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## Are islands islands? Some thoughts on the history of chalk and cheese

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The islands of the great Southern Sea comprising those which are in the neighbourhood of the Indian Continent, and the clusters which extend into more distant spaces in the ocean, present a field of enquiry extremely interesting to the natural historian of mankind. These insular countries are distributed through almost every variety of climate, and contain abundant diversity of local situation; therefore they afford us an opportunity of observing whatever influence physical causes may be supposed to exert over our species. James Cowles Prichard (1813:248–249)<sup>1</sup>

From continental to island life the change for the worse is very great with respect to opportunities and incitements to progress ... every nation upon a continent had one or more contiguous nations between whom and itself there was more or less of intercourse. Amongst contiguous nations there would be a free propagation of arts and inventions, which would tend to the general advancement of society throughout the entire area in which these influences are felt. Nations are apt to share in the more important elements of each other's progress.

On the other hand, the islands of the Pacific, except those adjacent to the main land, may be likened to so many cages in which their insulated occupants were shut in from external influences, as well as denied a knowledge of the uses of flocks and herds and of the principal cereals. Intercourse, at most, was limited to the inhabitants of particular groups of islands, who were thus compelled to sustain their national growth upon the development of their own intelligence exclusively, and without the great instruments of progress afforded by continental areas. Lewis Henry Morgan (1868:448–449)<sup>2</sup>

### Introduction

I append these two quotations to show that, in island archaeology at least, there is nothing new under the sun. They provide a rich source of tropes that remain with us as theoretical postulates of island singularity: the contrast with more complex continental situations, climatic and ecological variability, isolation through distance and navigational limitations that prevent 'external influences', evidence of cultural conservatism and/or loss, small populations and clear variations in social structure – all within a context of historical relatedness.

The first quotation, dating from 1813, shows that we can go back long before Darwin's account of his visit to the Galapagos Islands to find a Pacific foundational text – and in this case an explicitly anthropological one – that implies 'islands as laboratories' (possibly contra Kuklick's (1996) otherwise excellent discussion of the island model in British anthropology). In the extended quotation (Endnote 1), Prichard refers to 'informed voyagers' and in his book he makes much use of the literature generated by the voyages of Captain Cook and others. One could, therefore, trace back these ideas even further if required. I have seen neither of these quotations referred to so far in the underdeveloped literature on the history of island archaeology and the milieu from which it developed.

Island archaeology as a comparative topic has remained popular in conference titles and publications in archaeology for 50 years since some of the classic formulations of the 'islands-as-laboratories' trope were published in *Journal of the Polynesian Society* in 1957. Introduced by Margaret Mead, papers by Irving Goldman, Ward Goodenough and Marshall Sahlins set the scene for much subsequent discussion of cultural evolution in the Pacific and the wider world, particularly the better-known paper of Vayda and Rappaport on 'Island Cultures', first published in 1963. American and Southern Hemisphere scholars have tended to trace the ancestry of island archaeology explicitly to these works, and to the publication of *The Theory of Island Biogeography*, by MacArthur and Wilson (1967). Ten years ago, a special issue of *Human Ecology* (1997), edited by Ben Fitzhugh and Terry Hunt, considered the continued relevance of the laboratory trope (see particularly the papers by the editors, Burney, Dewar, and Terrell).

An explicitly archaeological treatment of island cultures by John Evans in 1973 in the *Explanation in Culture Change* volume, and his later contribution to *World Archaeology* (see Evans 1973, 1977) were decisive publications in introducing British scholars to the issues involved, and led on to much of the important comparative effort concerning Mediterranean islands found in the works of John Cherry (1981, 1990), Mark Patton (1996), Cyprian Broodbank (2000) and others.<sup>3</sup> The series of papers on island archaeology published in the June 1977 issue of *World Archaeology*, edited by Ian Glover, followed immediately after the February 1977 issue of the same journal on human biogeography, originating from a 1974 conference organised by John Terrell and William Fitzhugh (as discussed in Terrell 1997:419–420). This represented an approach heavily informed by MacArthur and Wilson (1967).<sup>4</sup> The island-archaeology volume represented a significant cross-fertilisation of the American and British traditions, with various papers there also referring to MacArthur and Wilson, as well as Vayda and Rappaport (1963), Goodenough (1957) and Sahlins (1958). The sole paper from New Zealand characteristically referenced papers from both the British and US traditions (Davidson 1977).

More recent general publications on island archaeology have tended to muddy the waters somewhat on what the comparative basis of the study is really all about, particularly by raising trenchant criticisms of the 'islands as laboratories' concept – Fitzpatrick (2004) and Rainbird (1999, 2007) are examples. But as they get further away in time from the foundational literature, I feel these treatments have forgotten some of the original ideas and contexts. It is worth questioning whether, to misuse maritime metaphors, island archaeology is being drawn ever onward by strong winds or currents in full sail, as might be hoped, or whether it is entering the ever-decreasing circles of a whirlpool, spinning faster – measured by increasing publication rates – but more terminally towards the hole in the middle!

