

## AN ARCHAEOLOGIST'S VIEW OF LANGUAGE MACROFAMILY RELATIONSHIPS

Peter Bellwood\*

*This is a slightly modified version of a paper presented to the Conference on Asia-Mainland/Austronesian Connections, held at the University of Hawaii at Manoa, May 10-13 1993. Because the conference proceedings will be published in a linguistics outlet (Oceanic Linguistics), the paper is presented here in the interests of discussion amongst archaeologists. The essence of the paper is that language macrofamily relationships probably chart ancient population dispersals on scales which the archaeological record often does not reveal. Archaeologists, particularly those working in Southeast Asia and Oceania, have tended always to favour the indigenous over the exogenous as a basis for explanation. Human reality, as seen through language and biology, suggests that population dispersals and replacements, especially those consequent on early developments of agriculture, were probably of very great significance on many occasions in Holocene prehistory throughout the world.*

The territory between historical linguistics and prehistoric archaeology has in general been rather an undisciplined one. ...only by using models that relate social and demographic changes, on the one hand, to language change, and on the other to changes in material culture, can we hope to make systematic progress (Renfrew 1992:15).

The linguistic literature has recently been enlivened by opinions about the existence or otherwise of macrofamilies of genetically related language families, such as Amerind, Nostratic, Austric and Austro-Tai (Ross 1991)<sup>1</sup>. The debate has opposed those who believe that relationships should be fully demonstrated, through comparative reconstruction of cognates at the family proto-language level, against those willing to use broader-brush methods such as lexicostatistics and mass comparison. To a non-linguist there seem to be merits on both sides, and no particular side is taken in this article. However, it can still be stated, as something fairly self-evident and non-controversial, that unless language families were all created *de novo* less than ten thousand years ago from a world-wide base of non-linguistic communication (a scenario clearly most unlikely given present understanding of prehistory), then each family must, of course, be related genetically to at least some other(s). The linguistic debate is clearly about *how* one

demonstrates genetic relationship, rather than with any dogmatic view that inter-family genetic relationships *cannot* exist.

Non-linguistic disciplines, such as archaeology and biological anthropology, can offer useful contextual information for this debate. For some years I have been examining very carefully the topic of why major language families exist and whether the extensive spreads of some of those in temperate and tropical latitudes can be related to population growth and colonization consequent upon the establishment of systematic economies of agriculture and animal husbandry. The archaeological record of early agriculture is sufficiently "explosive" in some regions to suggest that languages probably were carried outwards from them by colonizing populations. Some of these languages might have served as proto-languages ancestral to existing language families, and through geographical propinquity these proto-languages might already have shared with each other varying time depths of genetic relationship. Indeed, it is not essential to be able to *prove* phylogenetic relationship in order to demonstrate the sources of such putatively related proto-languages amongst adjacent and expanding early agricultural populations. This is because evidence for early borrowing can indicate the same conclusion just as well. Early borrowing is an extremely important phenomenon from the viewpoint of a culture historian, and I am sometimes surprised when linguists seem to dismiss it as being of little significance.

#### SOME CULTURE-HISTORICAL IMPLICATIONS OF LANGUAGE FAMILIES

One basic inference which I make about human prehistory is that language families are not just the results of convergence, through a mesh of unmoving populations, out of widespread prior systems of linguistic diversity<sup>2</sup>. Neither have they always existed in some ancestral form through maintenance of their internal relationships since the pre-40,000 BP colonizations of the (Middle? Upper?) Pleistocene world by hominids with language. My understanding is that the majority of linguists would also find such explanations to be without empirical support or historical precedent. Indeed, a central principle of comparative linguistics is that a true language family (by "true" I mean a phylogenetic entity that all linguists accept; Austronesian, Indo-European and so forth) must owe its existence, according to the reconstructed hierarchy of proto-languages, to dispersal and divergence from a relatively restricted geographical homeland region:

The fact that a set of languages can be shown to be genetically related entails that there was a real protolanguage, spoken by a particular group of people, in a particular region, at a particular time period (Kaufman 1990:21).

If we accept this, and accept that the ancestral forms of language families are unlikely to travel huge distances by language shift alone (a process which would rapidly erase phylogenetic linkages owing to the interference through shift phenomenon: Thomason and Kaufman 1988), then we can hardly avoid the issue of population spread by colonization.

