KEYWORDS: *Lower Palaeolithic - Navigation - Watercraft - Sunda Islands - Australia - Epistemology*

The origins of navigation and language

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This paper is dedicated to the memory of Theodor Verhoeven, one of the least recognised among the greatest scholars in Pacific rim archaeology. His achievements were consistently ignored for four decades.

Abstract. The two competing models of cognitive evolution of hominids, the gradualist and the explosion models, are engaged in this paper. After focusing on east Asian findings, particularly of recent years, the paper presents previous data from Wallacea that have so far been widely neglected. The ability of marine navigation is demonstrated from the end of the Early Pleistocene, through the evidence of the occupation of Flores, Indonesia, by *Homo erectus*. This and cultural as well as technological evidence is then used to infer language origins in the Early Pleistocene, between 1.8 and 0.8 million years ago. The paper also reports a current project of replicative archaeology, intended to illuminate the navigational capabilities of Pleistocene people in Wallacea. It concludes with the proposition that hominid cognitive and cultural evolution during the Early and Middle Pleistocene have been significantly misinterpreted, and thus validates the gradualist paradigm.

Preamble

Recent discussions of the processes that may have led to the rapid sophistication of verbal communication among hominids have often referred to a model of Davidson and Noble (1989), according to which the earliest archaeological evidence for language is figurative depiction in graphic art. Their hypothesis is that iconic depiction was essential to convey the meaning of words between individuals, and since we have no credible iconic graphic art before around 32 000 years BP, 'reflective language' can only have appeared very recently in hominid evolution.

In 1992 I published a critique of this model, based primarily on what I contended to be significant logical and empirical shortcomings in its premises (Bednarik 1992a). For instance, I pointed out the considerable discrepancy between the first landfall in Australia, perhaps 60 000 years ago and by presumably Middle Palaeolithic seafarers from South-east Asia, and the very much later appearance of iconic art. Davidson and Noble accepted that the initial colonisation of Australia must have involved the use of language, in fact they went as far as to assert that this event *is* the first archaeological evidence of 'modern' human behaviour, including language use (Davidson and Noble 1992).

Here I will examine this proposition and test it. The purpose of this paper is not as ambitious as may be implicit in its title: it is merely to examine the specific link between ocean navigation and human language origins. Initially it is useful to emphasise that there is no fundamental disagreement on one crucial question: the ability to cross the open ocean successfully, especially to colonise new lands, postulates the effective use of a 'consciously' modulated communication system, presumably verbal (language). Hence to seek the first evidence for ocean navigation is one way to determine the availability of advanced communication systems to hominids. I differ with the dominant paradigm in two important respects, however. First, I have long argued that ocean navigation has been practised successfully for at least 700 millennia, and not for 30, 40 or 60 millennia. Second, I have claimed that seafaring is not the only form of evidence of advanced communication, there are several other avenues of inquiry available to us, and if we are to examine this topic in a comprehensive, holistic and realistic fashion, then we need to consider all of them. I have consistently offered various types of relevant evidence which my opponents were not familiar with at the time they presented their hypotheses (e.g. Chase and Dibble 1987; Davidson and Noble 1989; White 1993, 1995; Groves 1995), frequently because it had not been published in English. This kind of discourse has not been particularly productive, presumably because the protagonists were not very willing to fairly consider evidence contradicting their favoured models. I consider that it is requisite to pursue and examine several different issues that are all related to the principal topic of this paper:

1. *The epistemology* - how do false but dominant models establish themselves in world archaeology, what are the dynamics reinforcing and perpetuating them, what are the reasons for their sometimes virulent defence? Why are they not refuted, as they would tend to be in one of the hard sciences?
2. *Seafaring hominids* - when and how did early humans

begin to explore marine navigation, what antecedents may have led to this development, what were its effects on human development and expansion?

3. *Competing paradigms* - what are they and how were they created, in the case of language origins? What is the principal evidence cited in their favour, and what are their effects on the major issues in Pleistocene archaeology, e.g. the origins of 'modern humans', or the two competing hypotheses of hominid evolution.
4. *The evidence* - what classes of hard evidence do we have to speculate about the cognitive, cultural and linguistic development of hominids? How do they correlate as indices of such developments, and what spatial and chronological frame do they demand for these developments?
5. *The taphonomy* - in what ways has taphonomy affected the relevant classes of hard evidence, and how does one construct a valid logical framework within which the evidence can be validly interpreted, if such interpretation were to be scientific rather than archaeological?
6. *The interpretations* - how has the evidence been interpreted? What are the arguments and the counter-arguments, what are their respective merits? How does the application of a scientific framework of logic, i.e. taphonomic logic, affect interpretation, and what levels of epistemology are involved. We thus arrive back where scientific reasoning begins and ends, at epistemology.

It is obvious that there are some rather fundamental issues before us, issues that have profound effects on the principal paradigms of world archaeology and on how we conduct archaeology. They deserve more than cursory attention from us. It is easy to say that archaeology is merely a consensus mythology about the past, as some of the more progressive practitioners recognise, but from a scientific point of view it is useful to examine why that might be so. Such examination is more likely to lead to the introduction of scientific modes of investigation than extreme epistemic conservatism, as it has characterised mainstream archaeology (Lewis-Williams 1993) up to the present time. I would argue that the discipline of archaeology is almost entirely guided by contingent prejudices, which have always determined its priorities, models, research designs, received knowledge as disseminated at universities and to the public, and treatment meted out to non-archaeologists. At every point in history, establishment archaeology, with its power base in universities and other institutions of society, has used its power to vigorously, and often viciously, oppose individuals who presented major new finds, innovations or changes in paradigms. Some of these individuals have been driven out of archaeology, some into despair, some into a premature death. This practice of archaeology dates back to its very beginnings, and it still exists today. Boucher de Perthes, the discoverer of Palaeolithic stone tools in the 1830s and 1840s, was opposed by the experts of the time for decades, who objected to the trespass of this 'amateur'. In 1858, a French archaeology congress determined that his Abbevillian tools were 'a worthless collection of randomly picked up pebbles'. In the following year, Johann Carl Fuhlrott published his interpretation of a skeleton from the Kleine Feldhofer Grotte in the Neander Valley of Germany, which prompted a very hostile reaction from the experts. His article was even accompanied by an editorial statement that the journal was opposed to Fuhlrott's views. He was for years ridiculed and humiliated by the 'great' 'scholars' of the day. Twenty years later, Marcelino de Sautuola and daughter Maria discovered Palaeolithic cave art in Altamira. De Sautuola presented his find at the 1880 congress in Lisbon, where it was universally rejected, and for many years he tried to clear his name of the implication of fraud, before prematurely dying in 1888. His death has remained on the conscience of the discipline ever since, but all to no avail. The way archaeology deals with its heretics has remained the same to this day. Over the following decade, the discoverer of *Homo erectus*, Eugène Dubois, found himself opposed by the experts of his generation (except Ernst Haeckel), who variously claimed that the Javan remains were those of an ape, a modern human, or belonged to different species. Raymond Dart, who discovered *Australopithecus africanus* in 1924, also found an icy reception. At that time, the experts of the world knew that hominids had evolved in England, where a fine specimen had been recovered from that famous gravel pit at Piltdown. Which raises another subject, the gullibility of archaeologists through history: they preferred a perfectly obvious fake to the real thing, just as today they prefer poorly based models to soundly based ones, as we shall see. Closer to our time, we recall the radiocarbon-phobia that swept through archaeology in the 1950s. In the 1980s and 1990s, we witnessed the hysterical reactions to 'direct dating' of rock art, in which the principal proponents of new dating methods were publicly humiliated after all measures to stifle them failed. It is fair to say that most truly important discoveries and innovations in Pleistocene archaeology were offered by non-archaeologists and were widely rejected by archaeologists, often with great displays of indignation and hostility. This pattern is so obviously reminiscent of the way the Roman Catholic Church used to operate in past centuries that a comparison is certainly warranted. Archaeology, after all, is a belief system: it is about the beliefs of archaeologists, who invent categories of lithics, ceramics and other remains, name them, place all finds in pigeonholes, interpret them, and then imply that the 'knowledge' they extract from these entirely invented, taxonomy-based interpretations is scientific. They claim that they can detect stylistic markers in the evidence *they* have decided constitutes archaeological evidence. They have never explained to we uninitiated how they acquired this special ability that enables them to accomplish this — for example, to tell the age of a rock painting of a totally alien graphic system by simply looking at its 'style' (cf. Rosenfeld and Smith 1997).

The present paper deals with many of the principal issues facing Pleistocene archaeology today, using the topic of the earliest ocean navigation as a springboard to delve into the origins of language, the closely related development of hominid cognition, and various other connected matters. While all of this is important, I believe it is of much greater importance to appreciate the underlying message, which deals with the susceptibility of the discipline to manipulation by vocal schools of thought, and its tendency of becoming absorbed in short-lived fads and hysterical debates about credibility. But before exploring these general issues, I think it is appropriate that I explore my specific topics in the depth that they deserve.

Introduction

One of the most debated issues in contemporary archaeology is the 'African Eve' hypothesis, according to which modern humans evolved in genetic isolation in some unspecified region of sub-Saharan Africa, towards the end of the Middle Pleistocene. They then expanded north, reached the Levant about 100 000 years ago, and poured across Eurasia, overwhelming the resident archaic *Homo sapiens* of the entire Old World without any genetic input from them. This total replacement scenario, the hypothesis implies, was made possible by superior technology and social systems, by the use of language and symbolism, including palaeoart, by superior hunting strategies, and all manners of other expressions of superiority. It resulted not only in the demise of people we think of as the Neanderthals, all other pre-modern humans of the world met with the same fate. By about 28 000 years ago, they had all become extinct, without ever affecting the genes of our victorious ancestors.

One might object that this scenario seems profoundly unconvincing, and that it resembles an origins myth or a religious belief more than a realistic model of phylogenetic evolution or demographic population dynamics. Where was this Shangrila of the African Eve tribe, and why was this group genetically isolated for such vast time spans that the race became incapable of producing offspring with any other human groups? (We know that other mammalian species, such as wolves and coyotes, interbreed very successfully.) The only evidence offered in favour of this scenario is the argument of some that mitochondrial DNA (mtDNA) (e.g. Stoneking et al. 1986; Cann et al. 1987; Stoneking and Cann 1989) or other genetic markers (Y chromosomes) (e.g. Hammer 1995) permit us to estimate the time when hypothetical populations split from each other. The mtDNA is passed on through female lineages, the Y chromosomes through the male, and the number of recorded mutations that have collected in separate populations are claimed to provide a 'molecular clock', telling the geneticist when the split between such populations occurred.

It is not surprising that the largest number of mutations were detected in African populations, the initially African evolution of early hominids is well documented in the palaeoanthropological record. However, all other premises and findings of these genetically inspired studies are debatable. The molecular clocks of different research teams tick at different speeds: the initial Berkeley team determined the time of the split between archaic and modern populations as 200 ka (200 000 years) ago, but their method was refuted by Templeton (1993) who demonstrated that they had inadvertently misused the computer program that constructed their evolutionary trees. The same data set they had used can generate 10^{267} different family trees, of which at least one billion would be just as plausible as the one chosen by the Berkeley team. Several alternative models have since been proposed, providing various different estimates of when the 'split' (which determines the genetic distances in nuclear DNA, i.e. the distances created by allele frequencies that differ between populations) is thought to have occurred (e.g. Vigilant et al. 1991; Barinaga 1992), and some alternative evolutionary trees even question an African origin (Goldman and Barton 1992). For instance, the alternative method of using Y chromosomes has been said to indicate that 'Adam' lived 188 ka ago, with a two standard deviation confidence interval from 51 ka to 411 ka ago. Other calculations range from 17 ka to 889 ka (Ayala 1996; Brookfield 1997).

These claims are, for all practical purposes, archaeologically meaningless numbers-crunching results. The mitochondrial ancestry is of no great evolutionary significance, it may have contributed very little to the evolution of our nuclear DNA. If all modern humans were the result of a migration of a single population out of Africa, the many subsequent regional populations would not have interbred, and the genetic mutations they shared initially would lead to significant global differences. This hypothesis demands that a single population evolved so much in complete genetic isolation within a few tens of millennia that it could no longer interbreed with neighbouring populations, but it then maintained its global genetic compatibility for at least 100 millennia afterwards! This is illogical, and yet it is today the dominant model in world archaeology. It is well known that African genetic traits can be well established elsewhere without any suggestion that there was total replacement of a population, for instance the sickle cell anaemia in Portugal is a result of interbreeding between Europeans and Africans. Some geneticists concede that the model rests on untested assumptions, others oppose it (e.g. Barinaga 1992). Brookfield (1997), for instance, points out that these hypotheses are entirely dependent upon preferred models of human demography, for which we lack any reliable data. Assumptions about a neutral mutation rate and a constant effective population size — variables determining the chronometric result — are completely unwarranted, yet without them the results of these speculations are largely meaningless. For instance, if the same divergence rate is applied to the human-chimpanzee distance, it produces a divergence point of 2.1 to 2.7 million years, which we consider to be unambiguously wrong. The initial 'Garden of Eden' hypothesis assumed a divergence rate of 2%-4% base substitutions per million years, but Nei (1987) suggests a much slower rate, 0.71% per million years. This would place the human-chimpanzee separation at 6.6 million years, which is similar to the estimate from nuclear DNA hybridisation data (6.3 million years), and the divergence time of moderns from other populations at 850 ka ago. This would be an archaeologically much more realistic scenario. Moreover, genetic research of other species, such as fruit flies (Wainscoat 1987) or hybrid Brazilian bees (Hall and Muralidharan 1989), suggests that the pronouncements made concerning mtDNA and hominid evolution are generally far too simplistic.

Some of the most cogent arguments against the 'mitochondrial Eve' have been contributed by Templeton (e.g. 1996). He demonstrates that the mtDNA data support the restricted gene flow hypothesis rather than the 'Garden of Eden' model:

> The mtDNA and nuclear DNA data show that gene flow occurred in Old World human populations throughout recent human evolution, but these data do not help one to discriminate between the multi-region and single-region gene flow hypotheses. This can only be done with the use of fossil data. ... The most important information that the genetic data have yielded so far is that humans evolved into their modern form as a single unit, despite past and current regional differentiation for some traits (Templeton 1996).

There is no archaeological or fossil evidence for either a massive migration out of Africa, or for the advent of a 'superior' technology or way of life coinciding with this hypothetical development. The Middle Stone Age continued right across all of northern Africa, from the Atlantic to the Indian Ocean, to about 20 ka BP, while the Upper Palaeolithic first appeared more than 40 ka ago in Siberia and soon after on the Iberian Peninsula. In the Levant as well as in western Europe (and probably in eastern Europe), archaic sapiens including Neanderthal-like types co-existed with 'anatomically modern humans' for millennia, indeed tens of millennia, and in all cases we know of, both hypothetically separate groups used the same technologies, even the same ornaments (Arcy-sur-Cure) and dwelling designs (for instance, the mammoth bone huts of eastern Europe occur both in the Mousterian and the Upper Palaeolithic). This would imply that, wherever Eve's victorious prodigy went to settle, they adopted the ways and tools of the resident population. The personal ornaments of the Châtelperronian Neanderthals of France (Figure 1) are indistinguishable from those of the 'Aurignacians' of the same time, which prompted the replacement advocates to speculate that the former must have 'scavenged' (White 1993) or 'traded' them from the latter. Again this is an illogical argument: why would a hominid who has no concept of symbolism trade or scavenge symbolic articles from a 'superior' race? Bearing in mind that there is not the slightest evidence that these early 'Aurignacians' possessed a superior, or even different, technology, what proof of technological or cognitive superiority do we have for them? It is much more likely that we have succumbed to our preconceived biases, and our subconscious desire to project our modern Eurocentric concepts of cultural superiority onto Pleistocene societies.

No evidence of any type exists, anywhere in the world, that might suggest that the advent of Upper Palaeolithic technology coincides with the introduction of 'modern human anatomy'. On the contrary, there is ample evidence suggesting a gradual introduction of the more complex technology in situ, in many regions (e.g. central and east-central Europe, Russia, Ukraine, China, Siberia), and a gradual human evolution (e.g. in China, Russia, northern Africa, central Europe and Java-Australia). There are numerous finds of intermediate hominids, displaying both archaic and anatomically modern characteristics, including those from the following sites: Mladec Cave, Krapina, Vindija Cave, Hahnöfersand, Starosel'e, Rozhok, Akhshtyr', Romankovo, Samara, Sungir', Podkumok, Khvalynsk, Skhodnya, Narmada, Jinniushan, and several more Chinese sites discussed below. There should be no doubt that a sapienisation process took place in many regions, and that anatomically modern humans occur in Mousterian contexts, at least in Ukraine, Russia (Roginsky et al. 1954; Yakimov 1980) and northern Africa. The occurrence of typical Neanderthaloid features in modern Europeans (e.g. nerve ending in mandible), or erectoid features in Mongoloids (e.g. shovel-shaped incisors) has long been noted. All of this renders the 'African Eve' hypothesis superfluous, and it is mostly information that has been available for a long time.

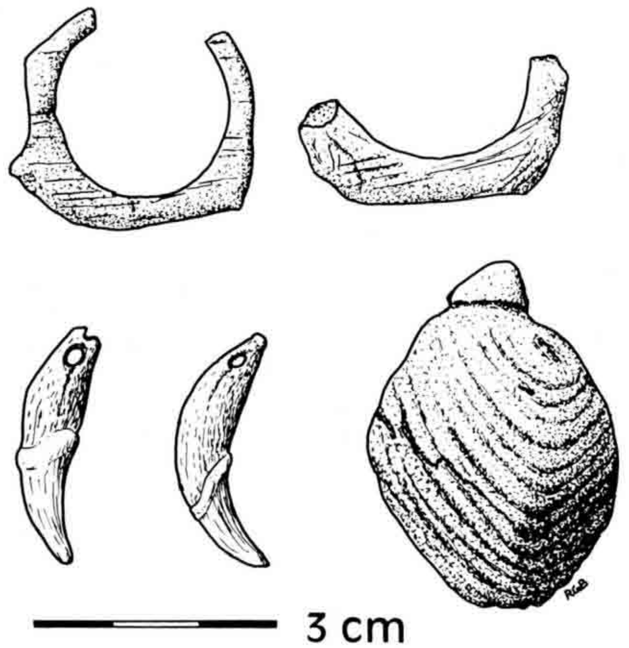

Figure 1. *Two ivory ring fragments, two perforated animal canines and a fossil shell with an artificial groove for attachment. Châtelperronian, Grotte du Renne, Arcy-sur-Cure, France. These objects were used, and almost certainly made, by Neanderthals.*

During the last two years there were two more relevant developments. First, after hominid remains from Atapuerca in Spain (Arsuaga et al. 1993; Stringer 1993; Bahn 1996) were proposed to be the common ancestor of both 'Neanderthals' and 'moderns', it was suggested that this supports the 'Eve' model. On the contrary, an evolutionary sequence in Europe provides no support for 'Eve', it contradicts it categorically. Or are we to assume that there was a mass-migration from Spain to southern Africa, before the one from southern Africa back to Spain? The probability of such finds as those from Atapuerca was predicted by Wolpoff (1989) years before the finds were made. Secondly, the recent detection of mtDNA in the original Neanderthal remains has been claimed to exclude the possibility that this was a direct ancestor of modern Europeans. Not only is the method used controversial and there are no corroboratory studies of other remains, the issue is hardly relevant to the origins of *Homo sapiens sapiens*, which are agreed to be earlier than the time of the late, i.e. classical, Neanderthals. In other words, the question of whether the late Neanderthals were major contributors to the extant human genome has little or no bearing on the origins of anatomically modern humans.

The 'Eve' hypothesis prefers a scenario with great cultural, cognitive, social and intellectual differences between Eve's descendants and their contemporaries, because without them most of its principal postulates are unconvincing. Language, obviously, plays a central role here. If the two *hypothetical* hominid groups were unable to interbreed, communicate or meaningfully interact, then the replacement hypothesis does appear attractive. This requires that significant innovations were introduced by the 'moderns', at least some of which can be perceived by archaeology. It also implies a significantly more archaic culture prior to the arrival of the 'moderns'. The principal factors that have been considered in this context are the introduction of blade tool industries and some specific lithic implements (e.g. hafted tools); the introduction of symbolism, palaeoart and language; the advent of human interment practices; new material extraction techniques and hunting methods; and various technological innovations, such as navigation — especially ocean navigation. Garden of Eden proponents have assumed or argued that all these innovations appeared together with or after the migration out of Africa they claim occurred, probably around 100 ka ago. This timing is based on the earliest known appearance of moderns in the Levant and the genetic divergence dates mooted.

