

CHRONOLOGY, EVOLUTION, AND DIFFUSION IN THE LATER SOUTHEAST ASIAN
CULTURAL SEQUENCE: SOME COMMENTS ON HIGHAM'S RECENT REVISION

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A decade ago Hutterer (1976) discussed at length some of the problems involved with traditional models as applied to the Southeast Asian cultural sequence. Such problems are to be expected in an area which until recently was an archaeological "terra incognita" (Higham 1972), lacking an established, generally accepted chronological framework and evidencing numerous "anomalies" in the form of very early dates for possible agriculture and metallurgy (Hutterer 1976:223-4). Claims for an indigenous and radically early development of bronzeworking in the region (Solheim 1968, 1972; Bayard 1970, 1979, 1980; Gorman and Pisit 1976) are perhaps the most notable of these anomalies. Unfortunately, after twenty years of fairly intensive research in Vietnam as well as Thailand, the controversy continues. The early dates for metalworking have recently been criticised by Loofs-Wissowa (1983), using primarily a traditional European model of social development; these have been answered elsewhere (Bayard and Pisit 1983). I am concerned here with the more up-to-date chronological and conceptual framework proposed by Higham in recent publications (Higham and Ampham 1982a, Higham, Ampham, and Manly 1982), and in particular in his recently published 1983 Mortimer Wheeler Lecture (Higham 1984a).

In this last paper, Higham offers: a) stringent criteria for the acceptability of radiocarbon samples; b) a revised chronology shortening the commencement dates for bronze and iron metallurgy in Thailand by over a millennium; c) the intimation that these technologies entered the region through diffusion from China; and d) a model for the development of complex societies in the region adopted from the Aegean Bronze Age. Although I am in agreement with many points in Higham's framework, I would like to offer some data which I believe argue against the last two of his points, and suggest that his downward revision of the dates is too extreme.

I think that the questions involved are ones with relevance beyond mainland Southeast Asia, touching as they do on the hoary question of diffusion versus independent invention, the problem of what constitutes an adequate chronology, and the more topical question of the processes involved in the development of state-level societies. Southeast Asia is obviously not the only area in the world burdened with such questions. As Higham emphasises (1984a:230), an agreed chronological framework is of crucial importance before we can begin considering more important matters of social evolution, and the derivation of models appropriate for the region as a whole. As he also correctly notes (1984a:256), what Southeast Asia needs is a

blend of both models and solid archaeological data, and it is my contention here that such models are better derived from the Southeast Asian data themselves or from general parallels elsewhere, rather than imported fully-formed from elsewhere with minimal modification or justification for their employment. It is my sincere hope that the arguments presented here will generate little heat, but at least some measure of light on the problems involved. Only through such constructive, detailed discourse will we arrive at a balanced view free from either European-oriented diffusionism or what now appear to be rather exaggerated claims for Southeast Asia as a sort of cradle of civilisation.

The chronological discussion which follows is necessarily rather tedious, answering as it must Higham's discussion (1984a:229-38); however, the questions are obviously crucial to the wider and much more important matters of social process in the region. I urge readers to examine both Higham's arguments and mine carefully and critically. New dates are arriving only very slowly (particularly from Vietnam), and it behoves us to devote considerable attention to the relatively small number available at present.

CHRONOLOGICAL MATTERS

1) Ban Na Di/Ban Chiang Hian. Higham's well-provenanced dates from the sites of Ban Na Di and Ban Chiang Hian indicate a fully developed and flourishing bronze technology in Northeast Thailand in the range 1300-1700 BC,⁽¹⁾ which is the one-sigma range of his earliest date for Ban Na Di. The bronze technology and ceramic typology of Ban Na Di have clear ties to the later part of White's Early Period at Ban Chiang, and in particular to the latest of the four subdivisions of that period, which White dates at 1500-1000 BC (1982:20; pers. comm.). The technology is also generally similar to that of the Non Nok Tha Middle Period, which I would date between 23/2500 and 500 BC. Unfortunately, no strict ceramic parallels are present with Non Nok Tha.

Ban Na Di produced none of the distinctive vessel types characteristic of the earlier Early Period at Ban Chiang, so the site provides no evidence for the origins or first appearance of bronze in the region; however, bronze was well developed there by ca. 1500 BC, and at Ban Chiang Hian at a somewhat later date (1175 BC on a sample 80cm above natural). Bronze is associated with the earlier types of ceramics at both Ban Chiang ("beakers"; see White 1982:2) and Non Nok Tha (rouletted ware; see below).

Higham dates Ban Na Di layer 6 to about 300 BC, and equates this level with the Ban Chiang Middle Period (formerly Phase IV), characterised by Om Kaeo ware. This "Om Kaeo" period at Ban Chiang is stated as equivalent to "terminal layer 7 and 6" at Ban Na Di in Higham's text (1984a:233), but is shown as fully developed only by mid-layer 6 in his Fig. 8. It must, however, be pointed out that the only ceramic data on body sherds from Ban Na Di published to date (Higham and Amphan 1982b) indicate that 75% of "Om Kaeo" (Ban Chiang

Middle Period) decorated body sherds were found in layer 7 rather than 6, which has only 14% by weight of the entire sample of Om Kaeo body sherds from the site (Higham and Amphan 1982b:1).

The sample of Om Kaeo body sherds recovered at Ban Na Di is of course a small one (less than 2kg), but the preponderance in upper layer 7 is certainly suggestive. These figures are only approximate, but data on body sherds are unfortunately absent from Higham and Amphan's massive final report on their researches (Higham and Amphan 1984). However, Om Kaeo rim sherds are clearly stated there to have been recovered in upper layer 7 contexts as well as from layer 6 (1984:231). Moreover, one of the few Om Kaeo-style vessels (clearly of White's Middle Period type) recovered from burials mentioned by Higham in his recent presentation at the Pacific Science Congress (Higham 1984b) was found in Burial 21, one of a series of three burials sealed by sand lenses in layer 7. A second vessel was recovered from Burial 18, also cut from uppermost layer 7. The Ban Chiang Middle Period is thus clearly represented in Ban Na Di upper layer 7. If the non-burial Om Kaeo sherds are in fact derived from disturbance of earlier burials in that layer, as seems likely given reuse of the same graves (Higham 1984b:74-75), the Ban Chiang Middle Period may equate more closely with mid- and upper layer 7 than with lower layer 6 at Ban Na Di: i.e., ca. 700 to 300 BC, and hence quite close to White's recent dating (1982) of 1000-400 BC (see Fig. 1).

