

LINGUISTICS, ARCHAEOLOGISTS AND AUSTRONESIAN ORIGINS: COMPARATIVE AND SOCIOLINGUISTIC ASPECTS OF THE MEACHAM- BELLWOOD DEBATE¹

Donn Bayard

Department of Anthropology, Otago University, Dunedin, New Zealand.

ABSTRACT

Archaeologists and linguists are often uncertain about the relative value of the other discipline to their own work and frequently dismiss, or misinterpret, results which do not "fit". The author, a prehistorian and linguist, stresses the mutual and positive value of debate between practitioners of the two disciplines. In the case of Austronesian origins the author feels that the Blust-Bellwood model takes better account of more of the archaeological and linguistic data than the alternative scenarios proposed by Meacham and Solheim.

SOME POSITIONS IN THE DEBATE

At the risk of seeming hyper-sceptical, I must confess to the conviction that linguistics has very little to contribute to the writing of prehistory, especially regarding population movements and cultural development (Meacham 1988: 92).

As a non-linguist, I find myself suspicious of conclusions drawn from comparative linguistics, especially where apparently clear patterns are presented despite a stated inadequacy of data and the need to go beyond the lexicon (Specht 1979: 155).

There may yet be a few prehistorians who remain unconvinced of the utility of [linguistic] data to our discipline; if so, I can only urge them to become more acquainted with linguistic methodology. Its limitations as expressed above are far outweighed by its value, and to ignore its implications in our present

state of knowledge is simply foolish (Bayard 1987: 116).

But my initial faltering wish to contribute was strengthened when my reading in the rather hazy overlap area between the fields of archaeology and language brought me to glimpse the possibility of an emerging synthesis on a grand scale (Renfrew 1992a: 11).

INTRODUCTION

Here we obviously have two diametrically-opposed views of the relationship between linguistics and archaeology. As one who has spent considerable time working and teaching in Renfrew's "rather hazy overlap area" for the past 30 years I would like to offer some thoughts on the reliability of linguistic prehistory, particularly as applied to the question of Austronesian origins. In the last few years an increasing number of archaeologists have become interested in the historical syntheses of comparative linguistics. Much of this interest has been stimulated by Renfrew's grand syntheses employing the mechanism of agricultural expansion to explain first the dispersal of the Indo-European (IE) languages (1987, 1988); and more recently the distribution of all of the world's major language families through the processes of initial colonisation by foragers, agricultural expansion and elite dominance (1992a, b).

As one who thinks of himself as both a linguist and prehistorian, I obviously welcome this development. I have no qualms at all with the general thesis that languages spoken by agriculturalists will tend to expand at the expense of languages spoken by foragers; this seems only common sense. However, I think that Renfrew's

"grand synthesis", like most such syntheses, has some serious specific difficulties, in most cases acknowledged by Renfrew himself. A few of these will be outlined below.

Anticipating Renfrew to some extent, Peter Bellwood has consistently and to my mind correctly emphasised the importance of agriculture in the expansion of the Austronesian languages from Taiwan, and ultimately East China, into territory occupied by non-Austronesian speaking foragers (Bellwood 1985, 1988, 1991). This scenario has been vigorously debated by Meacham (1988, 1991) and Solheim (1988, 1993, and this volume). As indicated by my quote at the beginning of this paper (from a review of one of Bellwood's books), I strongly support his scenario, based on the linguistic analyses of Blust (1976, 1988) and others, but certainly do not wish to say that Bellwood and Blust are completely 'right' and Meacham and Solheim totally 'wrong'. I think that much of the disagreement arises out of a confusion of linguistic and archaeological models, and hope in the course of this paper to demonstrate that the two sides to the debate are not wholly incompatible.

SOME GENERAL CONSIDERATIONS

The chief problem to my mind is a lack of understanding that linguistic and archaeological models are two different entities, arising from two separate disciplines and relying on different methodologies and data (Pawley and Ross 1993). This is certainly evident in Renfrew's presentation of his grand syntheses and serves to illustrate several areas of confusion current in the Austronesian origins debate.

- 1) *There exists a tendency to confuse various stages of development in linguistic prehistory, so that different phases of proto-language and sub-proto-languages are occasionally viewed as the same entity; e.g., Renfrew's proposal that "Indo-European speech first reached the Russian steppe lands as farming economy and Proto-Indo-European speech spread up from Greece into the Balkans" (1992a: 26).*

If Proto-Indo-European was spoken in Anatolia, and the Hittite and Luwian languages were the descendants of those who stayed there, obviously what was spoken in the Balkans, or Greece, or the Russian steppes, was not Proto-Indo-European (PIE), but rather one of its descendant proto-languages. Similarly, the languages of Jomon Japan were possibly displaced by "early Altaic (or Proto-Nostratic) speakers" (1992a:31). If in fact Proto-Nostratic ever did exist, I doubt that its point of origin was Japan; Hawaiian was not displaced by Proto-Germanic! Hence we find terms like 'group, family, phylum, macrophy-

lum', etc. are frequently used interchangeably. For example, Renfrew refers both to an Australian language 'family' and 'phylum'; Sino-Tibetan and Austric are both described as 'groups' (Renfrew 1992a: 36-37), although the first is a fairly well-defined family and the second a still-controversial phylum.

- 2) *There also exists a tendency to ignore contradictions between the archaeological scenario envisioned and the linguistic evidence for subgrouping.*

One of my chief concerns with Renfrew's IE scenario is that, if correct, it should have resulted in a clear hierarchical subgrouping of the IE languages through the sequential transformations of IE and its descendants as farming spread through Europe (Renfrew 1988: 441). If this had in fact occurred, we should see a very clear hierarchy of nested IE subgroups along the lines of those defined by Blust (1988: 47) and followed by Bellwood (1985: 107-10) for Austronesian (cf. comments of Coleman in Renfrew 1988: 453). No such hierarchical ordering of the various IE subgroups has ever been demonstrated by linguists. Renfrew is very aware of this difficulty (1992a: 21), but to my mind minimalises the amount of effort already devoted to subgrouping IE. In fact, despite still-controversial attempts to combine the well-recognised families of IE into larger entities (Balto-Slavic, Italo-Celtic, etc.), no overall hierarchical schema has emerged over the past 200 years of IE studies.

- 3) *There are inconsistencies between the content of the proto-language lexicon and the proposed archaeological scenario.*

If Proto-IE was in fact spoken in Anatolia and an early daughter of PIE in Greece and the Balkans, it seems to me it would be logical to find PIE forms for olive, wine, caper, ass, lion, cypress, laurel and other features of the Mediterranean world, even if with altered referents (e.g., the PIE term for snow meaning greasy in Indo-Aryan languages).² Instead we find words for bear, wolf, salmon, beech and other species less typical of the Mediterranean.

- 4) *There is sometimes a reliance on very controversial classificatory groups alongside much more secure ones.*

Nostratic, Indo-Pacific, Austric, Austro-Thai (AT) and Austronesian (AN) are constructs which by no means have equal amounts of supporting data in terms of either quantity or quality. Renfrew relies heavily on the 'macro-families' (e.g., Eurasiatic, Nostratic, Indo-Pacific, Austric, Altaic including Japanese) arrived at by Ruhlen and several others. He is careful to note the reservations that many linguists feel about such super-families, but I believe that "many" would be more honestly put as "most

linguists". Similarly, Renfrew states that - aside from Amerind - Ruhlen's scheme is "rather uncontroversial" (1992b: 450). In fact, Austric and particularly Indo-Pacific (Andamanese, Papuan and Tasmanian) are quite controversial groupings, with nowhere near the acceptance of universally recognised families like IE and AN. In my view, validity in linguistics, as in archaeology, is a matter of relative plausibility³ rather than a yes or no situation. Just as some archaeological assumptions are highly plausible and widely accepted while others are not, linguistic entities like AN or subgroups like Polynesian (PN) or Romance are universally accepted, while Indo-Pacific and Amerind are accepted by only a handful of linguists. Greenberg's lumping of African languages into just four families 30 years ago is now very widely accepted, and based on much good-quality evidence.⁴

However, Eurasiatic, Amerind and Indo-Pacific are not so well founded. As Crowley's excellent introductory text says of the Indo-Pacific hypothesis, "until someone can point to the existence of regular sound correspondences in any proposed set of cognates, it is likely to continue to be regarded by mainstream linguists as being close to the lunatic fringe" (Crowley 1992: 305).

