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Archaeology in Micronesia Since 1965: Past Achievements and Future Prospects¹

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ABSTRACT

Recent archaeological work in Micronesia is reviewed. There has been remarkable progress in many fields, including the establishment of basic sequences, studies of pottery and other artefacts, the investigation of subsistence systems and settlement patterns, and the exploration of archaeological manifestations of social complexity. Much of the progress is due to the recognition that Micronesia is an important area of study in its own right. However, there has been little progress in understanding the origins of Micronesian peoples and cultures. The currently orthodox model of Micronesian settlement, derived largely from modern studies in linguistics, needs careful evaluation. It is necessary to identify Micronesian origins if the range of human adaptations in the region is to be fully understood.

Keywords: MICRONESIA, ARCHAEOLOGY, PREHISTORY, LINGUISTICS, ORIGINS.

INTRODUCTION

In 1965, Micronesia (Fig. 1) was a neglected backwater in Pacific archaeology. Its backwater status was largely determined by the commonly accepted view of the area as a route to somewhere else, rather than a region intrinsically interesting in its own right (Spoehr *et al.* 1951). It was reinforced by the apparent lack of pottery in the central and eastern islands and the widespread notion that coral atolls were unrewarding for archaeological research. Micronesia had no scholars of its own, as Polynesia had in Sir Peter Buck, nor had it any long established centres of learning comparable to those in Hawaii and New Zealand. Pleas about its importance and interest for archaeological work fell largely on deaf ears, and continued to do so for another decade. It is only in the last 10 years that archaeology has really flourished in Micronesia, and it is only as a result of this work that a gathering such as the Micronesian Archaeology Conference has become possible.

Several writers have outlined recent archaeological work in Micronesia since 1977, particularly the burst of activity in the former United States Trust Territory of the Pacific Islands and its emergent new states following the establishment of the Historic Preservation Office's archaeological survey (Cordy 1980, 1982; Takayama 1982; Craib 1983; Russell 1983; Russell and Fleming 1986). One of the important factors in the success of recent work has been the strong research orientation established right at the start of historic preservation work in Micronesia. Other important factors have been the appearance of resident archaeologists in Guam, and of Micronesian scholars dedicated to their own history and prehistory. But perhaps the key feature ensuring the growth of archaeology in this area has been the shift in attitude to its potential. Micronesia is no longer a route to somewhere else, but an area of significance in its own right.

I should like now to review briefly some of the specific achievements as I see them, and then to consider some underlying problems concerning Micronesian origins and interrelationships which I believe need to be borne in mind if the impetus of recent years is to be maintained. From time to time I shall draw on material from Polynesia and elsewhere, for



Figure 1: Micronesia and adjacent areas.

Micronesia does not exist in a vacuum. I shall be concerned with prehistory, but I should like to mention here that the recent achievements of Micronesian archaeology also include the considerable amount of work devoted to historical archaeology, both that of the colonial era and that of the second world war.

BASIC CULTURAL SEQUENCES

Long before 1970, the potential of Micronesia for archaeological work had been demonstrated. It was known that there were long occupation histories in the western islands, with ceramic sequences to match (Spoehr 1957; Gifford and Gifford 1959; Osborne 1966); and that even coral atolls, or at least some of them, had deeply stratified deposits, containing shell artefacts, if not pottery (Davidson 1967a, 1967b). It is therefore not surprising that one of the immediate achievements has been the establishment of a considerable time depth of occupation throughout Micronesia, although this time depth is not yet as long as earlier work might have predicted. Yet these sequences have so far done little or nothing to resolve problems about Micronesian origins, a question I shall return to later. In part, of course, this is a reflection of the still poor state of knowledge about possible source areas. In part, however, it reflects the limitations of current thinking about this issue.

Radiocarbon dates, despite their manifold advantages, have posed a number of problems in Micronesia, as they have elsewhere. Even the most conservative view would certainly accept a time depth of 2000 years for all the high islands. It is less clear whether that can be extended to 3000 years for any high island apart from the Marianas, and whether the Marianas sequence begins around 1000 B.C., or extends back into the second millennium B.C. (Cordy 1982: 126-7; Bonhomme and Craib 1987).