However, these claims appear to be entirely false. Blade tools occur commonly before the Upper Palaeolithic, e.g. in the Amudian of northern Africa and the Levant (e.g. Haua Fteah and Jabrud; Rust 1950; McBurney 1967). The same applies to burins and hafted projectile points, for instance. The tanged Aterian points of northern Africa were almost certainly hafted, and we have residues of hafting resins from Middle Palaeolithic sites in Germany and Syria (Bednarik 1996a). Technological factors cannot tell us what type of humans produced specific hominid cultures during the early Late Pleistocene: some Mousterian is by Neanderthals, some is not, while the makers of the Bordesian Mousterian of Acheulian Tradition remain unknown. Palaeoart, too, occurs at numerous sites from the Lower Palaeolithic onwards (Bednarik 1992a, 1995a) and is in no way related to palaeoanthropological indices as we perceive them. There is no evidence that hunting methods and subsistence strategies differed markedly across the imaginary divide, and the Lower Palaeolithic hunting spears from Schöningen and other sites are as sophisticated as modern ethnographic specimens. In fact the relative proportion of very large game is often greater in the Lower than in the Upper Palaeolithic, and some human groups of the former era were apparently highly specialised big game hunters (e.g. at Bilzingsleben, Germany), which in itself seems to demand an effective social system. Human burials are well known from the Middle Palaeolithic, e.g. from La Chapelle, La Ferrasie, Teshik Tash and Shanidar (Gargett 1989; Harrold 1980; Leroi-Gourhan 1975). There are even several interments of classical Neanderthal infants, such as the ten-months-old Amud 7 specimen (Israel), the two-year-old infant in Dederiyeh Cave, Syria, or the infant burial 6 in La Ferrassie, France. Underground mining occurs in the Middle Stone Age of southern Africa and in the Middle Palaeolithic of Egypt and central Europe (Bednarik 1995b). These and other indicators of relative cultural sophistication, many of which I have reviewed systematically, are impossible to account for without an effective communication system, which very likely was a 'consciously' modulated verbal system or articulated sound of some kind — i.e. speech.

Leaving aside for the moment the fossil evidence for speech or language, we see that only one major category of indirect evidence postulating language use seems to remain unavailable prior to the Upper Palaeolithic: the ability to cross the open sea, and to colonise island regions in this way. Therefore one of the key arguments in the 'Garden of Eden' scenario is that the human crossing of Wallace's barrier, between the Indonesian islands of Bali and Lombok, occurred only around 50 ka ago, because by this time the 'moderns' had arrived in South-east Asia from the Levant (Bartstra et al. 1991). This is a cornerstone in Australian archaeology, and nearly all initial colonisation models ever enunciated for Australia take as gospel that the Lesser Sunda Islands (Nusa Tenggara) were not occupied any earlier, and that the entire region from Bali to Tasmania was colonised within about 20 ka. In the following paper I will show that this model is entirely false, and that all initial settlement models of this region are in error, at least in a chronological sense. This correction also has a significant effect on the Garden of Eden model. Not only does it eliminate the need to involve the descendants of African Eve in the general region's settlement pattern, it withdraws from that hypothesis the last possible archaeological support it may have been able to claim.

Clearly this is an important subject for Pleistocene archaeology and palaeoanthropology generally, and for the Australian discipline in particular. The acquisition of navigational competence would have been the crucial factor in attaining the ability to colonise isolated and otherwise inaccessible regions, such as Sahul (Greater Australia, including, among other islands, New Guinea and Tasmania).

Australasian archaeology has been traditionally considered as being of little consequence to the rest of the world, until the region's discoveries of the 1960s and 1970s helped establish a school of Australian archaeology. During the 1980s, the favoured model of initial Australian colonisation was based on radiocarbon dates from sites in both Australia and various islands to its immediate north that seemed to peter out at ages of around 40 ka. Consequently it was assumed that occupation of the south-east Asian islands east of Java was achieved quite swiftly, and entirely by *Homo sapiens sapiens*, which clearly supported the 'Eve' scenario. In recent years, a series of TL (thermoluminescence) and OSL (optically stimulated luminescence) dates from sediments at some northern Australian sites has been widely accepted as extending human occupation securely back to about 60 ka BP (Roberts et al. 1990, 1993). Implicit in this model is the assumption that the lack of radiocarbon dates of above 40 ka is attributable to a 'dating plateau' related to the method's own limitations. Some Australian archaeologists, however, reject this view, basically because the same plateau has not been observed with 'geological' radiocarbon dates from Australia, and they maintain that only ages of up to 40 ka are secure (Allen and Holdaway 1995).

From time to time, a long-range hypothesis for first Australian occupation has been advocated. This was based initially on palaeoecological evidence, particularly in the form of abrupt changes in pollen spectra and inferred incidence of vegetation burning (Singh and Geissler 1985; Kershaw 1993). A human presence in Australia by 140 ka to 130 ka ago is implied by this alternative. Recently, Fullagar et al. (1996) have presented a series of TL dates from Jinmium, Northern Territory, which are claimed to imply human occupation older than 116 ka BP. This interpretation, however, has been subjected to vigorous critical debate, particularly because the dating method is widely considered inappropriate for the saprolithic sediment type in question. A major factor in the preference of the short-range model is the underlying assumption that the first colonisers were fully modern humans (because they must have had the ability to build ocean-going vessels, which through circular reasoning is thought that only moderns could have achieved), an assumption that is impossible to reconcile with dates of 100 ka or more.

One aspect overlooked is that, at whatever time first landfall occurred in Sahul, it was by Middle Palaeolithic people, and certainly not by Upper Palaeolithic seafarers. In fact we have almost no evidence in the world that Upper Palaeolithic humans even practised seafaring (Melos is the only clear-cut instance, but that evidence is from the very end of the Pleistocene). So perhaps the African Eve proponents would need to demonstrate that navigation was even practised by Upper Palaeolithic people. There is no doubt that Middle Palaeolithic seafarers were extremely active, in the Wallacea-Sahul region and elsewhere. After their ancestors travelled through much of Indonesia and reached Sahul, they embarked on a great colonisation drive. Their presence can be detected on Gebe Island (between Sulawesi and New Guinea, in Golo and Wetef Caves) up to 33 ka ago, and around the same time on some Pacific islands, in the Bismarck Archipelago (Matenkupkum and Buang Marabak on New Ireland) and the Solomons (Kilu Rockshelter on Buka Island, 180 km from New Ireland) (Allen et al. 1988; Wickler and Spriggs 1988). The Monte Bello Islands, now 120 km off the north-western coast of Australia, i.e. in the Indian Ocean, were occupied about 27 ka ago (Noala Cave, Campbell Island; Lourandos 1997: 119). These great ocean crossings were still by Middle Palaeolithic people, and on Tasmania an essentially Middle Palaeolithic technology survived into the 19th century. Between 20 ka and 15 ka ago, obsidian from New Britain was transported to New Ireland, and the cuscus from Sahul to the Moluccas (e.g. Morotai and Gebe; Bellwood 1996). People with a Middle Palaeolithic technology travelled the ocean apparently habitually, and with admirable confidence. Their seafaring abilities probably were the ancestral basis of the incredible navigational feats of more recent peoples of Wallacea and the Pacific, unequalled in the rest of the world.

Some background information

In considering the question of the initial colonisation of Wallacea and Sahul we first need to examine the region's biogeographical history. Crucial to this biogeography is the Tertiary tectonic history of the region. It is dominated by one process and its consequences — the northwards movement of the Sahul Plate, Australia's part of Gondwanaland. As the sections of Gondwana drifted apart, most made contact with other continental plates. Sahul drifted north over the past 70 million years or so, colliding with the Sunda section of the Pacific Plate around 15 million years ago. Full engagement during the Pliocene brought about several tectonic developments. The subduction zone formed along the Sunda Islands led to the establishment of deep ocean trenches and uplift along the margins of the Sunda shelf, and the formation of a chain of islands. The region became volcanically very active, with some of the greatest eruptions in recent history occurring in this area of continuing subduction and seismic activity. But the continental collision has not yet resulted in land bridges to any of the deep-water islands formed by it, be it through uplift, subduction or the deposition of volcanic debris. Even during the lowest sea levels of the Pleistocene, perhaps 150 $\pm$ 10 metres below the present level (Hantoro 1996), most of Nusa Tenggara (Lesser Sunda Islands) failed to connect.

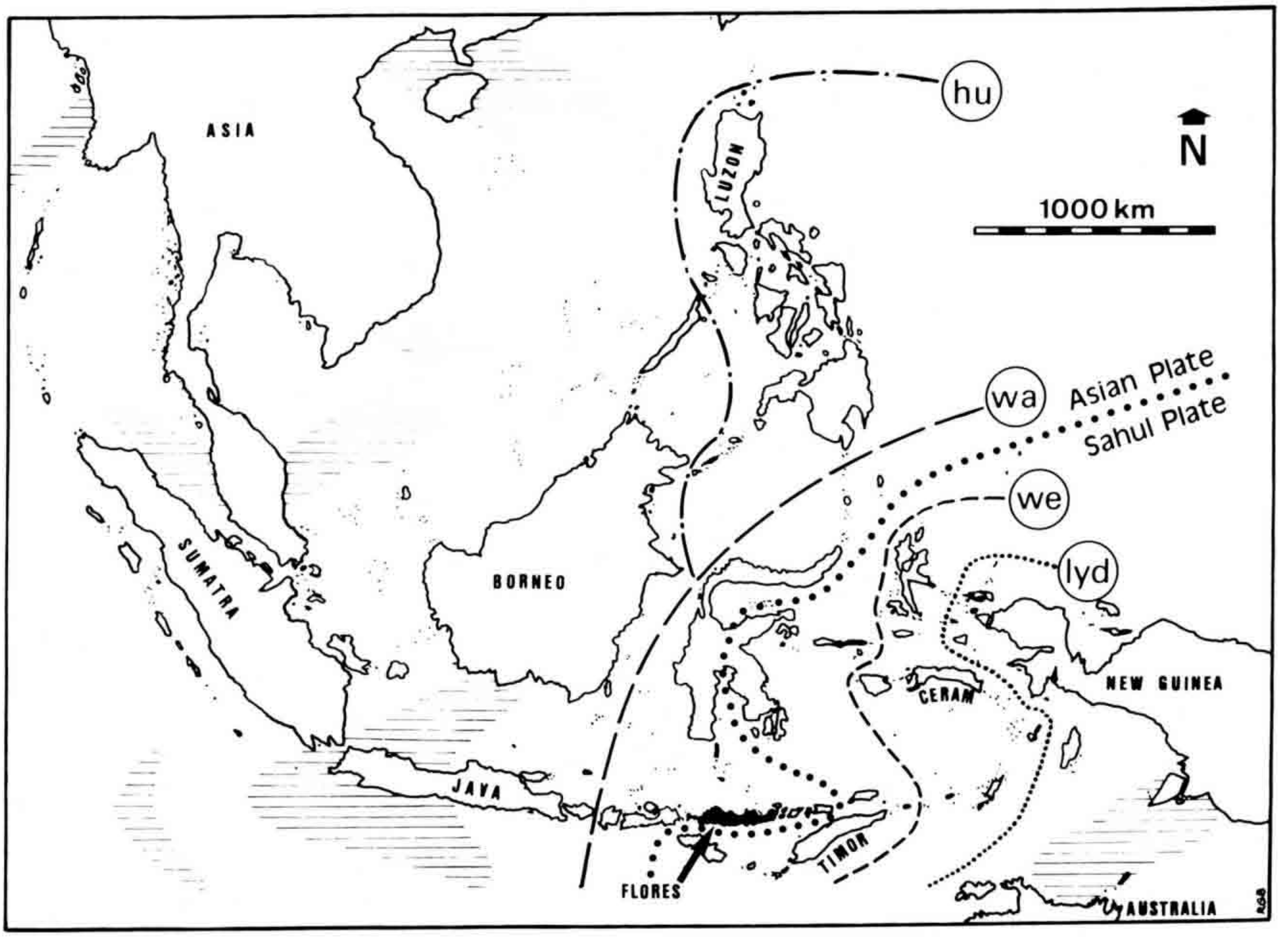

Figure 2. South-east Asia: hu = Huxley's modification of Wallace's Line; wa = Wallace's Line; we = Weber's Line; lyd = Lydekker's Line. The line supposedly dividing the Eurasian tectonic plate from the Sahul (Australian) plate is also shown.

These tectonic processes have led to the development of biogeographical zones which are defined by Wallace's Line (Wallace 1890) and its modification by Huxley (1868) (the furthest extent of Asian faunal complexes, dominated by placental mammals), Weber's Line and, furthest to the east, Lydekker's Line (the furthest extent of Sahul faunal communities) (Figure 2). Wallacea comprises the islands situated between the lines of Wallace-Huxley and Lydekker (1896), thus effectively straddling the Eurasian and Australian continental plates. The actual separation of the continental plates is, however, not entirely certain. The division depicted in Figure 2 reflects one model, but there is a possibility that parts of South-east Asia were initially continental fragments of eastern Gondwanaland. After a series of islands formed in the Tertiary, they collided with the ancient continent of Laurasia, being pushed northwards by the Sahul plate (Whiffin 1996).

The distribution of floras and faunas across this region reflects the geographical conditions profoundly. Sumatra, Borneo, Java and Bali were all accessible from the Asian mainland during periods of low sea level. Consequently they display a rich diversity in both the present and the Pleistocene past. The islands east of Lydekker's Line are occupied by Sahul species (e.g. monotremes), while birds of paradise and the cassowary extend west to Weber's Line. The cuscus even reached Timor and Sulawesi (perhaps transported by humans), and cockatoos made it as far west as Borneo and Lombok.

Several species were introduced by man moving from west to east. The dog, which reached Australia perhaps 4000 years ago, the pig, and probably the macaque all crossed Wallace's barrier as passengers. A variety of small mammals, mostly Muridae but including *Trachypithecus auratus*, managed to cross apparently unassisted, but rafting on floating vegetation was generally limited to small individuals (Diamond 1977, 1987a). Thus, the faunal influence gradually decreases from either side, with the various water barriers acting as a series of ecological filters in both directions. These filters have similarly determined flora (Polhaupessy 1996), together with the significant climatic oscillations of the Pliocene and Pleistocene. Palaeobotanical research has demonstrated that the tropical rainforests of Australia have a long geological history and are unrelated to those of South-east Asia (Morley 1996). Few plant taxa have crossed Wallace's Line, but Sulawesi is recognised as a possible bridgehead allowing eastward migration of flora.

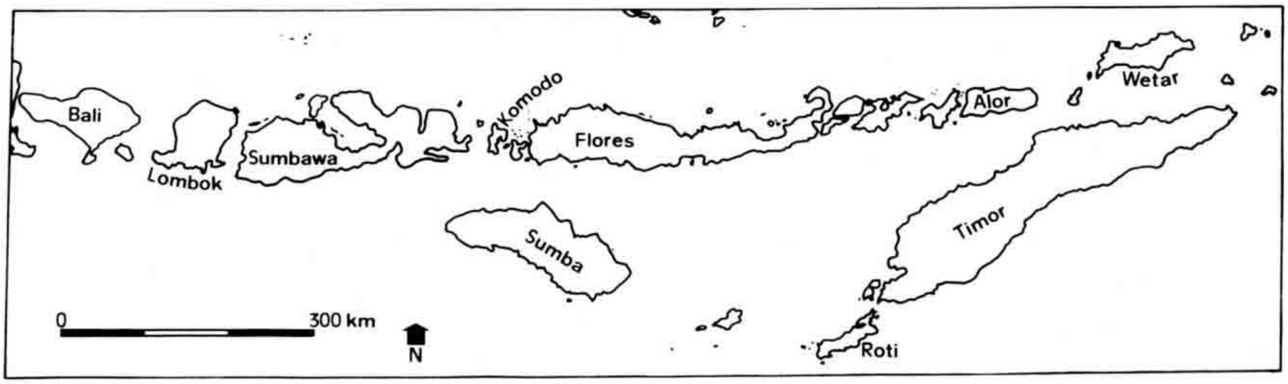

Figure 3. *Nusa Tenggara, the Lesser Sunda Islands, southern Wallacea.*

The two most impressive colonisers, however, were proboscideans and hominids. Elephants are known to be superb long-distance swimmers and might have travelled in herd formation (which would have provided a viable breeding population after landfall), helped by their buoyancy — especially in saltwater. Several fossil species of Stegodontidae and elephants are found in Wallacea, including on Sulawesi (three species; Groves 1976), Flores (Hooijer 1957; Verhoeven 1958), Timor (Verhoeven 1964; Glover 1969) and other islands east of Flores, even on Ceram and Irian Jaya (Hantoro 1996), and in the Philippines (on Luzon and Mindanao; Koenigswald 1949). These include large as well as dwarf species, the latter being the result of isolation. Dwarfism among proboscideans can be found on other islands, including Malta in the Mediterranean and the Santa Barbara Islands off California. A trend towards giant or dwarf forms, as well as rapid speciation, are typical of island endemism.

Apart from a number of exceptions, terrestrial fauna has been unable to cross the Strait of Lombok, apparently at any sea level, and for as long as the Lesser Sunda Islands have existed. The recent geological age of these islands, together with their isolation, has impeded the introduction of dense rain forest communities. These are found in the large islands to the west and north of Lombok, together with their highly diversified Asian faunas, rich in eutherians throughout the Quaternary.

Humans are known to have first occupied Java some time between one and 1.8 million years ago, i.e. since the first known emergence of *Homo erectus* in the general region. Bali, although now separated from Java by barely two kilometres of shallow water (under 100 m deep), was almost certainly connected to Java at various times during the Pleistocene. According to almost complete archaeological consensus, hominids never crossed Wallace's Line (between Bali and Lombok) until fully modern people arrived in the region (from Africa, presumably) with their 'new technology', shortly before the first known occupation of Australia. Bartstra et al. (1991) argue that Wallace's Line was first crossed by humans about 50 ka ago. Swisher et al. (1994) have provided what they term precision dating for the earliest appearance of *H. erectus* on Java at about 1.8 - 1.6 million years ago, which almost coincides with the species' first appearance in Africa. The recent report of what has been described as a mandible of *Homo habilis* or *ergaster* from a Plio-Pleistocene in Longgupo Cave, central China, has added fresh fuel to the debate between multiregionalists and those who favour the repeated dispersion of hominid species from Africa, because, especially in conjunction with the early dates from Java and Georgia, it raises the possibility that even *H. erectus* did not evolve exclusively in Africa (Huang et al. 1995). Here, however, more evidence is required, as the fragmentary fossil from Longgupo Cave cannot be reliably identified (see below). Finally, we have Swisher et al.'s (1996) dating evidence, which is said to suggest that *H. erectus* survived on Java well into the Late Pleistocene, to between 53 and 27 ka.

It seems generally agreed that humans would have required rafts or boats to cross Wallace's Barrier even as individuals, but especially in order to travel as colonising parties capable of founding new and viable populations on the chain of islands to the east. The precise distances to be crossed in each case are not known, because they would have been determined by a number of variables that seem impossible to establish. Elastic compression and decompression, of both continental and sub-oceanic crust, may be quite rapid, up to an order of centimetres per year (van Andel 1989). Isostatic compensation would be difficult enough to estimate in a perfect scenario, but in one of high tectonic activity, and against unknown rates of relative plate movement and inadequately known absolute eustatic variations, it would be impossible to guess actual shore distances at any point of time in the Pleistocene. The principal evidence for the isolation of these islands comes from biogeography, from the distinct paucity of species unable to cross major sea barriers, from the trend to endemism (such as speciation) and the preservation of 'relic' species such as *Varanus komodoensis* on Komodo. The longest sea crossing required between Bali and Timor, nevertheless, was Lombok Strait (25-30 km), followed by Ombai Strait (between Alor or Atauro and Timor, depending on sea level between 10 and 25 km). Lombok and Sumbawa may have been joined at low sea level, but Sape Strait (between Sumbawa and Komodo) was probably always open (perhaps 5-10 km). Komodo and neighbouring islands may have been connected to Flores at times, which may also apply to the islands east of Flores, as far as Atauro (Figure 3). Similarly, Roti is likely to have been joined to Timor during stadial peaks of the Pleistocene. All of this is, however, hypothetical, because present topography, both above and below water, does not provide reliable data. Tectonic uplift of the region continues, and was quite substantial in some parts of the region during the Pleistocene (e.g. on Flores, as we shall see), while not affecting others appreciably, at least not during the Late Pleistocene.

Moreover, we cannot be certain that the initial colonisation of Australia occurred along the lowest of Birdsell's (1957, 1977) possible routes. Butlin (1993), however, favours it strongly after considering wind direction, visibility of opposite shore and various other factors. I agree with him completely, especially for reasons that will become apparent in the present paper. Butlin's arguments, nevertheless, are themselves convincing enough, particularly as Pleistocene sailors may not have had great skills in directional sailing at their disposal. The crucial journey from Timor may have been long and arduous, but it required only a rudimentary raft, and the target to be hit at the end of it was almost impossible to miss. Butlin also argues, quite rightly, that the preoccupation of Australian archaeologists with lowest sea levels, in relation to the subject of sea crossings, is unwarranted; other factors may have been more important. Many coasts would have been far steeper and rockier at low sea level, while the availability of coastal resources would have been greater during high levels. It is also likely that dominant wind and water current directions were more favourable during high water levels, i.e. during interstadials. These factors may have more than cancelled the advantage of shorter distance between shores.