5) *There sometimes exists an assumption that somehow the scenarios arrived at through linguistics and archaeology should have a near-perfect fit.*

This can lead to an apparently unconscious tendency to 'make it so' in the face of contradictory evidence between the two scenarios. One possible example might be Renfrew's postulation of the division of a Russian-steppe daughter proto-language into Slavic (plus Baltic?), Indo-Iranian and Tocharian (1992a: 26-7), despite the fact that the last is a *kmtom language subgroup while the first two are in the *satem division of IE.

Of course, in a more general sense Renfrew's grand synthesis of linguistic, archaeological and genetic evidence is based on a sequence of shaky assumptions and 'if ... then' statements, beginning with the assumption that the Eve monogenetic hypothesis of human origins is correct (Renfrew 1992a: 17). Renfrew is careful to label these assumptions as such, pointing out their tentativeness and lack of wide acceptance, but if even a few of the assumptions prove to be false (or more likely untestable) the synthesis begins to unravel. This is not to say that I wish to condemn such attempts; Renfrew's ideas and exposition are fascinating and stimulating 'just so' stories. But I am somewhat concerned that those of us working closer to the ground in both disciplines may begin to doubt each other's overall reliability, by questioning (for instance) how reliable archaeology can be if archaeologists cannot even decide when the New World

was first settled - 13,000 or 37,000 BC; or how reliable linguistics can be if linguists cannot even agree on the linguistic affiliations of Japanese.

Thus, the models generated by the other discipline become, as one archaeologist put it, merely "a fascinating parlor game" (Meacham 1988: 92). This does worry me, for I am convinced that the two fields have much to offer each other at levels well below syntheses on a grand scale.

6) *Finally, there can be an apparent lack of understanding of the processes involved in language change.*

Renfrew has already been criticised for a "shockingly superficial" treatment of linguistic theory and process (Baldi, commenting on Renfrew 1988: 446). As Baldi points out, sociolinguistics is mentioned in passing by Renfrew, but with no discussion of sociolinguistic factors as the dominant mechanisms of language change. Thus when Renfrew states that "Clearly language replacement sometimes occurs" (1992a: 18), he is greatly understating the case. Language replacement is a frequent occurrence, and more so at the present than at any time in human history and prehistory. Thanks to the sociolinguistic research of the last several decades, we now have an increasingly clear picture of the processes involved in language shift (Gal 1979) and language death (Dorian 1989). While such processes have been studied almost wholly in contemporary societies, I think it is possible to draw on them for at least general parallels with possible prehistoric scenarios of language replacement, and will do so below.

LINGUISTICS AND AUSTRONESIAN ORIGINS

To my mind, a number of the points of disagreement Meacham and Solheim have with the linguistics-based Blust-Bellwood model of Austronesian origins arise from unintentional misunderstandings of the six types outlined above. Other points of contention of course arise from differing interpretations of archaeological data, but I will not be discussing these, as I lack the detailed competence in the regional archaeology of East and South China, Taiwan and the Philippines necessary to offer any worthwhile opinion.

1) *Confusion of linguistic entities.*

There has been a certain amount of confusion of the terms 'Austro-T[h]ai',⁵ 'Proto-AT', 'pre-AN', 'Proto-AN (PAN)', and 'AN', both as language groups in their own right and as referents to the people who presumably spoke them. Some such confusion is almost inevitable when archaeologists discuss linguistic models. I myself frequently refer to the Proto-Polynesians and Proto-Oceanic speakers in my lectures. It was to avoid such

confusion that Solheim coined the term *Nusantao* or Island People.⁶ Solheim is of course correct in pointing out that "Austronesian is the name of a very old and widespread language family; it is neither a people nor a culture" (1988: 77). But on subsequent pages we find pre-AN developing in Mindanao and northeastern Indonesia, Proto-AN-speaking people spreading to the coasts of South China and Taiwan, and Proto-Malayo-Polynesian (PMP) languages developing out of PAN in the Philippines. Simultaneously a PAN language moved down the coast of South China and Viet Nam, perhaps providing "the possibility for the development of Austro-Thai" (Solheim 1988: 80-81). If Benedict's Austro-Tai hypothesis is correct (and I believe it to be; see section 3 below), this scenario is clearly impossible: PAT must have preceded pre-AN, which in turn preceded PAN, which in turn had to precede Proto-Taiwanic and PMP. One cannot by definition have a group of related daughter languages descended from PAN in the Philippines while the parent PAN language moves south along the Vietnamese coast, and subsequently sails across to Palawan to join its daughters. This is, in linguistic terms, impossible: rather like having England invaded in AD 1066 by Proto-Germanic speakers!

Meacham similarly feels that the AN languages originated in "the broad triangular area formed by Taiwan, Sumatra, and Timor, where the reputedly oldest Malayo-Polynesian languages are found" (1988: 94-5).⁷ Here we find AN and MP apparently being used synonymously, which certainly was the case until Blust distinguished the two terms as representing different stages in AN history. However, what are the oldest MP languages? Is Tagalog older than Tongan? Is Bahasa Indonesia older than Maori? Obviously, when dealing with the modern daughter languages none is older than any other, as Renfrew realises (1992a: 39). Modern languages of course display differing degrees of conservatism, or retention of earlier features; but a language that is lexically or grammatically conservative (e.g., German) can be phonologically innovative (for instance, the High German Second Consonant Shift changing /t-/ to /ts-/, /p-/ to /pf-/, /d-/ to /t-/, etc.). And the converse is of course true; English has been remarkably innovative in simplifying its original Proto-Germanic morphology (Lass 1987) and acquiring most of its lexicon from other languages, but has (along with Icelandic) retained the original Proto-Germanic /σ/ and /θ/, which have become /d/ and /t/ in all other Germanic languages. If archaeologists are to employ linguistic models like *Stammbäume*, they must first realise that these are of necessity abstractions and secondly real-

ise what the models are meant to represent (see section 5 below).

2) *Contradictions between archaeological evidence and linguistic subgroups.*

This I think stems directly from not realising what linguistic subgroupings would result from proposed archaeological scenarios, and how probable or improbable these are linguistically. If we first inspect the subgrouping scheme arrived at by Blust on linguistic evidence (Figure 1), we see that it, a) provides a satisfactory west-to-east hierarchical set of subgroups from the highest-order Taiwanic and MP subgroups moving east to Proto-Oceanic; b) explains the ability to reconstruct lexicon reflective of a subtropical rather than tropical environment in the proto-language; and, c) allows for the probable but still unproven descent of pre-AN from a mainland Chinese offshoot of PAT.

But if we examine the subgrouping of AN that would have resulted if Meacham's scenario was in fact the case (Figure 2), we should expect to see a linguistic diversity among the languages of the Philippines (and parts south, perhaps) greater than that of the Taiwanese languages. Such is in fact not the case. The linguistic difficulties are compounded when we look at the subgrouping scheme that would result from Solheim's postulated 'Nusantao' movements (Figure 3). Proto-AN would have to exist at two different times, or to have remained unchanged in one location sufficiently long for PMP to have developed before moving to the Chinese coast. This is simply impossible; as decades of sociolinguistic research has shown. All living languages are continually changing (despite the best efforts of purists and prescriptivists to prevent this).⁸ Here PAN II presumably gave rise to PAT, an even greater linguistic and logical impossibility, tantamount to saying that Proto-Germanic gave birth to its mother language PIE! Nor is it at all clear which of the western AN languages are descended from PAN II and which from PMP; from which subgroup did Proto-Oceanic arise? On present evidence the Blust scheme appears to be the only one of the three that satisfactorily fits both linguistic and archaeological data.