It seems important therefore to return to the foundational papers of the 1950s and early 1960s – while bearing in mind the farther horizons from which they in turn derived – and recall what these early publications actually said about islands, and about what sort of islands. The 'ambivalence' about islands identified by Atholl Anderson (2004) in the Fitzpatrick volume

can be seen as having been there since the beginning. One might suggest that confusion in the minds of the early authors has simply been reproduced, rather than having been examined explicitly, creating a dangerous shoal in island-archaeology theorising.

## The foundational texts of the 1950s

### *Mead 1957*

Mead's 1957 introductory paper, precisely one paragraph long, was titled 'Introduction to Polynesia as a Laboratory for the Development of Models in the Study of Cultural Evolution' (Mead 1957). The three papers it introduced came out of a session on evolution and the comparative method at the December 1956 meetings of the American Association for the Advancement of Science. The theoretical context was the resurgence in evolutionary anthropology during the late 1940s and early 1950s associated with names such as Leslie White (1949) and Julian Steward (1955), but informed too by the writings of Vere Gordon Childe (for instance Childe 1951). The 'multilinear' evolution of Steward was particularly favoured by American anthropologists of the time (see for instance Mead 1958), not least because of the McCarthy-era witch-hunt of suspected communists, a brush with which both White and Childe were directly tarred (cf. Harris 1969:637–639).

What were needed to bring forward the evolutionary agenda were 'very detailed comparative studies of particular societies in a given culture area' (Mead 1957). Mead also tipped the hat to the 'related sciences' of archaeology and 'modern genetic theory', the former because of the recent development of radiocarbon dating and its application to Pacific sites, and the latter because of processes of adaptive radiation and genetic drift that might be seen as analogous to processes in cultural evolution. Works on genetics by Simpson (1949), Boyd (1950) and Muller (1956) are referred in Goodenough's article that follows on from Mead's. She believed that archaeology and genetics would provide a new perspective into which the more traditional analyses of genealogies and navigation patterns might be fitted. Mead was certainly prescient in that regard! Goodenough's own work on star navigation was probably among those she had in mind (Goodenough 1953), and in his own paper he further references Heyerdahl (1955), Whitney (1955), Gladwin (1956) and Sharp (1956). Goodenough (1957) also notes how Emory's synthesis of earlier genealogical dating by Fornander and Cartwright for the settlement of Hawaii fitted in with the first radiocarbon date to be obtained from that archipelago, from the Kuli'ou'ou rock shelter on O'ahu (as cited in Elbert 1953:168).

### *Goodenough 1957*

Goodenough's paper was called 'Oceania and the Problem of Controls in the Study of Cultural and Human Evolution' (1957). He notes that such laboratory-like conditions are not found over much of the earth, where it is impossible to sort cultures into phylogenetic units because 'even as they change, elements blend and reblend in the course of migrations, conquests and trade' (1957:146). Under these 'normal conditions', it is hard to examine cultural change 'within the framework of what biological evolutionists have called 'radiation'; that is to examine critically the processes by which phylogenetically related cultures become progressively different from each other' (ibid). He continues: 'Only when a culture X splits and moves into two previously unoccupied regions can we be certain that subsequent differences are due either to environmental adaptation or to something akin to what the geneticists call 'drift'. Only under these conditions can we get some idea of the capacity of the parent culture for modification without the addition of new cultural strains from new contact and borrowing.'

He recommends parts of the Pacific as being just such suitable laboratory-like locations

for phylogenetic study. In establishing this, he uses the nascent study of historical linguistics as a framework for discussion of the settlement of the Pacific Islands, particularly the notional sequence (and dating) of settlement provided by glottochronology (Elbert 1953). This he links to Sharp's (1956) views on the limitations of Polynesian navigational abilities and the prevalence of accidental drift voyages to suggest that 'once settled, eastern Polynesia as a whole remained isolated from the rest of the Pacific; and its remoter island groups remained isolated from each other' (1957:150). He concludes that 'Cultural differences in Polynesia, especially in eastern Polynesia, must be due to the adaptation of a common heritage to local ecological conditions or to random loss of portions of the common heritage in very small populations, as in the presumably frequent instances in which an island's inhabitants were derived from a canoe-load or two of initial settlers' (ibid).