The atolls, too, have posed dating problems. In my work on Nukuoro, I had no external comparisons against which to assess the single early date for the earliest deposit I encountered. A "modern" result on the rerun persuaded me to reject that early date (Davidson 1968: 55). Now, however, with results from other east Micronesian atolls going back for 2000 years (Craib 1983: 924; Riley 1987: 242-243) and perhaps for considerably more (*Pacific Magazine*, Jan/Feb 1987), it may be worth reconsidering.

In contrast to Nukuoro, dates for deposits with potsherds on western atolls can be evaluated against the pottery sequences of the high islands from which the sherds derive—once these in turn are securely dated. Thus, archaeologists working on Yap have been tempted to accept an earlier date for pottery found on Lamotrek than those preferred by the excavators (Intoh and Leach 1985: 153; Fujimura and Alkire 1984: 123; see also Takayama 1984: 2). On the other hand, relatively early dates for Ulithi are questionable if late Yapese pottery is associated (Takayama 1982: 101-2; Craib 1980). Thorough study of the potsherds themselves is necessary to resolve these issues.

Problems over dating will eventually be sorted out as more dates become available for different islands and consensus about length of occupation and interisland contacts is achieved. To some extent, Micronesian prehistory is now at a point similar to that reached in Western Polynesia 15 to 20 years ago. The troubling discrepancy between the apparent duration of Lapita pottery use in Tonga compared with Samoa was resolved by the revision of the Tongan sequence (Groube 1971) and is probably now forgotten by everyone except those most intimately concerned at the time. Similarly, the perplexing lack of Lapita pottery in Samoa was eventually explained by the chance discovery of the submerged site at

Mulifanua (Green and Davidson 1974). If Bikini was indeed settled during the second millennium B.C.², supporting evidence will turn up elsewhere in the Marshalls. If the lack of earlier sites in the high islands of the Carolines is due to submergence, sooner or later someone will find firm evidence of this.

I should like to stress, however, that problems will continue to arise from the dating by different laboratories of different materials using different standards (cf. Fujimura and Alkire 1984: 125). It is not easy to discover how individual laboratories actually calculate the results they report to archaeologists. A correction for marine reservoir effect (Bonhomme and Craib 1987), which can appropriately be made after consultation with a particular laboratory, may not be immediately applicable to results from another laboratory. Archaeologists should keep these difficulties in mind when discussing sequences.

For the moment, the establishment of respectably long occupation sequences for many parts of Micronesia is a considerable achievement. Moreover, most of these sequences do not have obvious hiatuses. In several parts of Polynesia there is still an embarrassing gap, or "Dark Age" between a well defined early occupation, and an equally well defined sequence beginning at some point in the last millennium and leading to the historically known culture of recent times (Davidson 1979: 94-5). This problem has arisen in Truk (Parker and King 1984; King and Parker 1984), but not elsewhere in Micronesia to the same extent.

POTTERY

It is no longer surprising when pottery, particularly early pottery, turns up on a previously "aceramic" island. The discoveries of relatively early pottery, which was probably locally made, on Truk (Shutler *et al.* 1984) and Pohnpei (Athens 1980; Ayres 1983), and of imported pottery on a number of western atolls (Fujimura and Alkire 1977, 1984; Craib 1984: 51-3; Takayama 1982; Intoh 1984) have been important both in tracing interisland contacts in the west, and in widening the possibilities regarding initial settlement in the east. At the same time, there has been little or no progress towards actually answering questions about Micronesian origins using the evidence of pottery.

In an archaeologically little known area, there is inevitably an element of tyranny of earlier discoveries over subsequent ones. "It is like/unlike Marianas Red [or Plain]" ... "it is like/unlike Lapita", are statements too plentiful in the still relatively sparse literature on Micronesian pottery, and too often based on a cursory examination of a small number of sherds. The first type of statement should die a natural death now that the Marianas pottery sequence has been greatly refined. Unfortunately, in an intellectual climate in which Lapita potsherds are increasingly being correlated with the spread of Oceanic languages (Bellwood 1983: 78; Pawley and Green 1984: 142), statements of the second type will probably be heard for some time to come.