3) *Proto-lexicon inconsistencies.*

By ascribing to PAN lexicon only those forms with reflexes in both PMP and the Taiwanese languages, Blust has made what is to my mind a quite convincing case for a PAN homeland that "was at best only marginally tropical" (1988: 48). The proto-form *qamiS(-an) ('cold/north wind'), suggesting seasonality, and the absence of PAN terms for banana, breadfruit, coconut, sago, etc. certainly support this conclusion, as do Bellwood's arguments for the origins of rice in a region with marked diurnal varia-

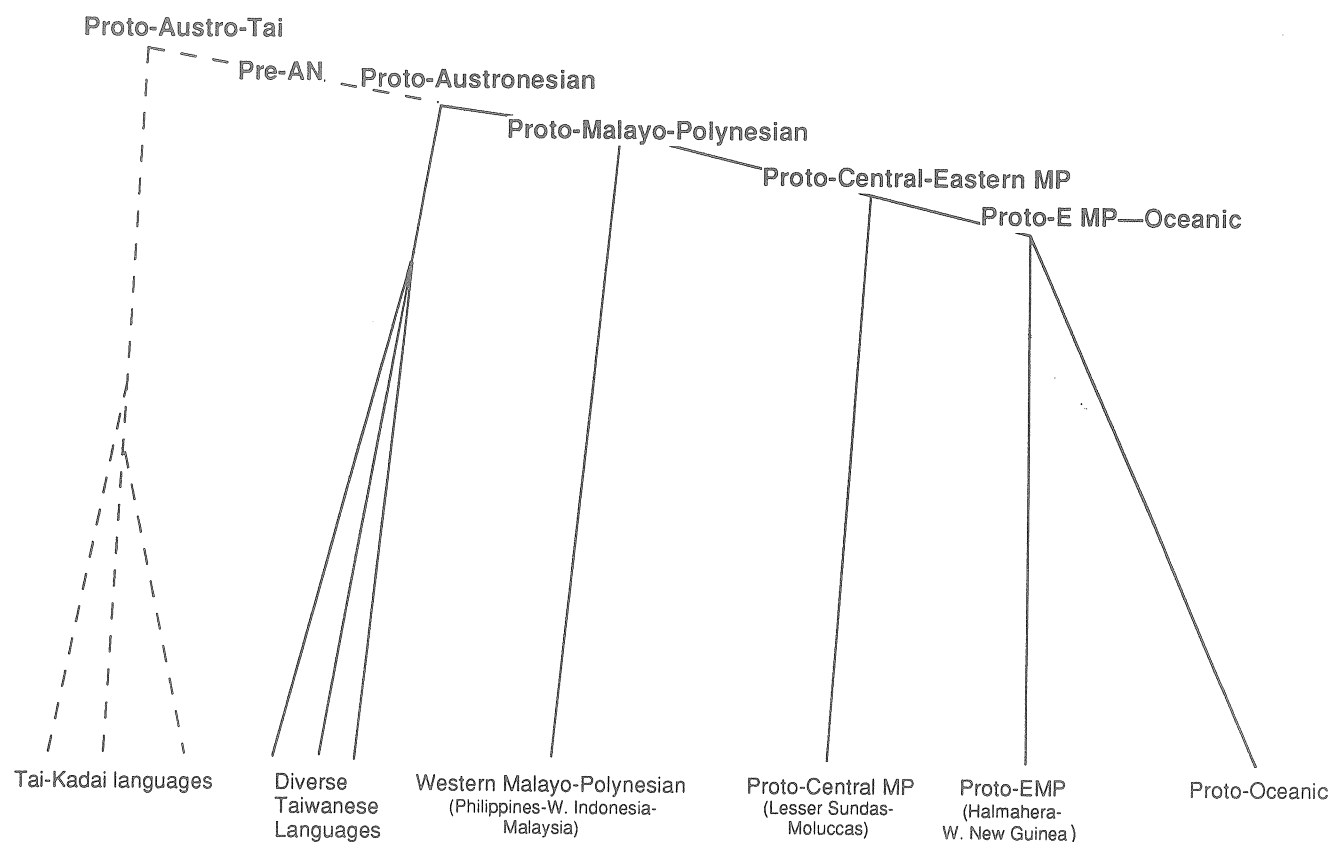


Figure 1. Orthodox model of Austronesian languages following Blust (1988).

tion (1985: 233). The sixth millennium BC dates for rice at Pengtoushan and Hemudu, north of Taiwan and even further removed from the tropics, add some archaeological weight to the argument.

One could of course postulate that PAN was indeed spoken in the tropics, but that the terms for coconut, breadfruit, etc. were lost in the move to Taiwan, where their referents no longer grew, but were retained in PMP. However, I find this unconvincing; the tropical East Polynesian settlers of New Zealand underwent an even more radical climatic shift to a temperate or cool-temperate flora, yet many reflexes of PPN terms for tropical plants were retained and applied to similar temperate species.⁹

Thus while a tropical homeland seems assured for PMP, the same seems unlikely for PAN. Although there are certainly areas of uncertainty in the data and recon-

structions, the overall weight of the evidence favours a PAN homeland in Taiwan. Moreover, if there is any merit in the PAT hypothesis - and I believe that Benedict (1975), Reid (1988) and others have made a plausible if still tentative case for it - then Taiwan of course provides a geographically convenient location for one branch of PAT (i.e., pre-AN, not PAN itself) to have developed in. Some of the heat in the present debate is generated by the confusion of pre-AN and PAN, and the use of the term South China for the place of origin of pre-AN. As Bellwood says by way of clarification, PAN itself was probably never spoken on the Chinese mainland (1988: 112); but like Bellwood, I believe its ancestor (presumably a descendant of PAT) was. However, the term 'South China' has customarily been used by myself and others to refer to the Yangzi Valley as well as regions further south, and Meacham (1988: 94) is I think

