics, shell fishhooks, shell ornaments – there is loss (and ethnographic evidence of replacement with perishable materials) but no sign of intrusion. However, the failure to reintroduce shell artefacts of almost any kind is important negative evidence. The Samoan propensity for perishable ornaments might indicate atoll influence, but actually, in the ethnographic period the people of Kiribati, for instance, made greater use of shell for ornaments than Samoans did (Koch 1965: plates 16, 18–20). Shell tools and shell ornaments were also a significant feature of the material culture of the high islands of Kosrae and Pohnpei as well as the atolls of the Caroline and Marshall Islands.

A final example of the frustrating lack of evidence of durable material culture in Samoan archaeological sites concerns tattooing, which the Samoans elevated to such a high art. Tattooing is thought to go back to Lapita times (Green 1979: 40), although the evidence is fairly flimsy. Three bone tattooing chisels from To-1 on Tongatapu are cautiously described by Poulsen (1987 (I): 207) as highly likely to be of an early period. As far as I am aware, no archaeological evidence of tattooing has yet been found in Samoa.

New ideas

The introduction of new ideas is particularly difficult for archaeologists to document. New ideas about society can be tentatively inferred from monuments, patterning of sites, shapes of houses and so on.

Round or oval houses were and are an important feature of Samoa culture, which had much to do with social ideology, as house architecture so often has. Barnes and Green (2008) have convincingly demonstrated that the fale afolau (a long house with rounded ends) was a nineteenth century introduction from Tonga. It was originally introduced as a Christian chapel design and serves as a good example of introduction (from Tonga), innovation (Samoan modification) and integration (it is now widely thought to be an indigenous Samoan form). On the other hand, the round or oval form is well documented from a variety of late prehistoric sites (Davidson 1974b: 232–236). Green (1974c: 111–113) presented plausible evidence for a small oval

house at Sasoa'a, dating to the first century AD. This suggests that oval houses have a very long history in Samoa, extending back to the period of use of plain ware ceramics. This type of house is highly unlikely to have been introduced to Samoa via Micronesia.

The appearance of monumental architecture is surely the outstanding innovation in the Samoan archaeological sequence, and is closely paralleled in Tonga. In both Samoa and Tonga, it takes the form of earthen, stone, or stone-faced mounds, which are sometimes very large, although, as Golson pointed out in 1957: "whereas in Tonga the mound was raised and elaborated in honour of the dead, in Samoa it was pressed into the service of the living" (Golson 1969: 14). Round and square or rectangular mounds are found in both Samoa and Tonga and large ones in both groups sometimes have access ramps, but the so-called star mound appears to be a unique and probably quite late Samoan innovation, highly unlikely to be an introduction from elsewhere. According to Clark *et al.* (2008: 1007), "the scale and density of monumental works at the chiefly centre of Lapaha [on Tongatapu] is unprecedented in Polynesia". However, it does not necessarily follow that Tonga was where monumental architecture first appeared in West Polynesia.

Monumental architecture, not surprisingly, is not a feature of atoll societies. The appearance of monumental architecture not only in Samoa and Tonga but in other parts of the Pacific including Pohnpei and Kosrae, Fiji and New Caledonia, is a really important topic in Pacific archaeology (Clark and Martinsson-Wallin 2007). On present evidence, however, there is no reason to interpret the Samoan evidence as part of the sort of Triple-I model proposed by Addison and Matisoo-Smith. The appearance of earthwork fortifications in Fiji, Samoa and Tonga is another innovation deserving of more investigation. Again, however, it is likely to have been an innovation within Fiji and Western Polynesia, rather than an introduction from elsewhere.

Conclusions

Examples from several Polynesian outliers show that Pacific island communities have probably always been exposed to new arrivals of people and ideas, most of which are not reflected in the archaeological record. When evidence of intrusion is identifiable, it is not easy to interpret in terms of numbers of people and extent of influence.

