

TOO GOOD TO BE TRUE? POST-LAPITA SCENARIOS FOR LANGUAGE AND ARCHAEOLOGY IN WEST NEW BRITAIN-NORTH NEW GUINEA.

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ABSTRACT

In this paper I discuss alternative scenarios for links between linguistically- and archaeologically-attested change in past human behaviour in the west New Britain-north New Guinea region of Papua New Guinea (Figure 1). I focus on some of the different ways in which linguistic and archaeological evidence might reflect patterns of past population movements. After setting out the specifics of the problem, I outline the relevant linguistic and archaeological evidence. I go on to discuss why I think the latter looks the way it does and how it connects readily with linguistic evidence for human migration despite an argument that it may not "look the way it should" if it were truly a reflection of a migration. In doing so, I contrast these developments with the spread of the Lapita phenomenon beyond the Bismarck Archipelago and with the appearance of ceramics on the Papuan south coast. Both of the latter examples conform with conventional expectations regarding archaeological evidence for a substantial relocation of a unified ethnolinguistic population. I conclude that migration can have a variety of archaeological expressions owing to the temporal scale of the cases in question, but I leave for another time the question of why some migrations are much faster than others (see Anthony 1990). Towards the end of the discussion, I speculate at some length on the derivation of post-Lapita ceramics in the west New Britain-north New Guinea region. I should point out that the position taken in this paper regarding the details of the archaeological sequence in the west New Britain-north New Guinea region refines earlier interpretations, even quite recent ones (e.g. Lilley in press a).

THE PROBLEM

The question at issue is how, if at all, Ross's (1988) evidence for an east-west dispersal of Oceanic Austronesian

languages from west New Britain to the Sepik coast and perhaps beyond might be squared with archaeological evidence for dramatic change which occurred in the same region along a chronological gradient from around 1500-1600 cal BP in the east and 1000-1200 cal BP in the west (Lilley 1986, Terrell and Welsch 1997).

Terrell and Welsch (1997:564) argue that their discovery of what they have labelled "Sumalo" pottery, in contexts dating to about 1200 years ago at Aitape on the Sepik coast,

is not inconsistent with the linguistic hypothesis that the presence of Austronesian (Oceanic-speaking) settlements in northern New Guinea...attests to east-to-west movements of people at different times from somewhere in or near the Vitiaz Strait between New Guinea and the Bismarck Archipelago in the last 2000 years...

They go on to note, though, that

If this hypothesis were correct...we would have thought that Sumalo ware would look like ceramic industries of comparable age found archaeologically in the Vitiaz Strait and northeastern New Guinea areas. Based on what is presently known about these parts of [Papua] New Guinea (Lilley 1986), this is not the case. Sumalo ware far more closely resembles undecorated Lapita wares and the early red-slip industries of eastern Indonesia and southern New Guinea than anything thus far discovered in the Vitiaz Strait region.

On this basis, Terrell and Welsch (1997:565) hypothesize that Sumalo ware signals the arrival of pottery-making people who came from somewhere west of Aitape [i.e. in or closer to southeast Asia], not from the Vitiaz Strait. Soon afterwards, however, these settlers were drawn into increasing trade and communication with people to the east of them – as evidenced by the transfer [to the Sepik coast] of obsidian from the Bismarcks and the appearance of pottery styles (incised and ap-