The best prospects for progress appear to lie in more detailed descriptions of Micronesian wares and a more exhaustive search for outside parallels beyond the perennially obvious choice of Lapita. The documentation of possible alternatives in Melanesia (e.g., McCoy and Cleghorn n.d.) should help. It remains to be seen whether the grouping of much early Micronesian pottery together in a Micronesian Calcareous Sand Tempered pottery tradition (Takayama 1984, Intoh and Leach 1985: 151-2) is a useful concept or not, given the apparently widespread if uneven occurrence of calcareous sand tempered pottery in the Pacific (Athens 1984a: 144).

Just as midden analysis often flourishes where artefacts are few and far between, so studies of pottery technology tend to blossom when decoration offers only limited scope for study. One of the notable achievements of recent Micronesian archaeology, I believe, has been the increasingly detailed studies of Micronesian pottery technology by successive workers in the Marianas, particularly Guam, and in Yap (Reinman 1977; Ray 1981; Moore 1983; Athens 1986; Leach, Davidson *et al.* n.d.; Intoh and Leach 1985). Such studies have a great deal to offer in understanding the adaptation of people to particular resources, in addition to the more obvious advantages in establishing sequences and tracing relationships.

Temper analysis of potsherds has been of great value to Pacific archaeologists attempting to trace interisland contacts (Dickinson and Shutler 1979). Micronesia has also benefited from this type of study, which has suggested that pottery on Pohnpei and Truk was probably locally made, and shown that pottery from Belau as well as Yap was reaching western atolls such as Lamotrek and Ngulu (Dickinson 1982, 1984; Athens 1984a: 143).

OTHER ARTEFACT STUDIES

Although some recent archaeological projects have produced disappointingly few artefacts, others have yielded quite an abundance. There has already been some discussion of artefact distributions using archaeological finds rather than museum specimens (e.g., Takayama 1982: 105; Takayama and Intoh 1980; Takayama 1984: 6-7) and this is undoubtedly a field in which useful work could be done in the next few years. Such comparisons should, of course, take account of the varying environments of the islands, the contexts of finds, and the functional status of the sites investigated.

There are now real possibilities of documenting and dating the movements of people or ideas which resulted in the known distributions of such characteristic artefacts as slingstones, stone and coral food pounders, "beaked" adzes, and *Terebra* shell adzes. (Deciding whether people or ideas were the prime movers may not be so easy.) As with pottery, the search outside Micronesia has to be thorough rather than cursory, but the exchange of information will be a two-way process. There are Melanesianists who would like to know more about the origin and spread of slingstones in Micronesia, and how the spread of *Terebra* adzes through Micronesia is related to their relatively late appearance in Santa Cruz and Vanuatu (Leach and Davidson n.d.).

When finds are relatively sparse, the vagaries of archaeological discovery can lead to over-hasty identification of prehistoric connections between islands. A good example from Polynesia is the supposed direct connection between the Marquesas and New Zealand, based on a few key early artefact types which were thought to be shared by New Zealand and the Marquesas but not found in the Society Islands (Davidson 1983). Subsequent work has filled in most of the gaps in the Society Islands, and current work in the Cook Islands, for a long time archaeologically unknown, is now making them seem equally likely as a source for the settlement of New Zealand. Thus we should be very careful in jumping to conclusions on the basis of a few newly discovered artefacts.

A particularly important aspect of some artefacts is their relationship to certain kinds of food preparation. The appearance of *Cypraea* peelers has been linked to the introduction of the breadfruit complex as known ethnographically in Truk (King and Parker 1984). Variations in types of food peeler have been noted not merely in Micronesia (King and Parker 1984: 130) but also in Polynesia (Sinoto 1979) and in the Polynesian outliers near Santa Cruz (Kirch and Yen 1982: 252-3; Leach and Davidson n.d.). Peelers are also found in

Lapita sites (Green 1979) and attention has been drawn to the similarities between Lapita peelers and those from early contexts in the Marianas (Intoh 1986: 18). More careful study of peelers (which have not usually rated much attention compared with other artefact forms) and their possible association with particular kinds of food in various parts of the Pacific may prove a useful exercise.