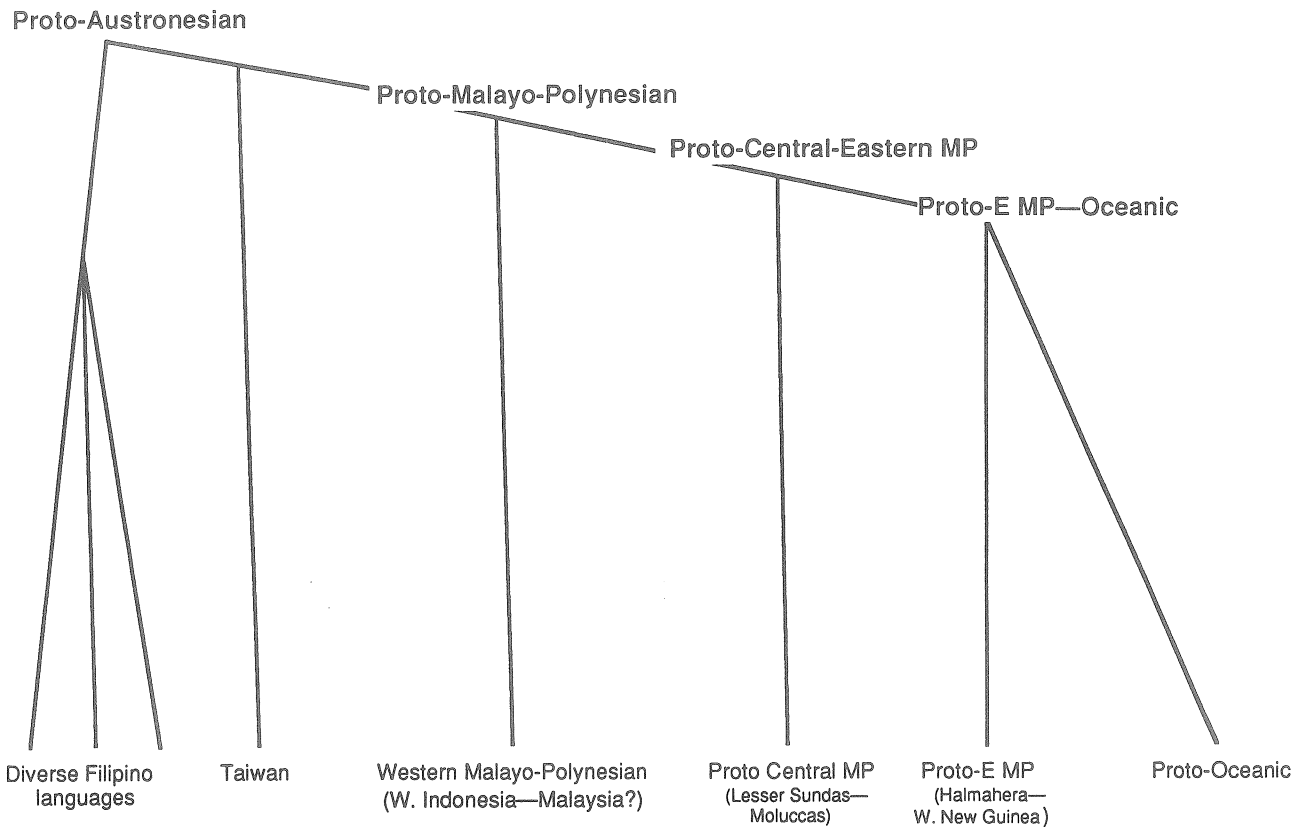


Figure 2. Hypothetical model of Austronesian languages suggested by Meacham's (1988) scheme.

correct to claim that pre-AN or PAT was never spoken in the southern area of Fujian-Guangdong-Guangxi. Norman's evidence (1988: 181-9, 213-4, 231) would certainly suggest an Austroasiatic rather than PAT substratum in southern South China. However, this would certainly not rule out the Yangzi Valley, and I, like Higham and Thosarat (1994: 143-5), feel it is probable that PAT was spoken in this general region - i.e., Central China.

4) Reliability of linguistic groups and subgroups.

Although perhaps not as clear-cut as Polynesian, Romance and other subgroups with a shallow time depth, AN is a long-established¹⁰ and quite clearly defined family of languages. While there are still problems remaining in determining detailed subgrouping - and indeed in establishing whether some of the AN languages of Melanesia are in fact AN languages (Lynch 1993: 249) - its unity as a clearly defined family descended from a common proto-language is without doubt. AT is consid-

erably less well-established, but still appears probable to enough qualified linguists to allow me as a non-specialist to tentatively accept its existence. The Austric phylum containing both AT and AA is of course much more tentative still, at a time depth which begins to approach the limits of comparative linguistic investigative capabilities. Nonetheless, Reid (1988 and see his paper in this volume; Blust, pers. comm.) has assembled a number of morphological similarities which suggest that the Austric hypothesis may well be true. As such, it appears to be a classificatory entity with considerably more validity than Eurasiatic, Nostratic, or Indo-Pacific.

The point I wish to make is the obvious one that linguistic models, like their archaeological counterparts, come in varying degrees of plausibility and mainstream acceptability. In Table 1 I have attempted to offer some examples of both, ranging in plausibility from those accepted by all workers in each field, through those which

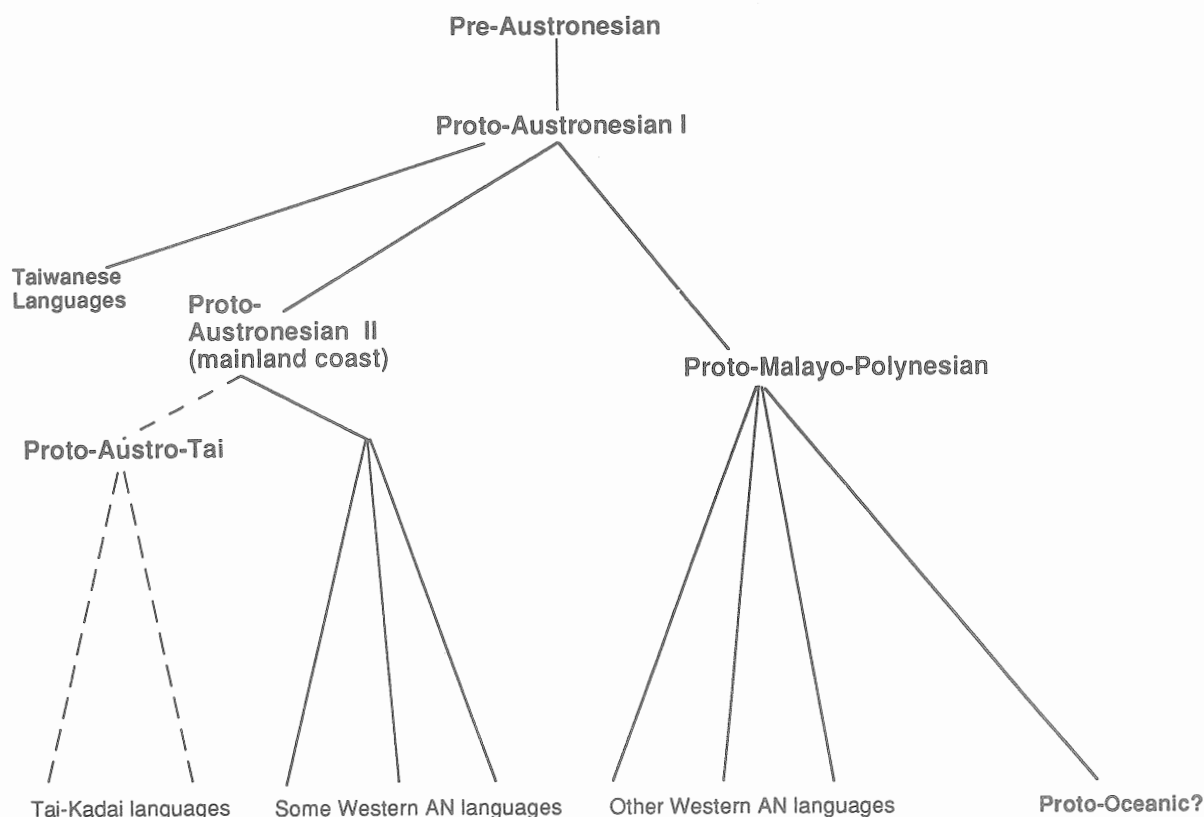


Figure 3. Implied model of Austronesian languages following Solheim (1988).

are highly controversial and not yet accepted by most, to ones only accepted by a minority. If they wish to use linguistic models, archaeologists would be well-advised to rely on those widely accepted by linguists, rather than incorporate a linguistic schema accepted by only one or two linguists (the converse is of course also true for linguists vis-à-vis archaeology!).

5) *The near-perfect fit of linguistic and archaeological models.*

As mentioned above, there seems to be some misunderstanding of the family trees or *Stammbäume* employed as models by comparative linguists. These are of course idealised constructs, not intended as models of actual prehistory; they do not incorporate important linguistic phenomena like borrowing or *Sprachbund* (areal) effects. A proto-language is similarly an idealised construct, representing a hypothetical language at the precise point just

before it split into two or more daughter languages. Accepting the Blust-Bellwood model of a Taiwanese origin for AN, and only a single Taiwanic subgroup (there may have been up to three - Blust 1988: 48), the idealised PAN would have 'existed' only just prior to the departure of the first settlers to Luzon; as the canoe sailed south into the Bashi Channel, the split of PAN into pre-MP and Pre-Taiwanic occurred. It is of course obvious that such an idealised model cannot fully represent prehistoric reality, with the possibility of multiple voyages, continued contact between the two daughter proto-languages and so on.