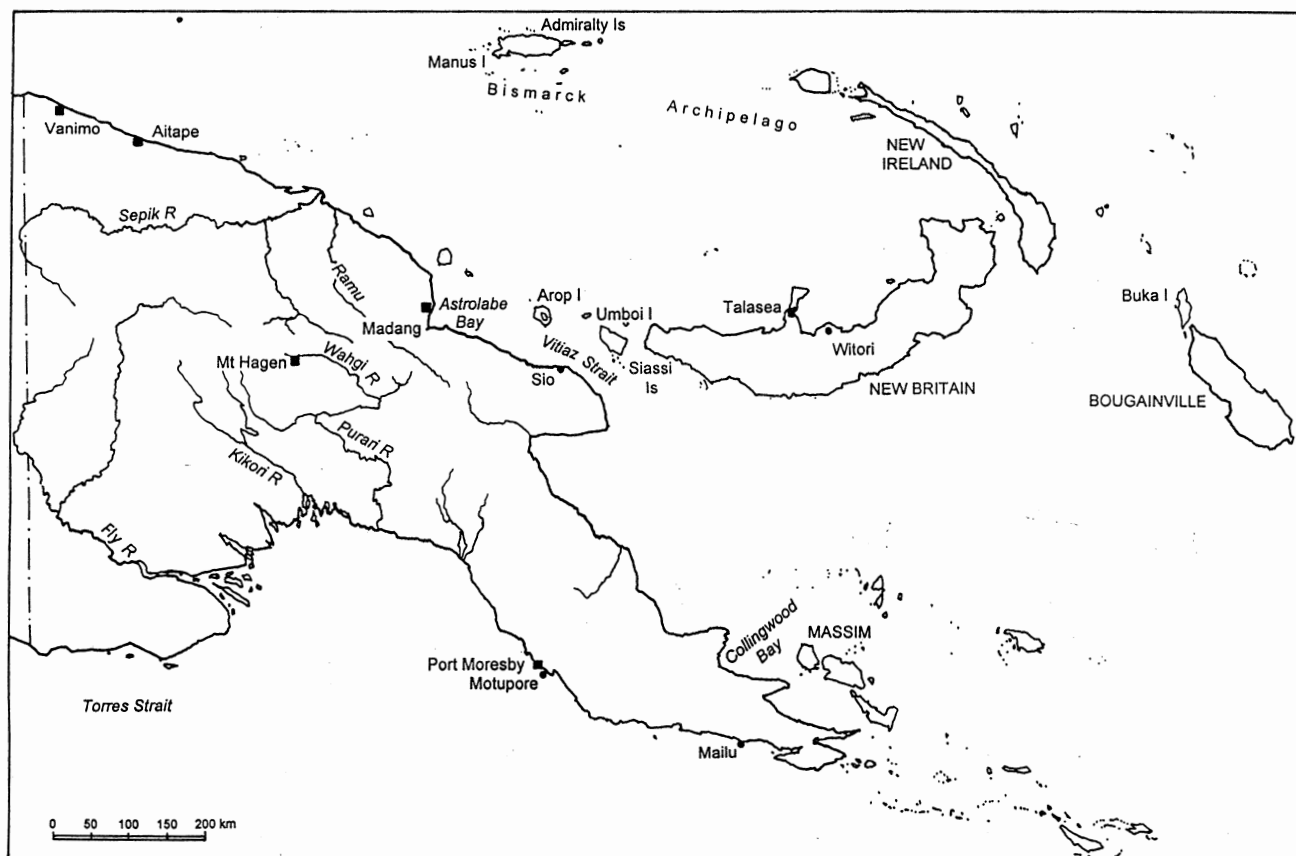


Figure 1: Map of New Guinea showing places mentioned in text.

pliqué) at Aitape that have widespread parallels elsewhere in Melanesia.

Terrell and Welsch's skepticism has much to commend it, but I do not agree with them on the westerly derivation of Sumalo ware even though they cite my work in support of their position. As will be made clear below, I *do* agree that people on the Sepik coast were drawn into "increasing trade and communication with people to the east of them". However, *contra* Terrell and Welsch, I argue that Ross's linguistic evidence can be connected with the post-Lapita archaeology of west New Britain-north New Guinea in a relatively straightforward manner to support the hypothesis of east-west population movement. I thus propose that the Sumalo pottery of the Sepik coast, like the Austronesian speakers who made it and moved it, derives proximally from the Vitiaz Strait and not Iron Age Southeast Asia. However, my propositions are based on premises unlike those conventionally used to underpin arguments concerning archaeological evidence for the

movement or migration of single or closely-related ethno-linguistic groups. Such arguments are based on the idea that bioculturally-related migrating groups should produce similar kinds of pottery (e.g. Adams *et al.* 1978, Anthony 1990, Rouse 1986), whereas I suggest that in circumstances like the one in question here, bioculturally-related migrating groups are equally likely to produce dissimilar ceramics.

THE LINGUISTIC EVIDENCE

I am an archaeologist, not a linguist, so I am unable to judge closely the details of Ross's (1988) re-ordering of our knowledge about Proto Oceanic and the Austronesian languages of western Melanesia. From an informed lay perspective, his work is comprehensive and cogently argued. Moreover, while aspects of it may be debatable, it has not to my knowledge been subjected to any sustained, widespread criticism from within the Pacific linguistic

community. On these grounds, I accept it as the current definitive statement on the linguistic matters in question.

Ross posits the following broad sequence of events (the parts of his complete sequence which relate to developments in other parts of Melanesia have been left out):