Food pounders, also, although they could be used to pound various plant products, were particularly associated with breadfruit preparation on Truk (King and Parker 1984: 143). There are indications that the more elaborate pounders are a relatively recent development from a simpler, less flaring prototype (King and Parker 1984: 145; Takayama 1982: 98), as they seem to have been in Eastern Polynesia too (Sinoto 1979: 122-3). The possibilities of parallel development or contact between the two areas need to be explored. On present evidence, the relatively simple, pestle-like pounders of the Truk area are older than any known pounders in Eastern Polynesia. The use of wooden pestles in many islands, of course, complicates the problem and reduces the usefulness of stone or coral pounders on their own, as an indicator of a particular form of food preparation. Moreover, the inhabitants of areas such as Samoa, where breadfruit was certainly important, practised an extremely makeshift approach to its preparation (Buck 1971: 111-2).

Another important aspect of artefact studies in Micronesia is the widespread use of shell in preference to stone. The old view that this is an atoll adaptation transferred to high islands has been seriously challenged, both by the discovery of relatively early pottery manufacture and use in islands such as Pohnpei and Truk and by the realisation that shell was preferred in parts of a putative Micronesian "homeland" in eastern Melanesia for at least the last 3000 years (Garanger 1972; Kirch and Yen 1982; Leach and Davidson n.d.; McCoy and Cleghorn n.d.). Shell adzes, although present (Green 1979: 39), are not such a conspicuous aspect of Lapita material culture. The widespread use of shell adzes in Belau and Yap is one of the features linking these islands to most of the rest of Micronesia. Even in the Marianas, where stone adzes were an important artefact form, shell adzes are also significant.

The rare stone adzes that do turn up on the other high islands (Athens 1984b; Ayres and Mauricio 1987) offer scope for sourcing studies as well as more typologically oriented studies to determine where they may have come from. The transport of stones, and items made of stone, has proved a powerful marker of past communications elsewhere in the Pacific (Green 1979: 37-39; Leach 1985; Leach *et al.* 1986). The potential for such work in Micronesia certainly exists (Brooks 1984). There are, of course, more exciting items than stone adzes, whose source needs to be determined, notably the Belau "money".

SUBSISTENCE

The study of subsistence is an important aspect of the study of human adaptation to island life. Progress here has been uneven but, in general, Micronesian archaeology has followed a trend already established in Polynesia, where the study of prehistoric horticulture or agriculture was for a long time the poor relation of settlement pattern studies and a preoccupation with sequences. This is not to say that horticulture has been entirely neglected. The hypothesis about the effect of the breadfruit complex on Trukese settlement patterns (King and Parker 1984) has already been mentioned. There has been a conscious attempt to study horticultural features in Pohnpei (Ayres and Haun 1985). But the recent achievements of Micronesian archaeology have not included successful resolution of the problems posed

by the terraces in Belau (Masse *et al.* 1984: 118; Lucking 1984) or the question of rice in the Marianas (Craib and Farrell 1981). Nor have such obvious archaeological features as Yapese yam gardens or the *Cyrtosperma* pits of so much of Micronesia been high among the priorities for investigation. The suggestion that intensive *Cyrtosperma* agriculture began on Majuro when the atoll was first occupied about 2000 years ago (Riley 1987: 249) may encourage further investigation of these features. The study of prehistoric horticulture throughout Micronesia has a major role to play in developing our understanding of human adaptation to individual island environments, as well as throwing light on Micronesian origins and interisland contacts. If indeed the high dependence on breadfruit is a relatively recent introduction in Truk, and perhaps elsewhere, this is certainly likely to have had implications for settlement patterns, for as Yen (1973: 72) has observed, the planting of trees restricts possibilities of movement of residence areas.

The role of domestic animals in Micronesian economies is another area in which information has come to light by accident rather than as a result of the pursuit of particular research objectives. Here, too, there are problems to be resolved in understanding the variations in prehistoric life on the variety of small islands which constitute Micronesia. The dog was apparently present at an early date in Truk (Shutler *et al.* 1984: 23), and perhaps also on Pohnpei (Ayres and Haun 1980: 149), with occasional appearances on some of the atolls such as Majuro (Rosendahl 1987: 152–154), Ngulu (Intoh 1984: 75–7), and Nukuoro (Davidson 1971: 89–90), but not Kapingamarangi (Leach and Ward 1981). Its presence in the Marianas is uncertain (Takayama 1982: 96; but cf. Takayama and Intoh 1976: 6, 26). There is now some convincing evidence that the pig was once present in Belau, and at least one of the western atolls (Intoh 1986). It is fairly easy to understand why pigs and dogs may have disappeared from atolls; rather less easy to understand why pigs should have died out in Belau and, for that matter, why neither pigs nor dogs seem to have become established in the Marianas.