The early part of the Samoan archaeological sequence is still poorly known. However, the extent of distribution of plain pottery on Upolu, at least, suggests uninterrupted population growth and spread commensurate with initial Lapita settlement.

There is very little evidence of anything other than continuity in Samoan subsistence practices. Plant fossil evidence of Lapita introductions of major staple crops such as yams, taro, and bananas elsewhere make it likely that these were introduced to Samoa, too, by the initial settlers. Increased clearance of land may simply reflect increasing

population and does not necessarily indicate intensification or new crops. Pigs and dogs do, on the present fairly limited evidence, appear to be later introductions, although their date of arrival is not yet determined. Faunal collections of all periods are still fairly limited.

The material culture sequence reflects loss in several categories of durable items, rather than new introductions, while the stone adze sequence provides strong evidence of continuity and local innovation. There is nothing to suggest introductions of new material culture from the west. The pale fuiono shell ornament, one of the very few durable innovations other than adze forms, seems to be of Samoan origin.

There is some evidence to suggest that the Samoan oval house has an antiquity of more than 1500 years, while the fale afolau has been shown to be a nineteenth century Christian introduction from Tonga, providing an excellent example of the intrusion, innovation and integration of a single architectural and religious idea. The appearance of monumental architecture and earthwork fortifications during the last millennium, not only in Samoa but elsewhere in the central Pacific and beyond, were major developments whose origins remain uncertain. Samoan innovation in this field is reflected by the so-called star mounds.

The Triple-I model proposed by Addison and Matisoo-Smith challenges archaeologists working in Samoa to examine their existing evidence carefully and seek new evidence to test the model. The idea of an influential package of introductions, leading to major change, has always appealed to archaeologists. However, a series of incremental small changes from a variety of sources over a longer period of time may be closer to historical reality. Canoes from many different islands have probably arrived frequently in Samoa over the centuries leaving little or no trace in the archaeological record. Pigs, new cultivars of existing crops, new dances, and new styles of perishable textiles and adornments, for example, would be readily adopted, while new lineages of rats could have jumped ashore from wrecked canoes. It is likely that there has been regular swapping of ideas and minor innovations between Tonga, Samoa and eastern Fiji as long as they have been inhabited by people. But evidence of a significant arrival at one point in time of new people, plants, animals, material culture and ideas has yet to be produced.

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Responses

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It is encouraging to see that the ideas we published in these pages in 2010 have stimulated direct response and discussion of issues we think are important in continuing to develop understandings of the processes involved in the initial settlement and subsequent prehistory of West Polynesia. We commend and respect Davidson for her direct discussion of some of our interpretations and inferences, as well as for her many decades of involvement in the archaeology of the region. We think that this kind of direct approach is exactly the way for understandings of regional prehistory to advance. In this spirit, we are happy that the editors of *Archaeology in Oceania* have asked us to respond to Janet Davidson's thoughtful paper.

One of the themes of our paper was that, while evidence is still fragmentary and there is much more to know, for several years we have been increasingly unhappy with trying to force new data into a model that seems unable to accommodate them. Surely, there are many ways to interpret the partial record that we have in West Polynesia. We attempted to offer another way of perceiving the available data, a way that we think may better accommodate both the older research findings Davidson cites as well as newer findings archaeological as well as biological/genetic. Below, we will attempt to address the topics Davidson raises in the same order as in her paper.

The Outlier analogy

We find that Davidson's portrayal (which we have no objection to) of the difficulty in identifying “intrusion” highly intriguing. As we understand it, her argument is that the Outlier cases exemplify the difficulty of archaeologically recognizing intrusion, even massive intrusion and culture change (as is hypothesized for the Outliers). This is perhaps where DNA evidence can be most valuable as the sudden appearance of a new lineage or lineages can be indicative of an intrusion or at least new influences from different populations.