1. Proto Oceanic, mother of all Austronesian languages east of about 138°E, emerged from pre-Proto Oceanic, a language of southeast Asian derivation, in the Vitiaz Strait-west New Britain region, probably in the vicinity of the Willaumez Peninsula on the north New Britain coast (Ross 1988:19-22).
2. Western Oceanic then developed in the same general area (Ross 1988:19-27).
3. The language(s) which became the languages of the Papuan Tip Cluster (which includes the Austronesian languages of the south coast of Papua) subsequently emerged in the Western Oceanic homeland. It dispersed south to the Massim, then west along the Papuan coast (Ross 1988:190-211; see also Ross 1998).
4. The language(s) which became the languages of the North New Guinea Cluster (NNG), which includes amongst others all Austronesian languages on the New Guinea coast between the Huon Peninsula and the Irian Jaya border, then emerged in the same Western Oceanic homeland in the Vitiaz-west New Britain region. Languages ancestral to those west of Karkar Island north of Madang dispersed first, probably moving directly to Manam Island, just east of the Sepik-Ramu delta, then moving further west along the offshore islands and, finally, establishing toeholds on the mainland. These languages form the Schouten Chain. Other languages then dispersed southwest to become what are now the relatively distinct languages of the Huon Gulf Family (which excludes the Tami Islands off Finschhafen). Still others later dispersed across the Vitiaz Strait and west as far as Karkar, as well as east to become the distinctive languages of southern New Britain. They now form the Vitiaz linkage (which includes Tami; Ross 1988:160-183).
5. The language(s) ancestral to those spoken in the Siassi islands in the Vitiaz Strait, in two localities on the north New Guinea coast and in three adjoining areas on the north coast of New Britain west of the Willaumez Peninsula, then dispersed from a homeland in coastal northwest New Britain. These languages comprise the Ngero family, which is closely related to but distinguishable from the languages of the Vitiaz linkage (Ross 1988:160-183, esp.173).

The main points to be drawn from this sequence are, first, that the Austronesian languages of the Sepik coast developed from language(s) which descended from the language(s) conceded by many, but not all, historical linguists and archaeologists to be spoken by most, if not all, makers and users of Lapita pottery, and did so in a distant locality. This sequence means the first are separated from

the last by a reasonable period of time and a significant geographical remove. This fact makes direct links between Sumalo and Lapita ceramics unlikely. It must be conceded, though, that it does not rule them out entirely. In addition to the generic visual similarities between Sumalo and Lapita, the only two pieces of Lapita pottery ever found on the New Guinea mainland are from around Aitape (though one is not archaeologically provenanced). Moreover, "a possible tiny trace of [pre-Proto Oceanic] is found in [non-Austronesian] Waskia...of Karkar Island" (Ross 1988:24). Neither Terrell and Welsch nor anyone else is inclined at this stage to make very much of these scraps of evidence (e.g. Terrell and Welsch 1997:558-561, 563-564).

The second point is that NNG languages are related quite closely to, but are probably the results of a more recent dispersion than those of the Papuan Tip cluster. Apart from the presence of a piece of Fergusson Island obsidian in a Lapita site in the Reef/Santa Cruz Islands east of the Solomons (Green 1987), all archaeological evidence which can be linked with an Austronesian-speaking presence in the Massim and on the south Papuan coast dates to 2000 BP or less (e.g. Allen 1977, Irwin 1985). This means that the NNG languages must have emerged primarily within the last two millennia. Third, however, the fact that there is noteworthy internal differentiation within the Cluster and within its principal constituent groups suggests its emergence and major dispersals have a reasonable time depth within the post-2000 BP period. This last means the dispersal along the north New Guinea coast is not likely to be associated with my archaeological evidence for significant change in the Vitiaz Straits in the last few hundred years (Lilley 1986). Ross (1988:425, note 131) hypothesizes these recent changes may be associated with the dispersal of the Ngero family (see 5 above), and I see no reason to disagree with him. In short, it seems most likely that the westerly dispersal of Oceanic Austronesian along the north New Guinea coast occurred between about 2000 BP and say 500 BP. As Ross (1988:425) suggests, it is therefore a good candidate for association with my archaeological evidence for significant change between about 1600 BP and 1000 BP.

Although we have no archaeological idea of what happened in the Huon Gulf, the sequence of linguistic events which Ross proposes fits quite neatly with the post-Lapita archaeological sequence of the region. This last can be developed from Terrell and Welsch's (1997) results from the Sepik coast, my (1985, 1986-7, 1988a,b, 1991) work in the Vitiaz Strait-west New Britain area, Gosden's (e.g. 1991, Gosden and Pavlides 1991) research in the Arawe Islands off southwest New Britain, Egloff and Specht's (1982) investigations on Arop (Long) Island and Egloff's (1975) work on Karkar and around Madang. The linguistic sequence sees groups leapfrogging (cf. Anthony 1990:902-903) from a homeland in the Vitiaz region, first

to the Sepik-Ramu and then to the Huon Gulf, with the areas in-between these extremes in-filled at a later date by speakers of Vitiaz languages, who at the same time were moving east along the south coast of New Britain. As discussed in more detail below, the archaeological sequence has the oldest post-Lapita dates in the Vitiaz-(north)west New Britain area, the next oldest on the Sepik coast, and then similar younger dates for the first appearance of post-Lapita ceramics in the Karkar-Madang area to the west and the Arawe Islands to the east. Both of the latter fall in the area in-filled by Vitiaz languages after the Schouten and then the Huon Gulf languages dispersed from the Vitiaz homeland.

This is exactly the pattern we would anticipate if Ross were correct and the archaeology reflected the linguistics. It seems likely the in-filling process shifted the foci of pottery manufacture and trade in the Vitiaz Strait-Astrolabe Bay region, leading to the appearance of one new pottery manufacturing centre, the demise of one and perhaps two others and the dramatic strengthening of the trading position of a third.

