

FURTHER CONSIDERATIONS OF THE HYPOTHESIZED AUSTRONESIAN NEOLITHIC MIGRATION FROM SOUTH CHINA TO TAIWAN AND LUZON

William Meacham*

INTRODUCTION

In this paper I examine some of the issues raised by Peter Bellwood arising out of discussions at the IPPA Conference in Peñablanca in 1985. Rather than renewing much of the former debate, already adequately covered by the published papers (Meacham 1984-85; Bellwood 1984-85), two recent articles on different aspects of the proposed Austronesian Neolithic migration into Island Southeast Asia are reviewed in some detail. The intention is to determine whether the hypothesized migration provides an explanatory framework of considerable strength, or whether it may obfuscate aspects of the data which may be of some significance.

Three elements of Bellwood's model for Austronesian expansion struck me upon re-reading:

1. There is generally no archaeological evidence to support a coherent migration of people from southern China into Taiwan and Luzon, but diffusion from the two adjacent land masses into Taiwan has taken place (Bellwood 1984-85:133).
2. The number of people in the first movement from the mainland of China was small ("one or a few families") and population pressure in southern China is not deemed to have provided the impetus for this initial movement.
3. These first "agricultural colonizers" were "Pre-Austronesian speakers" and it was only after a few centuries on Taiwan that the descendants of these migrants, plus any of the indigenous hunting gathering population they assimilated, began to speak Proto-Austronesian.

Taken together these ideas represent a line of reasoning much slimmed down from previous formulations, and what is left unpainted is perhaps the more interesting part of the picture. A single boatload of new arrivals is little more than the minimum mechanism required for the inter-island transmission of any cultural trait. Not only is it not a coherent

Indo-Pacific Prehistory Assn. Bulletin 11, 1991:398-407 (P. Bellwood ed., *Indo-Pacific Prehistory 1990*, Vol 2)

* Hong Kong Archaeological Society, Museum of History, Kowloon Park, Hong Kong

pattern of migration but it seems accidental, with no reasonable context. Further, it cannot be considered a "founder movement" since other resident and visiting groups would have been involved. The language which Bellwood argues eventually emerged as "Proto-Austronesian" was not necessarily derived directly from that spoken by the first agriculturalists, but would have been a hybrid of "Pre-Austronesian" and whatever languages were spoken by the resident peoples with whom the new arrivals interacted. It could even have been descended from the native language spoken on Taiwan before the hypothetical arrival of the migrant family or families. Similarly, he acknowledges that their culture could have been a blend of imported elements, assimilated traits and innovations.

These small groups "were able first of all to survive... and secondly to establish viable agricultural economies" (Bellwood 1984-5:115). When reduced to the precariousness of the survival of a few families, and without the imperative of population pressure and an agricultural expansion with strong links to the mainland, the reconstruction seems to be based more on the transmission of ideas than the actual movement of people. If it is accepted that the fisherfolk along the coast would have been more likely to have crossed the strait than settled rice farmers, the stage is set for a classic stimulus diffusion rather than an agricultural colonization.

Establishing a Prehistoric Migration

For a historical perspective on how the Neolithic of Taiwan has been viewed, and for a reasonable set of criteria to determine whether a migration has occurred, I will quote from Chang Kwang-chih's (1964a:197-8) remarks in a special Taiwan section of *Asian Perspectives*:

It is now solidly established that...each ceramic horizon of [western] Taiwan is a local facies of a corresponding and identical horizon of the South China coast...Two alternative interpretations immediately present themselves. Either the mainland cultures penetrated the island and were diffused among the aborigines who, before the penetration took place, possessed cultures of their own; or the mainland cultures were brought in by immigrants.