Irrespective of this, humans have been arriving in Australia most certainly for 40 ka, probably for 60 ka, and possibly for longer. So far, the identity of the people who first occupied Wallacea and thus moved towards Australia has never been considered to be anything but being that of 'anatomically modern humans', *Homo sapiens sapiens*. The present paper seeks to correct this very important misconception about the background of the first landfall in Australia, and it also explores the reasons for this significant error of judgment. To fully appreciate the scenario that culminated in the settlement of Sahul it is necessary to lay to rest most of the mythologies Australian archaeology has created about this topic, to take a deep breath, and to start anew. To do so, the initial precondition is to examine the background of the initial occupation of Wallacea, which has also never been done effectively in Australian archaeology. This involves, first of all, a brief consideration of the potential 'catchment area' of Wallacean seafarers, and thus the spatial and temporal preconditions for the origins of ocean navigation. To do this, and to do it effectively, we need to examine the Asian origins of these people, and we need to begin early. A lot earlier than a mere 50 000 years ago, as the textbooks would have us believe.

The Asian catchment area

The earliest supposedly hominid finds from Asia (Figure 4) are the late Pliocene mandibular fragment with two teeth and a single maxillary incisor from the Longgupo Cave site in China (Huang and Fang 1991; Huang et al. 1995; Wood and Turner 1995; Bednarik 1996b). Palaeomagnetic dating suggests an age of 1.96 to 1.78 million years ago, electron spin resonance dating has provided a conservative minimum age of a million years, and this is supported by the plentiful accompanying faunal remains (116 species). Together with the very early dates for Javan *Homo erectus* (see below), this find questions the sole African development of *H. erectus* (Culotta 1995). Lacking good diagnostic features, the Longgupo specimens are thought to resemble either *Homo habilis* (Ciochon 1995) or *Homo ergaster* (Wood and Turner 1995), both so far endemic to Africa. Most importantly, two stone tools were found in the hominoid/hominid-bearing sediment stratum of the cave. While the upper lateral incisor is generally accepted as being human, the mandibular fragment is not, being attributed to a pongid by some commentators (Schwartz and Tattersall 1996). Dennis A. Etler and Milford H. Wolpoff (pers. comm. Nov. 1996) have both expressed the opinion that the latter might be of *Lufengpithecus*, but this explanation does not account for the upper incisor or the presence of two stone implements. The incisor resembles both recent Asian specimens and *H. erectus*, but this would be as difficult to reconcile with the reported age as would be the occurrence of stone tools with an ape.

The east Asian anthropoid primate history from the Eocene (Beard et al. 1996) to the Pliocene is quite complex, and several species persist into the Pleistocene. *Lufengpithecus* was a chimpanzee-sized ape of great sexual dimorphism (Etler 1984), known from Lufeng (Late Miocene; Wood and Xu 1991) and Yunnan (Middle Pliocene; Wu and Poirier 1995). The limited dentition available from Longgupo (P4 and M1) seems to resemble that of the female *Lufengpithecus* (Kelley and Etler 1989), but this species has not yet been confirmed from the Plio-Pleistocene. The upper incisor, while clearly hominid, is small relative to the available maxillary Zhoukoudian *H. erectus* specimens and has so far not been convincingly identified. However, it should preferably be compared with the two much older upper incisors from Yuanmou (see below) rather than the Middle Pleistocene specimens. The two stone implements resemble the Oldowan industry, one being a lenticular flake with several flaking scars along the edge (Huang et al. 1995). As they are made of andesite-porphyrite, a material not occurring on the region's limestone karst, they must have been carried for a considerable distance. While some commentators have suggested that the hominid incisor might be intrusive, it seems more likely that

Figure 4. *Key hominid find sites of Eurasia mentioned in text, arranged by apparent age.*
Plio-Pleistocene to very early Middle Pleistocene: 1 - Longgupo, 2 - Sangiran, 3 - Dmanisi, 4 - Lantian, 5 - Yuanmou.
Late Middle Pleistocene to Late Pleistocene: 6 - Zhoukoudian, 7 - Bilzingsleben, 8 - Hathnora, 9 - Ngandong.

the tools are connected with the incisor, which might be of a very early *H. erectus*. The specimen differs from incisors of African habilines in some respects, resembling only one specimen, OH 6, in its crown index, and differs in its lingual tubercle development, crown curvature and marginal ridges. The greatest difficulty with this is its proposed timing, as it precedes the earliest known African find of *H. erectus*. However, it would be only 'slightly' older than the claimed earliest date of that species in Java (Swisher et al. 1994) and at Dmanisi, in the Caucasus region (Gabunia and Vekua 1995; Dean and Delson 1995). The Dmanisi find, thought to be about 1.8 million years old, consists of a single mandible, but the early *H. erectus* material from Java, much further from Africa, consists of a fair collection. Moreover, it should be cautioned that detailed dimensional studies of a single tooth are not adequate means to decide the issue. Dental reduction has been observed in many mammalian species in South-east Asia (Hooijer 1952) and could just as easily have affected hominids.

In China, *H. erectus* remains are presently available from Zhoukoudian Site 1 (6 calvariae, 12 cranial fragments, 15 mandibular pieces, 157 teeth, 13 post-cranial pieces), Gongwangling (partial cranium) and Chenjiawo (mandible) at Lantian, Donghecun at Luonan (upper M1), Qizianshan at Yiyuan (partial cranium, 1 supraorbital fragment, 1 distal humerus, 1 femoral head, 1 rib, 7 teeth), Tangshan at Nanjing (2 partial crania, some teeth), Longtandong at Hexian (calvaria, 1 supraorbital fragment, 1 parietal fragment, 1 mandibular corpus, 8 isolated teeth), Xiaohuashan at Nanzhao (lower P4), the Xichuan site (12 teeth), Danawu at Yuanmou (2 upper central incisors, 1 tibia), and four sites in Hubei Province: Quyuan River Mouth at Yunxia (2 crania, c. 350-500 ka), and the teeth (total 16) from Longgudong-Yunxian, Longgudong-Jianshi and Bailongdong. These remains are generally of the Early and Middle Pleistocene, mostly the latter, while hominid remains from three late Middle Pleistocene sites are considered to be transitional between *H. erectus* and archaic sapienoids. Miaohoushan Locality A at Benxi has yielded an upper canine, lower M1 and juvenile femoral shaft, and Yanhuidong at Tongzi seven teeth. Considerably better evolutionary evidence is available from the Jinniushan site near Yingkou, Liaoning Province, where one cranium as well as various postcranial finds (6 vertebrae, 2 ribs, 1 innominate, 1 patella, 1 ulna, carpals, metacarpals, manual phalanges, tarsals, metatarsals, pedal phalanges), still from a late Middle Pleistocene deposit, show clear transitional features. Jinniushan has yielded two distinctive occupation strata, the lower resembling the scraper and bipolar flake industry of Zhoukoudian Site 1, a classical *H. erectus* site. The hominid remains are from an upper stratum, and are particularly difficult to reconcile with the 'African Eve' model. Much the same applies to a site in Guandong Province, Maba (Shaoguan) at Shiziyan, where a somewhat more recent, early Late Pleistocene, calotte occurred together with a Middle Palaeolithic industry. Of similar age are the archaic *H. sapiens* collections from Xujiayao at Yanggao (2 parietals, 10 parietal fragments, 1 temporal, 2 occipitals, 1 left maxilla with I1, C, M1 and M2, isolated upper M1 and M2, 1 mandibular ramus), and at Dingcun site near Xiangfen (upper I1, I2, M2, and infant parietal), both in Shanxi Province. Especially one of the occipitals from Xujiayao is of pronounced erectoid morphology. The above-mentioned two crania from Yunxia, while usually being attributed to *H. erectus*, also possess already flattened, sapienoid faces several hundred millennia ago.

There are still several other Chinese sites with remains of archaic *H. sapiens* possessing erectoid features. For instance, the maxilla and occipital fragment from Yenshan at Chaoxian, Anhui Province, is of the Middle Pleistocene. The Dali cranium from the Tianshuigou site is more recent, being of the early Late Pleistocene, but has preserved distinctive archaic features, in particular its thick and prominent supraorbital torus. The maxilla from Wanlongdong at Changyang is of similar age, and is similarly archaic. Collectively, and in conjunction with the lithic technology often accompanying the hominid remains, this impressive corpus renders it impossible for Chinese palaeoanthropologists to accept the Garden of Eden scenario.

The Chinese evidence, however, is not unique, it is duplicated to a greater or lesser extent in various regions of Eurasia, as already mentioned above. Of particular interest in the present context is the Narmada skull, found in 1982 at Hathnora in the Narmada valley south of Bhopal, India. This specimen remains the only hominid fossil available to us from that vast and biogeographically crucial region between the Near East and Java. Despite this obvious importance, the Narmada cranium is rarely mentioned in the literature. Lumley and Sonakia (1985) described it as a very evolved *H. erectus*, but it seems to be of the late Middle Pleistocene (200-140 ka), being associated with a Middle Pleistocene fauna and Acheulian biface tools. Most importantly, the Narmada skull's morphology is not typically erectoid. The specimen is substantially incomplete, only the right half of the cranium, with zygomatic arch, right torus and right part of occipital are intact. My examination suggests the presence of pronounced archaic features, such as a thick torus (that may have been continuous, although this is far from certain), erectus-like bone thickness, and a very distinctive postorbital constriction. However, the vault capacity is between 1200 and 1400 cubic centimetres, almost above the upper range of *H. erectus*, the calotte is well rounded with high forehead, including the occipital section. The cranial volume is particularly noteworthy as the specimen is thought to be of a female in her 30s, so it is high even for an archaic *H. sapiens*. I therefore defined this specimen as being intermediate between the two species, but closer to *H. sapiens* (Bednarik 1995a: 611).

Leaving aside south-western Asia, we can observe that all evidence of the earliest hominid evolution in Asia comes from two regions: China and Java. The apparently earliest find, the controversial Longgupo material, has been mentioned. In China this is followed by the Lantian cranium from the Gongwangling site and the Yuanmou finds from the Danawu site (Quian 1985). These are of the Early Pleistocene, even if the much lower dating of Yuanmou at about 700 ka is preferred to the greater age of 1.7 million years. It would then match the estimated age of the Lantian specimens, also 700 ka. At the latter sites, the hominid finds occurred with a heavy-tool tradition that contrasts with the Middle Pleistocene flake tool industries found at Zhoukoudian, and that is assumed to characterise the early Palaeolithic tool technology of the region. This industry is also found at other very early habitation sites in China, such as Kehe and Dingcun, both in Shanxi Province. The second site has also yielded hominid remains, as noted above, but they are of archaic *H. sapiens*, not *H. erectus*, and belong to the early Late Pleistocene. The tools at Dincun include large flake tools, choppers, prismatic points and stone balls. Once again, continuity rather then replacement seems to mark the hominid history of the region.

The hominids of Java seem less well understood. A century ago, Dubois recognised the island as a treasure trove of hominid remains, a reputation which it has rightly retained to the present. Dubois (1894) discovered the first specimen of *H. erectus*, a skull cap, at Trinil on the Solo River and recognised in it the ancestral form Haeckel (1868) had predicted decades earlier. Klaatsch (1908) was the first to propose an evolutionary sequence connecting the Javanese hominids with Australoids. After the discovery of much more recent hominid remains at Ngandong, Weidenreich (1943, 1945) developed this idea, which was eventually incorporated in what is today known as the multiregional hypothesis of the origins of modern humans (Coon 1962; Larnach and Macintosh 1974; Thorne 1980; Thorne and Wilson 1977; Thorne and Wolpoff 1981; Wolpoff 1980, 1991). Early Pleistocene hominids were also recovered from the upper part of the Pucangan beds and the probably later lower part of the Kabuh beds at Sangiran. The difficulty with all early hominid remains from Java is that many if not most are from fluvial, secondary deposits, and they cannot reliably be related to lithic industries (cf. Soejono 1961). For instance, the Middle Pleistocene small flake tool industry Koenigswald (1936, 1939, 1956, 1973) described from Sangiran may be from secondary deposits. Movius' (1944, 1948) Pacitan (Patjitan) has been widely assumed to be of the early hominids of the island, although there is no solid evidence for such a link and the chronological position of the industry remains unresolved. Similarly, the traditional problems with estimating the ages of the Javan hominid remains directly continue to the present time (Theunissen et al. 1990). The Pucangan beds were dated to 1.16 million years by fission track analysis (Suzuki et al. 1985), but their base is 3-2 million years old. The Kabuh beds begin at c. 1.4 million years and extend to the boundary between the Early and Middle Pleistocene (Ninkovich and Burckle 1978; Suzuki et al. 1985). In 1994, Swisher et al. presented dating evidence for three early remains, the partial cranium of the Mojokerto child and two fragments from Sangiran. They used an improved method of potassium-argon analysis, in which single crystals can be dated. As in the luminescence methods (TL vs OSL), this increases precision and reduces contamination, because abnormal values can immediately be identified. According to the findings of Swisher et al. (1994), the Mojokerto child is 1.81 million years old (previously estimated 1.5 Myr), the Sangiran individuals 1.66 million years. The Mojokerto find is the earliest known hominid fossil in Java, being from the Pucangan deposits, and the reaction of disbelief to the dating of the fossil is surprising, in view of its expected great antiquity.

The second group of Javanese hominids are those from the High Solo Gravels, best known from Ngandong. Belonging to the late Middle Pleistocene or the early Late Pleistocene, these fossils are variously described as late *H. erectus* (*Homo erectus soloensis*) or as archaic *H. sapiens*. A total of fourteen partial or complete hominid calvaria, two tibia and some pelvic fragments have been recovered at Ngandong since 1931 (Santa Luca 1980). These remains are from a thin fluvial facies of sandstone, composed of volcanic debris and marl cobbles. Recently, Swisher and his team have analysed fossil bovid teeth from this hominid-bearing sediment facies at three sites in central Java. Electron spin resonance (ESR) and mass-spectrometric uranium-series dating provided a series of dates ranging from 53 300 $\pm$ 4000 to 27 000 $\pm$ 2000 years BP (Swisher et al. 1996). Excavations were conducted at Ngandong, Sambungmacan and Jigar and the teeth from all three locations yielded similar results. The ages for the Ngandong samples range from 46-27 ka, those from Jigar from 40-27 ka, and the Sambungmacan samples from 53-27 ka.

These dates appear to be more problematic than the earlier ones by Swisher and colleagues. The bovid teeth come from a fluvial deposit and may thus include specimens of different antiquities, some of which would be from an earlier deposit. Unpublished dates by Christophe Falguères, of about 300 000 ka, are from hominid remains themselves, rather than from other fossils of the fluvial sediment. This was possible because Falguères has developed a new method that does not require the removal of samples from the irreplaceable specimens. On the basis of the fauna, this would be a realistic estimate (Fachroel Aziz, pers. comm. Dec. 1996; cf. Bartstra et al. 1988).

While it is likely that the Ngandong remains are related to the much earlier Kabuh fossils (Weidenreich 1951), with so many shared archaic features, they are more appropriately assigned to archaic sapienoids. The evolutionary trends from Kabuh to Ngandong are broadly similar to those found elsewhere through the Middle Pleistocene. In the Ngandong hominids, the supraorbital torus is considerably reduced, their frontal bone is markedly broader, particularly across the frontal lobes, the mandible is projecting, the occipital region is more modern and the brain volume (Wolpoff 1997) is much greater than that of the Kabuh hominids (Kabuh: females 875 cm^3 [n=5], males 1032 cm^3 [n=2]; Ngandong: females 1093 cm^3 [n=2], males 1177 cm^3 [n=4]), and barely below Australian crania (females 1119 cm^3 [n=22], males 1239 cm^3 [n=51]) which are indisputably *H. sapiens*. Wolpoff, Thorne and others have argued convincingly that regional features have been confused with traits that are mistakenly taken to indicate evolutionary grade. Most particularly, the thick supraorbital tori and flat frontal squama Kabuh and Ngandong specimens share occur also in Australian fossils (WLH 18, 19, 45, 50, 69, Cow Swamp, Coobool Creek; Webb 1989). Thorne and Wolpoff (1981; cf. Larnach and Macintosh 1974) have compared Australian and New Guinea crania with Europeans and Africans, scoring them for the eighteen characters that Weidenreich had thought were unique for the Ngandong hominids. Six of these were absent in the modern samples, but nine features could be found and attained their highest frequencies in the Australian and New Guinean samples.

If the Ngandong and Sambungmacan hominids are *H. sapiens*, then the second set of 'dates' Swisher and colleagues have provided is no more 'sensational' than the first. The persistence or even emphasis of certain archaic features is not surprising either: Java was periodically sundered from Sumatra and the mainland by eustatic fluctuations, resulting in prolonged isolation of the population. As in the case of the 'classical' Neanderthals, which are more robust than the early or Levantine Neanderthals (cf. late 'robusts' and early 'graciles' in Australia), genetic isolation may well account for these effects. Neither the Ngandong hominids nor Neanderthals are *H. erectus*. The alternative and currently preferred explanation, that *H. sapiens* migrated to Southeast Asia, by-passing a resident population of the Greater and Lesser Sunda Islands that had been established three quarters of a million years earlier (see next chapter) to make its way to the Great Southern Continent is absurd. It is also clearly inconsistent with what its advocates, the 'Garden of Eden' proponents, have claimed occurred in the entire rest of the world: rapid and complete replacement.

Homo erectus on Flores

I now turn my attention to a specific region of central Flores, on the major island in central Nusa Tenggara (Figure 5). This region is located to the north-east of the town of Bajawa, roughly between the smaller towns of Soa and Boawea. It comprises essentially the upper drainage basin of the Ae Sissa (Ai Sisa, Rissa) River, which is about 20 kilometres wide and surrounded by several volcanic cones. For about twenty years from the 1950s to the 1970s, research by Theodor Verhoeven conducted on Flores and elsewhere in the archipelago focused especially on this region, and demonstrated the co-occurrence of very archaic stone tools and an extinct megafauna in a fossiliferous sediment facies exposed at a number of sites in central Flores. This work resulted in the publication of a series of scientific papers in the major journal *Anthropos*. Theodor Verhoeven first discovered remains of Stegodontidae on Flores in January 1957 (Hooijer 1957; Verhoeven 1958), near the abandoned village Ola Bula, and on Timor in August 1964 (Verhoeven 1964). In March 1957 he found stone artefacts, including blades and flakes, eroded out of the deposit at a surface site near Ola Bula (Verhoeven 1968: 400). He notified the Indonesian authorities, and Professors Wegner and Dyhrberg of the Museum Zoologicum Bogoriense were sent from Bogor, Djakarta, to examine the discovery. The resulting collection of Verhoeven, Wegner and Dyhrberg was then sent to Dr Hooijer in Leiden, Holland, for a more detailed analysis. Among these initial Flores finds, Henri Breuil recognised a number of Lower Palaeolithic stone artefact types (Verhoeven 1958: 265), and mentioned to Verhoeven that the Portuguese Antonio de Almeida had discovered similar lithics on Timor. Breuil fully accepted the presence of Lower Palaeolithic hominids on Flores in 1957. Von Koenigswald, on the basis of photographs sent by Verhoeven, realised the great biogeographical significance of the faunal remains (Koenigswald 1957). On the strength of the presence of tektites, he initially suggested an upper Middle Pleistocene age for the fossiliferous deposit (Koenigswald 1958: 44-46).

Hartono (1961: 20), of the Geological Survey of Indonesia, examined the area and found fossil bone fragments scattered on the surface, usually associated with what he thought were stone artefacts. His research suggested that the extent of the fossil-bearing sediment was considerably greater than Verhoeven had initially assumed. Hartono (1961: 19) found that it extends at least 50 square kilometres, which means that it covers much of the Soa Plateau.