THE ARCHAEOLOGICAL EVIDENCE

Archaeological researches in the west New Britain-north New Guinea area have revealed the following overall sequence of change:

1. Lapita ceramics were deposited during a short-lived occupation of Tuam Island in the Vitiaz Strait. They are well-dated to about 2700 cal BP. They also occur in several sites just west of the Willaumez Peninsula, though dating slightly older (Lilley 1991). Similarly, Gosden (1991, Gosden and Pavlides 1991) found a number of substantial Lapita sites in the Arawe Islands off southwest New Britain, dating from about 3300 BP. Although it occurs in many places on the south coast of New Britain east of the Arawes (Specht *et al.* 1992), there is no evidence for Lapita-based occupation on the Huon Peninsula side of the Vitiaz Strait, nor anywhere else on the New Guinea mainland, except for the two sherds from Aitape mentioned above, which are interpreted as trade items.
2. After Lapita there is a break in deposition – and probably pottery production and trade – of around 1000 years in the Vitiaz Strait, northwest New Britain and the Arawe Islands. In the former it lasts from about 2700 cal BP to about 1500-1600 cal BP, while in the Arawes it seems to last from 2000 BP to about 900 BP. Assuming the integrity of the sites is acceptable, the difference between the two regions seems to suggest pottery – which Gosden argues is the tail-end of Lapita – continued to be made in the Arawes for some time after Lapita disappeared elsewhere in western Melanesia (Spriggs this volume), but then ceased altogether, never to be resumed. Why this might be the case requires further thought and will not

be pursued here. The fact remains that even in the Arawes, where pottery and other material was deposited for perhaps 700 years longer than it was in the Vitiaz-northwest Britain region, a substantial hiatus currently occurs in the deposition of cultural remains after Lapita disappeared.

I should note in this connection that there may be an early post-Lapita ware in the Vitiaz region dating to around 2300 cal BP. I have not yet described it in detail, but I have labelled it Type Y (Lilley in press a). As explained in a forthcoming site report (Lilley in prep), I think it is far more likely that this well-made ware appeared suddenly with two other robust forms in the Vitiaz Strait-west New Britain region about 1500-1600 years ago, following the above-mentioned post-Lapita hiatus in deposition. If it is older than I believe, the presence of Type Y would shorten the post-Lapita hiatus in the Vitiaz region to about 700 years, but that is still a substantial gap in occupational debris within the time scales in question here.

As just intimated, two or more probably three quite dissimilar kinds of pottery appeared *de novo* in the Vitiaz Strait-west New Britain region about 1500-1600 years ago. I have mentioned one of them, Type Y. As yet I have little further information about this ware, but it is very distinctive visually and petrologically, and on the latter basis may originate in west New Britain (Glenn Summerhayes, then La Trobe University, pers. comm. 1995). As described in detail elsewhere (Lilley 1986, 1986-7, 1988a, 1991), one of the other two wares, Ancestral Sio, is the precursor of a trade ware still produced at Sio on the New Guinea mainland. In addition to its antiquity, the ancestral form is characterized by its petrological characteristics, the shape and robustness of its rims and by aspects of its decoration. The third kind of pottery that appears in the Vitiaz region around 1500-1600 cal BP, a highly-distinctive burnished ware called Type X (Lilley 1988b), was probably manufactured towards the eastern end of the Huon Peninsula. It has been found in northwest New Britain dating to the same period (Lilley 1991). I am not certain it occurs in the Arawes or further east on the south coast of New Britain, but it occurs on Tami Island on the northern edge of the Huon Gulf and on Long Island, just west of the Strait. Up to now, it has not been described from sites west of Sio on the north New Guinea coast.

Type X may have been the main form of pottery moved across the Vitiaz Strait until its manufacture ceased between 800 cal BP and 500 cal BP and it may have been moved primarily by traders from Tami Island rather than the Siassi Islanders who dominated cross-strait trade in the historic period. However, from around 1000 BP, pottery directly ancestral to modern trade wares made near Madang began to appear at Sio. Though the oldest dates for Madang ware in the vicinity of Madang itself currently stand at only about 500 BP, Egloff and Specht (1982)

have a date of about 1000 BP for Madang ware from Arop (Long) Island, between Siassi and Madang. At about the time Madang pottery first appeared at Sio, Gosden's (1991) excavations reveal that Sio pottery and perhaps Type X first appeared in the Arawe Islands. Taken together, the foregoing suggests there was an expansion of pottery manufacture and trade both to the east and the west of the Vitiaz Strait from around 1000 BP.

Over the next few centuries, this development may have led to the demise of Type X (and of Type Y as well if it dates to this period). However, at the same time that the manufacture of types X and perhaps Y ceased, Sio and to a lesser extent Madang pottery came to be characterized by the hallmarks of semi-specialist production for trade, and the former in particular began to be moved across the Vitiaz Strait in vast quantities, especially in the last 300-350 years.