The solidly established identity of the Taiwan ceramic cultures is now of course completely disestablished. And we have found that the aborigines did indeed possess cultures of their own before the arrival of mainland traits. Chang does recognize the basic and still unresolved question - how are the similar traits to be explained, by cultural penetration or by migration? Before attempting an answer, Chang cites criteria set out by Irving Rouse (1958) and still applicable today:

It is necessary to do five things in order to demonstrate adequately that a migration has taken place: 1) identify the migrating people as an intrusive unit in the region it has penetrated; 2) trace this unit back to its homeland; 3) determine that all occurrences of the unit are contemporaneous; 4) establish the existence of favorable conditions for migration; 5) demonstrate that some other hypothesis,

such as independent invention or diffusion of traits, does not better fit the situation.

When Chang (1964a, 1964b) applied these five criteria, he found (quite surprisingly for the evidence even then available) that "all five of the conditions are satisfactorily met for all three of the ceramic horizons" and that the Corded Ware Culture, the Lungshanoid and the Geometric Horizon all came across with migrants from southeast China. He does acknowledge that the origin of the Corded Ware Culture (now known as Tapenkeng) could be local if "evidence unavailable at present was to show that the island was occupied by preceramic or even paleolithic inhabitants." And this is of course the situation now. But on the Lungshanoid and Geometric he showed an unwarranted certainty:

...it is beyond question that the island comes within the sphere of expanse of this [Lungshanoid] southeastern ceramic horizon...Unquestionably, the colonization of Formosa by mainland farmers took place mainly during the [Geometric] period or, roughly, from 1500 to 200 B.C.

It has always been my contention that such confident and erroneous conclusions arise from attaching too much significance to a few "marker traits", from a general tendency to see the mainland as the obvious and only source of advances in material culture, and from a specific tendency (which Bellwood exhibits, among many others) to take the earliest dated occurrence of a trait or repertoire of traits as the place of origin of an expansion of those traits.

Bellwood's proposed incremental and gradual expansion defies Rouse's criteria; it is virtually unprovable and unfalsifiable. People and languages, material culture and ideas all move, and may change as they go. Suffice it to say that none of the five conditions set out by Rouse are satisfied in Bellwood's scheme; it remains an interesting and possible scenario, albeit a most unlikely one in my view. I shall examine below two recent works in which the Austronesian Neolithic migration model is applied, in an effort to test its strength as a framework to interpret the data.

DATING THE ISLAND SOUTHEAST ASIAN NEOLITHIC

In a valuable study of radiocarbon dates from Island Southeast Asia, Spriggs (1989) has attempted to "assess their reliability, to weed out those which cannot be depended on, and to build a secure chronology with those that remain". His chronometric hygiene did indeed clear out a surprising amount of grit - confused or unreliable dates on which much previous writing had drawn. Unfortunately, one radiocarbon date got through Spriggs' filter, and the secure chronology he proposes is still fouled by the unreliability he sought to avoid.

Rejecting 21 dates for various reasons, Spriggs found from the remaining 98 dated samples that "a previously obscured pattern can be detected showing a north-to-south spread of Neolithic culture in the region" (Spriggs 1989:590). A rapid spread of the Neolithic from Taiwan through Luzon to Sulawesi and Timor was perceived in the period

5500-4100 BP. The crucial part of this purported spread of Neolithic culture is the antiquity of the Taiwan and Luzon sites.

One very early date from Laurente Cave in Luzon was rejected because it "sticks out like the proverbial sore thumb" and it was noted that there is as yet no published report on the site. Although the sample was from a controlled excavation, it is the only sample to have produced such an early age for a context with pottery, and in a strict "hygienic" approach it was wise to disregard it. The same criteria should apply to any single date occupying an isolated position - clearly with radiocarbon chronology there is strength in numbers, and clusters or series of dates provide the only secure basis for interpretive schemes. The remaining early dates for the Neolithic in the Philippines are (in uncalibrated dates bp):

4260 4260 3900 3690 3620 3510 3470 3410 3400 3290 3280

One notes immediately that there are no outliers in this series, and if a single date is erroneous its removal will not skew or distort the fundamental conclusion: Neolithic technology (at least pottery and polished stone or shell tools) was present in the Philippines by 5000-4000 BP (calibrated age).