The shape of a language family tree (or the absence of a coherent tree) can also inform us how a colonization might have taken place. For instance, some family trees are "strong", in the sense that their component proto-languages branch in a tree-like fashion and rank nicely in a fairly spread-out chronological order, like Fijian and Polynesian (to use Austronesian examples). Others, perhaps the majority of language families and major subgroups in the world, tend to be "weak", or rake-like in structure, in the sense that their subgroup proto-languages are very hard to rank at all in any chronological sense, as for instance many of the Oceanic languages of Melanesia (Pawley and Green 1984:136, but cf. Ross 1989 for some degree of modification) and many of the subgroups of Western Malayo-Polynesian. I suspect these differences may relate in part to the speed of initial dispersal of the founder population - slow in the case of Polynesia (1200 BC to AD 1000), fast in the case of the earliest Oceanic languages of Melanesia (1500-1200 BC). There are doubtless many other factors which also affect weakness versus strength of tree structure (areal diffusion - as in Pama-Nyungan; strength of substratum, as in New Guinea Austronesian), but I think rate of early spread may be a crucial factor.

In the case of the Oceanic Austronesian languages, we know that the mechanism of founder language dispersal through those islands which did not have prior Papuan-speaking populations was colonization<sup>3</sup>, not diffusion or imposition by conquering élites. My reading of the archaeological and Austronesian linguistic evidence suggests to me that such colonization, replacing earlier Non-Austronesian hunter-gatherer populations (except on the Southeast Asian mainland, where Malay and Chamic speakers at one time replaced earlier Austroasiatic cultivators), also occurred through Island Southeast Asia. I suspect also that many of the other major and very widespread language families of agricultural latitudes (Indo-European, much of Afro-Asiatic, Bantu, Dravidian, perhaps Altaic with its long history of pastoralism, Sino-Tibetan, Austroasiatic, Otomanguean, Uto-Aztecan, Mayan, and several other families of eastern North America, the northern Andes and the Amazon basin) spread mainly by such means. I am unable to identify any historical precedents or logical reasons which would raise convergence, diffusion or élite domination to the level of major causative factors on such scales, and no linguist I have ever discussed this with has offered me any reason to change my mind. Issues of how languages spread are very complex and beyond easy discussion, but I must stress that I am here giving an opinion on major language spreads which took place in pre-state contexts, and presumably amongst quite small-scale societies with very simple means of communication (Bellwood 1991).

The view that major language families owe their historical foundations, at least in major part, to processes of prehistoric population colonization can be countered if one adopts the statistical viewpoint that language family phylogeny is merely an epiphenomenon of cladistic analysis. In biology, cladistics offers reconstructions of the origins of species in terms of radiation from and adaptation beyond a region of origin. Given the Darwinian rather than Lamarckian nature of biological evolution, biological species presumably do have restricted regions of origin. However, given the partially Lamarckian nature of linguistic transmission (via borrowing), one could argue that

language families do not really have such restricted origins in space but simply look as though they do, and therefore that there are no processes of actual population expansion in their histories. By this view, language families result from areal diffusion and convergence.

In my opinion, such a view would be unfounded. As already noted, no linguist has ever proposed a satisfactory sociolinguistic mechanism to explain how convergence on the scale of a major language family could occur, or, even if it did occur, how the resulting degree of homogeneity could be maintained through time. Furthermore, convergence seems unable to explain the sharing of core elements of vocabulary by widespread languages within a family, given that core vocabulary items are generally considered to be resistant to diffusion. As for maintaining homogeneity over millennia, it should be remembered that even modern English is differentiating, despite schools, television and mass tourism. Not only is there nothing in linguistic history to support such a geographically-diffuse explanation of convergence for language families, but it should also be evident that languages will have expanded very greatly in geographical extent whenever human population expansion (via colonization) has occurred. This is where other disciplines provide parallel witnesses, for the archaeological and biological records as they relate to the consequences of early agriculture indicate to me that very widespread colonizations undoubtedly did occur in prehistory. As I will discuss further below, the idea that agriculture would have been easily diffusible into mobile hunter gatherer communities, without a linked dispersal of the agriculturalists themselves, finds no support in the ethnographic record.

We thus appear to have hints, to be fleshed out more below, of a very pertinent agreement in the world-wide structures and patternings of the archaeological and linguistic records, an agreement based on independent sets of data rather than on circular reasoning from each discipline back into the other. Periods of major prehistoric colonization imply that the logic of the language family tree, with its dependence on a homeland concept followed by dispersal, might well have a very strong foundation in many quite specific historical events<sup>4</sup>.