Clay rollers, glass beads, and bronze bells at Ban Chiang are limited to the Late Period (White 1982:74-5). Glass beads are very common in Phase III and IV layers at Non Chai (Pisit and Bayard 1983; Bayard, Pisit and Somsuda 1986), three clay roller fragments were recovered from Phase IV, and fragments of moulds for casting bronze bells occur from Phase III upwards; bells themselves were recovered from Phases II and IV. Both Higham and I would date these phases at Non Chai to ca. 250-0 BC, again implying a date for the beginning of the Late Period at Ban Chiang which is quite close to White's 300 BC estimate. At Ban Na Di clay roller fragments were recovered in layers 3, 4, and 6 (Amphan 1984), suggesting that the Ban Chiang Late Period influence began sometime during layer 6. This may well have coincided with the abandonment of the Ban Na Di cemetery (Higham 1984a:244), and Higham's postulated transition to "Mode II" in the region ca. 300 BC (1984a:fig. 8).

Hence both Ban Na Di upper layer 6 and layer 5 are equivalent to the Ban Chiang Late Period, and the Middle Period influence at the site (lower layer 6 and upper layer 7) must have preceded ca. 250 BC (see Fig. 1). A recently published series of thermoluminescence dates on red-on-buff vessels characteristic of the Ban Chiang Late Period, although spanning a wide range from 1100 to 95 BC, has a mean date of ca. 500 BC $\pm 10\%$ (Mortlock and Price 1983), supporting White's estimate of 300 BC for the beginning of the period, rather than the 0 AD date given in Higham's Fig. 8. However, given the recurring problems with TL dating of Ban Chiang material (Carriveau and Harbottle 1983), it would be wise to deemphasise such dates, as I have said elsewhere (Bayard and Pisit 1983:13).

HIGHAM 1984 a. Fig. 8

SUGGESTED REVISION

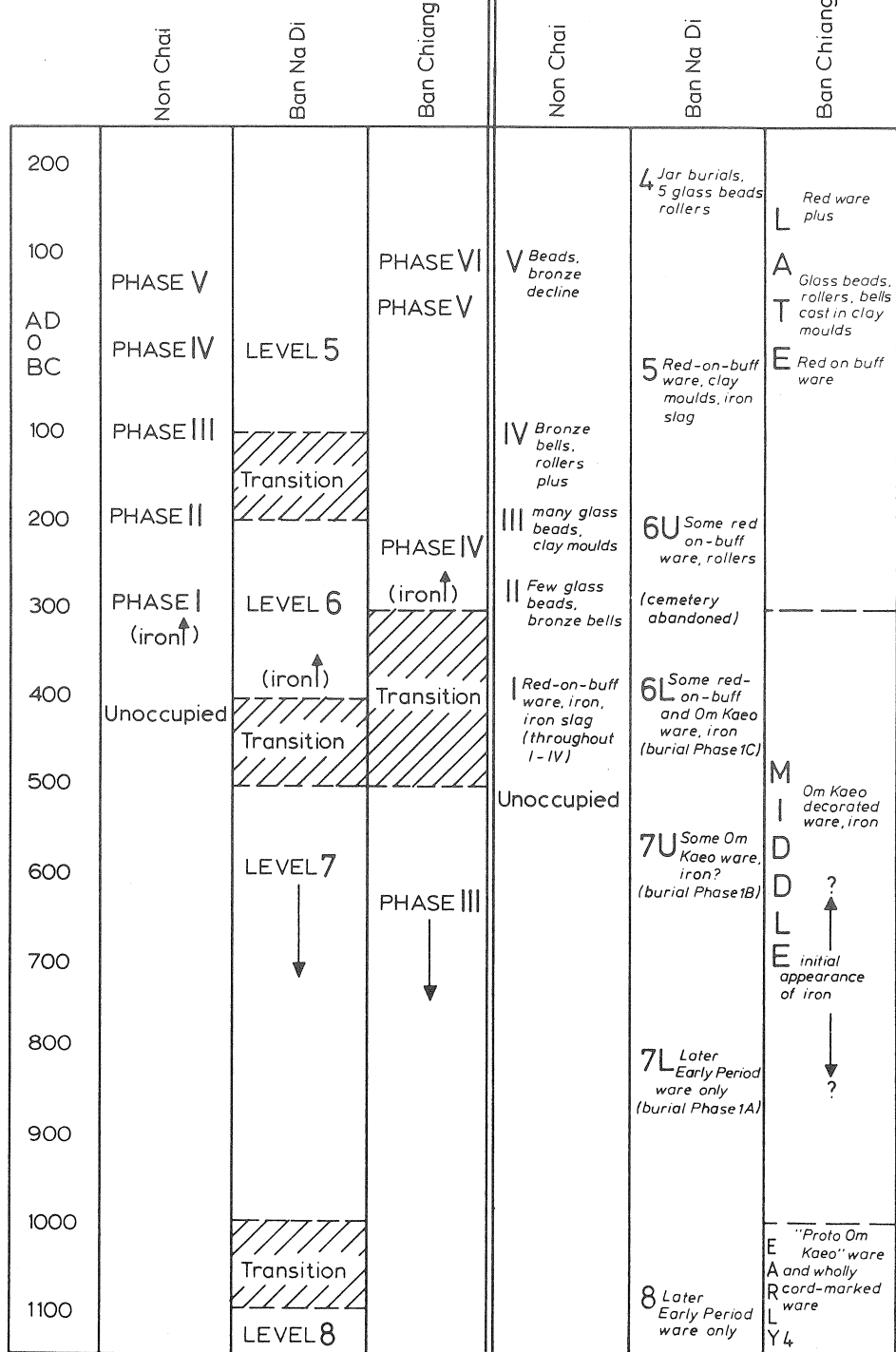


Figure 1. Sakon Nakhon - Khon Kaen chronology in the first millennium B.C. Left: as interpreted by Higham. Right: as suggested by ceramics and other artifacts.