Noting progressive genetic isolation from west to east in the Pacific and the correspondingly homogeneous physical appearance of Polynesians towards the eastern end of their distribution, Goodenough seeks analogous processes in culture: 'Thus in eastern Polynesia we should expect to find a more intense utilization of a narrower cultural heritage so as to lead to distinctive "flowerings" or "specializations" of ideas which in the west remain relatively unelaborated and more "generalized". Cultural differences relating to *mana* and *tabu* are a possible example. The greater elaboration of social stratification in some eastern islands may also reflect this process' (1957:151). He points to a tendency to cultural and linguistic loss, again citing Elbert (1953), who mentions loss of domestic plants and animals as one moves east in the Pacific. Again, continental situations are contrasted in which cultural loss is compensated for by borrowed elements from contact with other groups and the causes of loss are harder to discern.

Cultural adaptation to distinctive environments is brought into play here, with both Polynesian and Micronesian atoll cultures seen as independent developments, but both stemming from ultimately common high island cultures. Contrasting eastern Polynesian high island and atoll cultures would bring out environmentally specific adaptations of the original society, and comparison of Micronesian with Polynesian atolls 'would show how two closely related branches of a single cultural tradition were independently adapted to similar environmental pressures' (1957:153).

Isolation is clearly the key here, as it is to Goodenough's final reason for comparison: the reconstruction of the ancestral form before migration. He writes: 'Because the island societies were free from contact with unrelated cultures, all of the modifications and adaptations which have taken place, and all of the new elaborations, are expressions of the potential for change and development inherent in the ancestral culture' (ibid). As well as Micronesia and Polynesia being suitable for such comparisons, the Massim area of Island Melanesia is singled out for potential comparison among 'a number of small societies with a common linguistic and cultural heritage, every one of which has diverged from that heritage in a slightly different way from every other. Here again, the radiation process seems to have been operative, and once again in relative isolation' (1957:154).

#### *Sahlins 1957*

As noted by Mead (1957), Marshall Sahlins takes a different explanatory tack in his paper 'Differentiation by Adaptation in Polynesian Societies'. He states: 'My thesis is that Polynesian cultural differentiation was produced by processes of adaptation under varying technological and environmental conditions. A single culture has filled in and adapted to a variety of ecological niches' (1957:291). He contrasts two forms of descent system – ramages and truncated descent lines – and asserts that the first form is 'well constituted for exploitation of different, scattered

resources' (1957:294), such as the pattern found of coastal fishing and inland taro or yam gardening where a number of different production zones are found across the landscape, requiring redistribution of produce organised through genealogical seniority. Truncated descent lines 'are associated with an ecology wherein resource zones are not widely separated, but are clustered such that all domestic groups can engage in the entire range of production activities' (ibid), such as where narrow concentric production zones concentrate near the coast and small groups are capable of self-sufficiency.

This classification applies to high islands and is further contrasted with atoll organisation. Sahlins summarises the latter systems as deriving 'from the high islands and typically show divergence through adaptation to the unfavourable atoll opportunities. This divergence becomes more marked in proportion to the length of time in occupation, and is occasionally facilitated by small population effect. The direction of extreme modification of low island organization is toward an intricate system of interlocking social groups, each tending to be exclusively associated with control of different resources and specific forms of production and distribution' (1957:299).

Apart from noting the small size, poor resource base and vulnerability to natural disaster of atolls, the paper does not seek to explain differential degrees of social stratification in Polynesia, but merely to contrast ideal types. The paper is, however, in part a summary of some of the conclusions of his book then in press, *Social Stratification in Polynesia* (Sahlins 1958), completed in November 1955 and itself based on his 1954 PhD dissertation. There, it is quite explicit that resource productivity is at the root of differences in social stratification in Polynesia, allowing him to rank 14 Polynesian societies. Sahlins had been a student of Leslie White, which might explain the somewhat different emphasis than in Goodenough's paper, which is more in the Mead-Steward camp.

In the monograph, Sahlins states that 'the choice of Polynesia as a laboratory for the study of cultural adaptation is a judicious one, for all Polynesian cultures have a great part of their history in common – tradition is almost a constant. Nevertheless, cognisance will be taken of the possibility that diffusion or some other historical process, perhaps nonadaptive, may have been operating differentially on features of social stratification in particular cases' (1958:x). This certainly suggests that Sahlins was less sanguine about the isolation of Polynesian societies than Goodenough appeared to be, but he still used the same genetic metaphors, such as 'adaptive radiation' of the Polynesians as a 'single cultural genus that has undergone adaptive differentiation' (1958:248). It should be remembered, however, that beyond this, his model is not necessarily about islands per se: it could theoretically apply to any set of situations where both nucleated and dispersed resources can be found among related cultures.

