

CHRONOLOGY, EVOLUTION AND DIFFUSION IN THE LATER SOUTHEAST ASIAN CULTURAL SEQUENCE: FURTHER COMMENTS

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I am most indebted to Dr Bayard for his careful and thoughtful review of my Mortimer Wheeler lecture and parts of our report on the excavation of Ban Na Di (Higham 1983, Higham and Kijngam 1984). It is reassuring to find that he and I are so close in our respective interpretations of the chronology and cultural implications of the prehistoric sequence in Northeast Thailand. Only those who have conducted a major excavation in the area know of the particular problems posed by the stratigraphy of sites where occupation above ground level seems to have been widespread, particularly when working under the unforgiving conditions of the monsoon. This, together with the richness of finds encountered, necessitates much effort in bringing the results to full publication. As he says, the first three volumes of our report fail to consider the sherds from occupation levels, but I am glad to advise that Mr B.A. Vincent's analysis of these, on the basis of over 1000 thin sections, is nearing completion and should be available early in 1988. Naturally, we regret that six years have elapsed since the excavations were completed, but the study is a very detailed one and *inter alia*, will follow White (1986) in dispensing with the term Om Kaeo to describe what is, in fact, a highly variable material involving at least half a dozen different fabrics.

In terms of the relationships between Ban Na Di and Ban Chiang, Dr Bayard's paper does not take into account White's detailed consideration of the Middle Period, which not only rejects the term Om Kaeo, but also proposes three phases for the Middle Period: MP VI, VII, and VIII (White 1986). I have given consideration to this matter in the text of my book on Southeast Asian prehistory (Higham, in press) and, rather than enter upon a detailed and lengthy repetition of what it contains, I refer the interested reader to it. The essence of the argument is that White has extended the Middle Period into three phases, the earliest of which (MP VI) corresponds to most if not all our level 7 material at Ban Na Di. The earliest mortuary evidence for iron, however, comes from Middle Period VII, which corresponds to our upper level 7 and early level 6. This documentation of the Ban Chiang sequence is the sort of advance which makes meaningful discussions possible. White and I are close to agreement over the relationships between the two sites.

I have nothing further to say on the subject of the radiocarbon dates from Non Nok Tha, nor for that matter, on other aspects of this important site until the final report is published. Therefore, I leave it to the reader to judge, on the basis of the available dates (Fig. 1) and the actual size, provenience and estimate of the reliability of the samples (which can be obtained by writing to Dr Bayard) whether I am alone in failing to recognise a pattern. Figure 2 shows the Ban Chiang dates set against their proveniences. A detailed consideration of these has been provided by White. When I say that bronze was present "by 1500 BC" I mean just that. I have no objection whatever to an earlier initial date, be it 1700 BC, 2000 BC or 3600 BC, provided that the supporting evidence is derived in a manner consistent with the site's stratigraphy, and is published along with the resultant chronological framework such that the sample sizes and proveniences can be assessed. Such an assessment has been completed by White (1986) for Ban Chiang, and her statement on