Here, too, research in Micronesia will best prosper if it does not proceed in a vacuum. Detailed study of the species and varieties of pig present in the Pacific (Groves 1983) will contribute to an understanding of interisland movements and contacts in the past. There is also much to be learned about the ability of pigs and dogs to survive on long voyages and on small islands. Can the failure of the pig to reach New Zealand and Easter Island be seen as an expectable result of a voyage of a certain length rather than as an indication of the infrequency of such voyages? Can studies of pigs and dogs in Micronesia throw light on their distributions elsewhere, rather than vice versa?

In the study of marine exploitation, Micronesian archaeology has been well to the forefront of recent Pacific work, although much of this work, like so much else, is not yet fully published. One of the more interesting results has been the discovery that the prehistoric inhabitants of the Mariana Islands practised big game fishing on a scale associated in the popular mind only with Polynesians, but very seldom documented by archaeologists in Polynesia (Davidson and Leach 1987; Leach, Fleming *et al.* n.d.). In fishing, as in long distance voyaging, Micronesian people may very well prove to be the true "Vikings of the Pacific".

The identification of fish bones from Micronesian archaeological sites has already proceeded beyond the initial stage of documentation and quantification of remains from individual sites and projects to the point where size reconstructions of a particular fish family and study of the prehistoric and early historic exploitation of that family in a number of different islands is possible (Fleming 1986).

Ethnoarchaeological studies of marine exploitation have also been undertaken and include such diverse topics as shellfish exploitation in Truk (King and Parker 1984) and the former use of fish traps in Yap (Hunter-Anderson 1984). There is great potential for more studies of this kind.

SETTLEMENT PATTERNS

One of the most important fields of endeavour in recent Micronesian archaeology has been settlement pattern studies. This was one of the first kinds of study to show promising results when the boom began in the late 1970s, and already in 1982 it was possible to claim that an excellent picture of late settlement patterns had been achieved for all major islands (Cordy 1982: 127). It has been particularly pleasing to me to see my very preliminary work in the Eastern Carolines 20 years ago (Davidson 1967c) followed up by solid and sustained work in Pohnpei and Kosrae, and my predictions about the potential for such work so satisfyingly fulfilled. It is also pleasing to see that researchers have been willing to make a heavy investment of time and resources in this kind of study. Few parts of Micronesia have been untouched by settlement pattern studies (e.g., Masse *et al.* 1984; Hunter-Anderson 1982, 1983; Craib 1984, 1986; King and Parker 1984; Ayres and Haun 1980). It is only to be hoped that support will continue to be forthcoming for this kind of work, for there is a great deal more still to be learned.

SOCIAL COMPLEXITY

The study of social complexity has become one of the major preoccupations of archaeologists working in Micronesia. The various island groups offer a "laboratory" setting at least as good as that of Polynesia for studying complex societies and their histories. At the same time, Micronesia, with its generally smaller land areas, smaller societies, some relatively complex atoll societies, and different kinds of social organisation, offers a valuable contrast to Polynesia.

In the Central and Eastern Carolines, there is a contrast not merely between the complex societies of Pohnpei and Kosrae, but between those on one hand and the very different society of Truk on the other. It is possible to argue that the inhabitants of these three islands, at least, may share a common inheritance from a single ancestral society. The possible independent ancestry of the societies of Yap, Belau and the Marianas adds a new dimension to the study of social complexity which is lacking among the supposedly closely related societies of Polynesia. Whether the contrast is seen as between "simple-ranked and complex-ranked" (Cordy 1985) or between "power-based and resilient" (King and Parker 1984), and however these developments are explained (Graves 1986; Gumerman 1986), there is plenty of scope for further exploration of this field of enquiry.

The nature of proto-Oceanic society and indeed proto-Austronesian society are matters of great interest in our discipline at the present time (Pawley and Green 1984; Blust 1980). Micronesia, so much smaller in scale than Melanesia, yet more diverse and of less certain ancestry than Polynesia, is uniquely placed to make a really valuable contribution to this debate.