2) Non Nok Tha/Ban Chiang. The ceramic sequence at Non Nok Tha (and at Ban Chiang, following White 1982) is quite secure. Vessel and burial typologies within each sequence are well-established, and parallels at similar absolute date ranges are clearly apparent; e.g., impressed/rouletted ware found in Ban Chiang Early Period 2 (dated by White at 2500-2000 BC) resembles two clearly imported vessels recovered from burials bracketing the Early/Middle Period transition at Non Nok Tha, dating at 2300-2500 BC in my chronology. One-third of the Ban Chiang dates (Hurst and Lawn 1984) range between 2000 and 4000 BC when corrected, as do nine of the 22 Early/Middle Period charcoal and thermoluminescence dates from Non Nok Tha which Higham (1984a) pictures in his Fig. 2 (ten additional dates are not shown). Higham's statement that "Eighteen samples from the mortuary phases have been dated" (1984a:232) is incorrect; the figure should be 30, including the four TL dates.

Higham is also in error in stating that "None [of the samples] for dating conforms with the stratigraphic principles" he sets out (1984a: loc. cit.); in fact 21 of the 37 samples satisfy his criteria and are stratigraphically secure (Fig. 2). The blame for the aberrant dates on these samples (in particular the seven bone samples from skeletons) cannot be placed on imprecise provenance, but lies elsewhere. ⁽²⁾

Even if some of the Non Nok Tha dates reflect upward displacement of charcoal (impossible in the case of the TL and bone dates) by the factors Higham mentions, I cannot help but conclude--despite Higham's refusal to believe the site was occupied prior to 2000 BC and his statement that "there is no relation between the dates and their stratigraphic contexts" (1984a:232-33)--that there is an overall linear pattern present which dates in general the earlier portions of both Non Nok Tha and Ban Chiang sequences. I would estimate this beginning at ca. 3000 BC for Non Nok Tha, and perhaps slightly earlier for Ban Chiang (Hurst and Lawn 1984). Aside from one very early date on a basal hearth at Ban Chiang (pre-5000 BC), there is simply no other earlier cultural manifestation at either site that could have provided the charcoal.

The single pre-5000 BC date from Non Nok Tha must be viewed as an artefact of processing rather than collection; this is of course also the case with the seven dates on skeletal material from Non Nok Tha. They and the three post-1000 BC dates from EP 1-3 and MP 1 are obviously rendered highly implausible by the ceramic typologies of White (1982) and Higham and his students (Higham and Ampham 1984). Put concisely, if vessels characteristic of Ban Chiang Early Period 2 (dated by White at 2500-2000 BC) occur only in EP 3-MP 1 contexts at Non Nok Tha; and vessels of Ban Chiang Early Period 1-3 are lacking in basal Ban Na Di, some 25km from Ban Chiang; then I think it is safe to assume that the early Middle Period at Non Nok Tha predates basal Ban Na Di. Applying Higham's criteria strictly, we must otherwise postulate from the EP dates at Non Nok Tha an arrival of bronze there ca. 500 AD, which would have displeased even