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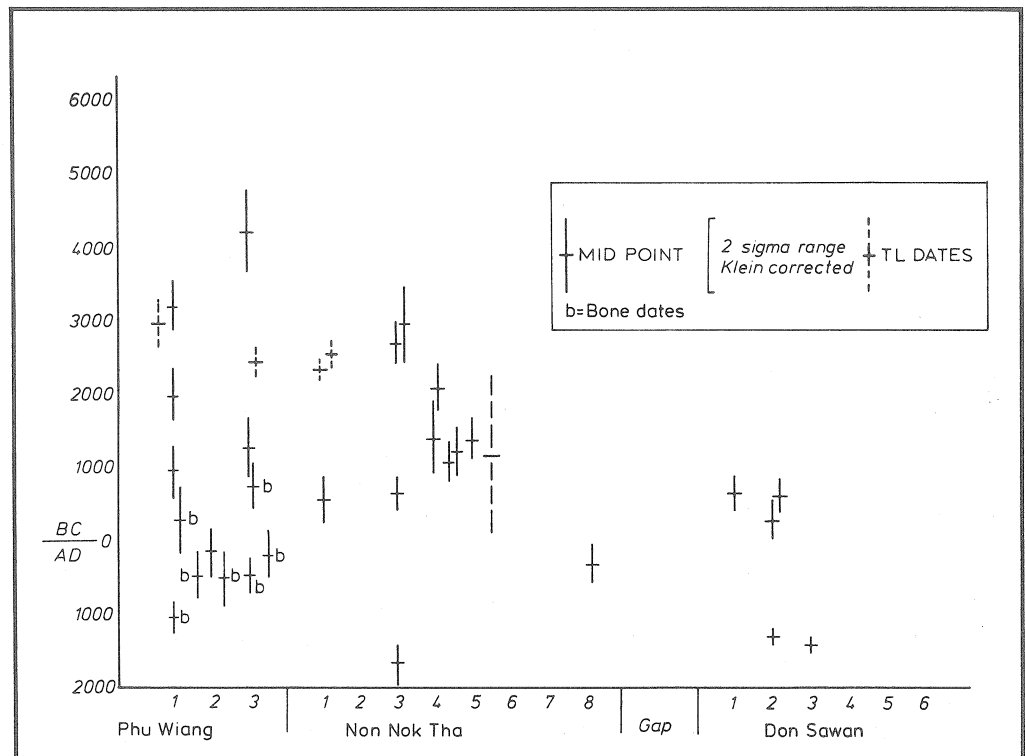


Figure 1: The distribution of dates from Non Nok Tha set against their provenience in the site.

early bronze is a model (if I may use such a word) of how to portray the situation:

"Since the spearpoint from EP III is such a crucial piece of evidence, it is unfortunate that the dating for this phase is so tentative and vague." (White 1986:289)

I am slightly mystified by Dr Bayard's allegation that because I did not adhere to my proposed criteria for the collection of samples at Non Dua, Bo Phan Khan, Don Taphan and Non Nong Chik (Higham 1977), his similar actions are somehow justifiable. Quite apart from the fact that two wrongs don't make a right, let me point out that I excavated Don Taphan single-handedly and collected all four radiocarbon samples from *in situ* contexts the results from which are perfectly consistent with the stratigraphy. Mr. R.H. Parker excavated the small test squares at Non Dua and Bo Phan Khan and took the samples. These reveal one stratigraphic inconsistency, but the dates are in harmony with the material culture between all three sites. At Non Nong Chik, I excavated one square and he the other. He took the samples after I was required to return to New Zealand. Let me conclude with the hope that the dates on human bone from Non Nok Tha currently under analysis in America finally resolve the chronology of this site, and that Dr Bayard's efforts to bring this issue to a close meet with success. If they disclose an Early Period III context well back in the third millennium, I shall be delighted and fascinated by the implications.