Applying the same criteria to Taiwan, and rejecting one outlier which Spriggs accepted, the series is as follows :

4000 3985 3937 3840 3722 3532 3322 3312 3282 3207.

Clearly there is no greater antiquity of the Neolithic here; rather, it appears slightly later, but with the uncertainties of radiocarbon dating one cannot attach any significance to this. What is abundantly clear is that the pattern of spread from north to south hinges on a single, very dubious sample. I will return to this date below.

Sulawesi/ Talaud and Timor likewise do not appear earlier than either Taiwan or the Philippines :

Sulawesi/Talaud 4390 4050 4030 3550 2820

Timor 3740 3545 3530 3470

What seems evident from the purified date list is that the Neolithic emerges almost simultaneously in all these areas. If this emergence relates in any way to the supposed southward movements of Austronesian speakers at around this time, it cannot be discerned through dating. There are of course many other scenarios which might explain the relatively sudden widespread appearance of Neolithic traits. The "nearly perfect fit" which Spriggs perceives between archaeological dating and linguistic reconstruction appears to have been in the eye of the beholder only.

Spriggs does draw attention to the single early date for the Taiwan Neolithic, and noted that "the next-earliest pottery associated date is 4500/4400 BP, later than Neolithic dates for Luzon, Talaud and Sulawesi" (Spriggs 1989:605). This observation should have set off warning bells; no "clear indications of a spread from the north (Taiwan) to the

south and east (Timor)" could come from a single date. Spriggs does ask if we can accept the single date, and then meanders into a discussion of other Taiwan sites, notably the aceramic sites of Ch'ang-pin and O-luan-pi with consistent dates down to 5600 BP. Again, the alarm bells are sounding, and one cannot escape with the claim that "there is certainly considerable Neolithic 'action' prior to 4500 BP". Would this not be equally true for Luzon, Talaud and Sulawesi? But then, no one (almost no one) is proposing these areas as the place of origin of the Austronesian agriculturalists.

The early sample (5480±55 bp in the original site report, corrected to 5892 by Spriggs for oceanic reservoir effect, but see below) comes from the site of Pa-chia-ts'un near Tainan. It has been questioned by W.H. Sung (personal communication, 1976) who doubts the association of the shell with the cultural layer. This doubt was mentioned in my paper at the 1985 IPPA meeting at Peñablanca. The original site report (Huang 1974) reveals other reasons for rejecting the date: the sample was not collected during excavation, but from the water's edge at the base of a river bank; no excavation was conducted at the site. In addition, and perhaps the most important reason for being wary of using this date on its own, is the fact that it appeared to come from a shell midden deposit with corded ware of Tapenkeng type, in stark ("sore thumb") contrast to all other Tapenkeng culture sites on Taiwan. All known and properly excavated shell middens have yielded material of later Neolithic type.

If these circumstances were not bad enough, no identification was published of the shell, although Chang refers to it as "an oyster shell"; the site is now 14 km from the coast and 21 m above sea level. The sample could have been a brackish or even fresh water species, in which case it would not qualify for Spriggs' list because of uncertainty about the correction factor. It could have been re-deposited by the river; or it could belong to an ancient beach deposit laid down before human occupation. Clearly it is not reliable.

All of this is not to say that the Tapenkeng Culture of Taiwan may not indeed date back to 5000 BP or more. My own feeling is that it does. There are dates on Neolithic deposits of 7600-5600 BP from the offshore island of Quemoy, and newly published dates (Tsang 1989) of 5100-4600 BP from the Pescadores in the Taiwan Strait. All these dated samples are unidentified shells, presumably marine.