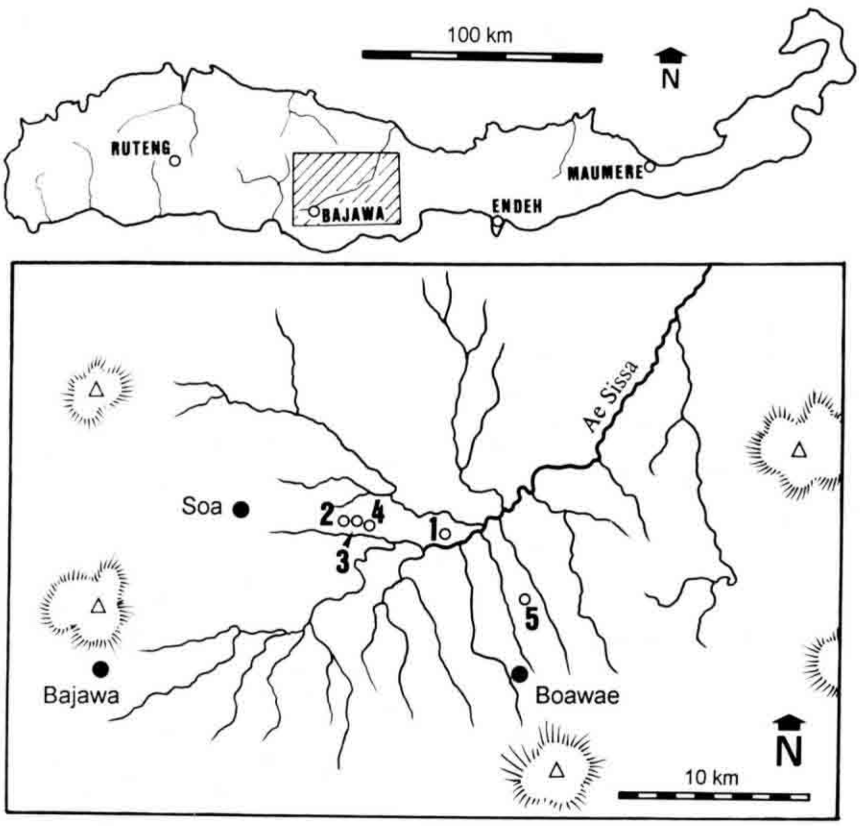

Figure 5. Flores, and the Soa Plateau, indicating the sites Ola Bula (1), Mata Menge (2), Lembah Menge (3), Boa Leza (4) and Tangi Talo (5).

In the northern summer of 1963, Verhoeven located further stone tools, *this time in situ*, directly in the layer containing the *Stegodon* remains at Boa Leza (Verhoeven 1968), thus confirming Breuil's view. *In doing so he demonstrated the coexistence of the fauna and hominids*, because the possibility that the two components had been mixed by fluvial action could be excluded on the basis of the description of the material, and also because the co-occurrence was not limited to a single site. He excavated at Mata Menge in 1965, where he also found artefacts in the fossiliferous stratum, later named the Ola Bula Formation. In 1968, while in Europe, he teamed up with Professor Johannes Maringer, a senior member of the Anthropos-Institut in Germany. The two scholars began working with three excavation teams in September 1968, at Boa Leza, Mata Menge and Lembah Menge.

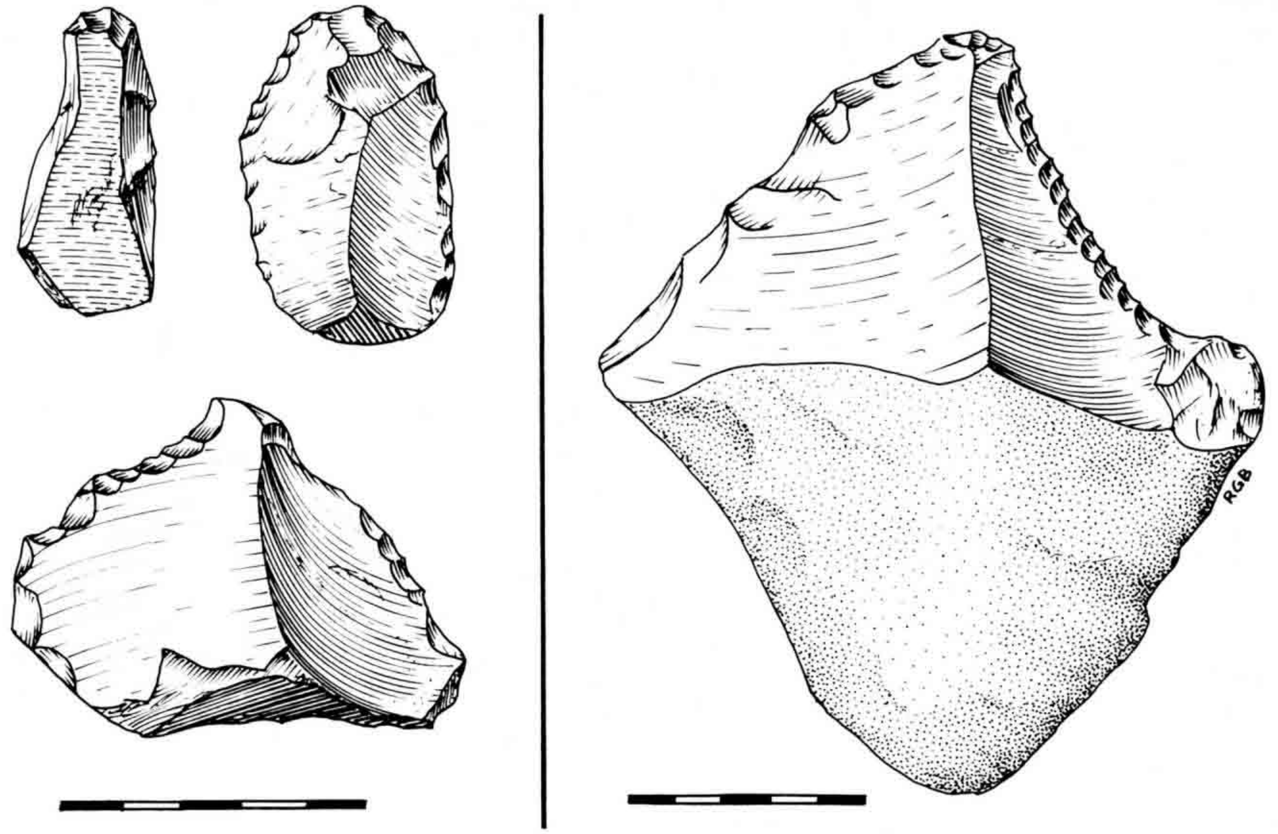

Figure 6. *Some Lower Palaeolithic stone tools from Ola Bula, central Flores. Scales in centimetres.*

The first Maringer and Verhoeven reports (1970a, 1970b) validated Verhoeven's observations completely and the initial article was accompanied by a further paper (Maringer and Verhoeven 1970c) which described surface finds of similar stone tools from the same region.

The geological sequence of the Soa plain was first described by Ehrat (1925), later by Hartono (1961). There are four principal facies: Ola Kile, a volcanic deposit sloping downwards about five degrees to the south, followed by the discordant, horizontal Ola Bula Formation, with a distinctive white tuffaceous sediment forming its base. This facies is a poorly consolidated sandstone, consisting mainly of plagioclase, with orthoclase, augite and hornblende, but poor in quartz. Only the lower part of the Ola Bula Formation, resting on the white tuff, contains the bone beds and artefacts, its two upper components contain leaves and molluscs. This formation, with an average thickness of about 80 metres (Hartono 1961: 7), is overlain by the third major facies, the Gero limestones, which has been subjected to considerable erosion. The uppermost facies consists of comparatively recent volcanic sediment, of which even less remains preserved.

The fossil horizon containing the *Stegodon* remains and the stone tools is just 1.0 - 1.5 metres thick, occasionally up to three metres. Subsequent to the deposition of the Ola Bula sandstone, the calcareous Gero Formation was formed during a period when the area was at, or even below, sea level (as indicated by the occasional presence of marine foraminifera), and before its uplift by between 200 - 400 metres during the later Pleistocene. This in turn led to the incision of river systems and the deposition of volcanic sediments. The geological as well as palaeontological contexts therefore seem to suggest a Middle Pleistocene age of the Ola Bula Formation. Hooijer (1957: 126) attributed the fauna to the Middle or Late Pleistocene. Heekeren (1975: 48-9) was more explicit, bracketing the fossiliferous and artefact-bearing stratum between 830 ka and 200 ka. Koenigswald preferred a greater age, 830 ka to 500 ka, and on the basis of the tektites eventually nominated 710 ka as the most likely age (Koenigswald and Ghosh 1973: 3-4; Ashok Ghosh, pers. comm. August 1996).

The Ola Bula fossil remains are dominated by *Stegodon trigonocephalus florensis*, an endemic subspecies (Hooijer 1957, 1972). One of its tusks from Boa Leza was 2.8 metres long, and the animal stood about three metres at the shoulder. Other remains from the deposit are of crocodiles and giant rats (*Hooijeromis nusatenggara*) (Musser 1981).

Maringer and Verhoeven (1970a, 1970c) compared the lithic industry from the Ola Bula Formation (Figure 6) with the traditions found in Java which are attributed to *Homo erectus*. Subsequent to 1968, they continued their efforts to locate further artefact material resembling the Javan Patjitan (Pacitan) on Flores, and reported one eroded assemblage from the Waiklau near Maumere (Maringer and Verhoeven 1972), and then an abundant similar industry at Marokoak (Maringer and Verhoeven 1975). More importantly, they also discovered a similar assemblage in the stratified deposit of a deep limestone shelter they named Liang Michael, where they excavated a trench of four by three metres, recovering 205 stone tools resembling Clactonian typology (Maringer and Verhoeven 1977). The steep slope in front of the shelter yielded a further 818 artefacts of the same typology. Finally, Maringer (1978) reports a remarkable single tool made of black opal from the Soa Plateau.

The fossiliferous layer in the lower section of the Ola Bula sandstone comprises two definable horizons, an upper, silty part and a lower, sandy component. The lithics from the upper part are of fresh appearance and have sharp edges, whereas those from its sandy, lower part exhibit moderate rounding due to water transport. The *stegodon* remains show similarly differential wear, so the two types of finds may have been redeposited in the lower part of the layer, but have probably experienced very little if any transport in the upper. Here, many osteal remains were found in articulation, e.g. limbs together with pelvis, or connected vertebrae. Fluvial mixing of components from different chronological units is therefore not a valid interpretation, and this is amply clear from Maringer and Verhoeven's (1970a) report. Recent excavations also confirmed their claims that the stone tools occurred among the fossils, sometimes even in direct contact with bone specimens (Morwood et al. 1997). Hence there is ample evidence that the makers of the stone tools shared the island with the Stegodontidae, and most probably included them in their food menu.

Subsequent to the death of Maringer (1902-1981), the age of the fossiliferous stratum in the Ola Bula Formation as suggested by Koenigswald and others was eventually confirmed in 1991-1992, when the work of Sondaar (1984, 1987) and others (e.g. Musser 1981) led to an attempt to date the fossiliferous facies palaeomagnetically (Sondaar et al. 1994). A series of nineteen samples from two sections was processed, and what appears to be the Matuyama-Brunhes reversal to normal polarity (730 ka BP) was found to occur 1.5 metres below the artefact-bearing deposit at Mata Menge. At that site, the Ola Bula Formation is 23 metres thick, and the magnetic reversal occurs near its base (Aziz 1993; Morwood et al. 1997). The great number of stone artefacts from the various sites that has been recovered between 1957 and 1997 implies that, by about 700 ka ago, the island was well occupied by hominids, and there can be no doubt that they were *Homo erectus*.

Of particular interest in the question of the hominid occupation of Flores is Tangi Talo, a site on the Soa plain, but closer to Boawae than the site complex between Mata Menge and Ola Bula, which is spread over a distance of some five kilometres. Its fossiliferous stratum appears to be about 900 ka old, as it is apparently of the Jaramillo normal polarity period (Sondaar et al. 1994). Its fauna is distinctly endemic: Pygmy *stegodon*, a giant tortoise (*Geochelone atlas* Falconer and Cautley; Hooijer 1971) resembling that of the Galapagos Islands, and the Komodo dragon which still survives on the island to the west of Flores. It has been suggested that the latter was a specialised predator of the tiny *Stegodon* which was little more than a metre tall (Diamond 1987b). In view of the uniformity of the faunas in the Ola Bula Formation, it is possible that the earlier Tangi Talo fauna was adversely affected by the arrival of *Homo erectus*, which could have led to, or hastened, the demise of the tortoise and the pygmy proboscidean (Sondaar 1987). Most important, however, is the complete absence of stone tools in the fossiliferous stratum at Tangi Talo (Sondaar et al. 1994: 1258). Perhaps this provides a *terminus post quem* indicator for hominid presence on Flores, although this is by no means necessarily so.

Research at the sites, which by now include besides Ola Bula, Boa Leza and Mata Menge also Lembah Menge, Dekoweku, Dozo Dhalu, Dozu Sogola, Kobatuwa, Nagerowe and Ngampa, continued in 1994 (Lumbanbatu and Aziz 1994; Aziz 1996). But all of this knowledge about the Pleistocene occupation of Flores remained largely unknown in Anglophone archaeology, partly because the principal publications prior to 1994, in *Anthropos*, were in German and thus ignored, while the few English reports had appeared in comparatively obscure publications (Bednarik 1997b). Although Flores does receive a mention in a few Australian syntheses on Wallacea's archaeology during the 1970s and 1980s, the Maringer and Verhoeven reports were misunderstood and it was mistakenly assumed that *no stratigraphical association* between the Stegodontidae and the stone tools *had been observed* (Bednarik 1997b). Realising that this was a very crucial issue, not only for the region's archaeology, but for hominid history generally, I explained the misunderstanding and its effects (Bednarik 1995c, 1995d, 1995e), with the result that one respondent chastised me for being critical. Groves (1995), while admitting that many of the points I made were valid, stated that the claims by Maringer and Verhoeven should have been checked. This is not a good enough explanation for ignoring them entirely *in all Australian archaeological literature* from the 1960s to the mid-1990s, and developing and disseminating complex models of various types that would have been rather pointless had the Flores data been taken into account (such as the language origins hypothesis of Davidson and Noble 1989; Noble and Davidson 1996: 184).

However, a major project initiated by an Australian archaeologist, with Indonesian and Dutch colleagues who had worked in Flores for many years, is now planned for the Soa plain (Morwood et al. 1997), after Morwood recorded stratigraphic cross-sections at Mata Menge in January 1997. First results indicate that all the crucial claims made over the past forty years are once again being substantiated, as they have been before every time they were investigated. Australian researchers have examined stone tools from Flores, some still in their sandstone matrix (Bednarik 1997a: Fig. 3). Australian sceptics of this model have spontaneously conceded that there can be no doubt about the artefact status of many of the lithics excavated from the Ola Bula Formation, when examining specimens in December 1996, at Monash University. Fission track, OSL and ESR dating of sedimentary deposits and animal tooth enamel from sites in the Ola Bula region is currently being planned or undertaken (Morwood et al. 1997). This may result in some adjustment of the age estimates for the advent of hominid presence on Flores, probably slightly upwards into the final Early Pleistocene, but there is nothing in the preliminary results of this current work that could be expected to refute any of the crucial propositions of Verhoeven, Maringer, Breuil or Koenigswald.

Seafaring and colonisation

In addition to Flores, evidence of very early, often similar stone tools has also been reported from other Wallacean islands, notably Timor (Verhoeven 1964: 634; Glover 1973; Glover and Glover 1970), Sulawesi (Heekeren 1957: 47-54) and Ceram (Hadiwisastra and Siregar 1996). In some cases, especially in Timor, these tools also seem to occur in stratigraphical context together with Stegodontidae and other extinct fauna. Although dating is not yet available in these instances, there is a strong probability that *Homo erectus* not only occupied Flores and, presumably, Lombok, but many other islands in the Indonesian arc to the east of Flores. If they reached Timor they would have been poised on the very doorstep of Australia, possessing a maritime technology that enabled them to undertake successful colonisations across open stretches of sea, beyond visual contact with the land. This provides not only a totally different context for the colonisation of Australia and New Guinea than that so far considered by archaeologists, it also confirms my argument (based on quite different types of evidence) that the technological, social and cultural capabilities of *Homo erectus* have been massively underestimated throughout the 20th century (Bednarik 1993a). His many achievements would have been quite impossible without the use of an advanced communication system. At the time of writing, mid-1997, there is still no interest in re-examining Verhoeven's (1964) report of the occurrence of Lower Palaeolithic stone tools together with *Stegodon* remains at three locations (Hedibesi, Wéaiwé and Fulan Monu) on Timor. Verhoeven excavated there with teams of up to forty people, and he describes a geological profile of five facies. The fossiliferous deposit is a conglomerate, most probably calcareous, and overlain by white limestone strata. These would be absolutely superb conditions of preservation, similar to those of some of the best preserved open air Lower Palaeolithic sites in Europe and Africa. And yet, a third of a century after Verhoeven wrote: 'Since I dutifully reported our finds to Djakarta, Bandung and Djogjakarta, others will soon continue the research', his reports probably still gather dust, and no archaeologist has bothered to pursue the matter. In a brief note on stone tools from Timor, Glover and Glover (1970) mention several archaic surface finds from the area Verhoeven had excavated in, but misunderstanding his report they imply that there is no stratigraphic evidence available, in the same way they think that the *Stegodon* fossils on the Soa plateau were 'apparently' not found 'in the same area as any of the stone tools mentioned by Mulvaney and ourselves' (Glover and Glover 1970: 189). The tools Mulvaney mentions, and describes without hesitation as 'well-trimmed flakes and large core tools, including possible cleaver-like forms', bearing 'the Patjitanian stamp' (Mulvaney 1970: 186), were not just found in the same area, or in the same section. They were consistently found in the same narrow stratum, and where they had not weathered out they were apparently never found anywhere but in that one stratum, the lower part of the Ola Bula Formation.

From what we have seen so far it seems perfectly possible that *Homo erectus* reached Timor, and it seems that Flores was very successfully colonised by that hominid. This raises, initially, the questions of seafaring and of colonising islands generally, and the 'island continent' specifically, through seafaring. In the present context it is worthwhile examining these subjects briefly.

In considering the long-term success chances of small colonising groups by simulation modelling, McArthur et al. (1976) showed that the odds would rise rapidly with even a modest increase in the number of individuals involved. Bearing in mind factors such as (presumably high) death rates, proportions of male to female offspring, incest taboos and marriage rules, it is realistic to assume that before a colonisation actually succeeds, many previous attempts may have ultimately failed, even if the population concerned managed to persist for a few generations. Such unsuccessful attempts are likely to be almost invisible in the archaeological sense, as the methods of that discipline are likely to record human presence only after a population has established itself reasonably well. Individual castaways or very small groups may well perish even if they reached land safely, or never in their lifetimes established contact with other such groups — especially in the vastness of the Australian continent.

However, once a viable population is established it may increase quite rapidly, especially under favourable ecological conditions, and expand to occupy a region the size of Australia within a few millennia (Birdsell 1957). New arrivals could then have injected valuable additional genetic material into the established population from time to time. If we consider the evidence offered by mitochondrial DNA analyses from Sahul relevant, it would suggest an input of at least twenty or so females (Stoneking et al. 1986).

The most parsimonious scenario is therefore that in both specific cases considered here, Flores and Australia, the earliest known archaeological record of human presence refers to well-established populations rather than occasional castaways or very short occupations by small groups. This is emphasised by the possibly lower sea level at the time, at which the earliest known Australian occupation sites could have been far inland. In both cases the initial settlement can be assumed to have been by people with a predominantly coastal economy, who would be likely to settle primarily the coastal regions initially. Therefore the known ages of first occupation need to be considered as conservative, and actual first landfall may have occurred considerably earlier.

Until recently there was a persistent assumption that, whenever the first landfall in Australia occurred, it was probably at a time of lowest sea level, which shortened the distance to be travelled considerably. However, recent commentators have taken the opposite view (e.g. Chappell 1993; Butlin 1993), suggesting that the benefits of travelling at a higher or even very high sea level could have compensated for the greater distance. A higher sea level would have favoured the crucial north-western monsoon, and the greater extent of flat coasts and their richer food resources would have provided an advantage over the steeper and rockier coasts of lower levels. Similarly, people occupying flat coasts with lagoons, mangrove swamps and coral reefs were most likely to develop maritime technology, which is a key requisite for achieving ocean crossing ability.

Although it is clear that watercraft have a long history we have no direct physical evidence for them from the entire Pleistocene, nor any identifiable depictions of such craft in that period's art. The oldest archaeological evidence for such equipment is all from Europe, consisting of the Mesolithic paddles found in peatbogs at Holmgaard (Denmark) and Star Carr (Yorkshire, England) (McGrail 1987, 1991; Clark 1971: 177). The Star Carr remains date from about 9500 years BP. Ellmers (1980) has proposed that a worked reindeer antler from the Ahrensburgian site of Husum (Schleswig-Holstein, Germany) is a boat rib of a skin-boat. If this were accepted, the find would be the earliest direct physical evidence of watercraft in the world. The Ahrensburgian is an Epipalaeolithic tool tradition of the very beginning of the Holocene, c. 10 500 BP. The most ancient boat we know of is the canoe from the peat of Pesse (Holland) (Zeist 1957), which is 8265 $\pm$ 275 years old according to its recently recalibrated radiocarbon date (Bednarik 1997c). Other very old boat finds are those from Noyen-sur-Seine (France), which has yielded a radiocarbon age of 7960 $\pm$ 100 BP, and Lystrup 1 (Denmark), at 6110 $\pm$ 100 BP (Arnold 1966). Indirect evidence for ocean navigation in Europe is the occupation of the Greek island Kefallinía by Middle Palaeolithic people (presumably Neanderthals) which involved a sea crossing of perhaps six kilometres (Warner and Bednarik 1996), and the much later presence of obsidian from the island of Melos in Frachthi Cave at about 11 ka ago (Perlès 1979; Renfrew and Aspinall 1990). Islands to the west of Italy, too, were apparently occupied in the Palaeolithic period. There have been occasional suggestions that some hominids were able to navigate the Strait of Gibraltar, from Morocco to Spain, even that this may have been possible to *Homo erectus*. However, this was based on pure speculation concerning the dispersion of hominids from Africa. It has also been suggested that there was a land-bridge from Tunisia to Sicily at the time of *H. erectus*. The most significant and best documented early sea crossings, however, are those that led to the occupation of Australia and several nearby islands, which clearly occurred in Pleistocene times.