There is another observation which tends to reinforce this view, from the data of glottochronology. While this technique has suffered from a bad press in recent years, I receive the impression that most linguists feel it can give approximate results if used carefully, particularly in situations where great disparity in rates of vocabulary change (as between some west Melanesian Austronesian languages and Malay, for instance: Blust 1992:46) can be taken into account. It also seems to work better the closer one comes in time to the present. In the Americas the origins of systematic and expansive Early Formative agricultural systems are closer in time than many parallel systems in the Old World. So too (according to glottochronology) are many of the language families which might, according to geographical location, be associated with the early expansions of such agricultural systems. This is evident from Kaufman's glottochronological dates for certain American language families (Kaufman 1990: I use here his preferred spellings): Otomange 6000 years; Yutonawa (Uto-Aztecan) 4500 years; Sian 4000 years; Paesan

5400 years; Chibchan 5600 years; Maipurean 4500 years; Tukanoan 4500 years; Kechumara 4500 years; Pano-Takanan 4700 years; Je 5400 years; Tupian 5500 years; Kariban 3700 years. While it is not pertinent here to go into fine detail, most of these glottochronological dates (perhaps excluding Je and Tupian) seem to correlate very well with the relative orders of archaeological (mainly radiocarbon) dates for the spreads of agriculture in the various regions represented.

For instance, Otomange, Paesan and Chibchan (with the more enigmatic Tupian) are the oldest families listed, and overlap geographically and chronologically with the earliest regions and dates of American agriculture in central Mexico and the northern Andes (especially coastal Ecuador and northern Peru). The date for Suan fits well with the beginnings of indigenous seed agriculture in the eastern Woodlands of North America, and that for Yutonawa (Uto-Aztecan) seems to correspond (albeit perhaps a little too old) with the oldest dates for the expansion of maize agriculture north from central Mexico towards the Southwest USA. The date for Kechumara also fits that for the establishment of sedentary agricultural communities in the Peruvian Andes. The Amazonian families seem slightly early for agriculture in the Amazon basin, but I think many archaeologists would agree that much of the record here is very fugitive for environmental reasons. Without labouring the point (and this is a topic which I am currently working on in more detail), I would suggest that these dates correlate too well with the order of archaeological dates for early agriculture to be due to pure coincidence, or due merely to some linguistic property of language families which makes them invisible when they are more than 6000 years old. Even if the glottochronological dates are slightly older or younger than they should be, their relative order of duration still correlates very closely with the relative order of regional agricultural beginnings.

#### HOW DID AGRICULTURE SPREAD?

I offer below some archaeological and related notes on matters which must be of concern to any linguist asking historical questions about how, where and when particular language families began their separate existences. An archaeologist can ask how, where and when agriculture developed and what happened to the populations and associated cultural traditions which first underwent the transition. Did they grow in numbers and colonize? Why did some agricultural groups expand more than others (e.g. Austronesians rather than Papuans across the Pacific)? And from a comparative perspective, would hunter gatherers in the paths of expanding cultivators

(1) have adopted just agriculture alone without language shift?

Or (2), have adopted both agriculture and language from the newcomers, as some Philippine Agta have presumably done from Austronesian populations, or Malayan Semang from incoming Austroasiatic speakers?

Or (3), have adopted neither and simply been absorbed or pushed away by the larger incoming agricultural population?

These are important questions, especially for language history, and there is no simple answer to them because the world contains so many unique trajectories of cultural development. However, some generalizations are possible. For instance, if agriculture spread mainly by diffusion through existing hunter gatherer communities without the adoption of new languages (case 1 above), we would not expect much change in pre-existing Palaeolithic language distributions. If new languages spread together with agriculture to pre-existing hunter-gatherers (case 2 above), we would expect a great deal of interference through shift as linguistically quite diverse local populations adopted the new tongues of the agriculturalists. If the original cultivators colonized with agriculture as well as their languages, and pre-existing hunter gatherers played no part in the transmission of either (case 3 above), then far less substratum effect should be visible. Just how far linguists can go in distinguishing these three situations in the remote past I am not sure (archaeologists have similar difficulties), but it seems to me that cases 2 (language shift) and 3 (colonization) are both recognisable in various regions of Austronesia, whereas case 1 (hunter-gatherer adoption of agriculture with no language change) seems to have been rather rare anywhere in the world. Basque and Yuman maybe, but I know of no convincing situations of this type in Southeast Asia.

I will now attempt to indicate that major human expansion, according to archaeological, botanical, environmental and, of course, linguistic data, did occur from agricultural homelands, especially those located in southwest Asia and China (as also from Sahelian Africa, highland Mexico and the northern Andes). These areas all served as primary agricultural homelands at various times between 9000 and 3000 BC. Other agricultural homeland regions include the New Guinea Highlands, the Mississippi Basin and perhaps parts of lowland tropical South America, but these do not seem to have produced such major population expansions, for reasons which may in part be to the nature of the production systems involved, especially the lack of major cereals and domesticated meat animals (the date of first arrival of pigs in New Guinea is now uncertain, and is not convincingly demonstrated to be pre-Austronesian).