But we do not have any radiometric grounds at present for claiming any greater antiquity for the Tapenkeng Culture. The opposite is true; an excavated sample of charcoal from the basal layer at the Tapenkeng site itself was rejected by the excavator (Chang) and, surprisingly, by Spriggs as "too young". Too young for whom? The result of 3080 bp was consistent with the dates of the overlying Yuanshan culture layer (2850, 2030 bp). It did not fit the picture expected by either Chang or Spriggs and it does pose certain problems in being contemporaneous with dates from layers which overlie the Tapenkeng Culture at other sites. Surely these are not sufficient grounds for rejecting it.

The sites of O-luan-pi and Ch'ang-pin are not far by crow flight from Pa-chia-ts'un (c.100 km to the south and east respectively). One is an open site, the other a cave site; both are on the coast. Both have reliably dated sequences of occupation which show that at 5600/5500 BP the culture was essentially Paleolithic. It is, of course, still *possible* that

the people of Pa-chia-ts'un had, even earlier at 6300 BP, a developed Neolithic culture that Huang describes as "more sophisticated in pottery shapes and patterns, more varied in stone tool types and shapes than seen in the Corded Ware layers at either Feng-pi-tou or Tapenkeng." But it is unlikely.

As I pointed out in my 1984-85 paper, it is this late start of the Neolithic which distinguishes Island Southeast Asia from the mainland of China and northern Indochina. Taiwan clearly falls in the sphere of Island Southeast Asia, especially when it is borne in mind that all sites in China and Vietnam (to my knowledge) which have been dated to the period 7000-4000 BP, and most of those dated to 10,000-7000 BP, are already Neolithic. The lingering aceramic Paleolithic of Taiwan finds its close counterparts in the islands further south (all dates uncalibrated bp):

Taiwan 5340 5240 5232 5202 4970 4870

Philippines 5840 5610 4482 4170 3950 3880

Sulawesi/Talaud 5740 4880 4860 3420

Timor 5520

If China and/or Vietnam (not to mention Japan) are the ultimate origins of the Neolithic traits that spread later into Island Southeast Asia, we are still far from establishing this fact by radiometric means. And it should be borne in mind that absolute dating of Neolithic assemblages does not address the question of whether they arose as a result of migration, diffusion or stimulus/local evolution.

THE PHILIPPINE NEGRITOS AS ABORIGINAL REMNANTS

Thomas Headland and Lawrence Reid (1989) describe in considerable and convincing detail the evidence that the Agta Negritos of northern Luzon have practiced swidden agriculture as a minor component of their subsistence since at least the early 18th century. They further describe a Neolithic site in the center of the Agta territory which has provided evidence of incipient agriculture at 1200 BC, and another on the western edge of the Agta area with evidence of rice cultivation by 1400 BC. Their commendably bold assertion (Headland and Reid 1989:47) that "prehistoric Negritos probably moved into the Neolithic at more or less the same time as their neighbors" is negated, however, by most of their interpretation of the archaeological and linguistic evidence, which seems to cast the Negritos inevitably in an aboriginal hunting-gathering mode. Specifically, why do they proceed to the conclusion that "rice-farming populations and *Negrito* hunters were living within a day's walk of each other in northeastern Luzon for at least the last 3000 years" (emphasis added). Assuming, in the absence of hard evidence, that the Negritos were present in the area at that early date, is there some reason why *they* could not have been the rice-farming population?

Despite their vigorous espousal of an "interethnic" model of interaction and exchange, the authors do not succeed in breaking free of the old isolationist view of the Negritos as

survivors from the Paleolithic. If such small indigenous societies are not to be regarded as fossils surviving from the Paleolithic, but rather as integrated components of an on-going cultural and adaptive evolution, it would be a much more cogent argument to link the prehistoric Negritos to the sites in their present area. To paraphrase Headland and Reid, the present "primitive" existence of the Negritos may well have arisen because they were forced into it at some point in relatively recent times by more powerful neighbors, and reduced to "commercial foraging" as "economically their most viable option in their very restricted circumstances." These circumstances may have arisen out of the impact of increased trade and intensified land use during the early Metal Age of the first millennium AD.