Plainware-using population growth rate, size, and density

Davidson states that “Samoa ... by 1500 BP should have had a population numbering many thousands”. Perhaps this statement accurately reflects the prehistoric reality of Samoa. However, “many thousands” spread over 9 islands with the second largest land area in Polynesia (NZ excluded) does not necessitate high density in any one place. Smaller islands such as those in the Manu'a Group likely

never had huge populations, even when at the limits of their carrying capacity.

Although small islands such as Manono and Apolima may have always had small populations as well, their proximity to ‘Upolu/Savai’i suggests that they could call on resources from those islands when needed. It can be inferred that the more remote location of the islands of Manu’a (~100 km from Tutuila, the nearest landmass) necessitated that their population levels be maintained below the islands’ maximum carrying capacity without recourse to outside assistance. If it is a question of low overall population being a requisite for significant intrusion, Manu’a seems the most likely place in Samoa. As we suggested in our 2010 paper, is there perhaps not something to the oral traditions from around the region that give primacy to Manu’a as the origin and “birth-place” of Polynesians? Manu’a may have been an ideal setting for an intrusive population to take hold and gain strength before attempting to spread to larger islands.

New introductions?

Davidson asks, “is there any evidence for a package of new introductions at a particular point in time?”. We suggested in our 2010 paper, that yes, there is at least sufficient evidence for archaeologists in the region to be considering the possibility that there were, and to actively look for such evidence. The adage “if I didn’t believe it, I wouldn’t have seen it” pertains here. If we are only looking to confirm what we already know (e.g. the current model), then it is likely that we will find exactly that.

As Davidson’s Outlier discussion suggests, archaeological evidence alone may not be enough – incorporating as many lines of evidence as possible (e.g. biology, oral history, etc) will help in refining our understandings of the past. The unquestionable appearance of dog remains in both Near Oceania and Polynesia at some point *after* 2000 BP is just one. The genetic evidence, while still relatively limited in terms of well-dated material, also suggests that new lineages of chickens and *Rattus exulans* may have been introduced to the Pacific at some point after Lapita but before expansion into East Polynesia.

What we attempted in the 2010 paper was to give an alternative model that would allow archaeologists in the region to find data to test and, if necessary, refute. We would welcome elements of the new model being falsified. In our view, this is how scientific understanding of phenomena moves forward. In short “please, prove the model wrong”. This should lead to new suggestions, new models and will refine our understanding of the reality of the past. We address Davidson’s specific points about introductions in Samoa below.

Samoa’s radiocarbon chronology

Before addressing Davidson’s specific points, a word should be said about Samoa’s corpus of radiocarbon dates. The first significant archaeological work in Samoa was done on ‘Upolu Island in the 1960s (Green and Davidson 1969, 1974), or more precisely with Golson’s preliminary field-

work in 1957 (Golson 1969). This research was foundational to understandings of Samoan and regional prehistory.

In the intervening decades many advances have been made both in the laboratory techniques of radiocarbon dating and in the selection of dating samples and their interpretation. Unfortunately, several of the dates from key sites underpinning the ‘Upolu chronology do not pass modern chronometric-hygiene protocols and remain ambiguous in their interpretation (Rieth 2007; Rieth and Hunt 2008; Rieth, *et al.* 2008).

The Lapita Signature in Samoa

The geographical and temporal extent of Lapita in Samoa has been addressed elsewhere (Addison and Morrison 2010) and there is little need to reiterate the arguments here. Until there are more actual data, the scant evidence currently available can continue to be interpreted in different ways. However, the fact remains that in the four decades since the discovery of the Mulifanua Lapita site, no research team has succeeded in finding another Lapita site in Samoa, even using research designs incorporating more sophisticated understandings of geomorphological processes in Samoa.