There are also cases where the *Stammbaum* model simply does not work; IE is perhaps the most famous one, where isoglosses (**kmtom* vs. **satem*, etc.) which cross-cut the basic subfamilies have led linguists to postulate a *Sprachwelle* model of dialect interaction to ac-

LANGUAGE GROUP	ROUGHLY PARALLEL ARCHAEOLOGICAL SCENARIO	PLAUSIBILITY-ACCEPTANCE
Proto-Polynesian, Proto-Romance	Lapita ceramic complex	Universally accepted
Subgrouping of Proto-Indo-European, Proto-Austronesian	Relationship between Far Western Lapita and Eastern Lapita	Very high; a few still controversial details
Proto-Austro-Tai, Trans-New Guinea Phylum	Ceramic ties of Phung Nguyen, Khok Phanom Di, Early Period Non Nok Tha/Ban Chiang	Probable; accepted by many
Austic, Japanese-Altaic, Proto-Papuan	Japan-Taiwan-Philippines ceramic ties; settlement of the Americas 40,000 BP	Possible, but still controversial
Indo-Pacific, Nostratic, Eurasian, Amerind	Common "Nusantao trader" origin for ceramics of Phung Nguyen, Sa Huynh, Taiwan and Lapita	Very tenuous; highly controversial
"Proto-World"	Initial settlement of Polynesia by American Indians	Accepted by very few
Derivation of Polynesian from Egyptian or Phoenician	Extra-terrestrial influence on Easter Island	Accepted by none

Table 1: Plausibility levels of various linguistic hypotheses

count for the lack of sets of uniquely shared innovations in these subfamilies. Finally, it has to be admitted that there are cases where the classic Comparative Method appears to fail outright, due to an inability to establish plausible cognate sets or close morphological parallels. Attempts to establish a clear-cut Papuan phylum have thus far failed (Foley 1986: 3; Lynch 1993: 39), presumably due to the great time depth involved. Similarly, it has proved impossible to tie Japanese conclusively to Altaic, AN, or any other family (or language; e.g., Ainu or Korean; Shibatani 1990: 94-6, 116-8), although most linguists have a feeling that a Korean-Altaic tie is likely.

The point is that there are obviously some problems in prehistory which linguistics cannot answer conclusively. Despite decades of relatively intensive linguistic (and archaeological) investigations, we cannot pinpoint the precise homeland of Proto-Polynesian; "one is left with an area, not an island group" (Green 1981: 154). Nor can we determine the point of origin of the first settlers of Eastern Polynesia, save that it was very probably in the East Futuna-Uvea-Samoa-Niutopotapu area (or perhaps even the Tokelau or Tuvalu atolls). The Polynesian Outliers, all with Nuclear Polynesian (NPN) languages, but some having Lapita-related deposits that would appear to predate the formation of NPN (Bayard 1976: 62-8), provide a third example of a lack of fit between the two disciplines. As Green puts it more generally, "there is no reason why changes in language should exactly parallel in time and space those in one or more

aspects of material culture that survive archaeologically. In fact one might expect the two to often differ" (Green 1981: 146-7).¹¹ Similarly, we cannot expect any precise, absolute correspondence between language affiliation and physical type, nor be surprised when "races and languages do not match as might be expected" (Bellwood 1985: 74). Green has summed up the problem of matching archaeological and linguistic models nicely in a more recent paper: "the message seems clear, at least to me: much of any such [linguistic] reconstruction may reflect only what is deemed to have gone on in people's heads. Its realisation in the ground will probably be a rather more mundane affair" (1994: 13). Obviously I wholly concur.¹² But despite all this we can most assuredly expect a general relationship between the results of linguistic, archaeological, and biological anthropological investigations; we ignore the evidence of any one of these at our peril.

6) *The processes involved in linguistic change.*

I think there are two important points in Meacham's and Solheim's scenarios for the *in situ* development of PAN in "Austronesia" which require comment. The first of these is Meacham's incredulity that the non-AN languages presumably spoken in the Philippines could have "completely vanished without a trace" (1991: 404). Based on the very frequent occurrences of language replacement or takeover in historic and modern times, I find such a disappearance highly credible. Figure 4 presents some possible scenarios for language contact situat-

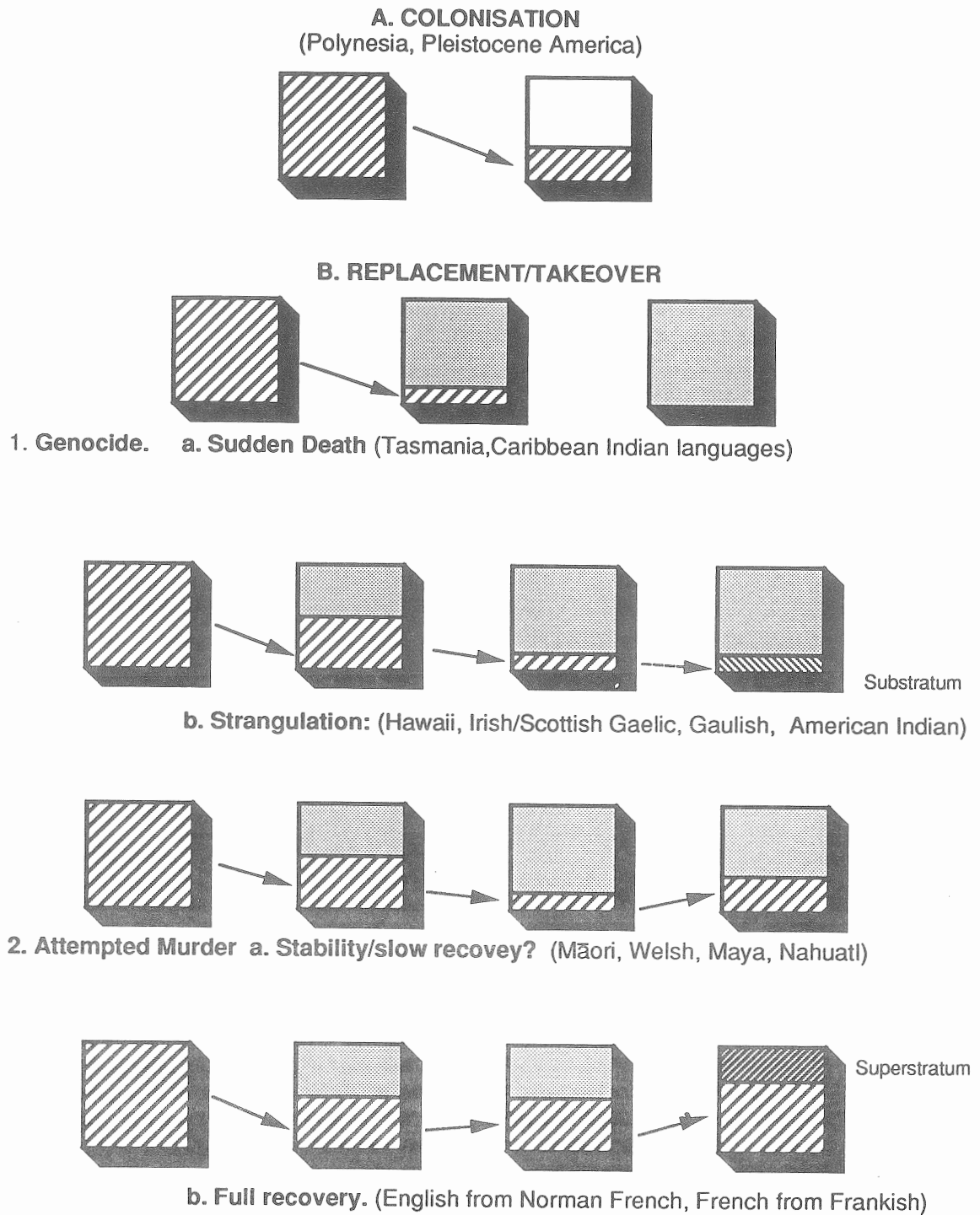


Figure 4. Prehistoric language contact possibilities.

ions and their outcomes based on historically documented cases.¹³ Given the fact that the modern Agta and Mbuti foragers are alive, I think we can rule out straightforward genocide; a process of slow cultural strangulation was much more likely to have effected a shift to AN languages. Headland and Reid (1989) put forth a plausible case for such a shift occurring as a result of intense interaction between the non-AN foragers and the arriving AN-speaking agriculturalists, pointing out linguistic parallels with the Mbuti foragers who of course speak Bantoid languages similar to those of the farmers they interact with. However, Meacham finds a language shift like this unlikely, seemingly requiring interaction of almost a sexual intensity!¹⁴ He considers that it is far more likely that the foragers themselves also spoke AN languages.