#### *Goldman 1957a*

Irving Goldman's paper, the third and final paper of the series, is called 'Variations in Polynesian Social Organization' and thus covers some of the same concerns about social structure as that of Sahlins, but from a somewhat different perspective. To Goodenough's justification for studying cultural evolution within a culture area, Goldman adds 'the central problem of evolution, namely variation' with an aim to 'reconstruct evolutionary processes, directions and stages' (1957a:374). In this case, he is interested in examining how kinship-based societies become transformed into 'social systems governed primarily by political and territorial principles' (ibid). Echoing Goodenough, he notes that: 'As a result of their relative cultural isolation, islands are more sheltered from the overwhelming effects of diffusion, and variant forms are more clearly observable. In this respect, the Polynesian Islands, the most removed from continental masses,



are most favourably situated for our purposes' (1957a:374–375). In a footnote, he references Sharp's (1956) book as 'corroboration of the thesis that Polynesian culture is to be explained primarily from internal developments rather than from diffusion' (ibid).

The engine of evolution is not in this case adaptation to environmental variation in any direct sense but is status rivalry: 'A constant in every Polynesian society, status rivalry seems to have provoked stresses to which the status system, along with other components of the social structure, adapted. These adaptations rearranged the total social structure producing new structural types' (1957a:375).

Based on this principle, Goldman arranged 18 Polynesian societies for which he had data into a hypothetical sequence of social stratification, a gradient or continuum he divided into three successive evolutionary types: traditional (the most conservative and close to a proto-Polynesian form of organisation), open and stratified. He admitted that economic, political, military and other factors were relevant as well to social change (1957a:377), but simply ignored them, or saw their effects as again a result of more generalised status rivalry. His major monograph, *Ancient Polynesian Society* (1970), substantially written before 1966, did not pay any greater attention to factors other than status rivalry. Despite the very different emphases as to the driving force of cultural evolution between Goldman and Sahlins, their two ranking systems produce roughly the same sequence of stratification, with no very significant shifts in position (Goldman 1957a:376; Sahlins 1958:249–250).

A slightly earlier paper of Goldman's reproduces his sequences of culture change, but notes that they 'take their character and direction in part from the momentum of status rivalry itself and in part from the particular physical and cultural setting of each island. Thus the differing ecologies of atolls and high islands, variations in population density, varieties of subsistence techniques, levels of economic productivity, systems of property relations, the role of migrations and military conquests, diffusion, and, finally the specific historical 'accidents' that occur in wars, migrations and contests for power – all influence and are in turn influenced by the dominant motive of status rivalry' (1955:680). The 1955 paper, which is a much more coherent statement of his views than the 1957 one, also has an enlightening discussion of parallel cultural trends that Goldman identifies as being present as societies moved from traditional to open to stratified forms in Polynesia (1955:689–694). Of the trio of papers that followed Mead's (1957) introduction, Goldman's is clearly the weakest, and is the least referred to today.<sup>5</sup>

### **Islands as laboratories and the culture area concept**

We can thus see how the 'islands as laboratories' idea came of age with a heady mix of new developments in genetics, archaeology, cultural evolutionary anthropology, and historical linguistics during the early- to mid-1950s. However, it was not really the fact of being islands per se that created the 'controls', the laboratory-like conditions in the Pacific Islands that would reflect back on more complex continental situations. These 'controls' were grounded in the culture area concept, going only slightly beyond the old geographical and assumed cultural divisions of Polynesia, Micronesia and Melanesia in Goodenough's paper to the extent that they were united by the presence of Austronesian languages, descendent from a common linguistic and assumed cultural ancestor to the northwest in Asia. In Sahlins' and Goldman's contributions, it was explicitly the Polynesian culture area that was the unit of analysis.

As Rainbird has noted (2007:29), there is a straight line back through Mead's teacher, Ruth Benedict, to her teacher, Franz Boas, who came out of a geographic background to 'found' American anthropology around 'a close study of limited geographic areas', as Goldman, another Boas student, put it (1970:xi). This led on to the concept of 'culture area', suggested by Boas

(1896) as an alternative to a failing 19th century evolutionist viewpoint, much developed by his students, such as Kroeber and Wissler (see Harris 1969 for a detailed discussion of this historical sequence). As Goldman (1955:680) summarises the concept: 'A culture area comprises historically related societies each showing significant variations from a common area pattern.' The concept of Polynesia as a culture area was already well known within Polynesian studies before the 1950s, based particularly on the work of American anthropologist Edwin Burrows (1938, 1939, 1940), who cited some of Wissler's works (1926, 1938) on Native American culture areas as a direct inspiration. The New Zealand scholar H.D. Skinner had published a paper on 'Culture areas in New Zealand' as far back as 1921. Skinner drew directly on the work of otherwise-unidentified 'American ethnologists' (1921:71), noting that 'some fourteen cultural areas' in the Americas were constructed on the basis of subsistence practices, whereas in the Pacific they would be better based on the origins of the cultures. From the context it is clear his immediate source is Wissler (1917). If Pacific parallels are to be used by island archaeology colleagues working in the Caribbean and the Mediterranean areas or elsewhere, the underpinnings of the culture area concept need to be understood. It could be argued to be more important in the original formulations discussed above than the fact of islands in a big ocean.