Again, we point out that current evidence indicates a weakening Lapita signature northward from Tongatapu to Samoa. Although geomorphological explanations can still be legitimately posited for absence of documented Lapita sites in Samoa, such is not the case in Tonga (Dickinson and Burley 2007). Decades of research there by Burley and colleagues have documented the extent and abundance of Lapita sites on Tongatapu and in Ha’apai and Vava’u and the northward diminishing pattern is clear (Burley 2007; Burley and Connaughton 2007). Kirch’s seminal work on Niutopotupu in the 1970s documented Lapita pottery on that island, located midway between Vava’u and Samoa (Kirch 1988). More work on the extent, abundance, and chronology of Lapita on Niutopotupu is needed to place it within the context of current questions about the spread of Lapita in West Polynesia.

Conclusion

Again, we applaud Davidson’s willingness to engage in this discussion. It is exactly this kind of interchange that we had hoped to encourage with our 2010 *Archaeology in Oceania* paper. Word limits imposed by the editors prohibit us from exploring in detail all the topics Davidson raised, but we hope that we have been able to include enough herein to warrant further discussion of these topics.

As Samoan say ‘*ia so’o ula le talanoaga* – may the conversation continue, and be joined by others!

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Prehistoric movement did occur along the Micronesian route in prehistory. For example, the 'Oceanic lateen' sailing technology, which probably transformed west Polynesian interaction around AD 1500, came almost certainly from Asian contact with West Micronesia and then down the Micronesian route (Anderson 2000). Nevertheless, I join Davidson in arguing against the proposal at issue here, that low-density occupation of Samoa was terminated by substantial intrusion from Micronesia 2000–1500 BP (Addison and Matisoo-Smith (2010)). I comment particularly on the evidence of introduced animals, but first, two other points.

The Triple-I model is not "the 'right answer' to the question of Polynesian origins" (Addison and Matisoo-Smith 2010: 9), because it is not an answer at all. It only lists ways in which the ubiquitous phenomenon of culture change can occur (whether in colonization, cuisine or cars) and encourages the illusion that putting data into categories generates self-evident explanation. In fact, explanations must be derived otherwise, through consideration of cultural and contextual dynamics, as of migration, demography, economics, seafaring, atoll emergence, etc.

The proposition that Lapita colonization failed in Samoa (Addison and Morrison 2010) is plausible theoretically (small colonies often fail demographically) and empirically (only one Lapita site known), if Samoa is considered in isolation but, in addition to points made by Davidson, it can be noted that sustained movement into the interior began about the same time in Fiji as in Samoa, ~2200 BP, suggesting that Samoa also had lengthy preceding coastal settlement.

Turning to introduced fauna, is there evidence that the dog reached west Polynesia through Micronesia? Addison

and Matisoo-Smith (2010: 5-6) say that the post-Lapita dog lineage, Arc 2, occurs throughout Polynesia, but they imply that it was not in Micronesia, contrary to their current hypothesis. Further, the earliest radiocarbon dates associated with dog bone in the central and eastern Micronesian islands are 1500–1800 BP (Anderson 2009a), but they are 2800–2300 BP at Naitabale in Fiji (Nunn *et al.* 2007), and 2800 BP at Tongoleleka, Tonga (where, however, they are associated with Polynesian plainware, Steadman *et al.* 2002). There are questions about stratigraphic disturbance and whether contextual dates refer accurately to the bones in both the Polynesian and Micronesian evidence. The pig, at least, is agreed as a Lapita introduction.

If there are two chicken lineages, one of Lapita origin and another evident only after ~1200 BP (Addison and Matisoo-Smith 2010: 5), then would not chickens dated to about 2000 BP in Micronesia (Storey *et al.* 2008: 252) have been of the Lapita lineage and the type taken to west Polynesia, rather than a putatively recent Asian lineage?