In fact, language shifts take place relatively quickly, and for both economic and social reasons. Gal has documented the shift of eastern Austrian peasants from their native Hungarian to German in the space of a few generations, based both on economic motivations (Hungarian as a 'peasant' language as opposed to German, the language of workers and the middle classes) and social factors - e.g., "peasant lads cannot get women" (Gal 1979:59). Obviously, Austria in the 1970s is not the Philippines at 3000 BC, but I think it is safe to assume that generally similar processes could have been at work. Again and again linguists have seen economically and socially inferior groups adopt the language of the economically and numerically dominant majority¹⁵, and I see no difficulties envisioning this taking place in a forager-agriculturalist context, although the time scale for such shifts may well be much greater.

But as I think the examples in Figure 4.B.b illustrate, strangulation, whether effected by peaceful means or coercion, is a far more likely alternative than the presence of an original PAN in the Philippines. Certainly this type of language shift often leaves a telltale substratum in the dominant language (e.g., Hawaiian lexicon in Hawaiian Creole English), but often the substratum is minimal. Aside from onomastic items, very little in the way of British vocabulary made its way into Old English; placenames like the six 'Avon' rivers and numerals used for counting sheep in northern England (Price 1984) are about the only traces that survive.

More importantly, the substrata left by these earlier languages are detectable only because we have written records and their modern descendants (Welsh and Breton in the case of British) to trace them to. In other cases languages have shifted to extinction without leaving any apparent trace. Pictish was apparently spoken in Scotland

until the latter half of the first millennium AD (Price 1984: 25), but has left no discernible traces in either Scottish Gaelic or English. Indeed, it is impossible to relate what little is known of Pictish to any other European language.¹⁶ In the case of the pre-agricultural languages of the Philippines, we have no hope of detecting such a substratum, even if it exists. It could of course be argued that such a process of shift and takeover could explain the absence of the very divergent Filipino AN languages that would be expected if Meacham's scenario is correct (Figure 2). However, such shifts and takeovers apparently occur only rarely among groups at a similar level of socioeconomic organisation (in this case, village-level agriculturalists); look at the linguistic diversity of the modern mainland Southeast Asian hill tribes.¹⁷ But we do have ties to Taiwan for the AN languages, and beyond PAN to the tentative but plausible evidence for AT - and perhaps even Austric - on the Asian mainland.

The second point concerns dialect chains and *lingua francas*. Meacham believes that PAN may have evolved *in situ* out of a "New Guinea-like linguistic diversity ... the early diversity, perhaps preserved in Taiwan, was honed into the more uniform Proto-Malayo-Polynesian" (1988: 93). Solheim similarly believes that PAN developed as a trade/barter language "among the maritime Nusantara along the coasts of northern Luzon, southern Taiwan and South China" (1988: 81). However, a dialect chain results from the breakup of an originally more uniform language; it does not give rise to a new language. For example, the dialect chain running from Parisian through Provençal, Catalan, Castilian and Galician to Portuguese developed through the breakup of Proto-Western Romance; standard French, Spanish and Portuguese developed through a process of standardisation of one socially, economically and politically dominant dialect, not from the honing of the dialect chain into a more uniform language. This is also the case with Received Pronunciation (the "BBC" accents) vis-à-vis other English English dialects.

Similarly, pidgins and creoles develop through the simplification of a dominant language in a contact situation with one or more other languages (Chinese Pidgin English, Haitian Creole French, Hawaiian Creole English, etc.); they are not amalgams in equal parts of their contributor languages (Crowley 1992: 262-3). English is currently the global *lingua franca*, but is very clearly a West Germanic language with a lexicon heavily influenced by French and Latin; it did not arise as a blend of different languages. As Bellwood points out, proto-languages are not developed "through mobility and communication" between speakers of highly diverse lan-

guages (1988: 112). It seems impossible to derive PAN origins from either a dialect chain fusion, a pidgin, or a *lingua franca*.

TOWARDS A SYNTHESIS

As I said at the start of this paper, the intention is not to present Bellwood and Blust as right and Meacham and Solheim as wrong. While I clearly believe the linguistic scenario held by Blust and Bellwood to be the correct one, I think there are elements of the Meacham and Solheim models which can be incorporated within it.

Firstly, while PAN origins in Taiwan and a mainland connection for pre-AN seem highly probable to me, Meacham is certainly right to question the weakness of present archaeological evidence for a priority of agriculture in Taiwan vis-à-vis the Philippines. However, I expect this will emerge in due course; thirty years ago who would have predicted the discovery of Peiligang, Cishan, Pengtoushan and Hemudu?

Secondly, both Meacham and Solheim are probably correct in thinking that dialect chains featured in AN dispersal, and PAN itself was probably a member of a pre-PAN dialect chain in Taiwan, possibly extending across the Formosa Strait to the Chinese mainland. The formation, interaction and breakup of dialect chains may also have contributed to the linguistic diversity of smaller, considerably more recent sub-subgroups of Proto Western Malayo-Polynesian (PWMP) and Proto Central Malayo-Polynesian (PCMP). Nusantao is perhaps a useful term to describe maritime-oriented Indonesian and Philippine peoples of c.2000 BC to the present,¹⁸ but I believe it should be divorced from any specific linguistic context; after all, not all Austronesian speakers, past and present, are maritime oriented. As Solheim (1993) uses this term to refer to a chronological range of from before 5000 BC to the second millennium AD, it should certainly not be tied to one or more of the stages in the history of the AN language family.

Thirdly, I continue to believe, as I did 20 years ago (Bayard 1975a, b), that Meacham (1988: 90-1) is correct to emphasise an essentially local evolution of agriculture, metallurgy and the like in East and Southeast Asia - in particular in the region he has named 'Nanhailand' (1988: 94) - rather than the spread of such innovations through massive diffusion or migrations by the Longshanoid Culture or any other group. However, I also believe that Bellwood is arguing for something similar in his schema of AN expansion: gradual movements of small groups of locally evolving AN speakers, rather than any massive migration carrying a pre-formed PAN culture with them. Meacham also recognises this, but feels such

a scenario is "virtually unprovable and unfalsifiable" (1991: 400). Here I cannot agree; I feel fairly confident that further archaeological work in Taiwan, the Philippines and Indonesia will provide at least suggestive evidence supporting the Blust-Bellwood linguistic scenario. In any case we would both agree that "For North China, South China, and Indo-China, a great migration during the Neolithic now seems out of the question" (Meacham 1988: 100).

Support is afforded to Meacham's belief in the importance of local evolution in Nanhailand by a recent attempt to synthesise archaeological and linguistic data by Higham and Thosarat (1994). This ingenious, plausible, but obviously tentative hypothesis accepts, as I did in 1975, the reality of the AT superfamily and its speakers as early rice agriculturalists in the Yangzi Valley.¹⁹ However, based on the evidence for the southern limits of Longshanoid-affiliated sites, Higham and Thosarat postulate that their expansion was halted by encountering another group of settled agriculturalists: "it is stressed that it is this southern edge of the Lungshanoid horizon where one encounters the region in which Austro-Asiatic languages are thought to have been spoken" (1994: 139). They go on to postulate another transition to rice agriculture paralleling that which occurred in the Yangzi Valley: a largely independent development by the AA speakers of Nanhailand (1994: 145). Given the apparent ease with which wild rice may be domesticated (Oka and Morishima 1971), and Nanhailand's location at the northern extremes of the tropics (cf. Bellwood's arguments on diurnal variation above), such a locally evolved transition to rice domestication is certainly plausible.

Moreover, Higham and Thosarat draw attention to general similarities in the incised and comb-stamped pottery of Phung Nguyen, Samrong Sen, Early Period Non Nok Tha and Ban Chiang, and Khok Phanom Di (1994: 141; Higham 1996). It is very tempting to correlate this with the westward expansion, under the stimulus of agriculture, of AA languages from Nanhailand through Viet Nam, Laos, Cambodia and Thailand to Burma and ultimately eastern India. As Higham and Thosarat point out, Zide and Zide (1976) have reconstructed a number of terms related to agriculture in Proto-Munda, and some of these (husked rice, rice husking pestle) appear to go back to Proto-AA as well. It has been generally agreed for some time now that the prehistoric and early historic inhabitants of Thailand and Burma spoke languages in the Mon-Khmer branch of AA (Bayard 1979: 279). Such an expansion would account neatly - in a general sense - for the distribution of the AA languages.