PEOPLE

Ultimately, we are concerned with people in the past. The physical origins of Micronesian people have largely been approached through studies of living populations, and only limited use has been made of prehistoric human remains (Howells 1973). The modern boom in archaeology does not so far seem to have been accompanied by a fresh burst of physical anthropological studies. This is a pity, for there have been considerable advances elsewhere in studying the life histories of individuals and the life styles of small communities through the remains of the people themselves, as well as gaining new understanding of human biology in the Pacific (Houghton 1980; Sutton 1979)³.

Much recent archaeological work, of course, has yielded no human remains, or only a few fragmentary remains. As has been found in New Zealand, even the most fragmentary remains can yield important information about life and death in the past. There are existing resources, however, particularly the material from Hornbostel's work in the Marianas, which will richly repay renewed study.

MICRONESIAN ORIGINS AND INTERISLAND CONTACTS

If we are fully to explore the diversity of human adaptations in Micronesia, we need to know where Micronesian people came from and what baggage they brought with them to their islands. We must also try to trace the broad patterns of contact and interaction within Micronesia over the last 3000 years, and identify the new influences which have filtered in from time to time from various directions.

A healthy sign of the change in emphasis in Micronesian prehistory has been the move away from the "stepping stones to Polynesia" model of Micronesian settlement. In Micronesia, as elsewhere in the Pacific, evidence from other disciplines, and particularly from linguistics, has provided a new and different model of origins and colonisation, which most archaeologists have happily accepted. This new model, which has now become the orthodox model, has been advanced by a number of people (e.g., Shutler and Marck 1975), and is well summarised by Bellwood (1978: 282). It postulates the independent settlement of *Belau*, the Marianas and possibly Yap from Indonesia or the Philippines, and of Eastern Micronesia from the same part of Eastern Melanesia that Polynesia was colonised from. This "basic dual origin" of the Micronesian cultures has been overlaid by a great deal of subsequent contact, and there has been contact also with Polynesian cultures across the boundary between Kiribati and Tuvalu and with the intrusion of Polynesian speakers to Nukuoro and Kapingamarangi.

Some studies in physical anthropology have tended to support this model by distinguishing western and eastern Micronesians, although these two divisions can appear to cluster with different groupings of Melanesians under certain circumstances (Howells 1973:40)⁴.

The widespread acceptance of this model is in line with a predominant tendency to accept that present day language relationships can provide information about past movements of people. This is seen not merely in the orthodox view of Polynesian dispersal (Jennings 1979: 3; cf. Kirch 1986) but in its more ambitious form in the current models of Austronesian expansion into the Pacific (Bellwood 1983, 1984) and in the growing tendency to equate Lapita colonisation with the dispersal of the Oceanic subgroup of Austronesian languages (Bellwood 1983: 78; Pawley and Green 1984: 142). A few scholars have cautioned against too ready an acceptance of present day languages as a map of past movements

of people, from Biggs' often quoted (but seldom really heeded) warning voiced at Sigatoka 18 years ago (Biggs 1972), to Terrell's recent and more vigorous rejection of the linguistic tyranny (Terrell 1986). I think we should be wary of wholeheartedly accepting the orthodox model of Micronesian settlement (cf. Takayama 1984). At the same time, Bayard (1987: 116) has a point when he cautions that "to ignore its implications [i.e., the implications of Austronesian linguistic evidence] in our present state of knowledge is simply foolish".

Linguistics has misled Pacific archaeologists in the past, and no doubt will do so again. The initial linguistic model for the settlement of Fiji and Western Polynesia was unsatisfactory and led to contortions by archaeologists trying to fit their data to it; linguists in turn were misled by incomplete archaeological data in the attempts to identify a proto-Polynesian homeland. It is not unduly pessimistic to expect similar problems to arise in Micronesia, but we can do our best to guard against them.