#### EARLY AGRICULTURE AND ITS CONSEQUENCES

The only region of the world for which a detailed picture of the transition to agriculture is available is southwestern Asia. By 15,000 years ago here the post-glacial climate was ameliorating to warmer conditions with an increasing seasonality of rainfall<sup>5</sup>. By 12,000 years ago (10,000 BC), expanding regions of oak-pistaccio woodland interspersed with stands of wild annual barley and wheat offered food supplies, combined with hunted meat, sufficient to support villages of circular huts in the Levant with populations estimated at up to 300 people. Some of these small villages (such as Jericho), probably already sedentary in a few cases, underwent major size increases after 9000 BC consequent upon the development of cultivation and cereal/legume domestication. These developments were possibly stimulated by the expansion of people into environments slightly marginal for wild cereal growth where the conscious planting and tending of tough-rachised seed stock was undertaken. By 7000 BC there is evidence of villages with

several thousand inhabitants (Jericho, Ain Ghazal, Abu Hureyra, Basta), covering 12 or more hectares. The western Asian population by this time, especially in the Levant, had evidently become so large that hunted meat was becoming almost unobtainable. Enter animal domestication, initially of goat, then sheep and cattle, adding to an economy of unprecedented productivity which by 5000 BC was thrusting its way through central Europe, up into southern Russia, across into Pakistan, and southwards into the Nile Valley and into the Sahara (then much wetter than now).

This is a very brief scenario of a very complex transition, but there are a number of points which I wish to stress. While the overall transition from mobile hunter-gatherers to sedentary agriculturalists took quite a long time (maybe 15,000 to 7000 BC), the key shift in activity, that to plant cultivation (via conscious planting), occurred very rapidly in the Levant between 9000 and 8000 BC. This led to dramatic population growth, recognisable in major increases in village sizes. Communities with similar lifestyles in terms of artefacts, house styles and crops had appeared from southern Israel to northern Iraq by 8000 BC<sup>6</sup>, and by 7000 BC we can identify a wider spread of a relatively high degree of artefactual and stylistic homogeneity into Turkey, Iran and Pakistan<sup>7</sup>.

Little is known about the health of these populations, but there are indications that early agricultural populations throughout the world in general enjoyed better health than their more crowded descendants. When agriculture began the world was only lightly populated and probably relatively free of major epidemic diseases. Populations who developed an ideology favouring expansion to found new settlements would have found few barriers, except purely environmental ones, or perhaps occasional resistance from hunter-gatherers in regions where they were fairly densely settled, and such early cultivators could probably have expanded their numbers very quickly indeed. I will expand on this ideological aspect of colonization later.

In regions beyond southwest Asia, such as Europe and Egypt, the homogeneity characteristic of the core region of agricultural origins rapidly broke down, as one would expect. This indicates that colonists from western Asia entered these regions but rapidly underwent cultural differentiation in relative isolation, rather like the earliest European colonists in North America and Australia. In the case of Egypt one also has to allow for the possible adoption of a southwest Asian economy (c.5000 BC) by an Afro-Asiatic population already present in the Nile Valley. Some degree of indigenous hunter-gatherer adoption of the new lifestyle must have been possible in many similar regions of high forager productivity (coastal regions of Europe, for instance), or in regions environmentally marginal for the agricultural economy.

Nevertheless, when looked at in entirety, the revolutionary nature of this early development of agriculture in western Asia demands the most careful attention by archaeologists and linguists alike. In my opinion, the very strong archaeological evidence for unprecedented population growth and the correlated very wide spreads of relatively homogeneous cultural entities make some degree of outwards population colonization almost inescapable. Frankly, it is impossible to visualise the process as one whereby completely unmoving hunters and gatherers simply adopted a full agricultural lifestyle

through diffusion from some distant southwest Asian source. Had this happened one would not expect to see the level of cultural homogeneity represented in the Levantine or southeastern and central European Neolithic (Keeley 1992). The same argument applies to Oceanic Lapita, to the Mesoamerican and Andean Early Formatives, and to the early Chinese Neolithic. Similarly, one would not expect coherent language families to exist through large regions if some imaginary foundation *lingua franca* had spread entirely through imperfect learning and language shift.

This kind of explanation becomes even less likely given the ethnographic scarcity of cases, anywhere in the world according to my literature searches, in which former hunters and gatherers have, *by choice* (i.e. without enforced "settling down" imposed under colonial conditions), successfully adopted agriculture as a full-time means of subsistence (rather than as a casual activity on the side). Activity-scheduling factors and ideological differences between collectors and producers simply make the transition a very stressful one, particularly when the hunters and gatherers are not stimulated by increasing populations of their own to adopt the new techniques.