Higham and Thosarat's scenario is an exciting one, but an equally exciting overview on a somewhat grander synthesis level has recently been presented by Blust (1994, forthcoming). Based on morphological evidence such as that presented by Reid (1988:31-32 and see his paper in this volume), Blust is convinced that Schmidt's Austric hypothesis is correct, and postulates an Austric homeland in the eastern Himalayan foothills in the area of the converging valleys of the Salween, Mekong, and Yangzi rivers. From this homeland, one group of rice-cultivating peoples moved down into the middle Yangzi Valley to give rise to the AT superfamily of languages by ca. 6500-6000 BC. Somewhat later the now-proto-AA speakers remaining in the homeland began to move down the valleys of the Salween and Mekong (Proto-Mon-Khmer), with pre-Munda speakers moving westward down the Brahmaputra.²⁰ Obviously this hypothesis is still fuzzier and more tentative than Higham's and Thosarat's, and is in at least partial conflict with the latter in postulating an initial domestication of rice in the Himalayan foothills rather than in the middle Yangzi Valley and Nanhailand. Moreover, lexical replacement has gone on in AA and AT to such an extent that any reconstruction of proto-forms is impossible, and this fact in itself has led Benedict in a witty paper to declare Austric "extinct" (1991). Nonetheless, the scenario is a fascinating one, with potential for testing against the archaeological evidence once research has been carried out in the almost totally uninvestigated middle and upper valleys of the Mekong and Salween.²¹

Finally, I think the next several decades of Southeast Asian prehistoric research are going to prove as exciting as the last few have been, as linguists and archaeologists, hopefully working together, attempt to confirm or refute hypotheses like these. While given present archaeological and linguistic knowledge²² we can say that hypotheses like Proto-Polynesian and Romance are 100% confirmed, and PAN and its Taiwanese origin perhaps 80-90% confirmed, PAN origins in a presumed AT superfamily are still perhaps only about 60-70% confirmed. Higham and Thosarat's Nanhailand agricultural hearth is considerably fuzzier, and Blust's fascinating scenario even more so. These will take decades of linguistic work and archaeological investigation of little-known areas like western Yunnan, Laos, upper Burma and Assam to provide further data.

However, fuzzy as these hypotheses are at present, they are still far less fuzzy and much closer to the ground than Renfrew's Grand Synthesis of prehistory going back to Eve. But if Renfrew's ideas, with their intrinsic fascination, can provide the stimulus for further collaboration

between linguists and archaeologists in our region they will have served as more than just food for thought. Since my Master's research in Polynesian linguistics 30 years ago I have been convinced of the potential value of linguistics to Southeast Asian prehistory. I think that the efforts of those linguists and archaeologists cited in this paper have justified this conviction, and hope that others, with a necessary comprehension of the limitations and pitfalls in both disciplines, will hop on the bandwagon. There is nothing to lose and a lot to gain!²³

NOTES

1. Before, during and after its initial writing, this paper has benefited from informal talk, e-mail and conventional correspondence with Paul Benedict, Bob Blust, Roger Green, Ray Harlow, Charles Higham, Bill Meacham, Andy Pawley, Bill Solheim and Richard Walter. However, the opinions expressed are of course strictly my own.

2. See comments by Anthony and Wailles (Renfrew 1988: 442), and Renfrew's admission that this is a "valid point which merits comment" (op. cit., p. 464); however, Renfrew offers no explanation.

3. One reader of this paper commented that **probability** was the proper term here; however, I prefer to reserve this for its more precise statistical definition, and instead use **plausibility** in the sense of an argument's general ability to convince other workers in the field (and certainly not in its older ancillary meaning of specious!)

4. I had the privilege of taking a senior undergraduate class taught by Greenberg in 1960 while he was formulating this hypothesis; he was able to demonstrate the plausibility of his four-fold classification very convincingly indeed, using a list of 100 or so words and his technique of multilateral comparisons. However, I am far more sceptical of this technique when it is applied on a larger scale (Eurasian, Amerind).

5. I tend to omit the 'h', as Tai is the name of the family (or subfamily of Tai-Kadai) to which the Thai language proper belongs.

6. Although strictly speaking I suppose the term should be *tao nusa*.

7. It should be noted parenthetically that Blust's failure to find any PAN terms for marsupials would rule out the eastern third of Meacham's triangle, east of the Wallace-Huxley line, as a possible homeland for PAN (Blust 1982).

8. The only way to 'freeze' a language is for it to become preserved as a liturgical or learned vehicle with no native speakers; e.g., Latin, Sanskrit, Pali, or Hebrew prior to the foundation of Israel. Once Hebrew was revived as the language of Israel, it of course again began to change like any other living language.

9. For example, PPN *fara 'pandanus', *kawa 'kava', *reŋa 'turmeric', and *ti 'ti' became Maori wharawhara '*Phormium* spp., NZ flax', kawa(-kawa) '*Macropiper excelsum*, kawakawa', rengarenga '*Arthropodium cirrhatum*, renga lily', and ti '*Cordyline* spp., cabbage tree' respectively. The different British, North American and New Zealand referents for 'robin' provide another example.

10. As Blust points out (1988: 59), the existence of the family was recognised by Europeans some 100 to 200 years earlier than IE.

11. As Meacham emphasises (1988: 92), this is certainly the case with Blust's 1976 PAN reconstruction of *bari for 'iron'. Here I would side with Meacham in that we have absolutely no evidence for the presence of metallic iron at such an incredibly early date, but would agree with Blust (pers. comm.) that the term could have referred to an iron ore such as hematite, possibly used for decorative or ritual purposes. However, the apparent presence of a term for 'tin' (*timeRaq) remains unexplained.

12. Green's paper, which I did not see until I had finished this one, has the same purpose: "to demonstrate that archaeologists who do attempt to control the technical arsenal of historical linguistics may yet turn out syntheses in Oceanic [or Southeast Asian] prehistory informed by the results of both disciplines" (Green 1994: 2; emphasis added).

13. Some of these scenarios are similar to those envisioned by Renfrew (1992a: 15-6), but were arrived at independently; I have omitted those scenarios more typical of state-level societies; i.e., my parallels to Renfrew's 'elite dominance' (e.g., Arabic, Turkish) and 'system collapse' (e.g., Latin to Romance).

14. Meacham's rather tongue-in-cheek scenario (1991: 405) would itself make a good mini-series ('Rootcrops' would be a possible title!).

15. I certainly do not wish to imply that language shift is inevitable in all such cases; Frisians, for example, have been able to maintain their language despite centuries of pressure from Dutch, German and Danish (Markey 1981). But despite such exceptions, shift is certainly far more common than maintenance.

16. Although Basque is one very tentative possibility (Price 1984).

17. Some of this diversity can be explained by relatively recent migration of some of the Tibeto-Burman groups into the region; but other groups, like the AA Khmu, Lawa, Lamet, etc., have clearly been there for a very long time.

18. Perhaps roughly paralleling Markey's 'North Sea Germanic Speech Community' (1981: 17-20) during most of the first and early second millennia AD.

19. Given Norman's evidence for an AA substratum in Yue and Min dialects mentioned above, I would now certainly redraw my map of presumed language areas in the fourth and third millennia BC (Bayard 1975b: 71), moving Tai-Kadai north and away from the Guangdong coast and extending AA up to take its place.

20. I am grateful to Bob Blust for kindly supplying me with prepublication copies of the two papers referenced; note that the second (Blust forthcoming) has been slightly revised from the version supplied to me.