One problem for archaeologists is that linguists keep changing their own models as their discipline progresses. Archaeologists accepted Pawley's (1972) version of the Eastern Oceanic hypothesis with enthusiasm as a guide to Polynesian and Nuclear Micronesian origins, but Eastern Oceanic seems to be back in the melting pot of Oceanic subgrouping (Pawley and Green 1984; Blust 1984) and is still undergoing revisions and redefinitions. If Lapita is to be promoted to the status of a proto-Oceanic-speaking culture complex, this may not matter very much, but I suspect that Melanesian prehistory will eventually prove more complex than that. At this stage it might be wisest simply to accept as a working hypothesis the idea that some of the early Micronesian settlers may have come from somewhere in Eastern Melanesia, rather than tying their departure point too closely to Malaita-San Cristobal, northern Vanuatu, or the central Pacific. There is far too much still to be learned about pottery in Melanesia in the last 3000 or 4000 years to be discouraged if early Trukese or Pohnpeian pottery is not instantly recognisable as Lapita-derived; at the same time we cannot yet totally exclude the possibility that Trukese or Pohnpeian pottery may have antecedents further west.

Terrell, like Biggs, has particularly criticised the idea that modern language distributions can be used to plot past migrations in an A to B to C fashion, arguing that the distribution of the major subgroups of Austronesian may be merely geographical, and not necessarily the result of a chronological sequence of dispersal (1986: 248-9). Somewhat the same problem arises with the Nuclear Micronesian languages. Linguists suggest an A to B to C dispersal from Eastern Melanesia to Kiribati and then gradually north and west, finishing with the rapid and relatively recent expansion of the Trukese continuum as far west as Sonsorol and Tobi. Yet the concept of the overnight voyage as a factor in maintaining mutual intelligibility (Marck 1986), together with the known voyaging propensities of Trukese speakers, suggests that here, too, geographical rather than historical realities may be involved.

In assessing the usefulness of this orthodox model of Micronesian settlement, we must consider whether a fundamental assumption inherent in this kind of model can be made about Micronesia. Can we safely assume that each island was effectively colonised only once by people who spoke a language directly ancestral to the language spoken on that island in recent times? I am not at all certain that the answer is yes in Micronesia. For one thing, I find it hard to believe that the islands of Tuvalu and Kiribati were each independently settled only by speakers of proto-Ellice and proto-Gilbertese respectively, and that the Polynesian/Micronesian cultural boundary has always been drawn between those two groups. For another, I am bothered by the anomalous position of Yapese and the difficulty

of deciding whether it is an Oceanic language or not. Might there not once have been other languages in the area, similar to Yapese, which have since been replaced either by Trukese, or by a non-Oceanic language, such as Palauan? And as mentioned above, I find it hard to decide whether the close relationship throughout the Trukese continuum is due to frequent interaction over a long period, rather than to recent dispersal.

Once we deny that basic assumption of the model, and allow for linguistic replacement in any part of Micronesia, the usefulness of the whole model is severely depleted. Let us therefore continue to accept the assumption for a little longer, and examine the separate parts of the model in a little more detail.

The part of the linguistic model dealing with eastern or Nuclear Micronesian settlement is strongest in its depiction of a Melanesian homeland, and weakest in its specification of an initial base and subsequent westward expansion. The development of pre-Nuclear Micronesian to the point where something called proto-Nuclear Micronesian began to break up need not have taken place on one or more atolls in what is now Kiribati or the Marshalls. Proto-Nuclear Micronesian could have been spoken by people who made pottery and lived in Truk or Pohnpei. It is not necessary to postulate an initial pottery-bearing migration from the west, followed by a movement of aceramic speakers of Nuclear Micronesian languages from the east, to accommodate the existing archaeological evidence, unless of course the pottery from Pohnpei or Truk is shown to be unambiguously derived from a western source, which is not so far the case.

In terms of archaeological evidence, there has always been a lot to be said for a Melanesian homeland. Shell adzes, slingstones, plain pottery, fishhooks, various kinds of shell peelers and a variety of shell ornaments have an antiquity of up to 3000 years in the Santa Cruz region, for instance. In that same region, "Classic Lapita" seems to have coexisted with plain pottery (McCoy and Cleghorn n.d.) and there is a jumble of modern languages, some of which are variously regarded as non-Austronesian, aberrant Austronesian, or both, as well as more recognisable Austronesian languages, including those of several Polynesian outliers. There are plenty of promising Melanesian sources for East Micronesian settlement. The Nuclear Micronesian leg of the model, then, has quite a lot to recommend it.