Terrell and Welsch's date of roughly 1200 BP for Sumalo ware clearly fits between the first appearance of post-Lapita ceramics in the Vitiaz Strait-west New Britain region about 1500-1600 BP and the expansion of pottery manufacture and trade between Madang and the Arawes from about 1000 BP. In view of the other archaeological evidence, the date for Sumalo ware sits comfortably with the idea that pottery-making Austronesian-speakers dispersing from a homeland in the Vitiaz region leapfrogged the area between the Huon Peninsula and Karkar Island to settle on the Sepik coast or adjacent islands such as Manam. Within the next couple of centuries, the area they had initially avoided was then filled-in by pottery-manufacturing speakers of Austronesian languages related to theirs. A leapfrog-and-infill pattern such as this is very common in human migration (Anthony 1990).

The foregoing fit between archaeology and language seems tight, but it may appear to those with some knowledge of Ross's work that there is a critical discrepancy. On the archaeological evidence, the inhabitants of Sio played a pivotal role in the prehistoric developments just outlined, as they did in the trade of historic times. The archaeological dates from Sio predate those for Sumalo ware by some centuries. On the basis of my earlier discussion of the linguistics, however, it could seem as if Ross has things the other way around: he argues the Schouten languages of the Sepik dispersed from the Vitiaz hearth area before the Vitiaz linkage to which Sio belongs. This requires an historical account of how current ideas about the region's prehistory developed. Initially Ross (1988:426) linked the dispersal of Vitiaz linkage speakers with the developments that I propose to have occurred in the Astrolabe Bay-Vitiaz Strait-west New Britain region between 1600 and 1000, but he did so about a decade before Terrell and Welsch published their dates for Sumalo ware. Moreover, this paper is the first place I have teased apart the initial appearance of pottery at Sio and Madang. Previously I lumped them as parts of a broadly-dated and

only vaguely characterized period of change. While I still think they are linked, it is now possible to refine our view of their chronological relationship considerably, as is done in this paper.

In addition, the fact that most of the more easterly and westerly languages of the Vitiaz linkage may have dispersed towards their present location from say 1000 BP does not mean *all* Vitiaz languages were shifting geographically at that time. In fact, the languages in the centre of the Vitiaz Strait homeland have probably not shifted noticeably for quite a long time. Ross (1988:160) points out that

Proto North New Guinea...was apparently that part of the Oceanic dialect linkage that was situated in the areas of the Vitiaz and Dampier Straits. Proto Vitiaz was the continuation of that linkage after the communalects ancestral to the Schouten, Huon Gulf and Ngero groups had separated from it.

With Tami and Mangap (which is spoken on the eastern side of Umboi (Rooke) Island), Sio is one of the three individual languages in the Vitiaz linkage which subgroup with no others. This suggests they have all been in situ for a considerable period. This notion is supported by the archaeological evidence from Sio itself, which shows no signs of cultural discontinuity from the time the site was first occupied around 1600 years ago. Some characteristics of Sio pottery do change quite dramatically after the proposed expansion of pottery manufacture and trade from 1000 BP. However, the changes in the ceramics do not occur until at least 800 BP, and perhaps not until around 600 BP. Moreover, they are evolutionary in nature rather than of a revolutionary sort which could more readily be linked with the replacement of an existing pre- or non-Vitiaz linkage population by speakers of Sio. In short, while Sio might be a Vitiaz linkage language, it has almost certainly been spoken in and around its present location since before the dispersal of the Schouten languages to the Sepik and of those Vitiaz linkage languages east and west of the Vitiaz Strait. There is no discrepancy between the archaeology and linguistics here.

MIGRATION AND POTTERY

If the archaeology and linguistics link as tightly as I have just proposed, why is Sumalo ware so unlike the wares of the Vitiaz region from which it would presumably derive? Viewing the ceramics in question in the light of the basic principle that bioculturally-related migrants tend to produce stylistically similar material culture, Terrell and Welsch would seem to have a point: the supposedly close archaeological links with language may in fact be too good to be true. Lapita pottery, a distinctive ware especially in its decorated forms, is often held to be so standardized because it was made by people who maintained close sociocultural ties as they moved beyond the Bis-

marks through a vast area of the southwest Pacific (e.g. Green 1991). Similarly, the similarities amongst early red-slipped ceramics of the south coast of Papua are usually causally linked with the westerly migration of culturally closely-related people from the Massim (Ross's Papuan Tip speakers; see Allen 1977, Irwin 1985, Ross 1998).