21. Certainly the little archaeological investigation done to date in the middle Mekong area has not yet encountered any early agricultural sites of the type we would expect to result from a very early movement of Mon-Khmer-speaking rice farmers down the valley. The Pa Mong survey, which covered the Loei Valley and adjacent areas of Laos (Bayard 1980), suggests that nucleated agricultural villages appeared quite late there, along with iron tools, in the latter part of the first millennium BC (what I have termed General Period C). On the other hand, such early agricultural settlers may have lived in very small hamlets of low archaeological visibility, particularly when overlain with later occupation sites and/or paddy agriculture. But I will have to add my doubts - shared with Benedict (pers. comm. 13 September 1994) - about the eastern foothills of the Himalayas as a likely spot for initial rice domestication.

22. It would be wise to remember the parable of the continental drift hypothesis; Wegener's ideas were considered absurd by many until further data became available 50 years later!

23. Blust has put it very well in one of his two recent papers: "we must not forget that language and material culture are simply different aspects of what to the native participant must be considered a 'way of life', and as a reflection of this fact I would hope that linguists and archaeologists alike come increasingly to realise the benefits of interdisciplinary cooperation in our common attempt to understand the history of our species as a cultural animal" (Blust, forthcoming: 16).

REFERENCES

- Bayard, D. 1975a. On Chang's interpretation of Chinese radiocarbon dates. *Current Anthropology* 16(1):167-69.
- 1975b. North China, South China, South East Asia, or simply 'Far East'? *Journal of the Hong Kong Archaeological Society* 6: 71-9.

- 1976. *The Cultural Relationships of the Polynesian Outliers*. University of Otago Studies in Prehistoric Anthropology 9. Dunedin: University of Otago.
- 1979. Comment on H.L. Shorto, The linguistic protohistory of Mainland South East Asia. In R.B. Smith and W. Watson (eds), *Early South East Asia: Essays in Archaeology, History, and Historical Geography*, pp. 278-80. Oxford: Oxford University Press.
- 1980. *The Pa Mong Archaeological Survey Programme, 1973-75*. University of Otago Studies in Prehistoric Anthropology 13. Dunedin: University of Otago.
- 1987. Review of Bellwood, *Prehistory of the Indo-Malaysian Archipelago*. *Journal of the Polynesian Society* 96(1): 114-8.
- Bellwood, P. 1985. *Prehistory of the Indo-Malaysian Archipelago*. Orlando: Academic Press.
- 1988. A hypothesis for Austronesian origins. *Asian Perspectives* 26(1): 107-17.
- 1991. The Austronesian dispersal and the origin of languages. *Scientific American* 256(1): 70-5.
- Benedict, P. K. 1975. *Austro-Thai Language and Culture, with a Glossary of Roots*. New Haven: HRAF Press.
- 1991. Austric: an 'extinct' proto-language. In J.H.C.S. Davidson (ed.), *Austroasiatic Languages: Essays in Honour of H.L. Shorto*, pp. 83-7. London: School of Oriental and African Studies.
- Blust, R. 1976. Austronesian culture history: some linguistic inferences and their relations to the archaeological record. *World Archaeology* 8(1): 19-43.
- 1982. The linguistic value of the Wallace Line. *Bijdragen tot de Taal-, Land- en Volkenkunde* 138: 231-50.
- 1988. The Austronesian homeland: a linguistic perspective. *Asian Perspectives* 26(1): 45-67.
- 1994. The Austronesian settlement of mainland Southeast Asia. To appear in *Proceedings of the Second Annual Meeting of the Southeast Asian Linguistic Society (SEALS II)*, in press.
- forthcoming. Beyond the Austronesian homeland: the Austric hypothesis and its implications for archaeology. To appear in the *Papers of the University of Pennsylvania Museum of Anthropology and Archaeology*.
- Crowley, T. 1992. *An Introduction to Historical Linguistics*. Second Edition. Auckland: Oxford University Press.
- Dorian, N. (ed.). 1989. *Investigating Obsolescence: Studies in Language Contraction and Death*. Cambridge: Cambridge University Press.
- Foley, W. 1986. *The Papuan Languages of New Guinea*. Cambridge: Cambridge University Press.
- Gal, S. 1979. *Language Shift: Social Determinants of Linguistic Change in Bilingual Austria*. New York: Academic Press.
- Green, R. 1981. Location of the Polynesian homeland: a continuing problem. In J. Hollyman and A. Pawley (eds), *Studies in Pacific Languages and Cultures in Honour of Bruce Biggs*, pp. 133-58. Auckland: Linguistic Society of New Zealand.
- 1994. Archaeological problems with the use of linguistic evidence in the reconstruction of rank, status and social organisation in Ancestral Polynesian Society. In A.K. Pawley and M.D. Ross (eds), *Austronesian Terminologies: Continuity and Change*, pp. 1-14. Canberra: *Pacific Linguistics* C-127.
- Headland, T. N. and L. A. Reid. 1989. Hunter-gatherers and their neighbors from prehistory to the present. *Current Anthropology* 30(1): 43-66.
- Higham, C. 1996. Archaeology and linguistics in Southeast Asia: implications of the Austric hypothesis. *IPPA Bulletin* 14:110-118.
- Higham, C., and R. Thosarat. 1994. *Khok Phanom Di: Prehistoric Adaptation to the World's Richest Habitat*. Fort Worth: Harcourt Brace.
- Lass, R. 1987. *The Shape of English: Structure and History*. London: Dent.
- Lynch, J. 1993. *Pacific Languages: an Introduction*. Suva: University of the South Pacific.
- Markey, T.L. 1981. *Frisian*. The Hague: Mouton.
- Meacham, W. 1988. On the improbability of Austronesian origins in South China. *Asian Perspectives* 26(1): 89-106.
- Norman, J. 1988. *Chinese*. Cambridge: Cambridge University Press.
- Oka, H.-I. and H. Morishima. 1971. The dynamics of plant domestication: cultivation experiments with *Oryza perennis* and its hybrid with *O. sativa*. *Evolution* 25: 356-64.
- Pawley, A., and K. Green. 1971. Lexical evidence for the Proto-Polynesian homeland. *Te Reo* 14: 1-35.
- Pawley, A., and R. Green. 1984. The Proto-Oceanic language community. *Journal of Pacific History* 19(3): 123-46.
- Pawley, A. and M. Ross. 1993. Austronesian historical linguistics and culture history. *Annual Review of Anthropology* 22: 425-59.
- Price, G. 1984. *The Languages of Britain*. London: Edward Arnold.
- Reid, L. A. 1988. Benedict's Austro-Tai hypothesis - an evaluation. *Asian Perspectives* 26(1): 19-34.
- Renfrew, C. 1987. *Archaeology and Language: the Puzzle of Indo-European Origins*. London: Jonathan Cape.
- 1988. CA book review: *Archaeology and Language: the Puzzle of Indo-European Origins*. *Current Anthropology* 29(3): 437-68.

- 1992a. World languages and human dispersals: a minimalist view. In J.A. Hall and I.C. Jarvie (eds), *Transition to Modernity: Essays on Power, Wealth and Belief*, pp. 11-68. Cambridge: Cambridge University Press.
- 1992b. Archaeology, genetics and linguistic diversity. *Man* 27(3): 445-77.
- Shibatani, M. 1990. *The Languages of Japan*. Cambridge: Cambridge University Press.
- Solheim, W. G. II. 1988. The Nusantara hypothesis: the origin and spread of Austronesian speakers. *Asian Perspectives* 26(1): 77-88
- 1993. Four lobes of the Nusantara maritime trading network. Paper presented at 34th Congress of Asian and North African Studies, Hong Kong, 22-28 August.
- Specht, J. 1979. Review of Bayard, *The Cultural Relationships of the Polynesian Outliers*. *Archaeology and Physical Anthropology in Oceania* 14(2): 155-6.
- Zide, A.R.K., and N H. 1976. Proto-Munda cultural vocabulary: evidence for early agriculture. In Philip N. Jenner, Laurence C. Thompson, and Stanley Starosta (eds), *Austroasiatic Studies Part II*, pp. 1295-1334. *Oceanic Linguistics Special Publication* 13. Honolulu: University Press of Hawaii.