Rattus exulans was distributed about as widely and early as people because of its invasional characteristics (especially the capacity to exert colonizing-propagule pressure) and maritime transportability (Anderson 2009a). Yet *Rattus exulans* bones date relatively late everywhere in Micronesia, occurring only about 1200–1000 BP in the Marianas and Marshalls. However, *Rattus tanezumi* (possibly *Rattus rattus*) occurs in the Carolines after about 1700–1500 BP and in Palau about 1000 BP (Clark 2005). Thus it might be conjectured that a human migration of any resolution and consequence through Micronesia ~1500 BP should have carried *Rattus tanezumi/rattus* to Samoa. On current evidence it did not do so. The *Rattus exulans* haplogroup III found throughout Polynesia and Micronesia could be a Lapita or post-Lapita introduction (Matisoo-Smith *et al.* 2009: 472; Anderson 2009b: 749) but there seems no reason to prefer a Micronesian dispersal route and, given the late occurrence of *R. exulans* in Micronesia, some reason to refrain from doing so.

Overall, the evidence of domestic animals is too uncertain to be employed with confidence in anything but the most general propositions about Pacific migration. Few radiocarbon dates are directly upon bones, some bone dating has been incompetent, and there is little agreement about genetic lineages or sources. Importantly, as most genetic data come from modern samples, it is difficult to distinguish translocation from drift or bottleneck effects within the region. As Matisoo-Smith *et al.* (2009: 471) acknowledge, 'only analyses of archaeological *R. exulans* remains can resolve this question'. Just so, and for all faunal introductions the answers to origin, migration and timing lie in research that has still to happen.

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In their recent *Archaeology in Oceania* paper, Addison and Matisoo-Smith (2010) throw down the gauntlet to Oceanic archaeologists, proposing a population migration into Samoa ca 1500 BP with significant implications for the origins of Polynesian culture. Janet Davidson accepts that challenge, arguing against the alleged intrusion, further questioning our abilities to recognize such an event in Oceanic archaeology more broadly. To illustrate the latter, she turns to a small-island analogy of the Polynesian outliers. Here, despite independent evidence for population infusions, the archaeological record remains silent. She marshals her skepticism on the Samoan case through a review of early settlement, subsistence patterns and material culture, providing an argument contra Addison and Matisoo-Smith. As she implies, and in this I concur, there is every reason to believe inter-island voyaging was a common occurrence by 1500 BP. The arrival of new ideas, pigs, and new lineages of rat within this context does not require a substantial influx of new people, nor do they necessarily signal a break in Samoan/Polynesian cultural continuity.

Samoan prehistory is not simple, and Davidson identifies several issues that underscore this statement. Perhaps most important is the almost total absence of pre-2500 BP sites, and a sparse settlement landscape for two to three centuries

following. Assuming this to be a consequence of geological volatility on the Samoan archaeological record, following Green (2002), she consequently states "it is difficult to believe that Samoa, once discovered, would not have experienced steady population growth, and inconceivable that it might have been, for a time, abandoned". Given the intensity of search by several of Oceania's leading archaeologists over the past half century, including Davidson, as well as abundant resource management programs in American Samoa, not all of her colleagues are so convinced (Reith *et al.* 2008). There is, in fact, a growing sense that absence of evidence is evidence of absence. Supporters do not deny possibilities for future discovery of Lapita or early Plainware sites, but predict these sites to be few. To the south, in the 71 island Vava'u group of northern Tonga, a notable parallel exists. Here a small resident population in the Lapita Phase continued to remain marginal until ca. 1600 BP or later (Burley 2007). Why has yet to be determined, but geomorphological processes are clearly not the answer (Dickinson and Burley 2007).

In contemplating Davidson's review of Samoan settlement, subsistence and material culture, I am struck by how different things seem to be from Tonga and other island groups in West Polynesia until late prehistory. There is, for example, no counterpart for the post 2500 BP Samoan Plainware ceramic complex in the region as Davidson notes. The paucity of shell artifacts in the Plainware Phase, an almost unique adze kit, and other variations similarly imply a developmental period in isolation. A recent analysis of Tongan and Samoan volcanic glass geochemistry independently supports this type of interpretation (Burley *et al.* 2011). As that study further intimates, it may be time to reevaluate Samoan prehistory on its own terms, one without the integrative framework of an ancestral Polynesian homeland common to West Polynesia. These issues, at the very least, facilitate questions requiring new ways of thinking. Addison and Matisoo-Smith fire the first volley.