### *Vayda and Rappaport 1963*

The end of the formative period of anthropological contributions to island archaeology is marked by the paper of Andrew Vayda and Roy Rappaport, 'Island Cultures', published in 1963 in the proceedings of the 1961 *Man's Place in the Island Symposium* of the 10th Pacific Science Congress held in Honolulu.<sup>6</sup> Vayda and Rappaport didn't claim originality for many of the points they made; indeed, some of them are referenced back to the Sahlins and Goodenough papers already discussed. But they expressed them in a clear, almost dot-point, fashion and that is why their paper is still regularly cited more than 40 years on in the island-archaeology literature. Again, what they actually said is less ably reported in subsequent citations. Their starting question was a simple one: 'What influence do relative isolation and limited territory have upon the evolution and differentiation of cultures?' (1963:133). Although islands are used to illustrate their answers to this, there is nothing island-specific in the question. Indeed, they straight away exclude from consideration large islands such as New Guinea and New Zealand which are continent-like, and even small ones regularly in touch with larger continental areas such as Zanzibar or the Maldives. They note there are other kinds of 'natural barriers' than the sea, and include remote valleys and mountaintops in continental situations as equivalent to the small islands they do consider, as well as communities such as ghettos cut off by 'man-made barriers'.

Vayda and Rappaport introduce another concept borrowed from genetics, the 'founder principle' or 'founder effect' so important to discussions of island biogeography: 'For the operation of the founder principle, isolation rather than limited territory seems to be critical. If the migration to an isolated place, whether a small island or a large continent, is by a relatively small group of people who are unable to reproduce in full the culture of the population from which they derived, then the culture in the new place will be immediately different from the culture in the homeland.' (1963:134–135) One of the first archaeologists to take up this biogeographical idea was Richard Pearson (1969) in his monograph on the Ryukyu Islands, between Japan and Taiwan, where Vayda and Rappaport's paper was extensively cited.

Vayda and Rappaport discuss next the idea of cultural 'drift', following Goodenough (1957), but put their own – not island-specific – spin on it by asking: 'whether appreciable cultural differences can develop among people only if they are kept from one another by geographical or other barriers ... We badly need further investigations, not simply to determine whether

isolation is a precondition of cultural differentiation but to determine just what kinds and degrees of cultural differentiation may presuppose just what kinds and degrees of isolation' (1963:135–136). This question underlies any archaeology of frontiers and boundaries, such as later developed in the 1970s with the work of Ian Hodder and others (Hodder 1978, 1982; Green and Perlman 1985).

They turn next to the role of limited territory and 'the development of cultural traits affecting the dispersion and size of island populations' (1963:136–137). They consider population limitation practices such as infanticide or voyages of exile, subsistence intensification and adaptations in social structure to match population distribution to resources across the landscape (cf. Sahlins 1957).

The final issue they discuss goes all the way back to the Prichard (1813) quotation with which this paper starts, the rate of change in isolated island cultures and its corollary, the openness or not of such cultures to outside influences when their isolation is broken by colonial or other forces: 'Are innovations in the ways of doing things less frequent in more or less isolated and small populations because there are not many people interacting with one another and making new combinations of acts and ideas? Or do such innovations as occur tend to spread among the members of a population more quickly and to become established in the culture more readily when the population is small and isolated? Might cultural change, therefore, be rapid in such a population despite a low incidence of innovations?' (1963:139–140)

## Discussion

### *Questions of scale and connectivity*

One of the commentators on the paper as given orally in 1961, David Schneider, raised the core issue, noting that 'it may very well be that there is nothing peculiar about water around a piece of land insofar as the cultural problems are concerned' (Fosberg (ed) 1963:143). Vayda and Rappaport had let themselves in for this criticism by making a distinction between what they called 'island cultures' and other kinds of cultures found on islands that were large or interconnected. Far from being the synthesis of the idea of island cultures that it is often cited to be, the paper, perhaps inadvertently, points to some fatal flaws in the idea. For instance, there are important issues of scale revealed: when is an island so large that it operates like something else? There might be little sense, for instance, in talking about New Guinea as an island in the same breath as Easter Island. And what about substantial inland island populations which have no direct links to the sea? Do these represent 'island cultures' in any meaningful sense (cf. Roe 2000)?