In the absence of any archaeological information about the type of watercraft used in the Ice Age we have to resort to ethnographic information and logical reasoning. The first ethnographic sources to consider, naturally, are those of Australia and New Guinea, the final destinations of the greatest of the very early sea voyages. All known Australian watercraft observed in recent centuries were small and not suitable for prolonged sea travel. In southern Australia, particularly the south-east, we have many reports of bark canoes (Massola 1971: 99, 110) which were very effective, but only on calm waters and for short distances. The Tasmanians used driftwood or simple rafts from bundles of bark lashed together which became waterlogged after a few hours. These watercraft were not taken more than 5-8 kilometres from the coast (Jones 1976, 1977). More seaworthy were the rafts made of several mangrove logs and used on the north-western coast of Australia. They were paddled along the coast over a distance of 8-16 kilometres (Flood 1995: Plate 2). The use of logs to cross several kilometres to the many islands of the Dampier archipelago was observed by some of the earliest Europeans to see the region. Mangrove or driftwood rafts were also observed at Bentinck Island in the Gulf of Carpentaria. These tended to become waterlogged and it has been reported that the death rate even on these comparatively short journeys could be exceptionally high. Tindale (1962) recorded an average death rate of 50 per cent on two sea voyages of about 13 kilometres each.

The recent bark canoes of northern Australia were perhaps the most seaworthy ethnographic watercraft of the continent. With a capability of carrying six to eight people and at up to 5.5 metres length, these vessels were generally used for distances of up to 10 kilometres. However, Flood (1995) mentions that one journey of 32 kilometres has been recorded off Arnhem Land, from the Sir Edward Pellew Islands to Macarthur River. Besides bark canoes and mangrove wood rafts, trunks of pandanus, palm trees and bundles of bark were also used in northern Australia for the construction of watercraft. Although surprisingly long journeys were occasionally undertaken in quite small vessels, it must be remembered that distances separating people such as those on Bathurst, Melville, Bentinck and Keppel Islands from the mainland, of around 10-15 kilometres, were sufficient to effect partial or near-total isolation of these populations from those on the mainland, which can be observed in both the material cultures and genetic markers.

All commenting authors seem to agree that the ethnographically known Australian vessels are probably not of the types one would expect the initial landfall in Sahul to have occurred with. Since considerable navigational ability is inferred by the journeys required to reach the continent, why was the inferred technology not preserved?

Figure 7. Reconstruction of Homo erectus *constructing a bamboo raft. (Art work provided by Peter Welch.)*

strips of black-palm bark, which often supported a bough shelter and clay hearth. The rafts were lashed together with a split forest vine (*kanda* cane). Such craft were made only with stone tools, but they could probably survive the rigours of moderate seas over a considerable period of time. Jones thinks that they would withstand a strait crossing of perhaps scores of kilometres, given suitable combinations of weather and current.

Birdsell (1977), the first to consider the initial occupation of Australia in great detail, already recognised that none of the ethnographically recorded Australian watercraft would have been suitable for the major ocean crossings of the Pleistocene, and considered bamboo rafts as the most likely vessels used (Figure 7). Thorne (1980, 1989) not only agreed with this point, he conducted an experiment by constructing a small bamboo raft in two hours, using as a model contemporary rafts seen in the South China Sea. He reports that this was surprisingly easy to steer and that he achieved a speed of four to five knots (8-9 km/h). A subsequent computer simulation experiment suggested that during the northwestern monsoon of the wet season, a raft setting out from Timor would reach the Australian coast within seven to ten days, even without using a sail.

The early seafarers reaching Australia had at their disposal the materials provided by the vegetation of Wallacea, most of which would have been lacking in Australia. For instance bamboo is widely available in South-east Asia, but its occurrence decreases across Wallacea and in Australia only a few species are found that grow as small isolated pockets on the coastal plains of northern Australia, notably in Arnhem Land (Jones 1989). They are mostly thin-stemmed species such as *Bambusa arnhemica*. Hence it seems likely that the maritime technology of Australia is an adaptation developed in response to local conditions, specifically to locally available materials. It may therefore reflect some specific technological traits that were preserved over a long period of time, but otherwise bear little resemblance to what was used in reaching the continent.

Perhaps a better idea of what the earliest seaworthy watercraft may have been like can be gained from some ethnographic examples in New Guinea. Jones (1989) describes rafts on the Sepik River, 8-10 metres long and 4 metres wide, which carried people and goods downriver for distances exceeding 100 kilometres. They were made from wooden poles arranged as four criss-crossing layers. This structure was capped by a deck made from

The First Sailors: a project of experimental archaeology

Repetitio est mater studiorum —
repetition is the mother of science.

Nautical archaeology has a distinguished history in the area of experimental archaeology, with its perhaps most famous example being the journey of Thor Heyerdahl's *Kon-Tiki* exactly half a century ago. This balsa raft was sailed from Callao, Peru, to Raroia Atoll in the Tuamotu Archipelago in 1947, to establish whether it would have been possible for Amerindians to have reached Polynesia (Heyerdahl 1948).

The First Sailors project intends to attempt the replication of some of the earliest sea travel in Indonesia, the region where ocean navigation probably originated. Preparations for this project are well under way, having commenced in 1996. In 1998 they are expected to culminate in an attempt to cross from Indonesia to Australia, using a rudimentary raft made and equipped entirely with Middle Palaeolithic tool replicas. It is to be launched from Roti (Rote), where preparations for its construction commenced in mid-1997 (Bednarik 1996c). The island of Roti, immediately to the south-west of Timor, was already during the Last Interglacial of a similar elevation as today: coral reefs of that age are about five metres above present sea level (Wahyoe Hantoro, pers. comm. Dec. 1996). The same project also considers the conditions under which Wallace's Line, between Bali and Lombok, could have been crossed 800 ka ago, and thus illustrate the level of marine technology possessed by *Homo erectus* at that time (Bednarik 1996c). Therefore the project will trace the first traversing of two important sea barriers by colonising hominids, and recreate the first landfall in Australia.

The rationale for this expedition is essentially scientific: we know that these Pleistocene sea crossings took place, but we do not know under what conditions the first known sea travel by colonising humans might have occurred. This project attempts to examine these conditions, the difficulties involved, and the minimum level of technology required to succeed. This in turn is expected to tell us more about technological competence of the original seafarers than archaeological excavation or armchair archaeology are likely to reveal. There are quite simply no traditional archaeological means available to us to establish how these human feats might have been accomplished — in fact we have no physical evidence at all of Pleistocene watercraft, as noted in the previous chapter. This leaves us with only one viable alternative: logic. We do know that the crossings did occur, hence they must have been possible. We can reasonably assume that they did not occur earlier because the technology was unavailable, so the first crossings might coincide with the most rudimentary conditions allowing a crossing to be successful. The most parsimonious model would stipulate that the most suitable material was used for the rafts, and there seems to be general agreement that it was most likely bamboo. This has the added benefit of explaining why the available technology was not preserved and further developed by the new population in Australia: the required raw materials were simply unavailable there, supporting only a more impoverished seafaring tradition. Many watercraft of Australia and New Guinea, even very small vessels, carried fire, so it is quite likely that hearths were aboard early ocean-going rafts. Middle Palaeolithic humans can be assumed to have placed some value on the possession of fire. It is also likely that simple bough shelters were incorporated in raft designs, similar to what was used on the Sepik River rafts. Possible evidence of shelter construction has been reported from a series of Lower Palaeolithic sites in Europe, Asia and Africa (e.g. Terra Amata, Bilzingsleben, El Greifa, Bhimbetka), especially from the Acheulian, so it can be safely assumed to have been at he disposal of people a mere 60 or 100 ka ago.

Two other details are more difficult to speculate about: how was the raft actually constructed, and did it carry a sail? Here, the most parsimonious answer would be: since the crossings did not occur before they first occurred, the requisite technology may have been comparatively new, and the crossings were just at the very limit of the humanly possible at the time in question. In other words, if the crossing was humanly possible without a sail, then it ought to be undertaken without one. If it is thought to be unlikely to succeed in this form, then we need to assume that a sail was used. Similarly, the construction of the raft was no doubt simple, but it clearly had to be adequate for the purpose. It would seem, judging from the design of contemporary rafts, that a horizontal arrangement of bundles of bamboo was the most likely basic structure used. Since the cargo would push a good part of this substructure below the water, it is likely that some form of light-weight cross-layered decking was added above the actual floaters, which in any case had to be tied together *very* effectively.

The next fundamental requirement was the size of the raft. This depends on such variables as number of crew, weight of cargo (earthen hearth, fuel, water and food for the period required) and maritime design issues. Naturally the journey could be attempted with a minimal number of people, even one person, but the most parsimonious scenario demands that the crew succeeded in founding the population of a continent. This immediately raises the question: was there one large vessel involved, or were there a number of small ones, either at the same or at different times. The second scenario enjoys some statistical advantages, but at the cost of reducing the founding population dangerously (through the potential loss of individual contributors to the gene pool). Once a group had landed, it would have been unlikely, in the vastness of the continent, to make random contact with some other group that reached the shore in some other part of the coast. In genetic terms, the chances of successfully founding a new population are dramatically reduced for very small groups (McArthur et al. 1976), and Jones (1989) suggests that a successful group would probably have to consist of several individuals of both genders. Even then there is a risk of recessive genes becoming effective, of a lack of genetic variability threatening the survival of a new population. There can be little doubt that there were subsequent landfalls (Webb 1989), which would have been most important in injecting new genetic material, but they may have occurred many millennia later. I have made the assumptions that a crew of under six people would have been very exposed to the possibility of ultimate failure to found a population, while a crew of more than a dozen would presumably require an excessively large vessel.

The Middle Palaeolithic people who reached Sahul would have been experienced seafarers, whose ancestral knowledge was derived from 700 000 years of navigation. That experience would have taught them that the amount of work to build one large craft was not greatly different from building an adequate flotilla of small ones.

There are no doubt valid counterarguments, but *The First Sailors* expedition opted for one single vessel, and our reasons are probably not so very different from those of the Middle Palaeolithic shipwrights. Even in relation to some aspects of motivation, the concept of replication may already be relevant.

These were the basic guidelines in designing the project, in that they determined size and design of the experimental raft. Experimental archaeology has been used quite extensively for all archaeological periods beginning with the Upper Palaeolithic (i.e. beginning about 35 ka BP), but for the two earlier periods, which were the most crucial in the development of hominids and human culture, such experimentation has been very limited to date, and on only the most modest scale. *The First Sailors* involves the greatest archaeological experiment ever attempted that relates to the time of early humans (the period from 2.5 million years ago to 35 ka ago, i.e. 98.6% of the entire existence of human beings). The project offers opportunities for a variety of scientific work, especially observation of the practical application of many technological, behavioural and cultural assumptions about hominids. An important component of the expedition will therefore be the systematic recording of archaeologically relevant details of both the preparations and the actual crossing attempt. It can be expected that this work will result in a considerable improvement in our knowledge of the conditions under which maritime technology initially might have developed, and how Wallacea and Australia were first colonised.

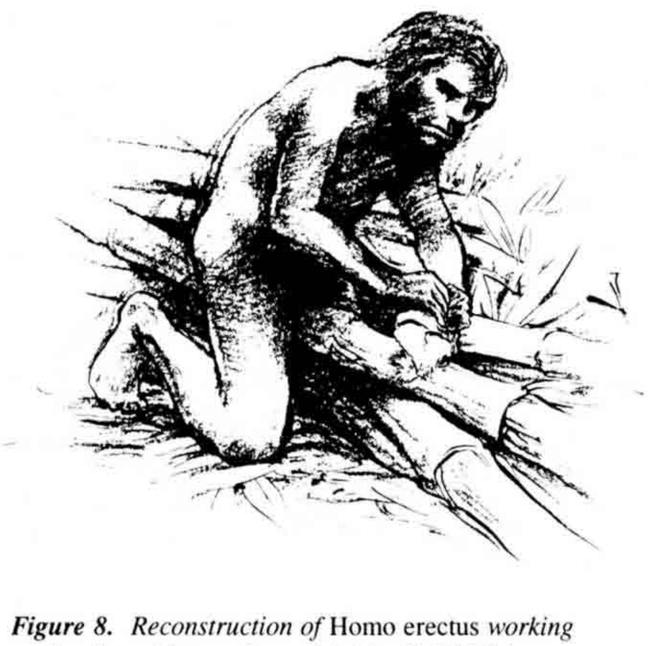

Figure 8. *Reconstruction of* Homo erectus *working bamboo. (Art work provided by P. Welch.)*

Lower Palaeolithic technology

The value and relevance of the planned expedition just described as well as our understanding of hominid capabilities in ocean navigation depend greatly on a most comprehensive understanding of Lower Palaeolithic technology. Practically all publications about very early technology deal primarily with stone implements, which is a result of taphonomically imposed limitations. In reality, stone tools were always a numerically minor component of early material cultures. This limits our knowledge of technology very significantly. It should include not only the use of non-lithic materials, but also the questions of procuring all materials used, their transport, curation, storage, processing, preparation, manufacture and maintenance.

The very significant under-representation of artefacts from relatively perishable materials has prompted distorted technological characterisation of Lower Palaeolithic traditions. For instance, bone, ivory, fibre, leather or wood are poorly represented, if at all — although there are in fact more wooden finds from the Lower Palaeolithic than from the Upper Palaeolithic (consider the Kalambo Falls Acheulian implements and the many other finds listed below). The technology of Lower Palaeolithic wood working has never been examined in a consistent and comprehensive fashion, even though we know that the period's stone tools were primarily used to work wood (Keeley 1977). The same applies to the Middle Palaeolithic (Beyries 1988). For instance, microwear studies by Anderson-Gerfaud (1980, 1990) of lithics from Pech de l'Azé, Corbiac and other sites showed that only about ten per cent were used for working hides, while the majority served to fashion wooden objects. Shea (1989), examining tools from Qafzeh and Kebara, found that in all collections, evidence of woodworking predominates, followed by hafting traces. There can be no doubt that astronomical numbers of wooden tools and weapons were made before the Upper Palaeolithic, but almost none survived from the Middle Palaeolithic. From the Lower Palaeolithic, we have a minute sample, but even this has not been considered in a collective technological perspective. It almost seems that lithocentric archaeologists do not wish to know about the bulk of early technology. In the context of a project such as *The First Sailors*, however, it is imperative that this be examined carefully. Besides the stone tools that would be useful in making rafts, other implements and weapons also need to be considered, as well as water vessels, food preparation and general survival skills.

A highly relevant example of woodworking from the Lower Palaeolithic is the Acheulian plank of willow wood, shaped and bearing anthropic polish, from Gesher Benot Ya'aqov, Israel (Belitzky et al. 1991; Bednarik 1991). It is of the Middle Pleistocene and at least 240 ka old. The fragment is 25 centimetres long and broken at both ends. Only one side bears polish, which is quite flat except for a slight convexity along one edge. This surface transects the grain of the wood at a low angle.

The probably older yew spear point from Clacton-on-Sea, England (36.7 cm long, found in 1911), and the complete spear found among the ribs of an elephant skeleton at Lehringen, Germany (Jacob-Friesen 1956), have long been known. The Lehringen spear is also of yew wood, with a fire-hardened point, and measures

2.38 metres reassembled. (Actually, *two* wooden staffs were found in the Lehringen skeleton, but one was not saved.) Yew wood is particularly suitable for making spears, but the three hunting spears from Schöningen were carefully fashioned from spruce wood (Bednarik 1996d). They are 1.82 - 2.30 metres long, of maximal 3 - 5 centimetres diameter, as carefully balanced as modern javelins (the point of gravity being one third of the length from the point), and the lower end of the stem, which is the harder, has been shaped into the point. These are aerodynamically designed, sophisticated hunting weapons, and they are about 400 ka old. Schöningen has also produced another five wooden artefacts (Thieme 1995; Bednarik 1996d), among them two notched staffs which are thought to have been hafts for stone flakes. At 400 ka age, they would be the earliest evidence of hafting in the world. There was also a flat wooden artefact found embedded among the remains of a butchered animal which is thought to be from a lance. An even longer apparent wooden lance (2.5 m length, 3 cm diameter) comes from the travertine deposit of another German site, Bad Cannstatt (Wagner 1990). A fragment of a Lower Palaeolithic wooden lance or spear was found at yet one more German site, Bilzingsleben, a site that yielded also other wooden fragments. Finally, possible wooden lances (Howell 1966: 139) were found among the many elephant remains of Torralba, Spain, most of them early this century, but details are fairly sketchy. To appreciate the full wealth of wooden remains available from some Lower Palaeolithic sites it is useful to reflect that over 200 have been recovered from Gesher Benot Ya'aqov, and several thousand at Schöningen. Among the latter, thirteen tree species were identified, one of which is represented by 2826 fragments (Bednarik 1996d). Wooden remains are less common from the subsequent Middle Palaeolithic, but we have a thin, worked and stone tool-shaped plank of mulberry wood from Nishiyagi, Japan (Bahn 1987); a curved wooden implement with parallel markings on the end from Florisbad, South Africa (Volman 1984); and several shallow wooden dishes from the Mousterian in Abri Romani in Catalonia, Spain.

In addition to having provided the earliest known apparent evidence of tool hafting, German archaeologists have also found the earliest solid evidence of resin use for stone tool hafting. The Mousterian of Königsaue and Kerlich has provided not only resin fragments, but also resin with imprints of both wooden haft and stone tool, as well as the complete hafted tool (Mania and Toepfer 1973). Middle Palaeolithic hafting resin was also found in the Bocksteinschmiede, Germany (Bosinski 1985), and at Umm el Tlel, Syria (bitumen on two tools; Boëda et al. 1996). Moreover, Hayden (1993) describes the indirect evidence of hafting on Levallois and Mousterian points as 'copious', and the tanged Aterian tools of northern Africa were apparently designed specifically for hafting.

Of particular importance to *The First Sailors* expedition is the question, were barbed harpoons available to Pleistocene seafarers. Many European archaeologists think that the harpoon first appears in the mid-Magdalenian, i.e. towards the end of the Upper Palaeolithic. Perhaps that is true for Europe (probably not), but it is certainly not for Africa and Asia. The seven beautifully carved bone harpoons from Katanda, Zaire (Figure 9), are between 150 ka and 50 ka old, according to TL, OSL, ESR, racemisation and uranium series analyses. Their most probable age is probably less than 90 ka, and they come from a Middle Stone Age industry (Brooks et al. 1995; Yellen et al. 1995). The even more sophisticated bone harpoon I have described from Lohanda Nala, India, falls between 25 790 ± 830 BP and 19 715 ± 340 BP (Bednarik 1993b). More relevant in the present context is the barbed bone harpoon from Ngandong, Java (Narr 1966: 123), although it is undated. Nevertheless, if Pleistocene hominids on Java had harpoons, there is no strong reason to assume that their contemporaries on Timor did not possess them.

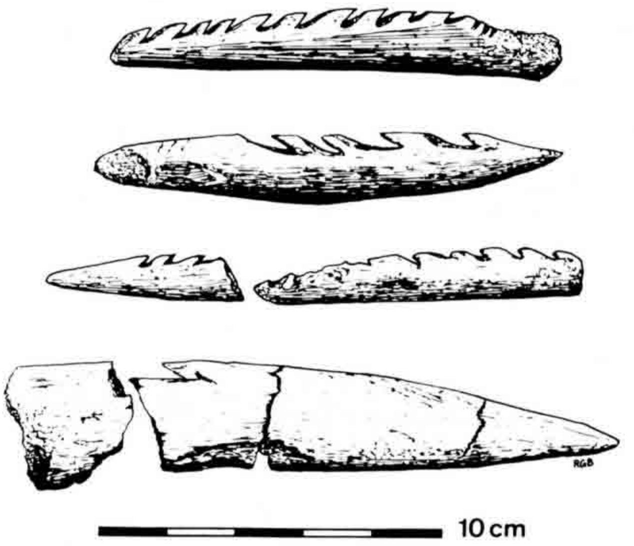

Figure 9. *Carved bone harpoons from Katanda, Zaire, of the Middle Stone Age.*

There is also a misapprehension among some archaeologists that bone points, and the skilled use of bone, ivory and antler generally, do not appear before the Aurignacian. This is also incorrect. Salzgitter-Lebenstedt, a German Micoquian site, provides ten bone points, mostly on mammoth ribs, besides the delicate and complex 'winged point' and an antler implement (Tode 1953). The polished Bilzingsleben ivory point is not just Lower Palaeolithic, it even seems to bear an engraving (Bednarik 1995a). Ivory points occur also in the Acheulian, for instance at Ambrona, where Howell and Freeman (1982) suggested that they may have been hafted. Even bifaces ('handaxes') have been made from bone, e.g. the specimen from Rhede, Germany (Tromnau 1983). During the Mousterian, bone was used widely, including for the building of dwellings (at Starosel'e), a use some archaeologists think was restricted to the Upper Palaeolithic. The carved harpoons already mentioned illustrate the competent working of bone and similar materials adequately, and the technology required has been available to hominids for hundreds of millennia.