Terrell kindly allowed me to examine a small sample of Sumalo ware in hand specimen. As he and Welsch point out, it does look more like plain Lapita and various other red-slip wares than it does Sio ware or Types X and Y. It is not so completely dissimilar to Madang pottery, which is red-slipped (and, also like Lapita, shell tempered; Lilley 1986). However, on the basis of the small sample of Sumalo ware I examined, I would hesitate to say the likeness is anything more than superficial and probably only coincidental. Thus the archaeological situation on the north coast seems to provide little if anything in the way of conventional evidence for population movement when compared with examples such as the spread of Lapita beyond the Bismarcks or the colonization of the Papuan south coast by pottery makers, where the close sociocultural links that are argued to have existed between migrating groups are clearly reflected in the spread of stylistically similar ceramics. So what might have happened to produce the situation in the Vitiaz Strait-north New Guinea region to produce the pattern described above?

I think "what happened" was *not* that linguistics and archaeology came to match only coincidentally in the manner implied by Terrell and Welsch, thus misleading us into seeing the links as the result of population movement. Rather, I hypothesize that the Vitiaz Strait-north New Guinea ceramic pattern results from migration, but ethnolinguistic population movement of a different sort from that associated with the Lapita phenomenon and the south Papuan ceramic pattern (and thus from that which forms the focus of work by the likes of Rouse (1986); cf. Anthony 1990:897-898).

There are three main reasons why I think it is worth exploring the foregoing hypothesis. First, Sumalo ware is not alone in its dissimilarity from the other ceramic wares in the Vitiaz-north New Guinea region. *None* of these wares is like any other except in the grossest terms (e.g. they are all made of clay). What is more, they were all quite distinct from the time of their first appearance. Thus it is *not* the case that there is a group of similar wares found between west New Britain and Astrolabe Bay, and then a dissimilar ware on the Sepik coast to the west. Second, as Terrell and Welsch themselves point out, Sumalo is generically like Lapita and south Papuan coast ceramics as well as Metal Age southeast Asian wares. This fact (and also Sumalo's general similarities with Madang ware) suggests that an easterly (Melanesian) derivation for the Sepik material is as likely as a westerly (Asian) one. Finally, the other evidence for external links and influences cited by Terrell and Welsch, namely obsidian and

applied-and-incised ceramics, points to regions east of the Sepik rather than to Asia. In short, the empirical evidence marshalled by Terrell and Welsch provides no immediately compelling reason to eliminate a link between Sumalo and ceramics from further east, or to dismiss the connections made earlier between post-Lapita archaeology and language in the region of concern.

In addition, it can be noted in support of the idea that all the pottery in question is historically linked to the dispersal of related Austronesian-speaking groups that all of the north New Guinea trade potters operating historically were speakers of NNG languages (except Sialum, a non-Austronesian community which developed ceramic manufacturing after WWII and intermarriage with Austronesian-speaking women from the Gitua community to the west near Sio; May and Tuckson 1982:151). In the absence of any evidence to the contrary, such as archaeological signs of population replacement, this strongly suggests that the two archaeological Vitiaz-north New Guinea wares which are still made, Sio and Madang, were originally established by speakers of early NNG languages. The languages of the makers of types X and Y are unknown, but on the foregoing basis the probabilities lie with the North New Guinea Cluster.

Given that all of this evidence and argumentation points to the migration of bioculturally-related people, why does Sumalo not look like more easterly wares (and by implication, why do those more easterly wares not look much more alike than I have elsewhere shown them to be)? This is a matter of the utmost seriousness which brings weighty and well-established theoretical expectations into play (e.g. Adams *et al.* 1978; Anthony 1990; Rouse 1986).

I do not propose to rehearse the theoretical considerations at issue. Rather, I will simply outline what I hypothesize to have been the case. First, the radiocarbon chronologies indicate that the appearance of pottery (and thus, by my argument, the spread of NNG-speakers) along the north New Guinea coast occurred much more slowly than the dispersal of Lapita potters beyond the Bismarcks or the colonization of the Papuan south coast by potters from the Massim. In the latter cases, the spread was archaeologically very rapid, in the case of Lapita over a vast area. The proposed dispersal of NNG, on the other hand, occurred over more than half a millennium.

Second, in view of the time involved, and the likelihood that it allowed and perhaps encouraged a period of interchange and perhaps physical exploration, I suggest that the territory into which NNG-speakers dispersed was more familiar to those on the move than were the territories encountered by dispersing Lapita potters or the first Papuan Tip speakers to move along the Papuan south coast.

Third, owing to the first and second factors, I suggest that while they may have been linguistically distinct, dis-

persing NNG speakers were otherwise culturally more similar to those whose territories they were moving into than was undoubtedly the case for Lapita potters, the first Austronesian speakers in the Pacific, or Papuan Tip speakers, the first Austronesian speakers on the Papuan south coast.