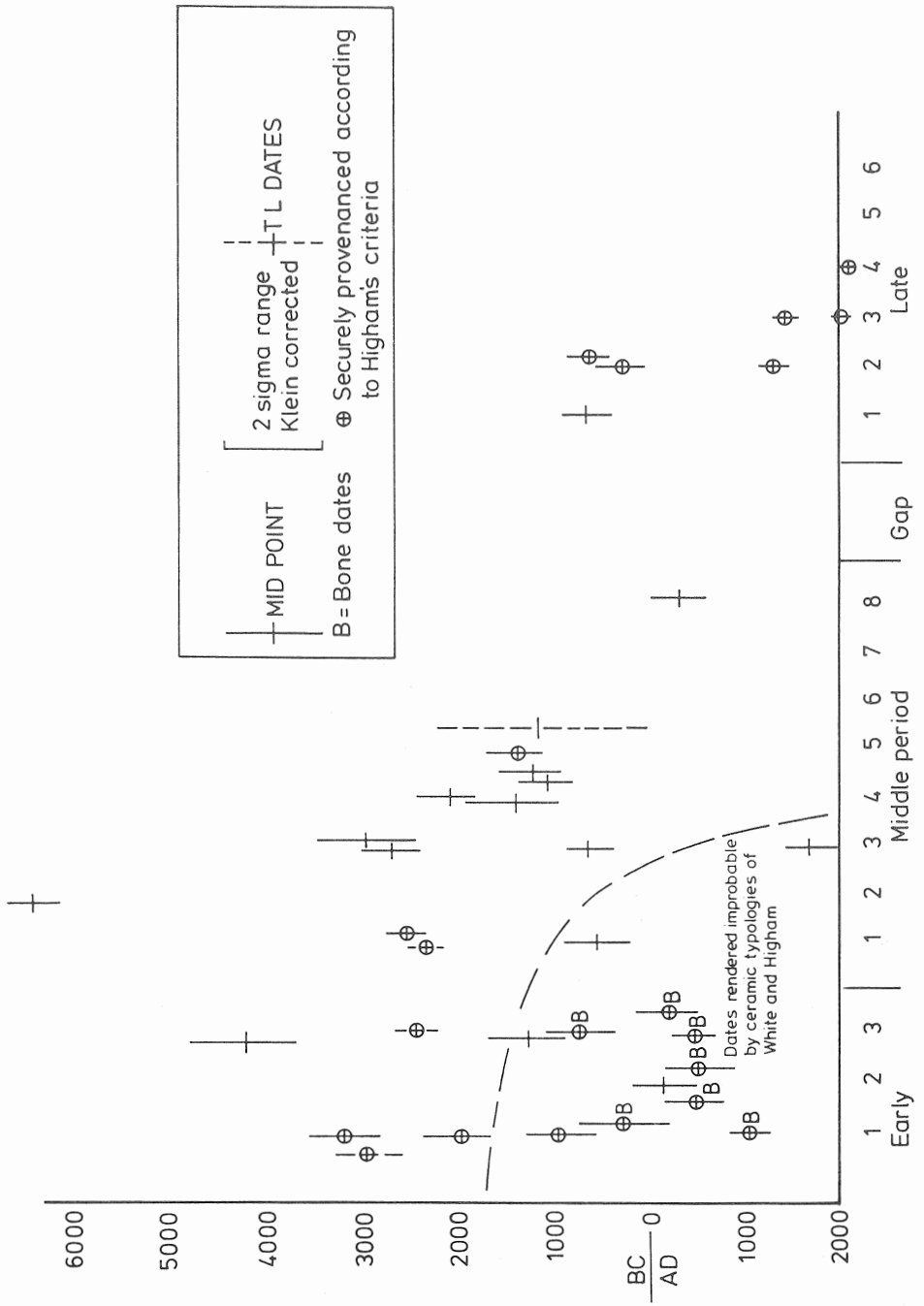


Figure 2. Non Nok Tha absolute dates (after Higham 1984a, Fig. 2).

Heine-Geldern! Higham's provenance criteria are of course ideal ones which we would all like to observe whenever possible, but as one commentator has remarked, "Many of Higham's fellow archaeologists however feel that his criteria for a valid date are unnecessarily strict" (Bronson 1985). In fact, Higham himself has collected samples for dating from grave fill and scatters in layers (Bayard 1977:88; Higham 1977:112), with predictable but perhaps unavoidable inversions and inconsistencies.

An earlier third millennium beginning for the Non Nok Tha sequence is reinforced by the internally consistent series of TL dates, three of which are on sand-tempered funerary vessels from graves containing bronze tools (rather than the grog-tempered sherds which have apparently caused so much trouble with the Ban Chiang TL dates). The marked cluster of mid-second millennium dates for the medial Middle Period also supports an early third millennium date for the earlier occupation of the site, as Smith has pointed out (1979:40-41). At neither site is charcoal present in quantities sufficient to have been caused by natural forest fires (in which case abundant charcoal would have been encountered in the sterile natural, which it definitely was not in the 50m² of sterile substratum excavated at Non Nok Tha); nor is substantial alluvial redeposition from such fires likely on a raised mound site. All these questions were in fact discussed in some detail in the only other article written (in 1973) on the chronology of metallurgy in Thailand (Bayard 1979); however, these arguments were not considered by Higham. As mentioned there, charcoal was rare in the earlier levels of both sites; hence our necessary reliance on small quantities. As stated above, Higham himself was forced to use similar samples to date his and Parker's excavations in the Phu Wiang and Roi Et regions (Higham and Parker 1970, Higham 1977), and seems to feel that his dates on the latter group of sites are correct.

It is also important to emphasise that the chronology of Ban Chiang has recently undergone some revision only briefly alluded to by Higham in the Wheeler lecture. During the late 1970s both Higham and I accepted the dates proposed by Gorman for bronze at or before 3000 BC and at least considered the possibility of iron from about 1600 BC. However, White's revision (1982) of the Ban Chiang chronology, based on a detailed typology of burial vessels, estimates that the first bronze tool found there dates from 2000 BC (or somewhat later) rather than the early third millennium, and suggests a more likely date for the introduction of iron in the range 1000-400 BC. This revised chronology was discussed at the Pacific Science Congress, and received general support from Higham and myself, save that he strongly questioned the introduction of iron before 500 BC (Bayard 1984b). However, four radiocarbon dates from Loei and Phimai, plus 11 TL dates from Phimai and Central Thailand (Carriveau 1978, Bronson and Han 1972), argue for a pre-500 BC date for iron, although I would place lesser emphasis on those TL dates with wide standard deviations. Hence the discrepancies Higham mentions, although still marked, are no longer "well over a thousand years." Fortunately, the range of debate has now narrowed very considerably,

although the questions of bronze before or after 2000 BC and iron before or after 500 BC are of course most crucial for considering the origins of metallurgy in the region, and the wider question of the applicability of diffusion models to explain social evolution there.

3) Vietnam. The six earlier dates from northern Vietnam which Higham presents (1984a:Fig. 5) strongly suggest a well-developed bronze technology there between 1300-1500 BC, with the two late Phung Nguyen dates implying metallurgy somewhat earlier; the additional date from Doc Chua in southern Vietnam indicates how widespread the technology was by the middle of the second millennium. However, as with Thailand, this tells us little about the origins of the technology, or how much further back in time it extends. Tan (1980:126) discusses three phases of the Phung Nguyen culture, only the latest of which is dated to Klein midpoints of 1650 and 1760 BC⁽⁹⁾ (not 1500 BC). During Higham's 1983 visit to Vietnam, Tan informed him that bronze fragments (but as yet no artefacts) occur in some 11 of the 56 Phung Nguyen sites excavated to date. This suggests to me that bronze metallurgy was practiced by at least late Phung Nguyen times, which Higham (1984a) shows as "pre-bronze" in his Fig. 8, and presumably appeared earlier in the span of the culture; unfortunately, no dates are yet available for the earlier two phases.