There are also issues of connectivity. Even small islands in regular communication with others present a different kind of phenomenon from more isolated ones. They are perhaps best conceived of as archipelagic cultures, rather than island ones, or examined in terms of their links to neighbouring coasts and mainlands. Their interrelations can be modelled by various kinds of network and peer polity interaction models that might be equally applicable to continental situations (see Bedford and Spriggs 2008 for an Island Melanesian example). There are also suggestive parallels between island colonisation and patterns of mainland coastal colonisation by sea travel in both the Mediterranean and the western Pacific that still await detailed examination (cf. Zilhao 1993).

### *How isolated do you have to be?*

Talk of 'relative isolation' always begs the question of how isolated is that, exactly? Work by Atholl Anderson and others on some of the 'mystery islands' of Polynesia, those that show evidence



of Polynesian settlement but that were abandoned by European contact, is documenting this precisely: see, for instance, Anderson's early work on the Kermadec Islands, and his subsequent work on Norfolk Island (Anderson and White 2001), the Subantarctic Islands south of New Zealand (Anderson and O'Regan 2000) and Christmas Island in Kiribati (Anderson *et al.* 2002), as well as the work of Weisler (1994, 2004) on Pitcairn, Henderson and Ducie.

Another major contribution has been Irwin's (1992) work on island size and accessibility in the Pacific that can now be compared directly in most cases with detailed settlement histories. Irwin (1992:175) plotted the islands of Polynesia and Fiji according to their distance and target angle of expanded sighting radius from their nearest occupied neighbours. Greater isolation occurs with greater distance and smaller angle of target. The table shows (relatively) large and isolated islands away in one corner – the 'stranded margins' of Easter Island, Hawaii and New Zealand, and the empty and abandoned mystery islands in the top left corner – at varying distances but with very low target angles. In the middle ranges we find islands that remained occupied but where inter-island voyaging had declined or ceased by European contact, and in the bottom left are the islands that maintained active voyaging spheres up to European contact, such as the Societies and the Tuamotus, Tonga, Fiji and Samoa. The table might even be read as predicting what islands were most likely to have become abandoned next: Manihiki, Pukapuka, Chatham, Penryn, Niue and Rotuma!

A further analysis factors in island size, as well as accessibility, with this time, the mystery islands appearing as the apex of a triangle and small and inaccessible (Irwin 1992:192). Intermittently occupied or utilised islands are seen to be those that are small but more accessible, and all other islands are occupied permanently. Rather unhelpfully, Irwin doesn't include quite the same set of islands in this diagram as in the previous table, covering only central and 'eastern' Polynesia. But again, we can identify potentially vulnerable islands: Manihiki, Pukapuka, Tongareva, Rakahanga and Rapa. Tongareva and Rakahanga hadn't been considered in the earlier table, and Chatham, Penryn, Niue and Rotuma did not appear in this one. Irwin's conclusion is that the pattern of abandonment and isolation is systematic and can be measured, dependent as it is on accessibility from other islands (Irwin 1992:195). An archaeology of isolation, as a special topic within island archaeology, might be most productive.

### **All factors are time-dependent**

One other important issue to consider when talking of factors such as isolation and connectivity, and even island size, is that all of them are time-dependent. This factor was not at all evident to the 1950s anthropologists who inspired island archaeology. Radiocarbon dating was only in its infancy and modern archaeology had barely begun in much of the Pacific outside of New Zealand during the 1950s, pioneered by scholars such as Avias, Emory, Golson, Green, Shutler, Sinoto and Suggs (Kirch 2000:27–32). Recall too that the first modern archaeological excavations in New Guinea took place only in 1959 (Bulmer and Bulmer 1964) and in Vanuatu only in 1963 (Shutler and Shutler 1966); even today, there are inhabited Pacific islands yet to experience the loving tickle of the archaeologist's trowel.

The discovery of a Pleistocene prehistory for the Bismarcks and Solomons only began at the turn of the 1980s with Jim Specht's excavation of Misisil Cave on New Britain (Specht *et al.* 1981). Only then did it become obvious that archaeologists working on Pacific islands outside of New Guinea would have to deal with the prospect of the changing size and location of islands, and even whether they were islands when humans first reached them, owing to the effects of glacial and post-glacial sea-level fluctuations. Many atolls in the Pacific may have only

become habitable after about 2000 BP as land-forming processes caught up with a stabilised sea level. The age of the island terrain itself can be seen as important to human habitability, as Rolett and Diamond (2004) recently reminded us in a paper examining environmental predictors of pre-European deforestation on Pacific islands.

There have been major changes too in climatic conditions that we are only now becoming able to map on to possible cultural responses. Not least among these are changes in the strength of El Nino conditions, as again Anderson, among others, has pointed out in key papers (Anderson 2002; Anderson *et al.* 2006).