The foregoing differences between dispersing NNG speakers on the one hand and dispersing Lapita potters and Papuan Tip speakers on the other suggests that in the first case there would have been little and probably no need for the migrants to increase their chances of survival by symbolically bolstering in-group cohesion through standardized ceramics, as I (Lilley in press a, b) and others have argued was the case with Lapita (and, by extension, similar examples such as the Papuan south coast). Lapita potters and dispersing Papuan Tip speakers seem to have focussed on contact amongst themselves rather than with bioculturally unrelated groups. In the NNG case, however, the archaeological situation and its well-described ethnographic endpoint indicates that dispersing potters survived in large part owing to their interaction with non-potters (presumed to be non-NNG-speakers). Interaction of this sort requires difference or otherness to succeed, and, as the ethnographic situation demonstrates (e.g. Harding 1967, Lilley 1986), in such situations people can go to extraordinary lengths to establish and maintain some form of distinctiveness, even in the absence of any obvious environmental imperative to do so.

In a case where people are culturally similar but linguistically distinct (as has been the case historically on much of the north coast; e.g. Welsch *et al.* 1992), and where migrating groups need to differentiate themselves from their source populations as much as from their new neighbours if they are to establish the new interaction networks they use to survive and prosper, it would behove them to establish emblems such as distinctive pottery from the outset. From this perspective, the distinctive ceramics seen in the NNG situation are critical to relations *among* identity-conscious ethnolinguistic groups, slowly moving into the territories of groups with whom they have close and multifaceted ties. The standardized pottery seen in the Lapita and south Papuan cases, on the other hand, seems to be associated with relations *within* an identity-conscious ethnolinguistic group very rapidly colonizing new territory with few other means of sociocultural support to enhance their survival rates.

It seems to me that differences in temporal scale are the most likely proximal cause of the differences between the NNG case and Lapita and the Papuan south coast. Issues of geographical scale might also emerge were the NNG situation and that on the Papuan coast similar, and Lapita differed from both in the manner described above, owing to the vast scale over which the Lapita phenomenon unfolded. However, the distances involved on the north New Guinea coast and the Papuan south coast are similar,

which eliminates geographical scale from consideration here. It is the speed with which populations moved, not the distance over which they moved, that seems to be critical to the way issues of identity affected the archaeological expression of migration in the cases in focus. It is difficult at this stage of research to hypothesize why the NNG process took such a long time in comparison with either of the other examples. It almost certainly relates to differences in the "negative 'push' and positive 'pull' factors...and intervening obstacles" involved (Anthony 1990, also Davis 1974). However, just what the factors and obstacles were and how they affected the specific developments in question requires further research. I (1990 and in press b) have elsewhere discussed such issues in relation to the origins of the Lapita phenomenon in the Bismarck Archipelago, though I have not yet considered whether the specific factors also apply to the spread of Lapita beyond the Bismarcks (which they would have to do to be relevant here).

WHENCE THE POST-LAPITA POTTERY?

From where might the post-Lapita wares of the west New Britain-north New Guinea region derive? My preliminary thoughts are as follows. It will be recalled that there is a significant temporal gap in ceramic production and deposition between terminal Lapita and the pottery in question. There is thus no direct Lapita derivation for the post-Lapita material. Furthermore, unlike the red-slipped wares at the start of the ceramic sequence on the Papuan south coast, which were similar from one end of that sizeable region to the other, the west New Britain-north New Guinea wares including Sumalo are completely distinct from each other from the time they first appear. In view of such dissimilarities, it is possible that at least the three earlier and more easterly industries, Sio, Type X and Type Y, arose completely independently, the similar dates at which they appear and likely proximity of their centres of manufacture notwithstanding. I think this possibility is inherently unlikely. I propose instead that the Massim and perhaps the Papuan south coast were in fact linked with the events and processes on the north coast that I have described, as hinted by initial dates of about 2000 BP for ceramics in the latter region, as well as by Egloff's (1979) hypothesis regarding parallels between certain ceramic forms from Collingwood Bay in southeast Papua and others from island southeast Asia. Together these factors suggest that there may have been westerly chains of connection reaching from the southeastern tip of Papua through the Vitiaz Strait to the north New Guinea coast and beyond to the west. These connections may have been of a form which at an earlier date than that considered by Egloff could easily have seen a diffusion of pottery technology from the Massim to the Vitiaz region, giving rise to Sio, Type X and Type Y wares around 1600 BP and, later, Sumalo and Madang wares.

Although this proposition is conjecture at present, I noted that after reading the conference presentation in which I first raised this idea (Lilley 1997; also in press a), Ross (pers. comm. 1997; my emphasis) alerted me to linguistic evidence for "a later Massim intrusion into the [Port] Moresby area when it was *already Austronesian speaking*". As he (1998, also 1994) has since discussed at some length in print, this intrusion could relate to a much-remarked shift some 1000-1200 years ago in the archaeological record of the Papuan south coast (e.g. Allen 1984, Irwin 1985, Vanderwal 1973). In my view it may also relate to the developments in the north New Guinea-west New Britain region which I have described above. If this were so, it would pay to investigate more closely the possible links between the south and north coasts that I have noted elsewhere (e.g. Lilley 1986:479-486) but hitherto have not discussed in detail. Ross (pers. comm. 1997) also pointed out that he "now think[s] that there was probably a New Guinea Oceanic supercluster, i.e. that the languages of the [North New Guinea] and Papuan Tip clusters are more closely related to each other historically than either is to the [more easterly] Meso-Melanesian cluster, since the first two clusters share a few innovations". By this he means that he is now inclined to agree with Milke's (e.g. 1965) New Guinea Oceanic hypothesis, which he previously thought (Ross 1988:351) could not be sustained.