In spite of their own arguments against it, the authors end up in partial concurrence with most tenets of the old isolationist stance as they describe it:

...the first human inhabitants of the Philippines were some type of Pleistocene *Homo sapiens* that evolved some 20,000 years ago into the Negritos found in that archipelago today; ...their original languages were not Austronesian; ...they had at most only infrequent contact with the Austronesian-speakers who began migrating into the Philippines around 3000 B.C.

The hypothesis of a migration of Austronesian-speakers into the region is I suspect the major problem, and mars what is otherwise an important summary of the evidence concerning the Negritos. Headland and Reid refer repeatedly to this migration as if it were a well-established fact, although they do cite in a footnote opposing views including my own on the possible homeland of the "Proto-Austronesians". While the southward migration hypothesis certainly has great popularity at the moment, there is very little archaeological evidence in support of it. The tortuous path that the authors follow to separate prehistoric Agta from sites in their area is an example of the difficulties in actually applying the hypothesis to the data. What evidence is there to support the notion of an influx of Austronesian-speaking agriculturalists encroaching on the Paleolithic, pure hunting-and-gathering, Pleistocene-derived aboriginal Negritos, speaking a language now completely vanished without a trace in all 25 groups throughout the Philippines? Would it not be more cogent and parsimonious, once again, to consider local evolutionary schemes that adequately explain the data? If the Neolithic arose in the Philippines principally from a diffusion of certain traits and local evolution of others, all population groups were probably involved in the process.

Interpreting the Negritos as only one of a number of ethnic groups indigenous to the Philippines from the early Holocene, collectively with a gradient from Australoid to southern Mongoloid, would be consistent with the evidence as far as it goes, and it would solve the extremely difficult conundrum of the supposed switch to Austronesian. There are problems with this approach, of course, but until there is much more compelling archaeological evidence for an intruding population, I would suggest that the predecessors of the Philippine Negritos be counted among the ancient Austronesians, just as the Negritos of Malaya probably derive from the main body of Austroasiatic speakers.

If certain archaic forms close to Proto-Austronesian survive in some of the Negrito languages in the Philippines, this would lend some credence to this proposition. Linguistic, paleoanthropological and archaeological evidence do not often form mosaics that match, and for Southeast Asia we have learned almost never to expect a neat agreement.

The more interesting questions of why, how and when the Negritos began to be marginalized by more powerful groups and reduced to foraging can then be addressed without the unjustified (on present evidence) overtones of aboriginal versus immigrant populations in the Neolithic. Whether or not the Agta were already speaking or converting to Austronesian as early as 3000 years ago, they clearly had contacts with non-Negrito groups speaking a related language and engaged in rice cultivation. Their survival down to the present as a distinct ethnic and racial group is quite extraordinary in itself. There must have been major forces at work to achieve their cohesion, whether in the form of pressure from increasingly powerful neighboring groups, as has been suggested, or generated from within their own culture.

There is certainly a mystery to be unraveled here, and Headland and Reid (1989:46) only hint at it in their suggested scenario to account for the supposed switch to Austronesian:

This interaction was so intense that the Negritos adopted the language as their own. Later these ancient Negritos separated themselves from their non-Negrito neighbors...

I trust that the authors will forgive me for remarking that their use of the word "later" reminds one of the modesty of some writers of romance, avoiding any description of the sexual act. I will resist the strong temptation to make a colloquial analogy in speculating about what happened to the ancient Negritos in the course of an "intense interaction" millennia before they separated themselves and took to foraging. If, as I suggest, the Philippine Negritos were speaking Austronesian from its earliest emergence, then there may not have been such intense and intimate relations between them and adjacent groups. Clearly there could never have been any significant inter-marriage. Is it possible that racial pride and/or prejudice played a role in both their survival as a distinct ethnic entity and their gradual separation from the mainstream economy as it developed with greater emphasis on agriculture? Or do the small surviving groups represent the remnants of resistance to quasi-serfdom in a society of increasing complexity and economic hierarchy, and resistance to absorption into the larger and by then faster-growing Mongoloid population?