The southwest Asian Neolithic data, therefore, suggest the very high probability of an outward expansion of colonists, particularly after 7000 BC, adapting, diversifying and incorporating elements of pre-existing hunter-gatherer populations, until either environmental barriers (seas, deserts) or other populations doing the same thing from another direction put a stop to the process. Should Nostratic exist, and should Indo-European, Afro-Asiatic, Kartvelian, Uralic, Altaic, and perhaps Elamite and Dravidian all be genetically related, then here might be source of the relationship, as already suggested by Renfrew (1992).

This need not mean, however, that the Neolithic developments in western Asia occurred amongst people speaking *one single* language. Several groups already quite diversified might have been involved because the background environmental changes were obviously quite widespread, and this is perhaps why Nostratic is more elusive than many linguists would like. Proto-Nostratic, if it ever existed, could date back well before the c.7000 BC (and onwards) colonizations of agriculturalists.

Expanding the argument into other regions of the world, similar expansions perhaps started in Mesoamerica about 3000 BC consequent upon the development of high-yielding varieties of maize (several millennia after maize was first domesticated), in the northern Andes at a similar time, and also in western Africa when the Bantu speakers began to expand southwards with an agricultural economy after 1000 BC. However, it should be noted that the initial transitions to agriculture in western Asia, New Guinea and throughout the Americas occurred long before the local inventions of pottery. This circumstance renders rather puzzling the transition to agriculture in China, which shows signs of having occurred at a relatively more evolved cultural stage.

#### EARLY AGRICULTURE IN EAST ASIA.

Although Chinese archaeology is a very well researched field, it is a puzzling fact that agriculture here, beginning before 6000 BC with millet in the Yellow river basin and at



about the same time with rice in the Yangtze, has absolutely no antecedent stages. There is no known transition - all appears fully-formed with large villages, pottery and attached animal husbandry (pig, chicken, water buffalo), as though straight from the head of Athena or the rib of Adam. Yet a diffusion or transference of agriculture from western Asia is very unlikely; there is simply no archaeological evidence in support and the environmental and crop differences, apart from foxtail millet which also occurs in the European Neolithic, are great. The people and languages are quite different too - not minor observations in the present context. I am forced to conclude that, no matter how diligent Chinese archaeologists have been, they have somehow missed a stage of settling down similar to the Natufian in the Levant or the Cotton Preceramic on the northern Peruvian coast. Why?

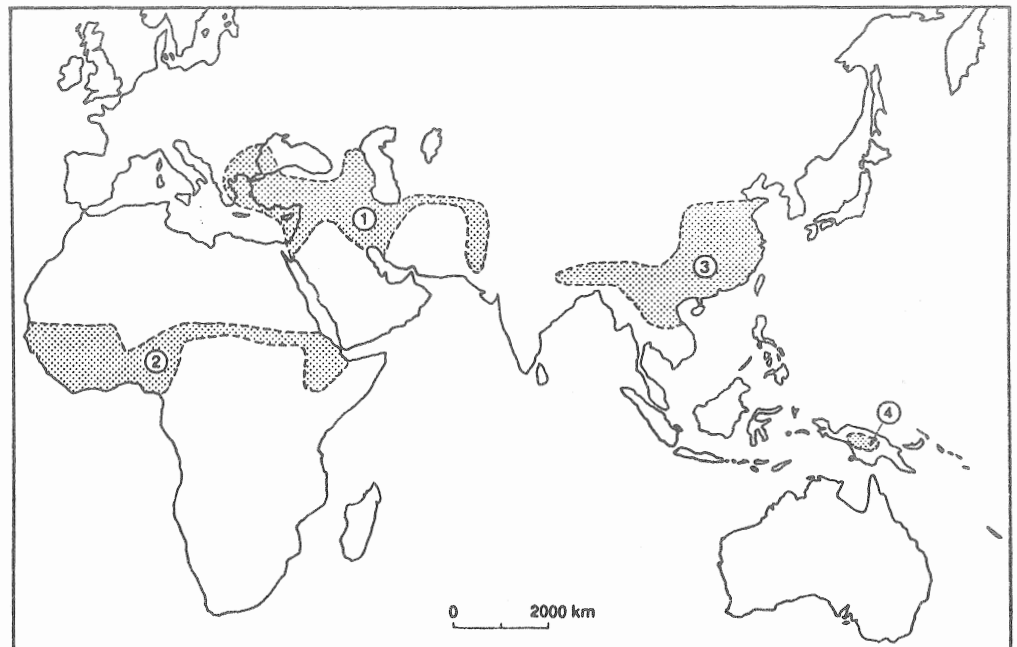
Was the transition perhaps so explosive in China that it is simply archaeologically invisible - a couple of centuries perhaps, during which time the populations involved in the transition could have multiplied their numbers exponentially every generation? If each couple averaged four children, who themselves, in healthy and relatively uncrowded circumstances, survived to reproduce to the same extent - a scenario perfectly reasonable given some figures for European colonists last century - then it is not hard to visualise how quickly the numbers could have grown. At this stage I would not stake my life on such an interpretation, but I would certainly not rule it out. If something like this happened we should see some impressive repercussions in the eastern Asian macrolinguistic picture.