However, to gain some possible insight into the origins of bronzeworking in the region, I think it is necessary to consider all available Vietnamese dates. If there were in fact a well-dated series of clearly Neolithic cultures (and no bronze-using ones) present in Vietnam during the third millennium BC, the argument could be settled fairly conclusively in favour of Higham's choice of a mid-to early second millennium date. But an examination of all sixth to first millennium dates available to me reveals this is not the case (Fig. 3). Only six third millennium dates are available. Two of these, on a Paleolithic/Mesolithic cave site and an early coastal Neolithic site (Bln-1273 and Bln-1486 respectively), are clearly aberrant. Two of the remaining four apparently date a late manifestation of the Northern Coastal Neolithic complex of the fourth and fifth millennia (Bln-2089, Bln-2193). The remaining two dates appear to be associated with bronze. One, from Ma Dong on the upper margins of the Red delta, dates the Neolithic-Bronze transition to 2740 BC (Bln-1277; Kohl and Quitta 1978:393; Klein corr.). A second from Hang Gon 9 in the south (Saurin 1968) dates a bronze-using site very similar to Doc Chua to 2450 BC, although as I have said this date is "obviously not of the highest quality" (Bayard 1979:31), and should be given minimal weight.

I think it is clear that at present Vietnam affords as little evidence for a Neolithic as for a bronze phase during the third millennium. Several Vietnamese archaeologists (including Huyen, quoted by Higham for the date of arrival of iron there) have placed the beginning of the middle Bronze Age in northern Vietnam at 1800 bc (ca. 2200 BC; Pham Minh Huyen and Diep Dinh Hoa 1981:33); a similar estimate of 2000 bc is also made by Hoang Xuan Chinh and Bui Van Tien (1980:63), while Huyen has recently opted for a late third

millennium date for the beginning of metallurgy (Pham Minh Huyen 1984). Taking into account Tan's already quoted remarks on the presence of bronze fragments in a number of Phung Nguyen sites, I think the weight of the admittedly scanty evidence at present is on the side of a third millennium date for the first bronze in Vietnam, particularly when we consider that all of the second millennium dates from the north, centre and south of the country are associated with bronze, excepting a clearly aberrant date from Cai Beo (Bln-1437) and the two dates from Ben Do, a site which is apparently contemporary with Doc Chua, and hence probably bronze-period as well (Fontaine and Davidson 1980:94). The same holds true for Thailand, where the only second millennium dates from "Neolithic" sites are two TL dates from Khok Charoen (Loofs and Watson 1972), and the series from the presumably Neolithic lower occupation levels of Ban Kao (cf. Bayard and Parker 1976). If bronze is a mid-second millennium introduction into Northeast Thailand, we would expect pre-metal sites dating from its earlier half; none are apparent. In fact, Higham has since clarified his position, and accepts a date of between 1500 and 2000 BC for the appearance of bronze in the region (Higham 1984c:3).

I believe as well that the overall pattern of the first millennium dates similarly suggests a first appearance of iron somewhat earlier than the 400-500 BC Higham supports. Sa Huynh urnfield sites, with numerous iron artefacts, have produced five dates ranging from 100 to 750 BC; two of these are pre-500 BC (Fontaine and Davidson 1980:95). Six of the ten available Dong Son dates are also pre-500 BC, although the association of these with iron is doubtful (Fig. 2). My feeling at present is thus that iron was fairly widely distributed in Vietnam by ca. 600 BC, and possibly also in Yunnan, where seven dates ranging from 450 to 650 BC suggest a greater antiquity than previously supposed for some Dian sites (Barnard 1980:36-38). These dates are somewhat too early for the knowledge of iron to be an import from one of the northern Zhan-guo states, although I recognise that this view is certainly open to question.

The great problem that we face in trying to put dates on the Vietnamese developments is simply the lack of dates, and the lack of detailed information about their provenances. The years since 1960 have seen some 3000m² of site area excavated in Thailand (excluding the Peninsula); these excavations are supported by approximately 190 radiocarbon and 35 thermoluminescence dates, most of which have at least some detailed provenance data. It is hard to make an estimate of the amount of excavation carried out in Vietnam during the incredible efflorescence of archaeology there during and after the Second Indochinese War; I would guess it to be in the vicinity of 8000m². The type site of Phung Nguyen alone involved almost 4000m², more than the entire area excavated in Thailand). Yet as of this writing we have only 78 Vietnamese radiocarbon dates available (not counting duplicate runs on the same sample) covering the entire span from Upper Paleolithic to Medieval, in most cases provenanced only by depth and cultural affiliation (the coffin dates are obvious exceptions).

For example, the ZK-310 date cited by Higham (1984a:256) has as its sole provenance "charcoal from burial" (Barnard 1980:145). The provenance cited by Higham for this sample ("stone-bordered grave pit") in fact supplied Bln-1324 and an obviously erroneous date of 1002 AD (Kohl and Quitta 1978:395). Hence I am puzzled at Higham's rejection of the Non Nok Tha samples (1984a:233, fn.1) on the one hand, and his heavy reliance on the 12 Vietnamese dates on the other. Although he has stated that if he were "persuaded that any date, be it Vietnamese or Thai, were based on unprovenanced charcoal, I would view it with extreme caution" (1984c:3), the fact remains that such provenances as "charcoal from burial" or "stone-lined grave pit" are what they say they are: no better in quality than some of those Higham rejects from Non Nok Tha.

4) China. Higham's intimation (and it is only that) that Southeast Asian bronze technology may derive from distant Gansu is difficult to support at present; as he points out (1984a:248, fn.1), no technological parallels are known, nor are there any chronologically or technologically intermediate sites along either the east Chinese coast or in the Red River Basin. Moreover, the bronze technology of South China and the Southeast Asian mainland "had no demonstrable metallurgical tie-ups northwards in China until late Chan-Kuo and Han" (N. Barnard, pers. comm.); i.e., post-300 BC. The lack of a developmental sequence for bronze in the region is also not a strong argument for an outside introduction. One must remember how long it took for a developmental phase to be discovered and ultimately accepted for North China, and for how long similar arguments were adduced to demonstrate that Chinese metallurgy was Western in origin.