A form of technological evidence that does not immediately seem related to seafaring, but is certainly related to my ultimate topic, language beginnings, is subterranean mining. It does, however, share some aspects with ocean navigation: the exploitation of a threatening environment that can only be traversed with the help of technology, the consideration and taking of the risks involved, the need for 'conscious' reflection and decision making, forward planning, and the use of complex material culture (i.e. tools and materials that involve considerable planning, diversity of sources and different technologies). So far, we have not secured any traces of underground mining from the Lower Palaeolithic, but this form of evidence is very difficult to recognise. Nevertheless, there is adequate indication of this available from Middle Palaeolithic traditions. Subterranean chert mining has been observed in Upper Egypt, at sites of the Middle and early Upper Palaeolithic (Vermeersch et al. 1989), and in one case illustrates the ability to predict the concealed continuation of a gangue stratum (Bednarik 1992b). Chert mining has also been reported from Europe, from dark underground caves, apparently dating from the Mousterian, at Bara Bahau in France (Bednarik 1986), Budapest-Farkasrét in Hungary (Gábori-Csánk 1988) and Löwenburg in Switzerland (Marshack 1989). In Australia, with the Middle Palaeolithic technology continuing to the end of the Pleistocene, underground chert mining evidence has been reported from nine deep limestone caves, and at least some of it seems to date from the Pleistocene (Bednarik 1986, 1992b). Again, the ability to predict the course of a geological stratum is demonstrated (in Gran Gran Cave, South Australia). There are also numerous 'ochre' mines in Australia, some of which are so large that they were very probably used during the Pleistocene (Bednarik 1995b). Large ochre mines have also been described in South Africa, especially Tsantsabane, where in the order of 73 000 tons of specularite was mined (Bednarik 1995b), and Lion's Cavern, where a carbon date of about 43 ka was obtained for the mining evidence (Beaumont and Boshier 1972).

A form of material evidence that is crucial for seafaring is the use of cordage. Strings, ropes and thongs were no doubt used for much of the Palaeolithic, but we have no physical evidence of knots and almost none of cordage (Leroi-Gourhan 1982; Nadel et al. 1994). The use of hunting nets has been suggested for the Gravettian of Pavlov, Czech Republic, after the impressions of weaved plant fibres were observed on burnt clay surfaces of 26-25 ka age (Pringle 1997). Warner and Bednarik (1996), in reviewing the issue, traced the assumed use of cordage back through its depiction in Upper Palaeolithic art (Figure 10) and the much earlier occurrence of drilled objects such as beads and pendants, and via other indirect evidence. This indicates that some form of strings must have been in use during Lower Palaeolithic times already. However, the evidence for the use of naturally perforated objects is very tenuous indeed, although it does extend to the Lower Acheulian. Artificial perforation of small objects suitable as beads or pendants appears only about 300 ka to 200 ka ago, according to current knowledge (see next chapter). However, it seems almost impossible to construct a raft without the use of rope-like materials, such as vines. Generally, it would be unreasonable to assume that ocean navigation could have been feasible without such materials, so it would appear that the earliest evidence we can find for marine navigation would coincide with the earliest indication of the use of some form of cordage.

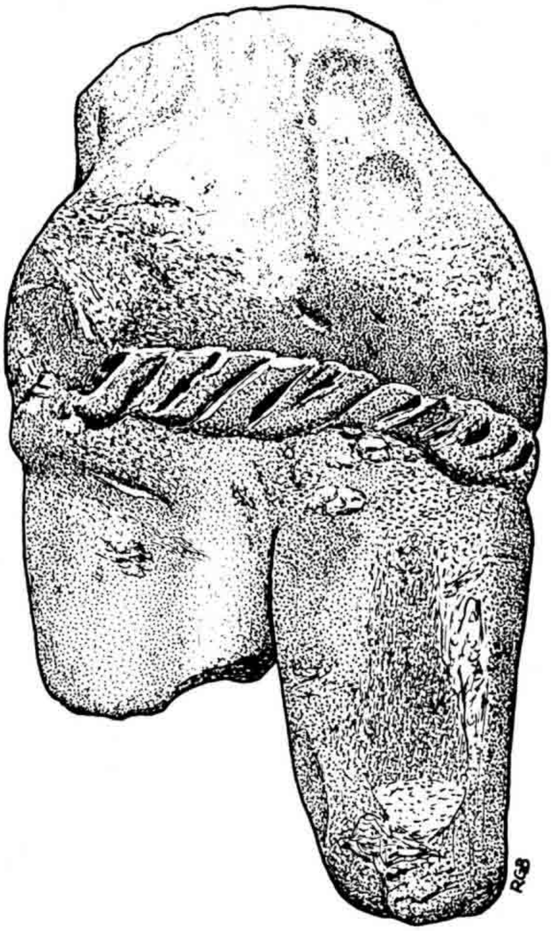

Figure 10. *Hip belt of fragmentary female torso from Pavlov, Moravia, of fired clay. Similar belts and armlets on Russian stone figurines are usually modelled as zigzag patterns.*

There are still other technological issues that have a bearing on the question of seafaring ability. They concern methods of carrying goods, of obtaining, storing, processing and cooking food of various types, of transporting water, and of meeting the technical requirements of whatever survival strategies were employed by the

Pleistocene seafarers of Wallacea and Sahul. Our archaeological knowledge about all these aspects is almost nil. For instance, we cannot even know with certainty whether *Homo erectus* in Flores (and presumably Timor) hunted any of the proboscideans, particularly the large *Stegodon* species whose remains are consistently, and at many sites, found together with their stone tools. The Stegodontidae, incidentally, were not restricted to South-east Asia, they occurred widely (e.g. Hooijer 1960), and in Israel together with Acheulian occupation evidence. Lower Palaeolithic hominids elsewhere, particularly in Europe, are well known to have been specialised large-game hunters. Bilzingsleben is a German site offering an extreme example. Of the total number of mammalian individuals recovered from that large living floor, rhinoceros accounts for 26.6 per cent, followed by elephants (12.1%), compared to a very small component of quarry of roe-size and smaller (26.0%). Bearing in mind that these percentages refer to numbers of individuals (Mania 1990), and not to weight, it seems that the Bilzingsleben hominids obtained much more than three quarters of their meat supply from the two largest species in their environment; they were highly specialised in this respect. The hominids of Ambrona, Torralba (both dominated by elephant remains) and Lehringen are also thought to have been successful elephant hunters.

Kuckenburg (1997) has reviewed the possible elephant hunting strategies of Lower Palaeolithic hominids in some detail. He lays to rest the Binford-inspired rejection of all hunting evidence prior to the appearance of fully modern humans. Elephants have been hunted and killed quite effectively by several methods in recent Africa, observed and described by many travellers. Driving into swamps, burning of dry vegetation, and the use of lances would have been available even to Lower Palaeolithic people. The latter method has been described as being quite effective: an elephant cannot see under its belly, so when a long lance is driven into it the animal is momentarily stunned, giving the hunter time to jump clear (Janmart 1952). If the lance touches the ground, it will be driven deeper by the animal's movements, and the quarry may impale itself in this way when it stumbles.

In view of the very limited number of potential food species on Middle Pleistocene Flores and Timor, of the hunting preferences of other *H. erectus*/archaic *H. sapiens* elsewhere, and of the consistent occurrence of tools with osteal remains at Flores it would seem reasonable that the island's earliest human inhabitants consumed a good deal of elephant meat. Nevertheless, it is worth noting that, if this were the case, it would refer to a period preceding the assumed elephant hunting in Europe's Middle Pleistocene.

We can also assume that, in order to have navigating technology, Flores *H. erectus* would have been adept in exploiting the rich coastal and marine environments. Perhaps this was even their main source of protein. Since we have already established that they are likely to have had simple spears, and for the crossing to Sahul later humans perhaps even had barbed spears or harpoons, it seems probable that they were capable of supplementing their food requirements at sea, by spearing marine fauna. In particular, it is known that in tropical waters, large fish often keep in the shade under vessels, travelling with them, and thus providing a ready store of potential food (Heyerdahl 1948). It seems extremely unlikely that the first sailors would not have availed themselves of this opportunity to supplement their supplies during long ocean voyages.

These are realistic assumptions, and to some extent they can be tested by means of 'replication'. Issues such as the means of carrying drinking water, for instance, will need to be resolved by such experimentation. However, one important variable to be taken into account is that we can confidently assume that the survival skills of the first sailors were superior to those of extant humans. Similarly, their threshold of dietary needs and tolerance of physical hardships would have rendered them considerably better suited for these journeys than *The First Sailors* crew is likely to be.

Lower Palaeolithic culture

Vita brevis, ars longa —
life is short, art is long (Hippokrates).

The gist of this paper, so far, is essentially that the first evidence we can detect of seafaring should indicate the minimum antiquity of what, for the sake of economy, I shall call 'language'. However, there are alternative means to consider this subject, and one of them is to look for alternative indications of symbolism. Verbal communication is a form of symbolic expression, in which uttered sounds can stand for concepts or ideas. In the social system of a species these sounds can have conventionalised meanings. Organisms possessing such sophisticated communication systems could conceivably share other forms of symbolism as well. Although language use cannot provide archaeologically visible direct evidence, forms of physical evidence of some other symbolic systems may survive over long periods of time, and if such forms of cultural evidence were created during hominid history we may still be able to detect traces of them.

In its scientific sense, 'culture' refers to the individually acquired system of 'understanding' which reflects the distinctive life trajectory of the organism in question (Handwerker 1989). It is a characteristic found in very many species, but especially well developed in primates. Cultural behaviour in ethology, including human ethology (which might be an appropriate scientific name for archaeology), involves the passing of information from one generation to the next by non-genetic means.

In reviewing the underground mining evidence above, special mention was made of the observed abilities of Pleistocene miners to anticipate the occurrence of not yet visible mineral resources, and to make economical or technological decisions on the basis of such predictions (Bednarik 1990a, 1995b). This is an example of complex intellectual reflection, and I have argued that I do not believe that such behavioural strategies were passed on genetically. Much the same applies to decisions about seafaring. In both cases, complex communication systems are necessary. Other archaeologically detectable activities that may indicate cultural behaviour, such as language use, are co-ordinated communal hunting, especially of very large species and with the use of specific types of weapons; the construction of shelters or separation of occupation sites into activity zones; and the collection of unusual objects (as manuports). In general, any trace of an activity is cultural if the behaviour it relates to had been transmitted by non-genetic means (i.e. by teaching).

There are basically two approaches that have so far been used to examine the possible cultural sophistication of hominids: through the assumed mental concepts involved in tool manufacture and in ecological strategies generally, and through the material evidence for 'non-utilitarian' activities. These may be seen as two distinct methods, but in reality they are parts of a continuum. This is best illustrated with the example of beads and pendants, which necessarily involve both complex technological processes and a social context within which such symbolic artefacts can have meaning. Nevertheless, in general I am inclined to favour the second type of evidence, non-utilitarian finds. Many authors have argued in favour of cognitive pronouncements about early lithic technology (e.g. Robson Brown 1993; Wynn 1993), but I find most of their pronouncements tenuous. It seems too easy to impose the analyst's concepts on the material evidence. An example is the claim, found several times in the literature, that Lower Palaeolithic stone tools provide evidence of 'mental rotation'. It is not the case that this does not appear to be a perfectly reasonable attribution, especially in relation to tools made in the Levalloisian technique. Their technique seems cognitively related to the ability of anticipating the continuation of a geological stratum in space. However, one appears to be on more solid ground with the second form of evidence, permitting us, it seems, a glimpse of cultural sophistication. Culture can lead to what are called 'non-utilitarian' practices: they provide no immediately obvious benefits for survival or reproductive success. These occur (and can be effective selection agents) both in humans and non-human species, but in humans they reach such unprecedented (at least in the history of this planet) complexity that they eclipse utilitarian cultural practices in significance. Some of these non-utilitarian practices can result in the production of objects, and some of these objects may survive.

I gather these objects together under the term 'palaeoart'. This means no more that they are 'art' (by whatever definition) than a peanut is either a nut or a pea; palaeoart is merely a name, a label. Palaeoart objects thus include portable 'art', rock art, beads and pendants, other objects thought to have been used in personal decoration, marked objects that imply a non-utilitarian activity, even unmodified natural objects that were used in symbolic capacities (e.g. perforated crinoid fossils used as beads). Naturally a symbolic capacity of an object is not always obvious, there have been almost countless examples of misidentifications: natural markings were seen as rock art, or vice versa (Bednarik 1994a); natural marks on portable objects were described as art (Bednarik 1992c); the description of manuports is usually tenuous; and it is perhaps often impossible to test whether natural objects, such as fossils, were used symbolically, although there are exceptions (e.g. Bednarik 1997d: 33). Therefore this subject is one of considerable controversy, which allows partisan opinions to dominate discussions and models. For instance, it is to be expected that 'African Eve' protagonists will make every effort to reject any instance of pre-Upper Palaeolithic symbolism, and having thus reduced the number of potential candidates to those that are impossible to discount, will point out that their number is too small to indicate 'systematic use of symbolism' (whatever this is intended to mean). When their respective models were challenged by demonstrating that they had been inadequately conversant with the relevant evidence offered over the years, Chase and Dibble (1992) and Davidson (1992), having earlier relied on the argument that quantity of evidence is too small, responded by maintaining their position in the face of a much greater quantity of material finds than they had been aware of.

Figure 11. *Jasperite cobble from Makapansgat, South Africa, deposited in an australopithecine-bearing sediment between two and three million years ago.*

The evidence suggesting symbolic capacities in Lower Palaeolithic hominids is of varied reliability, but such capacities did exist in that period. The very earliest glimpse we have of them relates to a find that predates the Palaeolithic period altogether. It is the Makapansgat cobble, found in the australopithecine-bearing breccia of a dolomite cave in South Africa in 1925. The reddish jasperite cobble was brought into the cave from some distance, presumably by *Australopithecus africanus*, between two and three million years ago (Figure 11). The first detailed analysis made of it in 72 years (Bednarik in press) showed that it bears no artificial modifications, the several faces on it are formed entirely by natural markings (cf. Bednarik 1996e). Nevertheless, if they were the reason why the cobble was carried into the cave, which seems to be the only sensible way to account for its presence in the cave, it would follow that the hominoids concerned possessed at least a rudimentary ability to recognise iconic properties of natural objects.

All other evidence of this kind is considerably younger. At approximately 800 ka ago, we can detect the first 'ochre' pebbles and quartz crystals in Acheulian sites, and the evidence that these types of minerals were collected and brought to home bases during much of the Acheulian has been found in India, France, South Africa and other countries. The quartz crystals are often much too small to have been collected for their use in making stone tools, some are only a few millimetres. The minerals collectively described as 'ochre' are so ubiquitous in all periods of human history that their presence in occupation deposits is sometimes equated with art production. While it is true that most ochre was probably used as pigment, this may have included body painting, the painting of artefacts, and it has been suggested that these minerals may even have been used in the processing of hides or the dressing of wounds. Whatever the case, ochre use refers to complex cultural practices, and its occurrence throughout the Palaeolithic periods, since at least 800 ka ago, illustrates cultural complexity.

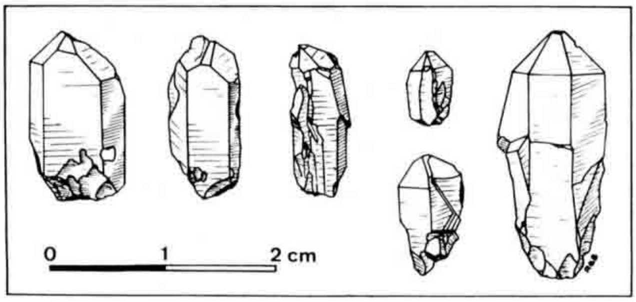

Figure 12. *Six small quartz crystal prisms from the Lower Acheulian of Singi Talav, near Didwana, Rajasthan, India.*

Lower Palaeolithic evidence of ochre use includes two haematite pebbles with striations of the Acheulian, one from Beçov, Czech Republic (Marshack 1981), the other with a striated facet, from Hunsgi, India (Bednarik 1990b). Several faceted fragments occur among 75 pieces of red, brown and yellow, probably fire-treated limonite from the Acheulian of Terra Amata, France (de Lumley 1966). The Acheulian also yielded an apparently shaped slab of ochre at Ambrona, Spain (Howell 1966). Two red pigment pebbles were even found in the Developed Oldowan, Olduvai BK II, Tanzania (Leakey 1958), and ochre occurs at most levels of the Acheulian in Wonderwork Cave, South Africa (Bednarik 1993c). Six complete quartz crystal prisms (7-25 mm), entirely unmodified except one (Figure 12) and all too small to have been tools, were excavated in the Lower Acheulian of Singi Talav, India (d'Errico et al. 1989). The Acheulian of Israel has yielded even smaller quartz crystals, at Gesher Benot Ya'aqov (Goren-Inbar et al. 1991). Lower Palaeolithic quartz crystals are available from several other sites (cf. Bednarik 1992a), and occur back to 900-800 ka ago in Wonderwork Cave, South Africa (Bednarik 1993c).

The 'curation' of fossils and other 'exotic' or unusual objects (Bednarik 1993c) has also been suggested to have a long history (Oakley 1981). It is of particular interest in the case of naturally perforated items (be they fossils or other materials) because they may have been used as beads. Such finds have been reported from Lower Palaeolithic deposits since the year of Darwin's *Origin of the species*, i.e. since the time the existence of a Lower Palaeolithic period was even recognised (and are still being ignored almost 140 years later!). Both Europe and Asia have yielded such material (Goren-Inbar et al. 1991; Marshack 1991), and it is found from the Acheulian through the Châtelperronian (Leroi-Gourhan 1965) and to the most recent past (Helmecke 1990: Pl. 13). Artificially perforated objects, expertly drilled through with stone points, have been found at two occupation sites of the Lower Palaeolithic, the Acheulian open site El Greifa in Libya and the Repolusthöhle in Austria (Bednarik 1992a, 1997d). The many claims that beads and pendants were introduced in the Aurignacian (e.g. White 1989, 1992, 1993) are rendered invalid by these, and the many similar Middle Palaeolithic finds.

Another frequent claim, found in almost all publications on the topic, is that rock art also began with the Upper Palaeolithic. Not only is the oldest rock art we know of (Bednarik 1993b) dated to the Acheulian (in Auditorium Cave, India), a great deal of other rock art is from essentially Middle Palaeolithic cultures, in four continents (Bednarik 1992a). Most especially, the corpus of Pleistocene rock art in Australia, which is thought to be considerably greater than that of European Pleistocene rock art, was created by artists with a Middle Palaeolithic technology. Indeed, there appears to be more surviving Middle Palaeolithic art in the world than Upper Palaeolithic.

A scoria pebble with several deep grooves that underline the object's shape of a female figure (Figure 13), of the Acheulian, >230 ka old, has been found at Berekhat Ram, Israel (Goren-Inbar 1986). The overall form is natural, but the grooves seem to be artificial and intentionally placed (Marshack 1997). Apart from the Pliocene Makapansgat cobble, this is the oldest indication we have of a possible recognition of iconic properties by hominids.

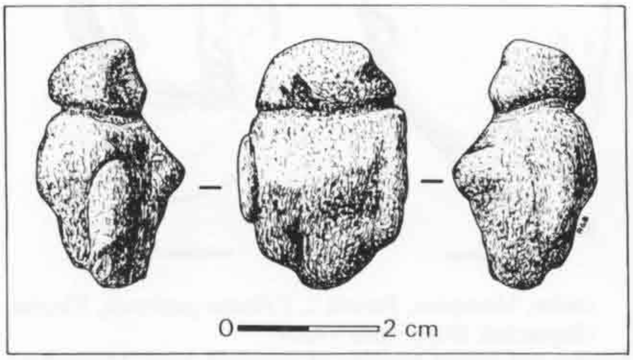

Figure 13. Scoria pebble with engraved lines, Acheulian, Berekhat Ram, Golan Heights, Israel.

*Figure 14. Detail of made pavement of rocks, elephant bones and teeth, around several dwelling foundations. An elephant molar (*Palaeoloxodon antiquus*) is visible in the foreground. The softer stone components are polished smooth from habitation traffic. Lower Palaeolithic, Bilzingsleben, north-eastern Germany.*

Figure 15. Engraved tibia fragment of a forest elephant, Lower Palaeolithic of Bilzingsleben, Germany.