The environmental background to the Chinese transition to agriculture is similar to that of western Asia - post-glacial climatic amelioration possibly interrupted by a Younger Dryas subglacial, combined with, in the monsoon regions, a greater seasonality and magnitude of summer (as opposed to winter) rainfall. These climatic changes, as in western Asia, could have encouraged a shift into cultivation when people attempted to grow plants along or beyond the edges of their ranges, as with the oldest domesticated rice along the middle and lower Yangtze. The Yellow river archaeological assemblages differ somewhat in stylistic terms from those of the Yangtze, just as the Early Neolithic assemblages of Iran differ in some ways from those of the Levant. More than one cultural, and possibly language, group was involved in these developments in both regions, but it is worth stressing that the Chinese Neolithic overall, if one interprets the data given by Chang (1986: Chapter 5) in terms of firmly dated Neolithic cultures, shows a surprising degree of homogeneity and interaction between 5000 and 3000 BC from the Yellow river southwards down through the middle and lower Yangtze to Hong Kong and Taiwan.

As with western Asia, I suggest that the Sino-Tibetan, Austroasiatic, Tai, Hmong-Mien and Austronesian peoples have all originated by outwards expansion (some have obviously expanded much further than others) from this large zone of Neolithic interaction. Thus, the proto-languages of these families, in so far as they can be reconstructed, should show some relationships, either genetic or due to early borrowing, or both. Because of the long passage of time - more than 8000 years since agriculture began in China - the traces of macrofamily relationship which survive will probably be very ambiguous, hence perhaps the great variety of macrofamily interpretations offered

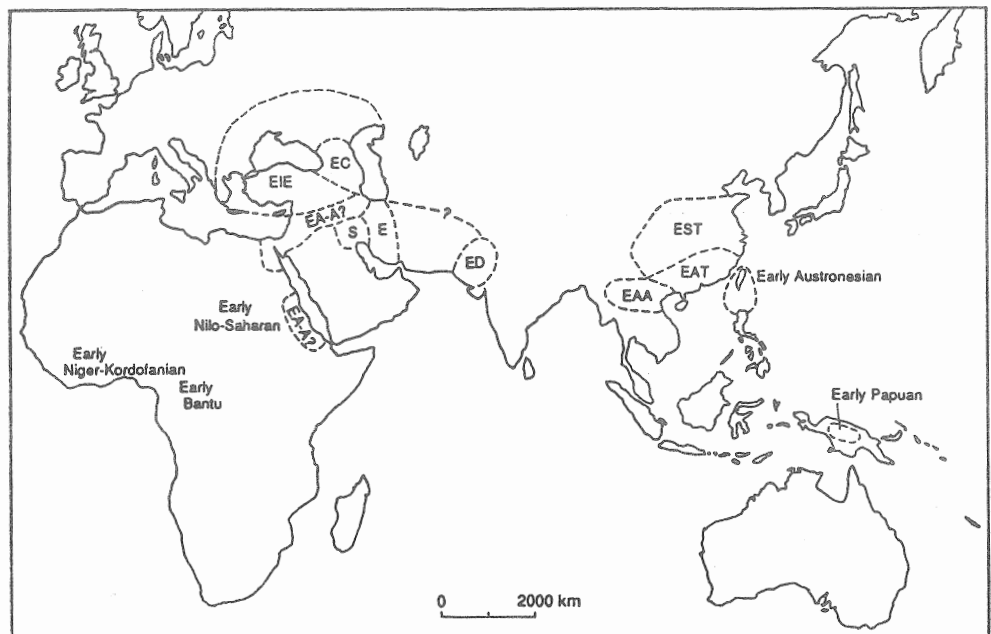
at the Honolulu conference where this paper was first presented. Everything may be related to everything else to some degree.

The archaeology of all this is of course very complex and not fully relatable here. My current views are enshrined in some detail in two recent summaries (Bellwood 1991, 1992). The speakers of early forms of what were later to become the Austronesian, Tai, Munda, Aslian, and presumably also Tibeto-Burman language groupings began their processes of colonization outwards from the Chinese agricultural heartland region starting perhaps somewhere around 4000-3000 BC. I strongly suspect, as noted above, that the Han Chinese cultural tradition has always been focused in the Yellow river basin, where the archaeological record has evidently been unbroken for 8000 years and where Chinese civilization eventually developed. If the Chinese migrated into this region from the mountains of inner Asia, as some linguists have suggested, then the archaeological record seems completely to have overlooked the fact. Such an inner Asian origin lacks any reason or explanation - why would people colonize outwards from a lightly populated region (upper headwaters of the great rivers?) which allowed no demographic, economic or technological advantages, still less impose their languages on one of the most densely settled Neolithic populations in world history?