What of the western part of the model? The peculiar status of the western languages should give us pause. Their very isolated nature should mean either that they have been there for a very long time, or that they formerly had relatives elsewhere (presumably in the Indonesia or the Philippines) which have since been replaced. Any need to postulate replacement of languages is dangerous, since it casts doubt on the reliability of present distributions as a guide to original dispersal. But the archaeological evidence for Yap and Belau, at least, does not seem to support the notion of long isolated occupation. This may prove to be a case where new evidence on language subgrouping or new evidence of antiquity of occupation will suddenly change the whole picture. In the mean time, something does not quite fit.

The useful distinction between western Micronesian cultures with pottery and Nuclear Micronesian cultures without pottery has gone the same way as the equally useful distinction between pottery-making Melanesian Fiji and aceramic Western Polynesia. In the Micronesian case, however, a common ancestral culture has yet to be identified. Indeed, it is possible to write that "there is nothing yet to suggest that settlement of the high island groups of western Micronesia . . . was related in any way" and "it is *likely* [italics mine] that all were settled from areas to the west" (Bonhomme and Craib 1987: 103). Here it seems

to be the linguistic model superimposed upon vestiges of the old stepping stones model which is dominating our thinking. Clearly, there are traits in the western island groups which point to the west; among these would be the Belau "money" and the problematical Marianas rice. Since it is reported that voyagers from the central Carolines sailed beyond Yap and Belau to the Philippines in recent times (Fujimura and Alkire 1984: 69), we should be prepared to find traces of western influence which may not be anything to do with ultimate western origins. I do not think we can at present completely exclude a Melanesian origin with western overlays for at least some of the Western Micronesian island groups.

If we now forget about languages for a moment and look at our region as dispassionately as we can, we can identify a number of points of entry to Micronesia. Those original stepping stones are temptingly poised to receive voyagers from the complicated Halmahera-West New Guinea region as well as from the southern Philippines. The west Micronesian chain as a whole could be said to face towards the Philippines, while almost any part of Melanesia could theoretically be a point of departure for the central or eastern Carolines. Last but not least, there are two other lines of stepping stones: up through Tuvalu from the Central Pacific, and down through the Marianas from Japan. We know or suspect that there has been contact across some of these points of entry in the fairly recent past: from the Philippines to western Micronesia; between Kiribati and Tuvalu; and perhaps between the Carolines and the so-called Micronesian outliers off the coast of New Guinea. For the next few years, at least, Micronesian archaeologists should keep open minds on all these possible routes into the region.

It has been customary to apply what I might call a centrifugal model to Polynesian prehistory; one which looks at the settlement of Polynesia in terms of dispersal from the centre to the margins, one which respects the simplicity and neatness of the related linguistic dispersal model and which by and large ignores the complicated patterns of subsequent contact and influence which are likely to have existed. In Micronesia, a centripetal model may prove more useful; one which emphasises repeated contact across the region, and the passing on of ideas and traits from the margins to the centre. In this case, however, the continuing existence of diversity will have to be explained. The Yapese language was not replaced by Trukese. Pottery making was not reintroduced to Truk from the west. Breadfruit did not become the pre-eminent food in the west. Rice did not penetrate further east. Items of material culture do have apparently distinct distributions. And so on, into the more difficult realms of settlement pattern and social complexity.

Micronesian archaeology is poised to make a real contribution to the understanding of human prehistory in the Pacific. Identifying the origins of Micronesian settlement is likely to be far more difficult than tracing the immediate origins of the Polynesians. Correctly interpreting archaeological evidence of external contacts in terms of people, languages or ideas will not be easy. Yet it is necessary to tackle the culture historical framework boldly and imaginatively if we are to explore the fullest potential of Micronesian prehistory in terms of human adaptation to island life. The task is not easy but the challenge is an exciting one.

NOTES

1. This paper is the text of a keynote address delivered at the Micronesian Archaeology Conference in Guam in September 1987. It has not been revised in the light of papers presented at the conference, although subsequent notes indicate some areas where new information was presented.
2. Evidence for a long occupation sequence on Bikini, beginning in the 2nd millennium B.C., was presented at the conference by Streck (n.d.).

3. A vigorous and stimulating session on physical anthropology presented results of studies in population demography, description of recently excavated skeletal remains, and comparative studies of Micronesians with other populations.
4. Papers presented at the conference emphasised the relative homogeneity of Micronesians, their resemblances to Polynesians and some Asian populations, and their dissimilarity to Australo-Melanesians.

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