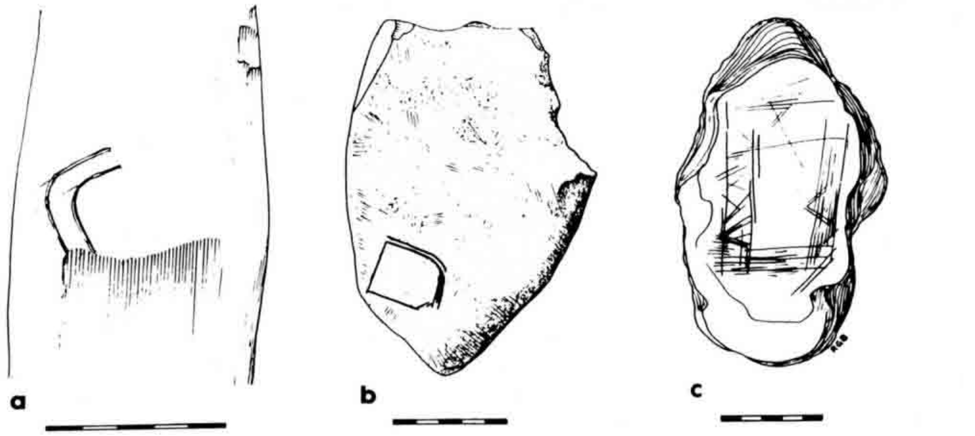

Figure 16. Three engravings from Bilzingsleben, Germany, on forest elephant ivory (a), sandstone (b) and elephant metatarsal (c). (Scales 5 cm.)

Relevant is also a set of engraved objects from the huge Lower Palaeolithic occupation site at Bilzingsleben, Germany. This site has distinct activity zones and features a remarkable pavement of rocks and large animal remains that have been pushed into the soft, swampy ground (Figure 14). Preservation conditions were among the best ever encountered at such an old site — Bilzingsleben is of the Holstein interglacial, c. 300 ka old. The engravings, clearly made with stone tools, have been found on five bone objects, mostly of the forest elephant; on a fragment of a polished ivory point; and on one sandstone slab (Mania and Mania 1988; Bednarik 1995a). It has been suggested that the engravings might be incidental marks, made when the objects were used as cutting boards.

However, this is unconvincing, for several reasons. The engravings on object 1 (Figure 15), a flat elephant bone fragment, are not on the broad flat surface, but on a narrow side bevel; those on object 2 are of repeated tool applications, on a surface quite unsuitable as a cutting board (which also applies to the sandstone object); and the double arc engraving on the ivory point fragment is certainly not incidental to a cutting action. The D-shaped engraving on the sandstone slab (Figure 16b) is the result of multiple tool application, which we can also observe on similar Middle Palaeolithic engravings on stone (Goren-Inbar 1990; Marshack 1996; Hovers et al. 1997). It seems impossible to find a plausible utilitarian explanation for this engraving, and since most of the Bilzingsleben engravings closely resemble engraved patterns of the later Middle Palaeolithic, they need to be seen in the context of early 'art' evolution (Bednarik 1995a: 614). They also resemble the engravings on a bone fragment, probably of a horse, bearing ten cuts along the edge, from the Upper Acheulian of Sainte Anne I, at Polignac, Haute-Loire, France (Crémades 1996), and a very similar arrangement on an elephant vertebra from Stránská skála, Czech Republic (Valoch 1987), also of the Lower Palaeolithic.

Engraved and notched markings on bones and the occasional stone plaque occur widely in the Middle Palaeolithic (Eurasia) and Middle Stone Age (southern Africa). They include the following (Figure 17):

Three engraved bone fragments and one engraved horse canine, Micoquian, Prolom 2, Crimean peninsula, Ukraine (Stepanchuk 193) (Figure 17a-c).

Serrated and incised fragment of mammoth bone, Mousterian, Schulen, Belgium (Huyge 1990).

Deeply engraved bone fragment, Mousterian, Bacho Kiro, Bulgaria (Marshack 1976) (Figure 17f).

Bovid shoulder blade with long parallel lines, Mousterian, La Quina, France (Martin 1907-10; Marshack 1991).

Small bone with numerous parallel lines, from a Neanderthal grave, Mousterian, La Ferrassie, France (Capitan and Peyrony 1921; Marshack 1976) (Figure 17e).

Small bone with several transverse notches, Mousterian, La Ferrassie, France (Capitan and Peyrony 1912).

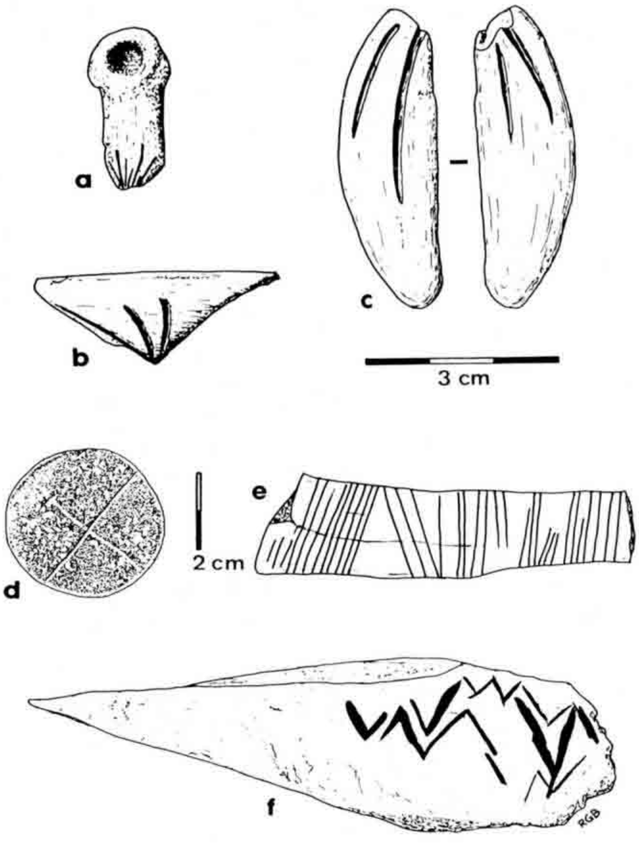

Figure 17. Six Middle Palaeolithic engravings from Europe: a-c: Prolom 2, Crimea; d: Tata, Hungary; e: La Ferrassie, France; f: Bacho Kiro, Bulgaria. Specimens a-c are Micoquian, d-f are Mousterian.

Five engraved bone fragments, Mousterian, Tagliente shelter, Italy (Leonardi 1988).

Bone retoucher with numerous incised lines, Mousterian, Tagliente shelter, Italy (Leonardi 1988).

Utilised bone fragment with a series of five barb-like, incised marks, Mousterian, Cueva Morín, Spain (Freeman and Gonzalez Echegaray 1983).

Rib fragment with paired line markings, Mousterian, Cueva Morín, Spain (Freeman and Gonzalez Echegaray 1983).

Bone artefacts with series of cuts, last Interglacial (< 100 ka), Taubach near Weimar, Germany (Kuckenburg 1997).

Fragment of a mammoth tusk, with a series of over twenty short, obliquely cut notches, Middle Palaeolithic, Wyhlen near Lörrach, Germany (Kuckenburg 1996: 141, 1997).

Engraved schist plaque with about 43 incised sub-parallel lines, which have been subjected to internal analysis, c. 50 ka old, from Temnata Cave near Karlukovo, Bulgaria (Crémades et al. 1995).

Engraved nummulite, Mousterian, Tata, Hungary (Vértes 1964) (Figure 17d).

Eight apparently intentionally notched bone fragments from the Mousterian of Abri Suard, Le Chaise-de-Vouthon, Charente, France (Duport 1960; Debénath and Duport 1971; Crémades 1996).

Other objects from Petit Puymoyen and Montgaudier listed by Débenath and Duport (1971), cf. Crémades (1996).

Bovid right mandible with nine regularly spaced engraved lines, crossed by another six lines of different length, from the upper of two Mousterian occupation layers in Peyrere 1 Cave, also called Noisetier Cave, near Arreau, Aure valley, Hautes-Pyrenées, France (d'Errico and Allard 1997).

Incised bones, Mousterian, Kebara Cave, Israel (Davis 1974).

Three rib fragments with serrated edges, Middle Stone Age (MSA), Klasies River Mouth, South Africa (Singer and Wymer 1982).

Small bone fragment with four parallel grooves, MSA, Klasies River Mouth, South Africa (Singer and Wymer 1982).

Notched rib fragment, MSA 3, Border Cave, Swaziland (Beaumont et al. 1978).

Two notched bone fragments, MSA 2b, Apollo 11 Cave, Namibia (Wendt 1974).

Engraved ostrich egg-shell fragments from two southern African caves, MSA (Bednarik 1993d).

This list of engraved and notched Middle Palaeolithic objects includes many items bearing marking strategies identical to those we have from the Lower Palaeolithic. It is therefore premature to reject the older examples simply because we have fewer of them. It is more appropriate to see them as part of an emerging overall pattern of early marking behaviour by hominids, and as an important source of information relating to Lower Palaeolithic culture (Bednarik 1995a). The number of instances known is, I have argued, irrelevant, because it is primarily a reflection of taphonomic factors (see below; Bednarik 1994b).

The origins of language

Natura non facit saltus —
nature makes no sudden jumps (Leibnitz).

Culture, I argued in the previous chapter, refers to the individually acquired system of 'understanding' which reflects the distinctive life trajectory of the organism in question. It refers to socially rather than genetically transmitted behaviour patterns and their products. 'Cultural dynamics', therefore, are the processes by which the intelligent organism alters its perceptible reality through its dialectic participation in the processes shaping it (Bednarik 1990). Since the inevitable outcome of such interaction between percepts, concepts and behaviour patterns is selection in favour of increased level of 'intelligence', it is to be expected to result in forms of 'consciousness', such as those of humans. The process is reified through the perceptible (e.g. to humans) externalisations of a species' concepts onto physical reality (art, in the case of humans), which renders possible the reality constructs of the species, because the neural structures supporting such concepts become available for processing natural sensory stimuli in a taxonomising format (Bednarik 1995a: 628). Since this is the basis of human consciousness, it would be pointless trying to understand human constructs of reality without considering this evolutionary context, or their nexus with cognitive evolution.

Since the purpose of the present paper is not to examine the origins of human language ability itself, but merely the question of the nexus between early marine navigation and language evolution, I will not consider the latter in any detail here. This would require a lengthy discussion, which can be summarised very easily: there is no consensus on this subject at all, and the hypotheses we have range from one extreme to the other. According to current hypotheses, the advent of language occurred between 2.5 million and 32 000 years ago. Another proposition all experts would agree on is that the Neanderthals' language ability was somewhere between that of an ape and that of a modern human. It is fair to say that we do not need experts to arrive at these two propositions, any reasonably educated member of the general public would have been able to do so without expert help. This illustrates the impotence of archaeology in resolving such a simple topic. If we then consider the seafaring abilities of the early human inhabitants of Flores, which could have easily been inferred for the past forty years, and accept with me Noble and Davidson's (1996) proposition that seafaring colonisation presupposes language use, we are more than justified in questioning the knowledge or motivation of all those who, during the past forty years, promoted models placing the advent of language in the Late Pleistocene, or indeed after the end of the Early Pleistocene (730 ka ago).

This short-term model, after all, is highly implausible even on the basis of simple biological and linguistic considerations (Bradshaw and Rogers 1993). The human system of producing verbal sounds differs profoundly from that of all other terrestrial mammals in one striking way. Darwin (1959) already observed 'the strange fact that every particle of food and drink which we swallow has to pass over the orifice of the trachea, with some risk of falling into the lungs'. Every year, thousands of humans choke to death on their food, whereas other mammals have separate pathways for breathing and feeding or drinking. Moreover, the problem is limited to human *adults*, being caused by the relatively low position of the adult human larynx. This appears to be an evolutionary trade-off, indicating a significant advantage in having complex verbal communication. The relatively short palate and lower jaw are less efficient for chewing than those of non-human primates and hominoids and they provide less space for teeth. However, the design of the human mouth and throat provides optimum conditions for differentiated sound production. The large size of the supra-laryngeal tract allows us to modulate and filter the frequencies of the sounds we make, in combination with the tongue and lips. These physical attributes are essential for the production of all human languages.

The second component of language evolution involved the brain structures responsible for the 'voluntary', 'intentional' control of speech (Bradshaw and Rogers 1993). In this respect, humans again differ significantly from other primates, even chimpanzees have great difficulty controlling their verbal expression (e.g. concealing pleasure). Chimpanzees, however, do possess a rudimentary ability to deceive (Byrne and Whiten 1988). But in the area of deceptive behaviour, humans are the undisputed masters. This, certainly, involves self-reflection and great neural control over the speech production centres. Lieberman (1991) attributes our control over language to certain changes in the brain, including the evolution of what is referred to as Broca's area, as well as the enlargement of the prefrontal cortex and a rewiring of concentrations of neurons, the basal ganglia.

What renders the short-term model of language evolution particularly absurd are its biological implausibility (e.g. encephalisation is more pronounced from hominoids to the first hominids, than during subsequent human evolution) and its inability to account for several simple observations. For instance, children are born with a genetic predisposition towards language acquisition, with an innate syntactic mechanism that appears to be biologically determined by neural structures. As Bickerton (1990) observed, there are 3 628 800 ways in which ten words can be arranged in a sentence. Take the sentence, 'Try to arrange any ordinary sentence consisting of ten words': only one sequence provides a correct and meaningful message, 3 628 799 variations are ungrammatical. Yet humans develop the correct understanding of syntax and grammar so rapidly, within the first years of their life. If ontogenic development were an approximate recapitulation of phylogenic evolution (which is not necessarily a valid measure, but does seem to provide a rough guide), language acquisition would precede iconic production and would develop along with tactile precision and discovery of the self's identity.

The involvement of so many cortical zones in speech production, with all their interconnections, render the evolution of this system within some tens of millennia biologically most improbable. Broca's and Wernicke's areas, which have been claimed to be detectable on cranial endocasts of *Homo habilis*, may be unreliable indicators of language ability, but their very early presence may indicate that some of the required structures were available to habilines. Similarly, the debates over the hyoid Neanderthal bone from Kebara Cave (Arensburg et al. 1989) and similar issues have remained unproductive. Aspects of hominid physical morphology have not contributed to resolving the question of language beginnings, nor are they likely to be a decisive factor. The most recent major syntheses on the subject return to linguistic and archaeological perspectives (Bickerton 1996; Dunbar 1996; Aitchison 1996), and their authors arrive at the same basic finding: human language is such a complex phenomenon that its evolution, in every sense, must have been a lengthy process. It must have developed from some form of proto-language, which in turn would have been derived from still more rudimentary beginnings. This, in effect, is fully consistent with the archaeological record, contrary to what many commentators have perceived in recent years. During the last few decades there has been a growing willingness of archaeology to be led by simplistic reductionist models and attention-grabbing slogans, and I regard recent language origin models as symptomatic of this trend.

The linguistic approach to the question is considerably more productive, but it will always remain ambiguous, especially in a chronological sense. Any tentative time frames applied to it are very doubtful and untestable. This leaves us with just one alternative, and it is the one I prefer. Reliable pronouncements can be secured from archaeology, by inferring hominid capacities from archaeological indices that have been subjected to taphonomic logic, the most powerful interpretational tool in archaeology. To illustrate this approach I shall select one class of objects, beads.

Beads (and pendants) provide a great deal of technological and cognitive information about their makers and users. First of all, they are usually reliably identifiable: small perforated objects may have been either beads or pendants, or they could have been quangings, pulling handles or buckles as reported ethnographically (e.g. Boas 1888: Figs 15, 17, 121d; Nelson 1899: Pl. 17; Kroeber 1900: Fig. 8). However, most of the utilitarian objects of this type are not only of a quite typical shape or design, they exhibit specific wear traces and material properties. To be more specific, small circular objects with central perforation are considered to be beads, especially when they occur repeatedly. Similarly, objects such as animal teeth, perforated near one end (near the root) are not thought to be pulling handles, nor are objects that are too fragile to function as such utilitarian equipment.

We can therefore safely assume that the drilled ostrich eggshell discs from El Greifa (Figure 18) and the drilled wolf's canine from the Repolusthöhle (Bednarik 1997d) were indeed beads. I have experimentally demonstrated that they were made as delicate and perfect as possible, they are deliberate technological masterworks, exploring the limits of the methods available to Lower Palaeolithic hominids. These objects were not just non-utilitarian, they represent *statements of excellence* (as, indeed, developed 'handaxes' often appear to do). Their specific qualities demand the existence of a socially shared and communicated value system, which would necessarily involve reflective communication. In fact, any use of beads and pendants must involve a sophisticated social system, a system of enculturated values.

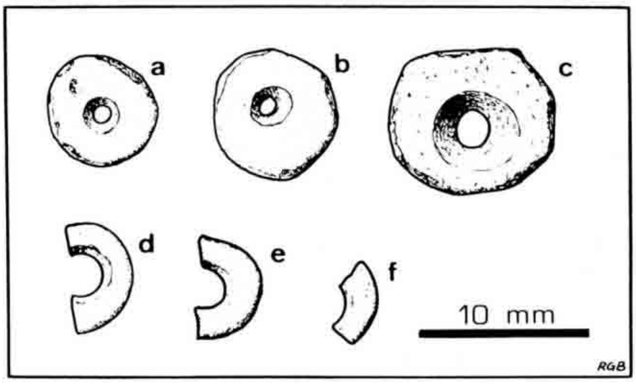

Figure 18. *Pleistocene ostrich eggshell beads from India (a-c) and Libya (d-f). The three lower specimens are of the Acheulian.*

Irrespective of what the actual cultural meaning of beads was, such meaning demands that these objects were made in some numbers. This is because it is *repeated* and socially 'structured' use which confers meaning on symbolic artefacts. Without such a mechanism, beads are meaningless, and the extraordinary manual effort that went into their manufacture would have been perfectly pointless. The statement that beads may have simply been 'decorative' is itself vacuous, being anthropocentric. There is no evidence that other species perceive beads worn by humans as decorative, nor would one assume that a hominoid would have perceived such a quality.

So far we have not even considered the technological aspects of such sophisticated artefacts, including those of procuring and selecting the raw materials (drills, grinding stones, strings, bead material), the required variety of manufacturing processes (re-sharpening of drills, making of knots), the ability of forward planning, or the multiplicity of skills involved in the production of beads (see Bednarik 1997d). Nor should we need to: the social context they demand for their very existence is sufficient to show the need for complex communication, of a form capable of conveying nuances such as 'quality', 'quantity', 'perfection', or whatever else a wearer of beads sought to communicate. Nevertheless, the technology provides further indications of the culture concerned. The perforation of a bead or pendant has but one purpose: to thread it onto some kind of string. This in turn almost demands the use of knots, because any alternative method to join the ends of a string would have been even more complex than a simple knot. We can thus begin to unravel the technology of the period in question, and every aspect of it points to a great degree of sophistication, approaching that which we have become accustomed to attribute to the Upper rather than the Lower Palaeolithic. Most certainly, these considerations should lay to rest the traditional view of seeing cultures such as the Acheulian as almost unchanging, as if the forms of bifaces and cleavers were a valid measure of cultural complexity.

There are still more pertinent questions to ask: why did *Homo erectus* suddenly, it appears, leave Africa and so rapidly colonise many parts of the Old World in such a short time? This phenomenal expansion, unequalled among mammalian species, cries out for a rational explanation. If there had been a hominid predisposition to expand, to colonise, then earlier species could have done so. Yet 1.8 million years ago, apparently within a geological instant, *H. erectus* occupied all readily accessible regions, and began to develop adaptations to occupy unfamiliar ecological niches, and a high tolerance to climatic diversity. By 800 ka his navigational ability was such that he could successfully colonise islands and establish flourishing settlements on them. There should be no doubt that, by that time, his language ability was sufficiently developed to organise successful ocean crossings, with all that this involves.

All of this tends to define the Lower Palaeolithic as being marked by technological and cultural innovations that exceed in importance anything *Homo sapiens sapiens* has been able to contribute to the ascent of man. The great developments that determined the direction of humans all took place between 1.8 and 0.8 million years ago. *H. erectus*, it would seem, is the greatest achiever in our ancestral line.