**FIGURE 1: AGRICULTURAL HEARTLANDS OF THE OLD WORLD; SOUTHWEST ASIA, CENTRAL AFRICA, EAST ASIA, NEW GUINEA**

My conclusions are indicated in the two attached maps of agricultural and language family origins<sup>8</sup> (Figures 1 and 2), which suggest that regions of agricultural origin (including New Guinea) are regions where populations have in general not been replaced (Turkish obviously offers an exception here), and from where the net tendency over time has been for populations to move out rather than in. Hence, areas of agricultural origin reveal great linguistic diversity in terms of the number of language families represented, and also in terms of their correlations with regions where many linguists believe specific and important language families first evolved<sup>9</sup>. This does not mean that only agriculture explains language diversity - it certainly does not in the hunter-gatherer prehistories of California, Australia or Khoisan Africa, and in these cases the more basic idea of demographic or environmental resistance to incursion over very long time periods is surely germane.



**FIGURE 2: PRESUMED HOMELAND REGIONS FOR THE MAJOR OLD WORLD LANGUAGE FAMILIES WHICH ARE BELIEVED TO HAVE BEGUN THEIR EXPANSIONS WITHIN AGRICULTURAL HEARTLANDS.**

The boundaries shown are not intended to be definitive. EIE = Early Indo-European; EC = Early Caucasian; EA-A = Early Afro-Asiatic; S = Sumerian; E = Elamite; ED = Early Dravidian; EST = Early Sino-Tibetan; EAT = Early Austro-Tai (Benedict); EAA = Early Austroasiatic.

#### THE SOCIAL REPERCUSSIONS OF EARLY AGRICULTURAL COLONIZATION

In 1990 I prepared a paper on this topic for a conference on hierarchy in the Austronesian world held at ANU (Bellwood 1990). In that paper I concluded that the

founders of new settlements in resource-rich and previously unoccupied or uncontested environments could have acquired many rights in excess of those to which they would have had access in competitive sibling-rich environments back home - plentiful land, access to new and tradeable resources, increased status, descendants who would have revered them as founder ancestors, and so forth. In the phenomenon of foundership we might see the origins of some of the ranking systems which define, for instance, many Austronesian and other agricultural societies. Because founders get the "first pick" of all the best resources in the new territory, it stands to reason that their descendants will try to keep preferential access to these resources. What better way to keep such access than by establishing descent groups as corporate land and property owning units and ranking them (founder lineages at the top) by criteria such as birth order and closeness to the founder line? This may be exactly what the early colonizing Austronesians did, at least if origin legends recorded amongst groups such as Polynesians and Micronesians are valid witnesses.

This line of reasoning suggests that ideologies of colonization and foundership, combined with the statuses to be acquired from such activities, could have become culturally institutionalised in many societies that were offered opportunities to colonize. As I have just noted, many Polynesian and Micronesian societies seem to illustrate in their folklore many aspects of this way of thinking. Papuan speaking populations in New Guinea do not have such histories of colonization, neither do they have concepts of lineage ranking, status by birth order or founding heroes. From a comparative perspective, therefore, the process of colonization could have potentially a great deal to do with the development of ranking amongst Austronesians<sup>10</sup>.

I can go further than this, because I suspect that any society which is actively undergoing colonization, especially into regions where prior populations, however small, exist, will tend to be identity conscious. Although the idea of ethnic identity consciousness before the emergence of states has been attacked by Morton Fried (1975), he was not considering societies in colonization mode simply because colonization of the type here presumed to have been associated with early agriculture has never been a major factor in the ethnographic record, apart from a few rainforest groups such as the Iban of Borneo or the Yanomamö of the upper Orinoco. The world in the past few centuries has been far too full to allow the great majority of indigenous societies to do anything else but contract. In my view, the Middle East after 8000 BC and the south Chinese coast after 6000 BC witnessed social phenomena of a kind unthinkable to an anthropologist living in the crowded world of the present. Therefore, I suggest that early agricultural societies, proto-Austronesians and proto-Malayo-Polynesians included, were successful precisely because they were identity conscious (although not necessarily reluctant to incorporate outsiders), colonization conscious, and conscious of the great advantages which would accrue to successful founders. They were also surely language conscious, and would have systematically spread their languages into new territories in the process of expansion, rather than willingly converting to the pre-existing languages of resident hunter-gatherer populations. Existing hunter-gatherers in the track of agricultural expansion would not

have expressed such social features and for the most part would have simply succumbed to cultural and genetic absorption (unless, like some Philippine Agta, they adopted agriculture early enough to set themselves up in survival mode: Reid 1987).