I also believe that there are problems with Higham's suggestion that open-fired ceramic technology argues against an indigenous origin for bronze in the region (1984a:248). One study has indicated that at least some of the Non Nok Tha Early Period vessels were fired at temperatures up to 975°C., higher than those usually encountered in open firing (Meacham and Solheim 1979:115). Vincent's more recent research on the Ban Na Di layer 6-7 ceramics suggests even higher temperatures in the range 1000-1100° C. (in Higham and Amphan 1984:674). These temperatures are more than ample for reduction of at least the carbonate ores of copper, and approach the melting point of pure copper (1083°C.; Coghlan 1975:28-29). Bronze and tin of course have lower melting points (ca. 1000° and 232° respectively).

I have similar reservations about Higham's derivation of iron from "China" (Yue? Wu? Chu? Shu? Ba?), where it was an exceedingly scarce commodity from mid-Chun-qiū times until the beginning of the Zhan-quo period; ironworking "techniques probably were not perfected and widely used until the fifth century [BC], as far as archaeological evidence is concerned" (Chang 1977:352). Barnard's arguments quoted above suggest that there was not a single centre of origin for East Asian iron; Bronson's recent studies (1985) provide an even more convincing case for the discreteness of the Chinese and

Southeast Asian technologies, and he points out that there are now some 15 radiocarbon dates associated with iron in Thailand in the first half of the first millennium BC (in addition to 27 in the second half of the millennium).

Finally, it must also be remembered just how little excavation has been carried out in the Southeast Asian region as a whole. The recent work of Higham and his students has added immensely to our knowledge of the northern portion of Northeast Thailand; yet this has arisen from only one area excavation of 65m² (Ban Na Di) and a series of test pits ranging from 1 to 9m², totalling about 22m². There is thus the possibility that a representative sample has not been obtained at some or all of these sites; had excavations of the same size or even somewhat larger than Ban Na Di been carried out on the southwestern half of the 1966 area of Non Nok Tha (ca. 80m²), or over the southwestern third of the 1968 area (68m²), not a single bronze artefact would have been found in the mortuary contexts Higham deems necessary (1984a:231, fn.2). Moreover, only one of Higham's test excavations (Non Kao Noi, undated) has produced pottery comparable to that from the earlier levels of Ban Chiang and Non Nok Tha (which together total an excavated 555m²). Hence I feel that Higham's wish to revise so markedly the sequences of the two sites is premature pending excavation of more sites in Northeast Thailand and many more dates from Vietnam. I think that we presently have two alternatives: either we consider--albeit with qualifications--all reasonable Vietnamese dates, along with those from Non Nok Tha, Ban Chiang, and other Thai sites; or we reject them in toto and accept Higham's 17 consistent dates on 87m² of excavation as our only reliable evidence from the region. Obviously, I prefer the former course, as do others (Bronson 1985).

ABOUT MODELS

I have argued elsewhere (1987) that what Higham, other workers, and I have referred to as "models" are in fact not such, but rather scenarios or "just-so stories". Here I will use model in its imprecise sense as a generalising device or account of an ethnographically or archaeologically parallel situation which attempts to suggest some measure of explanation for the archaeological data at hand. We are far indeed from the point where our data will allow the construction of models in the strict sense.

Higham's model of late prehistoric cultural change stresses the importance of agricultural intensification (i.e., fixed fields and the plough) for the development of complex societies in both Northeast Thailand and the Red River valley, and I agree that it is likely that intensification played some part (Bayard 1980:105; 1984c:163-64). I also concur wholeheartedly (1980:106) with the emphasis he places on the existence of interlinked exchange networks predating such intensification; Kennedy pointed this out some years ago (1977). However, I must defer agreement on three fairly important points: a) the implication that the chronology must be shortened because metallurgy itself inevitably leads to an increase

in social complexity (Higham 1984a:238); b) the applicability of a model borrowed without explanation from the Aegean (i.e., Renfrew 1982); and c) the expectation that the transition to statehood was latent in the indigenous populations of the region, and took place only because of contact with Indians and Chinese (Higham 1984a:255).

A. Concerning the first point, I certainly agree with Higham that the introduction of bronzeworking (but not the mere presence of the technology itself) had some impact on social complexity; the study of the Non Nok Tha burials (Bayard 1984a) which he cites demonstrates this fairly clearly. Moreover, Non Nok Tha also features a number of headless burials, indicating some degree of conflict (but not to my mind organised warfare), and thus arguing against White's "peaceful agrarian village societies" (1982:48). But must we shorten the span of the bronze period only because the presence of the metal is supposed to lead relatively quickly to states paralleling Shang China or the Aegean polities (Higham 1984a:238)? I think not. The transition from early bronzeworking in North China (Erlitou) to Anyang spanned almost a millennium; the "second radiocarbon revolution" has indicated that two thousand years separated the Balkan Copper Age from the early Minoan polities, and strongly suggests an independent origin for the former (Renfrew 1969, 1970). If diffusionist arguments have proved unsatisfactory for Europe, I can see no need to reintroduce them in Southeast Asia pending concrete evidence for the source of such diffusion. Moreover, metallurgy seems to have taken over 2000 years to spread from eastern to western Europe (Renfrew 1970:307). I feel certain that Higham will agree with me that metallurgy alone is not sufficient to cause a "race to literate civilisation" (Clark 1977:345); if it was, then the race was run very slowly in Europe. As Gilman has said of the latter region, "It would not seem, however, that copper and bronze played a significant role in maintaining the economic and social security of households. Accordingly, it is hard to accept that it called the elite into being. It is better to see metal as an index than as a cause of social stratification in Europe" (1981:5). I would argue that the same holds true for Southeast Asia.