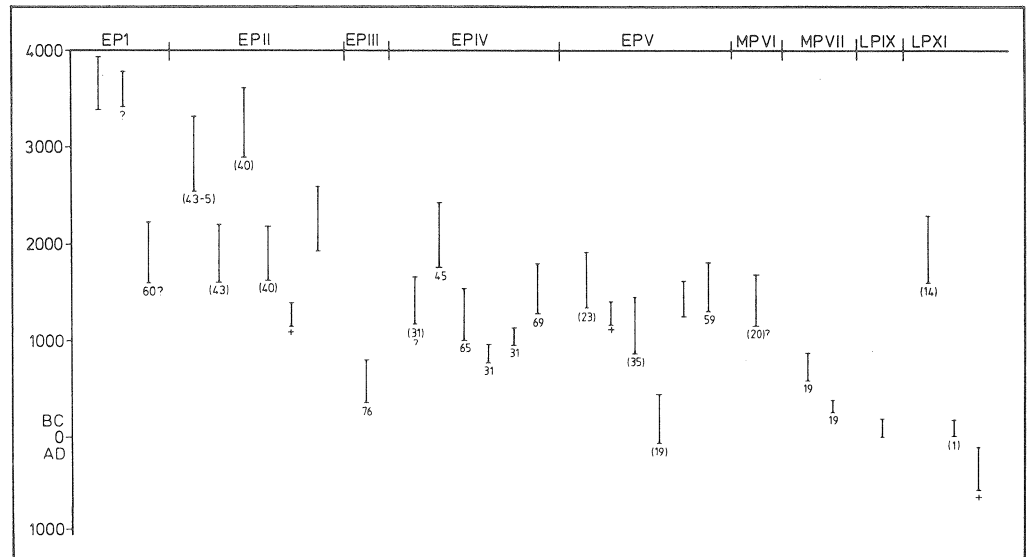


Figure 2: The Ban Chiang dates set against their provenience in the site. The ranges are 1σ . Numbers in brackets refer to burials from the 1974 excavations and others from the 1975 excavations. +: ascribed to this phase or later, and ?: a tentative ascription to this phase. The earliest bronze artefact from a mortuary context is burial 76.

I trust that the serious reader will take the opportunity to consult my Mortimer Wheeler lecture and read it in conjunction with Dr Bayard's comments. This should show that on several occasions, he has misunderstood my meaning. Within the constraints of a 50-minute lecture delivered to an audience with few Southeast Asian specialists, I found it convenient to use the terms Mode 1 and Mode 2 as a shorthand notation: the former as my lecture makes it clear, to describe autonomous communities which adopted bronze working, and the latter, to circumscribe the centralising societies manifested in Dong Son contexts in Viet Nam and large sites such as Non Chai in Northeast Thailand. To equate either with the precise characteristics of the Melian sequence, let alone claim I am applying a specifically Aegean model to a Thai situation, would be a serious misrepresentation of my words, which were only that "These *resemble* the entities recently identified on Melos". I subsequently devoted several pages to a review of the transition from *my* mode 1 to 2 on the basis of variables specific to Southeast Asia. I fear Dr Bayard has got rather carried away with his misunderstanding. Be it noted that when I wrote the conclusions on the excavations at Ban Na Di, freed from the constraints of a brief lecture, I did not employ the shorthand notation which guided my audience through the essence of a quite complicated explanatory just-so story.

One of the many issues which I felt would interest the audience, was the relationship between the Chinese and Southeast Asian bronze-working traditions. My words were as follows:

"The origins of South-East Asian bronze working are well beyond the scope of the present paper. Three points, however, must be made. The more conservative chronological framework promulgated in this paper brings bronze working in South-East Asia later than that in north China. At the same time, the associations and dating of the Gansu copper and bronze artefacts are not by any means secure. I am much indebted to Dr N. Barnard of the Australian National University for his pertinent comments on the dating of the material from the Majiayao, Machang, Qijia and Huoshaogou culture sites in Gansu. Finally, there are problems in isolating links, in terms of technology, between South-East Asian and north Chinese material." (Higham 1984:249).

On re-reading this passage, I cannot find any statement which need offend dedicated Southeast Asian scholars. During my recent visit to China, I had the opportunity of examining some of the above pieces and talking with Chinese colleagues. My strong conclusion was that the Southeast Asian tradition has no affinities with distant Gansu. Interestingly, however, artefacts similar to those in Southeast Asia are being found, as one has every right to expect, up the Mekong valley and well into Yunnan.

Again, I have never thought or, I hope, implied that bronze working leads necessarily or quickly to civilization or even centralisation. My words were directed against White's (1982) notion of a long and peaceful bronze age. They were:

"The shorter chronology proposed above dispenses with the need to explain the origins of metallurgy and its durability in simple village contexts. In its place, we can focus upon a particularly interesting change from small autonomous communities to a hierarchic settlement pattern incorporating central places."