Finally, Davidson's discussion of Samoan subsistence practices with reference to 13-year-old statements made by me on Lapita agriculture in Tonga seem unneeded, but serve as bait for clarification. Her use of the word "dismissively" in my description of Lapita agricultural activity is mystifying, though similar interpretation appears elsewhere (Davidson and Leach 2001). For the record, I have never denied Lapita an agricultural component. I have, however, characterized it as a low-level food production system implemented within an economic adaptation heavily reliant on reef foraging and the fishery. One only needs to dig a Lapita site in Ha'apai or Tongatapu to come to that realization. That this system persisted until at least 2000 BP seems also to indicate stability. After that time, evidence exists for a clearing of island interiors to create expansive dry land field systems capable of producing surplus. If this is not intensification in Davidson's view, so be it. On Ha'afeva Island in Ha'apai, pollen core data indicate this event was abrupt, occurring ca 1200 BP (as was cited pers. com. in the Addison and Matisoo-Smith paper noted by Davidson).

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In their paper on Polynesian origins, Addison and Matisoo-Smith (2010) challenge several assumptions underpinning the 'orthodox' model of Neolithic migration to Pacific Islands. The most significant of these challenges are: 1) the people who produced the Lapita culture (3000-2600 BP) did not contribute the biological and cultural foundations of the West Polynesian groups who colonised East Polynesia 1000-700 BP; 2) migrants from Micronesia arrived in Samoa around 1500 BP, and 3) the arrival of a new people in Samoa was accompanied by the introduction of new commensal animals and plants, forms of material culture and ideas that subsequently spread through West Polynesia and formed Ancestral Polynesian Society (APS)/Culture. Davidson's careful review does not find strong evidence, however, for the arrival of an influential package of introductions by a new people in Samoa and suggests APS likely formed within Fiji-West Polynesia.

As Addison and Matisoo-Smith (2010) note their 'rethinking' of Polynesian origins involves the resurrection of older ideas in academic scholarship such as the belief that Polynesians were relatively recent arrivals in the Pacific, that Polynesians migrated from Micronesia and were a people who did not make pottery (Buck 1958; Gifford 1951). Older ideas in scholarly fora are not necessarily bad, and may have been rejected because the paradigm/'orthodox' view to which they belong has been overturned, gone out of fashion or the ideas themselves contain logical flaws when set against emerging data sets.

Addison and Matisoo-Smith suggest that Lapita populations were closer to contemporary 'Melanesian' people than to modern 'Polynesian' people who were physically and genetically changed by a migration from Central/Eastern Micronesia at 1500 BP. Lapita expansion was clearly complicated and there is no obvious reason why a Lapita group from one part of the 4300 km dispersal range should be phenotypically identical to Lapita groups in another. The discovery of Lapita sites in New Guinea and

colonisation of Western Micronesia from different parts of Island Southeast Asia is evidence for a major phase of Neolithic dispersal that included many already occupied islands with resident populations in Central/Eastern Indonesia, New Guinea and the Bismarck Archipelago (a further linear 2500 km of territory).