CONCLUDING REMARKS

From the foregoing discussion it should be clear that there is value in keeping a certain critical distance from any popular model or hypothesis, and in examining questions of radiocarbon chronology and ethnic history from fresh perspectives.

Returning to Rouse's criteria described above, we can conclude that no migration of Austronesians or any other ethnolinguistic group from South China to Taiwan to Luzon has been demonstrated. Shorn of its vigor and forward force (as a coherent pattern of migration), Bellwood's reconstruction appears almost as a series of piecemeal fortuitous migratory events which had the cumulative effect of propelling the wholesale advance of a specific people bearing a new economic system. It is, but is not quite, a migration. It lies below the detectable limits and is probably irrefutable, since regional variations in material cultures could *possibly* mask the movement of a people who were changing, adapting and innovating as they moved. I would maintain, however, that systematic agricultural expansion of the type hypothesized to have marked the spread of Austronesian speakers southwards through Taiwan and Luzon could reasonably and usually be expected to have left a trail of evidence more more dramatic than what we have.

Further, it appears to me that the main impetus for the expansion of the Austronesians should be sought in the dynamics of inter-island travel, fishing and trade of the Philippines and Indonesia rather than in the settled farming communities of the South China lowlands. The cultural rather than the population pressure, in the form of an urge to discover, explore and settle new territories *overseas* (that Bellwood and I agree constitutes a major factor in the spread of the Austronesians), is not and has never been an important characteristic of the peoples of the Chinese mainland. Chang's earlier views notwithstanding, it was not until the 17th century AD that Chinese farmers settled in numbers on Taiwan. Even the Bronze Age Yueh (1500-200 BC) who "made their homes on the waters" of the coasts and river deltas had virtually no contact with Taiwan, the Ryukyus, Okinawa or the Japanese islands (all 140-500 km distant).

But for the coastal Austronesians, adapted to an archipelagic environment for millennia, the cultural imperative to expand to ever more distant lands across the sea seems a great deal more logical. In the period 1500-200 BC they had pushed out some 5000 km east from Luzon into Micronesia and 6000 km southeast through Melanesia to Fiji and Samoa. Agriculture was obviously a useful adaptive tool, especially in maintaining settlements in alien environments, and once it was known to the peoples of Island Southeast Asia it seems to have spread quite rapidly. But is the rice agriculture of South China the ultimate source of the Austronesian expansion? I think not.

REFERENCES

- Chang, K.-C. 1964a. Introduction. *Asian Perspectives* VII(1-2):195-202.
 ——— 1964b. Prehistoric ceramic horizons in Southeast China and their extension into Formosa. *Asian Perspectives* VII(1-2):243-250.
 Bellwood, P. 1984-85. A hypothesis for Austronesian origins. *Asian Perspectives* 26(1):107-118.
 Headland, T.N. and Reid, L. 1989. Hunter-gatherers and their neighbors from prehistory to the present. *Current Anthropology* 30(1):43-66.

- Huang, S-C. 1974. Report on archaeological investigations at Pa-chia-ts'un, Kui-jen-hsiang, Tainan, Taiwan. *Bulletin of the Department of Archaeology and Anthropology* (National Taiwan University) 35-36:62-68. In Chinese.
- Meacham, W. 1984-85. On the improbability of Austronesian origins in South China. *Asian Perspectives* 26(1):89-106.
- Rouse, I. 1958. The inference of migration from anthropological evidence. In R.H. Thompson (ed.), *Migrations in New World Culture History*. Tucson: University of Arizona Press.
- Spriggs, M. 1989. The dating of the Island Southeast Asian Neolithic. *Antiquity* 63:587-613.
- Tsang Cheng-hwa 1989. On three major problems in the prehistory of Taiwan. *Bulletin of the Department of Archaeology and Anthropology* (National Taiwan University) 45:85-106. In Chinese.