The ability to reach most islands is dependent on boat technology, and as Anderson has strongly argued over the past few years, there have been major thresholds reached in this during the time people have lived on Pacific islands, not least the invention of the sail and, within the past 1000 years or so, several further major developments in central and eastern Pacific boat technology in particular (Anderson 2000, 2001, 2004).

Any discussion of time-dependent processes also has to address those historical contingencies of what Vayda and Rappaport (1963:133) called 'man-made barriers', citing a Neolithic village surrounded by hostile (hunter gatherer?) neighbours or ethnic communities living in ghettos. The isolation of particular communities can increase or decrease through time, often in a cyclical pattern, depending on a range of socio-economic factors, both external and internal. For the Pacific, where there is a rapid decrease of long-range communication between islands after a couple of generations of the Lapita expansion around 3000 BP, a variety of reasons have been adduced. These include exchange system contraction, or specialisation, sociopolitical transformation (including 'system collapse'), absorption and/or secondary migration by other cultural groups, and local adaptation (Spriggs 1997:152–162).

This last theme has been developed most elegantly not by an archaeologist but by the linguist Andrew Pawley in his theory of the 'cycle of linguistic diversification' (Pawley 1981). A similar contraction of the inter-island exchange system is envisaged as in other models, but in this case not because the system was too socially costly to maintain but because it was no longer functionally necessary for the economic and biological survival of individual communities in Island Melanesia. Any effective links to the Lapita 'homeland' therefore became attenuated and finally broken. New factors must have spurred on the renewed contacts between western Pacific Island groups seen within the past 1000 years, a phase associated with, but not restricted to, the 'Polynesian Outlier' phenomenon (Bedford and Spriggs 2008:107–110; Spriggs 1997: Chapter 7).

Although the apparent cessation of widespread canoe travel in eastern Polynesia after about 1450 AD is often attributed to changing climatic conditions discouraging long sea voyages, a more social explanation along the same lines could potentially be equally applicable there. One recalls that for Micronesia, 'the late survival of voyaging here has been partly explained by the fact that almost all the eastern Micronesian islands are atolls, which are among the most precarious human habitats on earth' (Irwin 1992:194). Continued inter-island exchange there was thus certainly linked to the economic and biological survival of communities to an extent not found in the generally larger and/or more accessible eastern Polynesian islands that remained inhabited.

## Conclusion and postscript

All research and theory-building is of its time. By providing a wider context for a series of key theoretical texts significant in the development of island archaeology and reminding us of their actual content, I have sought to expose the limitations of their concerns and ideas for practitioners

today. The same tropes, the same issues and questions from that era remain fundamental to the constitution of island archaeology 50 years later. As Kuklick (1996) has shown, sociocultural anthropology has now very largely worked through these issues in a decisive break. They are now perhaps preventing the field from either reinventing itself as something else, or at the very least overcoming some important limitations and escaping the whirlpool of an under-theorised subject, into – if not calmer waters – at least those freshening conditions that an experienced sailor like Atholl can confidently handle.

This paper was complete and the final onerous task of reference checking was underway when I came upon the latest set of exchanges involving the usefulness of a concept of island archaeology (Boomert and Bright 2007; Fitzpatrick *et al.* 2007). Not surprisingly, Atholl was right in amongst it as one of the authors of the response to Boomert and Bright's call for the replacement of 'island archaeology' by 'an archaeology of maritime identity' (2007:18). Between them, the two papers provide a very up-to-date set of references in the field and Boomert and Bright provide a detailed overview of its recent history. As is often the case when finishing a paper, you wonder whether what you have written is of any use; don't people already know all this stuff? I am heartened to see that in this case, they don't. Boomert and Bright believe the concept of islands as laboratories for the study of cultural evolution 'was introduced to anthropology by Vayda and Rappaport' (2007:6). They state that: 'Evans (1973; 1977) was the first to apply biogeographical principles to insular archaeology' (*ibid*). Finally, they also contend that the insight of dramatic ancient human impacts upon island environments and biota in places such as the Pacific 'has only been reached over the last twenty years or so' (2007:12). Clearly, there is still a need to rehearse the earlier history and 'prehistory' of the island archaeology field!