We should not expect biological homogeneity among these early Neolithic populations nor in Lapita culture that existed for 200-400 years and which involved high levels of inter-archipelago mobility as suggested by regional and long-distance obsidian transfer. Studies of historic migration events indicate that migration volume is characterised by distance decay suggesting, in relative terms, the likelihood of greater biological variation in west Lapita compared to east Lapita (evident in west to east gene clines). To suggest, as Addison and Matisoo-Smith do, that the Lapita groups of Samoa and presumably also of Tonga and Futuna-'Uvea had a phenotype unlike that of contemporary Polynesians seems premature when there are as yet no securely dated Lapita remains from West Polynesia. The second point about the biological nature of Pacific populations was well made by Serjeantson and Gao (1995:177): "*Homo sapiens* is an evolving species ... founder effects, bottlenecks and mutations have resulted in a unique genetic profile in contemporary Polynesians ...". How has the phenotype and gene pool of the original inhabitants of West Polynesia been altered by evolutionary forces over ~3000 years? If we begin with the premise that the Lapita people of Samoa were similar to modern West Pacific people and later became 'Polynesian' then migration is an obvious and simple resort to explain Polynesian genesis. Alternatively, if Polynesians most clearly represent the original Lapita population, as in the 'orthodox' model, then West Pacific populations, so the thinking goes, must have been affected by post-Lapita 'Melanesian' migration (Clark 2009).

Intrusion/Migration did occur in the prehistoric Pacific with strong evidence in the last 1200 years (and possibly earlier) that interaction in Fiji-West Polynesia became more frequent, involved longer voyaging distances and had spread into the ethnological zones of Micronesia and Melanesia. Commensal plants, animals and ideas were widely mobilised during this time as seen in the arrival from Island Southeast Asia of rats and rice in the Marianas and pigs and rats in Palau, and in the Pacific by the colonisation of East Polynesia and 'Outlier' islands to the west and north of West Polynesia including Tuvalu, all of which likely mark the introduction of the double canoe with fixed mast and standing rigging that was critical to long-distance windward sailing (Anderson 2000). While I agree with Addison and Matisoo-Smith's contention that many elements of Ancestral Polynesian Society probably coalesced in the period 1300-700 BP, migration should not be the default explanation for the creation of Ancestral Polynesia Society nor for cultural change in Samoa's prehistory until we know more about the biological composition of the early inhabitants of West Polynesia and the selective forces that have affected its populations over three millennia.

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Reply

JANET DAVIDSON

I very much appreciate the gracious response from David Addison and Lisa Matisoo-Smith and the pertinent comments from Atholl Anderson, David Burley and Geoff Clark.

The discussion touches on a number of issues of wider interest to Pacific archaeology beyond Samoa. First is the theoretical one of how we organise our data and test our hypotheses – matters which often receive too little attention in this part of the world. Trying to force data into what may no longer be a useful paradigm is obviously not sensible. But as Anderson argues, models are not necessarily the answer either. Collegial debate of the present kind can only be beneficial.

Secondly, the Addison and Matisoo-Smith model and the responses show how far Pacific archaeology has come since my first fieldwork in Western Samoa in 1964. Material culture, which underpinned so much Polynesian archaeology at that time, has received only brief comment in the responses, touching on pottery and the apparent lack of contact between Tonga and Samoa on the basis of material culture evidence (Burley). Historical/ethnographic evidence may provide a cautionary tale relevant to the latter point. It is possible that in the remoter past, as in the early historic period, what Tongans wanted from Samoans was fine mats not adzes, and what Samoans wanted from Tongans was Fijian red feathers and large Tongan sleeping mats and barkcloth not obsidian (Kaeppler 1978: 249; Davidson 1978: 385).

Much of the thrust of the Addison and Matisoo-Smith model and the responses is concerned with the people themselves, their plants and domestic animals, and specifically with genetics. This is a field that simply did not exist when the old paradigm was developed – a paradigm that drew heavily on historical linguistics, often ignoring Biggs' warning at the Sigatoka Conference in 1969 of the dangers in "the application of linguistic data to problems of prehistoric movements of people in Polynesia" (Biggs 1972: 143).

At present, as Anderson and Clark note, there is a serious lack of suitable archaeological material for genetic study, particularly human remains, but also animal and plant remains. The possibilities are exciting. It is up to archaeologists to find the material that is needed. As Addison and Matisoo-Smith indicate, this should help with the Outlier problem, as well as with our understanding of Samoa and Western Polynesia. Here as in other areas of study we should not underestimate the complexities, as the responses of Clark on people and Anderson on domesticates show.