B. Concerning the second point, I think we must also question the specific applicability to mainland Southeast Asia of a model intended to clarify the processes of state formation on an Aegean island of some 151km², or the Cyclades in general; to what extent are Dian or the Dong Son complex comparable to a Greek polis; are they polities "of comparable scale" in Renfrew's terms (1982:264)? Is our documentation adequate to permit the importation of the "Modes" model without modification, qualification, and most importantly explanation of its suitability? How does the "Modes" terminology alone increase our understanding of Southeast Asian political entities? Just how do the "Modes" "resemble the entities recently identified on Melos and described in similar terms by Renfrew" (Higham 1984a:248)? Renfrew equates "Mode II" with "state polity" (loc. cit.), but as Wilen has pointed out (1984, 1986), a two-tiered hierarchy of "primate" and "ordinary" sites is at odds with most such models, which are based on three or four tiers.

The major problem to my mind with Higham's use of Renfrew's model is that Renfrew is applying it to "state formation in microcosm" (1982:289), in the context of a wider Aegean oikoumene based on shared belief systems and maritime-orientated trade networks; these are archaeologically well-documented and dated. In contrast, we lack any concrete data on the beliefs and political systems of any of the pre-Chinese or Indianised entities in Southeast Asia, and can rely only on incomplete and often ethnocentric Chinese accounts for the period after the onset of such influences. Indeed, the whole question of whether or not true cities were developed prior to or only because of these influences is still open to debate (Davidson 1979). My own preference at present is for a model similar to the historic muang or mŏng of the Shan: a series of administrative centres or hua muang exerting control over a varying number of peripheral villages, the number depending on the waxing and waning of the chao ("lord") and his administrative forces. I have developed this idea more fully elsewhere (1987). The inland-orientated system of Dian/Dong Son, although very probably involved in the specialised production of bronze luxury items which were disseminated throughout island Southeast Asia by maritime trade, shows little evidence in its abundant plastic arts of a maritime (as opposed to riverine) emphasis. Such a system, and those developing in Northeast Thailand at about the same time, would seem to have some significant differences from the marine-focussed oligarchic polities of the Aegean.

Renfrew's processes of intensification, interaction, and exploitation are of course more general phenomena in the formation of complex societies, and doubtless played as important a part in Southeast Asia as elsewhere (Renfrew, loc. cit.). However, supporting evidence from our region is of variable quality and quantity. Intensification of agricultural production seems very likely for Northeast Thailand as a result of Higham's innovative studies (Higham and Amphan 1979, Higham et al. 1980), and almost certainly occurred in northern Vietnam as well. As mentioned above, interaction in the form of expanding and very probably formalised regional trade networks was also present and important (cf. Kennedy 1977). However, with Renfrew's third process, exploitation, we are on somewhat shakier ground, at least as far as Northeast Thailand is concerned. Dian and Dong Son art certainly hint very strongly at the presence of rigidly stratified societies with the elites firmly in control, but little or no evidence of this sort comes to mind for Thailand; there are no mortuary data suggestive of a rigid division between a small elite rank and a much larger body of commoners. Of course, as we only have a sample of at most 500 burials in toto to work from, such evidence may yet come to light. But I do not feel that the unqualified borrowing of the "Modes" model has much to contribute to our region, and am pleased to see a deemphasis of the term in Higham's most recent work (Higham and Amphan 1984).

C. My third question deals with the nature of the shift from autonomous village to state (or "Mode I to Mode II" in Renfrew's terms). Higham posits the possibility that this transition "was latent in indigenous societies" (1984a:255; 1984c:4), and arose as a result of gaining preferred access to the products of expansive states--China in the case of the Red River valley, and India in the case of Thailand. This was almost certainly a factor in the later development of states in the region, but I believe that Higham's own model, calling for a beginning of such a transition ca. 500-400 BC, argues against external influence as a major cause. Dates for Dian and Dong Son now extend back into the sixth and even seventh century BC (Barnard 1980; also see Fig. 3), with no real sign of any influence from the pre-Chinese states of Chun-qiū times. Similarly, in Thailand we have what appear to be fairly large communities like Chan Sen I and Non Chai I-IV in existence before any definite signs of Indian contact.

In any event, population pressure and restricted agricultural land, put forth by Higham as two probable causal factors in the rise of social complexity in Northeast Thailand (1984a:254), would now not seem to be of primary importance. I have argued elsewhere against population growth as a cause of agricultural intensification (1987); this has been supported by Welch's research (1985), which makes a convincing case against population growth and scarcity of agricultural land as factors leading to more intensive agriculture. While I feel that Higham is almost certainly correct in the importance he places on the strengthening of pre-existing trade networks and agricultural intensification, my own view is that changes to more centralised political systems were taking place in both Thailand and Vietnam before significant influences from now-unified India and China entered the region (it must be remembered that such unification under Aśoka and Qin Shihuangdi did not take place until the third century BC). In my interpretation, these influences bolstered and encouraged the further development of a system which was already established.

A rough parallel to this two-step process probably exists in the transition of Dong Son from an independent state (or states?) to Chinese domination (i.e., "Mode II" to "Mode III"), which similarly was not a one-step process. Chinese suzerainty was first vaguely established under Zhao Tuo and his kingdom of Nam Viet ca. 208 BC, but Chinese control was not instituted until a century later, under Han Wudi, and direct administration followed only in 43 AD, after the unsuccessful revolt of the Trung sisters (Hall 1964:183-4). The whole process thus took 250 years, and was by no means as abrupt as the Athenian takeover of Melos so dramatically documented by Thucydides (Renfrew and Wagstaff 1982:319-22; one might add that Southeast Asia sadly lacks a Chinese or Indian Thucydides!). Hence I wonder if the inter-mode transition is always a rapid, one-step process as implied by Higham; suzerainty would appear to represent an intermediate step or series of steps ranging from almost complete domination by an external empire to the type of vassalage to China found in Southeast Asia in the last century: even under Rama III

(1824-1851) Siam was still sending triennial tribute missions to China (Cady 1964:331), but surely could not be considered to have a Mode III political organisation.