Stech-Wheeler and Maddin (1986) have also taken issue with me on this point and in particular my description of the impact of the claims for fourth millennium BC bronze which were:

"The scientific community was invited to accept the indigenous development of bronze and iron working in inland Southeast Asia, in a context of unchanging, small-scale village communities. Such a juxtaposition, if validated, would run counter to any notion of even the most general regularities of culture process. *We shall address this issue at length.*" (Higham and Kijngam 1984:1)

I do wish that my critics had read carefully beyond page 1. Let me elaborate on the point I wished to make. It would be absurd to accuse me of concluding that metallurgy forces people onto the high road to civilization. At the time, I was arguing against the concept that for at least three millennia, the Ban Chiang material discloses a peaceful, unchanging bronze age. When I did address the issue at length, I argued that access to copper and tin, and their exchange far from their sources, were components of autonomous but competitive communities. More than one person has taken issue with this just-so story, so let me elaborate further. White (1982, 1986) has suggested that there was a long-term peaceful bronze age in small village communities. It sounds like a prehistoric Elysium. I argue against this on two grounds. Firstly, I seek evidence for the early third millennium bronze which Solheim is still proposing (Solheim 1984). Secondly, I consider the artefacts which were cast in bronze. These include spearheads and a wide range of projectile points. Does Dr White really feel that this precious metal was cast into spearheads to hunt deer when sharpened bamboo would do just as well if not better? Does she feel that the superb arrowheads from Ban Na Di were used lightly to kill porcupines? It should not be

overlooked that Pietruszewsky (1984) found that the skull of an adult male from Ban Chiang revealed a wound compatible with injury from a spear blow to the front of the head, and that a broken spearhead was found in the same grave. What of Bayard's discovery of extra heads in some of the Non Nok Tha burials? When we come to the Middle Period at Ban Chiang, is it possible that the bi-metallic spearheads were symbols of peace and harmony? How about the widespread interest among early ironworkers for the forging of spearheads? There is also the overwhelming evidence from Dong Son contexts for very vicious warfare and the taking of prisoners. What about the small but consistent body of evidence that some centralisation *did* occur within this period? Whether or not the bronze age lasted for 1500 or 3000 years, there are surely some grounds for suspecting that intercommunity raiding may have taken place. It is difficult, of course, to recover absolute evidence for friction in such contexts, but in my view, the use of bronze in display and conflict even if of a limited and ritual nature, is more compatible with the evidence than a peaceful and unchanging bronze age. To suggest that there might have been a similarly peaceful iron age reveals a limited appreciation of the data available.

If it is hard to be specific on the situation in Northeast Thailand, it is almost impossible in the case of Viet Nam. I agree with Dr Bayard that the interpretation of the data from the critical area of Bac Bo is fugitive. My own inclination is to see the many Phung Nguyen sites, most of which have not yielded any bronze despite the enormous areas excavated, as being predominantly third millennium BC and the latest horizons as being within the timespan 2000 to 1500 BC. The Dong Dau and Doc Chua material would then fall within the second half of the second millennium BC. In my Fig. 8 (Higham 1984:249), which has been the source of many of Dr Bayard's concerns, I put in stippled areas to represent transition zones. If I had the benefit of hindsight, I would have extended the transition zone for early bronze back in time to 2000 BC or thereabouts. I hope this satisfies Dr Bayard's anxieties on this point.

While it is true that White (1982) proposed a shortened chronology for iron working, it is also the case that the excavators of Ban Chiang had suggested a far earlier chronology and that White's resumé, published in a catalogue to a travelling exhibition rather than in a scholarly and documented paper, did not address the actual evidence in the same way that her doctoral dissertation does. Of course, the origins and chronology of early iron working in Southeast Asia remain unresolved, but I am more conservative about this than some colleagues. I am relaxed about a date as early as 500-400 BC, but become increasingly dubious the further back we proceed towards 1000 BC. If further evidence for the earlier context is forthcoming, I will examine it as best I can and if it seems reasonable, be happy to accept it.

This introduces one of the most surprising results of my lecture, the chorus of criticism at my criteria on the selection of radiocarbon samples. These guidelines are, in my view, nothing other than common sense. The fog which shrouds the Non Nok Tha and Ban Chiang chronologies, which, let it be remembered, were held to underpin the earliest evidence for bronze and iron working in the world, is the best demonstration of this point. At the same time, I do appreciate the difficulties encountered when trying to obtain a reasonable chronological framework for these sites, having encountered them myself on more than one occasion. At present, I am awaiting the determinations for the basal samples from Khok Phanom Di and those from the pollen cores with considerable trepidation, in the hope that they form a reasonable and coherent pattern.