More and better data will eventually resolve the question of almost no Lapita in Samoa. I am not yet willing to accept that absence of [Lapita] evidence is evidence of absence, as Burley suggests. The difficulty of finding Lapita sites on the large islands of the Fiji group and the recent transformation

of Vanuatu from a Lapita gap to an important Lapita centre suggest that all may not yet be lost in the Samoan case. I accept that Lapita decoration probably disappeared earlier in Samoa than in most or all other island groups and suspect that there is a great deal of plain pottery still to be found in Upolu and Savai'i. These large islands have not been thoroughly surveyed. It is wise to remember that both Mulifanua and Vaialele were revealed by non-archaeological works when an archaeologist or an interested lay person just happened to be present. And as Anderson points out, movement inland seems to have begun at about the same time in Samoa as in Fiji.

The paucity of evidence makes it difficult to do more than speculate on population density at any given time. It may be that an incoming group could have an impact on a small island in the Manu'a group, as Addison and Matisoo-Smith suggest, but that would not ensure that their genes and introductions could take over the rest of the archipelago.

I take the point made by Addison and Matisoo-Smith that I have continued to rely on some radiocarbon dates that do not meet the requirements of chronometric hygiene. This is an ongoing problem in Pacific archaeology and I hope that it may yet be possible to submit comparable samples from old excavations in Samoa to resolve at least some of these 'ambiguous' dates.

I also note a polite reference to my citation of older literature. The sad fact is that the old monographs still provide most of the data on material culture and faunal remains, with which much of my paper is concerned. Recently published summary and review papers, by their nature, do not carry this sort of information.

An important aspect of the subsistence debate – the extent to which Samoans and Tongans at various periods relied on horticultural produce for a significant component of their diet – may also be resolved by analysis of the remains of people themselves. Quantification of the proportion of plant foods in the diet of archaeological populations has been notoriously difficult until the application of isotope analysis to human remains. I had interpreted Burley's various comments on this subject to imply that there was a major switch in Tongan diet from a primarily 'strandlooper' diet with a minor vegetable component to a much stronger reliance on plant foods and significantly less marine food. Much better data on well dated and thoroughly analysed middens in Tonga might throw light on this question. Even so, impressively large shell middens do not necessarily equate to a very high proportion of protein in the total diet. My point was that low-key swiddening in Samoa was, at least until very recently, capable of providing the sort of surplus needed for major ceremonies, particularly the installation of high chiefs, and that there was no reason to suppose that there had been any change from a system introduced by the initial settlers. I did not actually see anything in Tonga in the 1960s to suggest gardening there was more intensive than in Samoa. The spread of horticulture is a tricky aspect to explore rigorously. How do we test whether increased clearance of land simply reflects the application of existing

cultivation techniques to a larger area to feed a larger population (and provide surplus for more and larger ceremonial events) or whether it reflects some kind of 'intensification' of practice, perhaps driven by increasing stratification?

I conclude by taking up Burley's suggestion that we should perhaps "reevaluate Samoan prehistory on its own terms, one without the integrative framework of an ancestral Polynesian homeland common to West Polynesia". If we set historical linguistics aside, it is easy to envisage separate groups colonising Tonga and Samoa from different parts of the wider Lapita region. On the world scene, some settler groups strive to replicate their home society (or an idealised version of it), while others set out to found a brave new world. For my presentation in Apia in 2011, I toyed with the idea of a humorous account of how Tongan settlers tried to replicate what they had left, while the colonists of Samoa sought to break away from the hidebound traditions of their homeland. This is, of course, fiction if not fantasy. But perhaps we should be seriously considering abandoning Ancestral Polynesian Society as it stands, before resorting to Micronesian invasion of Samoa.

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