If this linguistic connection can be demonstrated beyond doubt, its archaeological manifestations could easily encompass a south-north transfer of pottery technology from the Massim to the Vitiaz region of the sort I envisage. At this juncture, it is also important to note that Ferguson Island in the Massim is one of only three obsidian source-areas known in Near Oceania (Bird *et al.* 1981). The others are in the Talasea-Cape Hoskins area of New Britain, mentioned again below, and in the Admiralty Islands on the northern edge of the Bismarcks (Figure 1). A link between obsidian-producing areas in the Massim and New Britain may prove pivotal to the model of change in northwest Melanesia that I am sketching. To outline some ideas I will enlarge upon elsewhere, it may be that between the terminal phase of the Lapita period and no later than about 2000 BP, speakers of New Guinea Oceanic languages spread from west New Britain to the Massim to develop further an obsidian resource. This resource, while it may have been known from at least Lapita times (Green 1987), was certainly not exploited in any significant way until the last "couple of thousand years" (Green 1987:245), from which time it was used throughout the Massim and along the Papuan south coast (Ambrose 1976). This last was undoubtedly a product of the expansion of speakers of emerging Papuan Tip languages along the south coast, as is well-demonstrated archaeologically and linguistically.

In a later pulse of activity, Papuan Tip speakers may have reached directly or through intermediaries back to-

wards the Vitiaz Strait about 1500-1600 years ago, where they connected in some manner with dispersing North New Guinea speakers, resulting in a south-north transfer of pottery technology which underpinned the development of Sio, Type X and Type Y ceramics. They may have reached out again along the Papuan south coast about 1000-1200 years ago, where the evidence of their influence, and perhaps their physical presence, seems to be attested by the aforementioned shifts in the archaeological and historical linguistic records there. It is possible this last pulse of activity in the Massim had a northwards extension as well as a westerly one. If so, it may have had some impact on developments in the west New Britain-north New Guinea region that saw the dispersal of Schouten Chain speakers and, later, Vitiaz linkage speakers, as well as the emergence of Sumalo and Madang wares.

CONCLUSION

In this paper I have addressed differences in processes of population movement that are apparent between post-Lapita developments in the west New Britain-north New Guinea region and the Lapita phenomenon and post-Lapita developments on the Papuan south coast. It has been suggested that the proximal cause of these variations is the temporal scale of the migrations in question and concomitant differences in the way the migrant communities interact with each other and with the people into whose territory they move. Specifically, it was hypothesized that in the cases of Lapita and the Papuan south coast, where people moved swiftly into poorly or unknown territory, the migrants were principally concerned about interactions with each other, whereas in the west New Britain-north New Guinea area, where the migrants moved more slowly and into better-known or familiar territory, people are more concerned with their interactions with their neighbours.

I also argued that the immediate origins of the relatively recent post-Lapita pottery discovered by Terrell and Welsh on the Sepik coast may lie to the east in the Vitiaz Strait area rather than to the west, in Iron Age southeast Asia, as those scholars propose. I went on to speculate that the Vitiaz pottery may in turn have its origins in the Massim, to the southeast, and that it may therefore be more directly linked to developments on the Papuan south coast than has previously been thought.

Obviously much needs to be done to flesh out and test my tentative hypotheses and even more tentative suggestions concerning the matters at issue. Considerably more archaeological field work needs to be undertaken throughout New Guinea (i.e. PNG and Irian Jaya), but in the present instance I suggest a focus on the north coast in the areas west of the PNG border and between the Vitiaz Strait and Astrolabe Bay, as well as on the northeastern coast between the Huon Peninsula and the Massim. Investigations in Irian Jaya and around Astrolabe Bay would

help establish the nature of links between the Sepik and Madang and areas to their east and west. Exploration of the northeast coast, on the other hand, would enable study of the nature and history of links between the Massim/Papuan Tip region and the west New Britain-north New Guinea area. These areas have long been treated as almost completely separate spheres of interaction. This perhaps owes something to the colonial separation of (originally British) Papua and (originally German) New Guinea, but there is a real dearth of ethnographic evidence for significant ties in recent times. However, if there is anything archaeology has taught us about recent patterns of interaction in coastal and island Papua New Guinea, it is just that: they are (often very) recent, and frequently bear little obvious resemblance to what went before them.

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