Epistemology and politics in archaeology

Seen in its proper context, the issue of navigational origins brings into focus the greatest case in which a human group (our entire species, collectively) could be said to have appropriated the credit due to another group (the species preceding us), in order to write its preferred version of history. This, essentially, is what the 'African Eve' hypothesis seeks to underpin, perhaps unwittingly. Throughout the known history of humans there has been a distinctive tendency of creating false histories of ethnic groups, nations, ruling houses, individual rulers and political causes, always at the expense of other groups or individuals. Some societies even extinguished their historians whenever a new ruler came to power, in order to create new versions of history that favoured new rulers. However, never before has an entire hominid species been implicated in appropriating the achievements of a preceding *species*. The trend in recent world archaeology, best exemplified by the 'Eve' scenario, has been to gradually whittle away the human features of our Pleistocene predecessors. The Neanderthals are one example of the victims of these recent developments in world archaeology, which I regard as being attributable to the introduction of fundamentalist ideology into the discipline. The 'African Eve' or 'Garden of Eden' hypothesis, which lacks credible archaeological evidence of any form and is presented as some kind of origins myth of Western ontology, is a classical example, but there are various more subtle endeavours apparent in politicising archaeology.

The most significant effect of the 'African Eve' model is not so much how it shapes ideas of hominid evolution or genetics, but that in order to survive it needs to negate all evidence of human sophistication prior to the advent of what is often defined as 'anatomically modern humans' (cf. Tobias 1995 for a cogent critique of this concept). Its fundamental ideological theorem is that only these modern humans possessed the technological, cultural, intellectual, cognitive and social skills to develop symbolism, art, language, advanced tool-making abilities, self-consciousness — and seafaring. When they displaced the various inferior resident populations throughout the Old World this led to a quantum jump in human progress. Such an 'explosion-like' development (White 1989, 1992, 1993) explains the eradication of the archaic sapienoids, including Neanderthals, much as the destruction of historical indigenous societies by European colonisation was justified by the 'superior' cultural and moral values of the colonisers. These pernicious ideologies are not usually made explicit in archaeological discourse, rather they are implied, as is the fundamentalist ontology driving their hypotheses (coyly expressed in Biblical metaphors at times). I have for years presented evidence that this 'punctuated equilibrium' model of cultural evolution is largely false, and that it can readily be refuted by considering various factors: taphonomic logic (Bednarik 1994b), significant ignorance about existing archaeological data (1993a, 1995d, 1995f), the pigeonholing effects of untestable taxonomies in archaeology (1990/91), extremely uneven research efforts in different parts of the world (1994c), artificial plateaus introduced by such factors as specific dating methods and cultural chronologies (1996f), the confirmationist epistemology of the discipline, and the tendency to seek to defend unfalsifiable constructs in archaeology, as well as to collect and interpret data in accordance with preconceived paradigms (1992a, 1995f, 1996f). Here I have presented a classical example in which the ignorance of crucial information has led to a whole swathe of false models and ideas in the discipline.

Metamorphology, the scientific study of archaeological epistemologies (Bednarik 1995f), is concerned with several aspects of archaeological knowledge claims. One of the most interesting of these, in helping to establish patterns of heuristic dynamics in the discipline, is 'the ignorance of archaeologists concerning existing data ..., how language barriers and other biases limited the flow of information in this field, or how false constructs ... flourished' (Bednarik 1995f: 120). Here I have presented a classical example of this, and a case history that will provide us with the opportunity of a continuing study of academic dynamics in this discipline. We will be able to observe and then analyse the responses of those whose pet theories are refuted by the data discussed above. These responses, I expect, will include the usual measures questioning the academic credibility of those found to have been right. This pattern of response is typical in archaeology, a discipline in which saving face is consistently considered more important than refutation and veracity. It has been the case since the days of Boucher de Perthes, during the first half of the 19th century, and examples can be found in every decade since then. Even from the most recent two decades, several specific examples come to mind, in which archaeological heretics were victimised, viciously attacked or 'black-balled'. Such heretics may of course be patently wrong, this is not the issue. The issue are the kind of responses of those whose models are under threat, which include character assassination, public vilification, intentional misinterpretation and distortion of the controversial claims, and various other unworthy methods. Archaeologists of unusually great personal integrity and honesty, such as Professor Revel Mason of Johannesburg, have explained this phenomenon in some detail, and such findings need to be considered in any realistic metamorphological enquiry into the heuristic dynamics of archaeology. Until his and similarly critical ideas about archaeological epistemology become part of mainstream knowledge, archaeology cannot fully appreciate its own ontology and political role, and the devastating effects its preoccupations have on veracity, not to mention the personal effects on those who have tried to correct a false record.

In the present example, we have seen that world archaeology, particularly its Anglophone sector, has remained ignorant of crucially important data. Numerous archaeological theories announced in recent decades are either negated or significantly weakened by the evidence that *Homo erectus* possessed navigational ability and, presumably, language at least 700 or 800 ka ago. This evidence has been available for forty years. For instance, if Davidson and Noble (1989 *et passim*) had been aware of this, their hypothesis of very recent language origins would presumably not have been postulated. Books such as those of Gamble on global colonisation (1993) would hopefully not have been written, nor the various debates about language and the human mind that appeared in the *Cambridge Archaeological Journal*. The same applies to nearly all other theorising on the beginnings of language, symbolism and culture, even early technological capacities. In fact it is fair to say that if most English-speaking archaeological authors had not remained ignorant of the archaeological evidence presented from Wallacea forty ago, Pleistocene archaeology would have taken a rather different direction during this time: it would have been governed by different ideas and priorities, and not have wasted a great deal of effort on models and research directions that were predicated on inadequate knowledge. The cognitive sophistication implied by pre-Upper Palaeolithic palaeoart finds (Bednarik 1990a, 1992a, 1995a) would have received a much more favourable consideration. Since both this and the complete lack of any technological or cultural diffusion attributable to the perceived advance of 'anatomically modern humans' out of Africa seem to negate a simplistic diffusionist model we can also assume that the 'African Eve scenario' would have enjoyed a somewhat less enthusiastic reception. In short, it is true that the availability of language would have been the greatest barrier to the cognitive development of hominids, but in the case discussed here it seems that the more relevant language barrier is not that of *Homo erectus*, but that among archaeologists.

In 1995 I took archaeology (not *Australian* archaeology, as some claimed) to task over this matter (Bednarik 1995d). In response, Groves (1995) chided me for implying that I understand pre-History better than any Australian archaeologist, that 'Australian archaeologists really ought to learn to read German', and various other transgressions he perceived. For instance, he took great umbrage over my suggestion that *Homo erectus* may have persisted in Java into the Upper Pleistocene. This, of course, was a year before Swisher et al. (1996) presented their evidence, according to which the Ngandong population persisted not just into the Upper Pleistocene, but into its latest phase. Another example of his critiques is that I mentioned erectoid features in robust Australian crania. 'I wonder where Robert Bednarik has been over the last 15 years', Groves declared triumphantly. Nothing has occurred in the past 15 years that could have prompted Wolpoff or Thorne to change their minds on this issue (cf. Webb 1989). It would be much more appropriate for me to ask, where has Groves been for the past 40 years, seeing that he has written so much about Wallacea, without ever considering the single most important archaeological evidence in the region, and to examine his lack of knowledge about Wallacean pre-History. This would be vindictive of me, however, and I am not inclined to focus on individual shortcomings, because I am much more interested in the systematic issues, the issues of epistemology. In this case: why has the Flores evidence been totally ignored in all Australian, and indeed Anglophone, archaeological literature?

It is obvious that until 1995, all publications on the Early or Middle Pleistocene of Flores, be they archaeological or palaeontological, were published in German, Dutch or French journals. Over the decades I have formed the opinion that, of all the archaeological data ever published, only a small percentage has ever been published in the English language. A good part of this appeared in very small, comparatively unknown journals, so it does not form any part of the general archaeological knowledge available to Anglophone archaeology. During the course of the 20th century, this problem has been compounded as English became the favoured international language, which in this case introduced a linguistic bottleneck: as archaeological 'world knowledge' was determined by what had become known in that one language, other knowledge was even more marginalised. Through extensive travel and collaboration with numerous specialists world-wide, I have become aware of a great deal of archaeological knowledge, some of it very important, which remains totally unknown to monolingual English speakers. In many cases, such information was in fact first made available in English by me. For instance, the existence of Chinese rock art remained unknown outside China until 1984, at which time some 10 000 sites were known in the country. I published the first English-language report on this topic in 1984.

At the end of the 20th century, English publications have become the benchmark of global knowledge in archaeology, for better or for worse, and they determine almost exclusively the 'big picture' models in this discipline. I find this unsatisfactory. To illustrate with an example: the amount of archaeological knowledge available in English, from that vast area of the former Soviet Union, is negligible, and in my view quite inadequate. Much the same applies to many other world regions. Yet the major models are based on these systematic distortions in knowledge availability. It is impossible to appreciate the magnitude of these problems without having examined them, and to respond to my concerns, as Groves does, by interpreting them as a request that Australian archaeologists ought to learn German, only illustrates his lack of understanding. This is a major issue, and should not be dealt with so lightly and flippantly, if archaeology is to be taken seriously.

Graeca sunt, non leguntur —
it is Greek, one does not read it.

Final discussion

The theoretical implications for archaeology of the evidence briefly summarised here are far-reaching. Contrary to popular archaeological mythology, *Homo erectus* appears to have been innovative and enterprising, and his ability to colonise most environments of the Old World should be considered as the product of culture (transference of practice by non-genetic means; Handwerker 1989; Bednarik 1990a). If there had been a hominid predisposition to adapt to a great variety of environments, then earlier hominids could and would have done so. The remarkable and rapid expansion of *H. erectus* implies the involvement of new adaptive tools, of abilities not available to earlier hominid or hominoid species. A distinctive change preceding the expansion of *H. erectus* is an increase in meat eating among hominids, which has important ecological implications (Brain 1995). Herbivorous animals are in most cases tied to particular plant ecologies, their populations expand and contract in accord with vegetation changes. Carnivorous species are not directly affected by this, all meat being much the same. Therefore they can become independent of ecological niches and they tend to expand their home ranges as well as their geographical ranges. Indeed, some taxa show considerable climatic tolerance in doing so, such as wolves and tigers, which range from the Siberian taiga to the Indian tropics. It seems likely that, as omnivores such as early hominids adopted a meat-dominated diet, they also developed a predisposition to expanding their territorial range. One of the most conspicuous aspects of *H. erectus* is a developing specialisation in carnivorous diet towards large and even very large species, focusing eventually on rhinos and elephants. Increasing reliance on meat eating may thus have created favourable conditions for the rapid expansion of *H. erectus* as implied by recent Asian evidence discussed above, which the new tool of proto-language was able to exploit effectively. That expansion would also explain the absence of an Acheulian east of the Movius Line (between India and South-east Asia). If *H. erectus* was present in eastern Asia almost two million years ago, i.e. before the appearance of the Acheulian in Africa, he may not have adopted the 'hand-axe' technology east of the Movius Line.

Colonisation by navigation virtually presupposes skilled, purpose-specific communication and symboling abilities in the population concerned (Bednarik 1995d; Noble and Davidson 1996). While there is no proof that this included uttered language, there is also no evidence that *H. erectus* lacked language. On the contrary, various forms of physiological and linguistic evidence would strongly favour the use of language by that species (Bradshaw and Rogers 1993). The probable colonisation of various Wallacean islands implies repeated success in settling new lands permanently, and the almost habitual use of navigation by the descendants of Wallacean *H. erectus*. In stark contrast to Groves' (1995) negative response to my challenge to archaeologists, to take a serious interest in Wallacea, Morwood began to investigate the Flores evidence during 1996 (Morwood et al. 1997). His collaborative project with Indonesian and Dutch colleagues is among the most important archaeological work now conducted in the Pacific rim regions. Their preliminary findings fully support those of Verhoeven during the 1950s and 1960s, and the subsequent work by Maringer and others. I consider Morwood's ongoing work as the most exciting in Australian archaeology for many years.

The evidence summarised in this paper does not establish the origins of language. There is overwhelming evidence in favour of ocean navigation having begun 800 000 or more years ago, and a nexus between early seafaring evidence and communication has been examined here. Another aspect are the implications for the taphonomy of navigation evidence. All the earliest hard evidence we have of seafaring, or indeed any form of navigation, is of the Holocene. The earliest known examples, as noted above, are from the beginning of the Holocene, their number increases over subsequent millennia, and rapidly increases as the middle Holocene is approached. The traditional, simplistic archaeological interpretation of this pattern is that navigation began with the end of the Pleistocene, was practised rarely initially, but became progressively more common, and was well established by Pharaonic times. I reject completely this kind of reasoning, which interprets the archaeological sample as representative and random. I have two reasons for this. First, no archaeological sample can be random, be it in the physical, statistical or interpretational sense; it has undergone innumerable selective processes before it is *subjectively* recorded by the archaeologist. These selection processes are complex, often cumulative in their distorting effects, and are in many cases beyond the appreciation of the interpreter (Bednarik 1995f). Secondly, such a simplistic interpretation would ignore the phenomenon of taphonomic lag, which in this case is particularly dramatic. With navigation being at least 800 ka old, and its oldest reliable hard evidence being little more than 8000 years old, it is obvious that the taphonomic lag amounts to a staggering 99 per cent in this case.

A taphonomic lag applies to every single class of archaeological evidence, to every type of material. It can range from less than one per cent to more than 99 per cent, but it can never be 0% or 100%. It is important to realise that examples of finds of a class of evidence can be recovered from the taphonomic lag period, but they are extremely rare and their occurrence, spatially or temporally, is almost irrelevant (Bednarik 1994b: Fig. 2), while their quantity is totally irrelevant. In the case of navigation, we have no known finds from the lag period. Classes of evidence such as those made from leather, sinew, fibre, wood, bark, resin and so forth all can be assumed to have taphonomic lag periods well in excess of ninety per cent of the duration of the material's use, and the upper end of their lag times all fall into the Holocene. Thus, in accordance with taphonomic logic, the number and distribution of such Pleistocene finds should not be discussed within the logic of traditional archaeology. The same, very importantly, applies to hominid remains: for instance, the geographical distribution of such remains simply reflects geological and other preservational or taphonomic factors. It is almost unrelated to actual temporal or spatial distributions of former hominid populations. This kind of logic is not just very foreign to archaeologists, it is the precise opposite of what they base their interpretations on.

These issues are further complicated by other metamorphological factors, besides those of taphonomy, including methods and other modes of recovery, strategies of interpretation, modes of reporting and selective dissemination, statistical treatments, the biases of the reporting archaeologists (determined by education, perception, cognition, religion or ideology, and a host of further variables, especially language limitations, relevant knowledge etc.) and those of the interpreting reader, and several variables related to the context in which many archaeologists need to work (e.g. priorities of research traditions, institutes and funding agencies). Some of these metamorphological factors are very apparent in the case considered in the present paper.

This paper leads to the reasonable proposition that language in some form was available 800 ka ago, a proposition I have tested by examining other archaeological evidence. Cultural and technological indices seem to demand considerably greater sophistication for the Lower Palaeolithic period than has hitherto been accepted by mainstream archaeology. An interesting side issue, not explored in the present paper, is that the Flores stone tools, albeit still quite archaic, could well be described as some of the most advanced lithics of their time. There appears to be a good case for South-east Asia having been a hub of technological and other development towards the end of the Early Pleistocene. Similarly, the evolution of language is most realistically seen as a development of that same geological/palaeontological period, from about 1.8 to 0.8 million years ago. Both the apparently rapid spread of erectoid populations and the development of navigational ability seem to have been results of this developing language ability.

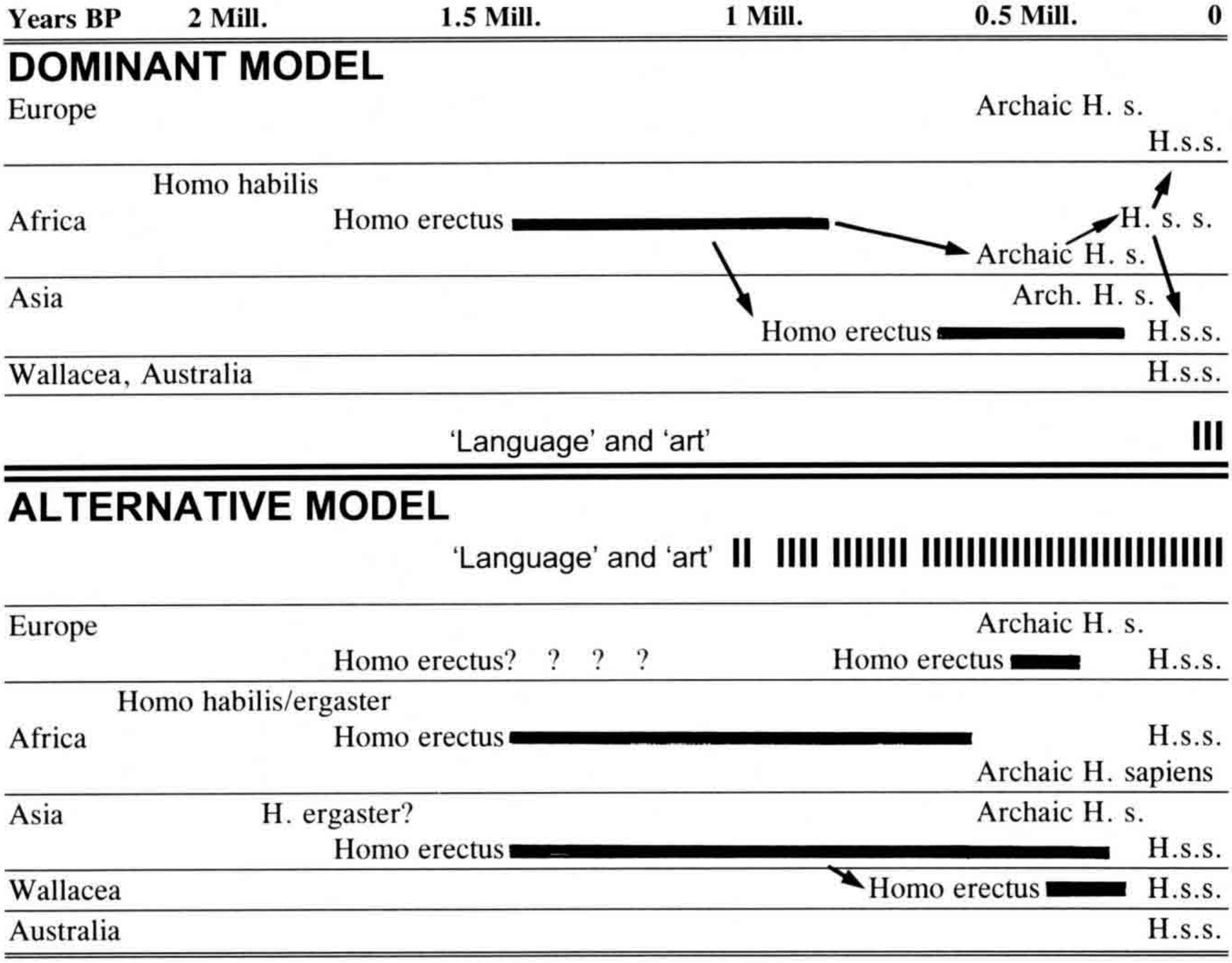

Figure 19. *Two models of human origins, illustrating the transience of all these models. In this example, the recently dominant model is compared with one incorporating the current paper's findings. This is not to suggest that my alternative model is correct; rather it is 'likely to be less false' than the dominant model, and one can only hope that it will soon be superseded by better models. My alternative model, nevertheless, is a great deal more realistic than the currently still dominant model.*

This model is at massive odds with the currently dominant paradigm (Figure 19), according to which only fully modern people, essentially of *Homo sapiens sapiens* stock, developed language, the ability of forward planning, complex technologies, 'art', even culture. Davidson, one of the principal proponents of this paradigm, is quite right when he declares: 'it is entirely clear from every phrase of [Bednarik's] article that our objectives are entirely different' (Davidson 1992: 52); 'It is clear that the convention of understanding of the past that is the starting point for Bednarik is fundamentally opposed to the intent and practice of the exploration that Noble and I have been engaged upon' (ibid.: 56). The sound Davidson hears is that of a very different drum, and he arrives at conclusions and models that differ from mine so much that we are almost incapable of communicating about them. Davidson is guided, throughout, by a humanistic desire to create fundamental differences between humans and other animals (a point well made by Mithen 1997), and since no such differences can be identified by a scientific form of discourse (e.g. concerning human ethology), he needs to establish an arbitrary division line. Like the 'Garden of Eden' protagonists, he places this Rubicon between archaic *H. sapiens*, such as Neanderthal, and 'anatomically modern humans'.

This epitomises the differences between human ethology (scientific pre-History) and archaeological 'prehistory', the latter being an entirely ethnocentric, sapiens-centric and anthropocentric pursuit. The scientist is obliged to treat a human species in precisely the same way as any other. The orthodox archaeologist has a rather different agenda, firmly rooted in humanist Western ideology. Herein lie the irreconcilable differences between, for example, Davidson and myself. To Davidson, *my* archaeology is hard to understand, esoteric and probably humbug; to me, *his* archaeology is a belief system, a religion based on a mixture of sound and unsound — and in most cases untestable — propositions.

Errare malo cum Platone quam cum istis vera sentire — I would rather err with Plato than be right with them [the Pythagoreans] (Cicero).

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