## Endnotes

1. The passage continues: 'In this point of view we also derive advantage from the remote distances which separate the islands, and from the imperfect knowledge of navigation which the natives possess: for these circumstances prevent intercourse among the different tribes, and preclude those frequent changes or intermixtures of population, which perplex our inquiries into the history of continental nations. An equal diversity characterizes the moral condition of these people. Some tribes are the rudest and most destitute savages found on the face of the globe, while others have gained a considerable advantage in the arts of society ... The regions above mentioned are inhabited by races of people who bear strong indications of a near connexion in history, if indeed their affinity be not so clear as to justify the opinion of the best informed voyagers, that they are all propagated from one original.'
2. The passage continues: 'They were also denied the advantages of numbers which is a most important element in the progress of human society. Under such circumstances it would be expected that isolated populations would remain in a stationary condition through longer periods of time than the inhabitants of continents. Immigrants, presumptively, from original continental homes, their posterity would be expected to reflect the condition of their ancestors at the epoch of their migration, since the probabilities of retrograding in knowledge would be at least equal to those of progress under the physical limitations with which they were subsequently surrounded. These hindrances would tend to preserve their domestic institutions within narrow limits of change.'
3. Evans' two publications, particularly the 1973 one, are deserving of a paper on their own for their foundational influence on British island-archaeology studies. Indeed, there is now some evidence that they are pushing out any memory of the 1950s background to these discussions: in

Fitzpatrick's edited volume (2004) there is only a single reference to Vayda and Rappaport (1963) and that in the editor's introduction, whereas Evans (1973) is referenced in no fewer than five other papers and Evans (1977) in three papers other than the introduction. In Rainbird's two works on island-archaeological theory (1999, 2007) both of these Evans papers are cited but none of the key American foundational texts discussed in this paper get a mention. Colin Renfrew has recently gone so far as to declare that Evans 'introduced the idea of islands as laboratories for the study of culture process' (2004:283)! Even if only the archaeological field is being referred to, this is scarcely fair to Pacific scholars who have been aware of this trope at least since the 1950s texts discussed here – and indeed before. In 1961, for instance, Robert Suggs in his monograph on the archaeology of the Marquesas Islands stated: 'The simpler environmental situation of Polynesian cultures makes the area an ideal laboratory for study of cultural ecology, as has already been demonstrated' (1961:194; cf. Kirch 1982). Golson (1958, cf. 1959) was equally aware of this literature, referencing Goldman, Goodenough, Hawthorn and Belshaw, Mead and Sahlins from the relevant *Journal of the Polynesian Society* issues. The lack of references in Evans (1973) is perhaps partly to blame for obscuring this genealogy, one he makes clear in his later paper (Evans 1977), where Vayda and Rappaport (1963) occurs among the citations. It is clear from his earlier remark that the particular qualities of islands are 'more fully recognized by natural scientists and anthropologists' (1973:517) that earlier literature informed his views, confirmed in personal communications from Professor Evans to me in October 2007 and January 2008. In the second of these, he recalled the influence of MacArthur and Wilson (1967), particularly the final chapter 'Prospect', on his original 1973 paper.

Golson (1958:27) also reminds us that New Zealand archaeologist Roger Duff had explicitly used the 'islands as laboratories' concept before its popularisation by Mead (1957). He cites Duff (1956:1), but the same wording occurs in Duff (1950:1), a book completed in 1947–1948: 'The student of the ethnography of Polynesia has at hand a remarkable human laboratory in which to study whatever laws determine the evolution of human culture in time and space.' The statement is unreferenced, suggesting it was a commonly used idea at the time. Duff also provides an elegant discussion of the concept of 'cultural drift' without, however, naming it as such (1950:2–3). His contributions on these topics appear to have been generally ignored by scholars apart from Golson.

4. Terrell is certainly among the earliest archaeologists to engage with MacArthur and Wilson's (1967) ideas. He refers to having come across a copy of their book in 1971 (Terrell 1974:ii), and its influence was plain in publications by Terrell and his associates that reference it not long afterwards (for instance, Kaplan 1973; Terrell 1972).
5. Goldman was kept busy during 1957 in the pages of *Journal of the Polynesian Society* answering his critics. Hawthorn and Belshaw (1957) published a critique of Goldman (1955) in the first issue of the *Journal* for the year, to which Goldman (1957b) replied in the second issue immediately after Goodenough's paper. The 1957 paper that has already been discussed appeared in the fourth issue for the year, the issue after that in which Sahlins' paper appeared, but was evidently written before Goldman had seen Hawthorn and Belshaw's critique, which is not referred to.
6. The symposium volume (Fosberg (ed) 1963) was enormously influential in several areas, not least as being the first sustained discussion of human impacts, negative and positive, on island ecosystems in the pre-European contact period. Major programs of archaeological research on human-induced environmental impacts on Pacific islands in particular over the succeeding decades were directly or indirectly inspired by this publication (Kirch 2000:57–62), not least by a companion paper originally delivered by Vayda and Rappaport at the symposium and revised for publication by Rappaport, entitled 'Aspects of Man's Influence upon Island Ecosystems: Alteration and Control' (Rappaport 1963).

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