Bayard's final concern relates to my use of the term "Ban Chiang Culture". I chose this as the title of my lecture after careful consideration, in view of my expectation that the majority of the audience would have heard of the site in the contexts of its beautiful pottery, claims for early metallurgy and, in the popular press, even for very early civilization. Viewed from Europe, Ban Chiang conjured up an image of something new, unexpected and controversial. The intention was

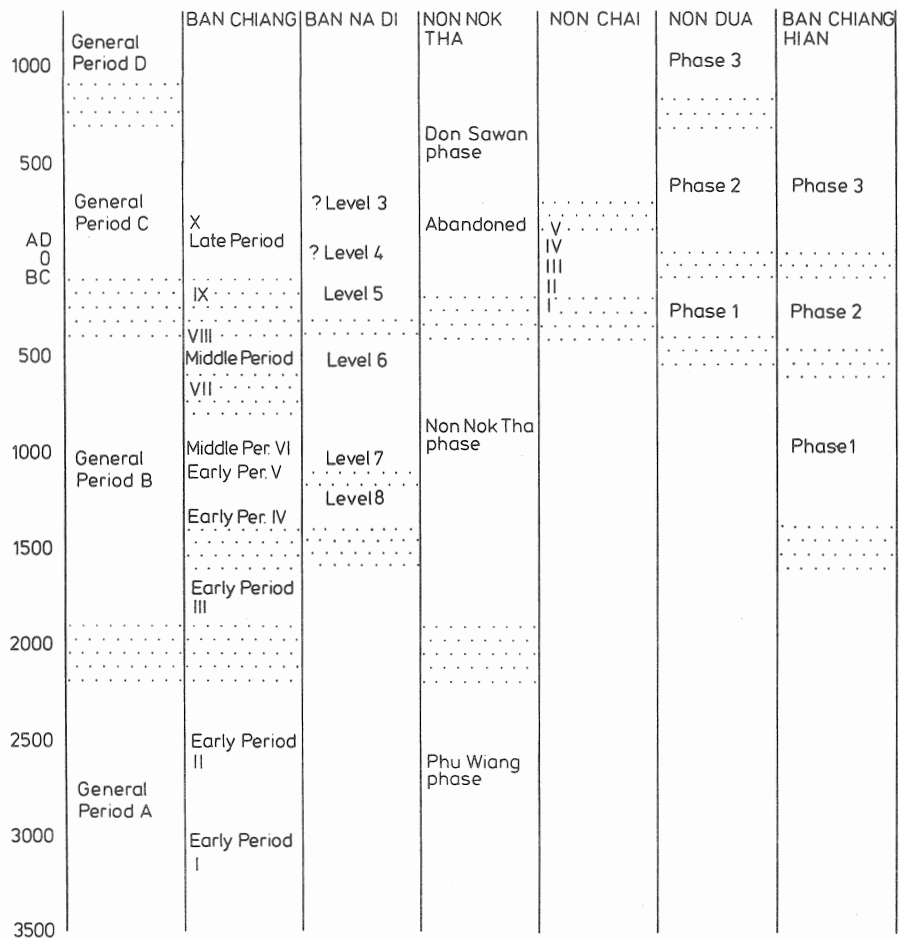


Figure 3: The suggested relationships between the sequences of the principal prehistoric sites in Northeast Thailand.

to present an alternative view based on a sober consideration of the available data.

I hope this helps clarify the concerns expressed by Dr Bayard. All these issues are covered in my forthcoming book which should be available in early 1988. One of its figures might help in relating the various sequences, so I include it here. I conclude by reiterating my thanks to Dr Bayard for taking so much trouble over my lecture and the Ban Na Di excavation report, and to express the hope that it will one day be possible to give the same detailed scrutiny to the results of his own excavations.

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