Finally, I am left with the "bottom-line" question: why did some groups who underwent early transitions to agriculture (such as the Austronesians) colonize, but others (such as the Papuans of New Guinea) stay mostly at home, practicing in agricultural intensification rather than colonization to alleviate population growth? The productive capacities of the early agricultural economies surely relate to this - Papuans had no cereals and originally no domestic animals, Austronesians had both, thus we might assume that the Austronesian potential for population growth was of a greater order than that in New Guinea. Yet somehow this does not explain everything, not least why Papuan speakers never attempted to colonize adjacent Australia. I have to admit here that I am at a loss to offer an answer, although I suspect that societies far back in the Pleistocene were already beginning to differentiate themselves into colonizers versus "people of the land", for reasons perhaps to do with highly sensitive relationships between environmental factors such as resource patchiness and density, relative population densities, and climatic fluctuation. Hunter-gatherers obviously colonized during the Pleistocene, not least to Australia and the Americas, but in these cases mostly into unoccupied territory. Yet even in these chronologically remote situations it is not difficult to conceive of relationships between colonization and leadership which might have formed faint precursors of the far more tumultuous events of the Holocene.

#### NOTES

1. Because of the broad spread of the discussion in this paper, I have deliberately limited myself to a small number of references in order to avoid an excessive bibliography. I am currently developing the thoughts here presented at much greater length.
2. It seems to me that convergence sufficient to create a *family* of languages (as opposed to a few widespread linguistic features) can only occur if the languages concerned are related in the first place. One does not find convergence of this type in regions where languages are from completely different families, no matter how widely or frequently communication occurs or how much bilingualism might exist (e.g. between coastal Papuan and Austronesian speakers in Melanesia). I find it impossible to conceive how convergence alone can create a language family from previously unrelated components - it has certainly not done so in Mesoamerica or India, regions where areal diffusion is known to have played a major role in language history.
3. Luckily for Oceanic, the archaeological and linguistic records correlate particularly well since we can presume on very strong grounds that the first settlers beyond the Solomons were colonizing Austronesians entering previously uninhabited islands.
4. European colonization, after all, has produced vast dispersals of English and Spanish which, a millennium or more in the future, may become language families. And these dispersals have been spread mostly by colonization, albeit with some language shift on the peripheries.

5. Winter rainfall in this case, as opposed to the summer rainfall of southern and eastern Asia and Mesoamerica.

6. Early Levantine Neolithic or Pre-Pottery Neolithic A in archaeological terms.

7. As identified especially by early forms of agglutinative rectilinear architecture - Levantine PPNB, Cayonu, Ganj Dareh Level D, Mehrgarh.

8. I have recently been putting the Americas on to these maps but have not yet had time to finish the task. For Mexico I foresee no major problems, since the beginnings of maize cultivation there occurred in the general region where all the major language families meet (i.e. Valley of Mexico, Guerrero, Oaxaca, Chiapas). On South America I am still thinking; the picture is made more complex by the possibility of an independent domestication of manioc in the lowlands.

9. In this regard it is worth noting that Dyen's lexicostatistical classification of Austronesian suggested a Melanesian homeland, a conclusion at variance with all other sources of information. Comparative linguistic research suggests south China and Taiwan as the origin regions for AN, thus rendering suspect any lexicostatistical analysis which does not take into account varying rates of vocabulary change. In the Austronesian case, heavy borrowing and numerous instances of language shift in and around New Guinea have obviously distorted the picture. Why? In my view it is because New Guinea was another centre of early agriculture and population growth, albeit a poorly understood one at present. Papuan-speaking people built up large populations long before AN settlement in the region, and thus attained an unchallengeable survival mode.

10. It could be pointed out here that some modern "egalitarian" populations such as the Iban of Sarawak have a dramatic history of colonization which has led to no major degree of status ranking in society. I would agree that there may well be other social factors which pull in the other direction, towards an egalitarian ideology, but I would like here to quote from a recent paper by Clifford Sather which indicates to me that the linkage between colonization and status is very real amongst Ibans, even if not institutionalised in a heritable way. Sather is discussing Iban colonization of the Paku river in Sarawak:

The first groups to arrive were organised around migration leaders (*tuai mindah*), self-made men, most of them with a reputation for military prowess and charismatic leadership. Once such leaders succeeded in laying claim to a significant stretch of river, they and their successors assumed the role of regional leaders (*tuai menoa*), allocating their followers to settlement areas within these domains and defending their integrity against outsiders...Under their direction, groups of migrants were progressively dispersed into separate longhouse territories from which, once the process of clearing had begun, groups of newcomers might be excluded (Sather 1990:21).

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