My concern is not about the importation of European models per se; any model of heuristic value is worthwhile considering--indeed, the more different ones the better. But should we then expect a close fit between any one such model and the Southeast Asian data? Obviously no single explanatory model will subsume or explain the entire Southeast Asian sequence (Hutterer 1976). If, after 200 years of archaeological investigation, the processes leading to the rise of complex societies in Europe are still very much a matter of debate (Gilman 1981), can we expect a clear-cut explanation at present in Southeast Asia? Certainly the factors that Higham has delineated are crucial and important ones, but much more research is necessary before positive answers can be arrived at. Higham has aptly stated that what is required is "a blend of solid archaeology and the generation of explanatory models" (1984:256); but I suspect our final models will be considerably more complex than the admittedly preliminary one put forth by Higham, and will be arrived at more by internal generation of fairly precise values for the variables we wish to test (e.g. Welch 1985), rather than by wholesale importation of more or less likely scenarios from elsewhere. Once our data reach the abundance of those from Melos or Mesoamerica, we can begin to adapt and apply complex models like those of Renfrew and Steponaitis (1981) in depth, and here I think Welch's work marks a step in the right direction.

Finally, I think we should turn briefly to the question of what is meant by the "Ban Chiang culture." The title of Higham's address suggests that it encompasses the whole of the Ban Chiang and Ban Na Di sequences, but Higham is explicit in his belief that the famous red-on-buff ware, and the abandonment of the Ban Na Di cemetery, are the results of an intrusion from the Chi Valley to the south. But just what is to be considered the archetypal Ban Chiang culture: the Early/Middle Period (Ban Na Di layers 8 to lower 6) or the Late Period (Ban Na Di upper layer 6 to 4)? At the Pacific Science Congress I put forth a tentative regional chronology for Northeast Thailand based on phases (Bayard 1984c). This has the obvious heuristic advantages of avoiding ambiguity in such cases; it also has equally obvious disadvantages, such as de-emphasising cultural continuity and increasing the terminological burden for non-specialists in the area. But it does seem clear that cultural boundaries were shifting and unstable, particularly during the crucial first millennium BC (and even earlier, as witnessed by the Early/Middle Period transition at Non Nok Tha). The degree to which these shifts represent trade and influence (cf. Renfrew's conclusion that states legitimise themselves by the existence of similar states; 1982:289) or are the results of direct takeovers remains to be established by future research. However, there is at present little evidence for organised militarism in Northeast Thailand (in marked contrast to northern Vietnam); the stray finds of ge and other Chinese-inspired weapons may well reflect trade rather than organised

conflict. In any event, it is apparent that the presence of military takeovers in Northeast Thailand during the period 300 BC-500 AD, although likely, will require further documentation.

CONCLUSIONS

In the course of this review I hope to have demonstrated the following points, ones which I believe are crucial to our understanding and explanation of events in mainland Southeast Asia before and during the period when complex societies were developing.

1) The evidence at present is as insufficient to support a date of 1500 BC for the initial appearance of bronzeworking as it is for 3000 BC; the actual date lies somewhere within that range. Higham has recently (1984c:3) indicated his willingness to accept a date in the vicinity of 1500-2000 BC; I continue to opt for a range some 500 years earlier.

2) The evidence for the initial appearance of iron supports a date about one to three centuries prior to 500 BC, although it now seems clear that Higham and White (1982) are correct in discarding the 1600 BC date for iron at Ban Chiang proposed by Gorman and Pisit (1976).

3) As a corollary to 1) and 2), the derivation of developed bronze- and ironworking technologies from those areas later to become the Qin Empire is unlikely at present; this does not, of course, rule out some sort of stimulus diffusion.

4) Bronze metallurgy in itself is neither a necessary nor sufficient cause for a transition to complex chiefdoms or state-level modes of organisation. Catching the "hare" of three millennia of stable bronze-using village societies (Higham 1984a:238) only frees another hare: bronze as a sole or at least primary cause of state formation, particularly in view of at least a millennium of bronze use in an autonomous village context prior to 500 BC.

5) While general parallels are present between Southeast Asia and the Aegean, they are no more marked than parallels of the former to other regions of state formation. Ethnoarchaeological or historical comparisons with Britain and Imperial Rome (Higham 1984a:253) are perhaps better eschewed in favour of either locally developed models (Hutterer 1976, Kennedy 1977) or models of more universal application (Higham, Ampham and Manly 1982; Steponaitis 1981).

6) Finally, Higham's address opens the whole question of the nature and extent of the Ban Chiang culture. His analysis seems to limit it to ca. 2000-300 BC. Were the inhabitants of Late Period Ban Chiang conquerors from the Chi Valley, or just local elaborators of Chi innovations? My view at present is that the region is best served by an areal phase chronology which can emphasise cultural

parallels without necessarily implying cultural priorities or dominance (Bayard 1984c).

In short, even after two decades of intensive research in the region, the data are still inadequate to provide conclusive answers on the origins of metal and complex societies. Nevertheless, due in significant part to Higham's prolific research of the past five years as well as to the efforts of many others within and outside Southeast Asia itself, a broad but still fuzzy outline of the region's prehistory has emerged. The clarification of this picture during the next decade will require open minds, the analysis of all available data, and emphasis on particularistic as well as general questions of culture process.

NOTES:

(1) BC dates have Klein corrections applied; bc dates are uncorrected.

(2) As must be the case with four of Higham's Ban Na Di dates (Higham 1984a:233, fn.2). Full provenance data for the Non Nok Tha dates will of course appear in the final site report, and in the interim I will be happy to supply copies of these data to interested readers.

(3) A third date for late Phung Nguyen of 1108 BC (Bln-1409) is unacceptable to the excavators, Higham, and myself.

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